Do Vouchers Lead to Sorting under Random Private School Selection? Evidence from the Milwaukee Voucher Program*

Rajashri Chakrabarti†

Federal Reserve Bank of New York

Abstract

This paper analyzes the impact of voucher design on student sorting in the application and enrollment phases of parental choice. Much of the existing literature investigates the question of sorting where private schools can screen students. However, the publicly funded U.S. voucher programs require private schools to accept all students unless oversubscribed and to pick students randomly if oversubscribed. In the context of a theoretical model, this paper argues that this feature coupled with the absence of topping up of vouchers can preclude sorting by income in the application stage, although there is still sorting by ability. This design can avert sorting by ability in the enrollment stage, but revelation of new monetary costs can lead to sorting by income. Using a logit model and student level data from the Milwaukee voucher program for 1990-94, this study finds robust evidence that this indeed has been the case in reality.

Keywords: Vouchers, Sorting, Cream Skimming

JEL Classifications: H0, I21, I28

*I thank Steve Coate, Ron Ehrenberg, Andy Haughwout, Brian Jacob, George Jakubson, Dean Lillard, Miguel Urquiola, Wilbert van der Klaauw and seminar participants at the Econometric Society Meetings, American Education Finance Association conference, Southern Economic Association Conference for helpful comments and suggestions, and the Program on Education Policy and Governance at Harvard University for postdoctoral support. Scott Nelson and John Grigsby provided excellent research assistance. The views expressed in this paper are those of the author and do not necessarily reflect the position of the Federal Reserve Bank of New York or the Federal Reserve System. All errors are my own.

†Federal Reserve Bank of New York, 33 Liberty Street, New York, NY 10045. Email: Rajashri.Chakrabarti@ny.frb.org

1 Introduction

School choice and especially vouchers are among the most hotly debated instruments of public school reform. Several issues relating to vouchers have received widespread attention in the media as well as popular debate. One of the more important among these is the question of who takes advantage of the choice opportunity. Do vouchers lead to sorting or cream-skimming, that is, a flight of high income and more committed public school students and parents to the private sector? Or do they facilitate the movement of lower income and less able students to the private sector? While studying sorting gives us an idea as to which households are most likely to apply and enroll with vouchers, understanding the phenomenon is important also because sorting can potentially change student composition and peer group in public (and private) schools.

This paper analyzes the impact of voucher design on student sorting, and more specifically investigates whether there are feasible and realistic ways of designing vouchers that can eliminate sorting. Can the absence of private school screening obviate student sorting or do vouchers lead to sorting even when private schools pick students randomly? Much of the existing literature investigates the question of sorting in a framework where private schools can screen students on the basis of their observed characteristics. But the publicly funded voucher programs in the U.S. (Milwaukee, Cleveland, Florida and Washington DC) are characterized by an absence of private school selection, they typically require private schools to pick students randomly. The ability of private schools to choose their customers undoubtedly acts as a strong force in favor of cream-skimming. Consequently the question arises as to whether vouchers lead to cream-skimming when the private school selection factor is absent and private schools are required to accept students randomly. Another important feature of some of the publicly funded voucher programs in the U.S. (Milwaukee and Florida) is the absence of topping up of vouchers, that is parents do not pay anything over and above the voucher amount. Rather, private schools are required to accept vouchers as full payment of tuition. Can the absence of topping up and the acceptance of vouchers as full payment of tuition avert student sorting?

The paper analyzes these questions in the context of each of the two stages of parental choice process,–the application stage and enrollment stage. Studying the two steps separately is useful, as the decisions involved as well as the constraints faced in the two stages are different and they can lead to very different forms of sorting (as the paper finds later). Surprisingly, no paper so far, except Campbell, West, and Peterson (2005) and Howell (2004) distinguish between and analyze separately the application and enrollment phases. Even there "application" refers to application for scholarship (that would entitle successful scholarship applicants to apply to private schools) rather than application to private school. Application to private school involves very different incentives and constraints, as discussed below. Another functional difference is that Campbell et al. (2005) and Howell (2004) were in the context of programs that had different designs and incorporated both private school selection and topping-up.

I study sorting in the context of the first five years of the Milwaukee voucher program. Implemented in 1990, the Milwaukee Parental Choice Program (MPCP) made all Milwaukee Public Schools (MPS) students with family income at or below 175% of the poverty line eligible for vouchers to attend private schools. The program requires the voucher amount to be taken as full payment of tuition by the private schools (that is, vouchers are not allowed to be topped up) and the latter are not permitted to discriminate between students. Specifically, the private schools have to accept all students unless oversubscribed and have to pick students randomly if they are oversubscribed. Even the student application form for the MPCP does not ask any question relating to the race, sex, parents' education, past scores of the student, other prior records (example, truancy, violence) etc. The questions asked are specifically geared only to ascertain whether the student is eligible for the program (that is, satisfies the income cutoff).[1]

The paper develops its argument in the context of a theoretical framework where utility maximizing households care about two goods, school quality and a numeraire good. School quality available to the households can be of two types—public or private. The households are characterized by an income-ability tuple. Ability is taken here as a broad measure that captures the ability of the child and family, and the motivation and commitment of the parents. It can more appropriately be thought of as an "ability" measure of the household rather than the child. The purpose of the model is to analyze the implications of the two crucial features of the program—random private school selection and acceptance of vouchers as full payment of tuition—on student sorting in the application and enrollment stages. Random private school selection alone cannot avert sorting as tuition costs would still generate sorting. The model predicts, in an equilibrium framework, that random private school selection coupled with the absence of topping up can preclude sorting by income in the application stage, although there is still sorting by ability. Note that sorting here is not caused by the supply side factor of private school

[1] It should be noted here that this paper focuses on the first five years (1990-94) of the Milwaukee voucher program. Following a Wisconsin Supreme Court ruling, the Milwaukee program underwent a major shift in 1998, when the religious schools were allowed to participate for the first time in the program. This led to a major expansion of the program, both in terms of the number of choice schools and in terms of the number of choice students (Chakrabarti, 2008a). Due to lack of availability of adequate student level data, this paper focuses on the smaller Milwaukee voucher program and the empirical findings pertain to that part of the program. The theoretical predictions of the paper should continue to hold in the second part of the program also as the key features that drive the results continue to remain the same, but I do not have a way to test their validity in the latter part of the program.

selection. Rather, it is induced by the demand side factor of parental self-selection. In the enrollment stage, revelation of new monetary costs leads to sorting by income.[2] But the design can preclude sorting by ability in the enrollment stage unless new cost in the enrollment stage is exceedingly high.

Using individual level demographic and survey data from Milwaukee for 1990-94[3], the paper investigates sorting in the application and enrollment stages. Implementing a logistic estimation strategy, it first finds that there is no statistically significant difference between the successful and unsuccessful applicants on the basis of a variety of demographic and socio-economic characteristics, suggesting random selection by private schools. Next, using several alternative logistic specifications, I find strong evidence of sorting by ability in the application stage, but no sorting by income. I find evidence of sorting by income in the enrollment stage, but no sorting by ability. The findings are reasonably robust in that they survive several robustness and sensitivity checks.

The findings of the paper have important policy implications. Both the theoretical and empirical analyzes of the paper suggest that random private school selection coupled with acceptance of the voucher amount as full payment of private school tuition can preclude sorting by income in the application stage. This design, however, cannot avoid sorting by ability as it is driven by the differences in interest and motivation of the parents towards their child's education. In contrast, this design can avert sorting by ability in the enrollment stage. On the other hand, in spite of the design of absence of top-ups, monetary cost realizations in the enrollment stage can lead to sorting by income.

The last decade has seen several theoretical and empirical studies that look at the issue of student sorting. For a comprehensive review of this literature, see Levin (1991, 1992, 1998), Cohn (1997), Belfield and Levin (2002, 2005), Belfield (2006), Howell, Peterson, Wolf, and Campbell (2002). Epple and Romano (1998) look at the effect of vouchers on the choice between public and private schools when private schools can screen students by means of tuition discounts. They show that even a simple public-private system (without vouchers) leads to stratification by income and ability, while vouchers exacerbate stratification. Using NELS (National Educational Longitudinal Survey) data, Epple, Figlio, and Romano (2004) find considerable support in favor of this theoretical prediction. In the context of a computational model, Epple and Romano (2008) examine how alternative voucher designs can affect stratification and technical efficiency. Epple and Romano (2003, 2006) study sorting in the

[2] Transportation costs are revealed in the enrollment stage. The application process to the voucher schools starts in February. Accepted households desiring transportation are required to submit a transportation request form to the MPS by May 15. MPS's decision is conveyed to the parents before enrollment. While MPS provides transportation or reimburses transportation costs in some cases, they are not covered in others. Moreover, other hidden costs (often contingent on the specific school) such as fees for extracurricular, social activities, and donations are revealed in the enrollment stage.

[3] For the remainder of the paper, I will refer to school years by the calendar year of the fall semester.

market for higher education. They find a hierarchy of colleges by quality that is characterized by considerable sorting by income and ability. Ferreyra (2007) estimates a general equilibrium model of school quality, residential and school choice where there are multiple school districts and private schools. In the context of this model, she studies the effect of universal vouchers and vouchers restricted to non-sectarian schools. While both programs increase private school enrollment, the expansion is less under non-sectarian vouchers than under universal vouchers.

Hsieh and Urquiola (2006) and McEwan, Urquiola, and Vegas (2008) focus on the nationwide voucher program in Chile and show that wealthier families and families with higher education are much more likely to avail of vouchers. Studying Florida's Tax Credit Scholarship program, Figlio, Hart, and Metzger (2010) find that lower performing students from lower performing schools typically participate in the program. In the context of the CSF (Children's Scholarship Fund) program, Campbell et al. (2005) find that the chances of both application for scholarship and actual voucher take-up increases with mother's education and decreases with family income. In the context of the School Choice Scholarship Foundation (SCSF) program in New York City, Howell (2004) finds that the chances of application for scholarship increases with mother's education and decreases with family income, but the chances of take-up increases with income. The designs of CSF and SCSF were considerably different from the Milwaukee program. Vouchers required topping-up and private schools could decide whether or not to admit the scholarship applicant. Also "application", in their papers, refer to application for the CSF or SCSF scholarship and not application to private schools. A successful application would entitle the applicant to apply to a private school using the scholarship. Hence, unlike in this paper, costs of gathering information on private schools and their programs did not need to be incurred in the application stage for scholarship. On the other hand, enrollment, in their studies, was a combination of application to private school and enrollment in private school (they do not separate the two). Hence, the enrollment stage involved information/time costs of finding the right private school, monetary cost of top-ups, transportation etc. as well as involved selection by private schools.

Peterson, Howell, and Greene (1999), Metcalf (2003) and Belfield (2006) study the Cleveland voucher program. They find that voucher students in Cleveland have higher mother's education and lower income than public school students. Howell et al. (2002) finds that higher income families are more likely to take a voucher in New York City, low income families are more likely to do so in Dayton, with no difference observed in Washington, D.C. In a survey of previous literature, Levin (1998) finds that choosers are more advantaged both educationally and economically than non-choosers.[4] Most of the above studies

[4] Some recent studies analyze the extent of popular support for vouchers. See, for example, Brunner, Imazeki and Ross

explore the question of sorting where private schools can choose their customers. In this paper, I am interested in analyzing the issue of sorting where the supply side private school discrimination factor is absent.

The studies most closely related to this paper are Witte and Thorn (1996) and Witte (2000) (W-T and W respectively from now on). They study the characteristics of students and families in the MPCP and find that the MPCP applicants had lower income, lower prior math scores, higher mother's education and higher parental involvement than their counterparts in the MPS.

This study differs from W-T and W in some fundamental ways. First, this paper analyzes both the application and enrollment stages in the choice process and to that end poses two questions: (i) Out of the eligible students, who chooses to apply and who does not and how do they differ? (ii) Out of the accepted pool of applicants, who chooses to enroll and who does not and how do they differ? To study the second question, I compare, both in terms of summary statistics as well as multivariate logistic analysis, enrolled students with non-enrolled students who were eligible to enroll, but chose not to. While W-T and W study the first question, they do not study the second question. While studying the first question is important, studying the second question, in addition, is necessary to understand the entire choice process of households. In fact, as this paper finds later, dynamics have been very different in the second stage than in the first. Studying the second question is also very relevant,—perhaps more so than the first,—after all, actual enrollment (rather than application) is what determines both private and public peer quality and school quality. Again, it is actual enrollment that determines and shapes future outcomes and returns of students. Moreover, lessons obtained from studying the second question have immense policy relevance and yields some novel insights (as discussed in the paper).

Second, the focus of this paper is to understand the effect of voucher design on student sorting and especially if voucher design can mitigate student sorting. Specifically, it finds that the absence of topping up in the application stage can prevent sorting by income. Even though W-T and W study this stage, they never show this or make this link. Also, unlike these studies, this paper finds that design plays an important role in the enrollment stage as well. Third, unlike W-T and W, this paper has a theoretical counterpart which aims to understand, in a simple framework, the effect of a Milwaukee-type voucher program on student sorting in the two stages and how voucher design might affect them. Fourth, even in the part that overlaps with W-T and W (empirical analysis of the application stage), this paper does a more careful analysis in that the regressions control for school, grade and year indicator variables; allows the different measures of parental involvement to have different effects; and analyzes year-by year effects

(2010) and the references therin.

as well since considering all years together might mask important patterns. Inclusion of school indicator variables allows me to compare applicants and eligible non-applicants within schools. This rules out any role of school specific factors and ensures that the effect of parental actions is not confounded with that of school specific actions (example, enthusiastic administrators encouraging movement, or vice-versa). Moreover, the data on prior test scores pertain to different years, schools and grades. Therefore, it is important to use the corresponding dummies to rule out any contamination of the effects due to year-specific, school specific and/or grade specific factors.

2 The Model

The model consists of a single group of agents—the households. Households are characterized by an income-ability tuple (y, α), where $y \in [0, 1]$ and $\alpha \in [0, 1]$. The "ability" of the household is used here as a broad term that includes the ability of the child, seriousness, motivation, and commitment of the parents, parents' education levels, parents' desire for child's education etc[5]. A household obtains utility (U) from the consumption of the numeraire good (x), school quality (θ) and its ability (α).

The household utility function is assumed to be continuous, separable, and twice differentiable and is given by $U(x, \theta, \alpha) = h(x) + \alpha u(\theta)$. The functions h and u are increasing and strictly concave in x and θ respectively. It follows that households with higher ability, that is, those that are more highly educated and more motivated and committed, have a higher preference (marginal valuation) for school quality, $U_{\theta\alpha} > 0$.[6,7] This is a common assumption in the existing literature. However, to motivate this assumption further and to examine how far this assumption holds up in the Milwaukee data, I investigate whether households with higher mother's education had a higher valuation for education and school quality. The available Milwaukee data allow me to do this. Using wave 1 survey data (described in section 4) for both the eligible MPS students and the choice students, Table 1 finds that households with

[5] In the empirical part of the paper, "ability" is proxied by mother's education, the number of times the household contacted the school for a variety of purposes, whether the household participated in parent-teacher organization related activities, amount of time the parents spent with the child in various activities, and educational expectations of the parents for their child. The latter four measures are likely closely related to the motivation and commitment of the household towards their child's education. So I include "motivation and commitment" in the description of ability here to foreshadow the measures in the empirical part.

[6] The MPCP applicants were mostly elementary school children (approximately 81% were in grades K-5, 6% were in grade 6, 7% were in grade 7, 2% were in grade 8 and the remainder were in high school grades 9-12). Since at the elementary level, it is usually the parents who make the school choice decision (for their children), ability in this paper more appropriately relates to ability of the household, such as parents education, motivation and commitment. On average, it is typically the case that parents who are more educated are more motivated and committed. Note that there may be kids who are high ability (with high test scores), but are uninterested in school. But typically that is not the average scenario. Also, this assumption is more likely to hold at the household level (which is the focus of the paper).

[7] The assumption of separability is made here for simplicity. All results hold if the utility function is not separable, but continue to satisfy standard single crossing conditions. The assumption $U_{\alpha\alpha} = 0$ is also made for simplicity. All results go through under $U_{\alpha\alpha} < 0$.

higher mothers education were more likely to attach higher value to education vis-a-vis each of a number of other attributes like "good job", "money", "religion", "ethnic traditions" and "living conditions", and these differences (between the more and less educated households) are often highly statistically significant. This is true for both the eligible MPS students (MPS students who were eligible to apply, but chose not to) as well as choice students, that is, it holds in my entire sample. Another useful survey question from the same Wave 1 survey also casts some more light into this issue. Respondents were asked how "educational quality at the chosen school" mattered in their school choice decision. Households with more educated mothers were more likely to give greater importance to educational quality at the chosen school in this decision. Approximately 99% of families with mothers "more than high school educated" regarded "educational quality" as "Important or Very important", while 97% and 95% respectively of households with mothers "high school educated" and "less than high school" did so. Moreover, the difference between the lowest and highest education categories is also statistically significant. In other words, it seems that in the context of the Milwaukee sample used in this paper, the assumption that higher ability households had higher preference towards school quality is reasonable.

School qualities available to a household are public school quality and a continuum of (exogenously given) private school qualities. The public school is free and offers exogenously given quality q to all households that choose to attend it. There is a continuum of private schools providing a continuum of qualities $Q \in [\underline{Q}, \bar{Q}]$. Households pay a tuition $T = t \cdot Q$ $(t > 0)$ to attend a private school of quality Q.[8] Each private school is modeled as "passive". This is in keeping with the feature of the Milwaukee voucher program that private schools were not allowed to discriminate between students. They had to accept all students unless oversubscribed and had to accept students randomly when oversubscribed. However, in the voucher program, private schools could choose whether or not to participate. I abstract from this here for simplicity. It should be noted here, though, that the voucher amount compared very favorably to tuition, so the voucher amount was not a constraining factor to private school entry. Consistent with this, Table 2 finds that the characteristics of schools that actually participated in the program (voucher schools) were very similar to those of the schools that elected not to participate (non-voucher schools). Characteristics of these two groups were not statistically different from each other in terms of any of the characteristics listed (except % asian, where the difference was only barely significant at 10% level).

Now, consider the households. A household incurs switching or relocation costs if it decides to switch from the public school to a private school. There are two kinds of relocation costs: (i) non-

[8] Note that private school quality always exceeds public school quality. Otherwise, no household would pay to attend a private school.

monetary or utility costs of relocation, c_1 (example, information and time costs) (ii) monetary costs of relocation, c_2 (example, transportation costs, costs of new uniforms, books, fees for extracurricular and social activities, donations etc.) Information cost includes the cost of accumulating and absorbing information on the MPCP program, the voucher schools, their programs and peer groups, going on school tours, talking to school administrators and other parents etc. Time costs refer to the time that has to be spent on such activities, collecting and filling out the application forms etc.

The paper considers two alternative scenarios: (i) a simple public-private system, where the voucher amount is zero and (ii) a voucher system where vouchers take an exogenously given value v. In the simple public-private system, the household observes the public and private qualities, the tuitions and costs, and chooses the school quality that maximizes its utility. A household choosing private school (or switching to a private school) incurs tuition cost as well as relocation costs (c_1, c_2) discussed above.[9]

The household decision-making process in the voucher system consists of two stages: application stage and enrollment stage. MPS sometimes provides transportation to MPCP households or pays the household a certain amount for their transportation costs. But whether or not transportation costs will be covered is revealed only in the enrollment stage, not in the application stage.[10] Moreover, other monetary costs (costs of new uniforms, fees for extracurricular and social activities, donations etc.) are also revealed in the enrollment stage and are uncertain in the application stage. Consistent with these, I assume that in the application stage households face uncertainty as to whether monetary costs will be incurred in the enrollment stage. They perceive that with a certain probability p' monetary costs $(c_2 > 0)$ will be incurred in the enrollment stage, while with probability $(1 - p')$ no monetary cost will be incurred. Uncertainty is resolved in the enrollment stage and monetary costs are revealed.

Similarly, households face uncertainty in the application stage as to whether or not their application will be successful. They perceive that conditional on applying, their application will be successful (accepted) with probability p, but will be unsuccessful with probability $(1 - p)$[11].

The two stages are modeled as follows: (i) Application stage: A public school household incurs

[9] c_1 in the public-private stage relates to the cost of accumulating information on the private schools rather than voucher schools. For simplicity, I do not differentiate between application and enrollment stages in the pre-program scenario. All results continue to hold if I do.

[10] The MPS requires MPCP households wanting transportation to submit a transportation request form to the MPS by May 15. The MPS corresponds its decision about transportation to the parent before enrollment. If the MPS declines transportation, MPCP households have to provide for their own transportation if they choose to enroll. The application process to MPCP starts in February. In some cases, the households may not know the accept/reject decision by May. In such cases, they are not able to submit a transportation application to the MPS by May 15 and have to provide for their own transportation if they choose to enroll.

[11] I assume that p is not too small. If p is too small, then there is little chance that the applying households will eventually enroll and hence the households do not have much of an incentive to incur relocation costs and apply. In the Milwaukee data, 68% of the applicants were accepted.

information and time costs ($c_1 > 0$) if it chooses to apply. It realizes that its application will be successful with probability p. It also anticipates that conditional on acceptance, monetary costs will be incurred with probability p' with enrollment in the enrollment stage. Facing these costs, the household decides whether or not to apply. Voucher schools conduct their lotteries and successful and unsuccessful applicants are announced.

(ii) Enrollment Stage: Monetary costs are realized. If a household is accepted, it decides whether or not to enroll. In the Enrollment stage, utility costs mentioned above are likely less important. I assume $c_1 = 0$ in the Enrollment stage. Towards the end of the next section, I consider other cost possibilities as well.

3 Characterization of Equilibria

This subsection analyzes the household behavior under the public-private and voucher systems. Under each of the two systems, households make their utility maximizing decisions, given parameters q, v, t, c_1, c_2, p and p'.

3.1 Public-Private System

If the household chooses to go to public school, it gets utility $h(y) + \alpha u(q)$. If it chooses private school, it gets utility $h(y - t.Q^* - c_2) + \alpha u(Q^*) - c_1$, where Q^* is the optimal private school quality choice of household (y, α) given t, c_1 and c_2. A household (y, α) chooses private school iff $h(y - t.Q^* - c_2) + \alpha u(Q^*) - c_1 > h(y) + \alpha u(q)$. Define $D = [h(y - t \cdot Q^* - c_2) + \alpha u(Q^*)] - c_1 - [h(y) + \alpha u(q)]$. Note that $\frac{\partial D}{\partial y} > 0$ and $\frac{\partial D}{\partial \alpha} > 0$, so there is stratification by income and ability before the imposition of vouchers.

For each y and given t, q, c_1, c_2, there exists a unique household $0 < \hat{\alpha_0} < 1$ such that all households with lower ability choose the public school and those with higher ability choose a private school. This $\hat{\alpha_0}$ is the unique solution to $[h(y - t.Q^* - c_2) + \alpha u(Q^*) - c_1] - [h(y) + \alpha u(q)] = 0$, where Q^* is the optimal private school quality choice of the household $(y, \hat{\alpha}(y))$.[12] Since the indirect utility function is continuously differentiable and $D_\alpha > 0$, by the implicit function theorem, $\hat{\alpha} = \hat{\alpha}(y; q, t, c_1, c_2)$ is a continuously differentiable function. Using the implicit function theorem it is straightforward to check that for each income level, the cutoff ability level $\hat{\alpha}$ is increasing in q, t, c_1 and c_2. Given all other parameters, the cutoff ability level varies inversely with y. This is because both higher income and higher ability increase preference towards private schooling.

[12] To save some notation the optimal private school quality choice of the corresponding household is always denoted by Q^*. It is obvious that the value of Q^* will change with income and ability as well as with change of the other parameters across the different systems.

Similarly, for each α and given t, q, c_1, c_2, there exists a unique household $0 < \hat{y}_0 < 1$ such that all households with lower income choose public school and those with higher income choose private school. Again using implicit function theorem $\hat{y} = \hat{y}(\alpha; v, q, t, c_1, c_2)$ is a continuously differentiable function and the cutoff income level (for each ability) is increasing in q, t, c_1, c_2.

3.2 Voucher System

I solve the household decision problem here by backward induction. First consider household decision in the enrollment stage. Conditional on having been accepted, a household chooses whether or not to enroll. Monetary costs are revealed at the beginning of this stage. If monetary cost is zero, households choose to enroll (since there is no cost of enrolling and it has already incurred c_1 in the application stage in hopes of enrolling). More interesting is the case when monetary cost is non-zero (c_2). If a household chooses not to enroll, it gets utility $h(y) + \alpha u(q)$. On the other hand, if it chooses to enroll, it incurs monetary cost c_2 and receives utility $h(y - c_2) + \alpha u(Q^*)$, since vouchers fully cover tuition.

A household (y, α) chooses to enroll iff $[h(y - c_2) + \alpha u(Q^*)] > h(y) + \alpha u(q)$. Define $D_2 = [h(y - c_2) + \alpha u(Q^*)] - [h(y) + \alpha u(q)]$.

For each α and given t, v, q, c_2, there exists a unique household $0 < \hat{y_2} < 1$ such that all households with lower income ($y < \hat{y_2}$) choose not to enroll and those with higher income ($y > \hat{y_2}$) choose to enroll. Households with income $\hat{y_2}$ are indifferent between enrolling or not. For simplicity, I assume households with income $\hat{y_2}$ choose to enroll. Here, $\hat{y_2}$ solves $D_2 = 0$ and $\hat{y_2} = \hat{y}(\alpha; v, q, t, c_2)$ is decreasing in v and increasing in q, t, c_2.

Solving the decision problem backwards, let's now solve the application decision problem of these households. The households anticipate that conditional on applying, they will be accepted with probability p. They also anticipate that conditional on acceptance, if they choose to enroll they will face a monetary cost (c_2) with probability p'.

First, consider the decision of households with $y \geq \hat{y_2}$. A household in this group chooses to apply iff:

$$p\Big[p'[h(y - c_2) + \alpha u(Q^*)] + (1 - p')[h(y) + \alpha u(Q^*)]\Big] + (1 - p)\Big[h(y) + \alpha u(q)\Big] - c_1 > \Big[h(y) + \alpha u(q)\Big]$$

Define $D_1 = p\Big[p'[h(y-c_2)+\alpha u(Q^*)]+(1-p')[h(y)+\alpha u(Q^*)]\Big]+(1-p)\Big[h(y)+\alpha u(q)\Big]-c_1-\Big[h(y)+\alpha u(q)\Big]$. For each α and given t, v, q, c_1, there exists a unique household $0 < \hat{y_1} < 1$ such that all households with lower income choose public school and those with higher income choose to apply. $\hat{y_1}$ solves $D_1 = 0$ and $\hat{y_1} = \hat{y}(\alpha; v, q, t, c_1)$ is decreasing in v and increasing in q, t, c_1. In contrast, consider the households with $y < y_2$. A household in this group chooses to apply iff:

$$p\Big[p'[h(y)+\alpha u(q)]+(1-p')[h(y)+\alpha u(Q^*)]\Big]+(1-p)\Big[h(y)+\alpha u(q)\Big]-c_1>h(y)+\alpha u(q)$$

Proposition 1 *In the application stage, there is sorting by ability, but there may or may not be sorting by income in a Milwaukee-type voucher system (of no topping up).*

All proofs are in appendix A. The intuitive argument here is as follows. Vouchers enhance the choice options of public school households, but applying to private school is associated with a switching cost (information and time cost) c_1. In addition, households with $y \geq \hat{y}_2$ perceive an additional cost. They anticipate that with a certain probability they will incur monetary cost (c_2) in the enrollment stage. Facing these costs only the higher ability households at each income level prefer to apply to a private school. This is because the higher ability households have a higher marginal valuation of school quality and hence are more willing to bear the switching costs to apply to a private (voucher) school. This leads to sorting by ability. Note that sorting by ability here is not caused by the supply side factor of private school selection. Rather, it is induced by the demand side factor of *parental self-selection.*

In any setting, the crucial driving force behind sorting by income is that households with different income are affected differently—while the higher income households find it worthwhile to pay private school tuition, or to top up the voucher amount, the lower income households do not. First, consider the behavior of households with $y < \hat{y}_2$. For these households not only do vouchers serve as full payment of tuition but they also do not anticipate any monetary cost in the enrollment stage. This is because they know they will choose public school in the enrollment stage if the realization of monetary cost is nonzero. Therefore, at each ability level, public school households with $y < \hat{y}_2$ behave symmetrically and there is no sorting by income in the application stage for these households.

Now consider the households with $y \geq \hat{y}_2$. While for these households also, vouchers fully pay tuition, they anticipate a monetary cost with a certain probability in the enrollment stage with enrollment. Households with higher income are more willing to incur this perceived cost,–this leads to a tendency towards sorting by income. There are two possibilities that might occur and which possibility prevails depends on certain parameter (p, c_1) values. <u>Case 1.</u> The first possibility is that at each ability level only the higher income households in this group find it worthwhile to apply (i.e., ($\hat{y}_1 > \hat{y}_2$)).[13] This leads

[13] One point here, though, might deserve more discussion. It might appear counterintuitive at first glance that households with $\hat{y}_2 \leq y \leq \hat{y}_1$ would choose to enroll in the enrollment stage if they were to reach that stage, but their optimum behavior in the application stage (while anticipating the enrollment stage behavior) is to not apply in the application stage. The reason is as follows. In the enrollment stage, c_1 is already sunk and higher private quality is available with certainty, so these households find it worthwhile to incur c_2 and enroll. In contrast in the application stage in addition to the expected monetary cost, there is some uncertainty as to whether or not private school alternative will finally materialize (unlike in the enrollment stage) plus they have to decide whether or not to incur c_1. So they find it worthwhile not to apply in the application stage (though they would have enrolled in the enrollment stage had they found themselves in that position).

to sorting by income within this income group $y \geq \hat{y}_2$. Case 2. On the other hand, if utility cost (c_1) is sufficiently small and probability of acceptance (p) is sufficiently large, all households in this income group ($y \geq \hat{y}_2$) find it optimal to apply. In this case, all households in this income group ($y \geq \hat{y}_2$) behave symmetrically and there is no sorting by income in this group too.

Even if the first possibility prevails ($\hat{y}_1 > \hat{y}_2$, sorting by income in the $y \geq \hat{y}_2$ group), there may or may not be sorting by income taking all income groups together, as there is no sorting by income in the $y < \hat{y}_2$ group. If the second possibility prevails, there is no sorting by income in the application stage. This is because at each ability level there is no sorting by income either in the $y < \hat{y}_2$ or $y \geq \hat{y}_2$ range. To summarize, whether or not there will be sorting by income in the application stage is an empirical question. I address this question further in the empirical part of the paper.

Another potential cost possibility is worth discussing here. One might imagine that information costs are inversely related to income. For example, it might be easier for higher income households to get information about private schools, for example through friends who send their children to private schools already. In such a case, the scenario is similar to that discussed above. There can be two possibilities. The first possibility is that at each ability level only the higher income households choose to apply thus leading to sorting by income. However, one other possibility arises if information cost is low and probability of acceptance is high, in such a case all households choose to apply and there is no sorting by income. Taken together, there may or may not be sorting by income in this scenario too, and whether or not there was sorting by income is an empirical question.

Note that how households came to know of the choice program promises to cast some light into the costs they faced in gathering the information. Using wave 1 survey data (described in section 4), I investigate whether or not there was variation by income in terms of how households learnt about the choice program. Table A1 in the online appendix (available on the website of the journal) presents the results.[14] While the higher income households were more likely to get information from newspapers and private schools, the lower income households were more likely to get this information from the community center and friends or relatives. In general there was not a lot of variation between the different income groups in terms of how they gathered this information. Also, it is not aprioi clear that getting the information from the community center is more costly than that getting from private schools and newspapers. In other words, this table does not find much evidence that getting this information was more costly for one of the income groups. Either way, as emphasized above, whether or not there was sorting by income in the application stage is an empirical question, and I study this in the empirical

[14] All appendix tables can be found in the supplementary on-line material corresponding to this paper.

part of the paper.

Proposition 2 *There is sorting by income in the enrollment stage in a Milwaukee-type voucher system (of no topping up). There is no sorting by ability unless c_2 is too large.*

In the presence of monetary relocation costs, there will be sorting by income, even though there is no tuition cost or topping up of vouchers. This is because at each ability level, only the higher income households find it worthwhile to pay this cost and switch to a private school.

Now consider sorting by ability. The applicant households incurred a relocation cost in the application stage when there was some uncertainty as to whether they will be able to enroll finally. That is, they chose to apply when the payoff was an weighted average of private and (lower) public utilities. In the enrollment stage, they are already accepted,–choosing to enroll gives them private utility with certainty. So, they would be ready to incur a higher cost than that in the application period to enroll. So, if the cost in the enrollment stage is not too high, they elect to enroll. Then, at each income level, there is no difference in enrollment behavior between the different ability households implying that there is no sorting by ability.[15]

Corollary 1 *If $c_1 > 0$ and $c_2 = 0$ in the enrollment stage, there is no sorting by income in the enrollment stage in a Milwaukee-type voucher system (of no topping up). There is also no sorting by ability unless c_1 in the enrollment stage considerably exceeds that in the application stage.*

Here, while there is no monetary cost of enrolling, there is a non-monetary (utility) cost to switching to private schools. Since the successful applicants facing the enrollment decision already incurred a cost in the application stage in hopes of getting into the private school, they are willing to enroll unless the cost in the enrollment stage becomes exorbitantly high. Note that these households incurred non-monetary cost c_1 in the application stage when the private school payoff was available with a probability. In the enrollment stage, since the private school payoff is available with certainty, they are willing to bear a higher cost to enroll in private school. Only if the utility cost becomes exorbitantly high will there be sorting by ability, with households having higher valuation of quality at each income level enrolling in private schools. Since there is no monetary cost, at each ability level, public school households of different income levels behave symmetrically and there is no sorting by income in the enrollment stage.

The findings from proposition 2 and corollary 2 allow us to think of a scenario when there are both monetary and non-monetary costs in the enrollment period. The presence of monetary cost would lead

[15] There is no sorting by income for the random group of households that has a monetary cost realization of zero. Still there would be a net sorting by income in the enrollment stage because the other group that randomly faced a non-zero monetary cost realization would exhibit sorting by income (as outlined above).

to sorting by income. However, there will be no sorting by ability unless the cost in the enrollment stage is very high and far exceeds that in the application stage.

Corollary 2 *If $c_1 > 0$ and $c_2 > 0$ in the enrollment stage, there is sorting by income in the enrollment stage in a Milwaukee-type voucher system (of no topping up). There will be no sorting by ability unless the relocation cost is considerably higher than that in the application stage.*

Given the institutional details and structure of the Milwaukee program, the most reasonable scenario seem to be those represented by proposition 1 in the application stage, and proposition 2 or corollary 2 in the enrollment stage. However, which scenario actually prevails ultimately is an empirical question and can best be answered empirically which I next proceed to do.

4 Data

This paper uses the Milwaukee Parental Choice Program public release data files [Witte and Thorn (1995)]. They contain descriptive data on individual students and schools, student scores for ITBS reading and math and extensive survey data on both MPS students and students who applied to the choice program for the period 1990 through 1994. Both the descriptive data and survey data enable identification of statuses of students (eligible, applied, accepted, enrolled). As noted earlier, I refer to school years by the calendar year of the fall semester. Descriptive characteristics of students consist of personal characteristics (age, birth date, grade, present school, distances to previous and present school) as well as demographic characteristics (gender, ethnicity, whether free/reduced price lunch eligible).

Among the survey data files, wave 1 surveys were mailed in the fall of each year from 1990 through 1994 to all parents who applied for enrollment slots for their children in one of the choice schools for the first time in that year. MPS control group surveys were sent in March of 1991 to a random sample of parents of students in Milwaukee Public Schools. The random selection was done by selecting children with birth dates that were the 15th or the 28th of any month. Among other purposes, the surveys were intended to assess parental knowledge of and evaluation of the choice program, parental valuation of education versus other attributes, the extent of parental involvement in prior public schools, the expectations parents hold for their children etc.

To analyze characteristics of the voucher and non-voucher private schools, I use data from the Private School Surveys (PSS), conducted by the National Center for Education Statistics, an arm of the U.S. Department of Education. These surveys provide information on various characteristics of private schools in Milwaukee and this data were the source of comparison of voucher and non-voucher schools in the last section.

5 Empirical Strategy

The empirical part of the paper seeks to investigate whether there was sorting by ability and/or income in the application and enrollment stages. Household ability is measured by the following set of variables—mother's education, the number of times the parent contacted the school in the prior year over various issues ("contact"), the number of times per week the parent participates in different activities with the child ("child-time"), whether the parents participated in parent-teacher organization and activities in the prior year ("PTO participation"), educational expectations of the parents and prior test scores of the child. Mother's education, contact, child-time, PTO participation and educational expectations are respectively measured on scales of 0-2, 0-3, 0-3, 0-5 and 0-3. Higher values indicate higher levels of the corresponding variable. For a more detailed description of the survey instruments, categories and codes, see online appendix tables A10 and A11. The income measure used here is household income. It is measured in a scale of 0-3. In addition to considering mother's education and household income as variables ranging on a scale of 0-2 and 0-3 respectively, I also run alternative specifications where I consider dummy variables for different levels of mother's education and household income respectively. In such cases mother's education is modeled by three dummies denoting categories: "Mother<High school graduate", "Mother High school graduate" and "Mother>High School Graduate" and income by four dummies denoting categories: "Low income", "Middle income", "Upper-middle income" and "High income". (For a more detailed description of these variables, see online appendix table A11.)

Since the theoretical analysis and predictions relate to the case where the private schools pick students randomly, I first investigate whether this has been the case in practice. To investigate this, I first compare the demographic characteristics, household incomes and ability indicators of choice applicants accepted by private schools ("accepted applicants") with those of choice applicants who failed to get a private school seat ("non-accepted applicants"). Next using the dichotomous variable "accept" (that takes on a value of 1 for successful applicants and 0 for unsuccessful applicants), I estimate a multivariate logit model. The purpose is to investigate whether the probability of success depends on a systematic way on any of the underlying demographic variables, income or ability measures. Random acceptance would obviate any such relationship. If random acceptance is supported by the data, I proceed to investigate whether there was sorting by ability and income in the application and enrollment stages.

Application Stage: To investigate sorting in the application stage, I first compare the household income and different measures of ability of the choice applicants with that of MPS students who were eligible to apply but chose not to apply. Any MPS student who is eligible for free or reduced price lunch is eligible to apply to the MPCP.[16] Therefore, using the random MPS survey, I extract the group of students who were eligible for free or reduced price lunches but did not apply. The group of students who applied to the choice program during 1990-94 form my first group.

Next I estimate a series of logistic regressions to test for sorting in the application stage. I define a dichotomous variable "apply" that takes on a value of 1 for choice applicants and a value of 0 for eligible non-applicants. Stratification by income and ability would respectively dictate a positive relationship between the probability of "apply" and household income and a positive relationship between probability of "apply" and the different measures of ability.[17] In each of these regressions, I control for student specific demographic and socio-economic factors. I use two alternative comparisons to check the robustness of the results. First, pooling together the choice applicants for all the years 1990-94, I compare their characteristics with those of the eligible non-applicants. Second, I do a year-by-year analysis because an aggregate analysis might disguise year specific patterns (if any). Here I compare the characteristics of the choice applicants in each year separately with the characteristics of the eligible non-applicant group.

A disadvantage of the above analysis is that it compares applicants and non-applicants across all schools, so that the effect of parental actions is confounded with that of school-specific actions. To get around this problem, I include school dummies, so that applicants from a certain school are compared to eligible non-applicants from that school only,—this enables me to get rid of any school specific effects (example, enthusiastic administrators encouraging movement and vice-versa). Witte (2000) and Witte and Thorn (1996) compares applicants with non-applicants and do not include school indicator variables.

A note on prior scores of students is in order here. First, a large number of applicants and non-applicants do not have data on prior scores. A reason for this is that the MPS tests students in their second, fifth, seventh and tenth grades, so that there are fewer prior test scores for students who enter the program in the lower grades (Witte & Thorn, 1996). Since there is heavy enrollment in the MPCP

[16] Under the MPCP, all students with household income at or below 175% of the poverty line are eligible to apply for vouchers. Households at or below 185% of the poverty line are eligible for reduced price lunches and those at or below 135% of the poverty line are eligible for free lunches. (Note that the cutoffs take into account both income and the number children in the households.) The cutoff of 175% is not strictly enforced and households within this 10% margin are often allowed to apply (Hoxby (2003)). Moreover, almost 90% of the students who were eligible for free or reduced price lunches also qualified for the free lunch program (Witte (1997)). So there were very few students with household income between 175% and 185% of the poverty line.

[17] To be more precise, stratification by income implies that at each ability level, students with higher income apply, while stratification by ability implies that at each income level, students with higher ability apply. Empirically, the coefficient of relevance for the former is the logit coefficient of income in a regression that controls for ability and other student-specific demographic variables. The coefficient of relevance for the latter is the coefficient of ability in a logit regression that controls for household income and other demographic variables.

in the pre-kindergarten through second grade, a large proportion of the students do not have data on their prior scores. Still another problem is that the students who have data available on the prior scores may not be random samples of the applicant and non-applicant pools respectively. Therefore the initial logistic regressions used to investigate stratification in this study do not include prior test scores. (Results with prior test scores are reported in table 8.)

Second, the prior test scores of applicants (non-applicants) relate to the last *available* test before application (before survey), and in many cases this is not the last year's test. The available prior test scores relate to years 1984-94 while the data relates to applicants for the years 1990 through 1994. Third, the test scores relate to the different grades in which the ITBS was given. The analyses in Witte (2000) and Witte and Thorn (1997) pool the data on the prior scores of applicants and non-applicants, irrespective of the year and grade of test. In addition to doing this, I also include year, school and grade dummies that control for any idiosyncratic behavior relating to any particular year, school or grade.

Another difference with Witte (2000) and Witte and Thorn (1997) is in the use of the ability measures. They aggregate the different measures of parental involvement into a single measure of parental involvement or participation. I, on the other hand, allow the different measures of parental involvement (contact, child-time, PTO participation) to have different effects. Since the use of multiple measures might lead to multicollinearity, I also test for this problem, as discussed in the results section.

Enrollment Stage: To investigate sorting in the enrollment stage, I consider the pool of accepted applicants. First, I compare different measures of ability and household income of enrollees with the corresponding measures of the non-enrollees who were accepted but chose not to enroll. Next, I estimate a set of logistic regressions to test for sorting in the enrollment stage. I define a binary variable "enroll" that takes a value of 1 for successful applicants who chose to enroll and 0 for successful applicants who chose not to enroll. A positive relationship between probability of "enroll" and household income would indicate sorting by income, while a positive relationship between "enroll" and various measures of ability would indicate sorting by ability. These regressions control for student specific demographics and socio-economic factors. As in the application stage, I first pool accepted applicants from all five years, 1990-94, and use logistic regressions to compare the characteristics of the enrollees with those of the non-enrollees. Next, I do a year-by-year analysis (just in case the pooled analysis masks important patterns), where I compare characteristics of enrollees and non-enrollees in each year separately.

Since the above analysis pools enrollees and non-enrollees across all schools, the effect of parental decisions are confounded with school decisions. For example, some public schools may persuade the successful applicants to stay back, while some other schools may be neutral, while still others may even encourage them to enroll in private schools. To get around this problem, I include school fixed effects which allows comparison between enrollees and non-enrollees from the same school.

I next include prior test scores in the logistic regressions to test whether the enrollees and non-enrollees differed in terms of prior test scores. As in the application stage analysis, I also include year dummies to control for the fact that students took the prior tests in different years. Inclusion of school and grade dummies in the (enrollment stage) regressions with prior test scores reduces sample size very drastically, and hence they are not reported in the paper. But the results remain qualitatively similar.

6 Results

The results are arranged in the following order in this section. Subsection 6.1 analyzes whether private schools picked choice applicants randomly as was required by the program. Subsection 6.2 investigates whether vouchers in Milwaukee led to stratification by ability and income in the application stage. Subsection 6.3 investigates stratification by income and ability in the enrollment stage.

6.1 Did Private Schools Pick Applicants Randomly?

Table 3 compares the summary characteristics of the accepted and non-accepted choice applicants in the MPCP during 1990-94. The two groups are very similar and there is no statistically significant difference between the two groups in terms of any of the demographic characteristics, income and ability measures.[18] I also do a year by year comparison of the two groups in terms of each of these variables for the years 1990 through 1994. These results are very similar (and hence are not reported here), —once again there is no statistically significant difference between the two groups.

To investigate whether results from this bivariate analysis continue to hold when the different variables are considered simultaneously, I run a series of logistic regressions. Table 4 reports these results. The dependent variable is a dichotomous variable "accept" that takes on a value of 1 if the student was accepted by an MPCP private school and 0 if the student was unsuccessful in getting a private school seat under MPCP. Since the coefficients from a logit regression are relatively non-intuitive, I also report the corresponding odds-ratios. The first specification (columns (1)-(2)) includes gender, race, mother's education and household income as regressors. The second specification (columns (3)-(4)) adds a more elaborate set of ability measures and a dummy indicating whether the mother was employed full-time.

[18] In addition to comparing the two groups in terms of the overall contact, PTO participation and child-time variables, I also compare them on the basis of each of the component measures that constitute the overall measure. The results remain very similar and hence are not reported here. The component measures are outlined in online appendix table A10, available on the website of the journal.

The third specification (columns (5) and (6)) reports results from a model that, instead of using the scaled variables mother's education and income, includes dummies for different levels of mother's education and household income.[19] The final specification (columns (7)-(8)) includes prior reading and math normal curve equivalent scores as ability measures in addition to mother's education.[20] The results are very similar across all the specifications—none of the variables are statistically significant, indicating that change in none of the variables has a significant effect on the probability of acceptance. The evidence is very robust—not only are the results from the logistic regressions consistent with those from the bivariate analysis, but inclusion or exclusion of variables do not change the coefficients or odds-ratios by much. Also the model chi-square is never significant, indicating that the null hypothesis that the coefficients are zero cannot be rejected.

6.2 Application Stage: Was there Stratification by Income and Ability?

This section investigates whether sorting took place in the application stage. Table 5 compares the summary characteristics of the choice applicants during 1990-94 with that of the eligible non-applicants. Mother's education of the choice applicants is considerably higher than that of the non-applicants and the difference is statistically significant. Consistent with this, the proportion of mothers in the lower levels of education is much lower and the proportion in the higher levels much higher for the applicants and these differences are statistically significant. The picture is similar for the other ability measures. In terms of the number of times the parents contact the child's school on a variety of issues, time spent with the child in different activities (like reading, math, writing, sports etc.), educational expectations for the child, proportion of parents participating in various parent-teacher activities, the applicants score considerably higher than the non-applicants and these differences are always statistically significant. Applicant households are, however, virtually indistinguishable from the non-applicant households in terms of household income and the very small differences between the two groups are never significant. Year-by-year comparisons of the different ability and income measures have very similar results and hence are not reported here.[21] Thus the results obtained from the bivariate analysis are very much consistent with the predictions from proposition 1.

These patterns are mirrored in the graphical analysis (figures 1, 2 and 3). While there is not much

[19] Inclusion of dummies for different categories is useful in that this allows the different categories to affect the probability of application differently. The problem with this formulation, however, is that the presence of too many categorical variables may lead to a degrees of freedom problem.

[20] Inclusion of the other ability measures in this regression leads to a drastic fall in the number of observations, so I do not include them here,—the results however remain similar.

[21] The only difference is that in 1994 the applicant households had economically and statistically higher income than the non-applicants.

difference between the income distribution of the applicants and non-applicants, the distribution of the different ability measures are very different between these two groups. For each of the ability measures, the proportions of choice households in the upper categories are much higher than the corresponding non-applicant proportions while the proportions of applicants in the lower categories are much smaller. This again supports stratification by ability.

I now investigate whether the pattern that arises from the bivariate comparisons and the income-ability distribution figures is confirmed in a multivariate logistic regression framework. Using data for MPCP applicants for 1990-94 and eligible non-applicants, table 6 reports the results from four alternative logit specifications. Columns (1)-(4) pool all the years together, while (5)-(9) do a year-by-year analysis. The dependent variable here is a dummy variable "apply" that takes on a value of one for choice applicants and a value of zero for eligible non-applicants. Each of the columns controls for race and sex of the student, columns (3), (4), (5)-(9) also control for an indicator variable indicating whether mother is employed full-time. All columns of this table report odds-ratios for easier interpretation, the corresponding logit coefficients are available on request. Odds ratio and standard error of the odds ratio are given by e^b and $S.E(b)e^b$ respectively, where b denotes the logit coefficient. Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio.[22] The odds-ratio says by how much the odds of "apply" increases as the explanatory variable is increased by one unit.

The first column includes household income and the most basic ability measure, mother's education. Column (2) includes a more elaborate set of ability measures. These include the number of times the household contacted the child's school in the previous year ("contact"), whether the parents participated in parent-teacher activities ("PTO participation"), the time the parent spends with the child in various activities ("child-time") in a normal week. Column (3) includes educational expectations of the parent for the child and also includes a dummy variable indicating whether the mother is employed full-time. The results are very similar across the different specifications. Household income is never statistically significant and its odds-ratio is always very close to one, indicating that income does not have much effect on the probability of application. On the other hand, all the ability measures are statistically significant (except child-time), with odds-ratios exceeding one by large margins. Consider the logistic regression in column (3). Although child-time is not significant, with odds-ratio exactly equaling unity, the other

[22] Odds-ratio gives the ratio of odds of a one unit change in the explanatory variable and always takes the value e^b, where b denotes the logit coefficient, irrespective of the point of measurement. If p denotes probability, then odds at x is given by $\frac{p(x)}{1-p(x)}$, odds at $(x+1)$ by $\frac{p(x+1)}{1-p(x+1)}$ and the odds-ratio at x by $\frac{\frac{p(x+1)}{1-p(x+1)}}{\frac{p(x)}{1-p(x)}}$. Using the logistic distribution, it can be easily seen that the odds ratio equals e^b, where b is the coefficient.

three ability measures are highly significant. A unit increase in mother's education increases the odds of application by 67%. A unit increase in the variables contact, PTO participation and educational expectations increases the odds of application by 7%, 12% and 62% respectively.

Using the specification corresponding to column (3), column (3′) displays the impact on probability of application if each of the categorical income and ability measures are changed from their minimum to maximum values, while holding every other variable constant at its mean. Having a mother with a college degree rather than one who dropped out of high school increases the probability of application by 0.21. Having a household income of above \$20,000 rather than a household income of below \$5,000 increases the probability of application by 0.04, however note that income is not statistically significant. Moving from the lowest category of the contact variable (parents never contacted the school) to the highest category (parents contacted school 35 or more times in the course of the previous year) increases the probability of application by 0.31. Students with parents who participated in all 5 parent-teacher activities considered have a 0.12 higher probability of applying compared to students whose parents never participated in any such activity. Parents who expect their students to go to graduate school have a 0.26 higher probability of sending their child to a choice school than parents who expect their child to just finish some high school. Spending time with the child, on the other hand, do not seem to have a major effect on the probability of application, which is perhaps not what one would have expected. Column (4) reports odds ratios from a logit regression that instead of using the previous formulation of mother's education and household income, includes dummies for various categories of mother's education and household income. The results are very similar to those obtained above.

Since considering all the five years together may camouflage some distinctive patterns that may be present in one or more years, I next do a year-by-year analysis. Household income is never statistically significant except in 1994 when the odds ratio equals 1.63. In each of the years, mother's education is highly significant, with the odds-ratios ranging from 1.75 in 1992 to 2.58 in 1994. The results for contact, PTO participation, child-time and educational expectations are also similar to those obtained from the earlier analysis.

Since the regressions above have multiple measures of ability, a potential concern here is multicollinearity. Using several methods, I find that multicollinearity is not likely to be a problem. First, the correlation between the different variables never exceed 0.4 and almost always is less than 0.2. Second, the estimates are very robust to dropping of variables. Finally, the variance inflation factor corresponding to the different coefficients never exceed 1.8. A rule of thumb often used is that a variance inflation factor above 10 indicates multicollinearity.

To rule out the effect of any school specific factors, I next run logistic regressions that control for school dummies. These regressions compare choice applicants to eligible non-applicants within the same school. Columns (1) and (2) of table 7 pool all the years together while columns (3)-(7) report results for individual years. All regressions control for race and sex of the student. Column (1) includes mother's education and household income as regressors, while column (2) includes a more elaborate set of ability measures. The regressions for the individual years do not include the wider set of ability measures since this leads to a considerable fall in sample size, (although the results remain similar). Once again, table 7 shows that an increase in each of the ability measures (except child-time) increases the probability of application, in fact the effect for mother's education is now even stronger than before. The pattern in income is very similar to that obtained earlier.

Table 8 looks at the effect of prior reading and math scores of students on the probability of application. These logistic regressions also include mother's education, income, sex and other demographic variables. Since the prior scores pertain to different years, schools and grades, column (2) includes year dummies, column (3) year and school dummies and column (4) year, school and grade dummies. The patterns for mother's education and income are very similar to earlier, although household income is no longer significant in 1994. Prior reading score is never statistically significant. Although prior math score is initially significant, it no longer remains significant after inclusion of year, school dummies (column 3) and year, school, grade dummies (column 4). Moreover, the odds ratios for both prior reading and prior math scores are very close to one. This indicates that not only statistically, but economically also, an increase in prior scores has no effect on the probability of application. Prior scores are often looked upon as ability measures in the literature. However, it should be noted here that the above pattern does not suggest an absence of sorting by ability. While interpreting these results, it should be kept in mind that many students do not have data on prior scores (for example, just the inclusion of prior reading and math scores leads to a fall in sample size from 1638 (table 6, column 1) to 765 (table 8, column 1), and this smaller sample may not be a random sample of the applicants and eligible non-applicants.

It might be worth mentioning here that because of the design of the program, the eligible households are all low income (free or reduced lunch eligible). So a concern here is whether there is enough variation in income in the data and whether the result of absence of sorting by income is an artefact of the program feature of income truncation. To address this question, I first look at the extent of variation in income among eligible families and compare it with that in the whole Milwaukee sample. Figures 4a and 4c respectively graph the distributions of income of eligible households taking all years together (Figure

4a) and considering the years separately (Figure 4c). The graphs reveal that there is quite a bit of variation in the data. I next compare the income distribution of the eligible families to that of all MPS families (Figures 4a and 4b). As might be expected, the variation is less in 4a, but quite close. The reason may be that Milwaukee is a poor city and hence truncating the income range to 175% of the poverty line does not make a lot of difference. I also compute coefficients of variation to get at this issue. The coefficient of variation of income in the whole sample is 0.74 and in the eligible sample is 0.73. Moreover, not only are the income estimates above not statistically significant, but the coefficients are very close to zero and correspondingly the odds ratios are very close to one. So the point estimates also strongly suggest an absence of sorting by income. Also at the risk of foreseeing the results in the next section, I obtain sorting by income in the enrollment stage where (as might be expected) the variation of income is narrower. So, it is not the program feature of income truncation that is driving the absence of sorting by income in the application stage.

The findings obtained in this section can be summarized as follows. There is strong and robust evidence in favor of stratification by ability. Not only is it manifest in simple bivariate and graphical analyses, but is also supported in a logistic framework. This pattern is robust to inclusion of school dummies and holds for a variety of ability measures such as mother's education, contact, PTO participation and educational expectations. There is not much evidence in favor of stratification by income. It is statistically significant only in 1994, even that effect no longer exists after inclusion of year-of-test, school, grade dummies and prior scores. (However, as discussed above, the regressions in table 8 should be interpreted with caution.) Moreover, the odds ratios for income are never very far from one (except 1994), which provides further evidence that income was not a major factor in the application decision. Thus the empirical findings strongly support the predictions obtained from proposition 1.

6.3 Enrollment Stage: Was there Stratification by Income and Ability?

This section investigates whether sorting took place in the enrollment stage. Using the sample of successful applicants during 1990-94, table 10 compares summary characteristics of enrolled students with those of non-enrolled students (students who were successful, but chose not to enroll). The two groups are very similar in terms of various ability measures and they do not differ either statistically or economically in terms of any of the ability measures except time spent with the child in math and sports, both of which are higher for the enrollees. Enrollees and non-enrollees do not differ statistically in terms of mean household income, but means often mask distributional patterns. Splitting the households into different income categories, table 9 finds that while there was not much difference between proportions of enrollees and non-enrollees in the other groups, the proportion of enrollees in the upper middle income group is statistically and economically higher than the corresponding proportion of non-enrollees.[23]

Table 10 reports results from a multivariate logistic regression framework. Columns (1)-(4) pool all the five years together, while columns (5)-(9) do a year-by-year analysis. All columns control for the race and sex of the student and report odds ratios. Column (1) includes mother's education as the only ability measure, column (2) adds a more elaborate set of measures ("contact", "PTO participation", "child-time"), column (3) adds educational expectations. Instead of using mean scaled income and mother's education, column (4) includes dummies for various categories of income and mother's education. Results across the various specifications [columns (1)-(4)] are very similar. There is no evidence of any sorting by ability in terms of any of the ability measures. In contrast, there seems to have been sorting by income. In columns (1)-(3), the odds ratio for income always exceeds 1 and by wide margins in columns (2) and (3), and these latter effects are also statistically significant, although at the 10% level. Column (4) reveals that enrollees were more likely to come from higher income ranges. While the odds ratios for the upper income brackets always exceed one, that for upper-middle income by far exceeds 1 (2.4) and is also highly significant.

Columns (5)-(9) reveal similar patterns for income in the individual years.[24] The odds ratios for income in the individual-year regressions always exceed 1, but are not statistically significant except in 1992. While mother's education is not statistically significant in the latter years, interestingly it is highly significant in the first year after program and the odds ratio by far exceeds 1 (2.61). This may be because less information relating to the program and voucher schools were likely available in the first year and enrollment in the first year was also likely more risky. It is possible that the more educated (and hence better informed and more motivated) mothers found it worthwhile to bear the risk and enroll their children in the first year.

Table 11A includes school dummies, thus enabling comparison between enrollees and non-enrollees within a school. Results remain qualitatively similar. The odds-ratio for income is 1.38, although it is not statistically significant. Regressions in table 11B include prior reading and math normal curve equivalent scores. Column (2) also includes dummies for the year of the test.[25] The results mirror those obtained above. There is no evidence of sorting by ability. But there is evidence in favor of sorting by income. Not only are the effects statistically significant, but the odds ratios are also economically

[23] Year-by-year comparisons yield very similar results and hence are not reported to save space.

[24] Inclusion of more detailed ability measures in the year-by-year regressions in tables 10 and 11A lead to a drastic fall in sample size and often creates degrees of freedom problems. Hence these regressions are not reported here.

[25] Inclusion of school and grade dummies drastically reduces sample size and hence those regressions are not reported here.

meaningful. Caution should, however, be employed while interpreting the results of this table as many students do not have data on prior scores and this smaller sample may not be a random sample of the enrolled and non-enrolled students.

The results obtained from the enrollment stage analysis can be summarized as follows. There is no evidence in favor of sorting by ability except in the first year after program. In contrast, there is evidence in favor of sorting by income and the effects are economically and statistically meaningful in many cases.

To conclude, voucher design matters and can go a long way to alleviate sorting by income. The above analysis suggests that random private school selection along with absence of topping up can preclude sorting by income in the application stage. Sorting by ability in the application stage is harder to prevent, as it is caused by parental self-selection. However, the Milwaukee design can prevent sorting by ability in the enrollment stage. This is because once households have self-selected themselves in the application stage, there is no further sorting by ability in the enrollment stage unless additional costs are substantial.

Two caveats are worth noting here. First, in general, inclusions of additional ability measures (example, Table 10) and school fixed effects (Table 11A), lead to steep falls in the number of observations. A question that naturally arises here is whether these drops in sample size lead to selected samples. In sections 7.1 and 7.2, I investigate whether these declines are non-random and led to selected samples. Second, while there is evidence in favor of sorting by income in the enrollment stage, it is important to emphasize that the estimates for income are not always statistically significant. While the odds ratios for income always exceed one, they are statistically significant in only six out of thirteen cases, and in two out of these six cases they are significant only at the 10% level. This should be kept in mind while interpreting the results for income in the enrollment stage. Also, it remains to be seen whether the results for income will survive the sensitivity tests in the next section, and especially whether the statistical significance is maintained.

7 Robustness Checks

7.1 Are the Results being Driven by Non-Random Missing Data?

A potentially concerning issue is the steep fall in the number of observations as additional variables are included in regressions corresponding to both the Application stage (Table 6) and Enrollment stage (Table 10). This happens due to missing observations for the additional variables (“contact”, “PTO participation”, “Child-time”, and “Educational Expectations”), and may very well not be random. In this section, I look closely at this issue, and if and how problematic this might be.

7.1.1 Application Stage

In this subsection, I focus on the drop in sample sizes in the "Application stage" regressions. As can be seen in Table 6, the inclusion of "contact", "PTO participation" and "Child-time" leads to a drop in sample size from 1638 to 1013; while addition of "educational expectations" leads to a further drop in sample size from 1013 to 905. A valid question is whether these drops are leading to selected samples. Fortunately, this is a testable issue.

Since I have data on mother's education and income for (i) the whole sample (used in Table 6 column (1)); (ii) the sample that has non-missing data on the first set of additional variables and hence included in Table 6 column (2); and (iii) the sample with missing data on this first set of additional variables (that is, the balance sample or the remaining sample)—I can formally test if mother's education and income in the latter two samples are statistically and economically different from the whole sample that I started with (sample used in Table 6 column 1). Columns (1), (2) and (3) in Table A2 report the results from this exercise. I use both the continuous variables for mother's education and income, as well as the dummies denoting different categories of mother's education and income. As can be seen, mother's education and income as well as the proportion in the various mother's education and income categories are very similar in all the three samples. In fact, none of the attributes in the sample that has non-missing data on the additional variables (column 2) are statistically different from that in the whole sample (column 1). The attributes in the sample with missing additional variables (column 3) are also very similar to the corresponding attributes in the whole sample (column 1) and not statistically different in any of the cases except for the proportion in middle income category. With a large number of comparisons, one expects a few to be statistically different from zero even by sheer random variation, so this statistical difference is not concerning.

Column (4) presents the summary statistics for the sample with data on the complete set of additional variables (used in column 3 of Table 6), while column (5) presents these statistics for the sample with missing data on one or more of the complete set of additional variables. As can be seen, the summary characteristics of these samples are very similar to those of the whole sample and not statistically different from these except in only one case.

7.1.2 Enrollment Stage

In this subsection, I investigate the drop in sample sizes in the "Enrollment stage" regressions. As can be seen from Table 10, inclusion of "contact", "PTO participation" and "Child-time" leads to a drop in sample size from 590 to 142; while addition of "educational expectations" leads to a further drop in sample size from 142 to 141. In this subsection, I investigate whether these drops are non-random.

For this purpose, I first investigate whether the summary statistics for mother's education and income in the whole sample in the enrollment stage (used in column (1) of Table 10) differ economically and statistically from those obtained from (i) the sample that has non-missing data on the first set of additional variables and hence used in column (2) of Table 10, and (ii) the sample that has missing data on the additional variables. The results from this analysis are presented in Table A3, columns (1)-(3). As can be seen, the estimates in columns (2) and (3) are very similar to those in column (1), and none of the estimates in columns (2) and (3) differ statistically from those in column (1).

Column (4) presents summary statistics for the sample that has non-missing data on the complete set of additional variables and hence used in column (3) of Table 10; column (5) presents summary statistics for the sample that has missing data on any of the complete set of additional variables. Once again the estimates in columns (4) and (5) are very similar to those obtained in column (1), and none of the estimates are statistically different from the corresponding ones in column (1).

To summarize, the discussion in this section implies that while there are sharp declines in sample sizes for both application and enrollment stage regressions when additional variables are included, none of these drops are problematic in the sense of inducing non-randomness. In fact, there is no evidence that these drops are non-random or lead to a selected sample. This analysis increases confidence in the findings obtained above. Even, in spite of this, it should be noted here that inclusion of these additional ability measures leads to small sample sizes, especially in the enrollment stage analysis, and hence makes it difficult to generalize these findings to other settings. This caveat should be kept in mind while interpreting these results. However, it is comforting that the results for both sorting by income and ability remain qualitatively similar in the various specifications, and hence in the various samples. This attests to the robustness of the findings.

7.2 Are the Inclusions of Fixed Effects Leading to Selected Samples?

A related and valid concern is whether the drop in sample sizes due to addition of school fixed effects (in the fixed effects regressions) is leading to selected samples. Also of importance is how many fixed effects (schools) are used in the various fixed effects regressions, and how many students from each school are included in the sample. If only a few students from each school are included, then this may be problematic—for example, if there is one student in a school then that observation is essentially dropped.

To address these issues, I first investigate how many school fixed effects are used in each of these regressions and what is the distribution of students in these schools. Next, I investigate whether the inclusion of school fixed effects leads to a non-random loss of observations, and hence to selected samples. I do these analyses both for the application stage and enrollment stage fixed effects regressions.

7.2.1 Application Stage

In the application stage, inclusion of fixed effects leads to a drop in sample size from 1638 (Table 6 column 1) to 1250 (Table 7 column 1); correspondingly the number of schools drops from 189 to 123. The fixed effects regression that includes the complete set of ability measures (Table 7 column 2) includes 741 observations and 100 schools. Table A6 shows the distribution of students in these schools. Panel A focuses on the distribution of students in schools included in Table 7 column 1; panel B focuses on the distribution of students in schools included in Table 7 column 2. As can be seen, most schools had more than 5 students included in each school.

I next examine whether the inclusion of fixed effects leads to non-random samples. For this purpose, I first investigate whether the summary statistics for mother's education and income in the whole sample (sample used in Table 6 column 1) differ economically and statistically from those obtained from (i) the sample that includes school fixed effects (used in Table 7 column 1) and (ii) the balance sample for this fixed effects sample, that is part of the whole sample that is dropped due to the inclusion of school fixed effects. Table A4 columns (1)-(3) presents the results. Using both continuous measures of mother's education and income and dummies for different categories of mother's education and income, I find that the statistics from these samples are very similar both economically and statistically.

I next investigate whether the summary statistics of the sample that includes the complete set of ability measures (used in table 6 column 3) economically and statistically differ from those obtained from (i) the sample that includes school fixed effects in addition to the complete set of ability measures (used in Table 7 column 2) and (ii) the corresponding balance sample, that is part of the sample in (i) that is dropped due to the inclusion of school fixed effects. The results for this analysis are presented in Table A4 columns (4)-(6). Again, the estimates in the three columns are very similar to each other and there is no evidence that either of the estimates in columns (5) or (6) statistically differ from those in column (4).

7.2.2 Enrollment Stage

In the enrollment stage, inclusion of fixed effects leads to a drop in sample size from 590 (Table 10 column 1) to 179 (Table 11A column 1); correspondingly the number of schools falls from 146 schools to 32 schools. Table A6 Panel C shows the distribution of students in these schools used in the fixed effects estimation in the enrollment stage (Table 9 column 1). 56% of these schools had between 2-5 students, though most of these schools had 5 students included in the sample; approximately 41% had between 6 and 10 students and 3% of these schools (1 school) had between 16 and 20 students.

Table A5 investigates whether the inclusion of fixed effects in the enrollment stage analysis and the corresponding drop in sample size leads to a non-random sample. Column 1 presents summary statistics from the whole sample used in enrollment analysis (sample used in Table 10 column 1); column 2 presents summary statistics from the sample that includes fixed effects in addition (used in Table 11A column 1); column 3 presents summary statistics from the balance sample, that is the part of the whole sample used in Table 10 column 1 that are lost in Table 11A column 1.[26] The results (Table A5 columns 1-3) show that the statistics in these three samples are very similar to each other both economically and statistically—specifically, there is no evidence that any of the estimates in columns (2) or (3) are statistically different from those in column (1).

To summarize, while inclusion of school fixed effects leads to declines in sample sizes, there is no evidence that the loss of observations leads to non-randomness in the samples in any of the application or enrollment stage estimations. This finding is encouraging and increases confidence in the results. However, at the same time it should be noted that inclusion of fixed effects leads to small sample sizes, especially in the enrollment stage regression, which makes it difficult to generalize these findings to other settings. However, the results remain qualitatively similar even in spite of the drop in sample sizes—this reinforces the robustness of the results.

7.3 Does Clustering by School Affects Results?

As is clear from the above analysis, there are multiple student observations within schools. While the above analysis includes school fixed effects to control for any fixed characteristics within schools, it does not cluster standard errors at the school level. Since it is reasonable to expect standard errors within a school to be correlated, I allow for clustering of standard errors within schools in this section, and investigate whether clustering by school affects results.

The corresponding results (for both the application and enrollment stage analysis) are presented in Table A7. As is expected, clustering standard errors drops observations that have missing school identifiers. Of note here is that implementing clustering in the non-fixed effects regressions does not

[26] The drop in sample size with the inclusion of fixed effects are contributed by two factors: (i) missing school identifiers (ii) absence of variation by "enroll" even in spite of the presence of school identifiers. The latter scenario can arise even if there are multiple student observations in a school, but, for example if all of them are enrollees. In such a case, since there is no variation by "enroll", these observations are dropped.

drop samples to as low as those in the corresponding fixed effects regressions. This is because while clustering requires the presence of non-missing school identifier for an observation to be included, it does not necessitate the presence of variation by "apply" (or "enroll") within a school for observations to be included in the analysis, unlike that in fixed effects regressions.

Table A7 Panel A presents the results for application stage analysis, while Panel B presents the results for enrollment stage analysis. Columns (1)-(4) estimate the same regressions as in Table 6 columns (1), (2), (3) and (4) respectively, except that the standard errors allow for clustering by school level. Columns (5)-(6) correspond to the fixed effects estimates reported in columns (1)-(2) of Table 7, but they also allow the standard errors to be clustered at the school level.

Panel B presents the results for the enrollment stage analysis. Columns (7)-(10) correspond to Table 10 columns (1)-(4), column (9) corresponds to Table 11A column 1—the only difference in Table A7 regression estimations is that they allow for the standard errors to be clustered at the school level in addition.

The results for both the non-fixed effects and fixed effects specifications for each of the application and enrollment stage analyses are very similar to their corresponding non-clustered counterparts, both in terms of economic and statistical significance. This, again, attests to the robustness of the results.

7.4 Are Differences in Family Size Driving Results?

The real income of a family depends critically on the number of children, in addition to income. For example, a family of size four (with two children) and an income of $30,000 has a lower real income than a family of three (with one child) earning the same amount. Moreover, the probability of choosing private school declines with family size, even after controlling for family income (Conley & Glauber, 2006).This is because families are typically reluctant to send one child to private and another to public school, and usually try to provide similar educational opportunities to all children. Therefore, it is reasonable to conjecture that the school choice decision of families depend on their number of children. A question that arises then is whether the estimates for income obtained above are being driven by differences in family size rather than income.

To explore this issue, I re-run both the above application stage and enrollment stage regressions, but now also control for the number of children in each family. The objective is to investigate whether or not the resulting estimates differ qualitatively from those obtained above. The results from these analyses are presented in Table A8. Panel A of Table A8 focuses on the application stage, while panel B focuses on the enrollment stage. Columns (1)-(4) of Table A8 correspond to regression estimates in columns (1), (2), (3) and (4) respectively in Table 6, while columns (5)-(6) correspond to the fixed effects regression estimates in Table 7 columns (1)-(2), the only difference now is that regression estimations reported in this table also control for the number of children in addition. Similarly, columns (7)-(10) in panel B correspond to the enrollment regression results presented in Table 10 columns (1)-(4), and column (11) corresponds to the fixed effects enrollment regression result presented in Table 11A column 1. Once again, the only difference of the estimates in Table A8 Panel B is that these estimates are obtained from regressions that control for the number of children in addition.

The results are encouraging. The estimates for both application and enrollment stages (after controlling for the number of children) are both economically and statistically similar to those obtained above, and all the findings obtained above continue to hold. This also speaks to the robustness of the results, and implies that the results for income above were not driven by differences in family size. Also interesting is the magnitude and statistical significance of the variable "number of children". The estimates for "number of children" in the application stage estimations imply that increases in family size indeed decreases the probability of application, and this variable is highly statistically significant. In contrast, the estimates for "number of children" in the enrollment stage regressions imply that it does not affect enrollment decisions. This is reasonable as *conditional on application (and acceptance)* in a private school, number of children should not affect the decision to enroll.

7.5 Investigating the Role of Distance in Enrollment Decisions

As discussed earlier, monetary costs (especially transportation costs) in the enrollment stage leads to sorting by income. If this is indeed the case, then controlling for transportation cost should wipe out the sorting by income we saw earlier in the enrollment stage. In this section, I investigate whether controlling for transportation costs indeed affects enrollment stage results.

Distance from student's home to private choice school is a reasonable proxy for transportation costs. In this section, I investigate whether controlling for this distance in the above enrollment stage regressions affects results. The results from this analysis are presented in Table A9. Columns (1)-(3) of Table A9 corresponds to columns (1)-(3) of Table 10.[27] Interestingly, the estimates for income are no longer statistically significant, and the magnitude of the odds-ratios also fall. This finding provides suggestive evidence that transportation costs constituted the driving factor behind sorting by income in the enrollment stage.

[27] Fixed effects estimates corresponding to Table 11A column 1 are not presented here as inclusion of transportation costs further reduces the sample size.

8 Conclusions

This paper analyzes the impact of voucher design on student sorting in the application and enrollment phases of parental choice. More specifically, it investigates whether random private school selection and the absence of topping up of vouchers can eliminate student sorting in these two phases. This is the first paper to analyze student choice and sorting separately in the two phases of private school choice,—application to private school and actual enrollment in private school.[28] Studying both the phases is essential to understand the entire parental choice process, as the incentives involved and the constraints faced differ across the two phases. Differentiating between the two phases also yields some novel insights that have important implications for voucher design and education policy.

Much of the existing literature investigates the issue of sorting where private schools can choose their students. However, in the publicly funded voucher experiments in the U.S.—Milwaukee, Cleveland, Florida and Washington DC—the private schools are not permitted, by law, to discriminate between students. The ability of private schools to screen students acts as a strong force in favor of sorting. This study investigates whether vouchers lead to sorting even when the supply side force to sorting (private school screening) is absent.

It focuses on the first five years of the Milwaukee voucher program. The Milwaukee voucher program is characterized by both random private school selection and the absence of "topping up" of vouchers. In the context of an equilibrium model of household behavior, the paper finds that random private school selection coupled with the absence of "topping up" can obviate sorting by income in the application stage, but not sorting by ability. In contrast, this design can eliminate sorting by ability in the enrollment stage. However, revelation of monetary costs in the enrollment stage (as was the case in Milwaukee) can lead to sorting by income in the enrollment stage.

Implementing a logit estimation strategy, using multiple measures of ability and student level data for 1990-94, the paper finds that indeed the application stage was characterized by sorting by ability but no sorting by income; while the enrollment stage was characterized by sorting by income, but no sorting by ability. These results are robust to alternative specifications and various measures of ability.

The paper makes some important contributions to the literature on sorting—(i) It distinguishes

[28] As mentioned in the paper, Campbell et al. (2005) study application and enrollment in the context of CSF, but there "application" meant *application for the scholarship* rather than to a private school, and hence did not involve the non-monetary costs of gathering information on the private schools and their programs, and finding the right private school. Applying to private schools is a whole different story and requires considerable more involvement and commitment. Moreover, enrollment in their case is a combination of application to private school and enrollment in private school (they do not separate these two). In other words, enrollment in their case involved non-monetary costs of finding the right school as well as topping up of scholarships, transportation etc., as well as facing private school selection.

between parental decision making process and sorting in the application and enrollment stages to a private school (ii) it points out, both theoretically and empirically, that random private school selection along with the absence of topping up of vouchers have the potential to preclude sorting by income. While I do find evidence of sorting by income in the enrollment stage in the Milwaukee scenario, this picture may be different in more affluent neighborhoods where small monetary costs (such as transportation costs, fees for extracurricular activities, donations etc.) may not seem salient. In such cases, this design could be successful in preventing sorting by income in the enrollment stage as well. The analysis also implies that policymakers should not underestimate small costs such as transportation costs or fees for extracurricular activities,—covering such costs might make a lot of difference, especially in poor neighborhoods. (iii) it finds (both theoretically and empirically) that while the design cannot prevent sorting by ability in the application stage, it can thwart any further sorting by ability in the enrollment stage.

The findings of the paper have important policy implications. They strongly suggest that voucher design matters and that appropriate design of voucher policy can prevent sorting by income. While random private school selection alone cannot obviate sorting by income, a voucher policy that combines both random private school selection and full payment of tuition by vouchers can preclude sorting by income in the application stage and perhaps also in the enrollment stage, if subsidiary monetary costs in the enrollment stage are also paid for by the voucher or the policymaker. Sorting by ability, however, is considerably difficult to prevent as it is driven by parental self-selection. But while sorting by ability is likely in the application stage, the Milwaukee design (of coupling random private school selection with absence of topping up) can be successful in averting any further sorting by ability in the enrollment stage.

Appendix A: Proofs of Results

Proof of Proposition 1. Sorting by Ability: Before the imposition of vouchers, a public school household that is indifferent to switching to a private school satisfies the following equality: $h(y - tQ^* - c_2) - c_1 + \alpha u(Q^*) = h(y) + \alpha u(q)$. For each income level and given other parameters, there is a unique $\hat{\alpha_0}$ that solves this equation:

$$\hat{\alpha_0} = \frac{h(y) - h(y - tQ^* - c_2) + c_1}{u(Q^*) - u(q)} \qquad \text{A.1}$$

Now let's assume that vouchers are introduced. Consider households with $y \geq \hat{y_2}$.

<u>**Application stage $y \geq \hat{y_2}$:**</u>

Sorting by Ability:

After imposition of vouchers, a household that is indifferent between public and private options in the application stage satisfies

$$p\Big[p'[h(y-c_2)+\alpha u(Q^*)]+(1-p')[h(y)+\alpha u(Q^*)]\Big]+(1-p)\Big[h(y)+\alpha u(q)\Big]-c_1=\Big[h(y)+\alpha u(q)\Big] \qquad \text{A.2}$$

since vouchers are not allowed to be topped up. Note that since private school quality is not costly here, $Q^* = \bar{Q}$, where $\bar{Q}$ is the highest private school quality. Given other parameters, a unique ability level $\hat{\alpha_1}$ solves this equation.

$$\hat{\alpha_1} = \frac{\frac{c_1}{p} + h(y) - [p'h(y-c_2) + (1-p')h(y)]}{u(\bar{Q}) - u(q)} \qquad \text{A.3}$$

The denominator of A.3 is larger than that in A.1. Also note that $h(y) > p'h(y-c_2) + (1-p')h(y) > h(y - tQ^* - c_2)$, since $p'h(y-c_2) + (1-p')h(y)$ is a weighted average of $h(y)$ and $h(y-c_2)$. It follows that if p is not too small, $\hat{\alpha_1} < \hat{\alpha_0}$. This implies that if the probability of enrollment is not too low, higher ability households at each income level are willing to bear the utility cost and apply. (In the Milwaukee data, p=0.68, which is not too small.) Thus, in the income group $y \geq y_2$, vouchers lead to sorting by ability in the application stage even when vouchers fully pay for private school tuition.

Sorting by Income:

In the public-private equilibrium, a household that is indifferent between the public and private options satisfies:

$$h(y - tQ^* - c_2) + \alpha u(Q^*) - c_1 = h(y) + \alpha u(q) \qquad \text{A.4}$$

Given ability level and the other parameters, a unique income level $\hat{y_0}$ satisfies this equation.

Now consider application stage in a Milwaukee-type voucher system. A household that is indifferent between applying or not satisfies A.2. Given ability level and other parameters, a unique income level $\hat{y_1}$ solves this equation. Now, $\hat{y_0}$ solves A.4. This implies:

$$h(\hat{y_0} - tQ^* - c_2) + \alpha u(Q^*) - c_1 = h(\hat{y_0}) + \alpha u(q) \qquad \text{A.4}'$$

Plugging $\hat{y_0}$ in the LHS of A.2:

$$\begin{aligned} & p\Big[p'[h(\hat{y_0}-c_2)+\alpha u(\bar{Q})]+(1-p')[h(\hat{y_0})+\alpha u(\bar{Q})]\Big]+(1-p)\Big[h(\hat{y_0})+\alpha u(q)\Big]-c_1 \\ &= [pp'h(\hat{y_0}-c_2)+(1-pp')h(\hat{y_0})]+[p\alpha u(\bar{Q})+(1-p)\alpha u(q)]-c_1 \qquad \text{A.5} \end{aligned}$$

Now comparing with A.4$'$, it has to be the case that

$$\begin{aligned} [pp'h(\hat{y_0}-c_2)+(1-pp')h(\hat{y_0})]+[p\alpha u(\bar{Q})+(1-p)\alpha u(q)]-c_1 &> h(\hat{y_0}-tQ^*-c_2)+\alpha u(Q^*)-c_1 \\ &= h(\hat{y_0})+\alpha u(q) \qquad \text{A.5}' \end{aligned}$$

if p is not too small. This is because $\bar{Q} > Q^*$,
$pp'h(\hat{y_0} - c_2) + (1 - pp')h(\hat{y_0}) > (\hat{y_0} - c_2) > (\hat{y_0} - tQ^* - c_2)$. A.5′ implies that $\hat{y_1} < \hat{y_0}$.
Now, consider the enrollment stage. A household that is indifferent between enrolling or not satisfies:

$$[h(y - c_2) + \alpha u(\bar{Q})] = h(y) + \alpha u(q) \qquad \text{A.6}$$

since vouchers are not allowed to be topped up. Given ability level and other parameters, a unique income level $\hat{y_2}$ solves this equation. Plugging $\hat{y_2}$ in A.5, it follows:

$$[pp'h(\hat{y_2} - c_2) + (1 - pp')h(\hat{y_2})] + [p\alpha u(\bar{Q}) + (1 - p)\alpha u(q)] - c_1 \gtreqless h(\hat{y_2} - c_2) + \alpha u(\bar{Q}) = h(\hat{y_2}) + \alpha u(q)$$

since $[pp'h(\hat{y_2} - c_2) + (1 - pp')h(\hat{y_2})] > h(\hat{y_2} - c_2)$ and $[p\alpha u(\bar{Q}) + (1 - p)\alpha u(q)] < \alpha u(\bar{Q})$. This implies that $\hat{y_1} \lesseqgtr \hat{y_2}$. But, if p is sufficiently large and c_1 is sufficiently small,
$[pp'h(\hat{y_2} - c_2) + (1 - pp')h(\hat{y_2})] + p\alpha u(\bar{Q}) + (1 - p)\alpha u(q)] - c_1 \geq h(\hat{y_2} - c_2) + \alpha u(\bar{Q}) = h(\hat{y_2}) + \alpha u(q)$
implying $\hat{y_1} \leq \hat{y_2}$. Note that coupled with the restriction that $y \geq \hat{y_2}$ in this range, this implies $\hat{y_1} = \hat{y_2}$.
So, there are two possible scenarios: (i) $\hat{y_2} < \hat{y_1} < \hat{y_0}$ In this case there is sorting by income at each ability level in the $y \geq \hat{y_2}$ group in the application stage. This is because at each ability level, households with $y \geq \hat{y_1}$ choose to apply, while those with $\hat{y_2} \leq y < \hat{y_1}$ choose not to apply. (ii) $\hat{y_1} \leq \hat{y_2} < \hat{y_0}$[29] (The latter part of the inequality follows from proof of proposition 2 below.) In this case there is no sorting by income in the $y \geq \hat{y_2}$ group in the application stage. This is because at each ability level, all income households in the $y \geq \hat{y_2}$ range behave symmetrically (choose to apply) and there is no sorting by income.

Application stage $y < \hat{y_2}$:

Sorting by Ability: After imposition of vouchers, a household that is indifferent between public and private options in the application stage satisfies

$$\left[p'[h(y) + \alpha u(q)] + (1 - p')[h(y) + \alpha u(Q^*)]\right] + (1 - p)\left[h(y) + \alpha u(q)\right] - c_1 = \left[h(y) + \alpha u(q)\right]$$
$$\Rightarrow \alpha_1 p(1 - p')[u(\bar{Q}) - u(q)] = c_1 \qquad \text{A.7}$$

since vouchers are not allowed to be topped up. Note that since private school quality is not costly here, $Q^* = \bar{Q}$, where $\bar{Q}$ is the highest private school quality. Given other parameters, a unique ability level $\hat{\alpha_1'}$ solves this equation.

$$\hat{\alpha_1'} = \frac{\frac{c_1}{p}}{(1 - p')[u(\bar{Q}) - u(q)]} \qquad \text{A.8}$$

[29] Since this behavior pattern is relevant to households in $y \geq \hat{y_2}$ income group, this inequality reduces to $\hat{y_1} = \hat{y_2} < y_0$ as mentioned above.

The denominator of A.8 is larger than that in A.1. It follows that if p is not too small and p' is not too large, then $\hat{\alpha_1'} < \hat{\alpha_0}$. This implies that higher ability households at each income level are willing to bear the utility cost and apply.

Sorting by Income: Since A.7 is independent of income, there is no sorting by income in the $y < \hat{y_2}$ group in the application stage.

Now consider sorting taking all households together. It follows from the above analysis that at each income level, only the higher ability ones find it worthwhile to apply. This implies that there is sorting by ability in the application stage.

Now consider sorting by income. At each ability level, there is no sorting by income in the $y < \hat{y_2}$ range. There are two possibilities in the $y \geq \hat{y_2}$ range. (i)$\hat{y_2} < \hat{y_1} < \hat{y_0}$. This implies that there is sorting by income in this income range meaning at each ability level only households at or above $\hat{y_1}(> \hat{y_2})$ choose to apply. In this case, there is no sorting by income in the $y < \hat{y_2}$ range, while there is sorting by income in the $y \geq \hat{y_2}$ range. So it is not clear whether there is sorting by income taking all households together. (ii)$\hat{y_1} = \hat{y_2} < \hat{y_0}$ There is no sorting by income in this case in either range. It follows that there is no sorting by income taking all households together. ■

Proof of Proposition 2.

Sorting by Income: In the public-private equilibrium, a household that is indifferent between the public and private options satisfies A.4.

Given ability level and the other parameters, a unique income level $\hat{y_0}$ satisfies this equation.

Now consider enrollment stage in a voucher system. Conditional on acceptance, a household that is indifferent between enrolling or not satisfies A.6. Given ability level and other parameters, a unique income level $\hat{y_2}$ solves this equation. Since $\hat{y_0}$ solves A.4.

$$h(\hat{y_0} - tQ^* - c_2) + \alpha u(Q^*) - c_1 = h(\hat{y_0}) + \alpha u(q)$$

Plugging $\hat{y_0}$ in A.6, it has to be the case that:

$$h(\hat{y_0} - c_2) + \alpha u(\bar{Q}) > h(\hat{y_0} - tQ^* - c_2) + \alpha u(Q^*) - c_1 = h(\hat{y_0}) + \alpha u(q)$$

since $\bar{Q} > Q^*$, $c_1 > 0$, $(\hat{y_0} - c_2) > (\hat{y_0} - tQ^* - c_2)$. Since, $D_y > 0$, it follows that $\hat{y_2} < \hat{y_0}$.

Now let's compare with the application stage. As follows from above, there may or may not be sorting by income in the application stage. But in either case, households with $y \geq \hat{y_2}$ enroll in the enrollment stage, while $y < \hat{y_2}$ do not. It follows that vouchers lead to sorting by income in the enrollment stage.

Sorting by Ability: From above, a public school household that is indifferent between enrolling or not satisfies A.6. Given other parameters, a unique ability level $\hat{\alpha_2}$ solves this equation.

$$\hat{\alpha_2} = \frac{h(y) - h(y - c_2)}{u(\bar{Q}) - u(q)} \qquad \text{A.9}$$

It follows that $\hat{\alpha_2} < \hat{\alpha_0}$, since the denominator of $\hat{\alpha_2}$ is larger than that of $\hat{\alpha_0}$ and the numerator of $\hat{\alpha_2}$ is smaller. Also, comparing A.9 with A.3 and A.8, it can be seen that if c_2 is not too big, then $\hat{\alpha_2} < \hat{\alpha_1}(\hat{\alpha_1'})$. If we assume $h(y) = y$ for simplicity, then $\hat{\alpha_2} \lesseqgtr \hat{\alpha_1}(\hat{\alpha_1'})$ according as $c_2 \lesseqgtr \frac{c_1}{p(1-p')}$. This implies that there is no sorting by ability in the enrollment stage unless c_2 is too big. ■

Proof of corollary 1. If $c_1 > 0$ and $c_2 = 0$ in the enrollment stage, a public school household that is indifferent between enrolling or not satisfies $h(y + v - t \cdot Q^*) + \alpha u(Q^*) - c_1 = h(y) + \alpha u(q)$. Given other parameters, a unique ability level $\hat{\alpha_3}$ solves this equation and it is given by:

$$\hat{\alpha_3} = \frac{c_1}{u(\bar{Q}) - u(q)} \qquad \text{A.10}$$

It follows that $\hat{\alpha_3} < \hat{\alpha_0}$, since the denominator of $\hat{\alpha_3}$ is larger than that of $\hat{\alpha_0}$ and the numerator of $\hat{\alpha_3}$ is smaller. Also, comparing A.10 with A.3 and A.8, it can be seen that $\hat{\alpha_3} < \hat{\alpha_1}(\hat{\alpha_1'})$. That is all accepted are willing to enroll and they are willing to incur a higher cost than c_1 to enroll. This implies that there is no sorting by ability at the enrollment stage. If vouchers cannot be topped up, $D_3 = h(y + v - t \cdot Q^*) + \alpha u(Q^*) - c_1 - [h(y) + \alpha u(q)] \Rightarrow D_3 = \alpha[u(\bar{Q}) - u(q)] - c_1$ is independent of income, so there is no sorting by income. ■

Proof of corollary 2. A public school household that is indifferent between enrolling or not satisfies $h(y + v - t \cdot Q^* - c_2) + \alpha u(Q^*) - c_1 = h(y) + \alpha u(q)$. Given other parameters, a unique ability level $\hat{\alpha_4}$ solves this equation and it is given by:

$$\hat{\alpha_4} = \frac{h(y) - h(y - c_2) + c_1}{u(\bar{Q}) - u(q)} \qquad \text{A.11}$$

It follows that $\hat{\alpha_4} < \hat{\alpha_0}$, since the denominator of $\hat{\alpha_4}$ is larger than that of $\hat{\alpha_0}$ and the numerator of $\hat{\alpha_4}$ is smaller. Also, comparing A.11 with A.3 and A.8, it can be seen that $\hat{\alpha_4} < \hat{\alpha_1}(\hat{\alpha_1'})$ if the costs in the enrollment stage are not too high. Under the assumption $h(y) = y$, $\hat{\alpha_4} < \hat{\alpha_1}(\hat{\alpha_1'})$ if $c_1 + c_2 < \frac{c_1}{p}$. Given ability level and other parameters, a unique income level $\hat{y_4}$ solves this equation:

$$[h(y - c_2) + \alpha u(\bar{Q})] - c_1 = h(y) + \alpha u(q) \qquad \text{A.12}$$

Now, $\hat{y_0}$ solves A.3. Plugging $\hat{y_0}$ in A.12, it has to be the case that:

$$h(\hat{y_0} - c_2) + \alpha u(\bar{Q}) - c_1 > h(\hat{y_0} - tQ^* - c_2) + \alpha u(Q^*) - c_1 = h(\hat{y_0}) + \alpha u(q)$$

since $\bar{Q} > Q^*$, $(\hat{y}_0 - c_2) > (\hat{y}_0 - tQ^* - c_2)$. Since, $D_y > 0$, it follows that $\hat{y}_4 < \hat{y}_0$. Now let's compare with the application stage. As follows from above, there may or may not be sorting by income in the application stage. But in either case, households with $y \geq \hat{y}_4$ enroll in the enrollment stage. It follows that vouchers lead to sorting by income in the enrollment stage. $\blacksquare$

References

Belfield, C., & Levin, H. (2002). The effect of competition on educational outcomes: A review of the US evidence. Review of Educational Research, 72, 279-341.

Belfield, C., & Levin, H. (2006). Privatizing Educational Choice. Paradigm Publishers: Denver, Colorado.

Belfield, C. (2005). The Evidence on Education Vouchers: An Application to the Cleveland Scholarship and Tutoring Program. National Center for the Study of Privatization in Education (NCSPE) Occasional Paper No. 112.

Brunner, E., Imazeki J., & Ross, S. (2010). Universal Vouchers and Racial and Ethnic Segregation. Review of Economics and Statistics, 92(4), 1-16

Chakrabarti, R. (2008a). Can Increasing Private School Participation and Monetary Loss in a Voucher Program Affect Public School Performance? Evidence from Milwaukee. Journal of Public Economics, 92 (5-6), 1371-1393.

Chakrabarti, R. (2008b). Impact of Voucher Design on Public School Performance: Evidence from Florida and Milwaukee Voucher Programs. Federal Reserve Bank of New York Staff Paper No. 315.

Campbell, D., West, M., & Peterson, P. (2005). Participation in a National, Means-tested School Voucher Program. Journal of Policy Analysis and Management, 24(3).

Cohn, E. (Ed.) (1997). Market Approaches to Education: Vouchers and School Choice, Pergamon Press, New York.

Conley, D., & Glauber, R. (2006). Parental Educational Investment and Childrens Academic Risk: Estimates of the Impact of Sibship Size and Birth Order from Exogenous Variation in Fertility. Journal of Human Resources, 41(4), 722-737.

Epple, D., Figlio, D., & Romano, R. (2004). Competition between Private and Public Schools: Testing Stratification and Pricing Predictions. Journal of Public Economics, 88 (7-8), 1215-1245.

Epple, D. & Romano, R. (1998). Competition between Private and Public Schools, Vouchers and Peer Group Effects. American Economic Review, 62(1), 33-62.

Epple, D., & Romano, R. (2008). Educational Vouchers and Cream Skimming. International Economic Review, 49(4), 1395-1435.

Epple, D., Romano, R., & Sieg, H. (2003). Peer Effects, Financial Aid and Selection of Students into Colleges and Universities: An Empirical Analysis. Journal of Applied Econometrics, 18, 501-525.

Epple, D., Romano R., & Sieg, H. (2006). Admission, Tuition, and Financial Aid Policies in the Market for Higher Education. Econometrica, 74(4), 885-928.

Figlio, D., Hart, C., & Metzger, M. (2010). Who uses a means-tested scholarship, and what do they choose? Economics of Education Review, 29(2), 301-317.

Ferreyra, M. (2007). Estimating the Effects of Private School Vouchers in Multidistrict Economies. American Economic Review, 97(3), 789-817.

Howell, W., Peterson, P., Wolf, P., & Campbell, D. (2002). The education gap: Vouchers and urban schools. Washington, DC: Brookings Institution Press.

Howell, W. (2004). Dynamic selection effects in a means-tested, urban school voucher program. Journal of Policy Analysis and Management, 23, 225-250.

Hsieh, C., & Urquiola, M. (2006). The effects of generalized school choice on achievement and stratification: Evidence from Chiles voucher program. Journal of Public Economics, 90, 1477-1503.

Levin, H. (1991). The Economics of Educational Choice. Economics of Education Review, 10(2), 137-158.

Levin, H. (1992). Market Approaches to Education: Vouchers and School Choice. Economics of Education Review, 11(4), 179-285.

Levin, H. (1998). Educational vouchers: Effectiveness, choice, and costs. Journal of Policy Analysis and Management, 17(3), 373-392.

McEwan, P., Urquiola, M., & Vegas E. (2006). School Choice, Stratification, and Information on School Performance: Lessons from Chile. Economia, Spring 1-27.

Peterson, P., Howell, W., & Greene, J. (1999). Evaluation of the Cleveland Voucher Program after Two Years. Harvard University, PEPG No. 99-02.

Witte, J. (2000). The Market Approach to Education: An analysis of America's First Voucher Program. Princeton University Press, Princeton.

Witte, J., & Thorn, C. (1995), The Milwaukee Parental Choice Program, 1990/1991-1994/1995 [computer file]. Madison, WI.

Witte, J., & Thorn, C. (1996). Who Chooses? Voucher and Interdistrict Choice Programs in Milwaukee. American Journal of Education, 104, 186-217.

Table 1: Valuation of Education Relative to other Goals: The Role of Mother's Education

	Mother's Education		
	Less than High School (1)	High School Graduate (2)	More than High School (3)
Panel A. Eligible MPS Students			
Education as or more important than:			
Good Job	94.19	97.90††	99.17†††
Money	89.68	92.66	94.63††
Religion	80.65	83.92	83.47
Ethnic Traditions	84.19	90.21††	92.56†††
Living Conditions	88.71	91.26	94.63††
Panel B. Choice Students			
Education as or more important than:			
Good Job	93.94	95.16	97.26†
Money	89.70	93.23	95.73††
Religion	81.67	83.23	83.42
Ethnic Traditions	86.67	90.65	90.60
Living Conditions	92.12	93.23	93.79

†, ††, †††: column (2) or (3) statistically different from column (1) at 10, 5, and 1 percent level.

Table 2: Summary Characteristics of Voucher Schools and Non-Voucher Schools MPCP (1990-94)

	Voucher Schls. (std. error) (1)	Non-Voucher Schls. (std. error) (2)	(1)−(2) [p-value] (3)
Urbanicity	1.00	0.75	0.25
	(0.00)	(0.46)	[0.21]
Student Teacher Ratio	12.91	14.89	-1.99
	(5.90)	(16.71)	[0.73]
Percentage White	44.75	40.87	3.89
	(38.78)	(30.36)	[0.77]
Percentage Black	40.41	48.61	-8.20
	(42.19)	(32.33)	[0.57]
Percentage Hispanic	12.04	3.33	8.71
	(28.13)	(4.72)	[0.22]
Percentage Asian	2.56	0.56	2.00
	(4.53)	(1.12)	[0.09]
Percentage Indian	0.23	6.63	-6.40
	(0.73)	(24.08)	[0.41]
Percentage Male	61.21	56.10	5.10
	(17.14)	(17.07)	[0.45]

Table 3: Summary Characteristics of Accepted and Non-Accepted Choice Applicants MPCP (1990-94)

	Accepted (std. dev.) (1)	Non-Accepted (std. dev.) (2)	Accepted–Non-Accepted [p-value] (3)
Proportion Male	0.45	0.50	-0.06
	(0.50)	(0.50)	[0.12]
Proportion African American	0.78	0.79	-0.01
	(0.41)	(0.41)	[0.81]
Proportion Hispanic	0.15	0.14	0.01
	(0.36)	(0.35)	[0.69]
Proportion White	0.05	0.05	0.00
	(0.23)	(0.23)	(0.99)
Proportion Asian	0.00	0.00	0.00
	(0.04)	(0.06)	[0.56]
Mother's Education	1.40	1.39	0.01
	(0.74)	(0.76)	[0.82]
Household Income	1.47	1.52	-0.05
	(0.95)	(1.00)	[0.11]
Proportion of mothers employed full-time	0.40	0.39	0.01
	(0.49)	(0.49)	[0.76]
Contact	8.73	9.07	-0.34
	(5.02)	(4.69)	[0.35]
PTO Participation	2.42	2.39	0.03
	(1.50)	(1.41)	[0.76]
Child-time	10.63	10.75	-0.13
	(4.15)	(4.19)	[0.72]
Educational Expectations	2.31	2.33	-0.02
	(0.70)	(0.72)	[0.72]
Prior Reading NCE Score*	40.18	38.90	1.28
	(17.56)	(15.72)	[0.49]
Prior Math NCE Score*	40.25	37.25	2.99
	(19.60)	(20.19)	[0.19]
Number of Students	750	346	

Household income, mother's education, contact, PTO participation, child-time and educational expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. For a detailed description of the variables, refer to tables B.1 and B.2. *Many students do not have data on prior test scores so that the sample size falls to 255 (230) for the "accepted" and 123 (111) for the "non-accepted" in reading (math).

Table 4: Did Private Schools Pick Students Randomly?
Comparing Accepted and Non-Accepted Applicants, MPCP (1990-94)
(Using Logistic Regressions)

	Odds-Ratio (1)	Coeff. (2)	Odds-Ratio (3)	Coeff. (4)	Odds-Ratio (5)	Coeff. (6)	Odds-Ratio (7)	Coeff. (8)
African American	0.99	-0.01	1.08	0.07	1.13	0.13	0.80	-0.22
	(0.32)	(0.32)	(0.52)	(0.48)	(0.55)	(0.48)	(0.56)	(0.70)
Hispanic	1.00	0.00	1.86	0.62	1.93	0.66	0.43	-0.85
	(0.37)	(0.37)	(1.09)	(0.58)	(1.14)	(0.59)	(0.32)	(0.75)
Mother's Education	1.10	0.10	1.12	0.12			1.09	0.08
	(0.11)	(0.10)	(0.20)	(0.18)			(0.18)	(0.16)
Household Income	0.87	-0.13	0.89	-0.12			0.82	-0.19
	(0.07)	(0.09)	(0.12)	(0.13)			(0.10)	(0.13)
Contact			0.99	-0.01	0.98	-0.02		
			(0.03)	(0.03)	(0.03)	(0.03)		
PTO Participation			0.91	-0.09	0.92	-0.08		
			(0.08)	(0.09)	(0.08)	(0.09)		
Child-time			1.04	0.04	1.04	0.04		
			(0.03)	(0.03)	(0.03)	(0.03)		
Educational Expectations			0.84	-0.18	0.83	-0.19		
			(0.16)	(0.19)	(0.16)	(0.19)		
Mother Employed full-time			1.37	0.32	1.48	0.39		
			(0.35)	(0.25)	(0.40)	(0.27)		
Mother High School Grad					1.05	0.05		
					(0.42)	(0.40)		
Mother> High School Grad					1.16	0.15		
					(0.46)	(0.39)		
Middle income					1.32	0.28		
					(0.53)	(0.40)		
Upper-Middle income					1.06	0.06		
					(0.42)	(0.39)		
High income					0.79	-0.23		
					(0.33)	(0.42)		
Prior Reading NCE score							1.00	0.00
							(0.01)	(0.01)
Prior Math NCE Score							1.01	0.01
							(0.01)	(0.01)
Observations	852	852	293	293	293	293	314	314
Probability > Chi-Square	0.45	0.45	0.68	0.68	0.80	0.80	0.51	0.51

*, **, ***: significant at the 10, 5, and 1 percent level, respectively. Huber-White standard errors are in parentheses. The even numbered columns report the coefficients, while the odd numbered columns report the odds-ratios. Household Income, Mother's Education, Contact, PTO Participation, Child-time and Educational Expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. Mother High School Graduate, Mother> High School Graduate, Middle-income, Upper-Middleincome, High-income are dummy variables for the corresponding level of mother's education and income respectively. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 5: Summary Characteristics of Choice Applicants and Eligible Nonapplicants, MPCP (1990-94)

	Appl. (s. d.) (1)	Eligible Non-Appl. (s.d.) (2)	(1)-(2) [p-value] (3)		Appl. (s. d.) (4)	Eligible Non-Appl. (s.d.) (5)	(4)-(5) [p-value] (6)
Mother's Education	1.40	0.92	0.48	**PTO & PTC (Proportions):**			
	(0.74)	(0.81)	[0.00]	Attend PTC	0.91	0.81	0.10
Proportions in groups:					(0.29)	(0.39)	[0.00]
Less than High	0.16	0.37	-0.21	Belong to PTO	0.22	0.15	0.07
	(0.36)	(0.48)	[0.00]		(0.41)	(0.36)	[0.00]
High School Graduate	0.29	0.34	-0.05	Attend PTO Meetings	0.55	0.36	0.19
	(0.46)	(0.47)	[0.02]		(0.50)	(0.48)	[0.00]
More than High	0.55	0.29	0.26	Attend PTO Activities	0.50	0.29	0.21
	(0.50)	(0.45)	[0.00]		(0.50)	(0.45)	[0.00]
Household Income	1.44	1.42	0.02	**Child-time:**			
	(0.96)	(1.00)	[0.66]	Read/Week	2.05	1.67	0.38
Proportions in groups:					(0.90)	(1.08)	[0.00]
< \$5,000	0.17	0.20	-0.03	Math/Week	1.85	1.68	0.16
	(0.38)	(0.40)	[0.11]		(1.00)	(1.09)	[0.00]
\$5,000-\$9,999	0.37	0.34	0.03	Writing/Week	1.83	1.42	0.41
	(0.48)	(0.47)	[0.22]		(1.01)	(1.13)	[0.00]
\$10,000-\$19,999	0.29	0.28	0.01	T.V/Week	1.63	1.44	0.19
	(0.45)	(0.45)	[0.56]		(0.96)	(1.08)	[0.00]
>\$20,000	0.17	0.17	0.00	Sports/Week	1.35	1.27	0.09
	(0.37)	(0.38)	[0.54]		(1.01)	(1.11)	[0.08]
Contact:							
About Academic Performance	1.82	1.33	0.48	Proportion of Mothers	0.40	0.33	0.07
	(1.06)	(1.05)	[0.00]	Employed Full-time	(0.49)	(0.47)	[0.00]
About Classes Child took	1.22	0.85	0.37	Educational Expectations	2.32	1.98	0.34
	(1.09)	(0.93)	[0.00]		(0.70)	(0.82)	[0.00]
About Volunteer Work	1.04	0.49	0.55				
	(1.09)	(0.88)	[0.00]				
About Fund Raising	1.15	0.59	0.57				
	(1.00)	(0.82)	[0.00]				
About School Records	1.21	1.01	0.20				
	(0.93)	(0.92)	[0.00]				
About Child's Behavior	1.51	1.22	0.28				
	(1.19)	(1.10)	[0.00]				
About Helping in Class	0.92	0.50	0.42				
	(1.06)	(0.89)	[0.00]				

Household Income, Mother's Education and the various contact and child-time variables are scaled variables. Higher values indicate higher levels of the corresponding variable. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 6: Application Stage: Was there Stratification by Ability and Income?
Comparing Choice Applicants and Eligible Non-Applicants, MPCP (1990-94)

(Using Logistic Regressions)

	1990-94					1990	1991	1992	1993	1994
	(1)	(2)	(3)	(3′)	(4)	(5)	(6)	(7)	(8)	(9)
Mother's Education	2.07***	1.77***	1.67***	0.21		1.88***	2.02***	1.75***	1.98***	2.58***
	(0.15)	(0.18)	(0.19)			(0.28)	(0.29)	(0.20)	(0.37)	(0.62)
Household Income	0.95	1.04	1.06	0.04		1.09	0.90	0.97	0.93	1.63***
	(0.05)	(0.08)	(0.10)			(0.13)	(0.10)	(0.12)	(0.14)	(0.31)
Contact		1.07***	1.07***	0.31	1.07***	1.06***	1.06**	1.10***	1.03	1.08*
		(0.02)	(0.02)		(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)
PTO Participation		1.16**	1.12*	0.12	1.12*	1.22***	1.24***	1.06***	1.21*	1.00
		(0.07)	(0.07)		(0.07)	(0.09)	(0.10)	(0.09)	(0.12)	(0.12)
Child-time		1.00	1.00	-0.01	1.00			1.00	0.96	1.03
		(0.02)	(0.02)		(0.02)			(0.02)	(0.03)	(0.04)
Educational Expectations			1.62***	0.26	1.61***	1.10	1.36**	1.55***	1.76***	1.81***
			(0.18)		(0.17)	(0.14)	(0.19)	(0.21)	(0.34)	(0.40)
Mother High School Graduate					1.48*					
					(0.34)					
Mother> High School Graduate					2.72***					
					(0.63)					
Middle income					1.00					
					(0.24)					
Upper-Middle income					1.16					
					(0.30)					
High income					1.15					
					(0.34)					
Observations	1638	1013	905		905	737	723	713	657	653
Probability > Chi-Square	0.00	0.00	0.00		0.00	0.00	0.00	0.00	0.00	0.00

*, **, ***: significant at the 10, 5, and 1 percent level, respectively. This table reports odds-ratios. Huber-White standard errors are in parentheses. Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio. Odds ratio is given by e^b, where b denotes the logit coefficient. All regressions control for race and sex of student. Columns (3), (4), (5)-(9) control for an indicator variable denoting whether mother is employed full-time. Household Income, Mother's Education, Contact, PTO Participation, Child-time and Educational Expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. Mother High School Graduate, Mother> High School Graduate, Middle-income, Upper-Middleincome, High-income are dummy variables for the corresponding level of mother's education and income respectively. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 7: Examining Stratification by Comparing Choice Applicants and Eligible Non-Applicants Within Schools

	1990-94		1990	1991	1992	1993	1994
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mother's Education	2.09***	1.84***	2.70***	1.86***	2.21***	2.42***	2.84***
	(0.20)	(0.29)	(0.58)	(0.31)	(0.41)	(0.48)	(0.90)
Household Income	1.03	1.04	0.94	0.87	1.11	0.93	1.69***
	(0.08)	(0.11)	(0.11)	(0.11)	(0.15)	(0.12)	(0.38)
Contact		1.08***					
		(0.03)					
PTO Participation		1.19**					
		(0.10)					
Child-time		0.96					
		(0.03)					
Educational Expectations		1.83***					
		(0.26)					
School dummies	Y	Y	Y	Y	Y	Y	Y
Observations	1250	741	550	537	540	415	300

Table 8: Comparing Prior Test Scores of Choice Applicants and Non-Applicants, 1990-94

	(1)	(2)	(3)	(4)
Mother's Education	1.68***	1.59***	2.73***	2.73***
	(0.18)	(0.24)	(0.70)	(0.73)
Household Income	0.96	1.00	0.89	0.91
	(0.08)	(0.11)	(0.14)	(0.16)
Prior Reading NCE Score	1.01	1.01	1.01	1.01
	(0.01)	(0.01)	(0.01)	(0.01)
Prior Math NCE Score	0.99***	0.99***	0.99	0.99
	(0.01)	(0.01)	(0.01)	(0.01)
Year dummies	N	Y	Y	Y
School dummies	N	N	Y	Y
Grade dummies	N	N	N	Y
Observations	765	585	352	346

Notes for tables 7 and 8: *, **, ***: significant at the 10, 5, and 1 percent level, respectively. These tables report odds-ratios. Huber-White standard errors are in parentheses. Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio. Odds ratio is given by e^b, where b denotes the logit coefficient. All regressions control for race and sex of student. Household Income, Mother's Education, Contact, PTO Participation, Child-time and Educational Expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 9: Summary Characteristics of Enrolled and Non-Enrolled Students, MPCP (1990-1994)
(Sample of Successful Applicants)

	Enroll. (std. error) (1)	Non-Enroll. (std. error) (2)	(1)−(2) [p-value] (3)		Enroll. (std. error) (4)	Non-Enroll. (std. error) (5)	(4)−(5) [p-value] (6)
Mother's Education	1.39	1.43	-0.04	**PTO and PTC (Proportions):**			
	(0.74)	(0.74)	[0.67]	Attend PTC	0.90	0.93	-0.03
Proportions in groups:					(0.31)	(0.26)	[0.47]
Less than High	0.15	0.15	0.00	Belong to PTO	0.23	0.23	0.00
	(0.36)	(0.36)	[0.94]		(0.42)	(0.43)	[0.92]
High School Graduate	0.30	0.27	0.03	Attend PTO Meetings	0.55	0.57	-0.02
	(0.46)	(0.45)	[0.57]		(0.50)	(0.50)	[0.81]
More than High	0.55	0.58	-0.03	Attend PTO Activities	0.49	0.49	0.00
	(0.50)	(0.5)	[0.57]		(0.50)	(0.51)	[0.95]
Household Income	1.41	1.34	0.07	**Child-time:**			
	(0.93)	(1.06)	[0.52]	Read/Week	2.06	1.95	0.11
Proportions in groups:					(0.91)	(0.90)	[0.31]
< 5,000	0.17	0.24	-0.08	Math/Week	1.86	1.56	0.30
	(0.37)	(0.43)	[0.10]		(0.99)	(0.97)	[0.01]
5,000−9,999	0.40	0.38	0.02	Writing/Week	1.85	1.67	0.18
	(0.49)	(0.49)	[0.77]		(1.01)	(1.11)	[0.15]
10,000−19,999	0.29	0.18	0.12	T.V./Week	1.63	1.72	-0.09
	(0.46)	(0.38)	[0.03]		(0.97)	(0.88)	[0.43]
>20,000	0.14	(0.20)	-0.06	Sports/Week	1.39	1.18	0.21
	(0.35)	(0.40)	[0.17]		(1.00)	(1.00)	[0.09]
Contact:							
About Academic Performance	1.76	1.73	0.03	Educational Expectations	2.30	2.40	-0.10
	(1.05)	(1.09)	[0.85]		(0.70)	(0.66)	[0.25]
About Classes Child took	1.16	1.52	-0.37				
	(1.09)	(1.17)	[0.03]				
About Volunteer Work	1.08	1.16	-0.08				
	(1.11)	(1.16)	[0.64]				
About Fund Raising	1.15	1.16	-0.01				
	(0.99)	(1.12)	[0.96]				
About School Records	1.22	1.14	0.08				
	(0.93)	(0.90)	[0.59]				
About Child's Behavior	1.46	1.48	-0.01				
	(1.18)	(1.23)	[0.94]				
About Helping in Class	0.90	1.02	-0.12				
	(1.06)	(1.11)	[0.46]				

Household Income, Mother's Education, and the various contact and child-time variables are scaled variables. Higher values indicate higher levels of the corresponding variable.For a detailed description of the variables, refer to tables B.1 and B.2.

Table 10: Enrollment Stage: Was there Stratification by Ability and Income? Comparing Enrollees and non-Enrollees among Accepted Applicants, MPCP (1990-94)

(Using Logistic Regressions)

	1990-94				1990	1991	1992	1993	1994
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mother's Education	1.15	0.98	0.93		2.61^{**}	1.21	1.03	0.94	0.87
	(0.23)	(0.27)	(0.29)		(1.03)	(0.62)	(0.61)	(0.43)	(0.42)
Household Income	1.17	1.75^{*}	1.70^{*}		1.30	1.08	2.73^{**}	1.16	1.14
	(0.20)	(0.52)	(0.53)		(0.91)	(0.33)	(1.35)	(0.39)	(0.37)
Contact		0.98	1.00						
		(0.06)	(0.05)						
PTO Participation		0.96	0.95						
		(0.19)	(0.19)						
Child-time		1.05	1.04						
		(0.07)	(0.07)						
Educational Expectations			0.90						
			(0.40)						
Mother High School Graduate				1.64					
				(0.71)					
Mother> High School Graduate				1.38					
				(0.55)					
Middle income				1.60					
				(0.58)					
Upper-Middle income				2.40^{**}					
				(1.07)					
High income				1.35					
				(0.62)					
Observations	590	142	141	590	122	175	89	100	48

*, **, ***: significant at the 10, 5, and 1 percent level, respectively. This table reports odds-ratios. Huber-White standard errors are in parentheses. Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio. Odds ratio is given by e^b, where b denotes the logit coefficient. All regressions control for race and sex of student. Household Income, Mother's Education, Contact, PTO Participation, Child-time and Educational Expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. Mother High School Graduate, Mother> High School Graduate, Middle-income, Upper-Middleincome, High-income are dummy variables for the corresponding level of mother's education and income respectively. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 11A: Examining Stratification by Comparing Enrollees and Non-Enrollees Within Schools
(Sample of Accepted Applicants)
(Using Logistic Regressions)

	1990-94
	(1)
Mother's Education	0.88
	(0.31)
Household Income	1.38
	(0.42)
School Dummies	Y
Observations	179

Table 11B: Enrollment Stage: Comparing Prior Test Scores of Enrollees and Non-Enrollees
(Sample of Accepted Applicants)
(Using Logistic Regressions)

	1990-94	
	(1)	(2)
Mother's Education	0.74	0.89
	(0.30)	(0.41)
Household Income	2.51 **	2.97 **
	(1.11)	(1.52)
Prior Reading NCE Score	0.99	1.00
	(0.02)	(0.02)
Prior Math NCE Score	0.98	0.99
	(0.01)	(0.02)
Year Dummies	N	Y
Observations	211	199

Notes for tables 11 and 12: *, **, ***: significant at the 10, 5, and 1 percent level, respectively. These tables report odds-ratios. Huber-White standard errors are in parentheses. Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio. Odds ratio is given by e^b, where b denotes the logit coefficient. All regressions control for race and sex of student.

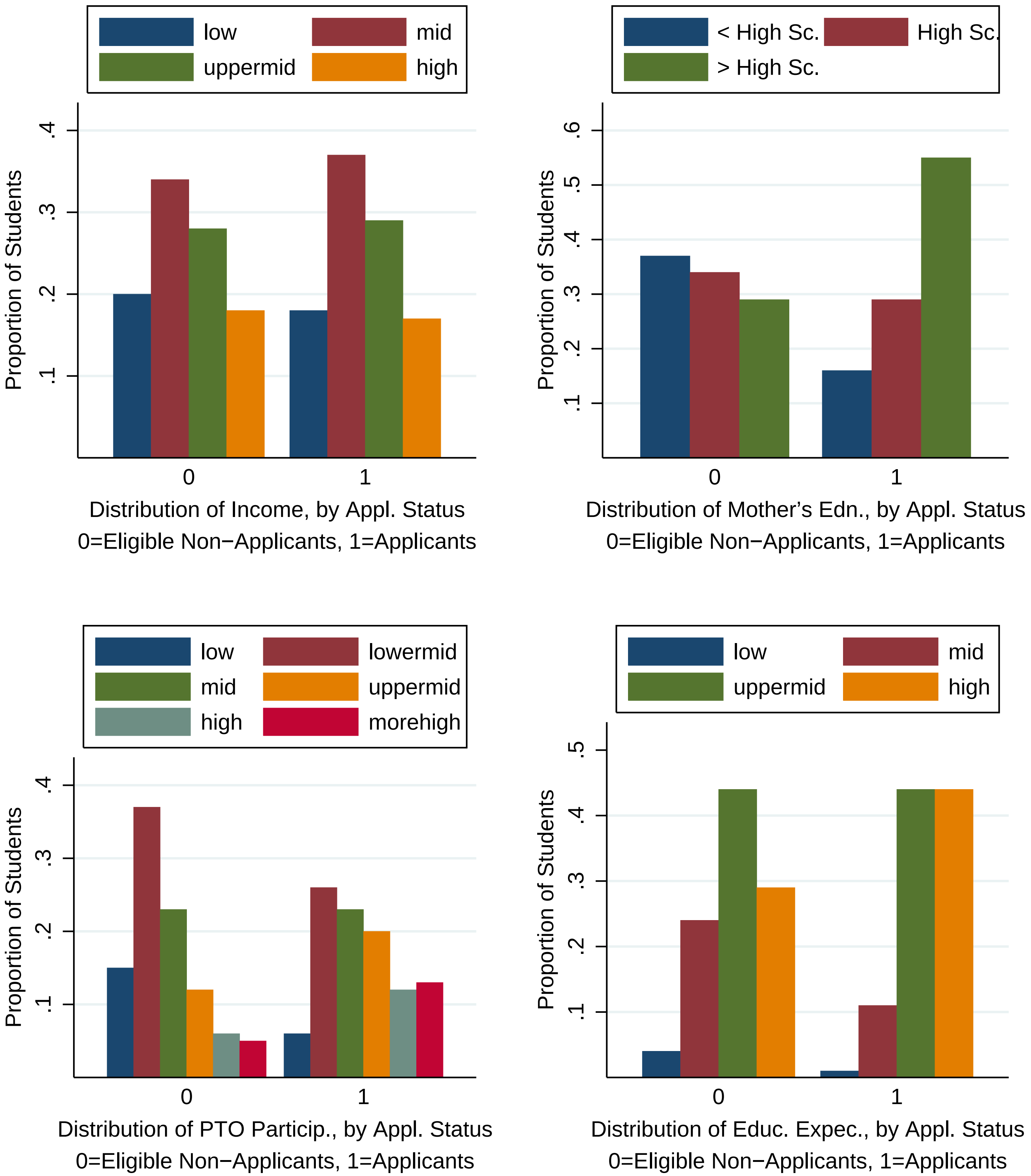

Figure 1. Application Stage: Stratification by Income and Ability, MPCP (1990−94)

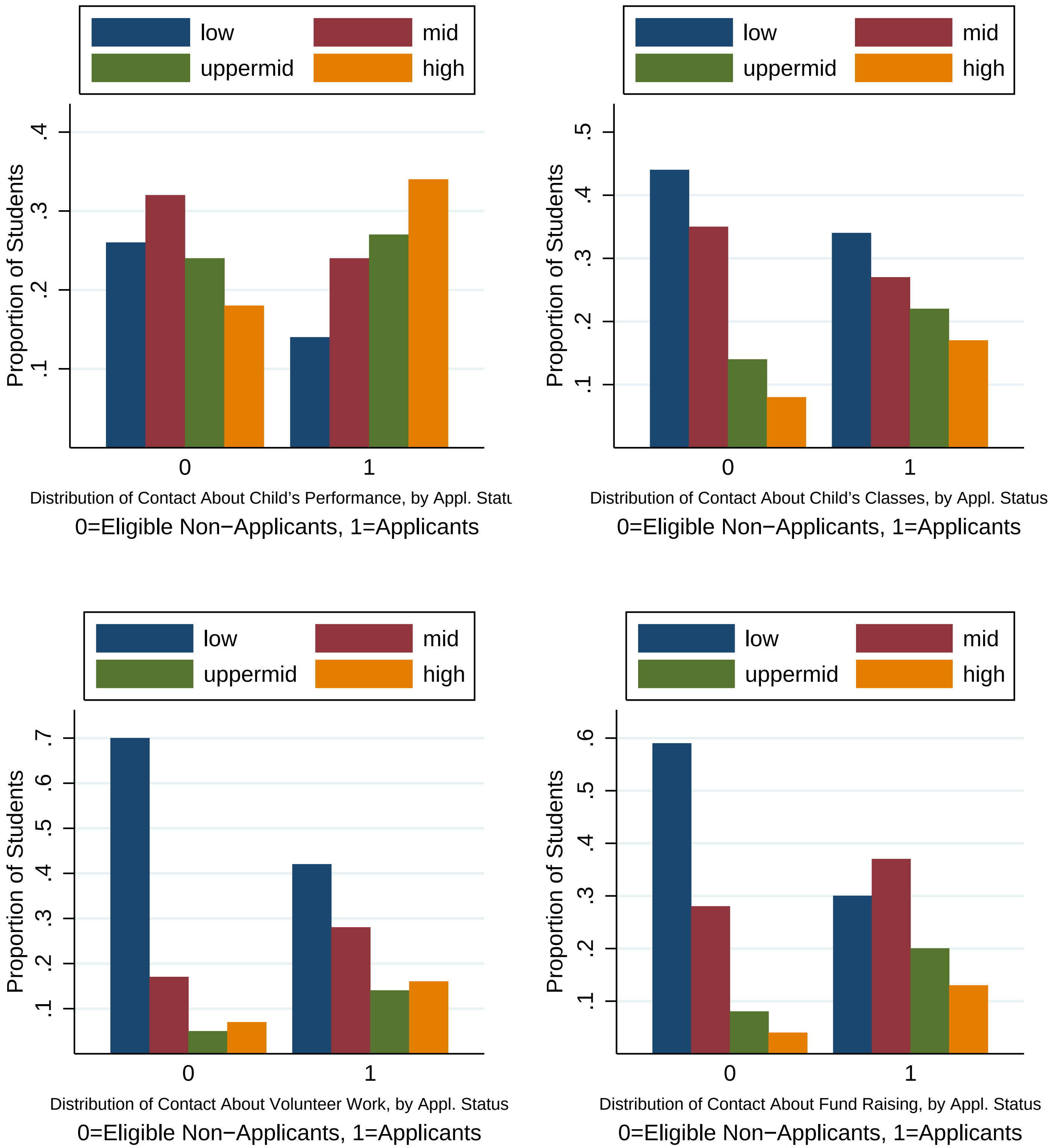

Figure 2. Application Stage: Stratification by Ability, MPCP (1990−94)
Ability Measure: Parents Contacting Child's School

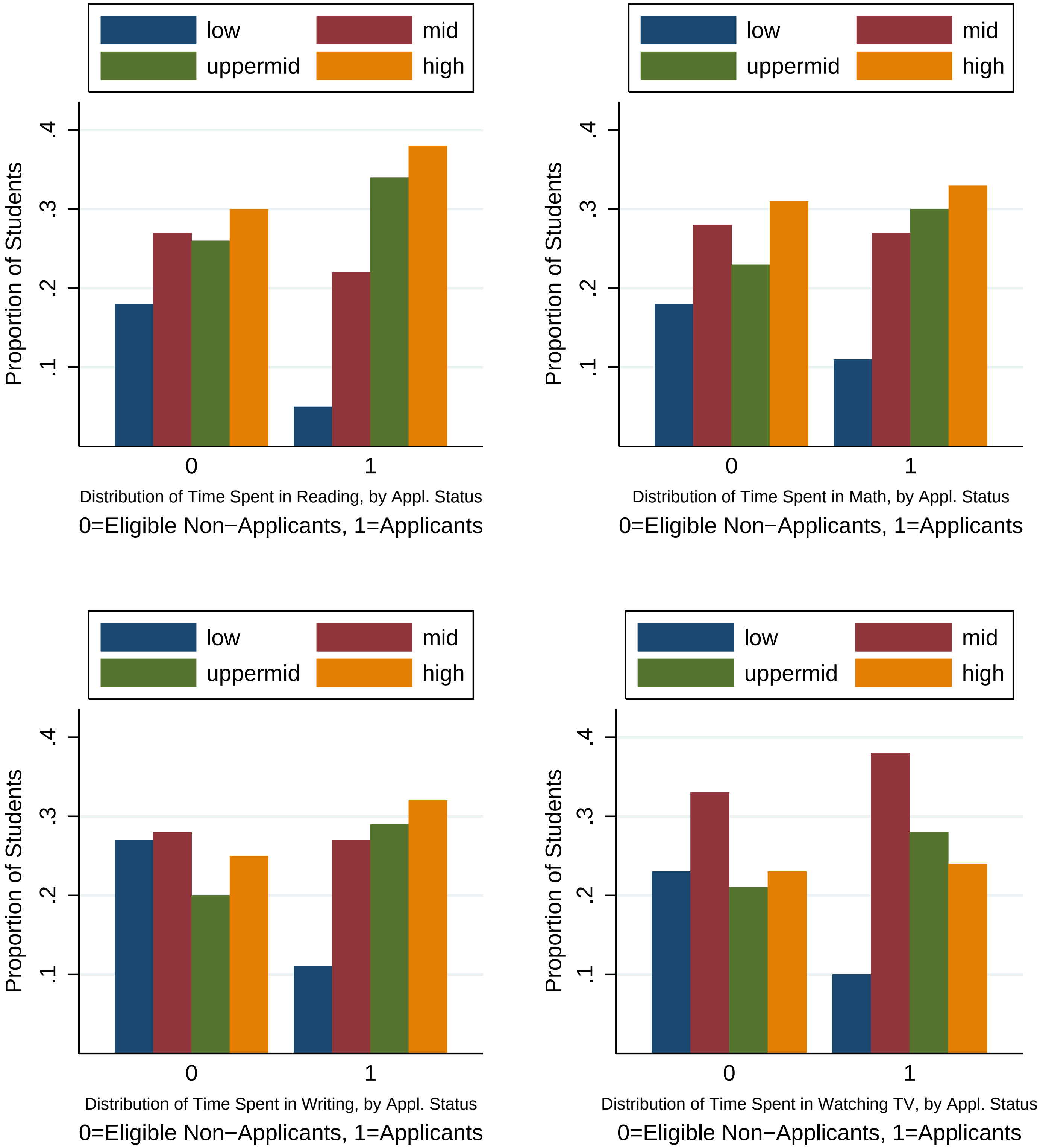

Figure 3. Application Stage: Stratification by Ability, MPCP (1990−94) (Continued)
Ability Measure: Time Spent by Parent With Child

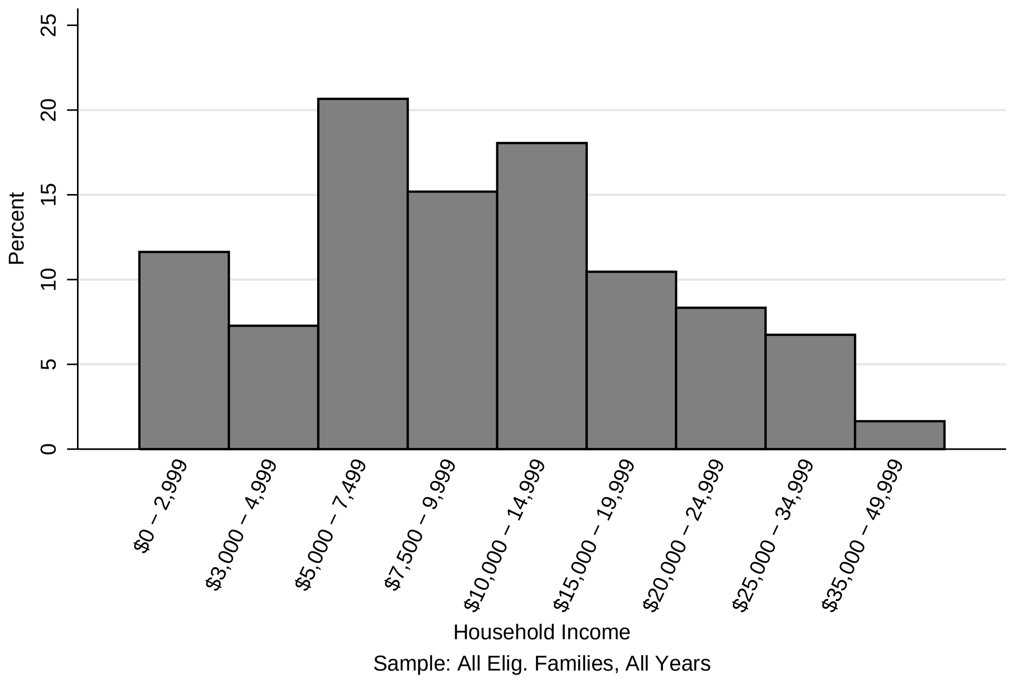

Figure 4a. Income Distribution Among All Eligible Families, All Years

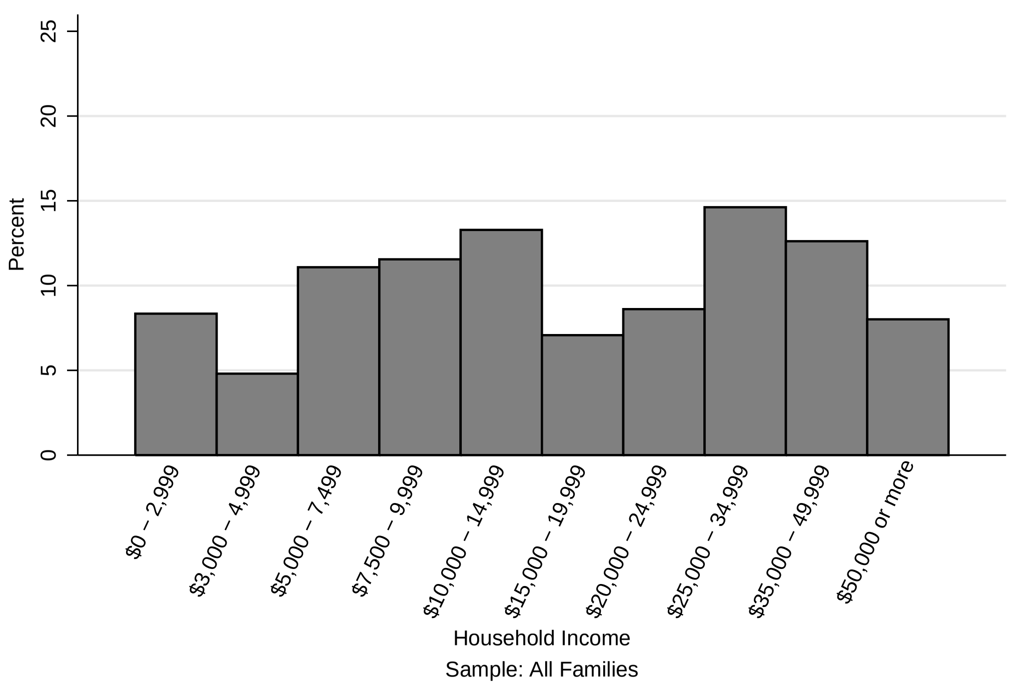

Figure 4b. Income Distribution Among All MPS Families, 1991

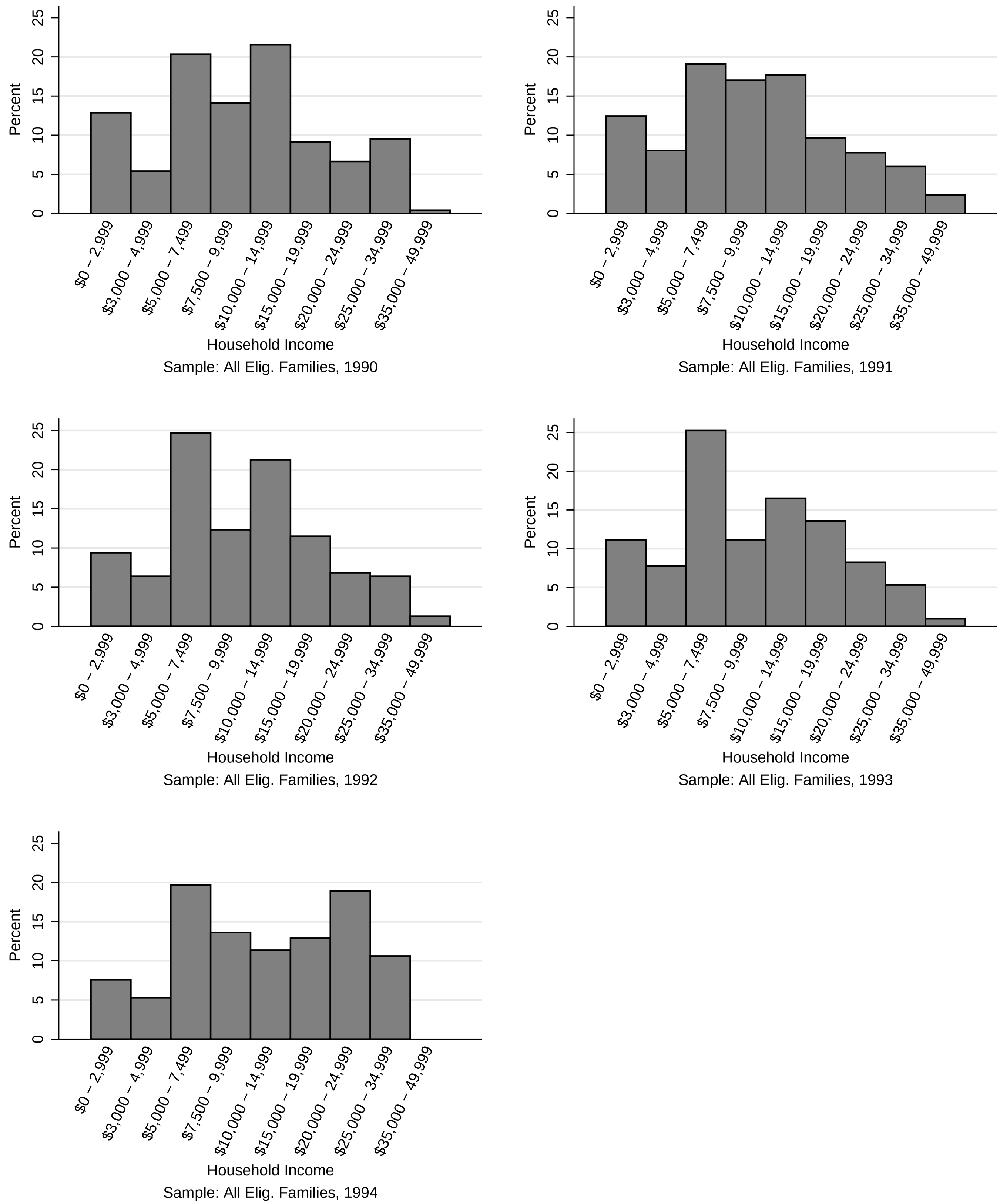

Figure 4c. Income Distribution Among All Eligible Families, by Year