Technical Report

THE IDENTIFICATION OF SCHOOLING PREFERENCES: METHODS AND EVIDENCE FROM POST-KATRINA NEW ORLEANS

EDUCATION
RESEARCH ALLIANCE
FOR NEW ORLEANS

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Updated July 5, 2019
Published January 15, 2017

EducationResearchAllianceNOLA.org

The Identification of Schooling Preferences: Methods and Evidence from Post-Katrina New Orleans

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Abstract: Established methods for estimating consumer demand and preferences require the restrictive assumption that omitted product characteristics are orthogonal to included characteristics. We provide evidence that this may be false in some cases and propose a new test for omitted variables bias. Our analysis focuses on families' revealed preferences for schools, combining the rankings from a deferred acceptance algorithm with detailed data about the product differentiation in the New Orleans charter-based school system. Contrary to most prior research, the results from our method suggest that families prefer schools with higher school value-added, more extracurricular activities, and low indirect costs.

Keywords: Consumer Preferences, Publicly Provided Goods, Education

JEL Codes: D12, H4, I2

Acknowledgements: The authors carried out this work within the Department of Economics at Tulane University. The center is funded by generous grants from the Arnold Foundation, William T. Grant Foundation, and the Spencer Foundation. In addition, the authors wish to thank the Cowen Institute for Public Education Initiatives, Orleans Parish School Board, and Recovery School District for providing data. Paula Arce-Trigatti, Lynn Dupont, Alica Gerry, Gregory C. Rigamer & Associates (GCR), Robert Santillano and especially Jill Zmmerman made important contributions. For their useful comments, we also thank Robert Bifulco, David Figlio, Bill Jackson, Neerav Susanna Loeb, Parag Pathak, Kenneth Train, Jon Valant, Patrick Wolf, and participants in sessions at Louisiana State University, the Association for Public Policy and Management (APPAM), the Association for Education Finance and Policy (AEFP) and the Great Schools conference in San Francisco, CA.

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1 Introduction

Consumer preferences are central for understanding market outcomes (Berry, Levinsohn, and Pakes [BLP], 1995, 2004; Train, 2009) and for policymaker understanding of the demand for publicly-provided goods, such as parks, roads, and health care, which generally lack a market-based price mechanism. In this study, we specifically consider demand and preferences for primary and secondary schools.

Preferences for public schools represent a noteworthy consumer example because of the rapid expansion of market-based school accountability in the United States and internationally (Ladd & Fiske, 2001; Betts, 2010; Harris & Witte, 2011; Hart & Figlio, 2014). For more than a century, children in the U.S. have attended schools based on where they live and pressure to improve has been limited to school board elections, inter-district housing decisions, and state test-based accountability (Friedman, 1962; Tiebout, 1956). This traditional government-driven model is increasingly being set aside with charter schools, vouchers, and choice among traditional public schools within and across districts. Rather than voting at the ballot box, markets allow families to vote with their feet, select the schools they prefer without moving households, and, in theory, increase competition that "lifts all boats" (Friedman, 1962; Hoxby, 2002).

The way in which choice and competition affect the market for education depends on the types of schools that families prefer. Measuring preferences can be difficult, however, especially when prices are not available, so that the usual methods (Berry, Levinsohn, and Pakes [BLP], 1995, 2004) cannot be used. Several prior studies have been able to address this problem using revealed preferences from coordinated school enrollment systems (Hastings, Kane & Staiger, 2010; Glazerman & Dotter, 2017; Abdulkadiroğlu, Pathak, Schellenberg, and Walters, 2017;

Abdulkadiroglu, Argawal, and Pathak, 2017). These systems, which often rely on deferred acceptance algorithms (Roth, 1982; Roth & Xing, 1997), use family rankings of schools that are part of the official application processes that determine which students attend which schools. Though not all mechanisms in use are strategy-proof,[1] they all at least partially reflect revealed preferences.

The findings across these four prior studies of schooling preferences are broadly consistent. Each finds that parents prefer schools that are closer to home and have high test score levels. Importantly, however, the three studies that examine school value-added (Hastings, Kane & Staiger, 2010; Glazerman & Dotter, 2017; Abdulkadiroğlu, Pathak, Schellenberg, and Walters, 2017) find that parents do not prefer higher-value-added schools. If true, this would pose a problem for the market efficiency given that value-added is widely considered to be a more valid indicator of school quality than test levels (Kane & Staiger, 2002; Harris, 2011).

One possible reason behind the apparently weak preference for high-value-added schools is value-added measures were not publicized in the way test levels were, so that families are poorly informed. We find evidence of another explanation, however. Specifically, it appears that the estimates in these prior studies are biased due to omitted variables. Prior studies only considered a small number of school characteristics and the omitted ones might have been correlated with school value-added. For the same reason, these prior studies are also incomplete. Any study of demand can only analyze preferences for product characteristics for which there is cross-product variation and where that variation can be measured. Having rankings from a strategy-proof mechanism is therefore not enough to identify schooling preferences.

We study preferences and the demand for schooling in arguably the most competitive

[1] The choice mechanism in Hastings, Kane & Staiger (2010), is not strategy-proof, as the authors themselves acknowledge. However, they argue that strategic behavior was unlikely in their case.

school market ever developed in the United States, created by two major policy changes. First, beginning in 2012, most families wishing to have their children attend a public school had to participate in a deferred acceptance algorithm.[2] Second, almost all the schools in New Orleans are now charter schools that have autonomy to operate more independently of government rules and union contracts, and this has led to horizontal product differentiation (Hotelling, 1929; Dixit and Stiglitz, 1977; Spence, 1976; Berry, 1994; Glomm, Harris, & Lo, 2005). The vast majority of these schools were required to provide transportation from anywhere in the city, and none were allowed to charge tuition. Not only do parents have more freedom to choose, but they apparently have a fairly wide variety of options to choose from, and detailed data regarding these options are publicly available (Arce-Trigatti et al., 2015). By studying New Orleans, we can therefore not only study revealed preferences, but avoid the omitted variables problem. New Orleans is perhaps the only city where all the necessary conditions and data exist to identify (stylized) preferences for a large majority of publicly funded schools and families.

An additional advantage of having a wider variety of school characteristics is that we can learn about parent preferences for more of these factors. This allows us to distinguish between two broad types of characteristics. Most school characteristics (e.g., school quality and extracurricular activities) arguably enter directly into the family utility function, so that our estimates about them reflect their preferences. However, other school characteristics pertain to the indirect costs of schooling. Distance/transportation is one example, but, as we show, not the only one. This distinction between utility-generating characteristics and indirect costs becomes

[2] In the wake of Hurricane Katrina, state and local agencies eliminated school attendance zones in New Orleans so that, in principle, the city's students could attend any public school they choose regardless of their home address; however, the choice process was initially decentralized and managed by individual schools. Later, local leaders decided to centralize the process. Only 14 percent of children attend the school nearest their home, and the distance to school attended increased by almost two miles. Pre-Katrina, 47 percent attended the schools they were zoned for.

especially important when we test for heterogeneity in preferences between low- and middle-income families, which has been a focus of prior studies of family preferences for schools (Hastings, Kane, & Staiger, 2010; Glazerman & Dotter, 2017). For low-income families, these indirect costs are more influential over schooling demand.

Given the clear importance of omitted variables, we also propose and implement a new test for it. This *information bias test* is motivated by the classic studies of demand estimation by BLP (1995, 2004), leveraging the fact that omitted product characteristics can only introduce bias if families are at least partially informed about which products have those (omitted) characteristics. Our test involves distinguishing informed from uninformed consumers and testing whether preferences appear different between these sub-groups. We outline the assumptions of this test and implement with the same data from New Orleans.

The next section outlines our discrete choice model and estimation procedures (rank-ordered, conditional, and mixed logit; in some cases, with school-level random effects) and outlines the information bias test. After explaining our many data sources, we present results regarding average demand and preference, heterogeneity. We conclude with discussion about how our findings may help resolve several puzzles in the vast literature on schooling markets.

2 Model and Methods

The choice of schools can be analyzed through a discrete choice random utility model (McFadden, 1984) in which the alternative schools to be ranked are mutually exclusive and finite. Below, we provide an abbreviated model and refer to the reader to prior and more complete discussions by BLP (1995, 2004) and Train (2009).

Families (indexed by i) rank schools to maximize utility, which depends on all of the observed fixed attributes of the school (X_j), the attributes of schools that depend on family characteristics, such as distance to each school (X_{ij}), unobserved school characteristics (ξ_j), and a random individual utility component (ε_{ij}) such that the expected utility from the *j*th school is:

$$U_{ij} = f(X_{ij}, X_j, \xi_j, \varepsilon_{ij}) \tag{1}$$

The random component ε_{ij} follows an iid extreme value distribution so that $\varepsilon_i^* = \varepsilon_{ij} - \varepsilon_{ik}$ follows a logistic distribution for all schools $j = 1 \ldots J$ (McFadden, 1984; Hausman & Ruud, 1984; Train, 2009). (For notational simplicity, we omit the unobserved school characteristics term and combine the X vectors into a single X_j in the equations that follow.)

We assume that the probability of observing the rank ordering $r \equiv (r_1, \ldots, r_J)$ for a given individual *i* is the following product of probabilities:

$$P\left(r|X_j, \beta\right) = \left(U_1 > \cdots > U_J\right) = \left(e^{X_1\beta} / \textstyle\sum_{j=1}^{J} e^{X_j\beta}\right) \cap \ldots \cap \left(e^{X_{J-1}\beta} / \left(e^{X_{J-1}\beta} + e^{X_J\beta}\right)\right) \tag{2}$$

The probability of observing school *j*=1 as the top-ranked school is represented in the first term. The next term is the probability of observing school *j*=2 as the second-ranked school, conditional on *j*=1 being top-ranked, and so on $\forall j \in J$ until the last term where there are only two options left. (The order of the subscript numbers here is arbitrary because any school can be top-ranked, not just *j*=1.) The log-likelihood takes the general form: $LL(\beta) = \sum_N \ln\left(P\left(r|X_j, \beta\right)\right)$ where *N* is the total number of individuals. The model is solved for β via maximum likelihood estimation (MLE). In general, we report Huber-White robust standard errors; however, the conclusions are almost always unchanged when we cluster by the Census block group (available upon request).

This general rank-ordered logit is common when examining stated preferences for consumer goods such as transportation options (e.g., Bhat, 1998; Erdem, 1996). The New Orleans school system is one of the rare market settings where markets clear via a coordinated matching process in which consumers rank their preferred options, reflecting revealed preferences.

We also estimate a mixed logit, similar to Hastings et al. (2010), which treats the parameters of interest as random for each student. Following Train's (2009) notation, the probability of an individual selecting a given option is first simulated as follows:

$$P\big(r|X_j,\theta\big) = \int P\big(r|X_j,\beta\big)f(\beta|\theta)d\beta \tag{3}$$

where $f(\beta|\theta)$ is the mixing function and θ represents the parameters describing the distribution of the parameters of interest (β). Since (3) generally has no closed form solution, this is approximated by taking draws from the assumed distribution using simulated maximum likelihood estimation (SMLE); these simulated probabilities are entered into the log likelihood.

One advantage of the mixed logit is that it relaxes the Independence of Irrelevant Alternatives (IIA) assumption, though, as we show below, the results are generally robust to IIA. Another advantage is that the mixed logit allows us to estimate the distribution of each taste parameter, allowing us to test how varied preferences are across families and how these variances differ across a wide range of school characteristics.

Beyond IIA, one of the main assumptions in all the models is that there are no omitted school characteristics that are correlated with included ones and with the rankings. In other discrete choice settings, researchers have addressed this problem using the BLP method (1995, 2004).[3] However, this method still rests on the restrictive assumption that omitted variables are orthogonal to the includes ones (Ackerberg et al., 2007), The same problem appears with other related methods (Blundell & Powell, 2001; Lewbel, 2004; Wojcik, 2000) and, as we show, in sectors where market prices are unavailable (i.e., those focusing on preferences as opposed to demand per se).

[3] BLP (1995) focus on situations where data is aggregated, while BLP (2004) focuses on the increasingly common situations with data from individual consumers. However, micro data do little to solve the omitted variables problem (Ackerberg, Benkard, Berry, & Pakes, 2007).

We address omitted school characteristics in two ways that are appropriate to the current setting where prices are fixed (at zero). First, we use an unusually extensive set of school characteristic indicators from a publicly-available guide to schools produced each year in New Orleans since 2008. This *Parents' Guide* includes hundreds of pieces of information on every school, including test scores, demographics, extracurricular activities, and other programs available. The data are initially self-reported by schools, but those in charge of the guide also interview school leaders to corroborate and edit some of the data elements. Having this much standardized information reduces the chances of omitted variables bias.

Second, we propose a test for omitted variables bias. Though BLP (1995, 2004) requires the restrictive orthogonality assumption, they do point to a possible solution when they write that unobserved product characteristics present a "difficulty to quantify aspects of style, prestige, reputation, and *past experience* that affect the demand for different products, as well as the effects of quantifiable characteristics … that we simply do not have in our data" (BLP, 1995, p.850-851, *italics* added). As explained below, we use data regarding parents' past experience with schools as a test for whether unobserved school characteristics might bias our preference coefficients.

Suppose consumers demand some product characteristic C_j that is unobservable to the econometrician (and correlated with the included variables X_j). If C_j is also unobservable to consumers (i.e., they are completely uninformed) then it cannot directly influence their product rankings, i.e., C_j is conditionally uncorrelated with the rankings. If we can also identify a variable Z_i that plausibly distinguishes informed from uninformed consumers, then we can use this to test for bias in the preference coefficients. Note that uninformed consumers need not be uninformed about all product characteristics, but only certain identifiable one, especially characteristics that are not contained in public sources such as public web sites and in the *Parents' Guide*, so that some parents can be clearly identified as uninformed.

We present a more formal delineation of this *information bias test* using the following notation from Wooldridge (2013). Suppose, for simplicity, that X_j and C_j are one-dimensional and $\tilde{\beta}_1$ represents the estimate with omitted variables bias. In this case, $E[\tilde{\beta}_1] = \beta_1 + \beta_2\tilde{\delta}$ so that the bias in $\tilde{\beta}_1$ is a product of the influence of C_j on the preference rankings (β_2) and the partial correlation between X_j and C_j (denoted $\tilde{\delta}$). If consumers are informed about C_j, as we would typically assume, then $E[\tilde{\beta}_1] \neq \beta_1$.

However, when consumers are completely uninformed about C_j, $\beta_2 = 0$ and $E[\tilde{\beta}_1] = \beta_1$, meaning that the coefficients on the observed X_j are unbiased. This would hold even if C_j enters consumers' utility because C_j cannot influence rankings if consumers have no information at all to form expectations about options on that dimension. In this extreme case, and assuming that the informed (i) and uninformed (u) consumers can be accurately distinguished through some variable Z_i, then there is no omitted variables bias in the coefficient for the uninformed group, i.e., $E[\tilde{\beta}_1^u] = \beta_1$.

Whether $\tilde{\beta}_1^u - \tilde{\beta}_1^i = 0$ is therefore a valid test for bias under four assumptions: (a) all consumers are well informed, and equally informed, about the characteristics observed by the econometrician (X_j); (b) a subset of consumers is uninformed about the factors that are also unobserved by the econometrician (C_j), and correlated with X_j; (c) a variable Z_i is available to the econometrician that distinguishes consumers who are informed or uninformed about C_j; and

(d) Z_i is orthogonal to preferences.[4]

In our analysis, we identify two variables that appear to meet the assumptions for Z_i and distinguish families by their level of information: whether parents have other children already in school and whether the grade level of the child (K-12) is typically the first grade in the school (kindergarten and grade 9). Families have the least information about their schools before any of their children enter and gain experience with them; both variables leverage this fact, though, as we discuss later, the presence of a sibling likely satisfies the assumptions more clearly that the first-grade-in-school variable.

All four of the above assumptions are therefore plausible in this case and perhaps other situations. Most products have a widely-available source of information describing their characteristics (e.g., through a mechanism such as *Consumers' Reports*). Also, with many products, additional information is obtained mostly from experience in consuming goods and services (Nelson, 1970). Note that, to the degree that any bias remains, the direction is unclear. The direction of the bias depends on whether the included variables are (conditionally) positively or negatively related with the omitted variables.

A final question emerges regarding how to characterize the parameters of interest (β). Since we are dealing with rankings, and there are no prices, these are not demand elasticities. Also, since the rankings reflect not only preferences but the constraints families face, they are not parameters of the utility function either. The most direct interpretation is that the coefficients estimated from (2) reflect the relationship between the school characteristics and the rankings (in odds ratios). They can also be viewed as estimates of a stylized consumer demand function in

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[4] Suppose, alternatively, that Z_i only distinguishes consumers who are relatively information and relatively uniformed. In that case, the latter group may be able to form rational expectations about all school characteristics, which would reduce, but not eliminate the bias.

which the metric of willingness to pay is replaced by the rankings, or as a projection of utility. These issues of interpretation are identical to prior studies, which use the same general methods.

In short, combining this new test with our rich set of school characteristics and a strategy-proof ranking system provides a higher likelihood of identifying parental (stylized) preferences for school characteristics.

3 Data

Our data have several advantages over prior studies. We are able to study revealed preferences from a strategy-proof mechanism with a rich set of school characteristics in a highly-differentiated market, and so do for both elementary/middle and high schools. We specifically study the demand for schooling among New Orleans' families who are considering publicly funded schools, including preferences for academic and non-academic factors as well as the indirect costs families incur in the schooling process. The main outcomes are families' OneApp school rankings and assignments in 2013, the second year the ranking system was used.[5] The covariates are measures of school characteristics timed to reflect the information parents had available at the time they were making the given school decision. Since school usually starts in the fall, this means we use the information available the prior spring of the same calendar year.

The school rankings come from the city's OneApp school application system, in which parents rank schools and this information is used to assign students to schools (Abdulkadiroglu, Pathak, & Roth, 2005; Abdulkadiroglu, 2009). Any parent interested in having their children attend a school in the OneApp had an incentive to fill out the application.[6] In assigning students

[5] When referring to school years, we use the year the school year starts, e.g., the 2013-14 school year is referred to as 2013.

[6] There are multiple rounds of the OneApp for families who are not satisfied with the assigned school in the first round. We use only the first round because this likely to be a more valid reflection of preferences.

to schools, the OneApp gives priority to students currently enrolled in the school, students with siblings in the same school, and those living within the schools' (large) geographic catchment areas.[7]

With rankings data, it is common to have explicit rankings for the top choices while other options are left unranked. This arises in our data as well for several reasons. First, the OneApp system only allows eight schools to be ranked.[8] Second, some families may have been willing to accept only a few public schools because they considered schools outside the OneApp system as their main alternatives. Third, among those families who already had a child in a OneApp school, those who wanted their children to return to that school the following year filled out a different section of the OneApp that did not involve ranking schools. The rank-ordered logit incorporates unranked options in the likelihood function as potential options for each individual. This includes the cases where families only indicate they want to return to their original school, in which case that school is listed as top-ranked and $J-1$ schools are ranked.

Our first measure of academic outcomes is the School Performance Score (SPS) assigned by the state of Louisiana to the vast majority of publicly funded schools.[9] The SPS is based on the average test score levels of students; in high schools, the test scores are combined with data about high school graduation rates. Since 2012, these have been translated into letter grades A-F.

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[7] There were only six such catchment areas in the entire city, averaging about 60 square miles, therefore they are unlikely to play much of a role.

[8] In cases where families do not include their current school in their rankings, the OneApp system automatically adds this school as the last ranked school (e.g., ranked 9th if the family provided eight ranked schools).

[9] We use a lagged SPS score since this is the SPS that was visible to parents when making their schooling decisions. For example, during the spring of 2013, parents choose the schools their children will start in fall of 2013, but they only see the SPS that is based on 2012 test scores. For newer schools, when the lagged score is unavailable, we used the current SPS score. In some situations, the "new" school is a school that has been taken over by a charter management organization. In this case it would also be possible to use the historic SPS scores under the prior regime. Some results are sensitive to using this method, but they appear to be outliers and not in line with other robustness checks. In 2013, there were two new schools that had no SPS information in 2012 or 2013. These schools are excluded from the analysis.

Since both the numeric score and the star/letter grade were publicly available we approach the analysis in two different ways, using the letter grade and re-scaling the SPS so that a one-unit increase is approximately equal to a one letter-grade increase.[10]

Since the publicly reported SPS index does not include a measure of school value-added, we calculated this ourselves using now-standard methods.[11] Value-added measures can differ significantly from test levels because of differences in initial test levels when students enter a school (Kane & Staiger, 2002; Harris, 2011). While value-added is a better measure of school quality, there are various reasons why it might not be related to family rankings of schools in practice, as has been found in the studies that have examined them (Glazerman & Dotter, 2017; Abdulkadiroğlu, Argawal, and Pathak, 2017; Abdulkadiroğlu, Pathak, Schellenberg, and Walters, 2017). In addition to the fact that parents do not observe them in Louisiana, school value-added measures tend to include considerable random error (Kane and Staiger, 2002) and they are correlated with test levels, leading to collinearity. The measured relationship between value-added and the school rankings therefore will likely be attenuated.

For the two post-Katrina years studied, the *Parents' Guide* was produced annually by a local non-profit organization and provides detailed school-reported information[12] about

[10] This SPS re-scaling is complicated by the fact that the number of SPS points required to make the next letter grade varies across letter grades. In particular, the highest and lowest grades (A and F) encompass a wider range of SPS scores. However, most schools are in the B-D range and in these cases the ranges are relatively equal at around 15 SPS points in 2013 and 20 SPS points in the earlier years; these are the scale factors we used. We have run robustness checks on which SPS letter grade is used in the regressions instead and find similar results.
[11] Following Kane and Staiger (2008) and others, we estimate the following simple model: $A_{ijt} = \lambda A_{ij,t-1} + \beta X_{ijt} + \theta_j + \varepsilon_{ijt}$ where A_{ijt} is achievement of student *i* in school *j* at time *t*, while X_{ijt} represents one or more student- or school-level covariates. The term θ_j represents the school effect or value-added. This is a large and growing literature on the various methods for value-added estimation. The Kane and Staiger (2008) study and most others focus on individual teachers rather than schools. Kane and Staiger (2008) compare different methods within the context of a randomized trial and we follow their preferred approach, though value-added estimates tend not to be sensitive to the inclusion of covariates or estimation strategy once lagged student achievement in accounted for.
[12] While these data were reported by the schools, the producers of the *Parents' Guide* did carry out some of their own validity checks. In 2013, there are three elementary schools and four high schools without Parents' Guide information and they are therefore dropped from most analyses. These are mostly specialized or alternative schools, which are very rarely actively chosen by parents.

availability of football and other sports, band and other extracurricular activities, school schedules, and other measures. For elementary schools, we also indicate whether the schools offer extended school days or after-school care (free or paid).

Using home and school addresses, we calculated the linear distance between each home and every school that serves the relevant school grade. The role of distance may be non-linear, however. In particular, families might think of the nearest school as the "default" no matter how close it is (Thaler & Bernatzi, 2004).

We also measure whether school locations are "in flux" in the sense that the *Parents' Guide* says they may be moving to a new location in the near future or if they have just recently moved location. This is relevant both for understanding the role of distance, but also the general desire for certainty and stability. Since the city's school reforms were still developing at the time we carried out this study, this is more common than it would be in most settings.

The quality of buildings varies considerably due both to the differences in flooding damage and the fact that some buildings were much older before the storm. With substantial funding from the federal government, the city embarked on a large school construction and renovation initiative. We therefore created a variable equal to zero for schools that were only modestly renovated, equal to one for schools with large renovations, and equal to two for each of the eleven brand new buildings. It is difficult to distinguish the role of building quality from being "in flux," however. The meaning of the in flux variable is clearly different when the school will be moving to a new building versus an older one that received less post-Katrina building funds.

We include a measure of school size for several reasons: (a) some families may prefer the more personal environment of small schools and others may prefer large schools because of the possibility of finding a group of friends to fit in with; (b) some extracurricular activities, such as band and football, require large numbers of student participants; and (c) there are some economies of scale to school size that affect the quality and quantity of services provided (Harris, 2007). To the degree that (a) and (b) hold, the omission of school size would bias estimates for the other variables correlated with it. However, if school size matters because of (c), then omitting it would yield less biased estimates of programs subject to economies of scale because the school size coefficient would absorb some of the preference for the quality of these activities. Since we cannot distinguish among these roles for schools size, we run the models with and without this measure. Schools vary in the number of grades they offer therefore we rely mostly on a measure of the average size of each grade.

Since our student-level data do not contain individual student or family demographic data, we substitute block group characteristics from the Census, especially average median household income. These are from the 2000 Census and the 2007-2011 American Community Survey respectively and are used in the analyses of preference heterogeneity. We considered additional variables but were limited by data and other factors.[13]

Our sample includes roughly 31,000 in 2013, compared with total enrollment of 44,791 (Cowen Institute, 2014). Nineteen schools (mostly run by OPSB) did not participate in the OneApp in 2013, explaining the lower sample size in that case.[14] Since all the selective admissions schools in the city were also in OPSB, this means the average academic ability of students in this analysis is below the city average.

[13] Given the role of race found by Schneider and Buckley (2002) and Glazerman and Dotter (2017), we could have done the same type of analysis based on the racial demographics of schools. However, more than 90 percent of students are racial/ethic minorities. We also considered adding a measure of neighborhood safety, but in exploring this option with local educators, they argued that the safety of the neighborhood was generally disconnected from the safety within schools, which we cannot measure.

[14] Families interested in non-OneApp public schools in 2013 had to apply for admission directly to the schools of interest and these applications are not available to us.

Table 1 provides descriptive statistics for the school characteristics. Here, and in all subsequent tables, elementary and middle school students are combined together because there are essentially no schools with traditional middle school grade structures in the city. For schools whose grades cut across elementary/middle and high school grades, we split the school based on the specific grades students are applying to (e.g., in a school with grades 7-12, grades 7-8 are coded as an elementary analysis and 9-12 are included as a separate high school).

The first part of Table 1 provides information about the school characteristics in elementary school. The average elementary school offered about three different sports and six extracurricular programs, and were given an SPS score of 78.7. Nearly 70 percent of them had an extended school day, with 24 percent offering free aftercare and 20 percent offering paid aftercare. The second part of the table shows similar characteristics for high schools. Nearly 90 percent of high schools offered some combination of band and football, and two-thirds of the current high schools have names similar to schools that existed pre-Katrina, or "legacy schools."

[TABLE 1]

Preference parameters cannot be identified under perfect collinearity, which in this case constitutes economic bundling, i.e., service A is offered if and only if service B is offered. Bundling is a common practice in markets and occurs because goods are jointly produced or complementary in consumption (Adams & Yellen, 1976).[15] In this case, football and band are complementary in consumption and come closest to perfect bundling in this case. There are no schools that have football but not band. Narrowly speaking, we can identify the role of band

[15] A third possible reason is that, in the presence of imperfect information, firms may try to differentiate themselves by offering combinations of services that raise the profile of their brands. For example, a school that wants to be known for its academics may offer a wide array of programs that are seen as preferable to academically minded students, even if they are not jointly produced or complementary in consumption. Also, Nalebuff (2004) shows that in an oligopolistic environment, bundling can be an effective deterrent to market entry.

because there is one school that has band and no football, but this one band program could be atypical (e.g., they might offer band but not marching band, or they may be very small programs).[16] In the analysis, our preferred specification has a variable equal to one when either band or football is offered. This approach requires the fewest assumptions but also provides the least separation in preferences for the two programs. We have nearly complete data on all schools in both the OneApp and enrollment samples.

4 Results

We begin by reporting average preferences for elementary and high schools (separately), and then examine heterogeneity by family income. Next, we implement the information bias test. This is followed by analysis of effect heterogeneity by family income and overall heterogeneity from the mixed logit.

4.1 Average Revealed Preferences from School Rankings

Table 2 reports odds ratios and robust standard errors from a variety of specifications. The first column is our baseline model with the rank-ordered logit, using all the rankings available. Column (2) adds a school random effect to capture unobserved school differences, while column (3) examines preferences related to the top-ranked school (conditional logit). Since the switch to conditional logit and addition of school random effects yield very similar results, we proceed with the rank-ordered logit without the school random effect.[17]

[16] It is also worth noting that there is only one high school with a new building and one high school without a parent group.
[17] The addition of the random effects does roughly double the standard errors. Relative to basic rank-ordered logit in column (1), the direction and significance levels only change for variables that are also sensitive to other specification changes in other columns discussed later. In other words, this has no bearing on our main conclusions.

The next four columns adjust the school characteristics.[18] We add a "nearest school" indicator (column 4) and squared terms for distance and SPS (column 5).[19] These first five columns include a measure of school size (the number of students per grade), while column (6) drops this measure. Finally, column (7) excludes the VAM score. Given the above issues with identification, we draw conclusions about preferences only when the results are generally robust across specifications.

As predicted, parents have strong preferences for particular school characteristics such as measured school performance and distance. Given that the SPS is publicly reported and has limited random error, the positive coefficient on that measure is unsurprising. However, even after controlling for SPS, school value-added is also positively related to school rankings.[20] The fact that value-added is related to preferences even after controlling for SPS conflicts with all the prior research on the topic (Hastings, Kane, & Staiger, 2010; Glazerman & Dotter; 2017; Abdulkadiroğlu, Pathak, Schellenberg, & Walters, 2017). We show later that this is probably partly due to omitted variables bias in these other studies that we are able to address by including a wider range of school characteristics as covariates.

It is useful to compare the relative magnitudes of coefficients. For elementary schools, increasing the SPS by the equivalent of one letter grade on the A-F scale increases the odds of a

[18] Recall that families who wanted to keep their current school were allowed to and they did not have to rank schools. These cases are included in the analysis by making the current school the top-ranked one and leaving the others unranked, as in a conditional logit. Since we have only the top-ranked school for most families, this is one explanation why the results are so similar between the conditional and rank-ordered logit.

[19] In one additional specification (not shown), we also use a separate indicator for schools that have band but not football. In that case, the coefficient on band/football captures preferences for schools that have band and football and the coefficient on "band only" measures preferences for schools with band only. This, too, has little influence on the results.

[20] While coefficients on value-added are much larger than on SPS, these two variables, and therefore their coefficients, are on different scales. A one-unit increase in school value-added means that students can expect their scores to increase by one full standard deviation per year more than the average student. We re-standardized these to the school-level standard deviation, which is only about 0.25; therefore, a one-unit increase in value-added covers nearly the entire range of value-added in New Orleans. On the SPS, a one-unit change is the equivalent of one letter grade (e.g., F to a D where a grade of A is the highest).

school being top-ranked by about 30 percent.[21] Increasing driving distance by one mile reduces the odds of ranking a school highest by about 40 percent (averaging across specifications). Taken together, this means that one letter grade is equivalent to three-quarters of a mile in driving distance.[22]

Even controlling for distance, parents are more likely to send their children to the nearest school. This might suggest that some families view the nearest school as the default choice, even when there is another viable school option only slightly farther away. Small changes in SPS matter less in the lower range (e.g., where F letter grades are given) compared to higher levels. (Non-linear and interaction terms are difficult to interpret in a logit, but we carried out additional tests and find that they do generally reflect the basic structure of the non-linearity in this case.)

[TABLE 2]

We are especially interested in the role of extracurriculars and indirect costs, which prior studies of revealed preferences have not been able to address. Football and band are particularly popular in New Orleans, so it is not surprising that families prefer high schools with these programs. Having either band or football increases the odds of making a high school top-ranked by about 20 percent, about the same as a half-mile of driving distance.

The patterns are similar across elementary and high schools, but there are also some predictable differences. Older children know more about their abilities and have more advanced skills that can develop further with more intensive and specialized extracurricular programs (Brown, 1992) and they are nearing college age when their academic outcomes, such as test

[21] Though the rank-ordered logit considers the entire set of rankings and not just the top ranking, recall that the probability of each set of rankings is sequential (see equation (2)), so this language is both accurate and simple.
[22] The basis for this calculation is not obvious from looking at the table because the absolute difference of the odds ratio from unity has a different meaning for coefficients above and below unity. The comparison of distance and SPS in the text is based on a second set of estimates in which we reverse-coded distance so that longer distances show up as lower values. The results are nearly identical when we replace the re-scaled SPS with the actual letter grade (available upon request).

scores, become more important for college admissions. This may partly explain why preferences for band/football, as well as other sports, are all stronger at the high school level.[23] We expected to see the same pattern with other extracurriculars as well, but families seem to pay little attention or even avoid non-athletic extracurricular programs in high school.

Indirect costs also play a role in the rankings.[24] In addition to transportation costs, parents are legally required to provide adult supervision to young children. Parents can meet this responsibility by sending their younger children to schools that provide after-school care (paid or unpaid). The results reinforce this theory as after-care (paid and unpaid) is consistently important to elementary families.[25]

Families appear to value high schools with a long tradition or "legacy" in the city, dating to the pre-Katrina years. This could be because families want to continue traditions, sending children to the schools that parents or other family members attended. Alternatively, this could reflect established reputations; though the schools now have new operators in the post-Katrina period, they may perceive that having the same name means that is has programs and qualities similar to prior years. The fact that legacy status seems especially important in high school might be because adults in New Orleans tend to identify themselves by the high school they attended.[26]

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[23] As further evidence of this, note that high schools are much more likely to orient their entire schools around specialized academic programs such as the arts or math and science. No elementary schools in New Orleans market themselves that way.

[24] The coefficient on distance may not reflect indirect costs only. For example, parents may want their children to attend schools with other children they know in their own neighborhoods.

[25] Preferences for weekend classes and extended school years are weaker. At the high school level, families seem to strongly prefer not having weekend classes. This could be because the students themselves are playing a role in school choices as they get older, and we would expect few high school students to actively pursue weekend time in school.

[26] This is based on anecdotal audience. It is also consistent with the fact that high school is the last educational institution most parents of New Orleans public school students typically complete. Also, note that one reason families might prefer legacy schools is that the new schools may only re-use the legacy name if it had a good reputation pre-Katrina. That is, the use of legacy names may involve self-selection by school operators.

Elementary school rankings are lower when schools are "in flux" (i.e., have recently changed locations and/or have plans to do so in the near future), although the role of this factor seems small in magnitude compared with the others. The estimates of the role of new and refurbished school buildings are erratic in both elementary and high school, due in part to the fact that some of the schools recently moved to new buildings, creating collinearity between the new building and in flux variables. When we drop the in flux variable, the preference for new buildings becomes stronger in high school.

In addition to distance, parents give extremely high rankings to schools for one child when a sibling already attends these schools. Having two children in two different schools, for example, would be costly to parents who would then have to go to two different schools for parent-teacher conferences and keep track of two sets of rules, policies, and schedules. The sibling schools seem to matter more at the elementary level, perhaps because younger children have less specialized interests. Younger students may also feel safer with an older sibling walking them to the bus stop[27] and watching out for them at school.

The extreme magnitudes of the sibling coefficients, however, largely reflect the structure of the data rather than actual preferences. First, the OneApp only allows families to indicate which schools siblings attend for the top eight schools. For all the other possible schools, it is implicitly assumed that there are no siblings, which automatically makes the sibling variable a strong predictor of rankings, independent of actual preferences. Second, the sibling coefficients likely capture school unobservables. That is, if there is an unobserved factor that led parents to select a school for one child, that same characteristic likely affects the ranking for the other sibling, regardless of the indirect costs. While this leads to an upward bias in the sibling

[27] For children under the age of nine, parents are legally required to escort their children to and from bus stops or to have an older sibling or designated adult do so.

coefficient (as an estimate of the actual desire to have children in the same schools), it also allows us to test for bias (see information bias test below).

To gauge the relative roles played by academics, extracurriculars, and indirect cost categories, we considered hypothetical combinations of school characteristics and then calculated the increase in odds ratios associated with these sets and the total increase in odds from each group of characteristics. Given the non-linear link function of the logit, we calculated the probability of giving the top ranking to a specific school under each scenario, and subtracted them.[28] For example, for high school #1, having a legacy status and band/football would increase the odds of giving a school the top ranking (average odds ratios: 1.7 for each, in Table 2). In contrast, high school #2 has neither a legacy status nor band/football. But if high school #1 has a C grade and high school #2 has a B grade (average SPS odds ratio: 1.3), then this would partially offset the high school #1 advantage, but not enough to make up for the legacy status and sports teams in high school #1.[29] The probability of selecting high school #1 in this situation is higher than for high school #2, even though high school #2 has a higher school grade.

At the elementary level, suppose school #1 is across the street from one's home and has paid after-school care and a school grade of C, while elementary school #2 is two miles away with a traditional school calendar and a letter grade B. In this scenario, the probability of selecting elementary school #1 is higher than school #2, despite the lower letter grade.[30] These are not extreme examples as they omit a variety of other non-academic factors that would make it even less likely that students choose schools with high letter grades.

[28] Specifically, we used the "margins" command in Stata to calculate the Xβ portion for each scenario (evaluating all other characteristics at their means), then calculated the probability under each scenario and subtracted.
[29] This assumes that the schools are otherwise identical.
[30] Again, averaging across the columns in Table 2A, increasing distance (average odds ratio: 0.7) by two miles and not having after-care (average odds ratio: 1.3) reduces the odds of choosing school #2. This is partially offset by a higher letter grade (average SPS odds ratio: 1.3), but not enough to make up for the indirect cost considerations.

The above results are generally robust across specifications from Table 2. We also estimated the model via mixed logit as a test of the IIA assumption. At the elementary level, these results show that the earlier results are robust to relaxing this assumption (see appendix).[31] The estimates are less precise because they impose fewer assumptions, yielding a few cases where coefficients become insignificant, but the coefficients are of a similar magnitude. None of the coefficients in the elementary analysis is statistically significant and of opposite sign in the mixed logit. In the high school analysis, some coefficients have implausible values and are highly sensitive to specification. This problem is unsurprising since the mixed logit is much more computationally demanding and we have only 15 high schools. See the appendix for more details.

Overall, these results suggest that family utility is influenced by a wide variety of school factors. Though they prefer schools with strong academic performance (as measured by both SPS and value-added), New Orleans public school families with high school students have strong preferences for specialized athletics. Also, even without direct tuition payments, families choosing schools for elementary students apparently incur non-trivial indirect costs that keep them from choosing the schools they perceive to be academically strongest.

4.2 Information Bias Test

Column (1) of Table 3 re-displays the same column from Table 2, while column (2) drops the sibling variable.[32] Dropping the sibling variable has relatively little influence on the results.

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[31] Standard errors are unadjusted in the mixed logit analysis because coefficients are treated as random components. We assume the taste parameters are distributed joint normal. Also, this analysis uses only the top choice, so that it is conditional mixed logit.

[32] Students are only included if the sibling box was checked for one of the siblings for at least one school. There are several reasons why multi-children families would not do so. In addition to the fact that some siblings may be too far apart in age to be in the same school, parents would only check the OneApp sibling box if the family wants a child to switch schools (recall that when a family wants a child to stay in the same school, they need not rank any schools and this means not checking the sibling box). If the family wants only one child to switch, we include the rankings

As noted above, however, we can also use the difference between the sibling (column 3) and non-sibling (column 4) subgroups to test for bias. While the coefficients are different between the two subgroups, their relative sizes, and therefore the broad conclusions of the study about preferences, are not. Column (5) provides the *p*-value for the test for differences between the two.[33]

[TABLE 3]

The information bias involves comparing the coefficients on the easy-to-observe (*Parents' Guide*) characteristics between the sibling and non-sibling samples. For the estimates to pass the test (suggesting limited bias), these coefficients should have small differences in coefficients between the two groups because they are similarly informed and expected to have similar preferences. This is generally what we observe. Almost all the characteristics are easy to observe and their coefficients generally remain similar between the sibling and non-sibling groups.[34] In particular, the coefficients on the SPS, where information is arguably easiest to observe, are nearly identical between the two groups.[35] Overall, these results from the information bias test suggest that our estimates have limited bias.

Information availability varies not only by whether a family has multiple children, but by the grade levels of students. Parents have the least information at the entry grades, especially

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for both children since this will flag both students as siblings. (For purposes here, first-time kindergartners or other students just considering entering a OneApp school are considered "switchers.")

[33] The *F*-tests easily reject the null hypothesis for differences between the two groups, but this is not especially informative given the power of the test and that the theory predicts differences for some variables (e.g. value-added and after-school care), but not others.

[34] The only two coefficients that maintain statistical significance and change direction are the two for aftercare. Perhaps not coincidentally, these variables violate one of the assumptions of the information bias test: that the informed and uninformed groups have the same preferences. The weaker preferences for these two with siblings is expected given that older siblings can take care of younger ones; when the sibling is not older, it may be that families already have alternative care options.

[35] While predictions regarding the other school characteristics are less clear in this test, it is worth noting that the hardest-to-observe measure in our study, school value-added, has a much larger coefficient for the sibling samples for both elementary and high school. This is likely because the measurement error in the school value-added is much lower for informed/sibling group, which reduces attenuation.

kindergarten and grade 9. However, grade is not a strong candidate for the information bias test because there are other reasons, beyond information availability, that might explain why preferences might vary across grades, violation of assumption (d) in our test. First, preferences themselves likely change as children get older. Second, once students enter a school in kindergarten, for example, there will be switching costs involved in changing schools, which also affects the rankings (including whether more than one option is ranked). We therefore leave the estimates by grade to the appendix.

4.3 More on Omitted Variables

The information test suggests that with our more extensive data on school characteristics, there are few signs of omitted variables. Here, we go further and examine what our conclusions would have been had we used data available in prior studies. In other words, we test whether the omitted variables problem in prior studies is of practical consequence.

To test whether our richer data from the Parents' Guide affect estimation of variables such as distance and test scores (SPS), we re-estimated with just the typical, more limited set of school characteristics. These results, presented in Table 4, show that the results are meaningfully different when the additional school characteristics are added. Preference for test scores levels in prior studies has apparently been somewhat over-stated (elementary and high school levels) and preference for value-added has been highly under-stated (high school level only).

[TABLE 4]

4.4 Preference Heterogeneity

Prior studies have tested for heterogeneity in preferences and demand by income (Hastings, Kane, & Staiger, 2010; Glazerman & Dotter, 2017). These analyses suggest that low-income families have relatively low demand for academic quality. However, given the above evidence that the estimation strategy influences the results, especially with regard to preferences for school value-added, these analyses, too, should be reconsidered.

Several theories have been put forth regarding why low-income families might have different preferences, and especially weaker preferences for academic quality. First, adults with low incomes tend to have lower education levels themselves, which may reflect weaker revealed preferences for academics. Second, since low-income students tend to have lower test scores, families may perceive that their children will be more comfortable and/or successful if their classmates are more similar academically.[36] Third, given the correlation between poverty, academic achievement, and high school graduation, low-income families might seek out extracurriculars to keep students motivated to do well in school.

Even among parents with the same schooling *preferences*, there are three reasons to expect lower-income families to have weaker *demand* for academics. This is, again, because of the issue of indirect costs. Diminishing marginal utility from income means that any indirect financial expenditures involved in schooling choices (e.g., child care and transportation) yield greater utility losses for low-income families, making them less willing to incur those costs.[37] Compounding this effect, some of the family inputs in the education production function are part of the household production function. In particular, low-income families are less likely to own

[36] Most research on the topic suggests that low-performing peers are academically better off with higher-performing peers, though there are non-linearities (Hoxby & Weingarth, 2005). In particular, some research on classroom peer effects posits that having classmates with dramatically higher achievement reduces achievement at the low-end of the distribution; also, there may be advantages to being in a somewhat lower-performing school in order to be relatively high in the achievement distribution.
[37] These costs may be non-trivial. For example, a school with one extra hour of class time per day and 200 days per year would save a family $2,000 per year in child care costs (assuming an hourly price of $10 per hour). Since these costs are being paid out of school revenues, this also means schools have fewer resources for academics and other activities. Running counter to the theory low-income families will have higher demand is the fact that, almost by definition, they have a lower opportunity cost of providing child care themselves; however, under general conditions the price effect still dominates and low-income families should have higher demand for school-based child care.

automobiles that are used for many purposes and this increases the marginal cost to families of sending their children to schools further away.[38]

Finally, school choices might differ because low-income families are less well informed about the true characteristics of schools. Social networks are determined partly by income so that low-income families tend to gather information from other low-income families who may be less informed (Akerlof, 1997). Low-income families also tend to have lower levels of education and this may make it more difficult for them to navigate and process any information they do gather. More broadly, people with lower levels of education seem to be more efficient consumers, making decisions more in line with their preferences (Wolfe & Haveman, 2002). Almost all of the above theories point in the same direction: compared with academic performance, extracurriculars and indirect costs should play a relatively larger role for low-income families relative to other families. This can also be framed as a problem of search costs (Glazerman & Dotter, 2017).

We estimate the baseline model from Table 2, separately by Census block group income terciles where the bottom tercile has the lowest neighborhood average median income.[39] The right-hand column of Table 5 displays the test statistics for the differences between the lowest- and highest-income groups.[40] The results are generally consistent with our predictions, especially at the elementary level. The lowest-income families (or, more precisely, those who live in the

[38] This arises because of the fixed costs of owning automobiles, including insurance and some types of repairs.
[39] It is important to recognize that families in the New Orleans public school sector have very low incomes compared with the average population. The vast majority of students is eligible for free or reduced price lunches. So, the "lowest income" group here has very low incomes and the "highest income" group is typically just middle class. The median income for the lowest, middle, and highest income are: $6,500-$23,365, $23,427-$36,154, and $36,613-$250,001, respectively.
[40] One complicating factor in any subgroup analysis within a logit framework is the assumption that the variance of random utility component is the same across groups. In effect, this means we have to assume that the variance of unobservables, which shows up in the residual, are the same across income categories. While this assumption only applies to the variance of the unobservables, and seems plausible, it is still possible that this biases the estimated difference between groups.

lowest-income neighborhoods[41]) with elementary-age children have weaker preferences for SPS. (The point estimates on value-added yield a similar pattern but they are statistically significant only between high-income and middle income.) The indirect costs also affect their choices more: low-income families rank higher those schools with free after-school care, extended days, and weekend classes.[42] The lowest-income families also have much weaker preferences than higher-income groups for paid after-school care, presumably for the same reason. These differences are striking, especially considering the differences across income groups are almost certainly attenuated.[43]

The patterns differ somewhat in high school. Band/football and other sports still seem more important to the lowest-income families (only the latter is significant between lowest and highest, though band/football is significant between middle- and high-income). There are no differences in either SPS or value-added preference and the pattern of coefficients runs in the opposite direction on value-added compared with elementary school.

Our results therefore partially conflict with prior studies. We find mixed evidence on whether low-income families have weaker preferences for academic quality, in an absolute sense. The t-tests in the last column show that coefficients on our academic measures are indistinguishable across income levels in three of the four cases (elementary test levels/SPS

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[41] Recall that we do not have student demographics at the individual level.

[42] In addition to transportation costs, national research shows that low-income families tend to be more socially isolated (Rankin & Quain, 2000; Tigges, Brown, & Green, 1998), so they may simply feel less comfortable sending their children to other parts of the city.

[43] The weaker demand for academics and strong consideration of extracurriculars and indirect costs among low-income families is all the more striking given the setting and data limitations. Our reliance on block group income information adds measurement error that likely attenuates differences between the groups. Also, the range of incomes within the sample is relatively narrow, with the highest-income families in the city often bypassing the public school market for private schools or attending selective admissions public schools that are omitted from the analysis. Therefore the differences in demand by income across the entire population are probably even greater than what we report.

being the exception). In fact, the point estimates on value-added are actually larger for low-income families than those of some of the middle- and higher-income groups.

However, with our wider range of factors, we can see that this test, which other studies have adopted, is not the best one. It is not the absolute difference, but the *relative* size of coefficients across school characteristics, that really matters. In this regard, we reach the same conclusion as prior studies: academic measures are relatively less important than extracurriculars and indirect cost consideration, as predicted. The larger implication is that school choice might not reduce achievement gaps by income because market pressures by low-income families are not sufficiently targeted to academic outcomes.

[TABLE 5]

4. Alternative Methods for Identifying Preferences

We also estimated the preference parameters in several other ways that are more in line with prior research, to test whether the advantages of our data and analysis, with rankings data and more detailed school characteristics, yield substantively different conclusions.

Similar to Hastings et al. (2010), we re-estimated the model using school assignments (similar to enrollments) instead of rankings. In this case, the estimated coefficients reflect both demand and supply, which means that, as estimates of preferences, they are likely biased toward the null; the characteristics of schools that are in high demand should lead to over-subscription in the schools that have those characteristics, making the number of students assigned/enrolled in a school lower than the number who wish to be there. This is what we find. The appendix shows that the coefficients are smaller in absolute magnitude for distance, VAM, and SPS when using enrollments, so that these factors are under-stated when using enrollment as the dependent variable.

Finally, the vast majority of studies of schooling preferences have focused on stated preferences. Our unique data also allowed us to compare revealed preferences to stated preferences, using parent surveys in New Orleans from the same period. Consistent with Manski's comment that "what people say is different form what they do" (Hausman, 2012, p.44), the results for stated preferences are fairly different from those presented above. In particular, the results for stated preferences give the impression of a stronger role for academics, which could reflect "social desirability bias" (Paulhis, 1991; Tourangeau & Deutsch, 2004) or other factors that we discuss.

5 Conclusion

There are numerous threats to the identification of consumer preferences in empirical work (BLP, 1995, 2004). We show that omitted variables bias remains a significant problem in studies of consumer preferences, including in the case of schooling and even when parent rankings are available from strategy-proof deferred acceptance algorithms. We address the omitted variables problem, first, by including the considerable product differentiation across schools in New Orleans unusual schooling context and, second, by developing and implementing an information bias test for bias.

Using these methods, we find, consistent with prior research, that families value schools that have high test score levels and short distances from home. However, our results conflict with prior research that suggests families, especially those with low incomes, are uninterested in higher school value-added. We cannot rule out all other explanations. For example, the correlation between school characteristics might differ in the other contexts that researchers have studied, especially those that have less actual product differentiation than New Orleans. What we can say with some certainty is that, if we had carried out this study using the more restricted variables common to other recent studies, then we would have come to the wrong conclusion about the role of school value-added, arguably the most valid measure of school academic quality. This incorrect conclusion would also been consistent with prior studies.

Studies using a more limited set of school characteristics are also incomplete. Our richer set of school characteristics allows us to show that extracurricular activities and indirect costs are also important to families. While parents are not required to pay tuition and fees in traditional public schools and charter schools, other costs vary indirectly, depending on which schools they choose. In addition to distance and transportation costs, the availability of after-school care is another important indirect cost. Predictably, these indirect costs seem to affect schooling demand for the lowest income families more than others.

These findings have important implications for the influence of increasingly popular school choice policies. The effects of these policies, such as charter schools and vouchers, depend on family preferences for schools. In particular, the fact that parents incur indirect costs and have preferences for a wide variety of factors means that even when schools do compete, it is not based only on academics. Instead, school leaders may have to re-allocate resources away from academics to pay for after-school care and other non-academic services in order to attract families. This would help explain why about half the studies on school choice find no effect of competition on student test scores and the effects tend to be small in magnitude (e.g., Gill & Booker, 2008); and why charter schools (Angrist et al., 2011; Center for Research on Education Outcomes, 2013) as well as vouchers (Abdulkadiroğlu, Pathak & Walters, 2018) have mixed effects on test scores. Parent preferences may not create sufficient pressure to increase these observable metrics.

The results in this analysis could also help explain the growing achievement gap between low- and high-income students (Reardon, 2011). While government funding has become more equalized over time by income (Jackson, Johnson, & Persico, 2015), our estimates suggest that low-income families do seem to weigh academic measures less heavily than their higher income counterparts. This is a significant issue given the arguments, going back to Friedman (1962) and more recently Howell and Peterson (2006), that school choice and competitive markets have the potential reduction in the achievement gaps.

Finally, this analysis may also have implications for other studies of consumer preferences, including those in competitive markets. The strong assumption in BLP (1995, 2004) that omitted product characteristics are orthogonal to included ones does not seem plausible and, at least in this context, its violation can have a substantive influence on the estimated demand and preference parameters.

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Table 1: Elementary School Characteristics

	# Schools	Mean	Std Dev	Min	Max
Panel A: Elementary/Middle Schools					
Distance	50	5.00	1.01	3.79	8.97
SPS (Scaled)	50	5.33	0.98	3.65	7.53
SPS (Score)	50	79.89	14.72	54.80	112.90
School VAM	50	0.05	0.18	-0.29	0.42
Sibling	50	0.00	0.00	0.00	0.01
Extended Day	50	0.70	0.46	0	1
Aftercare (Free)	50	0.24	0.43	0	1
Aftercare (Paid)	50	0.20	0.40	0	1
Weekend Classes	50	0.20	0.40	0	1
Extended Year	50	0.04	0.20	0	1
Legacy School	50	0.72	0.45	0	1
School "In Flux"	50	0.28	0.45	0	1
New Building	50	0.44	0.79	0	2
Parent Group	50	0.88	0.33	0	1
Total Sports	50	3.38	2.57	0	8
Total Extracurriculars	50	5.72	3.89	0	15
Band/Football	50	0.62	0.49	0	1
Band Only	50	0.36	0.48	0	1
Music (non-band)	50	0.62	0.75	0	3
Grade Enrollment	50	64.21	22.44	21.50	142.50
Panel B: High Schools					
Distance	15	5.49	1.30	4.04	7.90
SPS (Scaled)	15	5.37	1.70	1.99	7.45
SPS (Score)	15	80.52	25.48	29.90	111.80
School VAM	15	-0.10	0.32	-0.62	0.69
Sibling	15	0.00	0.00	0.00028	0.00504
Weekend Classes	15	0.27	0.46	0	1
Legacy School	15	0.73	0.46	0	1
School "In Flux"	15	0.40	0.51	0	1
New Building	15	0.27	0.70	0	2
Parent Group	15	0.93	0.26	0	1
Total Sports	15	6.00	1.69	3	9
Total Extracurriculars	15	7.13	3.72	2	14
Band/Football	15	0.87	0.35	0	1
Band Only	15	0.07	0.26	0	1
Music (non-band)	15	0.87	1.25	0	4
Grade Enrollment	15	106.81	48.71	50.75	227.5

Sources: 2013 edition of the *New Orleans Parents' Guide to Public Schools* ; Louisiana Department of Education; Recovery School District. The value-added (VAM) measures are bsaed on authors' calculations.

Table 2A: Estimates of School Choice Parameters (2013-2014)

Elementary/Middle School Students

	Rank Ordered Logit	Random School Effects	Conditional Logit	Nearest School Dummy	Quadratic	No Enrollment	No VAM
Distance	0.717***	-	0.696***	0.761***	0.523***	0.717***	0.720***
	(0.003)		(0.003)	(0.003)	(0.005)	(0.003)	(0.003)
Distance Squared	-	-	-	-	1.032***	-	-
					(0.001)		
Nearest School	-	-	-	2.335***	-	-	-
				(0.055)			
SPS Score	1.290***	1.286***	1.241***	1.289***	0.482***	1.449***	1.338***
	(0.010)	(0.019)	(0.011)	(0.010)	(0.035)	(0.010)	(0.010)
SPS Score Squared	-	-	-	-	1.080***	-	-
					(0.007)		
School VAM	1.958***	1.835***	1.976***	1.923***	1.932***	1.503***	-
	(0.077)	(0.137)	(0.091)	(0.076)	(0.078)	(0.057)	
Sibling	24.472***	-	25.821***	24.643***	23.816***	25.196***	24.722***
	(0.908)		(1.201)	(0.921)	(0.911)	(0.919)	(0.921)
Extended Day	0.951***	0.997	0.967	0.951***	0.991	0.910***	1.083***
	(0.017)	(0.034)	(0.021)	(0.018)	(0.018)	(0.016)	(0.019)
Aftercare (Free)	1.025	1.017	1.147***	1.021	1.076***	1.067***	1.063***
	(0.016)	(0.032)	(0.021)	(0.017)	(0.017)	(0.017)	(0.017)
Aftercare (Paid)	1.253***	1.263***	1.438***	1.204***	1.289***	1.282***	1.454***
	(0.025)	(0.051)	(0.034)	(0.024)	(0.026)	(0.026)	(0.027)
Weekend Classes	1.004	0.980	1.005	1.016	1.004	0.954***	1.044***
	(0.016)	(0.029)	(0.018)	(0.016)	(0.016)	(0.015)	(0.016)
Extended Year	0.896***	0.943	0.899**	0.896***	0.903***	1.157***	0.815***
	(0.032)	(0.061)	(0.039)	(0.032)	(0.032)	(0.038)	(0.028)
Legacy School	1.027*	1.114***	0.908***	1.033**	1.058***	0.978	1.043***
	(0.015)	(0.033)	(0.015)	(0.016)	(0.016)	(0.014)	(0.015)
School "in flux"	0.945***	0.980	0.918***	0.961***	0.989	0.975*	0.911***
	(0.013)	(0.025)	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)
New Building	1.036***	1.028	1.017*	1.047***	1.084***	1.015*	1.031***
	(0.009)	(0.018)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
Parent Group	0.845***	0.877***	0.938**	0.876***	0.829***	0.919***	0.909***
	(0.018)	(0.035)	(0.023)	(0.019)	(0.018)	(0.018)	(0.018)
Total Sports	0.966***	0.969***	0.976***	0.963***	0.962***	0.982***	0.958***
	(0.003)	(0.006)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Total Extracurriculars	1.012***	1.003	1.007***	1.015***	1.012***	1.002	1.010***
	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Band/Football	1.085***	1.134***	1.028	1.090***	1.127***	1.070***	1.079***
	(0.018)	(0.036)	(0.019)	(0.018)	(0.019)	(0.017)	(0.018)
Music (non-band)	1.009	1.025	1.001	0.998	1.027***	0.955***	1.036***
	(0.010)	(0.019)	(0.012)	(0.010)	(0.010)	(0.009)	(0.010)
Grade Enrollment	1.013***	1.012***	1.012***	1.013***	1.013***		1.012***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)		(0.000)
Number of Students	24,493	24,493	24,493	24,493	24,493	24,493	24,493

Notes: All columns except except the column labeled "Conditional Logit" are from rank-ordered logit regressions. Exponentiated coefficients are displayed, robust standard errors are in parentheses. Random effects standard errors are calculated using a 25% bootstrap sample.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%

Table 2B: Estimates of School Choice Parameters (2013-2014)

High School Students

	Rank Ordered Logit	Random School Effects	Conditional Logit	Nearest School Dummy	Quadratic	No Enrollment	No VAM	T-Stat Elementary vs High School
Distance	0.889***	-	0.875***	0.921***	0.697***	0.890***	0.888***	36.59
	(0.004)		(0.005)	(0.005)	(0.009)	(0.004)	(0.004)	
Distance Squared	-	-	-	-	1.021***	-	-	
					(0.001)			
Nearest School	-	-	-	1.783***	-	-	-	
				(0.076)				
SPS Score	1.283***	1.412***	1.267***	1.300***	4.199***	1.440***	1.341***	0.14
	(0.046)	(0.084)	(0.050)	(0.045)	(0.775)	(0.017)	(0.049)	
SPS Score Squared	-	-	-	-	0.868***	-	-	
					(0.019)			
School VAM	2.119***	2.459***	2.055***	1.857***	15.080***	2.077***	-	0.60
	(0.268)	(0.600)	(0.317)	(0.240)	(5.163)	(0.257)		
Sibling	4.820***	-	6.729***	4.645***	4.508***	4.812***	4.802***	13.60
	(0.547)		(0.962)	(0.533)	(0.512)	(0.548)	(0.548)	
Extended Day	-	-	-	-	-	-	-	
Aftercare (Free)	-	-	-	-	-	-	-	
Aftercare (Paid)	-	-	-	-	-	-	-	
Weekend Classes	0.668***	0.709***	0.822***	0.662***	0.939	0.711***	0.618***	8.31
	(0.031)	(0.052)	(0.049)	(0.030)	(0.063)	(0.030)	(0.028)	
Extended Year	-	-	-	-	-	-	-	
Legacy School	1.771***	1.778***	1.522***	1.812***	2.798***	1.680***	1.495***	11.17
	(0.082)	(0.148)	(0.086)	(0.084)	(0.251)	(0.073)	(0.054)	
School "in flux"	1.818***	2.036***	1.663***	1.700***	1.489***	1.911***	1.457***	11.62
	(0.099)	(0.200)	(0.113)	(0.094)	(0.085)	(0.099)	(0.056)	
New Building	0.833	1.202	1.128	0.827	0.605***	1.326***	0.826	1.51
	(0.120)	(0.269)	(0.176)	(0.114)	(0.083)	(0.034)	(0.123)	
Parent Group	1.018	1.220	1.178	0.972	0.988	1.297**	1.050	1.41
	(0.133)	(0.258)	(0.167)	(0.124)	(0.122)	(0.137)	(0.139)	
Total Sports	1.005	1.152**	1.091*	1.015	0.881***	1.149***	0.985	0.95
	(0.042)	(0.080)	(0.050)	(0.041)	(0.038)	(0.013)	(0.043)	
Total Extracurriculars	0.934***	0.871***	0.916***	0.941***	0.783***	0.881***	0.993	3.48
	(0.021)	(0.033)	(0.023)	(0.021)	(0.026)	(0.011)	(0.020)	
Band/Football	1.270*	1.515**	1.714***	1.180	3.457***	1.715***	1.105	1.24
	(0.160)	(0.270)	(0.241)	(0.142)	(0.693)	(0.136)	(0.136)	
Music (non-band)	0.748***	0.751***	0.752***	0.767***	0.914***	0.744***	0.773***	13.46
	(0.015)	(0.025)	(0.020)	(0.016)	(0.028)	(0.015)	(0.015)	
Grade Enrollment	1.008***	1.001	1.006**	1.008***	1.018***	-	1.008***	1.97
	(0.002)	(0.004)	(0.003)	(0.002)	(0.003)		(0.002)	
Number of Students	6,788	6,788	6,788	6,788	6,788	6,788	6,788	

Notes: All columns except except the column labeled "Conditional Logit" are from rank-ordered logit regressions. T-statistic is estimated by pooling the elementary and high school students into a single rank-ordered logit regression and interacting each variable with a high school indicator variable. The reported t-statistic is the t-statitic on this multiplicative interaction term. Exponentiated coefficients are displayed, robust standard errors are in parentheses. Random effects standard errors are calculated using a 25% bootstrap sample.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%

Table 3: Preferences for School Choice - Siblings and non-Siblings (2013-14)

	Elementary/Middle School Students					*High School Students*				
	Full Sample					Full Sample				
	W/ Sibling Indicator	W/O Sibling Indicator	Sibling Only Subsample	No Sibling Subsample	P-value	W/ Sibling Indicator	W/O Sibling Indicator	Sibling Only Subsample	No Sibling Subsample	P-value
Distance	0.717***	0.713***	0.730***	0.708***	0.001	0.889***	0.888***	0.917***	0.879***	0.000
	(0.003)	(0.003)	(0.006)	(0.003)		(0.004)	(0.004)	(0.009)	(0.005)	
SPS Score	1.290***	1.260***	1.243***	1.266***	0.356	1.283***	1.284***	1.420***	1.250***	0.193
	(0.010)	(0.010)	(0.021)	(0.011)		(0.046)	(0.045)	(0.128)	(0.048)	
School VAM	1.958***	1.993***	2.494***	1.881***	0.002	2.119***	2.099***	5.131***	1.642***	0.000
	(0.077)	(0.077)	(0.203)	(0.082)		(0.268)	(0.265)	(1.433)	(0.235)	
Sibling	24.472***	-	-	-		4.820***	-	-	-	
	(0.908)					(0.547)				
Extended Day	0.951***	0.965**	0.814***	1.015	0.000	-	-	-	-	
	(0.017)	(0.017)	(0.031)	(0.021)						
Aftercare (Free)	1.025	1.035**	0.767***	1.130***	0.000	-	-	-	-	
	(0.016)	(0.016)	(0.026)	(0.020)						
Aftercare (Paid)	1.253***	1.242***	0.816***	1.395***	0.000	-	-	-	-	
	(0.025)	(0.025)	(0.034)	(0.031)						
Weekend Classes	1.004	1.010	1.011	1.018	0.846	0.668***	0.671***	0.568***	0.706***	0.057
	(0.016)	(0.015)	(0.032)	(0.018)		(0.031)	(0.031)	(0.058)	(0.037)	
Extended Year	0.896***	0.904***	0.869*	0.920**	0.496	-	-	-	-	
	(0.032)	(0.031)	(0.064)	(0.036)						
Legacy School	1.027*	1.027*	1.100***	1.011	0.016	1.771***	1.768***	2.655***	1.575***	0.000
	(0.015)	(0.015)	(0.034)	(0.017)		(0.082)	(0.082)	(0.262)	(0.083)	
School "in flux"	0.945***	0.952***	0.954	0.952***	0.940	1.818***	1.811***	2.704***	1.640***	0.000
	(0.013)	(0.013)	(0.029)	(0.015)		(0.099)	(0.098)	(0.350)	(0.100)	
New Building	1.036***	1.018**	1.119***	0.994	0.000	0.833	0.843	0.646	0.908	0.413
	(0.009)	(0.009)	(0.021)	(0.010)		(0.120)	(0.121)	(0.250)	(0.139)	
Parent Group	0.845***	0.848***	0.733***	0.879***	0.000	1.018	1.035	0.626	1.122	0.159
	(0.018)	(0.017)	(0.032)	(0.020)		(0.133)	(0.135)	(0.245)	(0.154)	
Total Sports	0.966***	0.968***	0.955***	0.970***	0.062	1.005	1.009	0.956	1.025	0.559
	(0.003)	(0.003)	(0.008)	(0.004)		(0.042)	(0.042)	(0.105)	(0.046)	
Total Extracurriculars	1.012***	1.012***	1.048***	1.003	0.000	0.934***	0.933***	0.861**	0.953**	0.559
	(0.002)	(0.002)	(0.004)	(0.002)		(0.021)	(0.021)	(0.054)	(0.023)	
Band/Football	1.085***	1.071***	1.129***	1.054***	0.069	1.270*	1.274*	1.127	1.314**	0.665
	(0.018)	(0.017)	(0.038)	(0.019)		(0.160)	(0.159)	(0.370)	(0.178)	
Music (non-band)	1.009	1.006	0.967	1.015	0.045	0.748***	0.749***	0.688***	0.766***	0.017
	(0.010)	(0.010)	(0.020)	(0.011)		(0.015)	(0.015)	(0.027)	(0.018)	
Grade Enrollment	1.013***	1.013***	1.020***	1.011***	0.000	1.008***	1.008***	1.008	1.008***	0.998
	(0.000)	(0.000)	(0.001)	(0.000)		(0.002)	(0.002)	(0.006)	(0.003)	
Number of Students	24,493	24,493	2,942	21,551		6,788	6,788	874	5,914	

Notes: Estimates from rank-ordered logit regressions. Exponentiated coefficients, robust standard errors in parentheses. P-values on the difference between sibling and non-sibling preferences come from the interaction of being a member of the sibling subsample with each school characteristic.

*** Significant at 1%, ** Significant at 5%, * Significant at 10%

Table 4: Estimation of Preferences with Omitted School Characteristics

	Elementary/Middle School Students		*High School Students*	
	Full Characteristics	Minimal Characteristics	Full Characteristics	Minimal Characteristics
Distance	0.717***	0.716***	0.889***	0.885***
	(0.003)	(0.003)	(0.004)	(0.004)
SPS Score	1.290***	1.444***	1.283***	1.595***
	(0.010)	(0.009)	(0.046)	(0.019)
School VAM	1.958***	1.512***	2.119***	0.385***
	(0.077)	(0.050)	(0.268)	(0.017)
Number of Students	24,493	24,493	6,788	6,788

Notes: Estimates from rank-ordered logit regressions. Exponentiated coefficients, robust standard errors in parentheses. Full characteristic columns include the full set of characteristics from the first columns of Tables 2A and 2B. Minimal characteristics include only distance, SPS score, and school VAM.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%

Table 5: Preference Heterogeneity by Family Income (2013-14 Rank Ordered Logit)

	Elementary/Middle School Students				*High School Students*			
	Bottom Tercile	Middle Tercile	Top Tercile	Top vs Bottom	Bottom Tercile	Middle Tercile	Top Tercile	Top vs Bottom
Distance	0.714***	0.719***	0.703***	1.70	0.876***	0.888***	0.894***	0.98
	(0.005)	(0.005)	(0.004)		(0.007)	(0.007)	(0.007)	
SPS Score	1.169***	1.311***	1.423***	9.78	1.312***	1.332***	1.268***	0.38
	(0.016)	(0.018)	(0.021)		(0.077)	(0.087)	(0.084)	
School VAM	2.212***	1.661***	2.369***	0.69	2.425***	2.511***	1.507*	1.54
	(0.151)	(0.112)	(0.168)		(0.520)	(0.589)	(0.335)	
Sibling	23.397***	24.574***	23.716***	0.14	4.477***	4.973***	5.185***	1.54
	(1.400)	(1.554)	(1.836)		(0.728)	(1.090)	(1.219)	
Extended Day	0.998	0.912***	0.923**	1.69	-	-	-	
	(0.032)	(0.029)	(0.031)					
Aftercare (Free)	1.073**	0.998	1.000	1.74	-	-	-	
	(0.030)	(0.028)	(0.029)					
Aftercare (Paid)	1.083**	1.115***	1.586***	7.62	-	-	-	
	(0.039)	(0.039)	(0.056)					
Weekend Classes	1.094***	1.052*	0.850***	6.36	0.762***	0.747***	0.504***	3.56
	(0.029)	(0.029)	(0.025)		(0.059)	(0.063)	(0.043)	
Extended Year	1.237***	0.889*	0.692***	6.46	-	-	-	
	(0.078)	(0.055)	(0.045)					
Legacy School	0.814***	1.084***	1.238***	11.07	1.860***	1.836***	1.593***	1.40
	(0.020)	(0.029)	(0.035)		(0.139)	(0.162)	(0.131)	
School "in flux"	1.133***	0.872***	0.868***	7.58	1.961***	1.751***	1.772***	0.75
	(0.027)	(0.023)	(0.022)		(0.186)	(0.177)	(0.171)	
New Building	1.110***	1.031**	0.981	5.57	1.065	0.981	0.603*	1.56
	(0.017)	(0.016)	(0.016)		(0.256)	(0.255)	(0.166)	
Parent Group	0.762***	0.773***	1.000	5.02	1.001	1.288	1.018	-
	(0.029)	(0.027)	(0.039)		(0.209)	(0.316)	(0.254)	
Total Sports	0.924***	0.994	0.972***	5.75	1.136*	1.002	0.907	2.12
	(0.006)	(0.006)	(0.006)		(0.080)	(0.076)	(0.073)	
Total Extracurriculars	1.007**	1.012***	1.016***	1.87	0.900***	0.897***	0.984	1.53
	(0.004)	(0.004)	(0.003)		(0.035)	(0.037)	(0.043)	
Band/Football	1.071**	1.029	1.194***	2.66	1.403	1.959***	0.871	1.51
	(0.029)	(0.030)	(0.036)		(0.293)	(0.451)	(0.206)	
Music (Non-Band)	1.146***	1.028*	0.870***	11.06	0.707***	0.780***	0.766***	1.60
	(0.020)	(0.017)	(0.016)		(0.025)	(0.029)	(0.028)	
Grade Enrollment	1.012***	1.014***	1.012***	0.51	1.002	1.007*	1.013***	1.82
	(0.001)	(0.001)	(0.001)		(0.004)	(0.004)	(0.005)	
Number of Students	8,096	7,873	7,845		2,045	2,239	2,308	

Notes: All columns are from rank-ordered logit regressions. Income is defined as the median blockgroup income based on data from the 2007-2011 American Community Survey. Terciles are based on the population in the sample. T-statistic is from a pooled regression across all income terciles where all characteristics are interacted with indicator variables for middle and top tercile indicator variables. The reported t-statistic is from the multiplicative interaction with the top tercile indicator variable. Exponentiated coefficients are displayed, robust standard errors are in parentheses.
*** Significant at 1%, ** Significant at 5%, * Significant at 10%