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Universal Vouchers and White Flight

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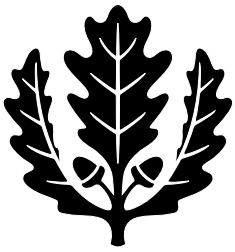

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Abstract

Opponents of school vouchers often argue that school vouchers will lead to white flight from public schools that are disproportionately nonwhite, creating more racially segregated schools. However, recent studies that examine white flight from public schools into private schools have produced conflicting evidence on whether or not white flight actually exists. In this paper, we present new evidence on whether universal vouchers will lead to more racially segregated schools. Specifically, we use data on vote outcomes from a state-wide universal voucher initiative to estimate the likelihood that white households with children currently in public schools will use vouchers to switch out of more-integrated schools. Our results indicate that white households with children attending schools with large concentrations of nonwhite schoolchildren are significantly more likely to support school vouchers, an effect that is absent for non-white households with children and households without children. However, it also does not appear to be race, per se, that is the primary concern, but other school factors that are correlated with race, such as test scores and limited English proficiency.

Journal of Economic Literature Classification: H3, I2, R2

I. Introduction

In the debate over school choice, one of the chief concerns about universal vouchers is that they may lead to more racially segregated schools. Since *Brown v. Board of Education*, school desegregation efforts have attempted to ensure equal educational opportunities for students of all races through more integrated schools. These efforts could be undermined if white households are more inclined to use vouchers to flee from the public sector, creating more segregated schools.[1] However, because no state has yet adopted a comprehensive voucher, we cannot directly predict what the effects of such a policy are likely to be. Instead, existing evidence on the impact of vouchers on racial segregation has come from studies that examine "white flight" within the context of existing forms of school choice (e.g., private schools, charter schools, or limited voucher programs). In general these studies have produced conflicting evidence on whether or not expanded school choice will lead to more racially segregated schools. For example, Lankford, Lee and Wyckoff (1995) and Fairlie and Resch (2002) find that higher concentrations of black or minority students in area public schools increase the likelihood that white households enroll their children in private schools, apparent evidence that vouchers will increase racial segregation. However, other researchers, including Campbell, West and Peterson (2005) and Buddin, Cordes, and Kirby (1998), find no evidence of 'white flight', suggesting that perhaps the concerns of voucher opponents are overstated.

In this paper, we present new evidence on whether universal vouchers will lead to more racially segregated schools. Specifically, we use data on vote outcomes from a state-wide universal voucher initiative to estimate the likelihood that white households with children currently in public schools will use vouchers to switch out of more-integrated schools. Our conceptual framework highlights the difference between the *causal* effect of race on the residential and school choice decisions of white households, and what we refer to as the *equilibrium* effect. The causal effect captures whether school racial composition, per se, causes white households to opt out of public schools, all else equal. In order to obtain causal estimates, the analyst must be concerned with potential bias from two types of omitted variables: unobserved household preferences that may be correlated with the racial composition of the school where the household currently resides, and unobserved school attributes that may be correlated with racial composition (such as school quality). Once these biases are removed, the casual effect measures the impact of racial composition if white households were randomly assigned to schools that are identical except that they have different levels of minority concentration. In contrast, the equilibrium

[1] Recent empirical evidence also suggests that the racial composition of a school is a large determinant of the black-white gap in test scores, with black students performing substantially better when they are in racially-mixed environments, with no measurable effect for white students (Card and Rothstein, 2004; Hanushek, Kain and Rivkin, 2002). Thus, in addition to the basic civil rights concern that all students have access to an equal education, more integrated schools appear to have a direct beneficial effect on black student performance.

effect measures the impact of racial composition on household choices *allowing* for the fact that households may have already sorted across neighborhood schools based on their preferences for racial homogeneity and *allowing* for the fact that a school's racial composition is likely correlated with other school attributes. The equilibrium effect is thus an indication of the *additional* sorting, given the choices that households have already made, that we should expect to occur under a comprehensive voucher system.

While the causal effect is a legitimate concern in its own right, we argue that it is clearly the equilibrium effect that is most important to policymakers concerned about the stratification effects of adopting a universal voucher. To illustrate that point, consider the behavior that must occur in order for vouchers to increase racial segregation beyond current levels: white households with children currently in relatively integrated public schools must use the voucher to leave the public sector at higher rates than non-white households with children in those schools. Note that from the perspective of whether vouchers are likely to lead to more racially segregated schools, it does not matter *why* white households are opting out, it only matters that they do. Whether whites are fleeing from minority students per se or from bad schools, high crime, immigrants, etc., doesn't really matter. If these are all highly correlated, the end result will still be whites fleeing schools with observably more minority students, and universal vouchers will still lead to increasingly segregated schools. On the other hand, it *does* matter that households have already chosen their residential location and school, as parents that are relatively satisfied with their current choice will be less likely to use the voucher to move. To use an extreme example, if there were costless mobility and enough schools such that every household could send their child to a school with a bundle of attributes that exactly matched their preferences (i.e., perfect Tiebout sorting), then the adoption of vouchers would have no effect on the behavior of white households, regardless of the racial composition of schools and even if the original sorting had been driven entirely by racial preferences.

Thus, whether vouchers will increase racial stratification depends on the relative impact of racial composition (and all variables correlated with race) on household choices, allowing for Tiebout sorting. Given the degree to which households have already sorted, the distinction between the casual and equilibrium effects is not a trivial one. As pointed out by Clotfelter (2001), urban public schools generally became more racially segregated in the 1990's, while Urquiola (2005) finds that MSAs with greater amounts of choice among districts are more stratified by income, race and educational attainment. Policymakers concerned with the impact of adopting a comprehensive voucher program should therefore focus on the equilibrium impact of the racial composition of schools on household behavior, recognizing that households have chosen to reside in their current school district. However, we argue that few studies of white flight have actually estimated this effect. Instead, we suggest that one explanation for the conflicting empirical estimates of white flight found in the literature is that previous authors have defined

white flight in different ways, estimating models that fall somewhere between the causal and equilibrium effects.

In this paper, we focus specifically on identifying the likelihood that *additional* stratification may occur under a comprehensive voucher system, given the choices that households have already made. Unlike previous studies of white flight that have generally extrapolated from data under existing forms of school choice (e.g., private schools or limited voucher programs), we use votes on a state-wide universal voucher initiative in California as a proxy for voter intention to use the voucher. Assuming that support for the initiative reflects a desire to take advantage of the voucher, voting patterns can reveal which schools are most likely to lose students if a comprehensive voucher were adopted. One distinct advantage of using this data is that, to the extent that voters are non-myopic, these patterns will take account of the general equilibrium effects (e.g. changes in public school quality or the availability of private school options) of a universal voucher.[2] That is, a universal voucher, available to all families on a state-wide basis, would fundamentally change the institutional structure of school finance and could impact household decisions in ways that limited, targeted programs would not. Rather than extrapolating from existing choices (that might have been very different under an entirely new system), our data may better represent the school choice decisions that households would make under a universal voucher.

We find that a universal voucher *is* likely to increase racial segregation. White households with children currently in public schools are increasingly supportive of the voucher if their children attend schools that have higher concentrations of minority students, an effect that is absent for non-white households. However, it does not appear to be race, per se, that is the primary concern, but other school factors that are correlated with race, such as test scores and limited English proficiency. This suggests that policymakers could mitigate the potential for segregation by working to improve school quality in schools with large concentrations of minority students.

II. Conceptual Framework

The primary motivation for this paper is the question of whether a universal voucher system will lead to increased racial segregation. Much of the evidence on this question has come from studies of 'white flight' out of predominantly non-white public schools. However, the concept of 'white flight' involves several issues that are not always cleared delineated in the literature. In the most general sense, white flight in education is simply the propensity for white households to opt out of public schools with high concentrations of non-white students.[3] However, this general definition begs several additional

[2] See Epple, Romer, and Sieg (2001) and Calabrese, Epple, Romer, and Sieg (2005) for a discussion of whether voters are myopic.

[3] Wikipedia actually defines white flight as "a colloquial term for the demographic trend of upper and middle class white people moving away from (predominantly non-white) inner cities, finding new homes in nearby suburbs or

questions. For instance, when white households opt out, where do they go? Original studies of white flight were concerned with white flight out of urban areas to suburban neighborhoods (i.e., residential choice).[4] More recent studies have focused on white flight into private schools (i.e., school choice).[5] These studies often use methods that control for the fact that white households may have already sorted residentially, attempting to purge any effect this sorting may have on the public-private school choice. However, from the perspective of whether vouchers are likely to lead to more racially segregated schools, white flight due to either residential or school sorting is important. Predicting the equilibrium impact of a voucher system on racial segregation must include both the direct impact of a school's racial and ethnic composition on the desire to opt out, plus the effects of preference differences between parents residing in predominantly minority and non-minority school districts.

Furthermore, note that increases in racial segregation will only occur if white households opt out at significantly greater rates than non-white households. If white and minority families are equally likely to use vouchers to opt out of the public sector when their children attend schools with high concentrations of minority students, then these schools may lose a large number of students but there may be little effect on the overall racial composition of public schools.[6] Thus, to predict the equilibrium impact of vouchers on residential segregation, one must examine how a school's racial composition affects the school choice decisions of both white and nonwhite households.

To develop the empirical implications of the points made above, we present a very simple model of voucher use that conditions only on the demographic composition of the school. Let Y_{is} denote the likelihood that an individual *i* who is a member of group k (k = white, nonwhite) in school *s* uses the voucher. This is assumed to be linear function of the minority share in the school, M_s, the individual's unobserved preferences for racial homogeneity, P_{is}, and an idiosyncratic error term, ε_{is}:

$$Y_{is} = \beta_0^k + \beta_1^k M_s + P_{is} + \varepsilon_{is} \qquad (1)$$

P_{is} may be correlated with M_s to the extent that households have already sorted based on race; that is, households may have already chosen their current school based in part on their preferences for racial

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even moving to new locales entirely, e.g. from the Rust Belt to the Sun Belt." The entry then goes on to discuss the effect of desegregation on white flight from urban public schools.

[4] See, for example, Clotfelter (2001), Crowder (2000), and Galster (1990).

[5] These include Lankford, Lee and Wyckoff (1995), Conlon and Kimenyi (1991), Buddin, Cordes and Kirby (1998), Lankford and Wyckoff (2000), Fairlie and Resch (2002), and Betts and Fairlie (2003).

[6] Many studies of white flight have used results solely from white households to draw implications about racial stratification under vouchers, but doing so implicitly assumes that only white households will flee while non-white households will stay. While this may be a relatively safe assumption, it is, of course, an empirical question.

homogeneity. β_1^k is the direct effect of minority concentration on voucher use by group k, controlling for these preferences.

The equilibrium effect of a change in minority concentration on voucher use by group k is thus defined as:

$$\beta_1^{k,Eq} = Mean^k\left(\frac{dY_{is}}{dM_s}\right) = \beta_1^k + \left(\frac{dE[P_{is} \mid k,s]}{dM_s}\right) \tag{2}$$

Let us assume that as a result of sorting, the preferences of an individual in school *s* can be described with a linear function of school minority share and an idiosyncratic error, μ_{is}:

$$P_{is} = \alpha_0^k + \alpha_1^k M_s + \mu_{is} \tag{3}$$

The average group preferences in a neighborhood are described by:

$$E[P_{is} \mid k,s] = \alpha_0^k + \alpha_1^k M_s + E[\mu_{is} \mid k,s] = \alpha_0^k + \alpha_1^k M_s, \tag{4}$$

which implies that $\frac{dE[P_{is} \mid k,s]}{dM_s} = \alpha_1^k$. Using equations (2) through (4), the equilibrium effect of a change in minority concentration on voucher use can now be expressed as:

$$\beta_1^{k,Eq} = \beta_1^k + \alpha_1^k, \tag{5}$$

Note that if white households with strong preferences for racial homogeneity have already sorted into more homogeneous schools, we would expect α_1^w (where w denotes white households) to be less than zero. Thus, equation (5) makes it clear that for white households, residential sorting will mitigate the equilibrium impact of racial composition on voucher use, relative to the direct effect alone.

For estimation, we cannot observe preferences so the expected value of the ordinary least squares estimator of β_1^k can be derived from equation (1) using standard theoretical results on omitted variables with one observable regressor, M_s; and simplified using equation (3). Specifically,

$$E\left[\beta_1^{k,OLS} \middle| M_s, k\right] = \beta_1^k + \left(\frac{E[P_{is}, M_s \mid k]}{E[(M_s)^2 \mid k]}\right) = \beta_1^k + \alpha_1^k \tag{6}$$

which equals the equilibrium effect derived in equation (5). Thus, with data on voucher use and school minority share, we can uncover the equilibrium effect of school racial composition on voucher use by

estimating a very simple model with OLS. A comparison of these effects for white and non-white households can then indicate the extent of any changes in racial segregation that may follow the adoption of a universal voucher program.

Unobserved school attributes

Defining white flight as the general propensity for white households to opt out of public schools with high concentrations of non-white students also raises the question of whether it is race, per se, that is driving white households away. Equation (1) is highly simplified in that school minority share is the only school characteristic that affects voucher use. However, it has been well-established that race is correlated with a number of school characteristics that may also factor into the decisions of white households, most notably school quality.[7] Many studies have therefore attempted to distinguish between race and other correlated factors as the motivation for white flight. Not surprisingly, controlling for other school attributes typically leads to smaller effects of race. Yet, from the perspective of whether vouchers are likely to lead to more racially segregated schools, it does not necessarily matter *why* white households are opting out; it only matters that they are. We therefore specifically do *not* want to include any variables that are correlated with school minority share that may affect a household's decision to use the voucher. This is not to say that the underlying reasons are not important, as they certainly hold significant social and policy implications. But in this particular case, it truly is correlation, not causation, that matters. If we want to answer the question of whether vouchers are likely to increase racial segregation, it is sufficient to estimate equation (1), separately for white and non-white households, to obtain the coefficient in equation (6), what we refer to as the equilibrium effect of racial composition.

In contrast, to estimate the true causal effect of racial composition, itself, on the likelihood that white households will want to use a voucher, we should control for both residential sorting and school attributes that may be correlated with race. The equilibrium effect includes both these biases but note that they work in opposite directions. The bias from sorting will generally lead to an equilibrium effect that is smaller than the causal effect (since households that have already sorted are less likely to use the voucher), whereas the bias from the omitted school characteristics will generally lead to an equilibrium effect that is larger than the causal effect (since it may not be race, per se, that causes households to opt out). With this framework, we can now begin to understand the seemingly "conflicting" results of previous studies of white flight. Many of these studies estimate models that fall somewhere in between the equilibrium effect and the causal effect and they find correspondingly larger or smaller effects of race.

[7] For example, Downes and Zabel (2002) and Brasington and Haurin (2005) find that households appear to use easily observable characteristics, such as race, when comparing public school quality among schools. Their results suggest that perceived school quality is higher in schools and school districts that contain lower concentrations of minority students.

In general, studies that account for sorting will tend to find larger effects than studies that do not. On the other hand, studies that include better controls for school characteristics that may be correlated with race will tend to find smaller effects of race, relative to studies with fewer or weaker controls.

It is therefore not surprising to find that studies that are generally considered to provide support for the white flight hypothesis tend to estimate models that control well for sorting but have weak controls for omitted variables. For example, Lankford, Lee and Wyckoff (1995), Conlon and Kimenyi (1991), Fairlie and Resch (2002) and Betts and Fairlie (2003) all measure racial concentration at the MSA and/or county level to examine white flight from public to private schools. Since the decision of which MSA to live in is likely driven by factors other than preferences for school attributes (for example availability of jobs or presence of family members), analysis at the MSA level provides relatively exogenous variation in minority concentration and thus controls relatively well for within-MSA sorting on school attributes. Thus, these studies that control for residential sorting within MSAs, are likely to produce estimates of white flight that are larger than the equilibrium effect. At the same time, in each of these studies, the only controls for school quality are the average level of spending per pupil and/or the average student to teacher ratio in the MSA or county. Given the lack of evidence that these measures are good proxies for quality, particularly at higher levels of aggregation, this is unlikely to do much to reduce any bias from omitted variables and may also lead to larger estimates. [8]

Conversely, studies that are generally cited as evidence against the existence of white flight tend to estimate models that do not consider residential sorting but have stronger controls for school quality. For example, Campbell, West and Peterson (2005) use the racial composition in the public school in which an individual student resides, a measure which is likely to suffer from much stronger sorting bias and lead to smaller estimates for the effect of race. They also include per-pupil spending for the district in which the student resides, arguably still a weak measure of school quality but stronger than spending at the county or MSA level and so would lead to somewhat smaller estimates. Buddin, Cordes and Kirby (1998) use test scores, a much stronger measure of school quality, and also find no statistically significant effects of race.

To our knowledge, only one paper, Lankford and Wyckoff (2000), attempts to deal directly with both residential sorting and omitted school characteristics, thus producing estimates that are closest to the true casual effect. They find a negative and statistically significant effect of school racial composition on the decision of white households to send their children to private school but find much larger effects on the decision of white households to move to the suburbs.

[8] See Hanushek (1986) for a review of the literature on the relationship between school spending and student performance.

Will vouchers increase racial segregation?

Note that none of these studies estimate effects that are truly equivalent to the equilibrium effect. Since predicting whether or not a universal voucher system will lead to increased racial segregation depends on this equilibrium effect, we believe this is still very much an open question. If we had data on actual voucher take-up in a universal voucher system, we could estimate equation (1) directly and β_1 would capture the equilibrium effect. Unfortunately, such data do not exist; although several states have considered legislation to establish universal vouchers, none have yet adopted such a policy.[9] In order to predict the effects of wide-scale vouchers, many of the studies mentioned earlier have extrapolated from the existing choice to attend private school. Campbell, West and Peterson (2005) use data on actual voucher use in a national voucher program. However, that program was targeted to low-income families and may not reflect behavior under a universal voucher system. A universal voucher, available to all families on a state-wide basis, would fundamentally change the institutional structure of school finance and could impact household decisions in ways that limited, targeted programs would not. For example, a universal voucher may lead to improvements in public school quality or in the availability of private school options, changes that may not arise under targeted programs. Thus, even if previous papers had been trying to estimate the equilibrium, rather than the causal, effect, it is unclear whether those predictions would hold in the very different institutional setting created by a comprehensive voucher.

In this paper, we estimate the equilibrium effect using data on vote outcomes from a state-wide universal voucher initiative in California. Specifically, we estimate equation (1) using votes for California's Proposition 38 as a proxy for voucher take-up. Proposition 38 was a statewide ballot initiative in 2000 that would have provided families with a scholarship for every child enrolled in a private school. The scholarship would have been the greatest of three amounts: $4,000, half the national average of public school spending per pupil, or half California's public school spending per pupil. The initiative placed few conditions on scholarship-redeeming schools and prohibited the state from placing additional conditions on these schools in the future. Because the scholarship would have been made available to all students, including those already enrolled in private schools, Proposition 38 would have created the first universal voucher system in the United States.

It seems reasonable to assume that someone who wishes to use the voucher would vote for Proposition 38. If we assume that voters are cognizant of the general equilibrium effects that may follow the adoption of a comprehensive voucher, then we can use those votes to predict which schools are most likely to lose students under the new policy. At the same time, we recognize that voting on Proposition

[9] It should be noted that other *countries* have adopted fairly comprehensive voucher programs. Hsieh and Urquiola (2004) find that that Chile's voucher program led to increased sorting as a disproportionate number of students from

38 may also have been influenced by factors other than a direct desire to use the voucher. For example, people may have voted for or against the initiative because they expected it to affect their property values or the quality of their child's school. An individual's political persuasion (i.e., Republican or Democrat, conservative or liberal) also may have impacted their vote. Because we want to capture voucher *take-up*, we will need to expand equation (1) to control for any variables that might affect voting for reasons other than a direct desire to use the voucher.

III. Analysis Using Survey Data

During August, September and October of 2000, the Public Policy Institute of California (PPIC) surveyed a total of 6,022 potential voters concerning issues related to the November 2000 ballot. The surveys were conducted by telephone, using a random-dialing procedure, and were restricted to people age eighteen or older. Baldassare (2000a, 2000b, 2000c) compares the distributions of various characteristics among survey respondents with the distributions of those characteristics from the 2000 Census. He finds that the survey distributions are quite similar to the Census distributions, indicating that the surveys were successful in obtaining a representative sample of California residents.

Our focus is on the voting behavior of households with children in public school. Specifically, we seek to ascertain whether white households with children in public school were more likely to support the voucher if their child attended a school with a large concentration of minority students.[10] Of the 6,022 respondents, 1,636 had children enrolled in public school. Because the PPIC survey does not identify the public school attended by each respondent's children, we used data from the 2000 Census geographic files to match the zip code of each respondent to the corresponding closest public high school. Specifically, we first matched the zip code of each respondent to a corresponding school district. We then matched the centroid of the zip code to the closest public high school within that school district. In cases where a zip code crossed district boundaries, we matched the zip code to the closest public high school in each of the districts that were associated with the zip code. We then weighted the characteristics of each school (fraction minority, student enrollment, etc) by the fraction of the zip code's population residing in that school district. Using this procedure we were able to assign detailed public school characteristics to each respondent in our sample. All school-level data for the 1999-2000 academic year were obtained from the California Department of Education.

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high- and middle-income households opted out of the public sector. Ladd and Fiske (2001) find that the introduction of a quasi-universal voucher system in New Zealand led to increased racial segregation.

[10] Note that households with children already in *private* school may be influenced by the minority concentration in the local public school as well, and may vote for the voucher to create a subsidy for themselves. However, because those students have already opted out of the public sector, it is only households with children in *public* schools that will have a marginal effect on the racial composition of public schools in the aftermath of a voucher.

Our baseline specification contains only minority concentration in a respondent's local public high school, and a small set of variables that are likely to affect voting on the voucher for reasons other than a direct desire to use the voucher. School minority concentration is measured as the fraction of students in a local public high school that are nonwhite. We also include indicator variables for gender, political ideology, homeownership status, and whether a school is located in a rural area. The gender indicator, which has a value of unity for female voters, controls for systematic gender differences in voter support for school vouchers. The ideology indicator takes a value of unity if the respondent reported being politically somewhat conservative or very conservative, and is included to account for the fact that school vouchers are a mainstay of conservative political ideology. The homeownership variable captures the fact that vouchers may affect property values. In a system where households must live in a particular neighborhood in order to attend a particular school, it is well-established that school quality will be capitalized into housing values (e.g., Black, 1999) as families must pay a premium to live in high-quality districts. Vouchers remove this residential constraint and as discussed in Nechyba (2000, 2003), vouchers may therefore cause property values to rise in low-quality districts and to fall in high-quality districts. This creates incentives for homeowners to vote for or against vouchers, depending on where they currently reside. This prediction is supported by Brunner, Sonstelie and Thayer (2001) and Brunner and Sonstelie (2003) who find that homeowners are significantly less likely to support school vouchers if they live in a good school district. To account for the fact that capitalization effects are likely to be weak or nonexistent in rural areas where the supply of housing is relatively elastic we also include an indicator variable that takes the value of unity if a school is located in a rural area.[11]

Finally, we also include a set of Metropolitan Statistical Area (MSA) fixed effects in our analysis to control for unobservable regional variation in support for school vouchers. We use the December 2000 definitions of metropolitan and micropolitan statistical areas, developed by the Office of Management and Budget (OMB), to define MSAs. Metropolitan areas are defined as urbanized areas with a population of at least 50,000, while micropolitan areas are defined as urbanized areas with a population of at least 10,000 but less than 50,000. Both metropolitan and micropolitan areas are defined in terms of counties and include both the county containing the central core of the urbanized area and adjacent counties that have a high degree of economic integration as measured through commuting patterns. Of the 58 counties in California, 46 are located in one of the 37 metropolitan or micropolitan areas within the state. For

[11] See Reback (2005) and Hilber and Mayer (2004) for a discussion of, and evidence on, capitalization effects in rural areas.

respondents located in one of the 12 counties that are not part of a metropolitan or micropolitan area we define MSAs in terms of the county within which the respondent lives.[12]

Our data have a number of limitations. The first limitation concerns missing data. Among the 1,636 respondents with children in public school, 182 did not answer the question about how they intended to vote on the voucher initiative, 29 refused to report their race, and an additional 58 respondents reported a zip code that either did not exist or was located in a state other than California. We eliminated these respondents, leaving 1,367 respondents. Several respondents also did not answer two of the questions used in the analysis. Specifically, 27 respondents did not answer the question about political ideology and 26 did not report their homeownership status. Rather than exclude these respondents from our analysis, we created two dummy variables indicating whether observations on ideology or homeownership were missing. We then included the ideology and homeownership variables (with missing values recoded to zero) and the two missing value dummy variables in our analysis.

The second limitation concerns zip codes that overlap school district boundaries. Recall that for zip codes that cross district boundaries, we use a weighted average of the fraction of nonwhite students. Specifically, for each district within a zip code, we weighted the fraction of minority students in the closest high school within the district by the fraction of the zip code's population residing in the district. Because we cannot precisely match each respondent in our sample to a unique public high school, our measure of minority concentration most likely suffers from measurement error. This, in turn, would tend to bias the parameter estimate on minority concentration towards zero. To mitigate that bias, we restrict our analysis to include only those respondents that live in a zip code where at least 75% of the zip code's population resides within the boundaries of a single school district. That restriction reduces our sample by about 10%, leaving us with 1,225 observations for our analysis.

The final limitation of the survey data is related to the stated versus revealed preference nature of the survey. While the PPIC survey was successful in obtaining a representative sample of Californians, there is one dimension in which the survey is not representative. Forty-six percent of respondents stated that they intended to vote for the voucher initiative. In contrast, only 30 percent of the actual ballots cast in the November election were in favor of the initiative. We conduct our analysis under the working assumption that the survey responses are valid for testing whether certain factors affect the likelihood of supporting the voucher even though the responses in the aggregate predict more support for the voucher than was actually witnessed at the polls. We will test the validity of this assumption in section IV.

[12] We also estimated all the models and specifications reported in the paper using county fixed effects rather than MSA fixed effects. All of the results obtained using county fixed effects were quite similar to those reported in the paper. Results are available upon request.

Table 1

Summary Statistics

Variable	White Households	Nonwhite Households
Vote on Voucher Initiative	0.46	0.58
(1=yes, 0=no)	(0.50)	(0.49)
Homeownership	0.70	0.43
	(0.46)	(0.49)
Female	0.53	0.54
	(0.50)	(0.50)
Conservative	0.38	0.40
	(0.48)	(0.49)
Minority Share	0.46	0.71
	(0.24)	(0.25)
Rural	0.10	0.05
	(0.31)	(0.22)
Observations	672	553
Schools	380	314

Summary statistics for the variables used in our analysis are reported in Table 1. As mentioned earlier, vouchers will only lead to changes in racial stratification patterns if white and non-white households use the voucher at very different rates. Therefore, although our primary focus is on the voting behavior of white households with children, we will also examine how minority concentration affects the voting behavior of nonwhite households with children. Table 1 thus reports the mean and standard deviation (in parentheses) of each variable separately for white households with children in public school and for minority households with children in public school. A brief inspection of Table 1 reveals several interesting patterns. First, white households with children in public school were less likely to support the voucher, on average, than nonwhite households with children in public school. This complements the findings Campbell, West and Peterson (2005) who examine the determinants of voucher application and voucher usage among a sample of households that were eligible for the Children's Scholarship Fund (CSF), a program that offers private school vouchers to low-income families. Their results suggest that, relative to whites, African-Americans and Hispanics were significantly more likely to apply for CSF vouchers. Second, as expected, white households tended to have substantially higher homeownership rates than minority households. Third, children from white households tended to attend schools with substantially lower concentrations of minority students than children from minority households. That finding is consistent with the notion that there is already significant racial stratification across California's public schools.

Table 2

Estimated Coefficients for White Households with Children in Public School

Regression Specification:	(1)	(2)	(3)
Minority	0.246**	0.308**	0.282**
	(0.108)	(0.117)	(0.120)
Homeowner	-0.034	-0.035	-0.006
	(0.044)	(0.044)	(0.049)
Conservative	0.278**	0.281**	0.277**
	(0.040)	(0.040)	(0.040)
Female	-0.056	-0.053	-0.061
	(0.040)	(0.040)	(0.040)
Rural	0.143**	0.139**	0.130*
	(0.067)	(0.067)	(0.068)
Population Density		-0.102	-0.091
		(0.074)	(0.074)
log Income			-0.006
			(0.033)
Education			-0.047
			(0.041)
Age			-0.004*
			(0.002)
Observations	672	672	672
R-squared	0.16	0.16	0.17

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

Results

We examine the relationship between minority concentration and support for school vouchers using a linear probability model.[13] Parameter estimates for the sample of white households with children in public school are presented in Table 2 with standard errors in parentheses. Since the school-level characteristics in our analysis do not vary across respondents located in the same school attendance zone, we adjust the standard errors to account for clustering of the data at the school level. Results for our baseline specification that includes a limited set of control variables are reported in column 1 of Table 2. The coefficient on minority concentration reveals whether white households with children in public school were more likely to support the voucher if their child attended a school with a large concentration of minority students. The coefficient is positive and statistically significant at the five percent level. The point estimate also suggests that minority concentration has a relatively large effect on the propensity of

white households with children to support school vouchers. For example, our model predicts that increasing the share of minority students in a school from 10% to 80% would increase support for the voucher among white households with children in public school by 17 percentage points.

Column 2 of Table 2 presents results from an expanded specification that adds population density (measured at the zip code level) to the baseline model. We include this variable to further control for the fact that concerns over the potential impact of school vouchers on housing values may induce voters located in more densely populated areas to vote for or against the voucher for reasons other than a direct desire to use the voucher. Specifically, as demonstrated by Hilber and Mayer (2004), the degree to which school quality is capitalized into housing values will depend on the supply of land available for new development. Capitalization effects are likely to be stronger in more densely populated areas that contain a limited supply of land for new development.[14] At the same time, however, population density may directly affect whether a household is likely to use the voucher. In particular, population density is highly correlated with whether a school is located in an urban or suburban area. To the extent that households who have chosen to move to the suburbs have done so to escape high concentrations of minorities, controlling for population density will also control to some extent for preference for racial homogeneity. Thus, population density may also affect voucher take-up directly. For this reason, we do not include population density in the baseline specification but add it as a specification check. Adding population density to the model causes the coefficient on minority concentration to increase somewhat in magnitude from 0.246 to 0.308. The fact that the coefficient on minority concentration increases in magnitude when we control for population density is consistent with the notion that population density may be capturing differences across urban and suburban areas in preferences for racial homogeneity. In particular, as we noted in section II, controlling for unobserved preferences for racial homogeneity should produce estimates of white flight that are larger than the equilibrium effect.

We also estimate an additional model that adds a standard set of demographic variables found in the literature on public-private school choice (e.g. Figlio and Stone 2001; Lankford and Wyckoff 2000; Fairlie and Resch 2002). These include family income, parental education, and the age of the parent who responded to the survey.[15] In the PPIC survey, respondents were asked to report their family income and age in terms of six mutually exclusive ranges (e.g. income of $40,000 to $59,999 or age between 45 and 54). We used the midpoints of these ranges to assign a unique income and age value to each respondent. These variables may affect voting behavior through ideological preferences but are also quite likely to

[13] We estimate a linear probability model for ease of interpretation and for comparison with results reported later in the paper. We have also estimated all the specifications using a binary logit model. The results were qualitatively and quantitatively similar to those reported in the text. Results are available upon request.
[14] Hilber and Mayer (2004) provide evidence consistent with this hypothesis.

directly affect desire to use the voucher and we therefore include them as a specification check only. Results based on a specification that adds these variables to the model are reported in column 3 of Table 2. The inclusion of these additional explanatory variables has little effect on our parameter estimates, particularly the minority concentration variable.[16]

Recall that to mitigate measurement error we restricted our analysis to include only those respondents that lived in a zip codes where at least 75% of the zip code's population resided within the boundaries of a single school district. Table 2A of the appendix investigates how sensitive our baseline results are to this restriction. Column 1 presents results for a specification that uses the entire sample of 743 respondents that are White with children in public school. Column 2 presents results for a specification that limits the sample to include only those respondents that lived in a zip code where at least 65% of the population resided within the boundaries of a single school district. Similarly, columns 3, 4, and 5 present results for specifications where we limit the sample to include only those respondents that lived in a zip code where at least 75% (column 3) or 85% (column 4) or 95% (column 5) of the population resided within the boundaries of a single school district. Consistent with the notion that measurement error may bias the coefficient on minority concentration towards zero, the point estimates for the minority concentration variable reported in columns 2 through 5 are all larger than the estimate reported in column 1, and continually increase in magnitude. Furthermore, note that while the point estimates on the minority share variable reported in Table 2A vary in magnitude, they are all positive and, with the exception of the coefficient in column 1, they are all statistically significant at the 10 percent level or better.

Nonwhite Households with Children

The results reported in Tables 2 and 2A consistently suggest that white households with children in public school are more likely to support school vouchers if their child attends a school with a large concentration of minority students. Higher support for a universal voucher suggests that these households are more likely to use the voucher to opt out of the public school sector. Our results are therefore consistent with the notion that universal vouchers would lead to increased "white flight" from public schools into private schools. However, this alone does not necessarily imply that vouchers would lead to

[15] Forty-five of the respondents in our sample did not answer the question regarding income. We included missing observation flags for these individuals.

[16] We also estimated a model that added only income, education and age to the baseline model. Results based on that specification were nearly identical to those reported in column 1 of Table 2. In particular, adding these variables to the model caused the coefficient on minority concentration to decline modestly from 0.246 to 0.224.

Table 3

Estimated Coefficients for Minority Households with Children in Public School and Households without Children

Regression Specification:	White with Children	Nonwhite with Children	White no Children	Nonwhite no Children
Minority	0.246**	-0.109	0.003	0.135
	(0.108)	(0.107)	(0.056)	(0.088)
Homeowner	-0.034	-0.031	-0.064**	-0.098**
	(0.044)	(0.044)	(0.023)	(0.040)
Conservative	0.278**	0.169**	0.292**	0.129**
	(0.040)	(0.041)	(0.024)	(0.039)
Female	-0.056	-0.036	-0.027	-0.003
	(0.040)	(0.045)	(0.022)	(0.036)
Rural	0.143**	-0.085	0.048	-0.111
	(0.067)	(0.115)	(0.049)	(0.121)
Observations	672	553	2032	760
R-squared	0.16	0.09	0.11	0.08

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

more racially segregated public schools. For example, if minority families are equally likely to opt out of the public sector when their children attend schools with high concentrations of minority students, then vouchers may have little effect on the overall racial composition of public schools. Thus, to determine whether universal vouchers will lead to more racially segregated public schools, we must also examine how minority concentration affects the voting behavior of minority households with children in public school. We therefore estimated the same baseline specification (reported in column 1 of Table 2) using the sample of minority households. The results are reported in column 2 of Table 3. For comparison purposes, the results from our baseline specification for white households with children in public school are reported in column 1 of Table 3. For the sample of minority households with children in public school the coefficient on the minority share variable is statistically insignificant, negative, and quite small in magnitude, suggesting that the voting behavior of minority households is unresponsive to the share of minority students their children are exposed to. Thus, taken together, the results reported in columns 1 and 2 suggest that universal vouchers would lead to more racially segregated public schools.

Households without School-Age Children

We have argued that the results reported in Table 2 suggest that white households with children in public school are more likely to *use* the voucher if their children attend schools with high concentrations of minority students. We have included several variables to control for factors that may affect a

households' vote on the voucher initiative other than a direct desire to use the voucher. However, it is still possible that white households residing in areas with high-concentrations of minority students may be voting for the voucher initiative *not* because they intend to use the vouchers but because of other unobservable factors that affect their support for vouchers. To examine that possibility, we estimated our baseline specification using the sample of white households with no school-age children. Assuming that the results reported in Table 2 are being driven primarily by a desire among white households with children in more integrated schools to *use* school vouchers, the voting behavior of white household with and without school-age children should look quite different. In particular, we should observe white households with school-age children having a much stronger reaction to minority concentration than white households without school-age children. In contrast, if the results reported in Table 2 are being driven by indirect benefits associated with the voucher, the voting behavior of white household with and without school-age children should look similar.

Results for the sample of white households with no school-age children are reported in column 3 of Table 3. For the sample of white households with no school-age children, the coefficient on the minority share variable is statistically insignificant and quite small in magnitude, suggesting that the voting behavior of white households with no school-age children is unresponsive to the share of minority students that attend their local public schools. That finding gives us increased confidence that the results reported in Table 2 are capturing a relationship between support for school vouchers and minority concentration that is unique to white households with children in public school. As a further specification check, we also re-estimated our baseline specification using the sample of nonwhite households with no school-age children. Those results are reported in column 4 of Table 3. Once again, the coefficient on the minority concentration variable is small in magnitude and statistically insignificant. Thus, similar to white households with no school-age children, our results suggest that nonwhite households with no school-age children are also unresponsive to the share of minority students that attend their local public schools.

District Level Results

The results reported in Tables 2 and 3 are based on a school-level measure of minority concentration. Our decision to focus on schools rather than districts was motivated by the fact that parents can exercise Tiebout choice not only by choosing a particular school district but by choosing a school within a district.[17] Thus, to capture the equilibrium effect of minority concentration, we wanted to

[17] Epple and Romano (2003) provide evidence from Los Angeles county that suggest school-level sorting may be just as important, if not more important, than district-level sorting. Similarly, Urquiola (2005) and Rothstein (2004) both find evidence of significant racial stratification across neighborhood schools.

allow for as much sorting as possible. Nevertheless, given the nature of our survey data, there is a potential drawback to focusing on schools rather than districts. Because we do not know the actual school a respondent's child attends, we are forced to assume that a respondent's child attends the school that is closest to the centroid of the zip code in which the respondent lives. As a result, to the extent that this is not an accurate match, our measure of school minority concentration is prone to measurement error. Thus, there is an inherent tradeoff when choosing between measuring minority concentration at the school or district level. On the one hand, school-level measures of minority concentration most likely provide a better indication of the amount of racial stratification that already exists in the public sector. On the other hand, district-level measures of minority concentration are less prone to measurement error resulting from incorrectly assigning respondents to neighborhood schools.

In light of this tradeoff, we also estimated several specifications of our baseline model that measure minority concentration at the district level rather than the school-level.[18] Results based on the sample that includes respondents that live in a zip code where at least 75% of the zip code's population resides within the boundaries of a single school district are reported in Table 4. Column 1 of Table 4 presents results for the sample of white households with children in public school. Columns 2 through 4 present results for the sample of nonwhite households with children in public school, white households with no school-age children and nonwhite households with no school-age children respectively. In general the district-level results reported in Table 4 mirror the school-level results reported in Table 3. For white households with children in public school the coefficient on district-level minority concentration is positive, statistically significant, and slightly larger in magnitude than the coefficient obtained using the school-level measure of minority concentration. Furthermore, similar to the school-level results, minority concentration appears to have little to no effect on the voting behavior of any group other than white households with children in public school. Thus, the district-level results support our earlier conclusion that universal vouchers are likely to lead to increased white flight from public schools.[19]

[18] In cases where a zip code crossed district boundaries, we used a weighted average of the fraction of nonwhite students in each district. Specifically, for each district within a zip code, we weighted the fraction of nonwhite students in that district by the fraction of the zip code's population residing in that district.

[19] For the sample of white households with children in public school, we also estimated the same specifications reported in Table 2A of the appendix using district-level minority concentration rather than school-level minority concentration. Specifically, we estimated specifications of the baseline model that included the entire sample of 743 white households with children in public school, and specifications that limited the sample to include only those respondents that lived in a zip code where at least 65%, 75%, 85%, or 95% of the population resided within the boundaries of a single district. The results obtained were qualitatively and quantitatively similar to those reported in Table 2A except for the fact that the coefficient on minority concentration was slightly larger in magnitude and statistically significant at the 5% level or better in all specifications. Results are available upon request.

Table 4

Estimated Coefficients using District-Level Minority Concentration

Regression Specification:	(1) White with Children	(2) Nonwhite with Children	(3) White no Children	(4) Nonwhite no Children
District Minority	0.262**	-0.053	-0.059	0.180*
	(0.101)	(0.122)	(0.049)	(0.103)
Homeowner	-0.035	-0.017	-0.067**	-0.100**
	(0.044)	(0.039)	(0.021)	(0.043)
Conservative	0.278**	0.161**	0.291**	0.132**
	(0.039)	(0.039)	(0.022)	(0.036)
Female	-0.05	-0.015	-0.027	-0.004
	(0.045)	(0.052)	(0.026)	(0.042)
Rural	0.122	0.558**	-0.051	0.123
	(0.138)	(0.165)	(0.077)	(0.253)
Observations	672	554	2033	761
R-squared	0.16	0.1	0.11	0.08

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

Sorting bias

The results presented thus far capture the equilibrium effect of school minority concentration which, as discussed in Section II, includes both a direct effect of school racial composition on support for the voucher and the bias due to sorting. We have argued that this is the appropriate measure if one wishes to predict the impact of universal vouchers on racial segregation. However, for comparison with previous studies of white flight, and to add insight about household preferences, it is also informative to assess the sign and magnitude of the sorting bias. In order to do this, we can use an instrumental variables approach to isolate the direct effect. An effective instrument will be one that is correlated with minority concentration in the school but unrelated to a voter's preferences for choosing that particular school or to their preferences for vouchers. If we assume that a household's decision to move to a particular area of the state is exogenously determined, then following Dustmann and Preston (2001), we can use the minority concentration in the larger geographic area as an instrument for school minority concentration. For example, it is common to assume that people are drawn to a particular city or metropolitan area because of a job or family, and then choose where to live within that area based on preferences for schools and neighborhood characteristics. As discussed by Dustmann and Preston, the IV estimates may still be biased if the decision to live in the larger area is not completely exogenous (though the bias is

Table 5

Instrumental Variables Estimation

Variable	O.L.S	I.V.
Minority	0.246**	0.485*
	(0.108)	(0.252)
Homeowner	-0.034	-0.021
	(0.044)	(0.044)
Conservative	0.278**	0.280**
	(0.040)	(0.039)
Female	-0.056	-0.06
	(0.040)	(0.037)
Rural	0.143**	0.177**
	(0.067)	(0.079)
Observations	672	672
Hansen J Statistic p-value = 0.16		
C Statistic for exogeneity of 20 mile radius instrument p-value = 0.24		

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

generally smaller than the bias in the OLS estimates). We would thus want to use a geographic area that is sufficiently large. However, even if the instrument were strictly exogenous, the IV estimates will only reduce the sorting bias to the extent that the school minority concentration is correlated with the area minority concentration. Thus, we would want to use an area around the school that is sufficiently small. Keeping this trade-off in mind, we use three instruments that reflect areas of varying sizes to assess the size and direction of the sorting bias. Specifically, we use as instruments the average percent minority in all other schools within a 20, 25, and 30 mile radius around the household's chosen school.

Table 5 shows the results of the IV estimation. For comparison purposes the first column repeats the OLS results from our baseline specification in Table 2 while the second column reports the instrumental variables results. Note that the point estimate on minority concentration in the I.V. results is substantially larger in magnitude than the OLS estimate. This is consistent with the discussion in Section II which suggested that if white households with strong preferences for racial homogeneity have already sorted into more homogeneous schools, the OLS. estimate will be biased downwards relative to the true causal effect.

Note that because we have only one endogenous variable (minority concentration) and three instruments, we can use Hansen's J-statistic of overidentifying restrictions to test the validity of our instruments. Under the null hypothesis that the instruments are valid (i.e. orthogonal to the error term) the statistic is distributed chi-square with two degrees of freedom. The p-value associated with the test is

0.16, implying the instruments pass the overidentification test at standard significance levels. Using a "difference in Sargan" statistic or a C-statistic, we can also test the validity of a subset of instruments. In the current context the instrument that is most questionable is the average percent minority in all other schools within a 20 radius (i.e. the instrument based on the smallest geographical area). Under the null hypothesis that the instrument is valid, the statistic is distributed chi-square with one degree of freedom; the p-value is 0.24, and thus the instrument appears valid.

Omitted school characteristics

The results reported thus far lead naturally to the question of whether minority concentration is directly driving the voting behavior of white households or whether the voting behavior of those households is simply being driven by some omitted factor that is correlated with minority concentration. For example, if schools with high concentrations of minority students tend to have lower average test scores, our minority concentration variable could simply be picking up the fact that white families are more likely to support the voucher if their child attends a low-performing school. From the perspective of whether or not vouchers will lead to increased racial segregation, this distinction is unimportant. But to policymakers looking for ways to mitigate these stratification effects, uncovering the underlying preferences may yield useful insights. To examine this, we added two additional variables to our baseline model. The first variable is the 2000 Academic Performance Index (API) of high schools in our sample. The API is a score, ranging from 200 to 1000, that reflects the overall performance of students in a particular school on the Stanford 9, a nationally-normed test. The second variable is the fraction of students that are limited English proficient at each of the high schools in our sample. In California, the largest minority group, by far, is students are of Hispanic decent and a large percentage of these students are limited English proficient. Thus, minority concentration in California is likely to be highly correlated with the fraction of students that are limited English proficient. Furthermore, Betts and Fairlie (2003) find evidence that white families are significantly more likely to enroll their children in private school if they live in areas with high concentrations of immigrant children who speak a language other than English at home. We therefore include the fraction of students that are limited English proficient to examine whether "white flight" is likely to be particularly pronounced if a large fraction of minority students speak a language other than English.

Table 6

Estimated Coefficients for White Households with Children in Public School:

Expanded Specifications

Regression Specification:	(1)	(2)	(3)	(4)
Minority	0.246**	0.081	0.072	0.001
	(0.108)	(0.154)	(0.145)	(0.166)
Homeowner	-0.034	-0.037	-0.029	-0.033
	(0.044)	(0.045)	(0.044)	(0.045)
Conservative	0.278**	0.276**	0.276**	0.276**
	(0.040)	(0.040)	(0.040)	(0.040)
Female	-0.056	-0.057	-0.059	-0.058
	(0.040)	(0.040)	(0.040)	(0.041)
Rural	0.143**	0.142**	0.143**	0.143**
	(0.067)	(0.065)	(0.066)	(0.065)
Log API		-0.321	…	-0.215
		(0.224)	…	(0.238)
Fraction LEP			0.526*	0.406
			(0.277)	(0.304)
Observations	672	668	672	668
R-squared	0.16	0.16	0.16	0.16

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

Results based on the expanded set of explanatory variables are reported in Table 6.[20] For comparison purposes, column 1 of Table 6 reports the results from our original specification (column 1 of Table 2). Column 2 reports results from an expanded specification that also includes the Academic Performance Index. The inclusion of this variable causes the coefficient on minority concentration to decline rather sharply from 0.246 to 0.081 and become statistically insignificant.[21] Column 3 reports results from an expanded specification that replaces test scores with the fraction of students that are limited English proficient. Once again, adding this variable to the baseline model causes the point estimate on the minority concentration variable to decline sharply from 0.246 to 0.072 and become statistically insignificant. The final column of Table 6 reports results from an expanded specification that includes both the API index and the fraction of limited English proficient students. Including both of these variables causes the coefficient on minority concentration to decline even further to 0.001. Overall,

[20] The results are based on the sample that includes respondents that lived in a zip code where at least 75% of the population resided within the boundaries of a single school district.

[21] The fact that neither the coefficient on minority concentration nor the coefficient on the Academic Performance Index is statistically significant is not particularly surprising given the high correlation between the two variables. In our sample, that correlation is -0.78. If the model is estimated with the API included but minority concentration excluded, the coefficient on API remains negative and is statistically significant at the 5% level.

the results reported in Table 6 suggest that a large portion of the "white flight" uncovered in Table 2 can be attributed to factors other than school racial composition. In particular, white households appear to be responding not to racial composition of a school per se, but rather to factors that tend to be highly correlated with racial composition, such as the English proficiency of students and a school's overall academic performance.

IV. Analysis Using Aggregate Vote Returns

As noted previously, our primary analysis relies on polling data collected prior to the actual vote on the voucher initiative and thus represents stated versus revealed preferences for school vouchers. That fact raises the potential concern that our polling data may not accurately represent voting behavior. For example, in the case of school vouchers, a voter's response to a poll may be heavily influenced by his or her political ideology while their actual vote may be more strongly affected by economic factors, such as the net fiscal cost of the voucher program. In the present context, concerns over the stated versus revealed nature of our data may be particularly important given the disparity between the fraction of survey respondents that reported they would support the voucher (46%) and the fraction of voters that actually supported the voucher initiative in November (30%). Ideally, we would like to base our analysis on data from exit polls or actual voting data that was linked to detailed individual characteristics and residential location information. Unfortunately, such data are not available.

Nevertheless, while we do not have data on individual voting behavior, we do have block group level data on the fraction of voters that supported the initiative and their characteristics, data that provides us with the opportunity to conduct a robustness check of the results reported in Section III. To illustrate how we utilize block group-level vote tallies, consider dividing the voters within block groups into four mutually exclusive groups: white households with school-age children, nonwhite households with school-age children, white households without school-age children, and nonwhite households without school-age children. Specifically, let w denote whites, nw denote nonwhites and ' denote households with no school-age children. We specify four separate equations for the fraction of yes votes in a block group, one equation for each of the four groups. By aggregating the linear probability model described in equation (1) up to the block group level, the equation for each group k in block group j is:

$$y_{js}^{k} = \beta_0^k + \beta_1^k M_s + P_{js}^k + \varepsilon_{js}^k, \qquad (7)$$

where $k = [w, w', nw, nw']$, y_{js}^k denotes the fraction of yes votes on the voucher initiative for voters located in block group j and school attendance area s, M_s is the share of minority students at school s, P_{js}^k is the mean unobserved preference for racial homogeneity for voters of type k located in block

group j (with an expectation of $E[P_{is} \mid j,k]$) and ε_{js}^{k} is a mean zero average idiosyncratic error term for voters of type k located in block group j.

Since the proportion of yes votes cast by all four groups must sum to unity, we can express the total proportion of yes votes at the block group level as:

$$y_{js} = \pi_{js}^{w} \cdot y_{js}^{w} + \pi_{js}^{nw} \cdot y_{js}^{nw} + \pi_{js}^{w'} \cdot y_{js}^{w'} + \pi_{js}^{nw'} \cdot y_{js}^{nw'}, \tag{8}$$

where π_{js}^{k} is the fraction of households in group k. Using equation (7), equation (8) can be expressed as:

$$\begin{aligned} y_{js} = {} & \beta_0^{w}\pi_{js}^{w} + \beta_1^{w} M_s \cdot \pi_{js}^{w} + \beta_0^{nw}\pi_{js}^{nw} + \beta_1^{nw} M_s \cdot \pi_{js}^{nw} + \beta_0^{w'}\pi_{js}^{w'} \\ & + \beta_1^{w'} M_s \cdot \pi_{js}^{w'} + \beta_0^{nw'}\pi_{js}^{nw'} + \beta_1^{nw'} M_s \cdot \pi_{js}^{nw'} + \eta_{js} \end{aligned}, \tag{9}$$

where $\eta_{js} = P_{js}^{w}\pi_{js}^{w} + P_{js}^{nw}\pi_{js}^{nw} + P_{js}^{w'}\pi_{js}^{w'} + P_{js}^{nw'}\pi_{js}^{nw'} + \varepsilon_{js}$ and ε_{js} is the across-group weighted sums of the four group-specific errors terms, ε_{js}^{k}. Thus, η_{js} contains both an idiosyncratic error term and the group-specific unobservable preference components weighted by the fraction of voters in each group. Equation (9) can be estimated using block group-level vote returns and the characteristics of voters in each block group. The parameters of primary interest are β_1^{w} and β_1^{nw}. Specifically, β_1^{w} measures how white households with children respond to minority concentration while β_1^{nw} measures how nonwhite households with children respond to minority concentration.

We should note that our analysis using aggregate returns is simply presented to provide additional support for the results reported in Section III and does not represent strong evidence for an equilibrium effect of vouchers based on the aggregate data alone. By estimating the model of individual voting behavior separately by minority status, we were able to focus on the effect of school racial composition for white and nonwhite households separately. In an aggregate model of voting behavior, however, the demographic composition of the block group is used to capture the relationship between minority status and voting, and the expected value of parameter estimates arising from OLS are quite complex. For example, the expectation for aggregate data estimates associated with white households with children is:

$$E[\beta_1^{w,OLS} \mid X] = \beta_1^{w} + I2(X'X)^{-1}E[\eta_{js}, X], \tag{10}$$

where $X = (\pi_{js}^{w} \;\; \pi_{js}^{w}M_s \;\; \pi_{js}^{nw} \;\; \pi_{js}^{nw}M_s \;\; \pi_{js}^{w'} \;\; \pi_{js}^{w'}M_s \;\; \pi_{js}^{nw'} \;\; \pi_{js}^{nw'}M_s)'$ and

$I2 = (0\ \ 1\ \ 0\ \ 0\ \ 0\ \ 0\ \ 0\ \ 0)$. Even if $\pi_{js}^{w} M_s$ were uncorrelated with any other regressor, which is clearly untrue since the other population shares are negatively correlated with π_{js}^{w}, the second term in equation (10) will depend upon the correlation between $\pi_{js}^{w} M_s$ and the unobservable, η_{js}. The unobservable contains terms representing the average preferences of all groups in the neighborhood, and these terms are correlated with both π_{js}^{w} and M_s due to household sorting across neighborhoods and schools. If there were no sorting across neighborhoods within school attendance areas, equation (3) could be used to simplify the expression, but the expression would still depend upon α_0^k and α_1^k for all k not just α_1^w as in equation (6) for the survey data.

Data for Aggregate Analysis

The dependent variable in our analysis of aggregate vote returns is the fraction of voters in a block group that supported the voucher initiative. Block group-level data on vote outcomes for Proposition 38 were obtained from the Statewide Database, maintained by the Institute of Governmental Studies at the University of California, Berkeley. The database contains aggregate vote outcomes and voter registration information, for all statewide primary and general elections held in California since 1990.[22]

We use voter registration data from the Statewide Database and data from the 2000 Census to construct block group-level explanatory variables that match as closely as possible the explanatory variables we used in our analysis of the PPIC survey data. Those variables are: the fraction of the population that are homeowners, the fraction of voters that are registered Republicans, and the fraction of the voting age population (those individuals 18 years of age or older) that are female. Consistent with equation (9) we also include the fraction of households that are white with school-age children, the fraction of households that are nonwhite with school-age children, the fraction of households that are white with no school-age children, and the fraction of households that are nonwhite with no school-age children. To account for the fact that families with children in private school would have directly benefited from the voucher, we also include one additional variable in our analysis of aggregate vote returns, namely the fraction of K-12 students that are enrolled in private school. Following our analysis using the survey data we also include an indicator variable for whether a school is located in a rural area and a set of Metropolitan Statistical Area (MSA) fixed effects to control for unobservable regional variation in support for school vouchers.

[22] Detailed information on how the Statewide Database is constructed can be found at: http://swdb.berkeley.edu.

To assign public school characteristics to individual block groups, we used the following procedure. We first matched Census blocks to school districts and then to the closest public high school within the appropriate district. We then created pseudo-block groups by aggregating up all the blocks within a block group that matched to the same school. For 82% of our sample, these pseudo-block groups are identical to the block group. That is, all blocks within a block group match to the exact same school. In cases where a block group contains more than one pseudo-block group, we aggregated block-level data on vote outcomes and the number of voters that were registered Republicans up to the pseudo-block group level.[23] For variables that were only available at the block group level (e.g. fraction of students attending private school) we assigned the same values of the variable to all pseudo-block groups located in the same block group. Our final sample consists of 25,712 block groups or pseudo-block groups that are matched to 847 public high schools. Means and standard deviations for the variables used in our analysis of aggregate vote returns are reported in Table 7A of the appendix.

Results

Table 7 shows the results from the estimation of equation (9).[24] The equation is estimated using a weighted least squares approach, where the weights correspond to the total number of voters in each block group. In addition, the standard errors reported in Table 7 are adjusted for clustering of the data at the school level. The coefficients of primary interest are those on the interaction terms between minority concentration and the fractions of white and non-white households with school-age children.
The coefficient on the interaction between minority concentration and the fraction of white households with school-age children is positive and statistically significant at the 5% level, indicating that these households are more likely to support the voucher if their child attends a school with a large concentration of minority students. That finding complements the survey results reported in Table 2. Furthermore, the point estimate on minority concentration for white households with school-age children is 0.20, which is quite close to the estimate obtained using the survey data of 0.24.

As shown in Table 7, the coefficient on the interaction between minority concentration and the fraction of nonwhite households with school-age children is statistically insignificant, negative, and quite small in magnitude, suggesting that the voting behavior of minority households with children is unresponsive to the share of minority students their children are exposed to. Once again that finding

[23] Block groups containing more than one pseudo-block group arise primarily because some block groups cross district boundaries. In those cases, Census blocks within a block group are partitioned according to which school district they belong to.
[24] Note that we exclude the level variable (non-interaction variable) for the fraction of nonwhite households with no children in the analysis. Thus, the coefficients on the fraction of white households with children, the fraction of nonwhite households with children and the fraction of white households with no children reported at the bottom of Table 7 are all relative to this omitted group.

Table 7

Estimated Coefficients Using Aggregate Vote Returns

Variable	Coefficient	Standard Error
Minority*(White with Children)	0.204**	(0.038)
Minority*(NonWhite with Children)	- 0.039	(0.054)
Minority*(White No Children)	0.025**	(0.013)
Minority*(NonWhite No Children)	- 0.021	(0.047)
Fraction Homeowner	- 0.032**	(0.003)
Fraction Republican	0.397**	(0.012)
Fraction Female	- 0.103**	(0.025)
Fraction Private	0.024**	(0.004)
Rural	0.058**	(0.006)
Fraction White with Children	- 0.096**	(0.041)
Fracation NonWhite with Children	0.059	(0.084)
Fractoin White No Children	- 0.077*	(0.043)
Observations	25,712	
R-squared	0.72	

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

complements the results obtained using the PPIC survey, reported in Table 3. It is also important to point out that the coefficient on the interaction term between school minority concentration and the fraction of white households *without* school-age children, although statistically significant, is much smaller than for white households *with* children. This is also generally consistent with the results reported in Table 3, which revealed that white households with no school-age children were unresponsive to minority concentration. Thus, the results reported in Table 7 provide further support for our contention that among households with children in more-integrated schools, support for the voucher largely reflects a desire to use the voucher.

The coefficients on the other control variables reported in Table 7 are also generally consistent with those obtained using the survey data. For example, similar to the results reported in Tables 2 and 3, the coefficients on fraction homeowner and fraction female are negative in Table 7. Furthermore, as in

the survey data results, political ideology (as measured by the fraction of registered Republicans) has a strong positive effect on the propensity of voters to support the voucher initiative.

As an additional robustness check of the survey results reported in Section III, we also estimated several expanded versions of equation (9). Specifically, we added two additional school-level variables to our model; the 2000 Academic Performance Index (API) of high schools and the fraction of students that are limited English proficient (LEP). Following the logic behind equation (9), these variables enter our model as interaction terms. In particular, each of these variables is interacted with the fraction of households that are white with school-age children, the fraction of households that are nonwhite with school age children, the fraction of households that are white without school-age children and the fraction of households that are nonwhite without school-age children.

Results based on the expanded set of explanatory variables are reported in Table 8. Since the inclusion of these additional variables had little impact on the other variables in our model, Table 8 reports only the coefficients on the interaction terms between the three school-level variables (fraction minority, API, and fraction LEP) and the four ethnic/presence of school-age children variables. The first two columns of Table 8 report results when the Academic Performance Index is added to the model, while the last two columns report results with the fraction of students that are limited English proficient is added.

As with the survey data, adding these variables to the model causes the coefficient on the interaction between minority concentration and the fraction of white households with school-age children to decline sharply in magnitude. When the fraction of LEP students is added to the specification, the coefficient for white households with school-age children declines from 0.204 (Table 7) to 0.066 and when test scores are added, the coefficient actually switches sign, becoming negative. Furthermore, note that in the specification that includes the Academic Performance Index, the coefficient on the interaction between the API and the fraction of white households with school-age children is negative and statistically significant, indicating that these households are less likely to support school vouchers if their child attends a high-performing public school. Similarly, in the expanded specification that includes the fraction of LEP students, the coefficient on the interaction between LEP and the fraction of white households with school-age children is positive and statistically significant, indicating that these households are more likely to support school vouchers if their child attends a school with a high percentage of limited English proficient students. Thus, consistent with the survey results reported in Table 6, the results using the aggregate data suggest that white households with children appear to be responding not to the racial composition of a school per se, but rather to factors that tend to be highly correlated with racial composition, such as a school's overall academic performance.

Table 8

Estimated Coefficients Using Aggregate Vote Returns

Expanded Specifications

Variable	Including Test Scores		Including Fraction LEP	
	Coefficient	Standard Error	Coefficient	Standard Error
Minority*(White with Children)	- 0.119**	(0.052)	0.066	(0.055)
Minority*(Nonwhite with Children)	- 0.028	(0.059)	0.003	(0.057)
Minority*(White No Children)	- 0.021	(0.021)	0.051**	(0.019)
Minority*(Nonwhite No Children)	0.064	(0.045)	- 0.061	(0.049)
Log API*(White with Children)	- 0.745**	(0.125)	…	…
Log API*(Nonwhite with Children)	0.049	(0.087)	…	…
Log API*(White No Children)	- 0.061	(0.049)	…	…
Log API*(Nonwhite No Children)	0.121**	(0.061)	…	…
LEP*(White with Children)	…	…	0.418**	(0.166)
LEP*(Nonwhite with Children)	…	…	- 0.156**	(0.070)
LEP*(White No Children)	…	…	- 0.078	(0.051)
LEP*(Nonwhite No Children)	…	…	0.126*	(0.068)
Observations	25,445		25,712	
R-squared	0.75		0.72	

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

V. Conclusion

The debate over school vouchers has many dimensions and will no doubt continue into the foreseeable future. In this paper, we provide evidence on one consequence of universal vouchers, namely, their impact on racial segregation across schools. Although previous studies of white flight have provided insights about whether race is a factor in the school choice decisions of white households, we suggest that those studies have either under- or over-estimated the impact that a universal voucher program is likely to have on racial segregation. In our empirical work, we find that among white households with children, support for vouchers increases with the proportion of minority students in the

local public schools. This is not true of non-white households, suggesting that if a voucher program were implemented, schools that are currently more integrated would likely lose more white students and racial stratification would increase. At the same time, our results indicate that households have already sorted to a significant degree, so that the *additional* effect on segregation would not be as large as indicated by causal estimates of the effect of school minority concentration. We also find evidence that it is not race, per se, that is the primary motivation, but rather school characteristics that are highly correlated with race, including test scores and the proportion of students that are limited English proficient.

Our study faces a number of limitations. The ideal data for this sort of analysis would be individual voting data or exit polls. Instead, the PPIC survey data represent stated (versus revealed) preferences for school vouchers, while the voting tallies from the Statewide Database represent aggregate vote outcomes. Neither is perfect, though the extent of bias in each is unknown. However, the fact that our results are fairly consistent with both data sources gives us more confidence in our general conclusions.

It is also important to point out that extrapolation to other states depends on the amount of sorting that has already occurred in those states. On the one hand, California's size and diversity may make it easier for households to sort through residential location choice; if so, the equilibrium effect will appear smaller and the effect of vouchers on racial segregation will be larger in states where such sorting is more difficult. On the other hand, California has experienced a large increase in non-white immigration in the last decade, so it may be more difficult for white Californians to sort into a school with their most-preferred racial mix. In that case, the equilibrium effect of racial concentration on voucher support in other states would be smaller, not larger, than in California.

Finally, our results give some indication that it is not race itself that is driving voter behavior but other school characteristics, such as school quality. This suggests that policymakers could mitigate the effects of vouchers on racial segregation by weakening the link between school racial composition and school quality, although that itself certainly is a problem with no easy solutions. Our results also raise concerns about more limited voucher programs, particularly those targeted to low-performing school. These smaller programs may lead to racial segregation just as severe as a universal voucher, as the targeted schools are precisely the schools that white households are most likely to want to leave.

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Appendix

Table 2A

Coefficient Estimates for White Households with School-Age Children:

Alternative Sample Restrictions

Regression Specification:	(1)	(2)	(3)	(4)	(5)
Minority	0.151	0.198*	0.246**	0.275**	0.304**
	(0.101)	(0.107)	(0.108)	(0.110)	(0.121)
Homeowner	-0.033	-0.031	-0.034	-0.036	-0.008
	(0.042)	(0.044)	(0.044)	(0.045)	(0.048)
Conservative	0.255**	0.262**	0.278**	0.291**	0.266**
	(0.039)	(0.040)	(0.040)	(0.042)	(0.044)
Female	-0.072*	-0.069*	-0.056	-0.069*	-0.074*
	(0.038)	(0.039)	(0.040)	(0.041)	(0.044)
Rural	0.103	0.128*	0.143**	0.152**	0.137*
	(0.069)	(0.069)	(0.067)	(0.066)	(0.079)
Observations	743	698	672	625	561
R-squared	0.13	0.15	0.16	0.17	0.16

Notes: (1) Robust, clustered standard errors in brackets, (2) all specifications include MSA fixed effects, (3) * significant at 10%, ** significant at 5%

Table 7A

Summary Statistics for Aggregate Vote Returns

Variable	Mean	Standard Deviation
Fraction Voting Yes	0.29	0.07
Fraction H.H With Children White	0.14	0.09
Fraction H.H. With Children Nonwhite	0.13	0.12
Fraction H.H. No Children White	0.50	0.21
Fraction H.H. No Children Nonwhite	0.22	0.16
Fraction Homeowner	0.65	0.26
Fraction Republican	0.36	0.15
Fraction Female	0.52	0.03
Fraction Private	0.14	0.15
School Minority Concentration	0.55	0.27
Rural	0.07	0.15
Observations	25,712	
Schools	847	