DOCUMENT RESUME

ED 425 520 EA 029 485

AUTHOR Glazerman, Steven M.

TITLE School Quality and Social Stratification: The Determinants and Consequences of Parental School Choice.

PUB DATE 1998-04-00
NOTE 36p.; Paper presented at the Annual Meeting of the American Educational Research Association (San Diego, CA, April 13-17, 1998).
PUB TYPE Reports - Research (143) -- Speeches/Meeting Papers (150)
EDRS PRICE MF01/PC02 Plus Postage.
DESCRIPTORS *Educational Quality; Elementary Secondary Education; Models; *Parent Influence; Parent School Relationship; *Public Schools; *School Choice; School Segregation
IDENTIFIERS Minneapolis Public Schools MN

ABSTRACT

Those who favor expansion of consumer choice in education claim that competition would force schools to improve. Critics claim that it would sort students by race and class. A competitive market will provide what consumers demand, yet neither side has empirical evidence on such consumer preferences to back up their claims. This paper offers such evidence. This paper estimates a conditional logit model using data from a public school choice program in Minneapolis, Minnesota, in order to infer how families trade off the convenience of a shorter commute with school quality and peer group characteristics. The evidence suggests that consumer choice alone would not raise schools' academic performance. Parents in Minneapolis were not more likely to choose schools with high test scores or greater value added. Rather they preferred schools relatively close to home and ones where they were better represented ethnically and racially. The only discernable test score effect was one where families sought a match between their own child's ability and the mean ability level of similar students at the prospective school. Simulations suggest that expanding choice could ultimately lead to severe, but not total segregation by race and ethnicity. Unrestricted choice could present a tradeoff between consumer satisfaction today and increased racial segregation in the future. (Author/RJM)

School Quality and Social Stratification: The Determinants and Consequences of Parental School Choice

Paper presented at the American Educational Research Association Annual Meeting, San Diego, CA. April 13, 1998

Steven Glazerman
University of Chicago

ABSTRACT [1]

Those who favor expansion of consumer choice in education claim that competition would force schools to improve. Critics claim that it would sort students by race and class. A competitive market will provide what consumers demand, yet neither side has empirical evidence on such consumer preferences to back up their claims. Here we offer such evidence. This paper estimates a conditional logit model using data from a public school choice program in Minneapolis, MN, in order to infer how families trade off the convenience of a shorter commute with school quality and peer group characteristics. The evidence suggests that consumer choice alone would not raise schools' academic performance.

Parents in Minneapolis were not more likely to choose schools with high test scores or greater value added. Rather they preferred schools relatively close to home and ones where they were better represented ethnically and racially. The only discernable test score effect was one where families sought a match between their own child's ability and the mean ability level of similar students at the prospective school. Simulations suggest that expanding choice could ultimately lead to severe, but not total segregation by race and ethnicity. Unrestricted choice could present a tradeoff between consumer satisfaction today and increased racial segregation in the future.

This study is part of ongoing research conducted jointly with Robert H. Meyer, who provided extensive advice and guidance. The author also thanks Tom DeLeire, James Heckman, Derek Neal, and Kenneth Wong for helpful comments. Denny Lander, Juanita Huerth, Zib Hinz, Sherri Belton-Hardeman, and many others from the Minneapolis Public Schools were extremely patient and cooperative in explaining the data for this project and the context of choice in Minneapolis. James Heckman and the Center for Social Program Evaluation generously shared their computing resources. The Harris School and the Spencer Foundation provided financial support. The author is solely responsible for any errors.

1. Introduction

1.1. Why study family choice behavior?

Many major reforms in education ranging from voluntary desegregation plans to vouchers and charter schools have some element of parental school choice. Even policies that seem unrelated to choice, like class size reduction or the local adoption of a national curriculum, influence the type of families that decide to live in a given community and attend local public schools. Thus with or without market-type school reforms, parental choice behavior determines the composition of schools. To evaluate and predict the consequences of these policies one must better understand consumer preferences. In particular, we want to know how responsive families are to variation in different aspects of school quality and how they trade off different attributes of the school with the convenience of a shorter walk or bus ride.

"School quality" can include everything from the performance of the teachers to the characteristics of the peer group as well as amenities which do little to enhance human capital. Peer group characteristics that could influence consumer choice include not only the ability distribution at the prospective school but the racial, ethnic, or social class composition of the student body. Whichever of these has the strongest influence on consumers is likely to be provided in a system of pro-competitive, market-oriented choice. Critics of choice policies fear that unleashing market forces will lead schools to sort students by race and class. For these reasons, privately held consumer preferences have important consequences for public policy. Whether increasing consumer choice would foster or undermine the goals of school improvement and social integration is an empirical question, which this study addresses.

1.2. Traditional approach: questionnaires

There are essentially two approaches to the empirical study of consumer preferences in education. The most common approach relies on questionnaires and interviews of parents who took part in some type of school choice. Bridge and Blackman (1978) interviewed parents in California's Alum Rock District, the site of an early and innovative choice experiment. The authors reported that 70 percent of respondents cited "location of school" as a major decision factor, while curricular and programmatic features were less important. Yet a similar study (Nault and Uchitelle, 1982) conducted in a suburban community with optional attendance zones reached nearly the opposite conclusion. They found that parents preferred strictly academic and curricular qualities to "convenience of transportation", "physical facilities", and "similarity of children's backgrounds."

While the disagreement in findings may reflect differences in the sampled populations, it raises suspicions that self-reported attitudes are sensitive to question wording, question format, or interviewer bias. Dozens of parent surveys since then have not yielded a consensus on the determinants of choice. See for example Williams, et al (1983); Petch (1986); Strobert (1990); Baird (1990); Coldren and Boulton (1991); and Lee, Croninger, and Smith (1994).

Imprecision is also a problem with this approach. Too often survey research fails to quantify the rates at which consumers trade off different dimensions of school quality with each other and with commuting costs. Quantitative stimates of these relative preference weights along with standard errors that characterize the researcher's uncertainty could be useful for policy making and planning. Furthermore, direct questionnaires face a difficult challenge eliciting honest information about politically sensitive factors like race preferences.

1.3. Another approach: revealed preference

An alternative approach to studying school choice behavior analyzes actual consumer choices to infer the parameters that describe tastes. In other words, this research departs from the prevailing survey methodology by relying on revealed, rather than self-reported, preferences.

As we shall see in the next section, this study models behavior at the individual level. We pose the consumer's problem as one of choosing one school from a well-defined menu of alternatives. To empirically estimate this model we rely on a set of tools developed by economists, random utility models of discrete choice, particularly McFadden's (1974) conditional logit. Applied economists have used the conditional logit to study a wide range of phenomena, including choice of urban transportation mode (Ben-Akiva and Lerman, 1985), durable goods purchase decisions (Cragg, 1971), and occupational selection (Boskin, 1974).

Applications to education have only considered post-secondary school choice (Punj and Staelin, 1978; Kohn, Manski, and Mundel, 1979; Manski and Wise, 1983; Cameron and Heckman, 1994; Weiler, 1996). Discrete choice analyses that have considered elementary and secondary school choice (Lankford and Wyckoff, 1992; Goldhaber, 1996) provide a starting point for this analysis. These studies, however, only considered choice of sector, e.g. public or private, and therefore have yet to fully exploit school-specific data as we do in this study. As a consequence, the behavior of families facing a choice among elementary schools is not well understood.

2. Theory

2.1. A model of school-choosing behavior

We model the household's decision of which particular school to attend given that it has settled in a location and chosen the public sector and therefore faces a fixed menu of discrete alternatives. The public sector has open enrollment, which means that a family can choose any of the public schools in the district. We analyze a single period, so supply conditions are fixed. We also assume every household gathers as much information as is optimal and honestly lists its first choice. In future work we hope to expand the inquiry to allow for simultaneous residential choice with sector and school choice as well as strategic behavior, although such research would require considerably more data. It would also be interesting to obtain data for studying supply side behavior.

2.2. Determinants of school choice

We assume choosers select the one school whose attributes combine to produce the most satisfaction, or utility. Based on the past research on choice cited in Section 1 and the author's interviews and intuition, we group the key explanatory variables into four categories: costs, information, school attributes, and peer attributes. Costs are not explicit here. Since there is no tuition we consider the indirect costs of commuting, the value of time spent walking or riding a bus to school.

Information about school alternatives is a concern to many critics of consumer choice in education. The Carnegie Foundation (1992) noted that parents differ in how well informed they are about school differences. The authors argued that low income or non-English speaking families are at a disadvantage. Bridge and Blackman (1978) also raised the concern and documented the lower levels of awareness of the current policy and school alternatives among low socio-economic status (SES) households.

Here we make no particular assumptions about how much parents know. We only claim that information is costly to obtain. In the case of school choice it can mean spending time visiting schools, reading official publications and newspapers, or talking to friends and neighbors. Because this activity can be costly, households gather information up to the point where the marginal costs of doing so outweigh the marginal benefit. This point will be different for different families.[1] Where possible, however, we document and include control variables in the model wherever to describe the state of choosers' information believed to be unique to Minneapolis.

Costs and information influence the school choice decision, but a goal of this research is to learn what consumers judge to be a "good school." Using the empirical model we can determine the relative importance of school attributes -- curriculum, grade configuration, efficiency, and safety -- versus peer group characteristics -- ability, family race or ethnic identity, socio-economic class, or parental involvement. The result has strong implications for the effects of choice on sorting and stratification. Another way of looking at the problem is to test how much parents favor attributes that produce academic achievement relative to "non-academic" factors. This question is central to forecasting the role choice could have on school improvement.

Both school and peer characteristics are school-specific attributes, yet we can also consider their effects relative to some characteristics of the chooser. One example is the effect of average test scores. It is often assumed that schools with higher scoring students are uniformly better. An alternative theory likens the student to a big fish in a small pond, predicting that students might prefer to be in schools with lower ability students to enhance their own status. Yet several school choice studies -- Manski and Wise (1983), Punj and Staelin (1978), and Weiler (1996) -- have estimated some type of relationship between the chooser's own ability and the ability distribution of the student body at the prospective school and found neither of these effects. A fairly universal finding was that students seek a school where the average test score is about equal or slightly higher than their own score. This supports a theory of ability-matching that in a system of open competition could lead to self-segregation by student achievement level. We also explore the possibility of social matching. This refers to behavior where choosers select schools to be with classmates who are like themselves in terms of race, ethnicity, or social class.

If parents take into consideration the traits of the other students when choosing a school, then it is very important that the choice model use the appropriate reference group. One possibility is that parents only care about who is in their child's classroom and they therefore try to anticipate the choices of other students in their cohort. Choices would thus be interdependent and we would need to make additional assumptions to make any meaningful econometric inferences.

Rather than model interdependent choices, a more compelling argument can be made that choosers consider the characteristics of the students currently enrolled in the school when they make their selection. It is much easier for choosers to observe the current cohort than to guess

[1] A useful question might ask how responsive parents are to changes in the cost of information. For example, if a new school accountability system is introduced in which school rankings are created and publicized, how would the policy affect enrollments and the distribution of student types across schools?

the composition of the entering cohort. This leads to a recursive process, where the choices of one cohort influence cohorts that follow. For any given set of choosers we can consider the existing student body composition of each school as exogenous regressors in the conditional logit model.

This simplifies the problem considerably, but two questions remain in defining the reference group. First, a school may span several grades, so parents could base their evaluation of the school on students from one grade, all grades, or some average that might place declining weights on older cohorts. The grade horizon parents use in making their choice decisions can be important for policy because it affects the speed with which the school system would re-adjust to exogenous shocks like policy changes or waves of immigration.

The other open question has to do with the composition of the reference group. It is possible that the entire school population matters. But if choosers anticipate being divided by ability within a school or if they learn about schools through members of their own ethnic group or neighbors, then perhaps the average test scores that matter, for example, are the average scores of people like themselves rather than of the entire student body.

3. Estimation

3.1. The discrete choice problem

A key question is how to define the list of criteria choosers use to select a school, in other words, what makes up the utility function. For estimation we distinguish two types of school attributes. "Fixed" attributes, which we shall denote with a vector Q_j for the j^{th} school, do not depend on individuals. A vector Z_{ij} can be used for "relative" attributes that describe school j relative to the i^{th} individual. For example, distance to any given school is relative because it depends on the chooser's home address. Individual perceptions of fixed attributes can also be considered relative attributes, as can the interaction of fixed attributes with individual-level variables. Such interactions allow the researcher to model heterogeneity in consumer preferences. Let u_j denote unobserved fixed factors and e_{ij} denote unobserved relative factors. Then we assume person i's utility for the j^{th} school is linear in the parameters β and γ, which can be thought of as attribute preference weights:

$$U_{ij} = \beta' Z_{ij} + \gamma' Q_j + u_j + e_{ij} \tag{1}$$

The consumer's problem is to choose a school j such that $U_{ij} > U_{ik}$ for all k in the consumer's choice set J_i. In the random utility formulation this is a stochastic event, so we can select parameters to maximize the likelihood function based on the joint probability of that event being true for each individual $i=1,\ldots,N$. This probability is made tractable by using the assumption of McFadden's conditional logit, that the errors are independent and identically distributed (i.i.d.) draws from a Type I extreme value distribution.[2] Given a specification for

[2] McFadden (1974) also considered an alternative specification, the multinomial probit, where the e_{ij}'s are drawn from a general multivariate normal distribution. The probit model is more flexible, allowing alternatives to be similar to each other in unobserved ways. Hausman and Wise (1978) show how the multinomial probit can be used to model random coefficients, but they point out an enduring problem, which is that the model is computationally very difficult to estimate with more than a few alternatives. Since then several methods have been advanced to overcome this barrier. Lerman and Manski (1981), Supan and Hajivassiliou (1993), Geweke, Keane, and Runkle (1994) have made important contributions to simulation methods for evaluating probabilities expressed as high

(1), the expressions for the selection probability and the likelihood function are straightforward. See McFadden (1974), Hensher and Johnson (1981), Maddala (1983) or Ben-Akiva and Lerman (1985).

3.2. Two stage estimation

Many conditional logit studies ignore the the alternative-specific error component u_j. Estimating (1) directly by maximum likelihood may be inappropriate because u_j induces a positive correlation across choosers who select the same school. It amounts to assuming the composite error term $\epsilon_{ij}=u_j+e_{ij}$ is i.i.d. extreme value. Here we only assume e_{ij} is i.i.d. and we absorb u_j and Q_j into a fixed school effect α_j and estimate the model in two steps.[3] As a first stage one can estimate the following conditional logit model by maximum likelihood.

$$U_{ij} = \beta' Z_{ij} + \alpha_j + e_{ij} \tag{2}$$

Then we can recover γ in a separate stage by regressing the estimated school intercepts $\hat{\alpha}_j$ on Q_j using least squares. The dependent variable would be the true school effect along with estimation error, $\hat{\alpha}=\alpha+\omega$. This leads to a second stage model with two error components.

$$\hat{\alpha}_j = \gamma' Q_j + u_j + \omega_j \tag{3}$$

Ordinary least squares would be inefficient because we know that the estimation error ω is heteroskedastic and correlated across observations. The structure of that correlation is easily estimated, however, assuming the two error components are independent. We estimate equation (3) using Feasible Generalized Least Squares, with the following weighting matrix.

$$S \equiv E[(u+\omega)(u+\omega)'] = \sigma^2 I + \Omega_{\alpha\alpha} \tag{4}$$

A natural estimate for $\Omega_{\alpha\alpha}$ is the covariance matrix corresponding to the school intercepts estimated in the first stage conditional logit. Hanushek (1974) proposed a formula for computing a consistent estimate of the "true" error variance σ^2.

$$\hat{\sigma}^2 = \frac{s^2(J-k)-\sum \omega_j^2 - tr(Q'Q)^{-1}Q'WQ}{(J-k)} \tag{5}$$

where $W = s^2(I-\Omega_{\alpha\alpha})$. The second stage regressor Q is a (J x k) matrix. For s^2 we used the OLS estimate of σ^2.

Many interesting policy questions can be posed as linear restrictions on the set of parameters $(\beta,\gamma)'$. Yet to do inference one needs the full covariance matrix of the estimates across both stages of estimation. The diagonal blocks $\mathrm{Var}(\hat{\beta})$ and $\mathrm{Var}(\hat{\gamma})$ are straightforward to estimate from the first and second stage, respectively. For the off-diagonal block $\mathrm{Cov}(\hat{\beta},\hat{\gamma})$ we note that $\hat{\gamma}$, a GLS estimator, is a linear combination of $\hat{\alpha}$, so $\mathrm{Cov}(\hat{\beta},\hat{\gamma}) = \mathrm{Cov}(\hat{\beta},H\hat{\alpha})$, where $H=(Q'S^{-1}Q')^{-1}Q'S^{-1}$. The covariance term $\mathrm{Cov}(\hat{\beta},\hat{\alpha})$ is estimated as part of the first stage, so

dimension integrals. Cardell (1997) has proposed a conjugate distribition for the extreme value distribution which can be used to estimate variance components. Meyer (1996) has proposed a method for reducing the multinomial probit to a single dimension. The author is currently working on applying these advances to extend the basic econometric model of this paper.

[3] This procedure is similar to Borjas and Sueyoshi (1994). Their model is a binary probit in the first stage and they use a different estimator for the "true" error variance σ^2 in the GLS weighting matrix.

$Cov(\hat{\beta},\hat{\gamma})$ can be computed easily.

4. Data and Description of Choice in Minneapolis[4]

We offer a brief description here of the choice process in Minneapolis as well as of the students who participated and the schools to which they applied. More ample discussions can be found in Glazerman and Meyer (1995).

4.1. Descriptive statistics

The data used in this study come from the Minneapolis Public Schools (MPS), where a district-wide system of public school choice has been in operation in its current form since 1989. We analyze the school selections made by families of 881 children who were enrolling in kindergarten for the fall of 1993. The dataset uses MPS administrative records we merged with data from the 1990 U.S. Census. We also compiled a dataset describing the universe of 50 elementary schools from which these students chose. MPS officials supplied us with the geographic coordinate information allowing us to compute distances from every student to each school. For each household we have their ranking of their top three preferred schools in their open enrollment area as well as the school to which they were assigned.

Table 1 describes the racial and socioeconomic composition of the choosers in our sample. We do not observe family income or education level directly, but we do know the students' eligibility for free or reduced lunch, which is based on income. From the Census tract data we know the median family income level of the student's home neighborhood. It would have been useful to have some measure of the entering students' own preparation for school in terms of aptitude or intelligence measures. Lacking such data we merged in the academic records from when the students reached the first grade in 1995 and took their first standardized achievement test.[5] We use these later scores as proxies for prior ability. It is reasonable to assume that families have private information about how well prepared academically their child is at the time of choosing.

The sample we use to estimate the logit is much smaller than the entire kindergarten cohort, for many reasons noted below. It is important to recognize that the analysis sample is illustrative and interesting, but not necessarily representative. The sample we analyze has higher mean socio-economic status and academic ability and a higher percentage of white students than the rest of the school district.

Each of these families was required to make selections from a menu of between 19 and 26 of the city's 50 public elementary schools, depending on their home address. Many schools had limited attendance areas in order to reduce transportation costs, so that only a few schools were truly citywide schools of choice. Table 2 lists selected descriptive statistics for the schools. The school data come from MPS administrative records and publications. We also linked them to data from the U.S. Census and the Minneapolis Police Department on local area crime

[4] Much of the information from this section comes from Minneapolis Public Schools (MPS) official documents and interviews with MPS staff.

[5] All test scores in this study are from the California Achievement Test, scaled to a national mean and standard deviation of 0 and 1, respectively.

statistics.

It is worth pointing out some features of the racial composition across schools in Minneapolis. To comply with desegregation guidelines, almost all the elementary schools were within 15 percentage points of the proportion of white students in the entire school district: 39 percent, looking at grades K-2 only. Yet the ethnic composition of the remaining students varied greatly from school to school, see Figure 1. Each panel in Figure 1 gives a histogram of schools by the fraction of that particular race or ethnic group represented. African Americans made up as little as 8 percent and as much as 65 percent of the student body in individual schools. In Minneapolis there was a great deal of concentration of non-black minority groups, in particular Hispanics and Native Americans, within a few schools. Only six percent of the elementary schools served 40 percent of the district's Hispanic K-6 population; 480 of the district's 2,206 Native American elementary students -- nearly 22 percent -- were concentrated in just one school.

4.2. The nature of the choice program

School choice in Minneapolis is not the prototypical market-oriented school reform that tries to harness competitive forces to make schools improve. Rather it was born out of two distinct historical events in the early 1970's, school desegregation and an "alternative schools" curricular reform. The school choice program in Minneapolis is *public* school choice and it is mandatory at the kindergarten level. Unlike private school choice, tuition tax credit programs, or school voucher plans, there is a fixed set of schools and no explicit price mechanism for parents to bid into schools. In Minneapolis all parents are required to submit a choice card stating their top three schools. Officially there is no default or neighborhood school to which a student will be automatically assigned if he or she does not submit a preference card.[6]

The most significant feature of public school choice is that, despite site-based management reforms, a central administration runs the schools and assigns the students. This should strongly influence how we interpret the choice data. The nature of the school choice program, in terms of the policy goals it tries to address -- desegregation in this case -- partly determines the information available to parents and hence the basis on which parents distinguish school differences when making their choices. District officials, in fact, told us in interviews that they discourage competition among schools. They generally did not distribute information on school academic performance if it might be used to rank-order schools along a single dimension. Instead, the Parent Welcome Center -- the MPS office charged with information and placement -- published a School Guide and sponsored Kindergarten Fairs, both of which emphasized the uniqueness of each school and how students have specialized learning styles that must be matched with teaching and curricular specialties.

---

[6] While kindergarten is the main entry point for elementary schools, there is an optional school change request process for families who wish to transfer their children. In a typical year, less than 20 percent of the first grade students will request a change, and less than half of those actually change schools. Many of these school changes are made because the students' families moved within the city. Some schools do not house grades K through 6 in one building. Students in those schools must go through the transfer process when they reach the terminal grade for that school, which would be grade 2 or grade 3. These K-2 and K-3 schools have in recent years been phased out by the school district.

Thus the Minneapolis choice system did not aim to promote any standardized measure of schools' academic performance. The reason may be that school placement officials believed that all schools were equally good or that schools were so specialized that each had a comparative advantage in teaching certain types of students. Another possible reason might be the incentives facing district administrators to satisfy capacity constraints, since these officials have little control over expansion or contraction of schools. A way for them to simplify their task of preventing over-subscription or overcrowding would be to convince parents that all schools are good schools or that any given school is the best school in the district for somebody.

Second, Minneapolis has *controlled choice*. This means that the district must limit space in schools even if the school is not full, so long as the school is in danger of exceeding the district-wide fraction of white or non-white students by more than 15 percent. This means that a school will essentially have two different capacity constraints, one for white students and one for "students of color."

The existence of fixed capacity constraints has serious implications for whether we can treat parent school selections as honest revelation of true first choice preferences. If consumers think they might be turned away from a popular school, they may face incentives to misrepresent their preferences, as described in Section 2. In this study we analyze just the first choice data, even though parents were asked to rank their top three schools. We did so because the Minneapolis student assignment mechanism strongly favors honest revelation of first choices and strategic behavior for second and third choices. The actual assignment algorithm used by the school district heavily weights the first choice relative to the second and third choice. If a school is over-subscribed with students' first choices, then lottery numbers determine which students are admitted. The losers automatically go on a waiting list for that school and are not assigned to a second choice school until all other first choices are processed. The chances of admission to a second or third choice are quite slim unless the schools are very unpopular relative to their capacity. This mechanism is sometimes referred to as a "first choice maximizer."

Consistent with the assignment algorithm is the standard advice offered to parents by staff members of the Minneapolis Public Schools' "Parent Welcome Center." Parents were told to list their "dream school" first and "be realistic" about the others. If there were a guarantee of admission to first choice schools this would not be necessary, but in 1993, 18.8 percent of Minneapolis families who applied by the deadline were not initially assigned to their first choice, 22.3 percent of white families and 11.6 percent of nonwhites. Of the 50 schools available in the entire choice program, 38 were over-subscribed by at least one student. Of those 38, 18 were over-subscribed for whites only and 12 were over-subscribed for non-whites only. Eight schools were over-subscribed for both whites and non-whites. Most of the over-subscribed schools rejected only one or two students in the first round. Many students were later assigned to their first choice from a waiting list some time before autumn registration. It would be useful to work out the game-theoretic problem of whether such oversubscription could result when parents are listing their true top preference first. Here we assume it could.

4.3. The choice process

The student assignment process works as follows. Parents of entering kindergarten students spend the winter prior to enrollment learning about their options through site visits, a

Kindergarten Fair, and reading the official catalogue of school descriptions. They then submit preference cards by the deadline at the end of February. The school district assigns students to schools based on the preference cards, randomly assigning students to over-subscribed schools, and notifies parents in April.

This procedure only applies to about 31 percent of the entering cohort. More than a third of the students receive special priority either because they require special services for the disabled or limited English proficient (LEP), or they have an older sibling already enrolled in one of the elementary schools. Such siblings are practically guaranteed admission to the older child's school.[7] Likewise disabled and LEP students are guaranteed a school with their required services.

Also, about 20 percent of the parents enroll their children too late to take advantage of choice. This leaves slack in the system so that, while the late-comers are assigned to the least popular schools, the capacity constraints are not as strictly binding for students who apply on time.

It should be noted again that the sample we analyze is not representative of all Minneapolis public school families. Instead it represents the families who took active part in the choice process in 1993 by returning their preference cards before the filing deadline at the end of February. We call these people "on-time choosers." In the short run we may be most interested in this population because their decisions drive the choice system -- the sibling choices are predetermined and the late comers just fill available space -- but there may be systematic unobserved ways in which the on-time choosers differ in their school preferences from other students. In addition to selecting only the on-time non-siblings we excluded for two reasons the students who changed address after 1992. First, since home address determines eligibility for many schools we could not accurately reconstruct the movers' choice sets. The other reason is that if a student moved, we do not know whether they anticipated the new home or only knew about the old home address when making the school choice. There were 295 movers and other students with missing, incomplete or unusable address data, which is about 25 percent of the on-time choosers. We repeated most of the analysis with and without suspected movers. Movers would appear to prefer more distant schools because we measure distances from the wrong address. The model estimates were quite similar for all coefficients but the distance parameters, which had the expected downward bias when movers were included.

4.4. Choice results

Families in our sample, those who met the deadline in Minneapolis, did seem to exercise their right to choose rather than settle for the default school. Only 26 percent selected the school that was nearest their home. In fact, 66 percent chose a school that was neither their neighborhood school nor closest to their home. More than a quarter of the parents chose schools over two miles away, some choosing schools as far as six miles away. The school district considers anything within one mile as "walking distance," providing free bus transportation beyond that point. While nearly all of the students could have walked to at least one school by this definition, 55 percent chose a more distant school that would qualify them for busing.

[7] The vast majority of the special priority applicants are siblings. In principle one could analyze the decision to exercise sibling preference. The data are not available for this analysis, so we defer the question for now.

Commuting distance is only one of many factors in the school choice decision. One can also look, for instance, at the fraction of choosers who selected the school with the highest math scores in their choice set (8 percent) or the highest percentage of students of their own race/ethnic group (4 percent). There are many attributes that could matter, and the "best" is not obvious; because consumers are making complex tradeoffs, it is difficult to satisfactorily explain the pattern of choice behavior using simple statistics. For this reason we turn to the conditional logit results in the next section.

5. Results

Table 3 lists the parameter estimates from a specification of the conditional logit model that captures the essential features of the choice process outlined in Section 2. It serves as a baseline from which to begin a specification search. As such it represents a subjective judgement ("priors") informed by past research on school choice, interviews with Minneapolis placement staff, and the author's intuition. First we interpret this base model, listed in Table 3, and then test the robustness of the results to alternative specifications and functional forms. Note that in Table 3 each of the coefficients represents the effect of the explanatory variable on a latent dependent variable, utility, which is scaled to have arbitrary mean and variance. The utility values can be substituted into the multinomial logit expression,

$$P_j = \frac{\exp(U_j)}{\sum_{k=1}^{J} \exp(U_k)} \tag{6}$$

to compute the probability of selecting school j from a set of k=1,...,J schools.

5.1. Costs

We model indirect costs using the distance, in miles, from home to school as a proxy for commuting time. MPS students living more than a mile from school were eligible for bus transportation, so we estimate a separate distance effect for schools that are beyond walking distance. Both of the distance effects are interacted with a family income dummy (based on eligibility for free or reduced lunch) and a dummy variable indicating whether the student lives with a single parent or non-parental guardian.[8] It is expected that high income and single parent families would have greater time costs and hence pay a greater penalty in the utility function for choosing distant schools.

The resulting eight distance effects can be calculated by adding the appropriate estimates from the first section of Table 3. The main distance effect is -0.79 for schools within walking distance. As one might expect, the comparable distance penalty for higher income families is nearly twice as large, -1.56, adding the interaction estimate of -0.77. To illustrate the magnitude of these effects, consider two identical schools that are within a mile but one is half a mile farther than the other. The odds ratio favoring the closer school is 1.5 for low income families and 2.2 for higher income families. The household composition interaction is not large enough to be significant. Beyond the one-mile mark -- commutes by bus -- the distance penalty

[8] We occasionally refer to the household composition variable as "lives with one parent", although this category includes a small number of students who live with another relative or guardian.

is smaller and the SES interactions both disappear, so all families have the same penalty to the index (utility) function of about -0.55. A comparable half mile difference by bus would be favored by an estimated odds ratio of 1.3.

We also estimate a fixed cost of taking the bus, which is the coefficient on the dummy variable for bus eligibility in Table 3. Without this effect the finding that the slope is flatter beyond one mile would imply an incentive to choose farther schools just to become eligible for bus transportation. The negative sign on the bus eligibility dummy and estimated effect size of -0.90 suggests that taking the bus is itself quite onerous. This effect was also estimated separately for different SES groups, but the differences were not statistically significant. The results from that specification are available from the author.

Figure 2 illustrates the estimated relationship between utility and distance. The high income families, represented in Panel A, become eligible for bus transportation at precisely the point where they would prefer to switch anyway. Panel B would imply, if taken literally, that lower income families prefer to continue walking to school even though they are bus eligible. In fact this just means that we need to re-estimate the model with the discontinuity point farther out. A general limitation of the data is that we do not observe whether or not students ride the bus or whether they have alternative transportation, for example a private car. Nevertheless we intend in future drafts of this paper to explore the possibility of a different walk/bus crossover point for lower income families.

To test the robustness of the piecewise distance-utility relationship we estimated three alternative specifications for distance: quadratic, cubic and logarithmic. Each of these tries to capture the diminishing disutility of commute distance. In each case distance is interacted with the family income dummy based on free lunch eligibility. We drop the single parent interaction for each model and re-estimate the base model also without the single parent interaction for comparison.

The findings, listed in Table 4, suggest that the base model, piecewise linear, fits the data nearly as well as any of the alternatives. The quadratic model has a slightly higher likelihood with the same degrees of freedom, but implies non-monotonic preferences with a utility-minimizing distance of only 4.5 miles. The cubic terms are small and statistically insignificant. The log model has a very weak fit, with a log likelihood of -1945, compared to -1940 for the modified base model. The income effect found in the original base model, however, disappears when subjected to robustness analysis, as does the fixed cost of transportation. The piecewise (base) model with separate effects for bus and walking distances and a penalty for riding the bus is the more theoretically justifiable model, although Table 4 demonstrates that the data cannot easily distinguish between these competing explanations.

In either case, we find that consumers prefer closer schools and the differences matter less and less as all the schools being compared are farther from home. The estimates can be used as a metric to gauge the importance of other choice factors. For example, we can use them to say how far a student is willing to travel for a unit increment in school quality.

5.2. Information

Included in the base model of Table 3 are two control variables which account for the exogenous differences in available information about schools. Minneapolis introduced one new school in 1993, so no data were available because it had no track record. We included a New

School dummy in the second stage regression. The coefficient on that variable is not statistically significant, but it effectively removes the new school from the second stage regression.

The second variable is a dummy which indicates whether a school was unofficially available. The official printed materials sent to Minneapolis parents defined the attendance area boundaries of each school, but district officials decided to expand the availability of several schools. The officials informed parents in person and over the phone of their expanded choice set, but the delayed announcement might have imposed an information cost that, if we did not measure it, would cause us to underestimate the value of attributes of schools in the expanded choice sets. As expected, the coefficient was negative and significant.

5.3. Peers

Two broad dimensions of school quality we consider are peer group attributes and intrinsic school attributes. While commuting costs and information are important determinants of school selection, it may be the case that students would commute farther if they felt more comfortable with students at a more distant school. The base model includes estimates of such peer group preferences based on the fraction of one's neighbors attending the school, the average achievement level of the school, the racial composition of the school, and the average socio-economic status of students attending the school. We consider each of these in turn.

Neighbors. The result listed in Table 3 for neighbor effects suggests that parents are more likely to choose a school if a greater fraction of their neighbors' children who are eligible for the school attend. We tested this result against different definitions of neighborhood and neighbors. The base model includes students in grades 1 through 3 in 1992-93 who lived within a one mile radius of the chooser. Changing the neighborhood radius to 0.50 and then 0.25 miles and changing the grade horizon to just grade 1 did not make a substantial difference.

Ability. Perhaps the most widely reported and relied upon measure of a school's quality is its average score on some standardized achievement test. It would follow that a school with high scores would be chosen more often by parents, *ceteris paribus*. As we pointed out in Section 2, the literature on choice in higher education suggests that this might not be true, that students instead seek a match between their own ability and that of the student body at the prospective school.

In the case of elementary school choice in Minneapolis we were not able to detect a very strong relationship at all between test scores and selection probabilities. The model estimated by Manski and Wise and by others used the difference between the chooser's score and the mean score of the prospective school. Table 3 presents estimates from a more general specification that allows for a main effect of school average test scores, an interaction with the chooser's own test score, and an effect of school average test score squared.[9] All test scores were normed to a nationally representative sample with mean of zero and standard deviation of one so the regression coefficients would be easier to interpret. The average test scores in Minneapolis schools ranged from -1.59 to 0.66.

---

[9] The choice of test subject is arbitrary. This paper uses a composite score which is an unweighted average of reading, vocabulary, math concepts, and math computation sections of the California Achievement Test, Form E. All four subtests were very highly correlated. For a full examination of the robustness of results presented here with respect to choice of subtest content area, see Glazerman (in progress).

The standard errors for the linear and quadratic terms are too large to detect anything but dramatic effects of average test scores. Only the interaction term is statistically significant. Table 5 reports a regression identical to the base model, but with average test scores computed relative to a race-specific reference group. That is, it assumes choosers consider the average test scores of people like themselves at a given school. While overall average test scores vary across just 50 schools, a model that assumes choosers respond to race-specific means would allow us to observe consumer choice over a greater range of alternatives. As expected, the standard errors on those estimates are considerably lower. The linear term is positive, 0.389, but not significant. But the quadratic term is negative, -0.627, and significant. The interaction is positive and significant, 0.453. We reject the hypothesis that utility is quadratic in the relative test score (difference between the school's and student's test score), which we test by restricting the interaction term to equal -2 times the squared term.[10]

The estimated relationship between test scores and utility is depicted in Figure 3, where utility is rising up to a point near the chooser's own score, and falling thereafter. It appears that the data support the ability-matching hypothesis found to hold for students selecting colleges and graduate schools. The new finding in this study is that the interaction between own ability and school average ability is slightly weaker than the difference models would predict. One can see this by comparing the utility curves for low ($t=-1$), middle ($t=0$), and high ($t=1$) ability choosers. (Recall that choosers' ability "t" is measured in standard deviation units relative to the national norm). The utility-maximizing average test score relative to the chooser's own ability is actually falling. It is around zero for $t=-1$, around 0.25 for $t=0$, and around 0.75 for $t=1$.

Race/ethnicity. We proposed two ways in which race might influence school choices. One was through the representation of one's own group in the student body in the prospective school. The other was through the representation of one's own group in the curriculum. We include in the base specification of the logit model variables for both of these phenomena. One type of variable measures the fraction of the school's students from the same race/ethnic group as the chooser. The effects of this variable are estimated separately for each of the five race/ethnic groups. The other type of variable is a dummy measuring the presence of a special curricular program catering to the chooser's ethnic group. In 1993 selected Minneapolis schools had programs, primarily language instruction, geared toward Hispanic, Asian, or Native American students.

The coefficients on the fraction of own-race variables are very different for the five race groups, but the standard errors are also quite large. The results are reported in Table 3, but can also be found in Table 5 alongside a set of estimates of own-race effects from an alternative model with race-specific test scores. In both specifications the African American coefficient is large and significant. Using the smaller effect estimate of 2.12 we would predict that a black family deciding between two types of schools that are identical except one type has 25 percent black students and the other has 45 percent black students -- roughly the interquartile range in Minneapolis schools -- would be about 1.5 times more likely to choose one of the schools with

[10] The chi-squared statistic for this test is 4.63 with one degree of freedom, large enough to reject the 0.05 level.

more black students, all other things being equal. The own-group preference for Hispanics is considerably larger. For a comparable difference, say 5 percent and 25 percent Hispanic, we would expect the Hispanic chooser to select the second school 2.8 times or 5.5 times more often, depending on which estimate we believed.

An important parameter for policy planning is the own-race coefficient for whites. The two specifications in Table 5 list parameter estimates of 1.1 and 1.8, with a standard error of about 1.1. The low precision probably results from the fact that Minneapolis had managed to ensure through controlled choice that there was little variation across schools in the fraction of white students. Still, these estimates are probably lower bounds because they are based on families that decided to remain in the public schools. Those who attended suburban or private schools were probably opting for schools with more white students.

The curriculum-own race interaction reinforces the finding that Asian Americans did not have any own-race preferences, but suggests that the overall own-race effects for Hispanics and Native Americans are even stronger than what is implied by the own-race peer group findings. Hispanics were much more likely to choose a school with Spanish language programs and Native Americans were much more likely to choose a school with Native American language instruction. Again, there is not much precision given the collinearity with the fraction of students of one's own ethnicity, but the Native American effect of 2.4 is significant both statistically and substantively. The language programs are mostly geared toward Limited English Proficient students, who were given special priority in the choice process along with siblings and therefore are not included in our sample. That means the ethnic programming effect estimated here is based on native English speakers only, lending weight to the interpretation of these findings as preferences for self-segregation by ethnicity.

In Table 6 we estimate the same model but aggregated the same-race student effects by race. One rationale besides trying to aggregate for precision is that the distribution of observed levels of racial composition are very different for different groups. This specification allows us to estimate the low-end effects using primarily the poorly represented minorities and the higher end effects based on whites and African Americans, who are in the plurality if not majority of many schools. The finding was a statistically significant and substantively important coefficient estimate on the fraction of students of the same race as the chooser, 2.1.

We estimated the same model with a squared term to test whether the own-race preference was declining. The quadratic specification shows that own race preferences peak within the (0,1) range, suggesting that choosers may exhibit a taste for diversity. Using the delta method (see Greenberg and Webster, 1993) one can calculate the standard error of the utility-maximizing level of own race representation, but the result is uninformative. A reasonable confidence interval includes the entire parameter space. Because very few schools were so segregated, we cannot estimate the curvature term very precisely.

Finally we estimated a piecewise linear model, which would be appropriate if race preferences were defined in terms of thresholds or tipping points. The piecewise model shows a very large positive effect that declines sharply at first and then stays roughly constant at about 2.

The three specifications -- linear, piecewise linear, and quadratic -- are depicted in Figure 4. It appears that there is a large qualitative difference between choosers who face being in a small minority, less than 10 percent, and the rest. The own race effect of about 2.0 is fairly robust throughout the rest of the range of the data. This effect size should be considered in light of the fact that many school comparisons will involve shifts of about 10 to 20 percent in the representation of the chooser's ethnic group. This translates into modest effects, computing the selection probability from equation (7) for a utility difference of between .20 and .40. To better predict the long term effects of consumer choice, however, future research could benefit by studying cities where at least some schools are severely segregated.

Socio-economic status. Finally, we consider whether a school is less likely to be chosen if more of its students are from poor or single parent households. Not suprisingly, it is. The two coefficient estimates in the base model have the expected negative sign and fairly large coefficients. Because of high collinearity the estimates are not both significant and they are fairly sensitive to the specification of the rest of the model, which is somewhat arbitrary. Therefore we estimated a range of different specifications to test the robustness of these results.[11] Depending on what else was included, the coefficient estimates on fraction of students free-lunch eligible and fraction from single-parent households alternated with one of the two usually being statistically significant, their estimates being between -1.0 and -2.5.

We mentioned that the two SES measures are highly correlated (ρ=.78). Here we consider a way to summarize their joint effect. Each appears roughly normally distributed across the 50 schools with a mean of roughly 0.50 and standard deviation of about 0.15. Assuming they were functionally dependent we could construct a single variable, say SESindex = f(fraction poor, fraction single parent) to better summarizes the SES effect. Using an unweighted average of the two, the index has a coefficient of -3.00 with a standard error of 1.04. Altering the weights made almost no difference. From the evidence one can infer that SES matters a great deal, even after accounting for commute distance, race, and test scores. Using the SES index a difference of 60 percent of the students being low SES versus 40 percent favors the high SES school approximately 0.65 to 0.35, an odds ratio of 1.85.

5.4. School attributes

Along with the new school indicator, average test scores and average peers' SES, the school characteristics in Table 3 are "fixed" attributes (Q_j). We discussed the problems of collinearity and low power estimates. That much is evident in the standard errors, which overshadow all the estimated effect sizes. The most surprising result is that the value-added measures of school quality were so small and even negative.[12]

The meaning of this finding is not so obvious. Some might argue that a fully rational consumer trying to maximize human capital production would necessarily divine the true school

[11] The complete estimates from these robustness analyses are not reported in this paper, but they are available from the author.

[12] The value-added measures come from a separate study (Meyer and Grosjean, forthcoming) that derives esimates of school performance from an individual growth model. The authors used California Achievement Tests administered to Minneapolis elementary school students in 1992 and again in 1993. Normally one would try to adjust the choice model estimates for measurement error induced by estimation of the value added effects. In this case the effects are so small it would make no difference. Nevertheless, the problem posed by estimating a GLS equation with measurement error, where the measurement error is heteroskedastic and correlated across observations would be an interesting one for future research.

productivity. As a result, failure of a value added indicator to predict school choices is a sign of a flawed indicator. One possibility is that the true production function exhibits comparative advantage, so that a single measure of value added is an inadequate predictor of choice behavior when school effectiveness is in fact multi-dimensional. Another story is that value added is too hard for parents to know or guess *a priori*. This is the information problem alluded to at various points in this paper. If parents are unable to judge the school effectiveness in a school choice system like Minneapolis, policy makers would have to consider how and whether more pro-competitive policies would stimulate demand for better low-cost information about value added.

A third line of argument suggests that the value added measures focus on the wrong outcomes, standardized achievement test scores, especially for young children. Nevertheless we choose these measures because policy makers most often use such scores to measure progress of students and schools and to compare the district against the state and the nation. If these scores constitute the "bottom line" for stakeholders, then it would be interesting to know how it might change in response to policy changes like further expanding or restricting parental choice.

Several attempts to test the robustness of the school attribute results -- including aggregating value added measures further by test subject area -- did not have much effect on the parameter estimates. Only the fraction low income (free lunch eligible) students or fraction living with single parents were significant, as noted above. The coefficients on the crime rate, measured as the logarithm of assaults committed per four-month period, and absentee rate, measured as the fraction of students missing more than 10 days of school, had the expected sign, but median family income in the student's home census tract did not. More data collection in a variety of settings would be necessary before taking a stand on the importance of these factors.

5.5 Comparison of effect sizes

We have presented a long list of factors that influence school selection probabilities. To better focus the findings of the empirical analysis we list the explanatory variables together in Table 7 to compare their size and relative importance. We perturb each attribute one at a time and compute the change in the expected probability that a typical chooser selects the school whose attribute changed. Again, we use binary comparisons to illustrate the magnitude of the effect sizes, but the same analysis can be applied to choice sets of multiple schools.

The most striking effects in Table 7 are those for race/ethnicity. The race effects are particularly strong when the chooser faces the prospect of being either in a small minority or in a school whose curriculum is oriented to his or her own ethnic identity. This applies to Hispanic and Native American choosers, although there was no discernable effect for Asian Americans. The base model predicts that a school with Native American themes in the curriculum will raise the selection probability for a Native American family from 50 percent to 87 percent, a difference that is easily significant at the 0.05 level. Hispanic choosers would select a school with Spanish immersion, bilingual education, or some other Spanish cultural theme over an identical one without such programming 85 percent of the time, also a signficant difference from 50 percent.

The piecewise linear own-race model suggests that choosers are very unlikely to select schools if their group is very poorly represented (below 10 percent) in the student body. As the own-race fraction becomes higher, the effect zeroes out and we predict parents choosing among schools in the 40-60 percent range with roughly equal probability. The higher-end effects were estimated from the behavior of white and African American families, whose preferences for members of their own race were weaker. The estimated selection probabilities are correspondingly low.

Figure 1 shows the racial/ethnic composition across schools, separately by ethnic group. Since whites and blacks did not choose among schools with low fractions of own race representation it is unclear from the data whether whites and blacks would have stronger preferences like Hispanics and Native Americans if they also faced the prospect of being in a small minority.

The distance and information effects imply non-trivial and statistically significant changes in selection probabilities, but an interesting effect is what we call in Table 7 the residual school popularity. This is the unmeasured school effect u_j after controlling for school characteristics Q_j. We estimate the variance of this as part of the GLS estimation of the second stage of the choice model, described in Section 2. Taking a one-standard deviation change in residual school popularity, the chooser is expected to select the "better" school 66 percent of the time. We interpret this to mean that after controlling for race, test scores, value added performance, costs, etc. there is still unmeasured school-specific "quality" that families perceive in common. This could be the school's or teachers' reputation. Whatever it might be, it represents a powerful argument that families can identify and rank schools on some basis other than observed variables in our model.

The other effects listed in Table 7 are also substantively important. For an extra half mile walk the selection probability changed by either 13 or 19 points, depending on the functional form. An additional half mile added to one's bus ride changed the selection probability by 8 or 11 points. Test scores, which one might have expected to be a major influence on school selection, were only important when middle and high ability choosers had a school with many low achieving potential classmates in their choice set. Movements from -0.5 standard deviations to the national mean would raise the selection probability by an estimated 9 to 14 points. Low ability students actually would be less likely to choose the "best" schools as measured by test scores.

The effects of SES were large in our sample, but not statistically significant. Information costs in a binary comparison lowered the school's selection probability by a sizable 16 points. The neighbor effect appeared large in Table 3, but in fact very few choosers had neighbors' children concentrated in any one school in their choice set, so we predict a modest impact on the probability of actually choosing the school.

6. Simulations: effect of choice on stratification by race, SES, and ability

Each of the factors listed in Table 7 competes to influence parents' decision of which school to choose for their child. Comparing effect sizes, however standardized, may still fail to convey a picture of the consequences of giving parents the opportunity to choose schools. For instance a large fraction of each cohort in Minneapolis faced a degenerate choice set, either because the student had special priority or was a late applicant. Yet we might want to re-construct the choice sets for those students and make predictions about what they might have chosen under a different policy. Therefore we turn to a series of simulations that combine the parameter estimates to predict how students will sort themselves across schools.

The simulation works as follows. For students who did not submit choices before the deadline, we re-construct their choice set and use the logit model to predict where they would have chosen. A school's predicted enrollment is the sum of its predicted selection probability for each student, $E_j = \Sigma_i \hat{p}_{ij}$. The resulting distribution of students across schools can be used to update the school composition variables. We take a hypothetical cohort of students identical to the 1993 cohort and predict their choices given the new student body composition. We update the school composition variables and predict choices for another cohort. We continue iteratively until the student body composition replicates itself, a point we call the steady state. We repeat the analysis for different specifications of the choice model, reporting stratification measures for race, SES, and ability at each iteration as well as in the steady state.

The stratification measures are scaled from 0 (perfect integration) to 100 (perfect segregation). Table 8 lists the levels of stratification by race, SES, and ability under current policy, which is controlled choice, and compares it to two proposed alternatives. The first alternative is a system of neighborhood schools, where students are assigned to their closest school. This policy would have schools reflect the segregation level (55) that can be found in housing patterns. The second alternative would be to expand choice so that everyone was guaranteed their first choice. Under this policy the racial segregation would rise, perhaps slowly, but settle at 52, almost as high as neighborhood schools, a change of 35 points from the baseline. The steady state levels of SES and ability stratification would be comparable to the baseline, and slightly better than under neighborhood schools.

We also model the effects of a sibling preference rule. The aim is to identify not only the steady state levels of stratification but the time path, so that one can predict whether the steady state is likely to happen quickly, or if there will be a transition period. There is a possibility of an intertemporal tradeoff for policy makers. That is, we may want to advocate a plan that gives consumer satisfaction today in exchange for segregation in the distant future. A more realistic simulation would allow for entry and exit of consumers, so that expanding choice might actually attract more white and higher income families to the public school system.

7. Conclusion

7.1. The empirical evidence on consumer tastes

We proposed a simple model of school choice that conditioned on the chooser having committed to a residential location and the public school system. We allowed the probability of choosing each school to be influenced by commuting and information costs, peer group attributes, and school attributes. The empirical results challenge a widely held belief that parents naturally select schools with the highest academic performance. Traditional measures of academic quality, test score levels and value added indicators, had little or no predictive power. Nor were choices influenced much by school safety or neighborhood quality, two factors frequently cited in the author's informal interviews. Race, distance, and SES on the other hand, had strong effects.

The only test-score effect that fit the data was a model where choosers consider others like themselves as a reference group to evaluate the academic quality of a prospective school. Even then we rejected the monotonic (linear) effect in favor a quadratic relationship whose degree of steepness depends on the prior ability of the chooser. We conclude that social matching and ability matching are prevalent behaviors in the choice process. Interestingly, the self-segregating tendency was weakest for whites. The data cannot distinguish, however, whether this is actually "white" behavior or whether whites just happened to be well represented in all their potential schools, and they would have been just as self-segregating had they faced the same prospects of severe isolation as did other ethnic groups.

To see the practical implications of the estimated taste parameters we conducted a simulation where we predicted each cohort's choices recursively for several cohorts. We found that in the steady state the school system was much more segregated than under current policy, while stratificaton by social class and ability rose more modestly.

7.2. Do parents care about academic quality?

We cannot say parents are not concerned about their children's learning. Finding a match with one's peers may be one way of assuring a good learning environment. There may be intangible factors, including reputation, that account for the unexplained variation in school effects. Perhaps parents care less about cognitive outcomes than affective outcomes not measured in this study, like self-esteem of their children. We can say, however, that the evidence suggests that what parents consider to be important and what taxpayers and their representatives that fund public education consider important -- measured school performance -- are not the same.

The finding that value added indicators fail to predict school choices should be interpreted with care. The value added measures used here were not released to the public at the time parents made their school selections. In recent years the MPS leadership has begun to embrace the methodology of value added, so a followup study analyzing Minneapolis choice data over time could shed light on this important question of the effect of information on consumer behavior. Nevertheless, it is important to ask why such information was not readily available even by 1993 when parents had been choosing schools for years. There are at least two separate explanations. One story is that, as we strongly suggested in this paper, value added is less important to parents in the choice process than other factors. Another story is that controlled choice is a policy that discourages the spread of low cost information that could be used to rank schools by a single dimension of quality. The reason is that public school choice plans free up the demand side of the market while retaining a central authority that controls the supply side. As we pointed out in Section 4, district officials are concerned with keeping parents satisfied with their school assignments. Rank ordering the schools by any objective quality measure would lead to over-subscription of the high quality schools and would place stress on the administrative body that places students. A more competitive system might create incentives to provide and publicize much more detailed information about schools, including academic performance of their students and teachers. More school building autonomy and less control by a central school bureaucracy could achieve this same effect. Otherwise, increasing consumer choice in education would likely only result in higher consumer satisfaction at the cost of some segregation by race and class. For school improvement in traditional academic areas, "a little consumer choice" is not sufficient.

7.3. Extensions and questions for future research

While this study offers new evidence on consumer tastes and their implications for social stratification, it is far from conclusive. A useful next step would be to model the simultaneous residence, school sector, and school choice. More work needs to be done also to understand whether strategic behavior might bias inferences in this type of study. Ultimately one would want to predict the supply side effects of expanding consumer choice in education. The growing charter school movement could stimulate both demand and opportunities for new research to understand the entry and exit of non-public providers in the market for primary schooling.

There are many ways we hope to improve and generalize the econometric model presented in this paper. One extension would be to relax the logit assumption to allow for different forms of unobserved preference heterogeneity as well as heteroskedasticity in the school equation. Allowing the school-specific error terms to differ in their variance is one way to model schools that specialize in ways that appeal to unobserved tastes. Recent advances in econometric theory and computation noted in the text are promising for future work in this area.

As our understanding of the market for primary education grows policy makers will be better equipped to predict the effects of a range of educational policies on migration and enrollment patterns. A choice model like the one estimated in this paper could be useful as a planning tool, for instance, to decide where to locate a new school or whether to create schools-within-schools. Increased attention in general to consumer choice in education could focus policy makers' attention on tradeoffs between what parents want and what communities that fund public education want. For example one could find the degree of parental choice that maximizes social welfare subject to some contraint on racial segregation. The simulations suggest there might also be a tradeoff between consumer satisfaction today and racial segregation in the future. We have begun to answer the question of what parents value in education, but provoked new questions about what defines school quality and whether it matters. We hope education policy and the evaluation of education programs can benefit from this new perspective.

Bibliography

Baird, J. (1990) Examining parent selection in the East Baton Rouge controlled choice plan. Dissertation, Louisiana State University.

Ben-Akiva, M.E., & Lerman, S.R. (1985). *Discrete choice analysis: theory and application to travel demand.* Cambridge, MA: MIT Press.

Borjas, G.J., & Sueyoshi, G.T. (1994). A two-stage estimator for probit models with structural group effects. *Journal of Econometrics, 64*, 165-182.

Börsch-Supan, A., & V.A. Hajivassiliou. (1993). Smooth unbiased multivariate probability simulators for maximum likelihood estimation of limited dependent variable models. *Journal of Econometrics, 58*, 347-368.

Boskin, M.J. (1974). A conditional logit model of occupational choice. *Journal of Political Economy, 82*, 389-398.

Bridge, G., & Blackman, J. (1978). *A study of alternatives in education, vol. IV: Family choice in education.* Santa Monica, CA: The Rand Corporation.

Cameron, S.V., & Heckman, J.J. (1994). Determinants of young males' schooling and training choices. in Lynch, L.M. (ed.) *Training in the private sector: International comparisons.* NBER Labor Market Series. Chicago and London: University of Chicago Press, pp. 201-31.

Cardell, N.S. (1997). Variance components structures for the extreme value and logistic distributions with applications to models of heterogeneity. *Econometric Theory, 13*, 185-213.

Carnegie Foundation for the Advancement of Teaching. (1992). *School choice.* Princeton, New Jersey: Author.

Coldren, J. & Boulton, P. (1991). Happiness as a criterion of parents' choice of school. *Journal of Education Policy, 6*(2), 169-178.

Cragg, J.G. (1971). Some statistical models for limited dependent variables with application to the demand for durable goods. *Econometrica, (39)*, 829-844.

Geweke, J., M. Keane, & D. Runkle. (1994). Alternative computational approaches in the multinomial probit model. *Review of Economics and Statistics, vol?*(?), 609-632.

Glazerman, S.M., & Meyer, R.H. (1995). Public school choice in Minneapolis. In Testa, W. (Ed.), *Midwest Approaches to School Reform.* Chicago, IL: Federal Reserve Bank of Chicago.

Goldhaber, D.D. (1996). Public and private high schools: Is school choice an answer to the productivity problem? *Economics of Education Review 15*(2), 93-109.

Greenberg, E., & Webster, C. (1993). *Advanced econometrics: A bridge to the literature.* New York: Wiley.

Hanushek, E.A. (1974). Efficient estimators for regressing regression coefficients. *The American Statistician 28*(2), 66-67.

Hausman, J.A. & D.A. Wise. (1978). A conditional probit model for qualitative choice: discrete decisions recognizing interdependence and heterogeneous preferences. *Econometrica 46*, 403-426.

Hensher, D.A., & Johnson, L.W. (1981). *Applied discrete choice modelling*. London: Halsted Press.

Kohn, M.G., Manski, C.F., & Mundel, D. (1976). An empirical investigation of factors influencing college-going behavior. *Annals of Economic and Social Measurement, 5*(4), 391-419.

Lankford, H. & Wyckoff, J. (1992). Primary and secondary school choice among public and religious alternatives. *Economics of Education Review, 11*(4), 317-337.

Lee, V.E, Croninger, R.G., & Smith, J.B. (1994). Parental choice of schools and social stratification in education: The paradox of Detroit. *Educational Evaluation and Policy Analysis, 16*(4), 434-457.

Lerman, S., & C.F. Manski. (1981). On the use of simulated frequencies to approximate choice probabilities. in C.F. Manski & D. McFadden (eds.) *Structural analysis of discrete data with econometric applications*. Cambridge, MA: MIT Press.

Maddala, G.S. (1983). *Limited dependent and qualitative variables in econometrics*. Cambridge University Press.

Manski, C.F., & Wise, D.A. (1983). *College choice in America*. Cambridge, MA: Harvard University Press.

McFadden, D. (1974). Conditional logit analysis of qualitative behavior. In P. Zarembka (ed.), *Frontiers of econometrics*. New York: Academic Press.

Meyer, R.H. (1995). A Conditional Probit/Logit Model with Heteroscedastic Errors. University of Chicago, manuscript.

Meyer, R.H., & Grosjean, J.G. (In progress). Value added indicators of school performance in Minneapolis.

Petch, A. (1986). Parental choice at entry to primary school, *Research Papers in Education, 1*(1), 26-47.

Punj, G.N., & Staelin, R. (1978). The choice process for graduate business schools. *Journal of Marketing Research 15*, 588-598.

Romano, R.E. & Epple, D. (1994). Competition between private and public schools, vouchers and peer group effects. Northwestern University, mimeo.

Strobert, Barbara. (1990). Factors Influencing Parental Choice in Selecting Magnet Schools in the Montclair, New Jersey, Public Schools, dissertation, Teachers College, Columbia University.

Williams, M.F., et al. (1983) Parents and school choice: A household survey. *School Finance Project Working Paper*. Washington, DC: Office of Educational Research and Improvement.

Table 1. Characteristics of choosers

N= 881 students

A. Means

	Mean	Standard deviation	
Race:			
Native American	4%		
African American	21%		
Asian American	4%		
Hispanic	3%		
White	68%		
SES:			
Living with single parent or with non-parental guardians	31%		
Eligible for free or reduced lunch (low income)	33%		
Test scores:			
Average Math score	0.45	0.97	(National mean = 0, std deviation = 1)
Average Verbal score	0.12	1.03	(National mean = 0, std deviation = 1)
Home neighborhood:			
Median family income	$35,920	$18,488	

B. Correlations

	White	One parent	Free lunch	Math	Verbal	Income
White	1.00					
Lives with one parent or non--parental guardians	-0.35	1.00				
Eligible for free or reduced lunch (low income)	-0.29	0.44	1.00			
Math ability	0.33	-0.26	-0.32	1.00		
Verbal ability	0.28	-0.28	-0.35	0.70	1.00	
Median family income in neighborhood	0.28	-0.29	-0.30	0.16	0.21	1.00

Table 2. Characteristics of MPS elementary schools

J = 50 schools

A. Means

	Mean	Standard deviation	
Racial composition:			
Native American	0.08	0.11	
African American	0.36	0.15	
Asian American	0.09	0.10	
Hispanic	0.04	0.05	
White	0.43	0.12	
SES composition:			
Fraction living with single parent or with non-parental guardians	0.49	0.16	
Fraction eligible for free or reduced lunch (low income)	0.48	0.14	
Test scores:			
Average Math score	-0.07	0.53	(National mean = 0, std deviation = 1)
Average Verbal score	-0.62	0.46	(National mean = 0, std deviation = 1)
Neighborhood:			
Median family income	$39.884	$22,858	
Crime rate (assaults per quarter)	18.83	21.95	
Other:			
Value added, math	-0.04	0.88	
Value added, verbal	0.00	0.89	
Absentee rate	0.35	0.12	

B. Correlations

	Percent white	Percent single parent	Percent free lunch	Average math	Average verbal
Percent white	1.00				
Percent living with one parent	-0.78	1.00			
Percent free lunch eligible	-0.79	0.78	1.00		
Average math score	0.57	-0.58	-0.47	1.00	
Average verbal score	0.60	-0.70	-0.66	0.85	1.00
Median income	0.20	-0.25	-0.51	0.27	0.40
Crime rate	-0.22	0.18	0.12	-0.10	-0.13
Value added, math	0.02	0.11	0.01	0.08	0.06
Value added, verbal	0.21	-0.14	-0.36	0.17	0.17
Absentee rate	-0.30	0.48	0.21	-0.08	-0.15

	Median income	Crime rate	Value added, math	Value added, verbal	Absentee rate
Median income	1.00				
Crime rate	-0.32	1.00			
Value added, math	-0.09	-0.14	1.00		
Value added, verbal	0.29	-0.10	0.60	1.00	
Absentee rate	-0.11	0.13	0.17	0.07	1.00

Table 3. Base model

	Coefficient Estimate	Standard Error
Commuting Costs		
Distance x Walk	-0.789 *	0.421
"" x "" x High income	-0.767 *	0.411
"" x "" x Single parent	-0.387	0.425
Distance x Bus	-0.552 **	0.106
"" x "" x High income	-0.058	0.100
"" x "" x Single parent	0.068	0.102
Distance >= 1 mile (bus eligible)	-0.898 **	0.200
Information Costs		
New school	0.037	0.695
School unofficially available	-0.684 **	0.149
Peer Attributes		
<u>Neighbors</u>		
Fraction of neighbors who chose school	3.817 **	0.778
<u>Ability</u>		
Average test score	0.259	0.463
"" "" "" x own test score	0.391 **	0.094
"" "" "" squared	0.645	0.420
<u>Race</u>		
Fraction own race x Native American chooser	0.228	2.164
Fraction own race x African American chooser	2.116 **	0.853
Fraction own race x Asian American chooser	1.393	3.419
Fraction own race x Hispanic chooser	5.137	4.170
Fraction own race x White chooser	1.143	1.035
Native Am. Language x Native Am chooser	1.921	1.212
Asian LEP program x Asian Am chooser	-0.473	0.664
Spanish LEP program x Spanish chooser	1.747 **	0.851
<u>SES</u>		
Fraction low income	-2.659 *	1.444
Fraction single parent	-1.434	1.357
School Attributes		
Value added, math	-0.082	0.151
Value added, verbal	0.046	0.170
Neighborhood, crime rate	-0.087	0.062
Neighborhood, median family income	-0.009	0.007
Student attachment: absentee rate	-0.514	0.989
Grade configuration: K-6	-0.059	0.195

Notes:

* $p < 0.10$

** $p < 0.05$

Coefficients estimated in two stages. First stage included 49 school dummies, whose coefficients are suppressed for clarity. Second stage includes the New School dummy, average test score, test score squared, fraction low income, fraction from single parent household and all school attributes listed in the table.

Table 4. Distance effects, alternative specifications

	Modified Base	Quadratic	Cubic	Logarithmic
Distance	-1.024 **	-1.387 **	-1.715 **	
	(0.337)	(0.211)	(0.437)	
Distance x high income	-0.585	0.157	0.249	
	(0.360)	(0.212)	(0.463)	
Distance (bus)	-0.510 **			
	(0.083)			
Distance (bus) x high income	-0.083			
	(0.090)			
Distance squared		0.153 **	0.293 *	
		(0.039)	(0.176)	
Distance squared x high income		-0.037	-0.078	
		(0.045)	(0.206)	
Distance cubed			-0.016	
			(0.021)	
Distance cubed x high income			0.005	
			(0.026)	
ln(Distance)				-0.929 **
				(0.101)
ln(Distance) x high income				0.020
				(0.093)
Dist > 1 (bus eligible)	-0.901 **	0.021	0.086	0.021
	(0.200)	(0.144)	(0.160)	(0.140)
Log likelihood value	*-1939.8*	*-1937.6*	*-1937.1*	*-1945.2*

Notes:
* $p < 0.10$
** $p < 0.05$
Distance measured in miles. Standard errors are in parentheses.
Model includes 49 school dummies and all variables listed in base model. See Table 3.
First two rows in the modified base model refer to distance effects for distance < 1 mile.

Table 5. Test score effects, alternative specifications

	Base Model	Race-specific
Peer test score effects		
Average test score	0.259	0.389
	(0.463)	(0.319)
Average test score x own test score	0.391 **	0.453 **
	(0.094)	(0.125)
Average test score squared	0.645	-0.627 **
	(0.420)	(0.193)
Own race - race effects		
Fraction own race x Native American chooser	0.228	-0.669
	(2.164)	(2.176)
Fraction own race x African American chooser	2.116 **	2.382 **
	(0.853)	(0.867)
Fraction own race x Asian American chooser	1.393	3.475
	(3.419)	(3.693)
Fraction own race x Hispanic chooser	5.137	8.572 *
	(4.170)	(4.888)
Fraction own race x White chooser	1.143	1.813 *
	(1.035)	(1.066)
Own race - curriculum effects		
Native Am. language x Native American chooser	1.921	2.426 **
	(1.212)	(0.781)
Asian LEP x Asian American chooser	-0.473	-0.381
	(0.664)	(0.696)
Spanish LEP x Hispanic chooser	1.747 **	1.429
	(0.851)	(0.956)
Log likelihood	*-1937.61*	*-1933.49*

Notes:
* $p < 0.10$
** $p < 0.05$
Standard errors in parentheses.
All test score variables are normed to a national population with mean 0, std dev 1.
Full regression includes 49 school dummies and variables included in Table 3.

BEST COPY AVAILABLE

Table 6. Robustness of race effects, Functional form

	Linear 1	Quadratic 2	Piecewise linear 3
Fraction of own race/ethnic group in school	2.098 **	4.632 *	
	(0.495)	(2.591)	
Fraction own race squared		-3.207	
		(3.217)	
Min(Fraction own race,0.1) x (own race < 0.1)			17.028 **
			(5.626)
Min(Fraction own race,0.2) x (0.10 <= own race < 0.20)			-6.168
			(4.118)
Min(Fraction own race,0.3) x (0.20 <= own race < 0.30)			2.820
			(3.349)
Min(Fraction own race,0.4) x (0.30 <= own race < 0.40)			2.820
			(3.349)
Min(Fraction own race,0.5) x (0.40 <= own race < 0.50)			0.911
			(2.498)
Fraction own race x (own race >= 0.50)			0.219
			(2.548)
Log likelihood	*-1935.18*	*-1934.68*	*-1933.91*

Notes:
* $p < 0.10$
** $p < 0.05$
Standard errors in parentheses.
Full regressions includes 49 school dummies and variables included in Table 3.
Test score effects (not listed here) are based on race-specific average test scores.

Table 7. Effects of illustrative changes in explanatory variables on school selection probabilities

Explanatory Variable	Contrast School A	Contrast School B	Pr(choose B)
nothing: Two identical schools	*0*	*0*	*0.50 n/a*
Distance effects			
Base model high income, walk	1.0 mi.	0.5 mi.	0.69 **
Base model high income, bus	1.5 mi.	1.0 mi.	0.58 **
Quadratic model	1.0 mi.	0.5 mi.	0.63 **
Quadratic model	1.5 mi.	1.0 mi.	0.61 **
Race effects			
Fraction own race, piecewise linear	0%	10%	0.85 **
"" "" "" "" ""	10%	20%	0.35
"" "" "" "" ""	30%	40%	0.60 **
"" "" "" "" ""	40%	50%	0.52
"" "" "" "" ""	50%	60%	0.51
Fraction own race, aggregate	30%	50%	0.60 **
Spanish LEP, for Hispanic chooser	0	1	0.85 **
Native Am. theme for Nat. Am chooser	0	1	0.87 **
Test score effects (race-specific model)			
Low ability chooser	-0.5	0.0	0.53 *
	0.0	0.5	0.45 *
Average ability chooser	-0.5	0.0	0.59 **
	0.0	0.5	0.51
High ability chooser	-0.5	0.0	0.64 **
	0.0	0.5	0.57 **
Information effects			
School unofficially available	1	0	0.66 **
Neighbors' children in same school	0	5%	0.55 **
Other school effects			
Fraction higher income students	0.4	0.6	0.63 *
Fraction students from two-parent homes	0.4	0.6	0.57
Residual school effect ("popularity")	1 std dev increase		0.66

Notes:

* $p < 0.10$

** $p < 0.05$

Statistical significance refers to a test of the null hypothesis that Pr(choose B) = .50

Standard errors were calculated using the delta method, see Greenberg and Webster.

Test scores and ability measures are given in standard deviation units (z-scores).

Table 8. Stratification under alternative policies

	Type of stratification		
	Racial Balance	SES Balance*	Ability sorting**
(perfectly integrated)	*0*	*0*	*0*
(perfectly stratified)	*100*	*100*	*100*
Policy			
Controlled Choice (actual policy)	17.1	26.2	9.6
Unrestricted choice (steady state)	52.1	25.1	11.0
Neighborhood schools	55.0	28.0	15.8

Notes:

* Racial balance and SES balance measured as Dissimilarity Index scores for white/nonwhite and two-parent/other, respectively.

** Ability sorting is measured as the ratio of between-school variance in test scores to the total variance in test scores.

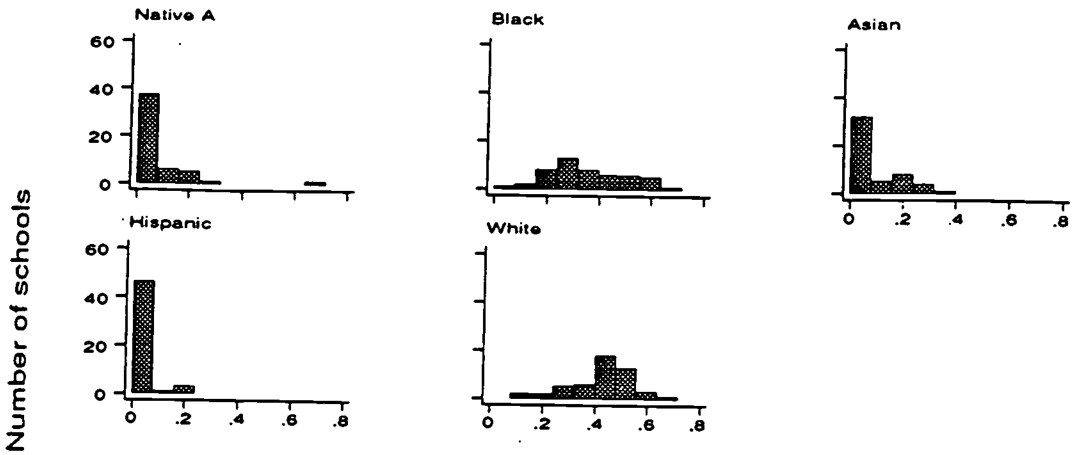


Figure 1. School histograms by race/ethnic composition

Figure 2. Effect of Distance on Utility

A. High income families

B. Low income families

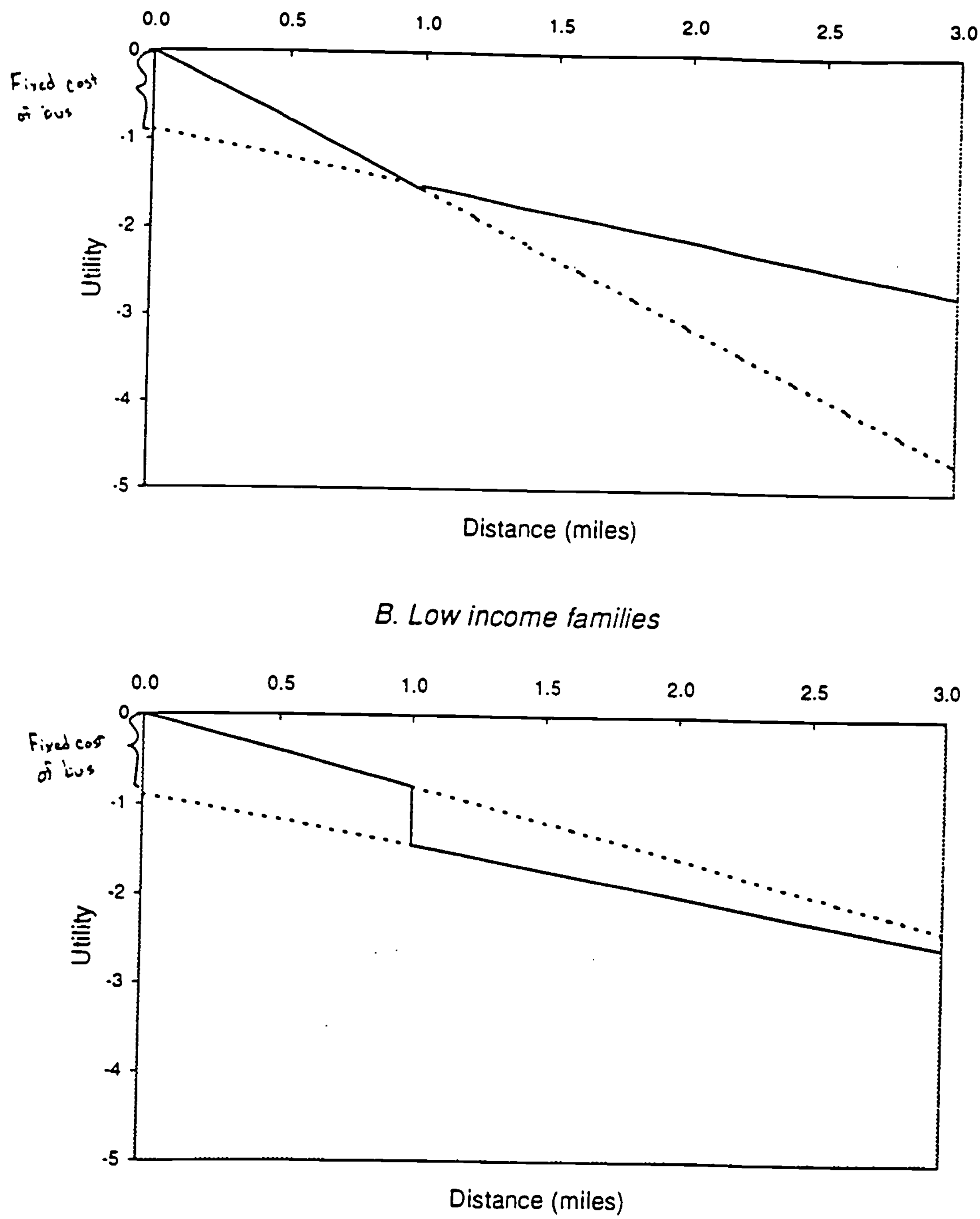


Figure 3. Effect of test scores on utility,
by chooser's test score

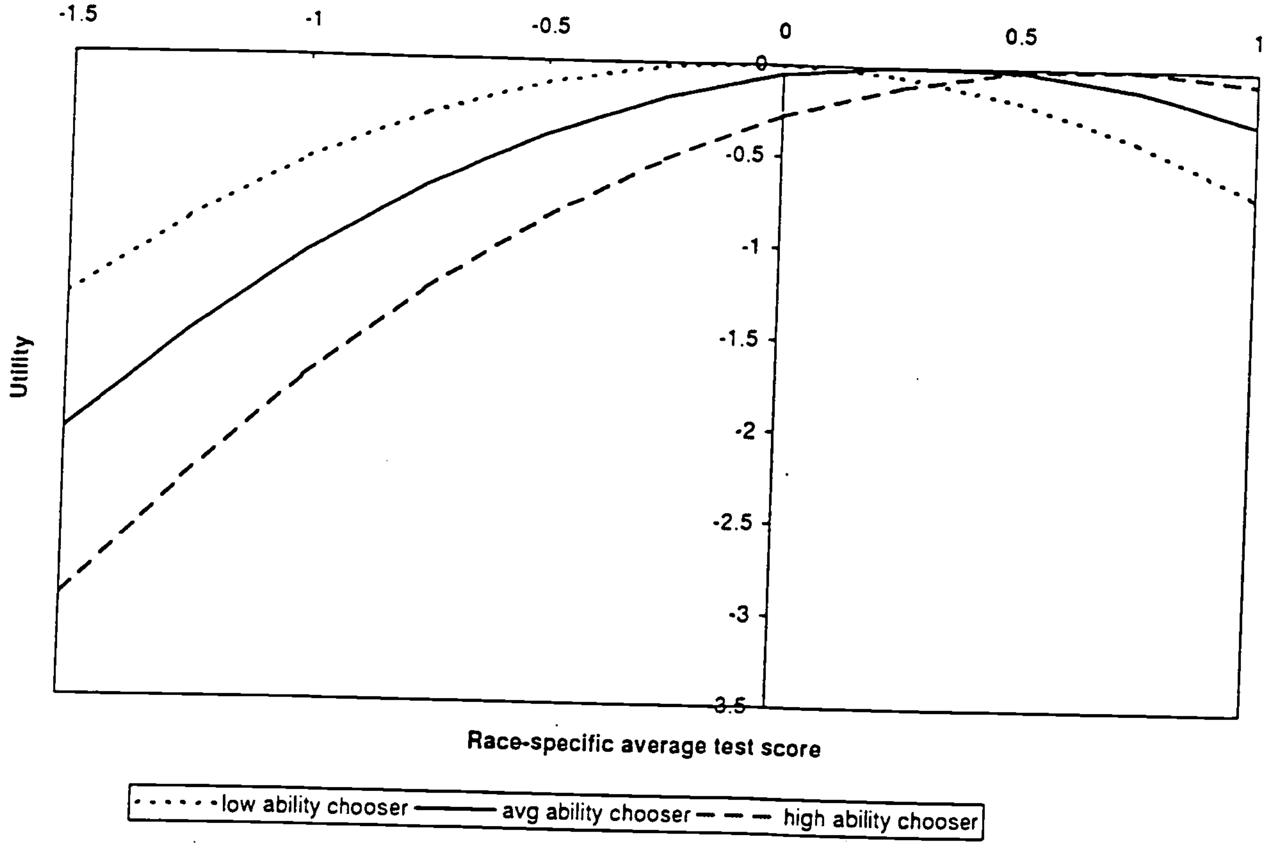
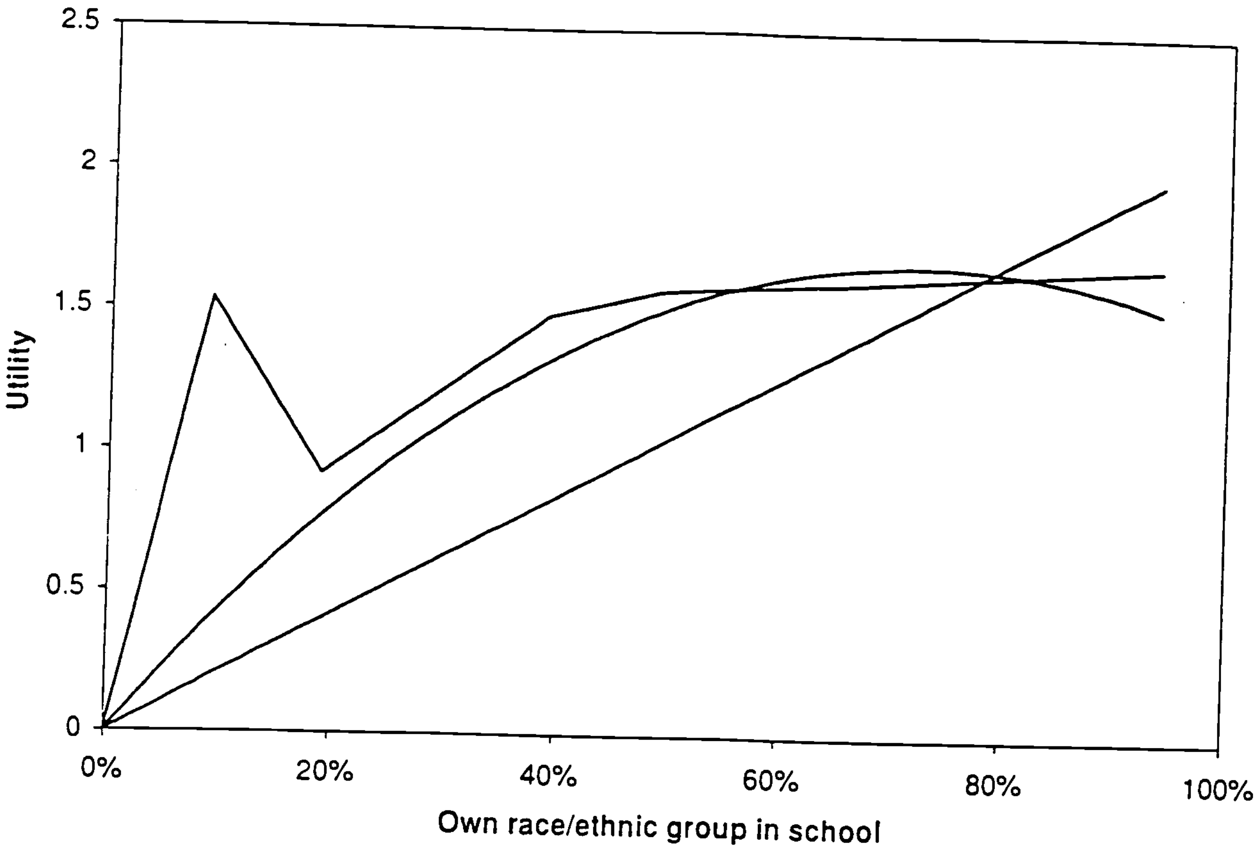


Figure 4. Effect of racial composition on utility



EA029485

U.S. Department of Education
Office of Educational Research and Improvement (OERI)
National Library of Education (NLE)
Educational Resources Information Center (ERIC)

ERIC®

REPRODUCTION RELEASE

(Specific Document)

I. DOCUMENT IDENTIFICATION:

Title: School Quality and Social Stratification: The Determinants and Consequences of Parental School Choice

Author(s): Steven M. Glazerman

Corporate Source:

Publication Date: April, 1998

II. REPRODUCTION RELEASE:

In order to disseminate as widely as possible timely and significant materials of interest to the educational community, documents announced in the monthly abstract journal of the ERIC system, *Resources in Education* (RIE), are usually made available to users in microfiche, reproduced paper copy, and electronic media, and sold through the ERIC Document Reproduction Service (EDRS). Credit is given to the source of each document, and, if reproduction release is granted, one of the following notices is affixed to the document.

If permission is granted to reproduce and disseminate the identified document, please CHECK ONE of the following three options and sign at the bottom of the page.

The sample sticker shown below will be affixed to all Level 1 documents	The sample sticker shown below will be affixed to all Level 2A documents	The sample sticker shown below will be affixed to all Level 2B documents
PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL HAS BEEN GRANTED BY Sample TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC) 1	PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL IN MICROFICHE, AND IN ELECTRONIC MEDIA FOR ERIC COLLECTION SUBSCRIBERS ONLY, HAS BEEN GRANTED BY Sample TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC) 2A	PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL IN MICROFICHE ONLY HAS BEEN GRANTED BY Sample TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC) 2B
Level 1 [x]	Level 2A [ ]	Level 2B [ ]
Check here for Level 1 release, permitting reproduction and dissemination in microfiche or other ERIC archival media (e.g., electronic) *and* paper copy.	Check here for Level 2A release, permitting reproduction and dissemination in microfiche and in electronic media for ERIC archival collection subscribers only	Check here for Level 2B release, permitting reproduction and dissemination in microfiche only

Documents will be processed as indicated provided reproduction quality permits.
If permission to reproduce is granted, but no box is checked, documents will be processed at Level 1.

I hereby grant to the Educational Resources Information Center (ERIC) nonexclusive permission to reproduce and disseminate this document as indicated above. Reproduction from the ERIC microfiche or electronic media by persons other than ERIC employees and its system contractors requires permission from the copyright holder. Exception is made for non-profit reproduction by libraries and other service agencies to satisfy information needs of educators in response to discrete inquiries.

Sign here,→ please

Signature: Steven M. Glazerman

Printed Name/Position/Title: Steven M. Glazerman

Organization/Address: University of Chicago, Harris School
1155 East 60th St. / Chicago, IL 60637

Telephone: 773-363-4639 FAX: 773-702-0926

E-Mail Address: s-glazerman@uchicago.edu

Date: 6/10/98

(over)

ERIC® Clearinghouse on Assessment and Evaluation

University of Maryland
1129 Shriver Laboratory
College Park, MD 20742-5701

Tel: (800) 464-3742
(301) 405-7449
FAX: (301) 405-8134
ericae@ericae.net
http://ericae.net

March 20, 1998

Dear AERA Presenter,

Congratulations on being a presenter at AERA[1]. The ERIC Clearinghouse on Assessment and Evaluation invites you to contribute to the ERIC database by providing us with a printed copy of your presentation.

Abstracts of papers accepted by ERIC appear in *Resources in Education (RIE)* and are announced to over 5,000 organizations. The inclusion of your work makes it readily available to other researchers, provides a permanent archive, and enhances the quality of *RIE*. Abstracts of your contribution will be accessible through the printed and electronic versions of *RIE*. The paper will be available through the microfiche collections that are housed at libraries around the world and through the ERIC Document Reproduction Service.

We are gathering all the papers from the AERA Conference. We will route your paper to the appropriate clearinghouse. You will be notified if your paper meets ERIC's criteria for inclusion in *RIE*: contribution to education, timeliness, relevance, methodology, effectiveness of presentation, and reproduction quality. You can track our processing of your paper at http://ericae.net.

Please sign the Reproduction Release Form on the back of this letter and include it with **two** copies of your paper. The Release Form gives ERIC permission to make and distribute copies of your paper. It does not preclude you from publishing your work. You can drop off the copies of your paper and Reproduction Release Form at the **ERIC booth (424)** or mail to our attention at the address below. Please feel free to copy the form for future or additional submissions.

Mail to: AERA 1998/ERIC Acquisitions
University of Maryland
1129 Shriver Laboratory
College Park, MD 20742

This year ERIC/AE is making a **Searchable Conference Program** available on the AERA web page (http://aera.net). Check it out!

Sincerely,

Lawrence M. Rudner

Lawrence M. Rudner, Ph.D.
Director, ERIC/AE

---

[1]If you are an AERA chair or discussant, please save this form for future use.

CUA

The Catholic University of America