

Vouchers

Memorandum

TO: Interested Parties

FROM: Tom Janenda
Neera Tanden

RE: Vouchers

The Senate will be debating vouchers as an amendment to the District of Columbia Appropriations bill today with debate expected to continue until at least Tuesday. Democratic Senators are presently planning to filibuster the amendment - the first cloture vote is expected Tuesday.

We thought the following materials would be useful for those who are talking to reporters and otherwise influencing the debate on vouchers. We have put together a packet which includes:

- An RNC press release urging the President to support vouchers. The release was sent out today.
- A document showing that Republican pollsters and political consultants are pushing vouchers and other initiatives as a political strategy for the '98 elections.
- A document highlighting that voters have rejected vouchers by large margins each and every time they have been put on a state-wide ballots.
- A two-pager detailing that Congressional Republicans are attacking the one school choice proposal we know works - charter schools - through the Gorton Amendment, which block grants the charter school program as well as other education funding.
- An article in the National Review by Chester Finn, President Reagan's Assistant Secretary for Education, in which he wrote that "charter schools are easily the most promising education-reform strategy around." [National Review, 9/15/97]

To: National Desk, Political and Education writers

Contact: Tim Fitzpatrick of the Republican National Committee, 202-863-8550

WASHINGTON, Sept. 25 /U.S. Newswire/ -- This morning, Republican National Committee Chairman Jim Nicholson hosted the third in a series of occasional press breakfasts with Republican governors from around the nation. His guest today was Utah Gov. Michael O. Leavitt, a past Chairman of the Republican Governors Association and the Western Governors Association. Chairman Nicholson made the following remarks on the topic of education:

``One of the most important issues for Republicans today is education reform. Today we are joined by Gov. Mike Leavitt, a leader among Governors in improving our education system. Before I introduce Gov. Leavitt, I would like to address the topic of education for a moment.

``Today President Clinton is going to Little Rock, Arkansas, for the 40th anniversary ceremony of the Little Rock Central High desegregation.

``It was important to open those schools doors in 1957. But we need to open the schools doors in 1997 as well. Because in 1997, the problem is not getting minority kids into schools -- it's getting them out of schools that are unsafe and where no learning is taking place.

``Right now the government forces too many poor children, many of them African-American and Hispanic, to spend their days in such deficient schools.

``I challenge Bill Clinton to free the children. I challenge Bill Clinton to open the school doors of America, to help parents choose the best schools for their children -- whether those schools are public or private, charter or magnet, secular or religious.

``It's well and good to celebrate the civil rights victories of 1957. Now it's time to achieve a civil rights victory for 1997 by supporting school choice.

``Students from poorer neighborhoods often require extra help and motivation to achieve their fullest potential. Gov. Mike Leavitt is meeting that need with his 'Highly Impacted Schools' program, helping more children get the attention they need to excel.

``Gov. Leavitt is meeting the challenges of providing higher education to more young people, and his work in establishing the Western Governors' University is giving more students greater access to college through technology education. He has a personal interest in education as well. As the father of five children, he has the same concerns about education as millions of parents across Utah and across the nation."

-0-

/U.S. Newswire 202-347-2770/

APNP-09-25-97 0946EDT

CONGRESSIONAL REPUBLICANS' EDUCATION AGENDA: DRIVEN BY POLITICS AND POLLSTERS

"Clinton holds a 20+ point lead over us in perceived ability to handle education. If we want to gain seats in 1998, we had better get started in 1997 by closing the education policy and communication gap."-- Frank Luntz, a pollster for the GOP, in The Luntz Research Companies, Language of the 21st Century, 1997

In the 105th Congress, Congressional Republicans have taken on a number of education initiatives - blocking voluntary national tests, creating tax shelters for those who send their children to private schools, and private school vouchers. At the same time, Republican consultants and pollsters have publicly argued that Republicans must engage on education in order to be competitive in the 1998 election cycle.

- **Ralph Reed and Action on Education:** Ralph Reed, former director of the Christian Coalition and now a Republican political consultant, recently wrote a memo to Republican leaders advising them on political strategies for the 1998 election cycle. The memo titled "Achieving Victory in 1998," including the following advice on education: "Our candidates must make education a major theme in the 1998 campaign. The education issue will whipsaw Republicans in 1998 and 2000 unless they get in front of a wave of concern among voters that is only now landing on the beaches of the political landscape...." [Ralph Reed, 9/8/97 (emphasis added)]
 - ▶ Ralph Reed has also said, "We control Congress. Therefore, we need to find some issues like the Coverdell amendment that are slam-dunks for our side and force Clinton to veto them, if he dares." [Gannett News Service, 8/22/97 (emphasis added)]
 - ▶ And he has said, "School choice is an outstanding issue with Republicans because it resonates with their traditional white base in the suburbs. ...At the same time, it's a great way to take the traditional view that Republicans are the party favoring the wealthy and being out of touch with the poor and minorities and invert that image." [Washington Post, 9/7/97 (emphasis added)]
- **Frank Luntz, Republican Pollster, Argues for Action on Education Because of the Polls.** According to Frank Luntz, a pollster for the GOP, "Clinton holds a 20+ point lead over us in perceived ability to handle education. If we want to gain seats in 1998, we had better get started in 1997 by closing the education policy and communication gap." [Frank Luntz, The Luntz Research Companies, Language of the 21st Century, 1997, (Strategy Book distributed to all House Republicans before August Recess)(emphasis added)]
- **Republicans See Education As An Election Issue:** "Representative John Linder of Georgia, the chairman of the National Republican Campaign Committee, has said Republicans 'will do something bold on education' in 1998 as part of the election-year message." [Washington Times, 9/5/97(emphasis added)]

- **Republican Pollsters Push for Action on Education:** Tony Fabrizio, former pollster for Republican Presidential Nominee Bob Dole, has said, “Nothing has been done in our party to take the offensive on child and education issues.”[Time, 8/25/97]
- **Republican Flip-Flopping on National Action on Education:** “After years of arguing that education is not a federal issue, Republicans are now embracing it.... Republicans hope to use education savings accounts to gain ground on the education issue.”[Washington Times, 9/5/97]
- **Another Republican Wedge Issue:** After Governor Arne Carlson’s Radio Address on school choice and education accounts, the AP wrote: “Carlson’s comments [on private school choice] reflect Republican strategy for the 1998 midterm elections: Try to undercut Democrats’ strength in polls on education matters by making school choice a *wedge* issue.”[AP, 8/16/97]

**REPUBLICANS & PRIVATE SCHOOL VOUCHERS:
AMERICANS HAVE REJECTED VOUCHERS AT EVERY TURN**

Republican Congressional Leaders are pushing legislation to create taxpayer-funded private school vouchers. They have offered an amendment to the District of Columbia Appropriations bill that would subsidize private or parochial education for 2,000 children in the District. However, when they discuss their proposal, there is one fact backers never mention. In each of the four states where private school voucher programs have been put to the voters in a state-wide referendum, they have been defeated by large margins, usually by a vote of two to one. In fact, the voucher idea has never garnered more than 37% of the vote.

Vouchers Defeated by the Voters

California: In 1994, California voters rejected Proposition 174 by a margin of 70% to 30%. The measure would have offered parents a voucher for \$2,500 to pay for private schools.[California Department of Education, 9/11/97]

Washington State: In November of 1996, Washington voters defeated Initiative 173, which would have provided school tuition vouchers by a margin of 65% to 35%.[Facts on File World News Digest, 11/7/96; The Ethnic NewsWatch, 11/6/96]

Colorado: In November of 1992, Colorado voters defeated the Choice School Reform Ballot Initiative by a vote of 62% to 37%.[Heritage Foundation Reports, 7/97]

Oregon: In 1990, Oregon voters defeated a voucher plan that would have granted tax credits of up to \$2,500 to families who wanted to send their children to parochial schools by a vote of 68% to 32%.[New York Times, 11/14/90]

CONGRESSIONAL REPUBLICANS & SCHOOL CHOICE

ATTACKING THE ONE "CHOICE" THAT'S WORKING -- CHARTER SCHOOLS

"The fast-spreading charter-school movement is challenging three hoary axioms about American education: that only private schools can elude bureaucratic micro management, that worthwhile school choice can be achieved only through vouchers, and that the left-leaning politics of public education have barred the schoolhouse door against any encroachment by entrepreneurial capitalism.... [T]wo years of visits, interviews, and surveys have convinced us that charter schools are easily the most promising education-reform strategy around."

---Chester Finn, Reagan's Assistant Secretary for Education, in the National Review, 9/15/97

President Clinton Strongly Supports Public School Choice and Charter Schools

As Governor of Arkansas, Bill Clinton was a leading proponent of public school choice to give parents the option of sending their children to any public school within the state. As part of its efforts to promote public school choice, the Clinton Administration is helping teachers, parents and community groups start charter schools -- innovative public schools that stay open only as long as they produce results and meet the highest standards. Today, 700 charter schools are in existence - there was only one when the President took office. And the President's proposed budget doubles funding to help start charter schools so that there will be more than 3,000 charter schools at the dawn of the 21st century.

CONGRESSIONAL REPUBLICANS AND CHARTER SCHOOLS

Republican efforts to block grant education spending may eliminate all charter school funds. By a 51-49 vote, Senate Republicans approved the Gorton amendment to the Labor/Health and Human Services/Education Appropriations Bill, which would convert several aid programs for schools into block grants for local school districts. The Senate amendment tosses federal education money into one pot and distributes it in an arbitrary way. It would eliminate the present charter school funding program and turn it into a block grant. Localities could choose to zero out all funding for this successful initiative which offers parents a choice among public schools.[AP, 9/12/97]

CHARTER SCHOOLS WORK, ACCORDING TO REAGAN'S ASSISTANT SECRETARY FOR EDUCATION, CHESTER FINN. Mr. Finn recently wrote in the conservative weekly magazine The National Review of the success of charter schools in offering parents choice and increasing the quality of the education their children receive: "The fast-spreading charter-school movement is challenging three hoary axioms about American education: that only private schools can elude bureaucratic micro management, that worthwhile school choice can be achieved only through vouchers, and that the left-leaning politics of public education have barred the schoolhouse door against any encroachment by entrepreneurial capitalism.... [T]wo years of visits, interviews, and surveys have convinced us that charter schools are easily the most promising education-reform strategy around." [National Review, 9/15/97]

REPUBLICAN GOVERNORS - SUPPORTING AND EXPANDING CHARTER SCHOOLS BECAUSE THEY WORK

Texas Republican Governor George Bush

- ▶ Governor George W. Bush has said of charter schools, “**They work.** Competition is something schools shouldn't be afraid of and charter schools bring competition to the public school system.”[The Fort Worth Star-Telegram, 11/13/96]
- ▶ “**Charter schools inject competition into the public education system.** I intend to ask the Legislature for a lot more charter schools for our state.”[Austin American-Statesman, 11/13/96]
- ▶ Governor Bush has pushed for expanding the number of Charter Schools in Texas from 20 to 100. [Dallas Morning News, 8/31/97]

California Republican Governor Pete Wilson

- ▶ Wilson has said that California's Charter Schools Plan will "allow Californians to design schools that **are more responsive** to the needs of the community, establish **alternative teaching methods, give parents a choice**, and, most importantly, **improve pupil learning.**"[San Francisco Chronicle, 9/22/92]
- ▶ During his attempts to expand the number of charter schools, Governor Wilson said, "We must provide others with this same opportunity--a true choice to restore excellence and accountability in this state's public schools."[Los Angeles Times, 3/8/96]
- ▶ Governor Wilson has supported dramatically increasing the number of charter schools in California from 100 to 300.[City News Service, 5/23/96]

Michigan Republican Governor John Engler

- ▶ Of his Charter School plan, Governor Engler has said, "It's the most sweetening reform in a century... There's no question ... **we have the nation's most- extensive charter school legislation.**"[United Press International, 12/24/93]
- ▶ Governor Engler was one of the first governors in the country to push for charter schools.[Daily Report Card, 10/6/93]

Still Crazy
About Che
— p.74

Burke, America, and Ireland — Conor Cruise O'Brien

NATIONAL REVIEW

What, Me Study?

*Just How Dumbed
Down Are America's
Schools?*

*Barbara Lerner • Glynn Custred
Chester E. Finn Jr. • Arthur Hu
Ronald Trowbridge • Richard Nadler
Ramesh Ponnuru • Alan Reynolds*

**Meeting with The
American Ambulance
Chasers' Association**

Matthew Scully

**Role Models —
Heroes For
Zeroes**

Florence King

III
DRUCKER



Or as the shop-worn doggerel goes, "The sage on the stage gives way to the guide on the side."

The notion that all learning should be "naturalistic," like the untutored learning of the mother tongue, and that public schools must institutionalize this process in all domains, flies in the face of common sense, long experience, and scientific research. It also ignores the practical realities of providing mass education for a heterogeneous population.

Another fallacy is that knowledge is less important than knowledge-seeking skills, since the explosion of knowledge today makes it impossible to learn everything about a subject. Thus schools shouldn't bother with "mere facts" but should focus instead on teaching "accessing skills" and such "higher order" or "metacognitive skills" as "critical thinking," "problem solving," etc.

However, since critical thinking is not a detachable tool but instead involves thinking critically *about* something, the more knowledge one possesses (in domains such as literature, carpentry, history, brick laying, mathematics, etc.), the more analogies one will have at one's disposal for thinking critically about something new. To emphasize "critical thinking" as an all-purpose skill while de-emphasizing knowledge actually serves to decrease a student's ability to think critically.

The formalistic-tool concept of learning is consistent with the bureaucratic mindset which elevates form and process over substance. It is, however, at variance with what we know about learning. Yet despite clear evidence of failure the education establishment not only pugnaciously defends its ideology, but also aggressively spreads it at every opportunity. To make matters even worse, the trend among education bureaucrats today is actually away from education, as defined by academic goals, and toward indoctrination, which attempts to mold attitudes in accordance with current fads, whether they be self-esteem and feel-good psychology, or the political correctness of post-modernism and multiculturalism. The results will be a dumbed-down and propagandized population with minorities lagging even further behind.

In short, rather than grapple with the tough problems of educating minority children, the education establishment supports a coercive and punitive regime of racial and ethnic preferences designed to engineer proportional representation.

The regime of preferences, however, is drawing to a close, and the next frontier of civil rights is opening up. Battles fought on this new front will be waged against this determined establishment in an effort to shake it loose from our institutions of public education. This will take the form of struggles for magnet schools, charter schools, school choice, and the necessary breaking of the monopoly of teacher certification now held by university schools of education. We can be sure that the educators, with their

vast resources drawn from public revenues and with their effective organization, will resist these measures at every step and in every way possible.

What will we give minorities once preferences disappear? The answer is that "we" *are* "they," united in a common struggle to take back our schools and thereby to advance better education everywhere, and in so doing to promote the more equal distribution of the important resource of intellectual capital among all our children. □

The New School

Charter schools offer the benefits of both public schools and private schools.

CHESTER E. FINN JR., BRUNO V. MANNO.

LOUANN BIERLEIN & GREGG VANOURÉK

THE fast-spreading charter-school movement is challenging three hoary axioms about American education: that only private schools can elude bureaucratic micromanagement, that worthwhile school choice can be achieved only through vouchers, and that the left-leaning politics of public education have barred the schoolhouse door against any encroachment by entrepreneurial capitalism.

No, this familiar trio hasn't been vanquished, and this is no time to relax. But all three assumptions are in retreat before what some are calling the "reinvention of public education"—a battery of new sorts of schools and educational practices, among which charter schools are the most significant. Today, some seven hundred of these are in operation, enrolling about 170,000 youngsters. Twenty-nine states have authorized charter schools. In some jurisdictions—notably Arizona, Michigan, California, Colorado, Texas, and Massachusetts—they have become major parts of the education-reform scene. In the District of Columbia, despite a rocky start, the prospect of a dozen charter schools is the brightest spot in a grim picture.

What exactly is a charter school? Visualize a hybrid, a public school with some of the most highly prized features of private schools. As a public institu-

tion, it is open to all who wish to attend, paid for with tax dollars, and accountable to state or local authorities for good performance (especially student achievement) and decent behavior (e.g., non-discrimination). It has authority to operate for a specified period—normally five years—and gets its charter renewed only if it delivers good results. In the manner of a private school, however, it is self-governing, free from most regulations, able to hire whomever it likes, in control of its own (secular) curriculum, and attended only by youngsters whose parents choose it.

No such schools existed five years ago. That they enjoy wide bi-partisan support today—President Clinton has called for three thousand of them by decade's end—indicates the appeal of this concept. But opposition is intensifying, too, and there are signs that the charter label is being applied loosely. Indeed, some states that boast of "charter laws" actually enacted such weak measures that the teachers unions are applauding—because any schools created on those terms will lack real independence in vital areas such as personnel.

The authors are on the research staff of the Hudson Institute. Their new report is available on the web at <http://www.edexcellence.net>

National Review's SHOWCASE OF RECOMMENDED SCHOOLS

ACADEMIC FREEDOM?



At too many colleges today, "academic freedom" means the freedom to lock God out of the curriculum, leaving a slavish dependency upon today's fads. In stark contrast to this worldly vision, fortunately, a truly Roman Catholic college is to be "completely dedicated to the research of all aspects of truth in their essential connection with the supreme Truth, who is God." (Pope John Paul II)

At Christendom College, academic freedom is freedom to discover the truth about God, man and the universe.

JOIN US AND BE LIBERATED!

Contact: Paul Heisler, Admissions Office, 134 Christendom Drive
Front Royal, VA 22630 ♦ 1-800-877-5456
<http://www.ewtn.com/christdm>



**CHRISTENDOM
COLLEGE**

A ROMAN CATHOLIC COLLEGE



That's so, for example, with the "charter" laws of Kansas, Georgia, Mississippi, and Rhode Island.

Still, "independent public school of choice" is no longer an oxymoron. Nor is it unique to the United States. Margaret Thatcher's England headed down this path first with "grant-maintained" schools that opt out of their local school bureaucracies and become self-governing, funded by block grants from Whitehall. New Zealand has made all its public schools quasi-independent.

Some U.S. charter schools (especially in California) are former public schools that wrested their freedom from the superintendent's office. Several states permit private schools to convert to charter status, too. But most are start-from-scratch schools, founded by parents, teachers, community organizations, even for-profit firms.

Yes, capitalism is welcome in much of the charter world. Companies that design and manage schools are springing up and either obtaining their own charters or (more commonly) contracting to

run schools whose charters are held by parent or community groups. Several small firms are managing two or three charter schools apiece this year, and two nationally known outfits (the Edison Project and Educational Alternatives, Inc.) are responsible for more—a full dozen in Edison's case. Whether they'll yield a profit for their investors remains

*The educational
choices afforded by
charter schools are
dazzling.*

to be seen—but the schools appear to be educational successes.

Instead of running entire schools, other private firms are taking small bites from the charter apple, furnishing, for example, hot lunches, tutoring, testing, or technology. Some of this is happening in regular public schools, too.

Elementary/secondary education is a \$300-billion enterprise, and private vendors (school-bus companies, textbook publishers) have marketed their wares to it for decades. What is changing is the willingness of schools to entrust instructional and managerial responsibilities to outside sources.

How significant are the educational choices afforded by charter schools? With the crucial exception of religious choice, they're dazzling. In a town where all the regular schools are "progressive" or "child-centered," a back-to-basics charter school is a treasure for disgruntled families. We've also been in communities whose 1950s-style public schools don't suit 1990s-style yuppie parents. In such situations, a charter school that offers open classrooms, mixed-ability grouping, and "developmentally appropriate" instruction is a pearl beyond price.

The price, moreover, is zero—at least, for the customers, because charter schools don't charge tuition. For low-income families in particular, the

charters option is an extraordinary boon.

That is why, contrary to critics' claims that these schools would skim off already-fortunate kids, we find that 40 per cent of charter-school pupils come from families poor enough to qualify for federal lunch subsidies. Half are minorities (compared with one-third of the kids in conventional public schools). Charters also appear to be enrolling many disabled and non-English-speaking students.

Some families are even leaving private schools for charters—11 per cent of the enrollment in our sample—and others (3

per cent) are switching from home-schooling to charters. Another 5 per cent of charter pupils are former dropouts.

As for results, the charter phenomenon is too new for pupil achievement to be fully appraised. But early signs are promising. Test scores in Massachusetts and Arizona, for example, indicate that charter schools are doing better than conventional schools, with similar or tougher kids.

We've spent the past two years studying these schools—visiting sixty of them in fourteen states and interviewing hundreds of their constituents. A few are

places we wouldn't send our own kids; one or two were a little weird. But the great majority of these schools are what most families crave: small, safe places with coherent academic missions and high standards, schools led and staffed by people who believe in those missions and care about kids' actually meeting the standards, schools full of students and teachers who want to be there—not least because nobody is obliged to remain.

We also gathered survey data from students, parents, and teachers. The numbers attest to the needs these schools

Abstaining from Sex Education

LAST month," warned Debra Haffner, president of the Sexuality Information and Education Council of the United States (SIECUS), "the Federal Government released guidelines for a new program that has as its 'exclusive' purpose teaching the social, psychological, and health gains to be realized by abstaining from sexual activity."

Miss Haffner's April letter sounded the tocsin for a massive state-by-state effort, mounted in the legislatures and the press, to block the teaching of abstinence in the schools. So when public health officials from all fifty states, *contra* SIECUS, submitted grant applications to install such programs, conservatives counted it a coup.

The \$50 million in "abstinence ed" grants will constitute the first significant counterbalance to the \$200 million in family-planning funds dispensed annually under Title X of the Public Health Service Act. Title X funds have played a major role in advancing the SIECUS agenda of "comprehensive sexuality education"—early and intensive instruction in sexual anatomy and technique, uninformed by any value base but the moral autonomy of the child and the purported objectivity of the educator.

"Enacted in 1970," says Gracie Hsu, policy analyst for the Family Research Council, "Title X was supposed to help

reduce the unplanned-pregnancy rate, especially in the teenage population. However, Title X has not only failed to reduce the teenage pregnancy rate, but teenage reproductive health has drastically worsened in every category." Miss Hsu cites explosions in the incidence of adolescent abortion, sexually transmitted disease, and unwed motherhood as spawn of the Title X approach.

In reaction to the persistence of these problems, Congress authorized \$250 million in abstinence-education grants under Title V of the Welfare Reform Act. The five-year program creates a "laboratory of the states" in implementing a new approach to sex education. With timely assists from two House committee chairmen—Tom Bliley at Commerce and Bill Archer at Ways and Means—the language of the act fairly represents the position of moral traditionalists. The legislation funds only programs which promote abstinence from premarital sex as "the expected standard for all school-aged children." Title V refers to abstinence as "the only certain way to avoid out-of-wedlock pregnancy, sexually transmitted diseases, and other associated health problems."

Of the various items on the social conservatives' agenda, reforming sex education seemed unlikely under this Administration. President William Jefferson Clinton is, in word and deed, the most ardent advocate of comprehensive sexuality education ever to occupy the

Oval Office. But within two years of his first inauguration, Clinton's preferred approach had suffered stunning setbacks, both as politics and as policy.

Joycelyn Elders's elevation to the post of Surgeon General answered no right-wing prayers—but it should have. An advocate of school-based instruction in petting and auto-eroticism, and of condom-distribution, Dr. Elders proudly publicized the obscurer tenets of the sex-education orthodoxy. What she said wasn't new. Tens of thousands of professionals—teachers and teachers-in-training—had heard comparable messages routinely through in-service seminars or department offerings. What Dr. Elders brought to the debate was publicity. Through her, the average American achieved a general notion of what present-day sex ed actually amounts to: one part cerebral petting zoo, one part flash-card anatomy. The Surgeon General's anti-religious, anti-family, anti-male diatribes translated the esoterica of sex educators into street English.

WHILE Dr. Elders's subsequent public traumas eroded the political viability of comprehensive sexuality education, a group of its academic friends were undermining its intellectual credibility. In "The Failure of Sex Education," Barbara Daffoe Whitehead described sex education as "a policy crafted outside the precincts of the academy. It is not rooted in a discipline, or even a set of disciplines, but can best be described as a jumble of popular therapies and philosophies, including self-help therapies, self-esteem and assertiveness training, sexology, and certain strands of feminism" (*Atlantic Monthly*, October 1994).

In 1993—nearly thirty years after SIECUS first described the general the-

Mr. Nadler is editor of K. C. Jones, a Midwestern political monthly.

meet and the high levels of satisfaction that they are producing. Consider five bits of evidence:

First, charter students and parents both report clear educational improvement. Among children who performed "poorly" in their previous school (as judged by their parents), for example, nearly half are now doing "excellent" or "above average" work. Students of all races report sizable academic gains.

Second, students and parents *like* their charter schools. Three-fifths of the kids say their teachers are better. Half are more interested in their schoolwork.

Three-fifths say the charter school is safer and has better discipline than the school their child would otherwise be attending. Four out of five plan to keep their child in the charter school as long as it's available.

Third, families and teachers are turning to charter schools for educational reasons. When asked why they chose their present school, charter parents' top answers are small size, higher standards, the schools' educational philosophy, greater opportunities for parental involvement, and better teachers. Major factors cited by teachers include

school philosophy, class size, the presence of like-minded colleagues, committed parents, and greater teacher authority.

Fourth, satisfaction levels are highest when it comes to educational matters (curriculum, teaching, etc.) and lowest in peripheral areas (food, sports, etc.), indicating that charters are successfully deploying their limited resources on the basics. When we asked students what they like best about their charter school, the most frequent answers were: "good teachers," "they teach it until I learn it," and "they don't let me fall behind." Not

ory—researchers began to disaggregate the impact of the various strands of "comprehensive sexuality education." The analyses were authored not by partisans of the Christian Right, but by luminaries of peer-reviewed medical research journals.

In "Factors Influencing First Intercourse for Teenage Men," researchers Leighton Ku, Freyn Sonenstein, and Joseph Pleck studied the relationship between sex education and sexual initiation by instructional topic. They divided sex education into four content cate-

gories: biological knowledge, birth-control methods, AIDS education, and "resistance skills"—how to say "no" to sex.

But for the caveat that fear of God might replace fear of AIDS, this is what the Religious Right had always maintained.

This study of first intercourse, published in the November/December 1993 *Public Health Reports*, undermined the foundations of comprehensive sexuality education by revealing its implicit behavioral tendencies. It remained for a blue-ribbon panel, commissioned by the Centers for Disease Control (CDC), to dismantle it in detail.

Reviewing the literature on school-based sex-education programs for their effectiveness in preventing teen pregnancy, co-authors Doug Kirby (ETR Associates), Janet Collins (CDC), Marion Howard (Emory University), Freya Sonenstein (Urban Institute), and Laurie Zabin (Johns Hopkins) compiled the best available teen-sex surveys and implementation data on sex-education programs. Based on their synthesis of best practices, Kirby and his colleagues—strong sex-education advocates, all—challenged the SIECUS orthodoxy platitute by platitude in the May/June 1994 issue of *Public Health Reports*.

Programs that reduced teen pregnancy were not open-ended in their objectives, but based on "the development of conservative group norms." Teachers were not neutral facilitators of morally autonomous wards, but active advocates who "presented a clear stand and emphasized clear behavior values." In effective programs, teens were provided information that "was not unnecessarily detailed or comprehensive." Rather, instruction focused on encouraging desired behaviors.

Comprehensive sexuality education as promoted by SIECUS was shown to be as methodologically inept as it was theoretically perverse.

Despite their string of victories, legislative, political, and intellectual, advocates for Judaeo-Christian sexual absolutes remain cautious—particularly those who have fought these wars the longest. At a recent Washington conference of abstinence educators and their allies, a persistent theme was the facility with which a hostile bureaucracy could hijack congressional intent. Already Rep. Bill Archer has fought (and won) a sharp battle with the Department of Health and Human Services to assure that the governors—mostly Republican—will remain involved in approving Title V grants. But horror stories are accumulating in the states: from Nebraska, a directive to combine abstinence programs with the findings of a hostile panel; from Tennessee, a program to fund after-school sports with Title V funds; from Wisconsin, a proposal to base abstinence grants on their compatibility with SIECUS guidelines.

THEIR unexpected congressional victory on sex education leaves triumphant conservatives in possession of a land-mined field in hostile terrain, occupiers of a forward position in a defensive war. What is best about Title V is that the outcome is now in doubt—an improvement from the consequences of Title X. The state-by-state grant process enables conservatives to intervene in what public-school children are told about the facts of life at levels of government closer to home. If a "laboratory of the states" is not what we would wish for our children, it is certainly preferable to mandatory moral freefall.

—RICHARD NADLER

The researchers reserved their greatest ire for role-playing with no hint as to the expected outcome.

gories: biological knowledge, birth-control methods, AIDS education, and "resistance skills"—how to say "no" to sex.

"In analysis of the full sample," wrote Ku and associates, "prior AIDS education and resistance skills are each associated with significant decreases in the risk of first intercourse (AIDS education: 26 per cent reduction; resistance skills: 29 per cent reduction). Significant increases in the odds ratios were detected for prior instruction in birth control (30 per cent increase) or in the biological aspects of sex (32 per cent increase)."

Put simply: what worked in postponing sexual activity among teens, and its attendant ills, was *fear* (AIDS awareness) and *directive abstinence* (resistance skills). What accelerated teen sex was adult validation of pre-marital carnal

a bad pupil's-eye summary of what schools are supposed to do.

Finally, the teachers prize the professional opportunities they find in charter schools. More than 90 per cent are satisfied with their school's educational philosophy, with their fellow teachers, and with their students; over three-quarters are pleased with their school's administrators and the level of teacher decision-making, and they like the challenge of starting a new school. Fewer than 3 per cent would rather teach elsewhere.

TENS of thousands of American families have chosen to avail themselves of these new-style schools. Yet the charter movement has reached a fateful intersection. If it is able to follow one path, thousands of these schools-of-choice will develop, offering true educational alternatives for millions of families. Down the other path lies a future not too different from the present: a handful of schools serving a smallish population of determined parents and students who had fared poorly elsewhere.

The second path, alas, seems the likelier. The foes of charter schools—both teachers unions and their allies in the education establishment—are doing their utmost to block access to the first. In fact, the foes are now almost as wary of charter schools as of vouchers—not least because the charter idea has wider political appeal and is spreading faster.

Statutory caps on the number of charter schools are a favorite weapon of the establishment. In Massachusetts, where the legislature just inched up its 25-school limit, the Commonwealth received 123 proposals over the past three years. In Texas, which recently raised the ceiling from 20 to 100 schools, nearly 300 applications are expected. (The Lone Star State has about 6,300 public and 1,300 private schools.) When New Jersey issued its new charter application package a couple of months ago, over 500 requests for copies arrived within days.

Discriminatory financial arrangements are another weapon. Capital funding for charter schools is almost non-existent today—which is why so many of them operate in shabby, rented quarters. Only a few states grant them per-pupil operating budgets equal to those of conventional schools. Most jurisdictions don't turn on the fiscal spigot until the kids arrive (i.e., no start-up funding). Several permit local school boards to charge

fat "overhead" fees. And Washington—despite Mr. Clinton's professed support and the popularity of the charter concept among GOP lawmakers—comes nowhere near assuring these schools the federal aid that their students would get in ordinary public schools.

A third weapon is lingering bureaucratic control of crucial parts of a school's finances or program. Recall that the whole idea of chartering a school is to give it operating freedom in return for results-based accountability—the reverse of traditional school practice, which micromanages the production process while turning a blind eye to the quality of the product. A well-crafted charter program keeps a few essential rules in place (in matters such as health, safety, and civil rights) and checks the backgrounds of would-be charter operators and staff. It sets academic standards for the schools to attain. But that's it. All else hinges on whether a school delivers the results it promises—and can attract and hold customers.

That's how it's supposed to work. But in many jurisdictions the enemies of charter schools have kept reels of additional red tape in place. Most California charters have no real fiscal autonomy. New Jersey's charter law retains a form of teacher tenure. North Carolina won't let charter teachers participate in the state retirement system unless their school is under the thumb of a local board. In Colorado local boards can maintain constant anxiety by issuing charters that last only a year at a time.

So long as barriers like these remain in place, charter schools will remain a

modest educational sideshow rather than a widely accessible alternative.

That's today's battleground, and charter foes have recently enjoyed many victories on it. Enabling bills were killed this year in Washington state, Alabama, Indiana, Missouri, Nevada, Virginia, and Tennessee. Gov. Pataki's once-robust proposal is barely breathing in Albany. The new statutes in Illinois and Pennsylvania are weak. And where charter schools got off to a strong start, their enemies are regrouping. Raising the cap in Massachusetts was a bloody political ordeal—and half the Bay State's new charters will be subordinate to local boards and unions. In Colorado, Gov. Roy Romer recently vetoed a bill to give charter schools a fairer financial shake; although Romer was an early charter booster, Colorado's potent school board and teacher groups finally bent him to their will. (That means just a few dozen charters in a state with 1,400 conventional schools.)

Not all charter opponents are found on the left, however, or in the school establishment. Many conservatives have been skittish about these schools because they are a sub-species of "public" education and susceptible to reversals of fortune at the hands of government.

Obviously, charter schools are not a cure-all for American education. They're not immune to curricular fads, fiscal blunders, and personnel mistakes. (That's why five have closed—or been shut down.) Nevertheless, two years of visits, interviews, and surveys have convinced us that charter schools are easily the most promising education-reform strategy around. □

Education and Race

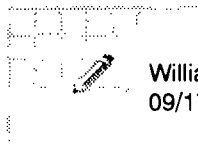
The performance of minority students in affluent areas refutes the prevailing educational shibboleths.

ARTHUR HU

DESEGREGATION is at a crossroads. As many analysts are declaring the integration experiment a failure, Harvard's desegregation guru, Gary Orfield, keeps telling us that minority education could be fixed if only we desegregated *more*. Edu-

cators and the media routinely slam city schools for poor minority performance while holding up affluent suburban districts as models because of their better

Mr. Hu is a writer living in Kirkland, Washington.



William R. Kincaid
09/17/97 02:03:34 PM

Record Type: Record

To: Michael Cohen/OPD/EOP

cc:

Subject: DC Schools

Mike--

fyi --

I am going to have Allison copy some materials that I got from Sandy Battle at ED re: 1. DC voucher legislation and 2) ED support for DC education reform efforts. The latter is a summary that was done for Riley to prep for a meeting with Becton in July. Sandy says that ED is currently working on a fleshed out memo (I believe to present to Raines) on things ED can do to assist DC with school improvement.

-- Bill

U.S. DEPARTMENT OF EDUCATION
OFFICE OF THE DEPUTY SECRETARY
600 INDEPENDENCE AVENUE, S.W.
WASHINGTON, D.C. 20202-0500

DATE:

9/17/97

TO:

Bill Kincaid

ORGANIZATION:

PHONE:

FAX:

456 5581

FROM:

Sandy Batte

PHONE:

401-8183

FAX:

(202) 401-9027

TOTAL PAGES (including cover)

8

Please feel free to call with any
request. We really need to ensure that
the auction has a chance to succeed.

THE DEPARTMENT OF EDUCATION'S EFFORTS TO SUPPORT EDUCATION REFORM IN THE DISTRICT OF COLUMBIA

This memo describes the Department of Education's (ED's) strategy to support education reform in the District of Columbia as well as some possible new bold actions the Clinton Administration could take that build on the existing strategy.

Based on extensive consultations with ED, experts and research recommended by ED, and on best practices supplied by ED, General Julius Becton, CEO of the DC Public Schools, has developed a one-year education reform plan and will be developing a long-term plan. General Becton's efforts concentrate on achieving fundamental improvements over the next three years in the following core areas: academics, school facilities and safety, and personnel and financial management systems. To support General Becton's efforts, ED has established three goals:

- Goal 1:** Make sure programs in DC are in compliance with federal laws and effective.
- Goal 2:** Help DCPS put challenging academic standards, performance assessments, professional development and accountability into the overall system.
- Goal 3:** Assist DC with school-level reform efforts in a specific number of schools each year, including establishing charter schools and using methods and strategies that research has shown to be effective in the inner cities.

GOAL 1: Make Sure DC Programs Are in Compliance with Federal Laws and Effective

ED has been moving vigorously to ensure that federal funds are spent in a way that is faithful to the law. DCPS is now taking steps to fix its internal administrative systems, including its contracts, personnel and accounting systems. DCPS has accepted some, but not all, of the ED's recommendations and offers of assistance in this area.

- A. Requiring full independent audit of DCPS use of federal funds.** At ED's urging, DCPS issued a request for proposals from independent accounting firms for a comprehensive audit of DCPS's use of federal funds, its internal financial and programmatic controls, and its recent past practices. DCPS is now reviewing bids.
- B. Helping with and Conducting Audits.** ED's Inspector General is helping DCPS with its audits, including one audit to accurately count the number of DCPS students and another investigating allegations of misuse of federal Safe and Drug Free Schools funds. DCPS is reviewing bids for the first audit. ED will soon be releasing the results of the other audit, which will document misuse of federal funds.

- C. **Assisting with Specific Department-Funded Programs and Activities.** ED continues to work aggressively with DCPS to address financial and programmatic issues in specific federally funded program. As described below, in many cases ED is withholding federal funds until DCPS develops quality, detailed plans for the funds:

Special Education. Based on serious ongoing non-compliance with special education programs (IDEA), ED is in the process of negotiating an agreement with DCPS on a compliance plan. Earlier this month, ED rejected DCPS's proposed compliance plan and asked DCPS to revise it and add more specifics.

Bilingual Programs and Programs for Limited English Proficient Students. DCPS never implemented a 1994 settlement agreement to ensure equal educational opportunities for limited English proficient students. ED is now in the process of negotiating a new and more specific settlement agreement to bring DCPS into compliance with the law, with the goal of reaching agreement in August.

Discriminatory Discipline of Hispanic Students. ED is in negotiations with DCPS on a recent complaint regarding discriminatory discipline on the basis of national origin against Hispanic students. In response to the complaint, DCPS is removing the principal involved, forming a Diversity Taskforce to address discipline, and hired a special assistant for Multi cultural Education.

Adult Education. DC has had difficulty providing the 25% match required to receive federal funding for adult education. In March 1996, DC stopped funding adult education programs despite a requirement in the DC School Reform Act of 1995 that it implement a "comprehensive and effective adult education and literacy program." ED is working with DC on a strategy, but it is unclear whether DC will use local funds for adult education in the future.

Homeless Education. DC continues to decline federal McKinney Act funding for the education of homeless children on the grounds that it cannot afford to participate. DC is the only eligible jurisdiction not taking advantage of McKinney Act funding.

Vocational Education. ED is concerned that DCPS is incorrectly allocating Perkins funds to schools and using as much as one-third of federal funds for administration. ED is working with DCPS to correct both irregularities.

Goals 2000. ED awarded DCPS \$1.4 million in FY96 Goals 2000 funds, but is withholding FY97 funding until DCPS develops an adequate plan, with specific goals and detailed steps to reach each goal. DCPS expects to submit such a plan to ED this month.

Technology. ED is withholding DC's Technology Literacy Challenge Grant funding until it has an adequate comprehensive technology plan, and has been helping DC to develop such a plan. Based on the draft plan submitted to ED earlier this month, ED expects to be able to award DC its funding --the last jurisdiction in the country to receive funding.

School-to-Work Opportunities. ED has extended the application period for DC to apply for federal School-to-Work funds. ED is providing significant technical assistance to DC on the development of its application and is using its national School-to-Work funds to pay for an independent private-sector assessment of DC's activities and recommendations. This process is about to begin and will last several months. DC is potentially eligible for more than \$9 million in federal funds. ED will also continue to explore options for federal agencies to operate career academies within the public school system.

Safe and Drug Free Schools. General Becton has made improving school security a top priority, and hopes to broaden their use of Safe and Drug Free Schools funds beyond drug prevention, to hire more security guards and better train the guards, and to learn from other urban school's experience. ED has provided DCPS with options, and will be providing information on best practices and recommendations, and will be bringing security chiefs and crime experts to DC to provide advice. ED will explore the potential for the Justice Department to develop a training program for DCPS security guards.

GOAL 2: Help DC put challenging academic standards, performance assessments, professional development and accountability into its overall system.

Over the past months, ED provided assistance, and advised DC on selection of systemwide standards and assessments and putting professional development and accountability into the overall system. Rebuilding DC's school system will be a complex, long-term process. We are working with DC on school reform top-down and bottom-up. DC is using Goals 2000 as its 'road map' for reform efforts and we have brought in Federal and non-federal experts in various subject areas(urban school districts reform strategies, management, curriculum and assessment) who are available to offer substantive feedback.

Accountability into overall system. We are working with DC to implement strong accountability mechanisms for students, teachers and principals focused on results, including student assessment at regular intervals to measure progress toward achievement of higher standards. DCPS is currently considering using 'Annual School-Level Report Cards' a system by which reports on progress and problem areas on a school-by-school basis would be shared within the system and with the public.

Challenging Academic Standards. By Fall 1997, DC plans to implement systemwide standards with aligned internationally bench-marked student assessments that will guide curricular choices and professional development for teachers. We recommended that DC initially will focus on reading and math and then expand to other subjects.

Professional Development. Through a public/private partnership, DCPS and the Center for Excellence in Municipal Management at George Washington University(GW) will establish a joint Leadership and Management Institute opening in mid-August for principals and key central office staff. We will help with the design of this effort.

Early Awareness Program and Information Campaign. While fundamentally restructuring programs to improve student learning, ED will establish an Early Awareness Program and Information Campaign to help motivate DCPS students to further their education beyond the secondary education level. ED's program will help DC students and parents understand the steps they need to take during the middle and junior high school years to get ready for college and the financial and other incentives available to them.

GOAL 3: Assist DC with school-level reform efforts in a specific number of schools each year using methods and strategies that research has shown to be effective in the inner cities and using charter schools.

A. Implementing Bottom-Up Individual Elementary School Change. DCPS has 110 elementary schools. In Fall 1997 DCPS will have intervention strategies in place -using research-based models and outside support -- for each of the 23 schools most in trouble (with the lowest academic performing students). Each model and the number of schools using each is discussed below.

Learning City Program Model. Temple University's Laboratory for Student Success will provide ongoing on-site help to improve 6 elementary schools focusing on classroom instruction, governance, parent and community involvement and staff development work.

Success for All. Johns Hopkins will work with 8 elementary schools using *Success for All* --designed to prevent or intervene in the development of learning problems early.

Early Intervention for Children with Reading Problems. This NIH research model will be used in 9 schools. 400 students each from two grades (K-1 only) will be followed for 5 years. The project focuses on instructional factors underlying the prevention and remediation of reading problems in culturally and linguistically diverse student populations.

B. Other Special strategies for Reading and At-Risk Populations. We offered to work with DC's Title I schools to ensure that they are addressing the needs of all students. DCPS is beginning negotiation with outside 'experts' to provide support for other

schools (e.g., the New American Schools Development Corporation). In addition to the NIH program above, DC is going to utilize other special strategies for helping students with reading.

Reading Recovery. Although costly, DC is planning to train 600 elementary teachers and 25 principals in developmental reading strategies and in reading and writing across the curriculum. DC is planning to assign Reading Resource or Reading Recovery teachers to every school with students scoring 'below basic' on recently administered the Stanford 9 reading achievement test. Several years ago DCPS trained 30 DC teachers in Reading Recovery; due to the cost of the program, only a few are now working as Reading Recovery teachers.

DC Reads Initiative. ED is helping DC with a reading initiative -- based on the President's America Reads Challenge. DC's reading initiative will operate at two levels. It will be an umbrella for all literacy efforts in DC and starting in the Fall about 300 work study students, AmeriCorps members and other volunteers will tutor (3rd and 6th grade) in 18 of DC's lowest performing elementary schools.

ED's Worksite Reading/Tutoring. ED also established a Worksite reading/tutoring program for low performing elementary schools (grades K-3) in which more than 160 ED employees participate. This summer we transported students from 4 schools to each of ED's 5 buildings for tutoring. We will continue our tutoring program at Garrison Elementary, the "Adopt A School" relationship with Amidon Elementary, and dedicating staff to provide assistance to DCPS through calling our DC Desk.

C. Implementing School Change Models for Middle, Junior High and High Schools.

Although the present DC strategy primarily targets elementary schools, DC is considering using CRESPAR Talent Development Model which starts with the assertion that all children can learn in demanding, high-expectations academic settings. DC's Renaissance 2000 high schools (5 high schools involved in comprehensive school reform) will implement various components of the CRESPAR Talent Development High School model. To help advance reform efforts, ED is working to inform other Middle and Junior High Schools of innovative models; we identified neighboring school districts and arranged for site visits.

D. Charter Schools. We have been working actively with DCPS and others on their charter schools strategy. ED assisted DCPS in implementing the District of Columbia School Reform Act of 1995 and implementing the DCPS grant in ED's Public Charter School program under Title X of the Elementary and Secondary Education Act. ED provided a list of names to the Mayor to create the DC Public Charter School Board. As part of its efforts, ED has increased its funding for and provision of technical assistance. ED also has been making experts available to share expertise, experiences, and promising practices with staff from the central office of DCPS, the two eligible DC chartering authorities, persons

interested in establishing charter schools in DC and the approved DC charter schools.

DC's two eligible chartering authorities are accepting applications for opening new charter schools for upcoming school years; they can approve a total of 20 charter schools each year. Last August, the DC Board of Education approved five charter schools. Two are open -- Options Public Charter School and Marcus Garvey Public Charter School. We expect that two will open during the 1997-98 academic year.

We have the money and the interest to make Charter Schools successful in really good way. We expect to see 15-20 new charter schools each year starting in 1998-99. We need to ensure that this effort includes the dissemination of information to potential interested parents and we should consider possibly paying the transportation cost for students.

POSSIBLE ADDITIONAL ACTIONS:

If additional funding were available for DC within the balanced budget plan, it could be used to build on the existing education reform strategy in any one of several ways outline below.

1. **School Construction Initiative.** Even before we enact a national school construction plan, we could provide DC with funding over four years for school renovation. DC received more than \$18 million earlier this year from the federal government from the privatization of Connie Lee, but DC still has substantial unmet needs and there is bipartisan support for helping DC address this need.
2. **Reading Initiative.** In February, President Clinton and General Becton announced DC Reads, DC's initial response to the President's America Reads Challenge. The Administration could build on DC Reads, an in-school tutoring program that uses AmeriCorps members and local College Work Study students, by providing funding for an aggressive teacher professional development strategy as well as for a reading-focused after-school day care program. In conjunction with Title I funding, \$20 million a year for five years could have a dramatic impact, enabling the retraining of all Title I teachers.
3. **After School Initiative.** The Vice President's Family Conference highlighted the national need and support for after-school programs. Either standing alone or in conjunction with a Reading Initiative, an After-School Initiative would address the need for safe places for children after school, help bring communities together around their schools, and provide a place for additional tutoring and services.
4. **Failing Middle Schools Initiative.** The existing DC strategy primarily targets elementary schools, using charter schools and innovative and proven school-level reforms strategies. Additional funding would allow DC to expand these efforts to

include the neediest junior high schools.

5. **College Access Initiative.** The Administration could enhance DC public school children's access to college in several ways:
 - A. **Transform UDC into a combination community college and four-year college.** DC does not have any community colleges and UDC is its only 4-year public college. The Administration could provide \$5 million a year to DC conditioned on its being used to transform UDC into a combined community college and four-year public college with a limited number of salient departments (e.g. English, history, biological sciences, physical sciences, mathematics, psychology, and social sciences)
 - B. **Subsidize needy DC students attending Virginia, Maryland, and DC colleges.** ED has had some discussions with OMB on options to allow DC students to attend Virginia, Maryland or DC colleges and universities at the in-state tuition rate or at the average state tuition rate, and on options to allow DC students to enroll in land grant colleges anywhere in the United States at the same cost as in-state students. The attached table estimates the cost of allowing DC residents to obtain in-state tuition at any land grant college, assuming the federal government pays the difference between the in-state and out-of-state student costs.

CRS-10

school, and the maximum would be the full AEPP. The AEPP would be adjusted to account for the additional costs of educating pupils with disabilities. Scholarships would not be considered to be taxable income to the parents of participating pupils or federal aid to participating schools. Participating schools must comply with Title VI of the Civil Rights Act, but would not otherwise be subject to regulation by ED beyond those regulations in effect on the date of enactment or established under this proposal. An evaluation of the program would be conducted by ED. This proposal is similar to H.R. 3467, introduced in the 104th Congress (see below).

H.R. 318, introduced by Representative Solomon, would authorize tax credits of up to \$1,000 per pupil per year for expenses of tuition, fees, books, supplies, and equipment for pupils attending private elementary or secondary schools, as well as public or private vocational schools or institutions of higher education. This is similar to H.R. 156, introduced in the 104th Congress (see below).



The "District of Columbia Student Opportunity Scholarship Act of 1997" was introduced in the House as H.R. 1797 by Representative Arney, *et al.*, and in the Senate as S. 847 by Senator Coats, *et al.* The proposal is similar to one that was contained in the House-passed and conference versions of H.R. 2546 of the 104th Congress (see the next section of this report). This legislation would authorize the provision of scholarships to District of Columbia resident students in grades K-12 from low-income families to attend public or private schools in the District or nearby suburbs ("tuition scholarships"), or to pay the costs of supplementary academic programs outside regular school hours for students attending District of Columbia Public Schools (DCPS) ("enhanced achievement scholarships"). The authorized appropriation level for this program would be \$7 million for FY1998, rising to \$10 million for each of FY2000-FY2002, plus a one-time authorization of \$250,000 for program evaluation. No more than \$500,000 of the appropriations for any year may be used for program administration or any purpose other than aid to students.

This program would be administered by a non-governmental District of Columbia Scholarship Corporation. The Corporation's Board would have 7 members — 3 appointed by the President from nominations by the Speaker of the House (in consultation with Minority Leader), 3 appointed by the President from nominations by the Majority Leader of the Senate (in consultation with Minority Leader), and 1 appointed by the Mayor. All Board members must be D.C. residents, and none may be D.C. or Federal Government employees (unless they are on leave). Board members would serve for 5-year terms, and would receive stipends of \$150 per day, up to \$5,000 per year. In carrying out its functions, the Board is to consult with the District of Columbia Board of Education, the Superintendent of the DCPS, other entities with jurisdiction over the DCPS (such as the current Board of Trustees named by the District of Columbia Financial Responsibility and Management Assistance Authority), and other scholarship programs in the District of Columbia.

Students in grades K-12 who are residents of the District of Columbia with family income below 185% of the standard federal poverty income threshold for families of the relevant size would be eligible for scholarships. During the initial years of the program (1997-99), first priority in the awarding of scholarships would go to students who have been attending DCPS schools (or are about to enter kindergarten in DCPS schools). In all years, priority would also be given to previous scholarship

CRS-11

recipients. Thus, the proposal places an emphasis at first on pupils who are attending *public* schools in D.C., but pupils already in private school may receive scholarships after these first priority pupils are served in 1997-99 (and previous recipients in 2000 and beyond). In addition, the Corporation would be required to "attempt to ensure an equitable distribution of scholarship funds to students at diverse academic achievement levels" (Section 4(c)).

There would be two types of scholarships: (a) "tuition scholarships" could be used to pay the costs of tuition and fees¹⁶ at a public or private (including a religiously-affiliated) school in D.C. or the inner ring of suburban jurisdictions¹⁷; and (b) "enhanced achievement scholarships" could pay the costs of tuition, fees, and transportation for "a program of instruction ... which enhances student achievement of the core curriculum and is operated outside of regular school hours to supplement the regular school program" (Section 4(d)(2)). The amount of *tuition scholarships* would be the lesser of \$3,200¹⁸ or 100% of tuition¹⁹, fees, and transportation costs if family income is *below the poverty level*, and the lesser of \$2,400 or 75% of tuition, fees, and transportation costs if family income is *between 100% and 185% of poverty*. The amount of *enhanced achievement scholarships* would be the lesser of tuition, fees, and transportation or \$500 for all students with family income below 185% of poverty. In all cases, the bill states that scholarships would be considered to be aid to the student, not the institution.

In order for its students to be eligible for either a tuition or an enhanced achievement scholarship, an educational institution generally must have been in operation for at least 3 years, with at least 25 students. Alternatively, institutions may obtain provisional, 1-year certification after providing a variety of information to the Corporation (e.g., a business plan, course of study, list of the institution's board of directors, etc.). Certification of eligibility may be revoked for "good cause" (such as failure to meet program requirements) or if 25% or more of the scholarship recipients at the school fail to "make appropriate progress" (Section 6(d)). Institutions must file annual reports on their budget; and may not discriminate on the basis of race, color,

¹⁶ There is some ambiguity in the bill regarding whether tuition scholarships could be used to pay transportation costs — Section 4(d)(1), "Use of Scholarship", does not include transportation costs, but Section 5(c), "Scholarship Payments and Amounts", includes transportation costs. Presumably, this ambiguity will be resolved if the bill receives further consideration.

¹⁷ These include Montgomery and Prince Georges counties in Maryland, plus Arlington and Fairfax counties, and the cities of Alexandria and Falls Church in Virginia.

¹⁸ Each of the specific maximum dollar amounts for scholarships (\$3,200, \$2,400, and \$500) would be increased in the future by increases in the Consumer Price Index.

¹⁹ Participating schools may not charge scholarship recipients tuition and fees in excess of the amounts charged to other District of Columbia resident students attending the school.

CRS-12

or national origin²⁰, and must comply with Title IV of the Civil Rights Act of 1964.²¹ Further, the bill states that, "[N]othing in this Act shall affect the rights of students, or the obligations of the District of Columbia public schools, under the Individuals with Disabilities Education Act" (Section 9).

Participating educational institutions would be required to make annual reports to the Corporation containing a variety of data on student achievement, disciplinary actions, academic program, parental involvement, etc. The Corporation would also be required to submit an annual report to the Congress, including information on how program funds have been expended, and the initial achievement levels of scholarship recipients. The U.S. General Accounting Office would be required to contract for an evaluation of the scholarship program within 4 years of the bill's enactment. The evaluation would include comparisons of scholarship recipients and similar but non-participating students in the DCPS with respect to test scores, graduation rates, parental satisfaction, and impact of the program on the DCPS. A one-time appropriation of \$250,000 would be authorized for the evaluation.

Finally, the Senate-passed version of H.R. 2014, the Revenue Reconciliation Act of 1997, would allow withdrawals from newly-authorized, tax-advantaged education savings accounts for elementary and secondary education expenses, including private school tuition. The Senate version of H.R. 2014, now being considered by a conference committee, would authorize a new form of Education Individual Retirement Accounts, the earnings from which would be exempt from federal income taxes. These accounts could be established on behalf of children under the age of 18 years; up to \$2,000, plus the amount of a related tax credit of up to \$500 per year for children aged 13-16, could be invested in the account for each child. The proceeds from these accounts could be withdrawn without penalty only if used to pay tuition and other qualified expenses of attending a postsecondary or elementary or secondary school (including private schools), postsecondary tuition prepayment programs, or expenses related to home schooling.

Legislative Proposals in the 104th Congress. The 104th Congress considered several legislative proposals that would have provided federal support for public and private school choice. The Congress considered a choice program that was included in the conference version of H.R. 2546, FY1996 appropriations legislation for the District of Columbia (D.C.). While the House passed original and conference versions of this legislation that included authorization for a private-public school scholarship program, the Senate failed to invoke cloture on a filibuster against the

²⁰ Discrimination on the basis of religion, sex, or age, which is currently prohibited for educational institutions that are deemed to be federal aid recipients, would not be explicitly prohibited under the bill.

²¹ The bill as introduced states that schools must comply with Title IV of the Civil Rights Act. However, since Title IV does not directly prohibit discrimination (its provisions deal mostly with technical assistance to public school systems undergoing desegregation), it is possible that the reference was intended to be to Title VI of the Civil Rights Act, which prohibits discrimination on the basis of race, color, or national origin in federally assisted programs.

**Table 39.—Enrollment in public elementary and secondary schools, by level and state:
Fall 1980 to fall 1995**

State or other area	Fall 1980	Fall 1981	Fall 1982	Fall 1983	Fall 1984	Fall 1985	Fall 1986	Fall 1987	Fall 1988	Fall 1989	Fall 1990
1	2	3	4	5	6	7	8	9	10	11	12
United States	40,877,481	40,044,093	39,565,610	39,252,308	39,208,252	39,421,961	39,753,172	40,008,213	40,188,690	40,542,707	41,216,683
Alabama	758,721	743,448	724,037	721,901	712,586	730,460	733,735	729,234	724,751	723,743	721,806
Alaska *	86,514	90,858	89,413	98,206	104,599	107,345	107,848	106,869	106,481	109,280	113,903
Arizona	513,790	507,199	510,296	506,682	530,062	548,252	534,538	572,421	574,890	607,615	639,853
Arkansas	447,700	437,121	432,565	432,120	432,668	433,410	437,438	437,036	436,387	434,960	436,286
California	4,076,421	4,046,156	4,065,486	4,089,017	4,151,110	4,255,554	4,377,989	4,488,398	4,618,120	4,771,978	4,950,474
Colorado	546,033	544,174	545,209	542,196	545,427	550,642	558,415	560,236	560,081	562,755	574,213
Connecticut *	531,459	505,386	486,470	477,585	468,145	462,026	468,847	465,465	460,637	461,560	469,123
Delaware	99,403	95,072	92,646	91,406	91,767	92,901	94,410	95,659	96,678	97,808	99,658
District of Columbia	100,049	94,975	91,105	88,843	87,397	87,092	85,612	86,435	84,792	81,301	80,694
Florida	1,510,225	1,487,721	1,484,734	1,495,543	1,524,107	1,562,283	1,607,320	1,664,774	1,720,930	1,789,925	1,861,592
Georgia	1,068,737	1,056,117	1,053,689	1,050,859	1,062,315	1,079,594	1,096,425	1,110,947	1,107,994	1,126,535	1,151,687
Hawaii	165,068	162,805	162,024	162,241	163,860	164,169	164,640	166,160	167,488	169,493	171,708
Idaho	203,247	204,524	202,973	206,352	208,080	208,669	208,391	212,444	214,615	214,932	220,840
Illinois	1,983,463	1,924,084	1,880,289	1,853,316	1,834,355	1,826,478	1,825,185	1,811,446	1,794,916	1,797,355	1,821,407
Indiana	1,055,589	1,025,172	999,542	984,384	972,659	966,106	966,780	964,129	960,994	954,165	954,525
Iowa	533,857	516,216	504,983	497,287	491,011	485,332	481,286	480,826	478,200	478,486	483,652
Kansas	415,291	409,909	407,074	405,222	405,347	410,229	416,091	421,112	426,596	430,864	437,034
Kentucky	669,798	658,350	651,084	647,414	644,421	643,833	642,778	642,696	637,627	630,688	636,401
Louisiana	777,560	782,053	784,027	800,193	800,941	788,349	795,188	793,093	786,683	783,025	784,757
Maine	222,497	216,293	211,986	209,753	207,537	206,101	211,752	211,817	212,902	213,775	215,149
Maryland	750,665	721,841	699,201	683,491	673,840	671,560	675,747	683,797	688,947	698,806	715,176
Massachusetts	1,021,885	947,037	908,984	878,844	859,391	844,330	833,918	825,320	823,428	825,588	834,314
Michigan	1,797,052	1,724,787	1,674,697	1,635,963	1,609,448	1,602,747	1,597,154	1,589,287	1,582,785	1,576,795	1,584,341
Minnesota	754,318	733,741	715,190	705,236	701,697	705,140	711,134	721,481	726,950	739,553	756,374
Mississippi	477,059	471,615	468,294	467,744	466,058	471,195	498,639	505,550	503,326	502,020	502,417
Missouri	844,648	818,705	802,535	795,453	793,793	795,107	800,606	802,060	806,639	807,934	816,558
Montana	155,193	153,435	152,335	153,646	154,412	153,869	153,327	152,207	152,191	151,265	152,974
Nebraska	280,430	273,340	269,009	266,998	265,599	265,819	267,139	268,100	269,434	270,920	274,081
Nevada	149,481	151,339	151,104	150,442	151,633	154,948	161,239	168,353	176,474	186,834	201,316
New Hampshire	167,232	163,827	160,197	159,030	158,614	160,974	163,717	166,045	169,413	171,696	172,785
New Jersey	1,246,008	1,199,643	1,172,520	1,147,841	1,129,223	1,116,194	1,107,467	1,092,982	1,080,871	1,076,005	1,089,646
New Mexico	271,198	268,091	268,632	269,711	272,478	277,551	281,943	287,229	292,425	296,057	301,881
New York	2,871,724	2,783,017	2,718,678	2,674,818	2,645,811	2,621,378	2,607,719	2,594,070	2,573,715	2,565,841	2,598,337
North Carolina	1,129,376	1,108,960	1,096,815	1,089,606	1,088,724	1,086,165	1,085,248	1,085,976	1,083,156	1,080,744	1,086,871
North Dakota	116,885	117,708	117,078	117,213	118,711	118,570	118,703	119,004	118,809	117,816	117,825
Ohio	1,957,381	1,898,501	1,860,245	1,827,300	1,805,440	1,793,965	1,793,508	1,793,431	1,778,544	1,764,410	1,771,089
Oklahoma	577,807	582,572	593,825	591,389	589,690	592,327	593,183	584,212	580,426	578,580	579,087
Oregon	464,599	457,165	448,184	447,109	446,884	447,527	449,307	455,895	461,752	472,394	472,394
Pennsylvania	1,909,292	1,839,015	1,783,969	1,737,952	1,701,880	1,683,221	1,674,161	1,668,542	1,659,714	1,655,279	1,667,834
Rhode Island	148,956	143,414	139,959	136,412	134,610	133,949	134,690	134,800	133,585	135,729	138,813
South Carolina	619,223	609,158	608,518	604,553	602,718	606,643	611,629	614,921	615,774	616,177	622,112
South Dakota	128,507	125,657	123,897	123,060	123,314	124,291	125,458	126,817	126,910	127,329	129,164
Tennessee	853,569	838,297	828,264	822,057	817,212	813,753	818,073	823,783	821,580	819,660	824,595
Texas	2,900,073	2,935,547	2,985,659	2,989,796	3,040,305	3,131,705	3,209,515	3,236,787	3,283,707	3,328,514	3,382,887
Utah	343,618	355,554	370,183	378,208	390,141	403,305	415,994	423,386	431,119	438,554	446,652
Vermont	95,815	93,183	91,454	90,416	90,089	90,157	92,112	92,755	93,381	94,779	95,762
Virginia	1,010,371	989,548	975,727	966,110	965,222	968,104	975,135	979,417	982,393	985,346	998,601
Washington	757,639	750,188	739,215	736,239	741,177	749,706	761,428	775,755	790,918	810,232	839,709
West Virginia	383,503	377,772	375,115	371,251	362,941	357,923	351,837	344,236	335,912	327,540	322,389
Wisconsin	830,247	804,262	784,830	774,646	767,542	768,234	767,819	772,363	774,857	782,905	797,621
Wyoming	98,305	99,541	101,665	99,254	101,261	102,779	100,955	98,455	97,793	97,172	98,226
Outlying areas											
American Samoa	9,647	9,896	—	10,124	—	—	11,055	11,248	11,764	12,258	12,463
Guam	26,420	25,084	25,676	26,249	—	26,043	25,675	25,936	26,041	26,493	26,391
Northern Marianas	—	5,300	—	4,499	4,841	—	—	5,819	6,079	6,101	6,449
Puerto Rico	712,880	721,419	708,794	701,925	692,923	686,914	679,469	672,837	661,693	651,225	644,734
Virgin Islands	25,201	25,525	25,699	26,126	26,122	25,448	24,435	24,020	23,492	21,193	21,750

Enrollment for School year 1996-97 = 78,648

**Table 39.—Enrollment in public elementary and secondary schools, by level and state:
Fall 1980 to fall 1995—Continued**

State or other areas	Fall 1991	Fall 1992			Fall 1993			Fall 1994			Estimated fall 1995 ¹
	Total	Total	Kinder- garten through grade 8 ²	Grades 9 to 12	Total	Kinder- garten through grade 8 ²	Grades 9 to 12	Total	Kinder- garten through grade 8 ²	Grades 9 to 12	Total
1	13	14	15	16	17	18	19	20	21	22	23
United States	42,046,878	42,823,312	31,088,304	11,735,008	43,464,916	31,504,032	11,960,884	44,108,775	31,894,333	12,214,442	44,661,558
Alabama	722,004	731,634	535,248	196,386	734,288	535,637	198,651	736,472	535,187	201,285	³ 735,947
Alaska ⁴	118,680	122,487	91,640	30,847	125,948	93,601	32,347	127,057	93,719	33,338	³ 125,257
Arizona	656,980	673,477	497,917	175,560	709,453	526,412	183,041	737,424	542,904	194,520	³ 766,498
Arkansas	438,518	441,490	317,598	123,892	444,271	317,713	126,558	447,565	319,282	128,283	³ 454,278
California	5,107,145	5,254,844	3,850,790	1,404,054	5,327,231	3,903,137	1,424,094	5,407,043	3,955,434	1,451,609	5,447,846
Colorado	593,030	612,635	451,321	161,314	625,062	459,930	165,132	640,521	469,755	170,766	³ 656,279
Connecticut ⁶	481,050	488,476	361,548	126,928	496,298	368,632	127,666	506,824	375,638	131,186	³ 514,627
Delaware	102,196	104,321	75,983	28,338	105,547	76,617	28,930	106,813	76,819	29,994	³ 108,461
District of Columbia	80,618	80,937	61,133	19,804	80,678	61,434	19,244	80,450	62,126	18,324	³ 79,802
Florida	1,932,131	1,981,407	1,469,850	511,557	2,040,763	1,515,194	525,569	2,108,968	1,567,328	541,640	2,172,794
Georgia	1,177,569	1,207,186	891,647	315,539	1,235,304	910,425	324,879	1,270,948	934,650	336,298	³ 1,311,126
Hawaii	174,747	177,448	128,610	48,838	180,410	131,638	48,772	183,795	133,675	50,120	³ 186,574
Idaho	225,680	231,668	164,634	67,034	236,774	166,999	69,775	240,448	168,887	71,561	³ 243,097
Illinois	1,848,166	1,873,567	1,344,549	529,018	1,893,078	1,356,329	536,749	1,916,172	1,368,041	548,131	1,927,519
Indiana	956,988	960,630	677,249	283,381	965,633	679,066	286,567	968,933	678,943	289,990	980,198
Iowa	491,363	494,839	348,648	146,191	498,519	348,006	150,513	499,550	344,754	154,796	³ 502,301
Kansas	445,390	451,536	328,244	123,292	457,614	329,708	127,906	460,838	329,211	131,627	³ 464,088
Kentucky	646,024	655,041	469,897	185,144	655,265	467,315	187,950	657,642	467,005	190,637	³ 638,634
Louisiana	794,128	797,985	590,824	207,161	800,560	587,490	213,070	797,933	583,892	214,041	³ 781,142
Maine	216,400	216,453	156,368	60,085	216,995	156,528	60,467	212,601	155,903	56,698	³ 219,225
Maryland	736,238	751,850	555,565	196,285	772,638	569,497	203,141	790,938	580,903	210,035	³ 805,580
Massachusetts	846,155	859,948	629,649	230,299	877,726	645,518	232,208	893,727	658,507	235,220	³ 910,020
Michigan	1,593,561	1,603,610	1,164,879	438,731	1,599,377	1,159,968	439,409	1,614,784	1,170,251	444,533	1,643,100
Minnesota	773,571	793,724	569,298	224,426	810,233	576,980	233,253	821,693	581,426	240,267	³ 835,418
Mississippi	504,127	506,668	370,006	136,662	505,907	368,688	137,219	505,962	366,846	139,116	³ 503,602
Missouri	842,965	859,357	621,712	237,645	866,378	622,171	244,207	878,541	628,286	250,255	³ 873,638
Montana	155,779	160,011	115,315	44,696	163,009	116,668	46,341	164,341	116,748	47,593	³ 165,499
Nebraska	279,552	282,414	202,439	79,975	285,097	203,426	81,671	287,100	203,055	84,045	³ 289,733
Nevada	211,810	222,974	165,348	57,626	235,800	175,054	60,746	250,747	185,336	65,411	³ 265,041
New Hampshire	177,138	181,247	133,182	48,065	185,360	136,211	49,149	189,319	138,851	50,468	³ 190,450
New Jersey	1,109,796	1,130,560	817,661	312,899	1,151,307	843,526	307,781	1,174,206	862,331	311,875	⁵ 1,197,560
New Mexico	308,667	315,668	217,418	98,250	322,292	226,287	96,005	327,248	229,168	98,080	³ 328,463
New York	2,643,993	2,689,686	1,893,303	796,383	2,733,813	1,920,609	813,204	2,766,208	1,949,245	816,963	2,830,000
North Carolina	1,097,598	1,114,083	810,576	303,507	1,133,231	828,171	305,060	1,156,767	847,463	309,304	³ 1,165,385
North Dakota	118,376	118,734	84,569	34,165	119,127	84,127	35,000	119,288	83,419	35,869	³ 119,090
Ohio	1,783,767	1,795,199	1,283,869	511,330	1,807,319	1,290,197	517,122	1,814,290	1,295,289	519,001	³ 1,838,411
Oklahoma	588,263	597,096	438,796	158,300	604,076	441,094	162,982	609,718	442,607	167,111	³ 616,497
Oregon	498,614	510,122	365,416	144,706	516,611	368,141	148,470	521,945	371,967	149,978	³ 527,914
Pennsylvania	1,692,797	1,717,613	1,215,974	501,639	1,744,082	1,233,113	510,969	1,765,891	1,244,103	521,788	1,801,970
Rhode Island	142,144	143,798	105,677	38,121	145,676	107,047	38,629	147,487	107,913	39,574	³ 148,978
South Carolina	627,470	640,464	467,305	173,159	643,696	466,951	176,745	648,673	468,798	179,875	³ 637,519
South Dakota	131,576	134,573	97,882	36,691	142,825	102,281	40,544	143,482	101,805	41,677	³ 144,114
Tennessee	833,651	855,231	621,801	233,430	866,557	630,015	236,542	881,355	640,534	240,821	³ 880,960
Texas	3,464,371	3,541,769	2,634,346	907,423	3,608,262	2,681,053	927,209	3,677,171	2,720,623	956,548	³ 3,740,260
Utah	456,430	463,870	329,883	133,987	471,365	329,926	141,439	474,675	328,482	146,193	³ 473,666
Vermont	97,137	98,558	73,865	24,693	102,755	74,828	27,927	104,533	75,590	28,943	³ 105,965
Virginia	1,016,204	1,031,925	757,847	274,078	1,045,471	767,347	278,124	1,060,809	774,319	286,490	³ 1,079,854
Washington	869,327	896,475	651,743	244,732	915,952	660,424	255,528	938,314	673,107	265,207	³ 951,696
West Virginia	320,249	318,296	219,037	99,259	314,383	215,784	98,599	310,511	212,808	97,703	³ 306,451
Wisconsin	814,671	829,415	588,447	240,968	844,001	595,717	248,284	860,686	601,215	259,471	869,172
Wyoming	102,074	100,313	71,798	28,515	100,899	71,402	29,497	100,369	70,185	30,184	³ 99,859
Outlying areas											
American Samoa	13,365	13,994	10,582	3,412	14,484	10,974	3,510	14,445	11,054	3,391	⁵ 14,406
Guam	28,334	30,077	22,428	7,649	30,920	23,153	7,767	32,185	24,189	7,996	³ 33,502
Northern Marianas	7,096	8,086	6,133	1,953	8,188	6,380	1,808	8,429	6,559	1,870	³ 10,634
Puerto Rico	642,392	637,034	469,764	167,270	631,460	464,117	167,343	621,121	455,653	165,468	³ 621,370
Virgin Islands	22,346	22,887	16,804	6,083	22,752	16,706	6,046	23,126	16,659	6,467	³ 22,221

¹ Data estimated by state education agencies.² Includes a relatively small number of prekindergarten students.³ Actual data.⁴ Beginning in 1983, data include students enrolled in public schools on federal bases and other special arrangements.⁵ Data estimated by the National Center for Education Statistics.⁶ Beginning in 1986, data include state vocational/technical schools.

—Data not available.

NOTE.—Some data have been revised from previously published figures.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Common Core of Data surveys. (This table was prepared August 1996.)

September 16, 1997

DC Public Schools

Input for Conversation with Moran

- This will be a distraction from General Becton efforts.
- General Becton and the Trustees are moving on many fronts: charter schools, district-wide content standards and assessments and school-level reform efforts. The proposed academic reform measures -- being undertaken by Becton and the Trustees-- while an improvement (at the district-wide level) the system needs more immediate resources to expand the number and intensity of school-level reform efforts. (The Department could help.)
- The Stanford-9 test scores in Math and Reading are so dismal that the Congress should support massive resources to help the greatest number of kids. DCPS **recently publicized scores Stanford-9 test for 3rd-and 8th-graders.**

At 3rd grade: 1/3 of the kids score below the basic level in reading and math.

At 8th grade: 72% scored below the basic level in math and 1/3 scored below the basic level in reading.

- We need skills training for the kids who are in high school. The Stanford-9 test scores in Math will show at the 10th grade -- schools with 99% of the students scoring below the basic level. District-wide --at the 10th grade -- 90% of the students score below basic. **(NB: These 10th grade scores are NOT yet public.)**
- Vouchers \ scholarships may make the Republicans feel good but it does not give poor kids true choice. They would be available to only a small portion of the population and will have no real effect on the system. Since a very large number of DCPS students are low-income -- scholarships for 2000 kids -- will not help the vast majority of similarly situated students.
- For school-year 1996-97-- of the **78,648 Students** who were enrolled
49,177 **Students** were eligible for free lunch plus
3,250 **Students** were eligible for reduced lunch

September 16, 1997

DC Public Schools

Input for Conversation with Moran

- This will be a distraction from General Becton efforts.
- General Becton and the Trustees are moving on many fronts: charter schools, district-wide content standards and assessments and school-level reform efforts. The proposed academic reform measures -- being undertaken by Becton and the Trustees-- while an improvement (at the district-wide level) the system needs more immediate resources to expand the number and intensity of school-level reform efforts. (The Department could help.)
- The Stanford-9 test scores in Math and Reading are so dismal that the Congress should support massive resources to help the greatest number of kids. DCPS **recently publicized scores Stanford-9 test for 3rd-and 8th-graders.**

At 3rd grade: 1/3 of the kids score below the basic level in reading and math.

At 8th grade: 72% scored below the basic level in math and 1/3 scored below the basic level in reading.

- Vouchers \ scholarships may make the Republicans feel good but it does not give poor kids true choice. They would be available to only a small portion of the population and will have no real effect on the system. Since a very large number of DCPS students are low-income -- scholarships for 2000 kids -- will not help the vast majority of similarly situated students.
- For school-year 1996-97-- of the **78,648 Students** who were enrolled
49,177 **Students** were eligible for free lunch plus
3,250 **Students** were eligible for reduced lunch

Table 62.—Private elementary and secondary schools, enrollment, teachers, and high school graduates, by state:¹ Fall 1993

State	Number of schools		Enrollment		Teachers		High school graduates, 1992-93	
	Total	Standard error	Total	Standard error	Total	Standard error	Total	Standard error
1	2	3	4	5	6	7	8	9
United States²	26,093	205	4,836,442	12,875	338,162	1,319	247,278	697
Alabama	410	79	72,630	4,724	5,424	456	4,174	348
Alaska	66	—	5,884	0	476	—	213	—
Arizona	263	—	41,957	—	2,796	—	2,415	—
Arkansas	179	30	29,011	3,995	2,023	335	1,023	—
California	3,145	65	569,062	1,987	35,170	248	24,436	65
Colorado	391	68	53,732	7,798	4,115	632	1,826	283
Connecticut	360	22	70,198	1,875	6,345	125	6,291	46
Delaware	90	—	22,308	—	1,780	—	1,446	—
District of Columbia	80	—	15,854	—	1,544	—	1,054	—
Florida	1,262	83	233,743	3,789	16,842	424	9,820	54
Georgia	580	81	97,726	3,586	8,283	300	5,630	127
Hawaii	121	—	30,537	—	2,144	—	1,886	—
Idaho	78	—	8,019	—	552	—	341	—
Illinois	1,347	12	293,038	794	17,550	70	14,724	98
Indiana	619	—	91,986	—	6,139	—	4,061	—
Iowa	290	30	50,602	211	3,291	34	2,495	—
Kansas	206	—	37,045	—	2,382	—	1,668	—
Kentucky	296	—	58,058	—	3,815	—	2,949	—
Louisiana	458	19	145,512	4,036	9,286	301	7,844	—
Maine	140	—	16,999	—	1,535	—	1,914	—
Maryland	522	—	112,481	—	8,646	—	5,648	—
Massachusetts	648	29	126,744	1,362	11,329	168	10,281	—
Michigan	1,075	—	187,741	—	11,322	—	8,925	—
Minnesota	542	—	86,051	—	5,595	—	3,453	—
Mississippi	221	30	58,655	1,564	3,995	150	3,901	180
Missouri	719	69	117,466	616	7,973	85	5,839	212
Montana	82	—	9,111	—	684	—	355	—
Nebraska	223	—	39,564	—	2,575	—	1,904	—
Nevada	58	—	10,723	—	654	—	646	—
New Hampshire	130	—	18,386	—	1,742	—	1,730	—
New Jersey	878	—	195,921	—	14,281	—	11,025	—
New Mexico	166	—	20,007	—	1,569	—	1,029	—
New York	1,985	59	473,119	4,776	34,771	482	26,625	125
North Carolina	463	18	69,000	1,803	5,746	147	2,983	—
North Dakota	59	—	7,577	—	529	—	332	—
Ohio	1,016	58	246,805	3,480	14,872	306	12,398	172
Oklahoma	190	62	25,837	3,584	2,250	450	1,536	288
Oregon	250	—	34,092	—	2,254	—	1,700	—
Pennsylvania	1,846	54	342,298	4,260	21,880	235	18,532	304
Rhode Island	112	—	23,153	—	1,835	—	1,408	—
South Carolina	297	21	51,600	1,819	3,989	155	2,383	—
South Dakota	96	—	9,575	—	707	—	254	—
Tennessee	496	54	84,538	2,909	6,684	162	4,970	—
Texas	1,353	98	211,337	7,591	16,726	708	8,447	469
Utah	66	—	9,793	—	749	—	590	—
Vermont	85	—	9,107	—	945	—	1,120	—
Virginia	515	55	84,438	4,584	7,391	621	4,580	—
Washington	486	53	70,205	1,858	4,798	348	2,644	—
West Virginia	145	—	13,539	—	1,085	—	672	—
Wisconsin	954	—	141,762	—	8,927	—	5,129	—
Wyoming	35	—	1,919	—	167	—	31	—

¹Includes special education, vocational/technical education, and alternative schools. Excludes prekindergarten enrollment.

²The National Center for Education Statistics employed an area frame sample to account for noninclusion of schools at the national level. However, caution should be exercised in interpreting state by state characteristics since the samples were not designed to produce such numbers.

—Insufficient data to compute a standard error.

NOTE.—Tabulation includes only schools that offer first grade or above.

SOURCE: U.S. Department of Education, National Center for Education Statistics, "Private School Survey, 1993-94." (This table was prepared August 1995.)

Vouchers

Focus solely on post-sec vs. el-sec

Post-sec: adults
training
historical development out of private schools
state assistance, but no guarantee

El-Sec: children--church/state concerns greater
wide range of functions, including citizen development
historical development of local common schools and community control
state commitments to serving every child

THE WALL STREET JOURNAL FRIDAY, MARCH 14, 1997 R 4

Opinions Are Mixed on School Choice

By THERESA WALKER

Special to the WALL STREET JOURNAL

The clear consensus among educators and the American public is that school choice is a good thing—when it comes to public schools. When choice is extended to include private schools, however, deep divisions emerge.

Adults in the latest Wall Street Journal/NBC News poll favor public-school choice by 73% to 25%. The Clinton administration and most educators think it will strengthen the public-school system if parents are given the option of sending their children to any school in their district or to magnet schools, which build a strong curriculum around a particular area such as science or the arts.

But the real battleground is over extending choice to private schools using taxpayer dollars; 52% of Americans oppose it, while 45% are in favor. The Clinton administration, teachers' unions and

most political liberals oppose using public money to fund private-school education. Most conservatives firmly support tax-funded vouchers or tax deductions to send children to private schools.

Despite the split in opinion, frustration with public education may be causing people to reconsider the issue, which they opposed 2-to-1 nearly three years ago.

"People are unhappy with the system, so they're searching for alternatives," says Fred Yang, an associate of Wall Street Journal/NBC News pollster Peter Hart. "But there is still a negative reaction [to private-school choice] because some people assume that this is just another tax break for the rich."

Educators charge these initiatives would drain much-needed financial resources from public education and cause a migration of talented and intelligent students to private schools—leaving nothing but a crop of below-average students

in its wake.

But supporters, including many Republicans and conservatives, vehemently disagree. They argue that America is built on choice and want that right extended to education. Parents are already using vouchers to choose private schools in places like Milwaukee and Cleveland.

"Any choice is better than none; it's unconscionable to trap kids in bad schools," says Chester Finn, a senior fellow at the Hudson Institute in Washington, D.C. "When you give people choice you allow them to vote with their feet. People tend to work better in schools they want to be in than schools they're in against their will." Therefore, competition will force public schools to improve themselves—and make it difficult for bad schools to survive.

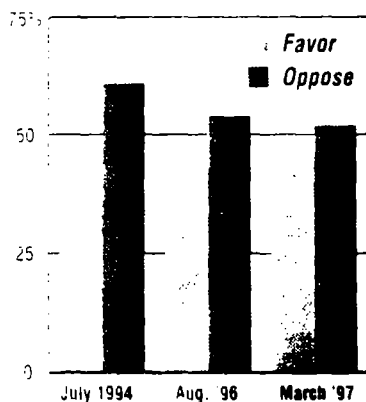
One problem is lack of information about the choice issue. "The average parent in an urban area is not well-informed enough about choice and is, therefore, at a disadvantage," says Sheila Simmons, director of the Center for the Advancement of Public Education at the National Education Association.

More important is the hot-button topic of religion. When parents choose a private school they are most often opting for religious education. About 23% of schools are private, but almost 79% of private schools are religiously affiliated. Experts cite fewer discipline problems and greater safety in private schools as reasons for their popularity. But don't count religion out. "A lot of people are starting to think that it's a good thing for kids; these schools are better at imparting values, morals, ethics and character," Mr. Finn says.

Supporters also claim achievement is higher in private schools; detractors insist there is no statistical difference. The battle of academic studies so far has been fought to a draw.

A Choice in Education

Do you favor or oppose providing tax-funded vouchers or tax deductions to parents who send their children to private schools?



Do you favor or oppose the following education proposals?

	FAVOR	OPPOSE	NOT SURE
Allowing parents to send their children to any public school in their local school district	73%	25%	2%
Providing tax-funded vouchers or tax deductions to parents who send their children to:			
Private schools	45	52	3
Private or religious schools	47	50	3

PANEL ENVISIONS NAEP MEASURING RANGE OF ABILITY

In the 21st century, the "nation's report card" should measure not only what students know and can do but also how they solve problems, a blue-ribbon panel of scholars, educators and researchers recommended Friday.

By 2015, the National Assessment of Educational Progress (NAEP) should zero in on students' individual use of knowledge and their performance on group tasks, the National Academy of Education says in a report to the Education Department.

The approach would broaden NAEP's definition of achievement to include the use of factual knowledge and skills in problem-solving.

In doing so, the academy says, the test should become more inclusive of students with disabilities and limited English skills, one of the existing goals for NAEP (ED, Feb. 28).

The panel says computers could help collect the new data.

For example, computers can track patterns students use in performing test

exercises, such as the number of times a student tries a particular strategy and fails, and the sequence of the student's efforts on a task.

Computers also can determine how much time a student spends on a task.

But the most important contribution may be computers' ability to provide an adaptive assessment. By tracking items answered correctly and incorrectly, computers can select tasks that are in the proper difficulty range for students. In addition, they can tailor reading tasks to the student's level.

When it comes to group activities, the panel suggests using computers and videotapes to score individuals' contributions.

Videotape can provide tremendous information on how students go about problem-solving, said project director George Bohmstadt, senior vice president at the American Institutes for Research, pointing to their recent use in an international study (ED, Nov. 21, 1996).

The panel set forth a research and

planning agenda that starts immediately for ED's National Center for Education Statistics and the National Assessment Governing Board, which administer and set policy for NAEP, respectively.

To have the new generation of NAEP ready by 2015, NCES must begin researching items such as the use of computer technology for administering and scoring the tests.

The panel calls on Congress to guarantee funding for NAEP for four-year cycles, rather than continuing to set funding only annually.

NCES and the governing board will consider the panel's recommendations, but no time frame has been set for taking them up.

"Assessment in Transition: Monitoring the Nation's Educational Progress" is \$32 from National Academy of Education, CERAS Bldg., Room 108, Stanford, Calif. 94305-3084, (415) 725-1003 —Regina Lightfoot-Clark■

NEWS BRIEF: VOUCHER OPPONENTS LAUNCH COUNTEROFFENSIVE

The NAACP and a civil liberties group have joined forces to promote public schools and fight private school voucher proposals in 25 states.

At a Thursday event in Baltimore, NAACP President Kweisi Mfume—a former Maryland congressman—criticized the Christian Coalition for pushing vouchers, calling them "snake oil" for inner cities.

NAACP and People For the American Way (PFAW), a liberal watchdog group,

formed the partnership to focus on improving public schools rather than giving parents public money to send their children to private schools.

Elliott Minberg, PFAW's legal director, said the two groups plan to "initiate a counter strategy" in urban communities and "promote more effective solutions."

Among their goals for public schools are more parental involvement, better technology in the classroom, smaller schools and tougher academic standards.

In addition to a voucher bill that was introduced in the Senate this year, opponents say voucher initiatives have been unveiled or will be introduced soon in 25 state legislatures, including California, Florida, Illinois, Michigan, New Jersey, New York, Ohio, Pennsylvania, Tennessee and Texas.

Minberg said the anti-voucher rally was held in part to counter a Christian Coalition event planned for Baltimore next month. —Dave Boyer■

WORKING PAPER #371
PRINCETON UNIVERSITY
INDUSTRIAL RELATIONS SECTION
DECEMBER, 1996

**Private School Vouchers and Student Achievement:
An Evaluation of the Milwaukee Parental Choice Program**

Cecilia Elena Rouse
Princeton University and NBER

December, 1996

I thank Orley Ashenfelter, David Card, Hank Farber, and Alan Krueger for insightful suggestions and conversations, and participants at the National Bureau of Economic Research's Program on Children Conference and the Princeton Labor Lunch for helpful comments. I particularly thank Lisa Boeger, Jay Greene, and John Witte for insights about the program and lots of help with the data. Jeff Wilder provided expert research assistance. All errors are mine.

Abstract

In 1990, Wisconsin became the first state in the country to provide vouchers to low income students to attend non-sectarian private schools. In this paper, I use a variety of estimation strategies and samples to estimate the effect of the program on math and reading scores. First, since schools selected students randomly from among their applicants if the school was oversubscribed, I compare the academic achievement of students who were selected to those who were not selected. Second, I present instrumental variables estimates of the effectiveness of private schools (relative to public schools) using the initial selection as an instrumental variable for attendance at a private school. Finally, I used a fixed-effects strategy to compare students enrolled in the private schools to a sample of students from the Milwaukee public schools. I find that the Milwaukee Parental Choice Program appears to have had a positive effect on the math achievement of those who attended a private school; but had no benefits for reading scores. I have found the results to be fairly robust to data imputations and sample attrition, however these limitations should be kept in mind when interpreting the results.

Cecilia Elena Rouse
Industrial Relations Section
Firestone Library
Princeton University
Princeton, NJ 08544
rouse@princeton.edu

At the cornerstone of many school reform proposals lies the premise that private schools are more efficient than public schools. This premise is particularly prominent in the current "school choice" debate. Proponents of school choice argue that governments should offer tuition vouchers to families who wish to send their children to private, rather than public schools. If private schools are indeed more effective than public schools, a voucher program may offer a cost-effective way to improve the quality of education. The key evidence of more effective private schooling is a study by Coleman, Hoffer, and Kilgore (1982a,b) who used the first year of the High School and Beyond data to show that students in private schools have higher achievement levels than those in public schools. Critics of school choice programs have argued that private schools would not necessarily do a better job educating students who are currently attending public schools. Rather, they argue that the observed superiority of private school students arises from the selection process that leads higher-achieving students to attend private schools (Goldberger and Cain (1982))¹.

Ideally, the issue of the relative effectiveness of private versus public schooling could be addressed by a social experiment in which children in a well-defined universe were randomly assigned to a private school (the "treatment group"), while others were assigned to attend public schools (the "control group"). After some period of time, one could compare outcomes, such as test scores, high school graduation rates, or labor market success between the treatment and control groups. Since, on average, the only differences between the groups would be their initial assignment -- which was randomly determined -- any differences in outcomes could be attributable to the type of school attended.² While such an experiment has never been implemented, the legislative

¹ See, as well, the collection of articles in the Sociology of Education (1982) and the Harvard Education Review (1981), as well as the review pieces by Cookson (1993), Murnane (1984), and Witte (1992).

² Short of an experiment, one could also look for an exogenous event that is correlated with the likelihood that an individual attends a private school but is uncorrelated with the error term in the outcome equation. Evans and Schwab (1995) conduct such an analysis on the relative value of Catholic high schools using whether the individual is Catholic as the exogenous event. They conclude that students who attend Catholic schools are more likely to finish high school and to enter a four-year college than students who attend public schools.

requirements of a recently enacted school voucher program in Milwaukee, Wisconsin allow one to come close to such an idealized experiment.

In 1990, Wisconsin became the first state in the country to implement a school choice program that would provide vouchers to low income students to attend non-sectarian private schools.³ The number of students in any year was limited to 1% of the Milwaukee public school membership in the first four years, but was expanded to 1.5% in 1994. Only students whose family income was at or below 1.75 times the national poverty line were eligible to apply. In principle, a family of three with an income of approximately \$21,000 would be eligible to apply to the program; in practice, as shown in Table 1, the mean family income of applicants was approximately \$12,300. The choice students were considerably more disadvantaged than the average student in the Milwaukee public schools (whose average family income was \$24,000), and the average non-choice private school student in Milwaukee (whose average family income was about \$43,000; Witte, Thorn, and Pritchard (1995)). The means in Table 1 also show that choice applicants were more likely to be minority and had lower math and reading test scores than the average student in the Milwaukee public schools. These test scores are nationally normed, suggesting that the students who applied to choice were scoring considerably lower than the national average, as well. On the other hand, the parental education for choice applicants, at least for those responding to the surveys, was comparable (or even a little higher) than non-applicants from the Milwaukee public schools.

As the program limited participation to independent, secular private schools, the participating schools were not representative of all private schools in Milwaukee, as the majority of private school enrollments were likely in religious schools (Witte, Thorn, and Pritchard (1995)). That said, until the constitutionality of whether religious schools can participate in voucher programs has been decided,

³ There was an attempt to include religious schools in the program, however the Wisconsin supreme court ruled that it would violate the separation between church and state. I only briefly describe the program here. For more details, see Witte, Thorn, Pritchard, and Claibourn (1994) and Witte, Sterr, and Thorn (1995). I use the terms "private schools" and "choice schools" interchangeably.

Table 1
Mean Characteristics of Applicants to the Choice Program
and Students in the Milwaukee Public Schools

	Applicants		Milwaukee Public School Sample
	Selected	Not Selected	
African-American	0.721 (0.011)	0.780 (0.012)	0.550 (0.007)
Hispanic	0.217 (0.011)	0.154 (0.011)	0.097 (0.004)
"First" Math Score (National Percentile Ranking)	34.560 (1.196)	35.073 (1.552)	43.353 (0.494)
"First" Math Test Score, Missing	0.704 (0.012)	0.779 (0.012)	0.382 (0.007)
"First" Reading Score (National Percentile Ranking)	33.869 (1.050)	34.341 (1.369)	40.227 (0.462)
"First" Reading Score, Missing	0.699 (0.012)	0.781 (0.012)	0.382 (0.007)
Family Income ($\div$ 1000)	\$12.123 (0.297)	\$12.715 (0.509)	\$23.897 (0.534)
Family Income, Missing	0.491 (0.013)	0.760 (0.012)	0.727 (0.006)
Mother's Education	12.559 (0.067)	12.399 (0.109)	12.108 (0.055)
Mother's Education, Missing	0.555 (0.013)	0.763 (0.012)	0.724 (0.006)
Father's Education	12.048 (0.092)	11.521 (0.152)	12.250 (0.068)
Father's Education, Missing	0.697 (0.012)	0.846 (0.010)	0.796 (0.005)
Grade of Application	2.997 (0.079)	3.875 (0.119)	3.477 (0.058)
Has Test Score Two Years After Application	0.536 (0.015)	0.284 (0.019)	0.535 (0.009)
Has Test Score Three Years After Application	0.467 (0.017)	0.236 (0.024)	0.405 (0.009)
Has Test Score Four Years After Application	0.407 (0.025)	0.183 (0.027)	0.364 (0.011)
(Maximum) Number of Observations	1544	1219	5318

Notes: Standard errors are in parentheses. For applicants to the choice schools, the "first" test scores are the "pre-application test scores" (the test scores from the year of application) or earlier ones if the pre-application test scores are missing; for students in the Milwaukee public schools, the "first" test scores are the first non-missing (post-1989) test scores. See text or Data Appendix for how I construct a "year of application" for the Milwaukee public school sample. The data in this table do not represent my analysis sample.

the experience in Milwaukee will be relevant for other cities considering such reforms. In the first year, 7 private schools participated; by 1995, this number had risen to 12. And, the schools represented a variety of educational approaches including Montessori and Waldorf schools, and schools with bilingual and African-American cultural emphases. These schools were not elite private schools. For example, the voucher was worth approximately \$3,200 in 1994-1995 which contrasts with a range of tuition and fees for schools participating in the choice program of \$1,080-\$4,000 (in 1993-1994). In fact, the voucher program helped to improve the financial status of several of the participating schools. While one could believe, a priori, that a program providing vouchers to elite private schools, such as Exeter and Andover, would result in higher achievement gains, it is not so clear that a program providing vouchers to local non-sectarian private schools (that are willing to participate) would have such effects.⁴

Finally, choice schools were only allowed to admit up to 49% of their students as part of the choice program, and were not allowed to discriminate in which students they took. This was interpreted to mean that if the school was oversubscribed, the students would be randomly selected from among the applicants. If a choice school was not oversubscribed, it was required to take all who applied, with only a few exclusions.

It is this feature of the Milwaukee Parental Choice Program that I primarily analyze in this paper. By using applicants who are not selected as a control group, I take advantage of this quasi-natural experiment to estimate whether private school vouchers could improve school achievement of participating students in the short-run, and whether non-sectarian private schools are, indeed,

⁴ On the other hand, in Table 1, the sample from the Milwaukee public schools scored, on average, at the 43-45 percentile which suggests that the Milwaukee public schools are performing below the national average. Coleman, Hoffer, and Kilgore (1982a,b) find that other-private schools perform at least as well as "Catholic schools".

better than public schools.⁵ I also use a fixed-effects estimator to compare the achievement effects of students in the private schools to a random sample of students in the Milwaukee public schools.

I am not the first to analyze the Milwaukee Parental Choice Program. The original evaluation of the fourth year of the program, conducted by Witte, Sterr, and Thorn (1995), compared the test scores of students in the choice schools to a random sample of all Milwaukee public school students and to low-income students (comparison groups) and concluded there were no significant relative achievement gains among the choice students. A subsequent analysis by Greene, Peterson, Du, Boeger, and Frazier (1996) criticized the Witte, Sterr, and Thorn study by noting that students in their comparison group were from substantially more advantaged family backgrounds than students in the choice program. Although Witte, Sterr, and Thorn argue that their comparison group is, in fact, comparable to choice students, conditional on observable covariates, it is nevertheless possible that unobserved factors remain which would tend to obscure any relative achievement gains among the choice student population. As an alternative to the use of a general comparison group, Greene, et. al. propose the use of the unsuccessful applicants as a "quasi-experimental" control group. Relative to the unsuccessful applicants, Greene, et. al. conclude that the choice students made statistically significant test score gains by their third and fourth years in the program. However, the analysis by Greene, et. al. makes a potentially important assumption by excluding from the choice group students who were successfully admitted to the choice schools, but did not attend them, or only attended for a short period of time.

I begin my analysis by returning to the "true" source of exogenous variation in the Milwaukee

⁵ Because the choice program was so small, it cannot provide an analysis of the potential benefits of "school choice". In the most unrestricted school choice program, all (or a substantial fraction) of the students in the public schools would be eligible to attend a private school. Since state funding would be tied to student enrollments, the public schools would have an incentive to improve, leading to no differences in the outcomes of students in "public" and "private" schools. Analysis of the Milwaukee Parental Choice Program can, however, indicate whether parents would prefer to send their children to a private school (see Witte, Sterr, and Thorn (1995)), and whether, in the short run, the academic achievement of those children that attend the private schools would likely increase.

Choice program – that of selection into the pool of students eligible to attend a choice school. I argue that a complete evaluation of the program's effects on student achievement should consider not only whether private schools are better than public schools (as do Greene, et. al (1996) and Witte, Sterr, and Thorn (1995)), but also whether students who are selected to attend a choice school will actually enroll and remain there. I do so by estimating the effect of being selected to attend a private school on student achievement. I next present instrumental variables estimates of the effectiveness of private schools using the initial selection as an instrumental variable for attendance at a private school; I also present fixed-effects estimates of the private school effect using both the unsuccessful applicants and the sample of students in the Milwaukee public schools as comparison groups. I find that students selected for the choice program scored approximately 1-2 extra percentage points per year in math compared to the control group, and those who were enrolled in a private school scored approximately 1.5-2 percentage points more per year than those who were in the Milwaukee public schools. I do not observe differences in the reading scores. I have found the results to be fairly robust to data imputations and sample attrition, however these limitations should be kept in mind when interpreting the results.

In the next section I lay out the empirical framework, in section three I describe my sample, I present the results in section four, and conclude in section five.

II. Empirical Strategy

A. *Evaluating the Choice Program vs. Establishing Whether Private Schools are Better*

In an "ideal" experiment the random assignment process completely determines the status of individuals in the treatment and control groups. In many experimental settings involving human subjects, however, there is slippage between the random assignment status of experimental subjects and whether or not they actually receive the treatment. For example, in randomized trials of medicines, some individuals in the treatment group may fail to take the prescribed treatment (see,

for example, Coronary Drug Project (1980) and Efron and Feldman (1991)). In some social experiments, the slippage can occur in both directions. For example, in a job training experiment, some people assigned to the treatment group may fail to show up, whereas others in the control group may receive training from other providers (Hausman and Wise (1985) and Heckman and Smith (1994)).⁶ In the specific context of the Milwaukee Choice program, the slippage between random assignment status – whether the individual was chosen to attend a choice school -- and "treatment status" – whether an individual actually attended a choice school -- is significant because the treatment lasted over several years. In fact, only about 50 percent of students who were randomly selected to attend choice schools in the first year of the program were still attending the school two years later. In order to understand the nature of alternative estimates of the relative effect of private schooling in this setting, it is necessary to develop a model that takes account of the link between the random selection process and the decision to attend a choice school.

Consider the following linear probability model that models child i 's actual attendance at a choice school in year $t-1$, P_{it-1} ($P_{it-1} = 1$ for students who attend a choice school; $P_{it-1} = 0$ for those who do not), as a function of i 's initial selection status, S_i ($S_i = 1$ for students who were selected, $S_i = 0$ for those who were not):

$$P_{it-1} = a + \rho S_i + X_i g + Z_i g' + v_{it-1} . \quad (1)$$

Here, X_i and Z_i are vectors of individual characteristics (to be distinguished later). Note that if individuals were forced to attend the school to which they were assigned, then $\rho=1$ and $a=g=g'=0$. More generally, however, students' attendance decisions depend on a variety of factors, such as their family's residential location, whether the school is attended by friends or siblings, and so forth. In

⁶ Another example is that of the Vietnam draft lottery. Angrist (1991) explains racial differences in the effect of the lottery on enlistment by the fact that the effect of the draft lottery on enlistment was greater for Whites than for non-Whites.

this case, the parameter ρ may be smaller than 1.

Consider next an outcome equation for the test score of student i in year t . To fix ideas, suppose that test scores depend on the characteristics of i and on her attendance at a choice school in the previous year. (I discuss the specification of the outcome equation more fully in the next section.) Specifically, assume that the test score of student i in year t (T_{it}) is determined by:

$$T_{it} = \alpha + \rho P_{it-1} + X_i\gamma + Z_i\Gamma + \varepsilon_{it} . \quad (2)$$

In equation (2), the parameter ρ reflects the relative effectiveness of choice schools over the public schools attended by students in the sample.

Finally, it is useful to combine equations (1) and (2) into a reduced-form equation:

$$T_{it} = \pi_0 + \pi_1 S_i + X_i\Pi_2 + Z_i\Pi_3 + v_{it} . \quad (3)$$

Note that $\pi_1 = \rho\beta$. Thus, the reduced-form effect of selection into the group eligible to attend choice schools is a combination of two effects: the effect of selection on the relative likelihood of attending a choice school (ρ) – what might be called a "takeup effect", and the "true" effect of choice schools on student achievement (β). In the treatment literature (e.g. Rubin (1974), Efron and Feldman (1991)), the parameter π_1 is referred to as the "intent to treat" effect.

There are at least two reasons why we might be interested in π_1 rather than the constituent parameters ρ and β . First, it is the only policy instrument available to policy makers. If the state of Wisconsin decides to provide educational vouchers to all low income students, not all will take advantage of the program and not all who enroll will remain. For example, see Table 2. Of the approximately 300 students who applied in 1990 and were selected to attend a choice school, 98% ever enrolled in a choice school, but only 76% remained through the first spring semester.⁷ By the

⁷ I can only observe if a student was still enrolled in the choice program in the spring if he took an achievement test. If a student was absent the day of the test, this measure has the potential to inflate the attrition from the program. As a result, for this table, if a student is observed to be enrolled the following year, I also assume he is enrolled in the current year (thus, for example, if a student is observed to be enrolled in the spring of 1992 and he applied in 1990, I assume he was

Table 2
Numbers of Applicants, Selections, and Enrollments

	Year of "First" Application			
	1990	1991	1992	1993
Number of Applicants	408	375	314	232
Number Selected	301	310	182	161
# Ever Enrolled in Private School in Fall	294	281	169	136
# Ever Enrolled in Private School in Spring	228	255	164	110
Enrolled in Spring of:				
1991	228			
1992	175	255		
1993	125	180	162	
1994	86	125	128	110

Notes: The year of "first" application is the first year that a student ever applied if they were never selected, and the first year that a student applied and was selected if they applied more than once. "Enrolled" means that a student was enrolled in a private school. Enrolled in the fall means that they were enrolled as of October of the year; enrolled in the spring means they took the Iowa Tests of Basic Skills in a private school indicating that they were still enrolled in the private school in the spring. The numbers in this table are based on African-American and Hispanic students who applied in grades K-8 between 1990 and 1993 (i.e., my analysis sample).

second spring, approximately 58% remained. There are similar patterns in later cohorts. Clearly, ρ can be substantially less than one. In the extreme case in which $\rho=0$, even if private schools are much better at educating children than the public schools, there will be no achievement gains from the program. Thus, the reduced form estimates reflect the overall potential gains from offering the vouchers. Secondly, as in many experimental settings, the randomization only occurred in the intent to treat and as such, the reduced-form estimate is the only unambiguously unbiased estimate that one can obtain from an ordinary least squares (OLS) regression, assuming the initial selection was truly random.

However, the reduced-form estimate is rather unsatisfying for those parents and policy makers who desire an estimate of the benefits that are likely to accrue to those who actually enroll and remain in a private school for a specified period of time (the effect of "treatment on the treated"). That is, it does not establish whether private schools are better than public schools precisely because not all who are assigned to attend a private school do so. Unfortunately, it is also more difficult to estimate whether private schools are better than public schools using the Milwaukee data. If, conditional on X_i and Z_i , take-up and attrition from the program is orthogonal to the error term in equation (2), the estimate of β in equation (2) by OLS will generate an unbiased estimate of the effectiveness of private schools relative to public schools. Greene, et. al. (1996) make this assumption when they exclude from their analysis children who were selected but did not enroll in a private school, and those who left the choice program and re-enrolled in a Milwaukee public school. However, in practice it is unlikely that students who do not take up the program or who leave the program are a random sample of students who applied to the program. In particular, students who are not being well served by the private schools may be more likely to leave, and those who do not

enrolled in the spring of 1991 as well.)

take up the program may not expect to be well served.⁸

I can, however, use the initial selection as an instrumental variable that is correlated with P_{it-1} and uncorrelated with the error term in the education production function. The instrumental variables estimate of β will provide a consistent estimate of a "causal" effect of attending private school for a period of time on test scores (Angrist, Imbens, and Rubin (forthcoming)). However, not all students who start at a private school remain there, rather some receive partial treatment; and I do not have a second instrumental variable that is correlated with students leaving the choice program (and not correlated with the error in equation (2)).⁹ However, I can define the treatment in many ways. For example, I can define the treatment as having attended private school for 1, 2, 3, or 4 years and compare the effect to those who have not attended for as long. For, no matter how one defines P_{it-1} (the treatment), the initial selection is still random.

However, with these definitions of treatment, one must interpret both the OLS and IV estimates of equation (2) with caution. Suppose that the true education production function were,

$$T_{it} = \delta_0 + \beta_0 P_{it-1} + \psi D_{it-1} + \omega_{it} \quad (4)$$

where, as in equation (2), T_{it} represents the test score of student i in year t , P_{it-1} is whether the student was enrolled in a private school, D_{it-1} indicates if a student had previously been enrolled in a private school in the choice program but is now enrolled in a Milwaukee public school (i.e. the student received partial treatment), β_0 represents the achievement gains of students who attended private school relative to students who never attended a private school, ψ is the partial treatment effect of

⁸ Take up was particularly a problem in the first year of the Milwaukee Voucher Program as it was not clear that the program would be declared constitutional. For evidence that there may have been negative selection in the take up in the first year, see Appendix Table 1a.

⁹ I have tried using the miles to the nearest choice school as a second instrumental variable that is correlated with leaving the choice program (and, hopefully, uncorrelated with the error term in the education production function), however the estimated standard errors were so large as to render the exercise uninformative.

private school for those who left the program, and ω_{it} is an error term. (I have excluded the vectors, X_i and Z_i for convenience.) In this case, the OLS estimate of β in equation (2) is likely to be an underestimate of the effect of private school relative to students who have never attended a private school. The reason is that the base group will include students who received partial treatment. That is,

$$\hat{\beta}_{OLS} = \beta_0 + \phi - \Psi\rho' \quad (5)$$

where ϕ is the selection bias in the OLS estimate of β , and ρ' is the proportion of students in the Milwaukee public schools who attended a choice school for some period of time. Except in the cases where there are no students who receive partial treatment or there is no effect of receiving partial treatment ($\rho'=0$ or $\psi=0$), or the selection effect is particularly large, the OLS estimate will be too small, i.e., $\hat{\beta}_{OLS} < \beta_0$.

The opposite problem occurs with the IV estimate of β , as I do not have a second instrumental variable to identify a separate effect for those students who leave the choice program. To see this, consider the point made above that the reduced-form estimate of the "intent to treat" is a function of take-up and the causal effect of private schools on achievement scores, i.e., $\pi_1 = \rho\beta$, or $\beta = (\pi_1/\rho) = (\delta T_{it}/\delta S_{it-1})/(\delta P_{it-1}/\delta S_{it-1})$. This estimate is known as the Wald estimator when the instrumental variable is binary and is equivalent to the IV estimator.¹⁰ To consider the potential bias in the IV estimate¹¹, re-write the true education production function (equation (4)) as,

$$T_{it} = \delta_0 + \beta_0\rho S_{it-1} + \psi(1-\rho)S_{it-1} + \omega_{it} \quad (4')$$

where S_i is whether the individual had been randomly selected to participate in the choice program,

¹⁰ See Wald (1940) and Durbin (1954).

¹¹ See Gruber (1996) for a similar derivation.

and p is the proportion of students selected who are actually enrolled in a private school. Further, consider a modified version of equation (1),

$$P_{it-1} = a + \rho S_{it-1} + v_{it-1} \quad (1)$$

where, again, the vectors X_i and Z_i have been suppressed. From these equations,

$$\hat{\beta}_{IV} = \frac{\frac{\delta T_{it}}{\delta S_{it-1}}}{\frac{\delta P_{it-1}}{\delta S_{it-1}}} = \beta_0 + \psi \Theta \quad (8)$$

assuming that $\delta p / \delta S_i = 0$ (i.e. that the marginal take-up is zero) and letting $\Theta = (1-p)/p$.¹² If $\psi = 0$ (those who receive partial treatment receive no additional benefits), then $\hat{\beta}_{IV} = \beta_0$. In general, however, as long as those who receive partial treatment receive some benefit (and $p < 1$), the IV estimate will be upward biased, i.e., $\hat{\beta}_{IV} > \beta_0$. Combining the biases in the OLS and IV estimators, we can generally expect that $\hat{\beta}_{OLS} < \beta_0 < \hat{\beta}_{IV}$ if private schools generate achievement gains for those who attend for some period of time.

Finally, evaluations using experimental data are not panaceas. One problem with the model outlined above is that I must assume a constant treatment effect across the population in order for the IV estimate to provide a consistent estimate of the common treatment effect. If, however, the effectiveness of private schools varies across the population (there are heterogeneous treatment effects), then the IV estimate may not provide a consistent estimate of even the average treatment effect. Unfortunately, to relax the assumption of a constant treatment effect would require larger samples than available with these data (Heckman (1995a,b), Manski (1992,1995)). A second problem is that randomization must occur at some point in the experiment and because in this program

¹² I estimate a similar range of estimates of the IV bias when I let $\delta p / \delta S_i \neq 0$. In this case, $\Theta = [(1-p) - S_i(\delta p / \delta S_i)] / [p + S_i(\delta p / \delta S_i)]$.

randomization was based on an applicant pool, both the causal and reduced-form estimates reflect the effect of the program or the effect of private schools relative to public schools among students interested in attending a private school. It is not the treatment effect that one would estimate for the general population (Angrist, Imbens, and Rubin (forthcoming), Heckman and Smith (1993)). To assess how sensitive the results are to using the not-selected applicants and to estimate an effect of private schools that would come closer to a treatment effect for the entire at-risk population, I conclude by estimating OLS and fixed-effects estimates using the sample of students from the Milwaukee public schools.

B. Modelling Educational Achievement

A second issue to consider is the proper form of the education production function. In the most basic equation, students' outcomes are modeled as a function of schooling inputs, such as whether the student attended a private school, as in equation (2). By only including one dummy variable indicating attendance at a private school, such a model implies that private schools generate a one time increase in student achievement but that the rate of learning is similar between public and private schools. A more sophisticated model estimates a change in student achievement as a function of schooling inputs (a value-added specification) or includes individual fixed-effects to compensate for a lack of previous schooling inputs and to control for any unobserved differences between students in private and public schools (Hanushek (1979)). This specification suggests that private schools may change the slope of the equation, but because most data have only one or two years of schooling information, one cannot separately identify whether the private schools have changed the slope of the education function, the intercept, or both.

However, the longitudinal structure of the Milwaukee data allows me to estimate richer and less restrictive models. In particular, I can estimate the effects of having been in a private school for some period of time as it is quite possible that a child's achievement does not improve immediately

as she may need some time to adjust to her new environment.¹³ In the basic model, I simply include dummy variables indicating how long the student has attended a private school, that is,

$$T_{it} = \alpha + \sum_{k=1}^4 \beta_k P_{it-k} + X_i \gamma + Z_i \Gamma + \varepsilon_{it} \quad (9)$$

where P_{it-k} are dummy variables indicating the number of years at private school¹⁴, and the vector β_k reflects the impact of previous attendance at private school on current test scores. For example, a pattern in which $\beta_k > \beta_{k-1}$ would suggest that the impact of private schools rises over time, i.e., private schools increase the rate at which children learn. I also constrain the year-to-year gains to be equal, and estimate the linear effect of years in the program on student achievement. Finally, I estimate a spline function that allows me to test for an intercept shift, a change in the slope, and discontinuities in the slope,

$$\begin{aligned} T_{it} = & \alpha' + \alpha'_p P_{it-1} + \delta' Y_{it-1} + \beta' Y_{it-1} P_{it-1} \\ & + \delta'_2 d_2(Y_{it-1}-2) + \beta'_2 d_2(Y_{it-1}-2) P_{it-1} \\ & + \delta'_3 d_3(Y_{it-1}-3) + \beta'_3 d_3(Y_{it-1}-3) P_{it-1} \\ & + X_i \gamma' + Z_i \Gamma' + \varepsilon'_{it} \end{aligned} \quad (10)$$

¹³ This is the argument advanced by Greene, et. al. (1996). Also, according to John Witte, the private schools in the choice program reported they were more concerned about getting the children interested in learning in the first couple of years than in increasing their test scores. Others have interacted school resources with schooling to estimate the effect of school quality on the slope of the production function. See, for example, Akin and Garfinkel (1980), and Behrman and Birdsall (1983).

¹⁴ I use the notation of P_{it-1} suggesting enrollment in a private school, but I also estimate these specifications for the reduced-form equations.

where Y_{it-1} is the number of years since the student applied to the choice program¹⁵, $d_2 = 1$ if the student has been in the choice program for at least two years, and $d_3 = 1$ if a student has been in the choice program at least three years.

III. Sample

The samples I analyze in this paper are drawn from the Milwaukee Parental Choice Program public release data files (Witte and Thorn (1995)). These files contain administrative data on information such as test scores, race, sex, grade level, whether the student applied and was accepted to the choice program, etc., as well as some descriptive information about the Milwaukee public schools the students are currently attending, or attended either pre- or post-enrolling in a choice school. In addition, there is family background information based on respondents to a survey administered each fall and spring to all first time choice applicants from the previous year, and to families from the Milwaukee public school sample in 1991. In total, there are data on approximately 2300 applicants to the choice program, and on a sample of approximately 5300 students from the Milwaukee public schools.

The most difficult feature of the Milwaukee Parental Choice Program public use data set for the modelling framework suggested by equations (1)-(3) is the fact that the data do not indicate the choice school to which a student applied. This is a problem because S_i , the probability of selection, is only random conditional on knowing to which school and grade a student applied. This is because applicants to some schools in a certain grade were more likely to be selected than applicants to other schools and/or grades. Therefore, if one does not control for the "application lotteries" (indicators for the school and grade to which an individual applied), any estimated effect of private schools could be spurious. Formally, this implies that the vector of control characteristics included in equations (1)-(3) must include indicators for the specific "application lotteries". In the notation of

¹⁵ Note that Y_{it-1} will be negative for the pre-application test score observations.

equations (1)-(3), these are represented by the vector, Z_i . Although one cannot observe the precise school to which an individual applied, Greene, et. al. (1996) note that one can infer the lottery in which an individual participated by knowing an individual's race, grade to which she applied, and year of application. They do so by highlighting that over 80 percent of the choice students were enrolled in one of three schools. Almost all students who applied to one of these schools were Hispanic, and almost all students who applied to the two others were African-American. Thus, for African-American and Hispanic students, selection can be assumed random, conditional on a set of dummy variables that represent interactions between a child's race, the grade to which he applied and the year of application.¹⁶

For purposes of most of the analysis in this paper, I use a subsample of the Milwaukee data: those who applied as a voucher student to attend one of the choice schools. I outline, in detail, how I constructed the sample in the Data Appendix, although I highlight some features here. First, I consider a student as having been selected for the choice program if they applied and were selected. I consider a student to have been in the choice program if they took an achievement test while in a private school.¹⁷ For students who either are never selected or are selected the first time they applied, I consider the year of application to be the first year they applied. For those who applied more than once, were initially not selected, but were selected later, I consider their year of application the year in which they were selected. I determine the grade to which a student applied from the grade level of his achievement test the following spring.¹⁸ My basic analysis sample consists of

¹⁶ There are 72 application lotteries (2 races*9 grades*4 years) when all four application cohorts are used. The median number of students in each lottery is about 28.

¹⁷ This means that I do not classify as choice students (in the structural analysis) some students who enrolled in a private school in the fall, but had left the school by the spring.

¹⁸ These are largely the same definitions used by Greene, et. al. (1996). Generally speaking the results are robust to alternative definitions of the year of application and the grade to which a student applied. The one exception is that the third year results (as in Appendix Tables 1a and 1b) are sensitive to the grade at application; that is, using alternative ways of determining the grade to which the student applied, the coefficient drops in magnitude and significance.

African-American and Hispanic students who applied to the choice program between 1990 and 1993 for grades K-8.¹⁹ Sample descriptive statistics are in Appendix Table 2.

I attempt to assess whether the selection was random in Table 3. In the upper panel, I regress the characteristic in the top row on whether the student was selected and (approximately 72) dummy variables indicating the student's applicant pool. In each case, I only include those individuals for whom there is non-missing information on the family background characteristic. It is clear that in four of the five cases, selection appears insignificantly related to the characteristic at the top. In the bottom panel, I "free up" the relationship between being selected and the individual's applicant pool by interacting whether the individual was selected with the applicant pool dummy variables. I present the p-value of the F-test of the joint significance of the interactions. In this case I treat each applicant pool as a separate lottery and test whether, jointly, selection appeared to be random. Whether the individual is female and the student's pre-application test score appear uncorrelated with selection. On the other hand, family income, mother's education, and father's education are not. These last three measures, however, are all drawn from a survey of choice applicants and the fact that the interactions are not jointly zero may reflect the survey response more than the randomization of the experiment. As a result, I consider selection broadly randomly, conditional on the approximations to the applicant pools.

That said, there are notable places where slippage between the actual application lotteries and these constructed lotteries could theoretically occur. First, the grade at the school may not have been oversubscribed. Second, race may not completely determine to which school an individual applied (note, in particular, that African-Americans are assumed to apply to one of two schools). Third, students could apply to more than one school, although I cannot adjust for that with these data.

¹⁹ As the Iowa Tests of Basic Skills does not appear to have been administered to students in grades 9-12, my analysis excludes the private high schools that participated in the program (so do Greene, et. al. (1996) and Witte, et. al (1995). The high schools were predominately "alternative" high schools designed for at-risk students.

Table 3

**Was Selection Conditionally Random?
Conditional Differences in Characteristics Between those Selected to Attend Private School and Those Not Selected**

	Female	Pre-Application Test Score	Family Income	Mother's Education	Father's Education
Constrained Estimate:					
Selected to Attend Private School	0.055 (0.034) [0.104]	1.779 (1.779) [0.318]	342.91 (852.63) [0.402]	0.290 (0.177) [0.102]	0.478 (0.157) [0.002]
Unconstrained Estimate:					
p-value of F-test on the joint significance of interactions between being selected and one's applicant pool	0.713	0.498	0.038	0.017	0.018
Number of Observations	1321	553	709	621	651

Notes: The first row is the regression coefficient on being selected from a regression of the characteristic in the top row on whether selected and applicant pool fixed-effects. Standard errors are in parentheses and p-values are in brackets. The p-values in the second row are based on a regression of the characteristic in the top row on dummies indicating the student's applicant pool and interactions between the applicant pool and whether the student was selected. The pre-application test score is the math test score.

Fourth, the imputation of the grade of application assumes that students continue to progress at grade level. Finally, if the sibling of a student is already enrolled in the choice program, then the student is admitted without having to go through the randomization.

Because I am not completely confident that the approximation perfectly matches the actual application lotteries, I control for whether the student is female, the family income of the student, and in the cross-sectional analyses by year, the student's pre-application test score (these constitute the elements in the vector X_i in equations (1)-(3)).²⁰ As noted above, the family income has a high rate of missing values as only 60% of those selected and about 43% of those not selected have non-missing values. One may be concerned that including this measure biases against finding an effect of the program due to the differential response rates. A typical concern might be that only the wealthier families among those not selected may have responded to the survey. However, as shown in Table 3, those who were selected had marginally higher levels of family income and parental education than those who were not selected. Thus, while controlling for family income may come at some cost, I believe that it results in a more credible estimate of the effectiveness of the program. The pre-application test score is from the year the student applied to the program. Approximately one-half of the students are missing these test scores (over one-half of those missing scores applied to kindergarten or first grade) and so I also include a dummy variable indicating that it is missing. In the analyses in which I stack the data (and create a panel), any pre-application test scores are included as additional (pre-application) observations. I also use the panel data to estimate models with individual fixed-effects which completely avoids the problem of missing observable covariates.

The test scores are the normal curve equivalent reading and math scores from the Iowa Tests of Basic Skills. These tests were administered to the choice students every spring. They were also administered to Chapter I students every year. Other students in the Milwaukee public schools were

²⁰ I have also tried controlling for mother's and father's education as well, with similar results.

tested less frequently (reportedly in grades 2, 5, and 7).²¹ Beginning in 1993, schools were no longer required to administer the entire battery of math sub-tests in order to receive Chapter I funds, rather they were only required to administer the problem solving component.²² As a result, I do not observe a "total math score" for a substantial fraction of students in the Milwaukee public schools.²³ At the same time, the public release version of the data do not include the problem solving component of the math tests for the students in the choice program. Fortunately, a subset of students in the Milwaukee public schools were administered the entire battery of math tests. Therefore, I impute the total math score by regressing the total score on the problem solving component using the random sample of students in the Milwaukee public schools who did not apply to choice and who took the entire test (see the Data Appendix for the equations). I use the predicted total score for those with only the problem solving component, and include a dummy variable indicating whether the score was imputed. I explore the likely effect of this imputation by comparing it with alternative ways of handling the missing data in Appendix Table 3. I believe that the results are fairly robust to alternative ways of handling this missing data problem.

IV. Results

A. *Evaluating the Choice Program: Reduced-Form Estimates*

I begin by estimating the mean effect of selection to the choice program on math and reading test scores for each calendar year, i.e., equation (2). The results are in Tables 4a and 4b. In the upper

²¹ There are very few students in the Milwaukee public schools with test scores from every spring, and the pattern of missing grades does not obviously suggest that all students were tested in grades 2, 5, and 7.

²² And in 1995, the Milwaukee public schools began administering state mandated tests rather than the Iowa Tests of Basic Skills (Witte, Sterr, and Thorn (1995)); as such I cannot evaluate the program using the 1995 test scores.

²³ In 1993 approximately 40% of the control group is missing the total score, and the percentage rises to 68% in 1994.

Table 4a

The Mean Effect of Selection to the Choice Program on Math Scores: By Year of the Test

	Without Other Covariates			
	1991	1992	1993	1994
	(1)	(2)	(3)	(4)
Selected to Attend Private School	3.143 (2.895)	0.219 (2.035)	3.493 (1.895)	2.793 (2.167)
Female	-0.607 (2.288)	-1.029 (1.587)	1.089 (1.394)	1.518 (1.417)
Imputed Test Score			1.475 (2.097)	1.505 (1.951)
R ²	0.128	0.059	0.125	0.165
Number of Observations	280	585	668	740
	With Other Covariates			
	1991	1992	1993	1994
	(5)	(6)	(7)	(8)
Selected to Attend Private School	1.851 (2.612)	-0.078 (1.966)	2.775 (1.790)	3.498 (2.021)
Female	-0.675 (2.052)	-0.690 (1.513)	1.194 (1.317)	1.384 (1.316)
Imputed Test Score			1.819 (2.009)	1.113 (1.811)
Family Income (+1000)	0.335 (0.161)	0.355 (0.128)	0.342 (0.108)	0.362 (0.105)
Family Income Missing	-2.115 (2.090)	-0.432 (1.534)	-1.020 (1.331)	-0.772 (1.317)
Pre-Test Math Score	0.586 (0.075)	0.490 (0.068)	0.472 (0.057)	0.584 (0.057)
Pre-Test Score Missing	-1.023 (2.370)	-3.085 (1.879)	-0.692 (1.602)	-1.616 (1.682)
R ²	0.313	0.154	0.228	0.290
Number of Observations	280	585	668	740

Notes: Standard errors are in parentheses. All regressions include a constant and "applicant pool" fixed effects.

Table 4b

**The Mean Effect of Selection to the Choice Program on Reading Scores:
By Year of the Test**

	Without Other Covariates			
	1991	1992	1993	1994
	(1)	(2)	(3)	(4)
Selected to Attend Private School	5.626 (2.599)	1.841 (1.788)	-0.278 (1.618)	1.536 (1.691)
Female	2.518 (2.058)	4.047 (1.426)	2.255 (1.270)	3.696 (1.272)
R ²	0.077	0.082	0.083	0.097
Number of Observations	270	573	675	731
	With Other Covariates			
	1991	1992	1993	1994
	(5)	(6)	(7)	(8)
Selected to Attend Private School	5.953 (2.357)	1.588 (1.745)	-0.775 (1.553)	1.463 (1.594)
Female	1.408 (1.861)	3.922 (1.367)	2.472 (1.211)	3.905 (1.186)
Family Income ($\div 1000$)	0.246 (0.140)	0.246 (0.115)	0.411 (0.099)	0.288 (0.095)
Family Income Missing	-1.424 (1.880)	-0.470 (1.396)	0.142 (1.214)	-2.236 (1.177)
Pre-Test Reading Score	0.732 (0.093)	0.481 (0.070)	0.440 (0.060)	0.595 (0.060)
Pre-Test Score Missing	0.326 (2.114)	-3.514 (1.654)	0.033 (1.493)	-1.180 (1.493)
R ²	0.267	0.164	0.177	0.226
Number of Observations	270	573	675	731

Notes: Standard errors are in parentheses. All regressions include a constant and "applicant pool" fixed effects.

panels of both tables, I report estimates of the "intent to treat" without controlling for family income and pre-application test scores; in the bottom panel I report estimates that control for these covariates.²⁴ In all cases, I include a constant and fixed effects for the applicant pools described above. In the upper panel of Table 4a, I generally find that the students selected to attend private school scored approximately 3 (roughly) percentage points higher on the math test, although the treatment effect is only statistically significant (at the 10% level) for 1993 (column (3)). In the bottom panel when I control for other covariates, the estimated coefficient falls in 1991, 1992, and 1993 by about 20-40%. On the other hand, the estimate from 1994 increases by 25%. Again, the point estimates suggest that the program has a mean effect of raising math test scores 2-3 percentage points, the p-value of the effect in 1993 is 0.122 and that in 1994 is 0.084.

The results for reading scores are in Table 4b. In both panels, I estimate a large, 5.6-6 percentage point increase in reading scores in 1990 (that is statistically significant at the 5 percent level) for those selected to the choice program. However, the point estimates in later years suggest a smaller intent to treat effect of only about 1.5 percentage points, although the standard errors are generally so large as to make inference difficult. Note, again, that the effect of conditioning on family background leads to an increase in the coefficient in one year (1991) and a slight decrease in all others.

In Tables 5a and 5b, I estimate whether the choice program altered the slope of the education production function, rather than just the intercept, by estimating the effect of years since selection on test scores, both constraining the effect to be linear and allowing the effect to differ by year and cohort. In the top panel of Table 5a, I estimate the linear effect of years since selection on math test

²⁴ I do not control for the grade of the student at the time of the test. This appears to make little difference to the coefficient estimates on the effect of the choice program. I, however, note the instances in which it does affect the estimates for the control group. I do not include the grade in the results presented here since whether a child is at grade level could be considered an outcome itself while also being a policy instrument of the school. It appears that approximately 5% of the students are held back in any year (and the overall rate does not differ between the public and private schools). Controlling for whether the student was held back does not affect the results.

Table 5a

**The Effect of Selection to the Choice Program on Math Scores:
By Year and Years Since Selection**

	Year of Test Score			
	1991	1992	1993	1994
	(1)	(2)	(3)	(4)
Number of years since selection	1.851 (2.612)	0.272 (1.169)	1.116 (0.798)	1.685 (0.689)
R ²	0.313	0.154	0.227	0.293
Number of Observations	280	585	668	740
	Year of Test Score			
	1991	1992	1993	1994
	(5)	(6)	(7)	(8)
1st year after selection (Year 1)	1.851 (2.612)	-2.311 (3.120)	3.785 (2.977)	-1.377 (4.404)
2nd year after selection (Year 2)		1.360 (2.509)	0.699 (3.128)	1.146 (3.282)
3rd year after selection (Year 3)			3.514 (2.796)	1.868 (4.182)
4th year after selection (Year 4)				9.328 (3.354)
p-value of F-test of jt. sign. of being selected		0.653	0.383	0.091
p-value of H ₀ : Year1=Year2		0.357	0.460	0.631
p-value of H ₀ : Year2=Year3			0.494	0.888
p-value of H ₀ : Year4=Year3				0.154
p-value of H ₀ : Year3=Year1			0.946	0.582
p-value of H ₀ : Year4=Year2				0.072
p-value of H ₀ : Year4=Year1				0.049
R ²	0.313	0.155	0.229	0.295
Number of Observations	280	585	668	740

Notes: Standard errors are in parentheses. All regressions include a constant, "applicant pool" fixed effects, a dummy indicating if female, family income, and pre-application math test score, and indicators if income and pre-test score are missing. Columns (3), (4), (7), and (8) also include a dummy indicating if the test score was imputed.

Table 5b

**The Effect of Selection to the Choice Program on Reading Scores:
By Year and Years Since Selection**

	Year of Test Score			
	1991	1992	1993	1994
	(1)	(2)	(3)	(4)
Number of years since selection	5.593 (2.357)	1.102 (1.035)	0.121 (0.709)	0.576 (0.564)
R ²	0.267	0.165	0.176	0.226
Number of Observations	270	573	675	731
	Year of Test Score			
	1991	1992	1993	1994
	(5)	(6)	(7)	(8)
1st year after selection (Year 1)	5.593 (2.357)	0.155 (2.779)	-3.619 (2.634)	3.138 (3.938)
2nd year after selection (Year 2)		2.498 (2.221)	-1.360 (2.840)	-0.812 (2.698)
3rd year after selection (Year 3)			2.379 (2.562)	1.366 (3.488)
4th year after selection (Year 4)				3.306 (2.961)
p-value of F-test of jt. sign. of being selected		0.531	0.396	0.712
p-value of H ₀ : Year1=Year2		0.508	0.558	0.407
p-value of H ₀ : Year2=Year3			0.327	0.620
p-value of H ₀ : Year4=Year3				0.670
p-value of H ₀ : Year3=Year1			0.103	0.735
p-value of H ₀ : Year4=Year2				0.304
p-value of H ₀ : Year4=Year1				0.973
R ²	0.267	0.165	0.180	0.227
Number of Observations	270	573	675	731

Notes: Standard errors are in parentheses. All regressions include a constant, "applicant pool" fixed effects, a dummy indicating if female, family income and pre-application reading test score, and indicators if income and pre-test score are missing.

scores in each year. In 1993 (column (3)), each additional year since being selected led to a 1.1 percentage point increase in math scores (p-value of 0.16). In 1994, the effect was even larger, 1.7 percentage points, and the yearly increase is significantly different from zero.

Note that in 1991 the only cohort included in the analysis is the group of students who applied in 1990; in 1992, the cohorts who applied in 1990 and 1991 are included, and so on. Thus, in the bottom panel one can follow a cohort by comparing the coefficients on the diagonals. For example, the lowest diagonal represents the yearly progress of the 1990 cohort; the diagonal just above it represents the progress of the 1991 cohort. In the first year, the 1990 cohort had an increase in test scores of 1.9 percentage points; in 1991, the same cohort had a difference of 1.4 percentage points. However, by 1993, the gap between those selected and those not selected had grown to 3.5 percentage points and by 1994 the gap was 9.3 percentage points. (The fourth year is the only one that is statistically significantly different from earlier years, and it is only different from the first and second years.) The 1991 cohort had a similar growth in their test scores, although the coefficient estimates are not significantly different from zero. The point estimates in this table suggest that there may have been faster math achievement gains among the private school students than among the public school students.

The magnitudes of the point estimates for reading, in Table 5b, are not so clear. In the top panel, I again find that the yearly increase, though positive, is far from statistically significant in 1992-1994. In the bottom panel, the yearly progress of the 1990 applicant cohort was not monotonic as they experienced a huge increase in test scores in the first year that was unmatched in subsequent years. On the other hand, the coefficient estimates for both the 1991 and 1992 cohorts suggest yearly gains, although the standard errors are quite large as well. In general, the results from Tables 5a and 5b suggest that being selected to a choice program may have altered the path of educational achievement, and not just the mean, although the confidence intervals are generally large.

In the previous tables, I estimated separate effects for each cohort in each year. There may

be some efficiency gain by fitting a pooled model that assumes the yearly effects are similar across cohorts and years. To implement this pooled model, I created a panel data set from my analysis sample, treating each year in which students took an achievement test (including pre-application tests) as a separate observation. I then estimated spline functions (see equation (10)) which allow me to measure the achievement path of the control group and test for discontinuities in the slope of the achievement gains. I present OLS estimates with Huber standard errors that allow for individual correlations and that are also robust to heteroskedasticity. I also present models that include individual fixed-effects.²⁵ In Tables 6a-6c I only present the effects on math test scores as I found no significant discontinuities or significant differences between the control and treatment groups for reading scores.²⁶

The basic results for the pooled model are in Table 6a. In the first specification (columns (1) and (2)), I estimate a basic spline function in which I allow only the intercept and slope to differ between the control and treatment groups. The estimates in column (1) are by OLS and those in column (2) include individual fixed-effects (FE). Both columns indicate that the intercept for those selected to the choice program is slightly lower than that for the control group, although the difference is not statistically different from zero. On the other hand, it appears that the two groups have significantly different slope estimates. The math test score for those who were not selected for the program decreased, on average, approximately one percentage point per year, however this may be due to students being in higher grades.²⁷ The test scores of those selected for the program increased an additional (statistically significant) 1.5 percentage points per year.

²⁵ I have also estimated generalized least squares random effects models with similar results.

²⁶ The results for reading scores are available from the author upon request.

²⁷ When I control for the grade at the time of the test, the control group actually had a positive (and significant) 4 percentage point increase in test scores per year. The magnitude and statistical significance of the coefficient on the interaction between years since application and whether selected remains basically unchanged.

Table 6a

The Effect of Selection to the Choice Program on Math Scores, 1991-1994: OLS and Fixed-Effects (FE) Spline Functions

	OLS	FE	OLS	FE	OLS	FE	OLS	FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Selected to attend private school (α'_p)	-0.841 (1.061)	-1.539 (0.962)	-0.062 (1.347)	-1.125 (1.287)	-0.197 (1.108)	-1.430 (1.032)	-0.271 (1.355)	-1.133 (1.297)
Number of years since application (δ')	-1.056 (0.384)	-0.952 (0.350)	-0.698 (0.440)	-0.760 (0.380)	-0.809 (0.403)	-0.830 (0.362)	-0.744 (0.443)	-0.773 (0.381)
Number of years since application*Selected (β')	1.509 (0.510)	1.487 (0.468)	0.459 (0.868)	0.916 (0.815)	0.779 (0.580)	1.252 (0.543)	0.744 (0.879)	0.944 (0.830)
≥ 2 Years (δ'_2)			-2.412 (1.445)	-1.575 (1.245)			-0.841 (2.113)	-0.887 (1.917)
(≥ 2 years)*Selected (β'_3)			3.516 (1.845)	2.199 (1.647)			0.757 (2.524)	1.436 (2.335)
≥ 3 Years (δ'_3)					-5.465 (2.815)	-3.149 (2.476)	-4.128 (4.071)	-1.799 (3.813)
(≥ 3 Years)*Selected (β'_3)					8.028 (3.242)	3.735 (2.847)	6.752 (4.545)	1.967 (4.194)
p-value of $H_0: \delta'_1=0, \delta'_2=0, \delta'_3=0$			0.006	0.012	0.003	0.011	0.010	0.027
p-value of $H_0: \beta'_1=0, \beta'_2=0, \beta'_3=0$			0.007	0.011	0.002	0.004	0.007	0.027
p-value of $H_0: \delta'_2=0, \delta'_3=0$							0.151	0.402
p-value of $H_0: \beta'_2=0, \beta'_3=0$							0.066	0.366
p-value of $H_0: \delta'_2=\delta'_3$							0.571	0.866
p-value of $H_0: \beta'_2=\beta'_3$							0.359	0.930
R ²	0.098	0.008	0.099	0.009	0.099	0.009	0.010	0.009

Notes: There are 3283 observations. Standard errors are in parentheses. The OLS columns report Huber standard errors that allow for correlations "within" an individual (since these data are stacked). The OLS regressions include a constant, "applicant pool" fixed effects, a dummy indicating if female, family income, and an indicator if income is missing. All regressions include a dummy indicating if the test score was imputed.

Table 6b

**The Effect of Selection to the Choice Program on Math Scores, 1991-1994: Robustness of Spline Functions
(OLS Estimates)**

	Basic Regression*			Spline Function with Discontinuities*		
	(1)	(2)	(3)	(1)	(2)	(3)
Selected to attend private school (α'_p)	-0.841 (1.061)	-2.335 (1.392)	-0.079 (1.112)	-0.062 (1.347)	-3.395 (1.693)	-0.167 (1.355)
Number of years since application (δ')	-1.056 (0.384)	-0.559 (0.484)	-0.871 (0.411)	-0.698 (0.440)	-0.566 (0.498)	-0.798 (0.447)
Number of years since application*Selected (β')	1.509 (0.510)	1.454 (0.773)	0.755 (0.581)	0.459 (0.868)	2.305 (1.093)	0.720 (0.879)
≥ 3 Years (δ'_2)				-2.412 (1.445)	0.391 (3.702)	-0.960 (2.133)
$(\geq 3 \text{ years}) \cdot \text{Selected}$ (β'_2)				3.516 (1.845)	-2.372 (4.158)	0.855 (2.537)
p-value of $H_0: \delta'=0, \delta'_2=0$				0.006	0.518	0.093
p-value of $H_0: \beta'=0, \beta'_2=0$				0.007	0.104	0.449
R ²	0.098	0.114	0.100	0.099	0.114	0.100
Number of Observations	3283	2102	3096	3283	2102	3096

Notes: Huber standard errors (that allow for correlations within an individual) are in parentheses. All regressions include a constant, "applicant pool" fixed effects, a dummy indicating if female, family income, an indicator if income is missing, and a dummy indicating if the test score was imputed.

* Column Descriptions:

- Column 1: Uses Full Sample
- Column 2: Only Uses 1991-1993 Cohorts
- Column 3: Excludes 1994 Test Scores for 1990 Cohort

Table 6c

**The Effect of Selection to the Choice Program on Math Scores, 1991-1994:
Robustness of Spline Functions
(Individual Fixed-Effects Estimates)**

	Basic Regression*		
	(1)	(2)	(3)
Selected to attend private school (α'_p)	-1.539 (0.962)	-3.024 (1.297)	-1.494 (1.040)
Number of years since application (δ')	-0.952 (0.350)	-0.691 (0.402)	-0.756 (0.369)
Number of years since application*Selected (β')	1.487 (0.468)	1.955 (0.686)	1.156 (0.547)
R ²	0.008	0.010	0.005
Number of Observations	3283	2102	3096

Notes: Standard errors are in parentheses. All regressions include individual fixed-effects and a dummy indicating if the test score was imputed.

* Column Descriptions:

Column 1: Uses Full Sample
Column 2: Only Uses 1991-1993 Cohorts
Column 3: Excludes 1994 Test Scores for 1990 Cohort

In the remaining columns, I attempt to establish if there were discontinuities in the achievement gains, as Greene, et. al. (1996) argue that the choice program led to significant jumps in achievement in the third year and then again in the fourth year. In columns (3) and (4) I allow for a discontinuity three years after selection. Now, the linear effect of being selected for the program (β') suggests a positive, but insignificant yearly increase in test scores of about 0.5-1 percentage point. On the other hand, the OLS estimate suggests there was a break in the slope at the third year for the treatment group that is statistically significant. In columns (5) and (6) I allow for a break in the slope in the fourth-year, and not the third, and find a similar result for the OLS estimate. The fixed-effects estimate, in column (6), indicates the linear effect of being selected was positive (and significant at the 5% level), but there was an insignificant increase in test scores in the fourth year. Finally in the last two columns I allow for discontinuities in both the third and the fourth years. Allowing for both reveals that the discontinuity in the third year estimated in column (3) was likely driven by the increase in the fourth year; further, the jump in test scores in the fourth year is no longer significant for either the OLS or fixed-effects estimates. Also note that the control group had a significant downward jump in test scores in the fourth year, as well, increasing the intent to treat effect.

As highlighted in Table 5a, the mean difference in test scores between those selected and those not selected was much higher in the fourth year for the 1990 applicant cohort than for any other year or cohort. To assess the extent to which the results might be driven by this cohort, I tried using alternative samples. First, I used the full sample. Second, I estimated the spline functions excluding the 1990 cohort. Third, I excluded the 1994 test scores for 1990 cohort, but included their 1991-1993 test scores. The results are in Tables 6b and 6c. The first three columns of Table 6b are the basic spline functions that do not allow for discontinuities in the yearly gains; the second three columns allow for a discontinuity in the third year.²⁸ The results for the basic spline function in the

²⁸ Given that I did not estimate significant discontinuities using the fixed-effects estimator, I do not present those results in Table 6c. I continue to find no significant effects using the alternative samples; these results are available from the author upon request.

first three columns are quite similar across the first two samples. The test scores of those selected for the program increase by about 1.5 percentage points each year above those who were not selected. Excluding the 1994 test scores for the 1990 cohort, however, the coefficient on the yearly increase in test scores drops by about one-half, and the estimate is no longer statistically significant. This differs from the fixed-effects estimates in Table 6c in which I estimate a (statistically significant) yearly increase of 1-2 percentage points per year using all three samples.

In the last three columns of Table 6b I allow for a discontinuity in the third year. In this specification, the three samples all suggest positive (in columns (1) and (3)), although insignificant, yearly gains in the treatment group relative to the control group. However, the discontinuity in the third year only obtains in the first column which includes the 1994 test scores from the 1990 cohort. Thus, while the yearly gains appear fairly robust, it is much less clear that there are discontinuities in the trend.²⁹

Before I turn to "causal" estimates of the relative effectiveness of private schools, I assess the potentially important effect of sample attrition on the parameter estimates. For example, only 46% of students who applied in 1990 had non-missing 1994 test scores (49% among those selected for the program and 35% among those not selected). Test scores may be missing for four reasons. First, the student may have still been enrolled in a choice or Milwaukee public school, but was absent the day of the test. Test score data missing for this reason can likely be considered random. Second, as mentioned earlier, students in the Milwaukee public schools were not necessarily tested every year. For those who were not tested because the school did not test their grade level, their scores could also be considered randomly missing. However, Chapter I students were allegedly tested every year which would suggest that those missing data from the Milwaukee public schools come from families with higher incomes than those who were tested more frequently (or that a student whose family

²⁹ I have also tried excluding those students who leave the choice school because they reached the terminal grade for the school. The results remain essentially unchanged.

income increases would be tested frequently in the earlier years, but tested less frequently in later years). Third, it appears as though students in grades 9-12 were not tested; therefore, older applicants would be missing test scores in later years. Finally, any student who enrolled in a private school not in the choice program or who transferred to a public school outside of Milwaukee is missing test scores.³⁰ Again, it is not clear that one should assume this group is randomly missing from the sample.

Figures 1a-1d are kernel densities of the distributions of (conditional) pre-application test scores by whether the student is missing a test score in 1994 or not, separately for the treatment and control groups.³¹ The distributions of the pre-application math scores are in Figures 1a and 1b. For both those selected for the program and those not selected, the pre-application math test score distributions of those missing the 1994 math test score and those with test scores largely overlap. Among the controls, however, it appears that those without test scores are of slightly lower "ability". On the other hand, for the treatment group, it appears that those without 1994 test scores are of slightly higher "ability". Together, these figures suggest that the previous estimates do not overstate the effectiveness of the choice program on math scores, and may well understate its effectiveness. Similar distributions for reading scores are in Figures 1c and 1d. There is even more overlap in the distributions for the reading scores, although in both figures, those with 1994 reading test scores appear to be marginally positively selected.

³⁰ Witte (1996) argues that a large fraction of students who were not selected in the lotteries chose to attend another private school (not participating in the choice program). This was made easier by a parallel, privately funded program (Partners for Advancing Values in Education (PAVE)) that provided scholarships for Catholic schools. It is for this reason that Witte, Sterr, and Thorn (1995) prefer to use the random sample of students from the Milwaukee public schools as a comparison group.

³¹ These test scores are adjusted for whether the student is female, family income and applicant pool fixed effects, and for the math scores, whether the test was imputed because the student only took the problem solving component of the math test.

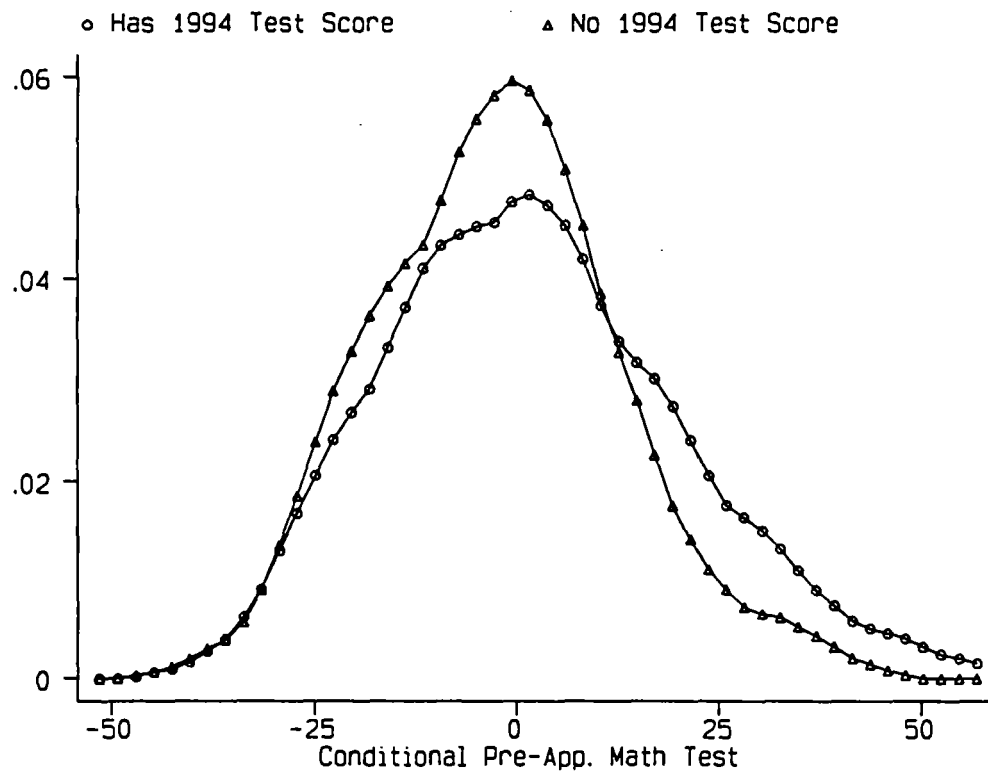


Figure 1a. Kernel Densities for Control

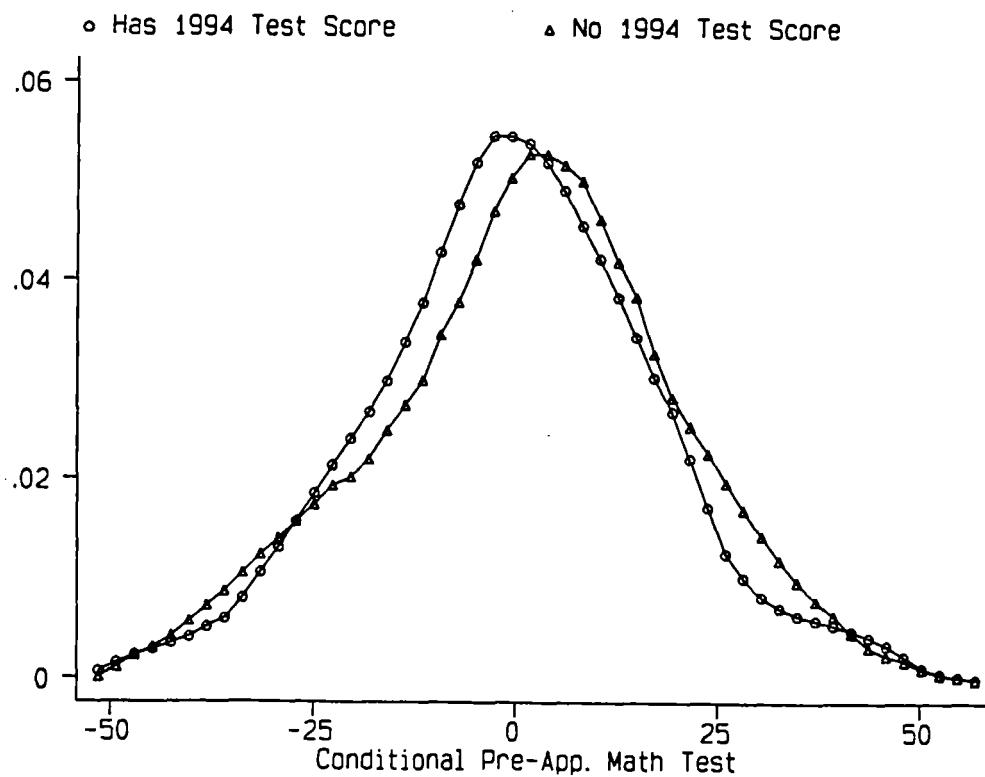


Figure 1b. Kernel Densities for Treatment

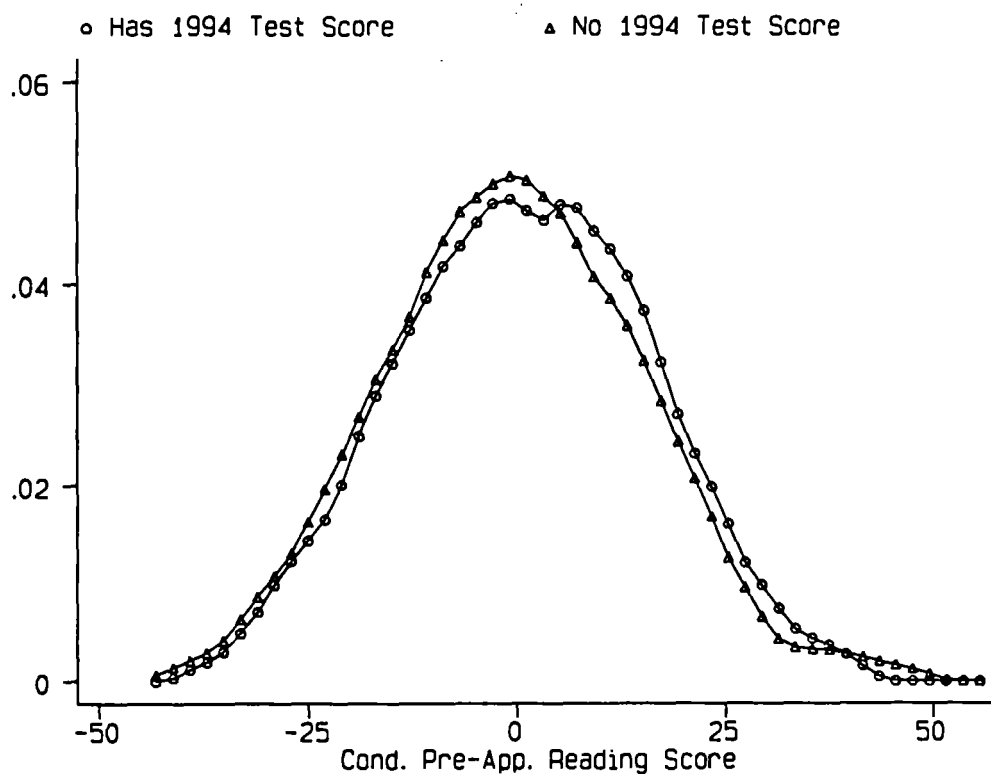


Figure 1c. Kernel Densities for Control

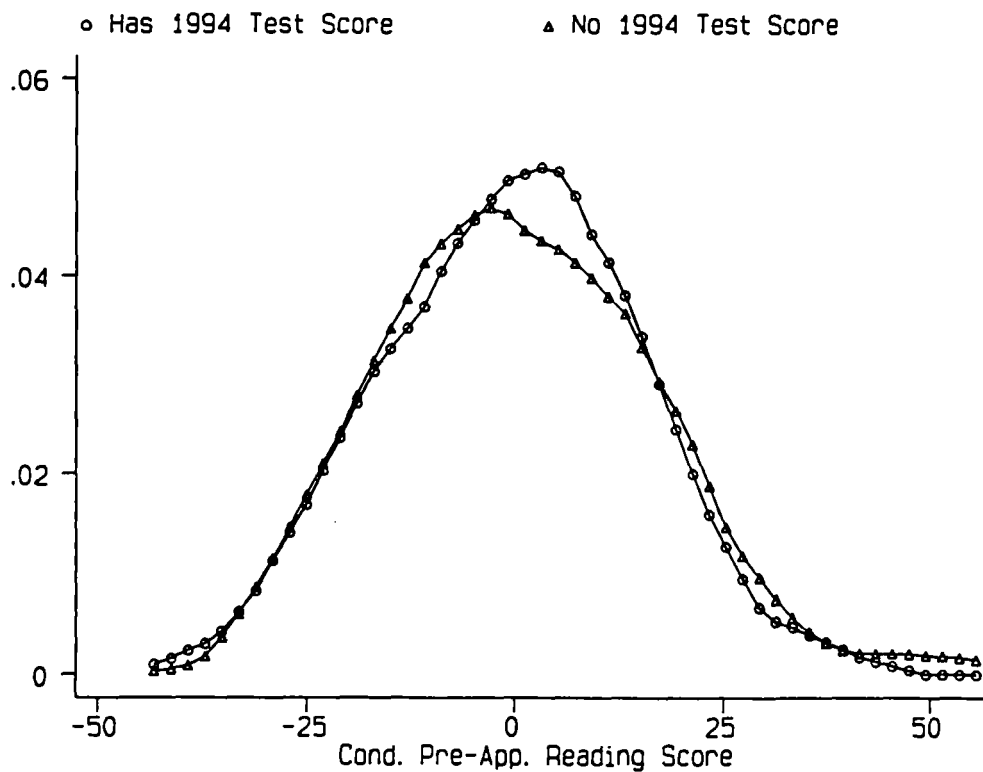


Figure 1d. Kernel Densities for Treatment

I more formally assess the likely effect of missing test scores on the results in Table 7.³² In this table I employ four alternative methods for handling the missing test score data. In the first column I present OLS estimates using my analysis sample. In column (2) I use a two-step Heckman selection correction in which I exclude the grade of the student in the year of the test from the second stage.³³ The point estimates for both the math and reading scores increase when I use a Heckman selection correction, and in both cases the estimate of λ is negative and significant. One problem with using a Heckman selection correction in this context is that there is not one underlying reason for the missing test scores. As a result, there is no single latent variable that may underlie the first stage probit.

In columns (3) and (4) I attempt to impute a test score for those missing a test score in two ways. In column (3) I first regress the current test score on the pre-application test score and, for the math scores, a dummy indicating whether the current test score was imputed (to account for the fact that the student did not take the entire battery of math tests).³⁴ I run separate regressions for the treatment and control groups. I then use the predicted test score as the current test score for those with missing data. For reading scores, this imputation does not substantively change the results from those in column (1). However, for math, the point estimate decreases by one-half and becomes statistically insignificant. In column (4) I impute the current test score by computing each student's decile in the (conditional) pre-application test score distribution. I then assign the mean test score from the corresponding decile in the test score distribution for those missing test scores in the current year. The point estimates using this approach are similar to those in column (1) for both reading and

³² For this analysis, I exclude individuals who are missing test scores and are enrolled in either a pre-kindergarten program or in grades 9-12.

³³ I use the grade at the time of the test as it was alleged that in the Milwaukee public schools, the tests were more consistently given in certain grades. First-stage estimates are available from the author upon request.

³⁴ The "pre-application test score" in this context is the test score from the year of application. I have also tried using higher orders of the pre-application test score with very similar results.

Table 7
OLS Estimates of the Effect of Selection to the Choice Program on Math and Reading Scores: Robustness to Sample Attrition

	Baseline	Two-Step Heckman Selection	Impute Missing by Method 1 [†]	Impute Missing by Method 2 ^{††}	Only use those with no missing test score data
	(1)	(2)	(3)	(4)	(5)
Math Scores					
Selected to attend private school (α'_p)	-0.841 (1.187)	-6.223 (1.238)	0.562 (0.698)	-0.147 (0.710)	-2.083 (1.629)
Number of years since application (δ')	-1.056 (0.399)	-1.416 (0.362)	-0.359 (0.179)	-0.457 (0.182)	-1.366 (0.611)
Number of years since application*Selected (β')	1.509 (0.574)	2.574 (0.569)	0.718 (0.325)	1.222 (0.331)	2.358 (0.822)
λ		-8.999 (0.875)			
R ²	0.098		0.072	0.068	0.128
Number of Observations	3283	6389	6389	6389	1607
Reading Scores					
Selected to attend private school (α'_p)	1.576 (1.051)	-1.556 (1.148)	0.892 (0.591)	1.984 (0.628)	2.010 (1.388)
Number of years since application (δ')	-0.794 (0.331)	-1.170 (0.328)	-0.506 (0.147)	-0.565 (0.156)	-0.721 (0.489)
Number of years since application*Selected (β')	-0.200 (0.517)	0.424 (0.520)	-0.046 (0.278)	-0.158 (0.296)	0.133 (0.727)
λ		-6.254 (0.899)			
R ²	0.067		0.049	0.049	0.105
Number of Observations	3268	6391	6391	6391	1567

Notes: Standard errors are in parentheses. See note to Table 5b for other regressors.

* The grade of the individual at the time of the test and interactions of the grade with whether the individual was randomly selected are excluded from the first stage equation.

[†] "Method 1" regresses the current test score on pre-application test score, and for math whether the current test score was imputed (because the student did not take the entire test battery) separately for the treatment and control groups. The predicted current test score is used to impute the actual test score if the test score is missing.

^{††} "Method 2" assigns those missing the current test score their application cohort's mean test score for the decile of the selected or not-selected distribution corresponding to their decile in their application cohort's pre-application test score distribution.

math.³⁵ Finally, in column (5) I restrict my sample to those without missing test scores (note that the sample size falls dramatically). These point estimates are a little higher than those in column (1) for math, and are now positive, but insignificant, for reading.

These results (together with Figures 1c and 1d) suggest that missing reading test scores are not likely a large source of bias for the estimates of the intent to treat. For math, the evidence broadly indicates that the finding of a significant and positive effect of being selected for the program is robust to the missing test scores, although one should keep the sample attrition in mind when interpreting the results.

In all, being selected to participate in the choice program appears to have increased the math achievement of low-income, minority students by about 1-2 percentage points per year. Given a standard deviation of about 19, this suggests effect sizes on the order of 0.05σ - 0.11σ which are quite large for education production functions (see, for example, Greenwald, Hedges, and Laine (in press)). On the other hand, the effects on the reading scores are not as large and are statistically indistinguishable from zero.

B. *"Causal" Estimates of the Effectiveness of Private Schools*

In this section, I estimate OLS models of the effect of attending private school on test scores, and instrumental variables (IV) estimates using the initial selection as the instrumental variable. Note that I define "attending a private school" as having taken an achievement test in a choice school in the particular year. The first-stage estimates (equation (1)) are presented in Table 8. In all years, having been randomly selected to participate in the choice program increases a student's likelihood of actually attending a private school by about 70 percentage points.³⁶ The estimate is statistically

³⁵ Note, however, that I have not adjusted the standard errors to reflect the two-step estimation strategy; the standard errors in columns (3) and (4) are likely understated.

³⁶ A few students who were not selected initially were enrolled in the choice program in the following spring. They were likely admitted off of a waiting list.

Table 8**Linear Probability Estimates of the Effect of Being Selected on Private School Attendance
(First-Stage Estimates)**

	Dependent Variable			
	In Private School in 1991	In Private School in 1992	In Private School in 1993	In Private School in 1994
	(1)	(2)	(3)	(4)
Selected	0.761 (0.050)	0.703 (0.038)	0.679 (0.039)	0.681 (0.040)
R ²	0.589	0.529	0.478	0.476
Number of Observations	280	585	668	740

Notes: Standard errors are in parentheses. All regressions include a constant, "applicant pool" fixed effects, a dummy indicating if female, family income, and pre-application math test score, and indicators if income and pre-test score are missing.

significant in all years, as well.

The mean achievement differences between students in the private schools and those in the public schools are presented in Table 9. The first four columns present the OLS estimates, and the last four the IV estimates. For the math scores, in three of the four years, the OLS estimates suggest that those who attended a private school scored lower on the math test than those who were not enrolled in a private school in that year, although these differences are not statistically significant. If, however, there is an achievement effect for those who receive partial treatment (i.e., attend a private school and then leave and return to a Milwaukee public school), the OLS estimate is likely to be an underestimate of the full treatment effect (see equation (5)). In the later years in these data, $\rho' \approx 0.5-0.65$; and, on average, those who have left the choice program received approximately one-half of the treatment as those stayed. Therefore, assuming that $\beta_0 = 2\psi$, one would expect the OLS estimate to understate the full effect by 30-50% (plus an adjustment for the selection effect). That the OLS coefficient estimates are zero, or slightly negative, suggests that those who received partial treatment may have benefitted as much as, or even slightly more than, those who received full treatment. These OLS estimates could be interpreted as a lower bound on the full treatment effect.

As discussed in Section II, the IV estimate is a re-scaled version of the reduced-form parameter, namely $\beta_{IV} = (\pi_1/\rho)$. Therefore, using the estimates of ρ from Table 8, we would expect that, on average, the IV estimate will be approximately $1.4\pi_1$. And this is the case. For example, in Table 9, the 1991 IV estimate is 2.43 which is $1.85/0.76$ (which are the estimates of π_1 and ρ from Tables 4a and Table 8). In general, the IV estimates show that students who were enrolled in the private schools scored approximately 3 percentage points higher than those not enrolled in a private school, although the effect is only significant in 1994. For reading, both the OLS and IV estimates suggest a significant 5-8 percentage point gain for those enrolled in private schools in 1991, although this is not evident in other years. Finally, as noted in equation (8) in Section II, these IV estimates likely provide an upper bound on the full treatment effect because of non-compliance. Again using

Table 9

The Mean "Causal" Effect of Private School Attendance on Math Scores: By Year

	OLS				IV			
	Year of Test Score				Year of Test Score			
	1991	1992	1993	1994	1991	1992	1993	1994
Math Scores								
Attended private school	-0.759 (2.365)	-0.730 (1.737)	2.581 (1.797)	-0.336 (2.377)	2.433 (3.447)	-0.110 (2.795)	3.238 (2.468)	4.327 (2.681)
R ²	0.312	0.154	0.228	0.287				
No. of Observations	280	585	668	740	280	585	668	740
	Reading Scores							
Attended private school	5.139 (2.115)	1.211 (1.548)	-0.854 (1.316)	0.352 (1.286)	7.822 (3.109)	2.284 (2.511)	-1.135 (2.275)	2.155 (2.354)
R ²	0.265	0.164	0.177	0.225				
No. of Observations	270	573	675	731	270	573	675	731

Notes: Standard errors are in parentheses. See notes to Table 5 for other regressors. The instrument is whether the student was randomly selected for the program.

the estimates of ρ from Table 8, Θ ranges from about 0.5 to 0.7. And, assuming that $\beta_0 = 2\psi$, the IV estimates likely overstate the full treatment effect by about 25%-35%.³⁷

In Table 10a, I estimate basic spline functions by OLS and IV while also assessing the influence of the 1990 cohort on the estimates; in Table 10b, I include individual fixed-effects. Focusing on the IV estimates in Table 10a, in the first specification which includes the entire sample, I estimate that those students who attended a private school had a lower (although statistically insignificant) initial math test score, but that they had significantly higher yearly gains than those not enrolled in a private school. The results in columns (2) and (3) which exclude the 1990 cohort and do not include the 1994 test scores for the 1990 cohort respectively, broadly support the trend derived from the full sample, although both suggest a slower yearly increase in test scores.³⁸ The results in Table 10b which include individual fixed-effects are quite similar.

The IV estimates for the reading test scores (in the lower panels) indicate that students enrolled in the private schools generally start with higher (but insignificant) scores than those not enrolled in the private schools, although there do not appear to be significantly greater yearly gains.³⁹

Taken together, the range of credible estimates of the causal effect of private schools on yearly increases in math scores using the applicant pool control group is about 2 percentage points per year (after adjusting for the upward IV bias), although there is no measurable effect on reading scores.

³⁷ The bias may actually be larger as it appears that in this program, $\psi = \beta_0$ which would imply a bias of about 100%. For evidence, see the OLS results in this table, and the results in Table 11 or Appendix Table 1a.

³⁸ When I use the Greene, et. al. (1996) sample restrictions, I also find there are significant yearly gains in the test scores, except when I exclude the 1990 cohort or exclude the 1994 test scores for the 1990 cohort. In addition, I do not estimate statistically significant jumps in the linear trend at either the 3rd or 4th years.

³⁹ I have also tried excluding the 1991 test scores for the 1990 cohort with similar results.

Table 10a
OLS and IV Estimates of the Effect of Private Schools on the Rate of Growth in Test Scores

	OLS			IV		
	(1)	(2)	(3)	(1)	(2)	(3)
Math Scores						
Selected to attend private school (α'_p)	-2.203 (1.153)	-4.565 (1.490)	-2.046 (1.204)	-1.313 (1.290)	-2.855 (1.638)	-0.277 (1.307)
Number of years since application (δ')	-0.659 (0.346)	-0.652 (0.438)	-0.814 (0.363)	-1.473 (0.521)	-0.662 (0.637)	-1.052 (0.521)
Number of years since application*Private School (β')	1.555 (0.621)	2.833 (0.899)	1.627 (0.671)	2.883 (0.975)	2.051 (1.302)	1.432 (1.010)
R ²	0.098	0.117	0.100			
Number of Observations	3283	2102	3096	3283	2102	3096
Reading Scores						
Selected to attend private school (α'_p)	0.013 (1.055)	-3.923 (1.403)	-0.086 (1.129)	1.693 (1.147)	-1.578 (1.501)	1.960 (1.210)
Number of years since application (δ')	-0.744 (0.253)	-0.190 (0.330)	-0.632 (0.281)	-0.810 (0.355)	-0.103 (0.432)	-0.629 (0.373)
Number of years since application*Private School (β')	0.317 (0.528)	1.688 (0.800)	0.253 (0.602)	-0.016 (0.878)	0.344 (1.200)	-0.535 (0.940)
R ²	0.066	0.068	0.063			
Number of Observations	3268	2086	3081	3268	2086	3081

Notes: Huber standard errors (that allow for individual correlation) are in parentheses. See note to Table 5b for other regressors. The instruments are whether randomly selected, and whether randomly selected interacted with years since application.

Column Descriptions:

- Column 1: Uses Full Sample
- Column 2: Only Uses 1991-1993 Cohorts
- Column 3: Excludes 1994 Test Scores for 1990 Cohort

Table 10b
OLS and IV Estimates of the Effect of Private Schools on the Rate of Growth in Test Scores
(Individual Fixed-Effects)

	OLS with FE			IV with FE		
	(1)	(2)	(3)	(1)	(2)	(3)
Math Scores						
Selected to attend private school (α'_p)	-2.724 (0.991)	-4.995 (1.345)	-2.828 (1.077)	-2.126 (1.149)	-3.776 (1.499)	-1.917 (1.219)
Number of years since application (δ')	-0.496 (0.277)	-0.503 (0.355)	-0.611 (0.303)	-1.204 (0.452)	-0.802 (0.482)	-0.865 (0.450)
Number of years since application*Private School (β')	1.167 (0.504)	2.439 (0.773)	1.473 (0.598)	2.644 (0.920)	2.737 (1.114)	1.776 (0.950)
R ²	0.007	0.015	0.007			
Number of Observations	3283	2102	3096	3283	2102	3096
Reading Scores						
Selected to attend private school (α'_p)	0.379 (0.937)	-2.580 (1.292)	0.372 (1.029)	1.335 (1.058)	-2.110 (1.420)	0.869 (1.145)
Number of years since application (δ')	-0.431 (0.220)	-0.135 (0.286)	-0.302 (0.247)	-0.364 (0.314)	-0.201 (0.361)	-0.416 (0.333)
Number of years since application*Private School (β')	-0.565 (0.461)	0.455 (0.736)	-0.766 (0.561)	-1.005 (0.803)	0.492 (1.055)	-0.591 (0.867)
R ²	0.006	0.005	0.005			
Number of Observations	3268	2086	3081	3268	2086	3081

Notes: Standard errors are in parentheses. All specifications include individual fixed-effects, and for the math scores, an indicator if the total test score was imputed. The instruments are whether randomly selected, and whether randomly selected interacted with years since application.

Column Descriptions:

- Column 1: Uses Full Sample
- Column 2: Only Uses 1991-1993 Cohorts
- Column 3: Excludes 1994 Test Scores for 1990 Cohort

C. *Estimating the Effect of Private Schools Using the Milwaukee Public School Comparison Group*

While these results are partially consistent with those estimated by Greene, et. al. (1996), the math score results conflict with those of Witte, Sterr, and Thorn (1995). Given that the primary difference between the studies is in the choice of control or comparison group, in this section I estimate a basic spline function using the random sample of students from the Milwaukee public schools. One reason why treatment effects may not emerge when using the sample of students from the Milwaukee public schools is that they may not be a representative sample of students eligible for the choice program. They may be too advantaged. As noted in Table 1, the mean family income of the students in the Milwaukee public schools is roughly twice that of students who applied to the choice program. However, if this is the case, even if mean differences in the choice students and the Milwaukee public school students are not statistically different from one another, the choice students may have experienced a faster growth in test scores. Further, including individual fixed-effects would control for any time-invariant unobserved differences between the choice students and the comparison sample.

Using this comparison sample also has some advantages over the applicant pool control group. First, many of the rejected applicants may have left the Milwaukee public schools to attend other private schools. For example, in Table 1 only 28% of non-selected applicants have test scores two years after application compared to 54% of the choice students who were admitted. This could result in a non-representative control group and is particularly a problem for the later years when the sample sizes for the control group are quite small. Second, I could not truly identify the schools to which students applied, and was therefore not completely confident of the application lotteries; the analysis using the Milwaukee public school students does not use such information. Third, I can include the entire population of choice students, not just African-Americans and Hispanics. Finally, the estimated effects of private school are relative to the entire population of public school students, and not just a group of public school students interested in attending private school.

For this analysis, I continue to use my sample of applicants to the choice program. In order to incorporate the students in the Milwaukee public school sample, I need to create an artificial "year of application" for them in order to have a year of reference from which to measure the yearly changes in scores. I do so by considering 1990 the "year of application" for students in the Milwaukee public schools who have a valid 1991 test score. For those without a 1991 test score, I consider their "year of application" to be 1991 if they have a valid 1992 test score, and so forth. This allows me to compare the yearly test score progress of consistent cohorts. Any tests taken before this "year of application" are also included and they allow me to identify the mean effect of attending a choice school while controlling for individual fixed-effects.⁴⁰ Finally, for the OLS estimates I continue to control for family income, and also mother's and father's education, and the race of the student.⁴¹ I allow a different intercept for students enrolled in a choice school, those in the choice applicant control group (i.e., those who were not selected and were never enrolled in a choice school), and those who were either selected for a choice school and did not enroll or who were enrolled at one time but had returned to the Milwaukee public schools (i.e., they received partial treatment). I also allow these groups to have different slope estimates. These categories are mutually exclusive in any test year.

Results using OLS (with Huber standard errors) and individual fixed-effects are in Table 11. I present results using only a sample of African-Americans and Hispanics (for comparison with earlier results), and using all students (not missing sex or race). In columns (1)-(4), I find that the students who were enrolled in a choice school had lower math test score intercepts than students in

⁴⁰ Witte, Sterr, and Thorn (1995) control for the previous year's test score which restricts the yearly growth to be constant each year. I get results more similar to his when I control for the previous year's test score.

⁴¹ I cannot control for the applicant pool fixed effects when using the Milwaukee public school sample. The point estimates are almost identical with and without them for the sample using only the applicant pool (without individual fixed-effects). In addition, I do not include the grade level of the test of the student for comparison with earlier tables. This has very little effect on the results.

Table 11
OLS and Individual Fixed-Effects Estimates of the Effect of Private Schools on the Rate of Growth in Test Scores
Using the Milwaukee Public School Sample as a Comparison Group

	Math Scores				Reading Scores			
	Minorities		All Students		Minorities		All Students	
	OLS (1)	FE (2)	OLS (3)	FE (4)	OLS (5)	FE (6)	OLS (7)	FE (8)
Enrolled in Private School	-2.458 (1.150)	-1.935 (1.039)	-2.854 (1.122)	-2.142 (0.999)	-1.029 (1.006)	0.802 (0.962)	-1.468 (0.991)	0.428 (0.935)
Choice Applicant Control Group	-1.779 (1.101)	-3.997 (5.807)	-2.016 (1.097)	-4.351 (5.676)	-0.114 (0.905)	-0.397 (5.364)	-0.761 (0.898)	-0.708 (5.304)
Selected for Choice, Not in a Private School	-0.120 (2.346)	0.636 (2.070)	-0.105 (2.340)	0.659 (2.007)	3.743 (2.147)	3.461 (1.904)	3.483 (2.140)	3.514 (1.869)
Number of years	-0.290 (0.204)	-1.142 (0.157)	-0.263 (0.177)	-0.955 (0.131)	-0.185 (0.166)	-0.760 (0.126)	-0.217 (0.144)	-0.487 (0.107)
Number of years*Private School	0.778 (0.569)	1.887 (0.454)	0.666 (0.544)	1.595 (0.429)	-0.347 (0.472)	-0.339 (0.419)	-0.312 (0.457)	-0.496 (0.402)
Number of years*Choice Control Group	-0.733 (0.473)	-0.014 (0.401)	-0.662 (0.462)	-0.181 (0.377)	-0.898 (0.423)	0.060 (0.365)	-0.838 (0.410)	-0.014 (0.347)
Number of years*Selected for Choice, Not in Private School	-0.043 (0.889)	1.319 (0.743)	-0.027 (0.877)	1.076 (0.717)	-1.819 (0.806)	-0.728 (0.680)	-1.805 (0.796)	-1.057 (0.665)
R ²	0.023	0.013	0.113	0.011	0.032	0.010	0.117	0.005
Number of Observations	8729	8729	11083	11083	8751	8751	11121	11121

Notes: Standard errors are in parentheses. The OLS standard errors allow for correlation "within" an individual. All regressions include a constant, sex, family income, race, mother's and father's education, and indicators if income and parental education are missing; for the math scores, a dummy indicating if the test score was imputed. The fixed-effects estimates include individual fixed-effects, and for the math scores, a dummy indicating that the total score was imputed. The "minorities" sample includes only African-Americans and Hispanics; "all students" includes other students, as well.

the Milwaukee public school comparison group, a difference that is statistically significant. The OLS estimates in columns (1) and (3) also indicate that students who attended a private school experienced greater math test score gains than the Milwaukee public school comparison group, although the increase is not statistically significant and less than one percentage point per year.

On the other hand, the fixed-effects models (in columns (2) and (4)) suggest that the math scores of students in private schools increased an additional 1.6-1.9 percentage points per year, and the effect is significant at the 5 percent level for both samples of students.⁴² At the same time, the math test scores of those who received partial treatment increased an additional 1.3 percentage points per year above other students in the Milwaukee public schools although the difference is not statistically significant (the p-value of the difference between those who received full and partial treatment is 0.079 in column (2) and 0.13 in column (4)).

The results for reading, in columns (5)-(7), suggest that students in the choice schools score lower than students in the Milwaukee public schools by about -0.3 percentage points per year, although this difference is not statistically significant. On the other hand, the differences between the applicant control group, those who received only partial treatment, and the Milwaukee public school comparison sample are statistically indistinguishable from zero. Including individual fixed-effects does not make a large difference to the estimated treatment effect of private school attendance, although it does decrease (or eliminate) the negative effect of partial treatment.

V. Conclusion

The results using the quasi-experimental applicant control group and the random sample of students from the Milwaukee public schools (when I include individual fixed-effects) are remarkably similar. On the one hand, I find that the Milwaukee Parental Choice Program and the participating

⁴² These results appear robust to alternative methods of dealing with the imputation of the total math score and sample attrition.

private schools likely increased student math scores by 1.5-2 percentage points per year. On the other hand, the results for reading scores were quite mixed with both positive and negative coefficient estimates. While it is not clear how to weigh the math results versus the reading results, when I total the math and reading scores, I estimate that private school students gained approximately 1.3 percentage points per year and the effect has a p-value of 0.063.⁴³

Although these results obtain using a variety of estimation strategies and samples, there are at least two caveats to keep in mind. First, I had to impute the total math score for a significant fraction of the Milwaukee public school students. Second, and most importantly, there is substantial sample attrition in the later years. While my analysis suggests that the results are robust to the imputation and sample attrition, my strategies cannot substitute for better data.

The data collection from Milwaukee should be applauded as it allows us to learn more about the effectiveness of this program than from many other reforms. Nevertheless, if one lesson emerges from this effort, it is that future evaluations of reforms of this sort should anticipate high mobility among the students and recognize that administrative data are determined by the needs of the schools and not those of the evaluator. An evaluation design that treats the participants (and control or comparison group) as a survey sample with independent follow-up, though more costly, would avoid some of the data problems experienced here.

Finally, although the experience in Milwaukee suggests that providing vouchers to low income students to attend private schools could help increase the mathematical achievement of those students who participate, it cannot shed light on whether vouchers provide an incentive for the public schools to improve and therefore increase the quality of education provided to all low income children. In addition, the results from one program implemented in one city cannot, and should not, be the only evidence on which important policy regarding the structure of American education is

⁴³ Based on the full sample with all students including individual fixed effects (i.e., such as columns (4) and (8) in Table 11).

based. It is only by piecing together evidence from many places that we will ever really learn whether private school vouchers could increase student achievement.

References

- Akin, John S., and Irwin Garfinkel. "The Quality of Education and Cohort Variation in Black-White Earnings Differentials: Comment," American Economic Review 70 (March, 1980): 186-191.
- Angrist, Joshua D. "The Draft Lottery and Voluntary Enlistment in the Vietnam Era," Journal of the American Statistical Association, 86, no. 415 (September, 1991): 584-595.
- Angrist, J.D., G.W. Imbens, and D.B. Rubin. "Identification of Causal Effects Using Instrumental Variables," Journal of the American Statistical Association, forthcoming.
- Coleman, James, Thomas Hoffer, and Sally Kilgore. High School Achievement: Public, Catholic and Private Schools Compared (New York: Basic Books, 1982a).
- Coleman, James, Thomas Hoffer, and Sally Kilgore. "Cognitive Outcomes in Public and Private Schools," Sociology of Education, 55 no. 2/3 (April/July, 1982b): 65-76.
- Cookson, Peter W. "Assessing Private School Effects: Implications for School Choice" in School Choice: Examining the Evidence, Edith Rasell and Richard Rothstein, editors (Economic Policy Institute: Washington, D.C., 1993).
- Coronary Drug Project Research Group. "Influence of Adherence to Treatment and Response to Cholesterol on Mortality in the Coronary Drug Project," New England Journal of Medicine, 33 (1980): 1038-1041.
- Behrman, Jere R., and Nancy Birdsall. "The Quality of Schooling: Quantity Alone is Misleading," American Economic Review 73 (December, 1983): 928-946.
- Durbin, J. "Errors in Variables," Review of International Statistical Institute, 22 (1954): 23-32.
- Efron, B., and D. Feldman. "Compliance as an Explanatory Variable in Clinical Trials," Journal of the American Statistical Association 86, no. 413 (March 1991): 9-17.
- Evans, William N., and Robert M. Schwab. "Finishing High School and Starting College: Do Catholic Schools Make a Difference?," Quarterly Journal of Economics 110, no. 4 (November, 1995): 941-974.
- Goldberger, Arthur S. and Glen G. Cain. "The Causal Analysis of Cognitive Outcomes in the Coleman, Hoffer and Kilgore Report," Sociology of Education 55, vol. no. 2/3 (April/July, 1982): 103-122.
- Greene, Jay P., Paul E. Peterson, Jiangtao Du, Leesa Boeger, and Curtis L. Frazier. "The Effectiveness of School Choice in Milwaukee: A Secondary Analysis of Data from the Program's Evaluation." University of Houston mimeo, August 1996.
- Greenwald, Rob, Larry V. Hedges, and Richard D. Laine, "The Effect of School Resources on Student Achievement," Review of Educational Research, 66 (in press).

- Gruber, Jonathan. "Cash Welfare as a Consumption Smoothing Mechanism for Single Mothers." National Bureau of Economic Research Working Paper Number 5738 (September 1996).
- Hanushek, Eric A. "Conceptual and Empirical Issues in the Estimation of Educational Production Functions," Journal of Human Resources, 14 (Summer, 1979): 351-388.
- Hausman, Jerry A. and David A. Wise. "Technical Problems in Social Experimentation: Cost versus Ease of Analysis," in Social Experimentation, Jerry A. Hausman and David A. Wise, editors, (The University of Chicago Press: Chicago, 1985).
- Heckman, James J. "Instrumental Variables: A Cautionary Tale," National Bureau of Economic Research Technical Working Paper 185 (September 1995a).
- Heckman, James J. "Randomization as an Instrumental Variable," National Bureau of Economic Research Technical Working Paper 184 (September 1995b).
- Heckman, James, and Jeffrey Smith. "Assessing the Case for Randomized Evaluation of Social Programs," in Measuring Labour Market Measures: Evaluating the Effects of Active Labour Market Policies, Karsten Jensen and Per Kongshoj, editors (Copenhagen: Ministry of Labour, 1993).
- Heckman, James, and Jeffrey Smith. "Substitution Bias in Social Experiments: An Analysis of JTPA Data," University of Chicago mimeo, 1994.
- Link Charles R., Edward C. Ratledge, and Kenneth A. Lewis. "The Quality of Education and Cohort Variation in Black-White Earnings Differentials: Reply," American Economic Review 70 (March, 1980): 196-203.
- Manski, Charles F. "Identification of Binary Response Models," Journal of the American Statistical Association, 83, no. 403, (September, 1988): 729-738.
- Manski, Charles F. "The Selection Problem," in Advances in Econometrics, C. Sims, editor, (New York: Cambridge University Press, 1992).
- Manski, Charles F. Identification Problems in the Social Sciences (Cambridge, MA: Harvard University Press, 1995).
- Murnane, Richard J. "A Review Essay – Comparisons of Public and Private Schools: Lessons from the Upstar," Journal of Human Resources, 19 (1984): 263-277.
- Park, Jin Heum. "Estimation of Sheepskin Effects and Returns to Schooling Using the Old and the New CPS Measures of Educational Attainment." Industrial Relations Section Working Paper #338, December 1994.
- Rubin, D. "Estimating Causal Effects of Treatments in Randomized and Non-randomized Studies," Journal of Educational Psychology, 66 (1974): 688-701.
- Thorn, Christopher A., John F. Witte, and Troy D. Sterr. "Documentation of Data Used in

The Milwaukee Choice Study". La Follette Institute of Public Affairs mimeo, August 1995.

Wald, Abraham. "The Fitting of Straight Lines if Both Variables Are Subject to Error," Annals of Mathematical Statistics, 11 (1940): 284-300.

Witte, John F. "Private School vs. Public School Achievement: Are There Findings That Should Affect the Educational Choice Debate?," Economics of Education Review, 11 (December, 1992): 371-394.

Witte, John F. "Reply to Greene, Peterson, and Du: 'The Effectiveness of School Choice in Milwaukee: A Secondary Analysis of Data from the Program's Evaluation'", University of Madison mimeo, August 1996.

Witte, John F. and Christopher A. Thorn. "The Milwaukee Parental Choice Program, 1990/1991-1994/1995 [computer file], Madison, WI, 1995.

Witte, John F., Christopher A. Thorn, and Kim A. Pritchard. "Private and Public Education in Wisconsin: Implications for the Choice Debate," University of Wisconsin mimeo, (1995).

Witte, John F., Christopher A. Thorn, Kim M. Pritchard, and Michele Claibourn. "Fourth-Year Report: Milwaukee Parental Choice Program." University of Wisconsin mimeo, December 1994.

Witte, John F., Troy D. Sterr, and Christopher A. Thorn. "Fifth-Year Report: Milwaukee Parental Choice Program." University of Wisconsin mimeo, December 1995.

Data Appendix

The data on the Milwaukee Parental Choice Program can be downloaded from http://dpls.dacc.wisc.edu/choice/choice_index.html. I construct the analysis sample according to the following definitions which are similar to those used by Greene, et. al. (1996):

Year of Application

I define the year of application as the first year in which a student applied if the student was either never selected or was selected the first time they applied. If the student applied more than once, I consider the first year they are accepted as the year in which they applied. In addition, in a few cases, the student applied, but was not selected but was nonetheless enrolled in a choice school the following spring (i.e., they were admitted off of a waiting list). I consider their year of the application the year in which they applied and were not accepted, as once a student was enrolled in a choice school they did not participate in the randomization the following years. I only include those who first applied in the years 1990-1993.

For students in the Milwaukee public school sample I consider 1990 the "year of application" for students in the Milwaukee public schools who have a valid 1991 test score. For those without a 1991 test score, I consider their "year of application" to be 1991 if they have a valid 1992 test score, and so forth.

Grade at Application

I only use the grade levels of the Iowa Tests of Basic Skills in determining the grade to which the student applied. As the test is administered in the spring semester, I consider the grade level of the test of the spring following the year of application as the grade to which the student applied. To impute the grade of application (in cases in which the student is missing a test score for the first spring following the year of application), I search backwards and forwards in the test data for a non-missing test grade and add or subtract the appropriate number of years. For example, suppose a student applied in 1990, is missing a 1991 test score, but has a test score for the spring of 1992. I consider the grade at application to be the 1992 grade minus 1. (An alternative method for imputing would be to use the grade of the student from the administrative data (the mastch.dat and chsrdb.dat files). The exact order in which one searches for a valid grade using the test score data and using the alternative method of imputation makes a small difference to the point estimates. (See footnote 18). I only include students who applied to grades K-8 in the analysis.

Race and Sex

I first determine the race and sex of the student from the master choice file (mastch.dat) files. Further, I require that the race and sex of the student be consistent in all years. (Thus, if the student appears to "change" sex or race across years, I consider the race or sex missing.) If the race or sex is missing from these files, I use the race and sex from the choice Student Record Database (SRDB) (chsrdb.dat) file. Again, I require that the race and sex not change over the data set. I only include African-Americans and Hispanics in the analysis.

Test Scores

I use the normal curve equivalent transformation of the math and reading scores. For both, I recompute them based on the transformation in Thorn, Witte, and Sterr (1995) as several of the test scores appear to have been mis-transformed. I only use current test scores. I impute math scores for students in the Milwaukee public schools who do not have a total score, but do have a score for the problem solving component. I impute using the following equations,

$$\begin{aligned} mn\hat{c}e93 &= 16.293 + 0.250mnpr93 + 0.023mnpr93^2 - 0.00029mnpr93^3 \\ &\quad + 0.0000013mnpr93^4 - 0.079tsyear93 \\ R^2 &= 0.810 \quad N = 3603 \end{aligned}$$

$$\begin{aligned} mn\hat{c}e94 &= 9.778 + 0.265mnpr94 + 0.023mnpr94^2 - 0.00031mnpr94^3 \\ &\quad + 0.0000014mnpr94^4 - 0.017tsyear94 \\ R^2 &= 0.814 \quad N = 3182 \end{aligned}$$

where mnpr9X is the total math score, mnpr9X is the problem solving component, and tsyear9X is the year the test was administered. I estimate these equations using the random sample of students in the Milwaukee public schools (and I include students with test scores from previous years).

The pre-application test score is the test score from the year the student applied. I substitute the mean score for those missing pre-application test scores.

Family Income and Parental Education

Family income is from the survey administered to choice applicants in the fall and spring of each year, as well as to a random sample of the families in the Milwaukee public schools. I average the reported income from the fall and spring of the year after application (to lower the measurement error). If a student applied more than once (and was not immediately selected) and is missing information on family income, I fill in the information with the reported income in earlier years. I convert the measure to 1994 dollars and substitute the average for those missing income.

I construct parental education in a similar fashion. I convert the categorical variable to a continuous education measure according to the mapping suggested by Park (1994): 8th grade or below=8, some high school=10, GED and high school graduate=12, some college=13, 4-year degree=16, and post-graduate work=17.

Choice Students

I consider an individual a choice student who attended private school if they took an achievement test in a choice school. In the analysis, I exclude a few students who only had valid test scores from a Milwaukee public school but who were indicated as enrolled in a choice school in the same spring. There are also 6 students who appear to be enrolled in a choice school in a particular spring and also

appear to apply for the first time in the same spring; and there are 2 students who apply in 1990 or 1991, only appear to be enrolled in a choice school in 1993, and who have test scores from both the choice school and a Milwaukee public school in 1993. I include these students, although the results are quite similar when I exclude them.

Appendix Table 1a

A Comparison with Greene, Peterson, et. al.'s Math Score Results

	Math Scores After 1 Year			Math Scores After 2 Years		
	(1)	(2)	(3)	(1)	(2)	(3)
Attended Private School	-0.372 (1.791)	-0.808 (1.743)	-0.766 (1.720)	-0.082 (1.860)	1.205 (1.817)	1.446 (1.811)
Female	0.976 (1.407)	0.863 (1.376)	0.707 (1.295)	0.026 (1.538)	-0.565 (1.541)	-0.133 (1.389)
Selected, but Did Not Enroll in Program			3.609 (3.734)			6.095 (3.784)
Left Private School			3.777 (3.114)			-0.120 (2.359)
R ²	0.155	0.156	0.154	0.098	0.093	0.087
No. of Observations	772	804	887	584	608	752
	Math Scores After 3 Years			Math Scores After 4 Years		
	(1)	(2)	(3)	(1)	(2)	(3)
Attended Private School	4.086 (2.535)	4.515 (2.409)	3.934 (2.479)	9.519 (4.653)	10.421 (4.070)	10.277 (3.866)
Female	0.206 (1.919)	0.398 (1.898)	0.207 (1.665)	1.458 (3.957)	1.894 (3.622)	2.391 (2.810)
Selected, but Did Not Enroll in Program			5.462 (4.767)			17.780 (13.570)
Left Private School			1.544 (2.667)			11.274 (4.031)
R ²	0.217	0.201	0.158	0.130	0.172	0.150
No. of Observations	300	313	447	112	123	187

Notes: Standard errors are in parentheses. All specifications include "application pool" fixed effects. "Attended Private School", "Selected, but Did Not Enroll in Program", and "Left Private School" are mutually exclusive dummy variables. "Year" refers to the year of application.

Column Specifications:

Column (1): Replication of Greene, et. al.'s results (i.e., Greene, et. al.'s sample construction and sample selection rules)

Column (2): Greene, et. al.'s results using Rouse's sample (i.e., Rouse sample construction and Greene et. al.'s selection rules)

Column (3): Add "non-compliers", and those who left the Private Schools (and returned to Milwaukee Public Schools) back into the Rouse sample (column (2))

Appendix Table 1b

A Comparison with Greene, Peterson, et. al.'s Reading Score Results

	Reading Scores After 1 Year			Reading Scores After 2 Years		
	(1)	(2)	(3)	(1)	(2)	(3)
Attended Private School	1.212 (1.568)	0.534 (1.547)	0.074 (1.546)	0.875 (1.663)	1.023 (1.593)	1.074 (1.606)
Female	2.287 (1.274)	2.265 (1.258)	2.088 (1.194)	3.069 (1.376)	2.766 (1.355)	2.840 (1.237)
Selected, but Did Not Enroll in Program			-0.615 (3.327)			4.635 (3.537)
Left Private School			4.212 (2.819)			-0.167 (2.075)
R ²	0.121	0.115	0.101	0.080	0.085	0.060
No. of Observations	734	763	846	594	617	763
	Reading Scores After 3 Years			Reading Scores After 4 Years		
	(1)	(2)	(3)	(1)	(2)	(3)
Attended Private School	3.445 (2.344)	3.359 (2.213)	2.945 (2.247)	4.603 (3.730)	6.525 (3.388)	6.128 (3.285)
Female	5.485 (1.789)	5.271 (1.760)	4.421 (1.515)	6.847 (3.234)	6.190 (3.056)	5.726 (2.395)
Selected, but Did Not Enroll in Program			1.245 (4.194)			-26.879 (16.015)
Left Private School			0.594 (2.416)			5.648 (3.406)
R ²	0.146	0.148	0.099	0.203	0.228	0.194
No. of Observations	301	314	453	112	123	187

Notes: Standard errors are in parentheses. All specifications include "application pool" fixed effects. "Attended Private School", "Selected, but Did Not Enroll in Program", and "Left Private School" are mutually exclusive dummy variables. "Year" refers to the year of application.

Column Specifications:

- Column (1): Replication of Greene, et. al.'s results (i.e., Greene, et. al.'s sample construction and sample selection rules)
- Column (2): Greene, et. al.'s results using Rouse's sample (i.e., Rouse sample construction and Greene et. al.'s selection rules)
- Column (3): Add "non-compliers", and those who left the Private Schools (and returned to Milwaukee Public Schools) back into the Rouse sample (column (2))

Appendix Table 2
Means and Standard Deviations

	Year of Test Score			
	1991		1992	
	Not Selected	Selected	Not Selected	Selected
Enrolled in Private School	0.016 [0.127]	0.844 [0.364]	0.000 [0.000]	0.789 [0.408]
Math Score	37.661 [20.436]	40.326 [18.806]	37.713 [18.260]	37.194 [18.751]
Reading Score	37.678 [16.988]	42.213 [16.405]	36.442 [17.862]	37.817 [16.440]
Female	0.565 [0.500]	0.523 [0.501]	0.496 [0.502]	0.547 [0.498]
Family Income	12.436 [6.649]	12.786 [6.560]	12.331 [5.973]	12.323 [5.969]
Family Income Missing	0.565 [0.500]	0.394 [0.490]	0.626 [0.486]	0.409 [0.492]
Pre-application Math Test Score	38.532 [14.986]	39.029 [13.536]	37.534 [10.619]	38.036 [11.403]
Pre-application Math Score Missing	0.484 [0.504]	0.514 [0.501]	0.539 [0.501]	0.611 [0.488]
Pre-application Reading Test Score	39.111 [10.227]	38.996 [10.155]	36.997 [9.321]	37.651 [9.700]
Pre-application Reading Score Missing	0.500 [0.504]	0.500 [0.501]	0.548 [0.500]	0.617 [0.487]
Black	0.661 [0.477]	0.863 [0.345]	0.765 [0.426]	0.796 [0.403]
Hispanic	0.290 [0.458]	0.110 [0.314]	0.191 [0.395]	0.181 [0.385]
Grade of Application	3.935 [2.374]	3.670 [2.290]	3.409 [2.438]	3.130 [2.256]
Applied in 1990	1.000 [0.000]	1.000 [0.000]	0.600 [0.492]	0.498 [0.501]
Applied in 1991	NA	NA	0.400 [0.492]	0.502 [0.501]
Number of Observations	62	218	115	470

Appendix Table 2 (continued): Means and Standard Deviations

	Year of Test Score			
	1993		1994	
	Not Selected	Selected	Not Selected	Selected
Enrolled in Private School	0.033 [0.179]	0.742 [0.438]	0.007 [0.085]	0.734 [0.442]
Math Score	36.255 [17.814]	39.725 [17.494]	39.785 [19.401]	41.582 [18.976]
Reading Score	36.279 [17.534]	36.242 [15.177]	36.969 [18.545]	37.845 [15.742]
Female	0.434 [0.497]	0.523 [0.500]	0.485 [0.502]	0.543 [0.499]
Family Income	12.093 [6.051]	12.269 [6.049]	12.139 [5.486]	12.428 [6.318]
Family Income Missing	0.533 [0.501]	0.389 [0.488]	0.536 [0.501]	0.385 [0.487]
Pre-application Math Test Score	37.122 [12.128]	38.789 [11.583]	40.859 [15.308]	38.859 [10.691]
Pre-application Math Score Missing	0.513 [0.501]	0.612 [0.488]	0.449 [0.499]	0.659 [0.474]
Pre-application Reading Test Score	36.089 [10.956]	37.715 [9.787]	38.506 [11.820]	37.888 [9.609]
Pre-application Reading Score Missing	0.493 [0.502]	0.607 [0.489]	0.449 [0.499]	0.658 [0.475]
Black	0.783 [0.414]	0.769 [0.422]	0.797 [0.404]	0.754 [0.431]
Hispanic	0.171 [0.378]	0.205 [0.404]	0.145 [0.353]	0.219 [0.414]
Grade of Application	3.579 [2.292]	2.791 [2.090]	3.239 [1.928]	2.531 [2.148]
Applied in 1990	0.335 [0.474]	0.364 [0.482]	0.275 [0.448]	0.247 [0.432]
Applied in 1991	0.250 [0.434]	0.430 [0.495]	0.152 [0.360]	0.311 [0.463]
Applied in 1992	0.414 [0.494]	0.205 [0.404]	0.348 [0.478]	0.234 [0.424]
Applied in 1993	NA	NA	0.225 [0.419]	0.208 [0.406]
Imputed Test Score	0.395 [0.490]	0.110 [0.314]	0.681 [0.468]	0.179 [0.384]
Number of Observations	152	516	138	602

Note: Standard deviations are in brackets. Based on the sample for math scores.

Appendix Table 3

**The Effect of Selection to the Choice Program on Math Scores By Years Since Selection
The Effect of Alternative Imputation Strategies on the Results in Table 4a**

	1993 Test Score			
	Impute Missing, No Dummy	Impute Missing	Impute All Controls	Exclude Imputed
	(1)	(2)	(3)	(4)
	1993 Test Score			
Number of years since selection	0.930 (0.761)	1.116 (0.798)	0.681 (0.780)	-0.015 (0.985)
R ²	0.227	0.228	0.197	0.198
Number of Observations	668	668	668	551
	1994 Test score			
Number of years since selection	1.492 (0.639)	1.685 (0.689)	1.228 (0.680)	1.010 (1.120)
R ²	0.293	0.293	0.279	0.283
Number of Observations	740	740	740	538

Notes: Standard errors are in parentheses. See notes to Table 5 for other regressors. Columns (2) and (3) include dummy variables indicating if the test score was imputed.

NATIONAL

1. Associated Press

05-02 2:07a For Release 11:15 a.m. EDT

Students have trouble with scientific thinking, test shows

By ROBERT GREENE
AP Education Writer

WASHINGTON (AP) - Students from high school on down know a few things about science but have trouble solving problems that force them to think through and explain answers, according to results of a national test released today.

The full meaning of the novel and expensive test on a sample of fourth-, eighth- and 12th-graders will not be known until later this year - after officials of the National Assessment Governing Board decide how much the youngsters should have been expected to know and when.

States that are trying to come up with academic standards are struggling with the same kind of issues.

The board, created by Congress, tests the progress of American students in major subjects. The Education Department pays for the tests and publishes results without giving individual scores.

Results are broken out by state, race, sex, poverty level, for public and private schools, and other categories. The tests are also meant to tell what share of students nationwide has basic knowledge, and what share is proficient or advanced.

This time, the science test took a major leap from past versions by requiring hands-on experiments and more written explanation of answers. Twenty percent of questions were multiple choice, about one-fourth their usual share, and the test took 90 minutes instead of an hour.

Materials for the hands-on problems accounted for about \$1.25 million of the

\$11.3 million cost of the test, given to 123,000 children.

The changes mean that before the board can fully interpret results, it must settle such matters as whether all 12th graders should know the advantages of eating spaghetti before a foot race, and whether only advanced ones can be expected to know Newton's second law of motion: that a heavier object will change its motion less under a given applied force than will an object of lighter weight.

"The board has received quite a range of opinions" from review panels of teachers, scientists and others, said Michael J. Guerra, a board member and officer of the National Catholic Education Association.

The testers promise a full science report card later this year.

General results released today show that youngsters in the Northeast and Central states outperformed those in the West and Southeast. Whites outperformed Blacks and Hispanics. The poor did poorly.

Boys slightly outperformed girls in the 12th grade, but not in the earlier grades. A recent international test has found a similar absence of gender gap in lower grades.

But a bothersome finding was most students' inability to show they understand science well enough to apply knowledge in new situations or explain how they arrived at answers.

"The results are disappointing" in those realms, said Michael T. Nettles, a member of the assessment board and professor at the University of Michigan.

According to today's results, 75

percent of fourth graders could explain in writing why other stars look smaller than the Sun even though they are much bigger.

Only 12 percent, however, explained that an experiment to find out whether beetles prefer light or shade required placing a water dish both in the lighted and the shaded parts of a box instead of just one side.

Ten percent of eighth graders could explain both how people may have gotten food poisoning at a summer picnic from eating potato salad with mayonnaise and how to prevent food poisoning.

Just 27 percent of 12th graders could fully answer a two-pronged question about why there is oil beneath Alaska's North Slope.

In one hands-on problem, fourth and eighth graders tried to float a pencil in salt water, fresh water and "mystery" water to determine if the mystery water had salt.

Eighth graders had to use a graph to figure out the percentage of salt in the mystery water, knowing the salt water was 25 percent saline.

The 12th graders did an experiment using a sieve, magnet, filter paper and water to separate a mixture of steel pellets, copper pellets, iron filings, sand and salt.

Because of the test's new approach, there's no way to tell whether kids are making progress in science until it is repeated in the year 2000. The last science-only test was given in 1990, and had also undergone changes that made its results impossible compare with previous tests. ■

2. Cincinnati Enquirer

May 2, 1997

Ohio court finds voucher illegal **2,000 Cleveland kids to lose subsidies**

BY MARK SKERTIC and SANDY THEIS The Cincinnati Enquirer

COLUMBUS - The nation's first attempt to improve students' achievement by paying their tuition at church-affiliated schools has been struck down by an Ohio appeals court.

The state's experiment with school vouchers is an unconstitutional support of religion because it "provides direct and substantial non-neutral government aid to sectarian schools," the Ohio 10th District Court of Appeals in Columbus unanimously ruled Thursday.

The decision will affect 2,000 Cleveland kindergarten through third-graders who this year attended one of 57 private schools because the state paid 90 percent of their tuition, up to \$2,500. The \$5.2 million program was scheduled to grow by another 1,000 children in the fall.

Gov. George Voinovich, a former Cleveland mayor and enthusiastic supporter of the voucher effort, promised a swift appeal to the Ohio Supreme Court and a fight to keep voucher money in the next state budget.

The voucher law, just two years old and limited to Cleveland, has already been the subject of magazine articles, speeches and policy battles. Public education advocates and those who support school choice have closely watched a battle that many acknowledge will eventually be decided by the U.S. Supreme Court.

The three appeals court judges rejected all the state's arguments in support of the program, ruling that the vouchers clearly sent taxpayer dollars to church-affiliated enterprises.

"The only real choice available to most parents is between sending their child to a sectarian school and having their child remain in the troubled Cleveland City Schools District," Judge John C. Young wrote. "Such a choice can hardly be characterized as 'genuine and independent.'"

The judges also found the voucher effort was unconstitutional because it was a law enacted specifically for one part of the state, a violation of the constitutional provision that requires uniform application of laws.

In addition, the judges rejected a part of the program that provided \$500 for tutors for students who remained in the Cleveland public schools. The disparity between the amount set aside for tutors and what was available for scholarships created an incentive to send kids to a religious school, the judges said.

Supporters of the program said it

offered a choice to parents dissatisfied with the Cleveland Public Schools, a system that in the past two years has faced bankruptcy, a state takeover and the threat of a teacher strike.

Opponents celebrate

Voucher opponents were quick to celebrate. The National Education Association (NEA) and the American Federation of Teachers (AFT), the nation's two largest teachers unions, released statements hailing the decision.

"In the last month, the courts of Ohio have delivered a one-two punch to the enemies of free, universal public education," said Tom Mooney, president of the Cincinnati Federation of Teachers and an AFT vice president. "Thank the Founding Fathers for the balance of powers. This decision is very consistent with the recent Ohio Supreme Court decision on public education."

On March 24, the Ohio Supreme Court ruled the state's system for funding schools was unconstitutional and gave the state a year to fix it.

Proponents of the voucher program were disappointed.

"Essentially, we bought these kids a year of quality education, but that's not enough," said John Kramer, spokesman for the Washington, D.C.-based Institute for Justice. It represented some of the parents and children in the voucher program when it was argued to the court of appeals.

Hope Central Academy in Cleveland was one of the non-sectarian schools that opened last fall to accept voucher students. The 282 children enrolled there have shown dramatic improvement in less than a year, said Principal John Morris.

Teachers were disappointed when he met with them in the afternoon and delivered news of the court ruling. "The next move will have to come from our supporters," he said. "I think the muscle will be amassed in favor of scholarships (vouchers)."

On Thursday afternoon, calls also began coming in to the Cleveland Scholarship and Tutoring Program office, the state office that runs the voucher program, said program administrator Bert Holt.

"We told them we're hoping to do business as usual, just like last year when we found out two weeks before school was going to start that we could have the program," she said.

When the voucher law was adopted as part of the state budget in 1995, Ohio

became the third state to experiment with giving families tax dollars to spend on private school tuition. Wisconsin has provided vouchers in Milwaukee since 1990. In Vermont, rural communities that can't afford or don't have enough students for a school will pay for their students to attend private schools.

But Ohio's program made history and attracted national attention because, unlike Milwaukee's or Vermont's, it allows the money to be spent at religious schools.

In July, Judge Lisa Sadler of Franklin County Common Pleas Court upheld the constitutionality of the program. That was followed by a period of uncertainty, as voucher opponents failed to delay the program until their appeal was heard.

Now, with a month of school left, plaintiffs will not insist the voucher program end immediately, said Cincinnati attorney Don Mooney, who filed the case on behalf of public school students and the Ohio Federation of Teachers. "As far as we're concerned, at the end of the school year, the voucher program can no longer continue."

Ohio Attorney General Betty Montgomery plans to file an appeal and stay request with the Ohio Supreme Court next week, aides said. If the stay is granted, Ohio could continue funding the program until all appeals are exhausted.

The current state budget proposal pending in a Senate committee includes \$15.8 million for Cleveland's voucher program over the next two years. The court of appeals ruling even has Republicans questioning plans to expand vouchers.

"Based upon the court ruling, it would be unwise to expand it," said Senate Finance Committee Chairman Roy Ray, R-Akron. "What we need to do is confer with attorneys and look at what the options are."

Senate Minority Leader Ben Espy, D-Columbus, renewed calls to remove the voucher money from the budget, saying that continued funding not only violates Thursday's ruling but the March 24 Ohio Supreme Court ruling.

"Any time you divert money from the public schools, you keep the state from creating a thorough and efficient system of public schools," Mr. Espy said. "I think the Ohio Supreme Court is going to believe that, too."

Senate President Richard Finan, R-Evendale, refused to discuss what, if any, effect the ruling would have on the voucher money in the next two-year state

budget.

"I believe this case will be appealed

all the way to the United States Supreme

Court."■

3. The Washington Post

05/02/97; Edition: FINAL; Section: A Section; Page A04

Cleveland Students Can't Use Vouchers for Religious Schools, State Court Rules

By Rene Sanchez
Washington Post Staff Writer

The only school voucher program in the nation that lets children attend private religious schools using public money was stopped yesterday by an Ohio court that ruled it violates the state's constitution.

In a case that educators and lawmakers nationwide have been following closely, the Ohio Court of Appeals decided unanimously that the voucher program created last fall in Cleveland for 2,000 disadvantaged children infringes on the separation of church and state.

The ruling is the latest among several recent legal setbacks for proponents of school vouchers and it could give new momentum to groups that are battling to stop similar plans under consideration in many states. Earlier this year, a Wisconsin court prevented poor families in Milwaukee from expanding a school voucher program to include religious schools.

"This should send a clear message to all the other states where voucher plans keep coming up," said Carole Shields, president of People for the American Way, one of the groups that challenged the Cleveland program. "Public money has to be for public use."

Advocates of school vouchers reacted with dismay to yesterday's decision, but vowed to appeal it immediately to Ohio's supreme court. "This is the Valley Forge of the school choice movement — a

dark, cold hour," said John Kramer, a spokesman for the Institute for Justice, a Washington group that is lending assistance to voucher cases across the country. "But ultimately we still think that we will prevail in the courts."

School vouchers have become one of the most divisive issues in American education. Those who support the idea tout it as a bold new way to give poor families trapped in bad public schools more choice about where their children are educated. They also argue that giving parents public money to choose whatever school they want will create a more competitive, market-oriented education system that will force public schools to improve.

Voucher opponents say the idea may help a relative handful of students, but it will harm many more by draining money from public schools. In Cleveland, the money for the voucher program is taken from state funds used to support public schools.

Legal debate on the issue has focused mostly on whether vouchers can be used for private religious schools. That has been an especially relevant point in both Cleveland and Milwaukee because the dollar amounts of the vouchers are often only large enough to help pay tuition for Catholic or other religious schools, and not private nonreligious schools.

In Cleveland, for example, participating parents have average incomes of less than \$10,000 a year and receive vouchers of up to \$2,500 a year

for tuition. About 80 percent of the 2,000 students eligible for vouchers have used them to attend religious schools, and most of those schools are Catholic. If yesterday's court ruling stands, this fall those children either will have to go to private nonreligious schools or return to the city's public school system.

In its ruling, the Ohio court said the voucher program should be stopped because it "provides direct and substantial nonneutral government aid to sectarian schools."

It also noted that the preponderance of Cleveland students receiving vouchers were using them to attend religious schools.

"The only real choice available to most parents is between sending their child to a sectarian school and having their child remain in the troubled Cleveland City School District," Judge John C. Young wrote.

Voucher advocates contend that giving students public money to attend primary and secondary religious schools is no different from the loans and grants some students receive from the state and federal governments to attend private religious colleges — and thus does not violate the separation of church and state.

But voucher opponents argue that there is a profound difference between a religious college that young adults choose to attend for all manner of study and a sectarian elementary school that teaches young children religion daily. ■

4. The New York Times

05/02/97; Edition: Late Edition - Final; Section: Section A; National Desk; Page 1, Column 1

THE BUDGET BATTLE: THE NEGOTIATIONS *Clinton and Congress in Accord On Budget, Except for Tax Cuts*

By RICHARD W. STEVENSON

WASHINGTON, May 1 — White House and Congressional negotiators agreed today on nearly all elements of a plan to balance the Federal budget in five years, but failed to reach a final deal largely because of continuing differences over tax cuts.

The emerging deal would give Republicans many of the tax reductions that they have insisted on and the White House the spending that it sought for President Clinton's priorities, including education, health insurance for children and welfare.

The two sides compromised on scaling back Medicare and agreed to a small reduction in the cost-of-living adjustment for Social Security and other Government benefits.

The President and his staff were in almost constant contact with Republicans throughout the day, as Congressional Democrats fumed on the sidelines, angry about not being consulted more closely and fearful that the deal would give away too much to Republicans.

As the day wore on, the Administration and Republican leaders said that they were still optimistic that the final pieces would fall into place but that they were also wary that their delicate compromise could melt if there was not a breakthrough soon on the remaining issues.

By late tonight, people involved in the process said they were making progress on the main sticking point, a dispute over how specific the agreement would be in apportioning the tax cuts. The Administration, goaded by Congressional Democrats, sought assurances that the cost of the tax reductions would not explode in the five years after the five-year period covered by the agreement.

But Republicans were resisting the White House's demand to put limits on specific types of tax cuts, including a reduction in the capital gains tax, and said they were reluctant to grant the President assurances that his proposals for tuition tax credits would be adopted in just the way he wanted.

After two years of bitter partisan warfare that led to shutdowns of the Government and ultimately helped revive Mr. Clinton's political fortunes, the two sides hammered out a framework that encompassed compromises on nearly all these issues that had divided them:

*Spending for domestic programs

would decline \$68 billion over five years after accounting for inflation. \$20 billion more than Mr. Clinton had first proposed. But the deal would contain provisions for \$32 billion in new spending on initiatives that the White House had demanded, including expansions of health insurance for children, student loan programs and restoration of welfare benefits for some legal immigrants. The deal would also include the President's proposals for tuition tax breaks.

*Taxes would be cut by \$135 billion over five years, paid for in part by \$50 billion in revenue increases from closing loopholes and extending existing taxes that would otherwise expire. The money would be allocated in a yet-to-be determined way between reductions in the capital-gains and estate taxes, credits for families with children and several smaller initiatives.

*The growth in Medicare spending would be cut \$115 billion over five years, roughly splitting the difference between each side's most recent offers. The growth in spending for Medicaid would be cut \$24 billion.

*Military spending would increase about \$23 billion, equivalent to a cut of more than \$80 billion after accounting for inflation.

*The Government would revise its formula for calculating cost-of-living adjustments for Social Security and other benefits recipients to reduce spending by tens of billions of dollars over the next five years.

Neither side has specified yet how big the adjustment would be, but they would be limited to technical changes by the Bureau of Labor Statistics to address the growing consensus that the Government's main inflation gauge, the Consumer Price Index, overstates increases in the cost of living.

That the two sides were so close to a deal was a result of not just a willingness to compromise, but also good economic news that over the last several years has helped push the deficit steadily lower, making the size of the problems facing the two parties that much smaller.

Although the deal, if completed, would probably have substantial economic benefits for the nation, it also represents a much broader effort at adapting the priorities of both parties to new realities, the need for a Democratic President and a Republican Congress to

work together and the need of both sides to satisfy the demand of a global economy for increased fiscal austerity.

Republican leaders and Administration officials rushed around Capitol Hill to sell the package to their parties, and there appeared to be enough votes from Republicans and centrist Democrats to insure passage. But there was little doubt that liberal Democrats and conservative Republicans found the deal unacceptable.

About a dozen conservative Republicans complained that the deal did not go far enough in cutting taxes, slashing spending on domestic programs and reducing the size and influence of the Government.

Senator Phil Gramm, Republican of Texas, likened the compromise to a dead fish. "You set it on the table for three or four days," he said, "and it will begin to stink."

House Democrats lashed out at the White House for keeping them in the dark about the details of the negotiations. And they argued that the White House was handing Republicans the opportunity to translate the broad budget agreement into the nitty-gritty of tax and spending bills completely on their terms.

Administration officials acknowledged that they were at risk of not meeting the President's commitment that any budget deal would have to win the support of a majority of the Democrats in both the House and the Senate.

"We want to get a majority of Democrats on board in the deal," said Michael D. McCurry, the White House spokesman. "We won't have them to begin with, and obviously you're beginning to hear some of the criticisms and concerns before we have had the opportunity to make the case for whatever the agreement is."

Senate Democrats seemed generally supportive. But a large contingent of House liberals appeared quite likely to vote against the deal, despite a concerted effort by top Administration officials, including Erskine B. Bowles, the White House chief of staff, Franklin D. Raines, the budget chief, and Treasury Secretary Robert E. Rubin to win them over in a series of meetings.

Part of the Administration's message was that Democrats were never going to get everything that they wanted.

04:55 EDT May 2, 1997■

**Private School Vouchers and Student Achievement:
An Evaluation of the Milwaukee Parental Choice Program**

EXECUTIVE SUMMARY

Cecilia Elena Rouse
Princeton University and NBER

December, 1996

I thank Orley Ashenfelter, David Card, Hank Farber, and Alan Krueger for insightful suggestions and conversations, and participants at the National Bureau of Economic Research's Program on Children Conference and the Princeton Labor Lunch for helpful comments. I particularly thank Lisa Boeger, Jay Greene, and John Witte for insights about the program and lots of help with the data. Jeff Wilder provided expert research assistance. All errors are mine. Author's address: Industrial Relations Section Firestone Library, Princeton University, Princeton, NJ 08544; email: rouse@princeton.edu

Introduction

At the cornerstone of many school reform proposals lies the premise that private schools are more efficient than public schools. This premise is particularly prominent in the current "school choice" debate. Proponents of school choice argue that governments should offer tuition vouchers to families who wish to send their children to private, rather than public schools. If private schools are indeed more effective than public schools, a voucher program may offer a cost-effective way to improve the quality of education. Some authors have found that students in private schools have higher achievement levels than those in public schools. However, critics of school choice programs have argued that private schools would not necessarily do a better job educating students who are currently attending public schools. Rather, they argue that the observed superiority of private school students arises from the selection process that leads higher-achieving students to attend private schools.

Ideally, the issue of the relative effectiveness of private versus public schooling could be addressed by a social experiment in which children in a well-defined universe were randomly assigned to a private school (the "treatment group"), while others were assigned to attend public schools (the "control group"). After some period of time, one could compare outcomes, such as test scores, high school graduation rates, or labor market success between the treatment and control groups. Since, on average, the only differences between the groups would be their initial assignment – which was randomly determined – any differences in outcomes could be attributable to the type of school attended. While such an experiment has never been implemented, the legislative requirements of a recently enacted school voucher program in Milwaukee, Wisconsin allow one to come close to such an idealized experiment.

In 1990, Wisconsin became the first state in the country to implement a school choice program that would provide vouchers to low income students to attend non-sectarian private schools. The number of students in any year was limited to 1% of the Milwaukee public school membership in the first four years, but was expanded to 1.5% in 1994. In addition, choice schools were only allowed to admit up to 49% of their students as part of the choice program (expanded to 65% in 1994), and were not allowed to discriminate in which students they took. This was interpreted to mean that if the school was oversubscribed, the students would be randomly selected from among the applicants. If a choice school was not oversubscribed, it was required to take all who applied, with only a few exclusions.

Because the choice program was so small, it cannot provide an analysis of the potential benefits of "school choice". In the most unrestricted school choice program, all (or a substantial fraction) of the students in the public schools would be eligible to attend a private school. Since state funding would be tied to student enrollments, the public schools would have an incentive to improve, leading to no differences in the outcomes of students in "public" and "private" schools. Analysis of the Milwaukee Parental Choice Program can, however, indicate whether, in the short run, the academic achievement of those children that attend the private schools would likely increase.

Analysis

My analysis consists of three components. The first two components use the unsuccessful applicants as a control group. By using applicants who are not selected as a

control group. I take advantage of the quasi-natural experiment provided by the fact that the schools randomly selected students from their (oversubscribed) applicant lists. The third component uses a sample of students from the Milwaukee public schools as a comparison group.

Component # 1: Did the Milwaukee School Choice Program Result in Higher Achievement Gains?

First, I compare the test scores of students selected for the program to those not selected for the program from among the applicants. (Note that these students were selected, but did not necessarily attend a choice school. I refer to this as the "intention-to-treat" or "reduced-form" analysis.) There are at least two reasons why we might be interested in the simple comparison of the test scores of those selected for the program and those not selected for the program. First, making vouchers available is the only policy instrument available to policy makers. If the state of Wisconsin decides to provide educational vouchers to all low income students, not all will take advantage of the program and not all who enroll will remain. For example, of the approximately 300 students who applied in 1990 and were selected to attend a choice school, 98% ever enrolled in a choice school, but only 76% remained through the first spring semester. By the second spring, approximately 58% remained. In the extreme case in which no students actually use the vouchers, even if private schools are much better at educating children than are the public schools, there will be no achievement gains from the program. Thus, these simple ("reduced-form") estimates reflect the overall potential gains from offering the vouchers. Secondly, as in many experimental settings, the randomization only occurred in offering the opportunity to attend a choice school (the "intention to treat") and as such, the "reduced-form" estimate is the only unambiguously unbiased ("true") estimate that one can obtain using typical statistical methods (i.e., ordinary least squares (OLS) regression.)

I estimate that being selected to participate in the choice program appears to have increased the math achievement of low-income, minority students by about 1-2 percentage points per year. Given a standard deviation (denoted by σ) of about 19, this suggests effect sizes on the order of 0.05σ - 0.11σ which are quite large for education production functions. On the other hand, the effects on the reading scores are not as large and are statistically indistinguishable from zero.

Components # 2 and #3: Did Students in the Non-sectarian Private (Choice) Schools in Milwaukee Have Greater Achievement Gains Than Those in the Milwaukee Public Schools?

In the second component, I continue to analyze only the applicants and use the initial selection as an instrumental variable that is correlated with attendance at a private school and uncorrelated with test scores. The instrumental variables estimate of the effect private schools will provide a consistent estimate of a "causal" effect of attending private school for a period of time on test scores. This method compares the test scores of those who actually enrolled in a private school to those of not-selected applicants in the Milwaukee public schools, and scales the difference in test scores by the probability that those who were randomly selected to attend a private school actually enrolled in a private school.

As a third component, I use a random sample of students in the Milwaukee public schools as a comparison group. One reason why choice students may not appear to achieve more than students in the public schools is that the sample of students from the Milwaukee public schools may not be representative of students eligible for the choice program. They may be too advantaged. If so, then it will be difficult to control for all differences between students in the public school sample and choice students. I can, however, include "individual fixed-effects" which control for all time-invariant differences between the choice students and the comparison sample. (If I had only two test scores, one from before the student attended the private school, and one after selected students had attended a choice school, the "fixed-effects" analysis is akin to estimating the effect of the choice school on the change in test scores.)

The results on whether the (non-sectarian) Milwaukee private schools are "better" than the public schools suggest that the math scores of students in private schools increased an additional 1.6-1.9 percentage points per year, and the effect is significant at the 5 percent level. At the same time, the math test scores of those students who attended a private school for some period and then returned to the Milwaukee public schools also increased an additional 1.3 percentage points per year above other students in the Milwaukee public schools although the difference is not statistically significant (the p-value of the difference between those who received full and partial treatment is between 0.079 and 0.13). The results for reading suggest that students in the choice schools may actually score lower than students in the Milwaukee public schools, although any differences are not statistically different from zero.

Comparisons with Previous Studies

These results are partially consistent with both Witte, Sterr, and Thorn (1995) and Greene, et. al. (1996), but not completely so. Witte, Sterr, and Thorn (1995) compare the achievement gains of students in the choice program to a random sample of students from the Milwaukee public schools. The biggest difference between the analysis presented here and that of Witte, Sterr, and Thorn is that I control for all (fixed) differences between choice students and students in the Milwaukee public schools, both observed and unobserved. Witte, Sterr, and Thorn control for observable characteristics, such as family income and previous test scores. However, they may not have controlled for all differences between students in the choice program and those in the Milwaukee public schools. Given that the students in the choice schools were more disadvantaged than the sample of students from the Milwaukee public schools, Witte, Sterr, and Thorn's estimates may understate the effect of the program.

The sample used in the analysis using only applicants to the choice program is quite similar to that used by Greene, et. al. (1996). There is one major difference, however. I include all students who applied to the choice program; Greene, et. al. exclude those students who were selected for the program but never enrolled in a private school and those who left a private school. If take-up and attrition from the program is random, Greene, et. al.'s exclusions would be unambiguously statistically valid. However, in practice it is unlikely that students who do not take up the program or who leave the program are a random sample of students who applied to the program. In particular, students who are not being well served by the private schools may be more likely to leave, and those who do not take up the program may

not expect to be well served. In addition, I combine all years of the program and estimate, in one equation, the yearly effect of the program.

Conclusions

The results using the quasi-experimental applicant control group and the random sample of students from the Milwaukee public schools (when I include individual fixed-effects) are remarkably similar. On the one hand, I find that the Milwaukee Parental Choice Program and the participating private schools likely increased student math scores by 1.5-2 percentage points per year. On the other hand, the results for reading scores were quite mixed with both positive and negative coefficient estimates. While it is not clear how to weigh the math results versus the reading results, when I total the math and reading scores, I estimate that private school students gained approximately 1.3 percentage points per year and the effect has a p-value of 0.063. (Based on the full sample with all students including individual fixed-effects.)

Although these results obtain using a variety of estimation strategies and samples, there are at least two caveats to keep in mind. First, I had to impute the total math score for a significant fraction of the Milwaukee public school students. Second, and most importantly, there is substantial sample attrition in the later years. While my analysis suggests that the results are robust to the imputation and sample attrition, my strategies cannot substitute for better data.

The data collection from Milwaukee should be applauded as it allows us to learn more about the effectiveness of this program than from many other reforms. Nevertheless, if one lesson emerges from this effort, it is that future evaluations of reforms of this sort should anticipate high mobility among the students and recognize that administrative data are determined by the needs of the schools and not those of the evaluator. An evaluation design that treats the participants (and control or comparison group) as a survey sample with independent follow-up, though more costly, would avoid some of the data problems experienced here.

Finally, although the experience in Milwaukee suggests that providing vouchers to low income students to attend private schools could help increase the mathematical achievement of those students who participate, it cannot shed light on whether vouchers provide an incentive for the public schools to improve and therefore increase the quality of education provided to all low income children. In addition, the results from one program implemented in one city cannot, and should not, be the only evidence on which important policy regarding the structure of American education is based. It is only by piecing together evidence from many places that we will ever really learn whether private school vouchers could increase student achievement.