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School Governance and Information: Does Choice Lead to Better-Informed Parents?

Brian Kisida[1] **and Patrick J. Wolf**[1]

Abstract

Political theorists have long argued that low information levels among average citizens provide the rationale for public policy to be guided by experts and elites. Other scholars counter that deference to elites perpetuates and even exacerbates the problem. Here we look at school choice programs as an environment to elucidate this important debate. Theories of school choice suggest that parents need to and can make informed decisions. Choice parents should have more incentives to gather information about their child's schools than parents without schooling options. Alternatively, a lack of any increase in information levels among school choosers would suggest that having choices per se is not sufficient motivation to overcome the costs of information gathering. Analyzing data from an experimental evaluation of the Washington Scholarship Fund, we find that presenting parents with choices does lead to significantly higher levels of accurate information on measures of important school characteristics.

Keywords

school choice, vouchers, school governance, school characteristics, political information, political efficacy

Do citizens gather more and better information when they are assigned the responsibility to make important choices? This question is central to the debate between the pluralist–democratic and elitist schools of thought

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regarding methods and systems of governance. If citizens generally lack the motivation or ability to gather information about candidates, policies, and programs and distinguish accurate information from misinformation, then governance by elites is arguably both necessary and desirable. If, however, common adult citizens of our republic can and do rise to the challenge of effectively educating themselves when important decisions lay in their hands, then the pluralist–democratic project of widespread citizen empowerment would seem to be more plausible and less of a pipe dream.

Here we test political science theories of the relationship between citizen responsibilities and knowledge by way of a randomized experiment. More than 1,500 low-income public school students in the District of Columbia applied and were deemed eligible for privately funded K-12 tuition scholarships in 1998. Because the program's resources were not adequate to fund all eligible applicants, about 800 of the students received scholarships by lottery. The remaining approximately 700 students represent the ideal comparison group, because they differ from the "treatment" group of scholarship recipients merely by chance. We use data from the 1st- and 2nd-year analysis of this school choice experiment to determine whether the parents of treatment group students possess more accurate information about their child's school than the parents of control group students. Below we discuss the political science theory that informs our study, the data and methods we used to test our hypotheses, the results of the analysis, and the implications for democratic theory.[1]

Theory

Information and Citizen Efficacy

Many debates about the effectiveness of political participation center on expectations of citizen competence. It is widely argued that citizens require information and that those with better information are able to make choices that are more closely aligned with their own self-interests. Yet, political scientists have reached a general consensus that the average citizen falls short of the ideal (Berelson, Lazarsfeld, & McPhee, 1954; Campbell, Converse, Miller, & Stokes, 1960; Zaller, 1992). Moreover, empirical evidence suggests that low general information levels can bias what mass attitudes would be if everyone were better informed (Bartels, 1996; Delli Carpini & Keeter, 1996). Factual information is especially important for citizens to realize policy-specific preferences (Gilens, 2001; Kuklinski, Quirk, Jerit, Schweider, & Rich, 2000; Schneider, Teske, Marschall, & Roch, 1998). Our analysis

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here is a twist on this line of research. Although many studies of information effects have found that increased information leads to more active and informed choices, our goal is to examine if the presence of more choices leads citizens to gather more and better information.[2]

The concept of the interplay between responsibility and preparation is not new. Many political theorists have argued that increased participation in the political system should have the effect of informing and training effective citizens (Dewey, 1916; Gutmann, 1987). Of course, their claims could be mere wishful thinking. Anthony Downs's (1957) classic work *An Economic Theory of Democracy* makes the case that in a democracy "it is individually irrational to be well-informed" (p. 246). Downs specifically points out that whereas the costs of gathering information are high, the benefits are low—both because a single individual's preferences are highly unlikely to affect democratic outcomes and because the benefits derived from democratic decisions are "indivisible" (Downs, 1957; see also Olson, 1965). That is, the aggregation of citizen preferences and government benefits in a democracy creates an incentive for citizens to be participation and informational free riders.

Other theorists have pointed out, however, that this simple cost–benefit equation is affected when different motivational factors for political participation and information gathering are considered (see, e.g., Denzau & North, 2000; Lodge & Taber, 2000; Verba, Schlozman, & Brady, 1995). Or, as Luskin (1990) succinctly puts it, citizens need to "have reason enough to make the effort" (p. 335). Similarly, Lupia and McCubbins (1998) point out that one of the fundamental lessons of cognitive science is that learning is "goal oriented" (p. 18; see also Fiske & Taylor, 2008).

Political scientists have identified many examples of motivational factors that can lead to higher levels of information among certain citizens, yet these studies have largely focused on the interplay of issue saliency, citizen traits, and citizen resources. For example, Bobo and Gilliam (1990) found that Blacks living in areas with greater numbers of Blacks in political positions of authority are more likely to gather political knowledge because they see Black officials as potentially more responsive to their concerns. Luskin (1990) finds that those in occupations more affected by government policies have higher levels of political knowledge than those in occupations that are less affected by government policies (see also Hutchings, 2001; Iyengar, 1990). Other researchers have found that civic motivation can be affected by the framework of political institutions and the selective benefits offered to participants (Campbell, 2002; Gimpel, Lay, & Schuknecht, 2003; Mettler, 2002; Niemi, Hepburn, & Chapman, 2000; Smith, 2002; Soss, 1999; Verba et al., 1995).

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Along these lines, some theorists have suggested that citizen motivation can be enhanced by altering the institutional arrangements of the political system in such a way that citizens are provided incentives to participate and become informed. Mark Warren (1992) captures this argument well, writing the following:

> On the expansive view, were individuals more broadly empowered, especially in the institutions that have most impact on their daily lives (workplaces, *schools* [italics added], local governments, etc.), their experiences would have transformative effects: they would become more public-spirited, more tolerant, *more knowledgeable* [italics added], more attentive to the interests of others, and more probing of their own interests. (p. 8)

From this perspective, citizen information is a precious good that is enhanced by community participation that can itself be motivated by redistributing responsibility and power. Specifically, Benjamin Barber (1984) recommends a voucher approach to delivering government services when laying out his view of institutional reforms that could serve his ideal of "strong democracy." On vouchers, Barber writes the following:

> Their great virtue is that they are intolerant of state bureaucracies in that they mobilize parent/student constituencies in a fashion that also serves to mobilize citizenship . . . to care for and to act on behalf of one's own interests is the first step toward civic activity in a lethargic representative system where individuals are accustomed to deferring to politicians, bureaucrats, experts, and managers. Vouchers are a form of power, and power is the most effective catalyst citizenship can have. (pp. 295-296)

As Barber (1984) notes, one of the potential benefits of vouchers is that they mobilize citizens by increasing ways in which they can directly achieve their *own* interests.[3] As a result, Downs's (1957) point about the indivisible nature of government services and its detrimental effects on participation are abated through the use of vouchers, because they forge a link between informed participation and consequent benefits that is more direct and personal. School choice, in a sense, brings elements of participatory democracy into the world of compulsory education and, thus, can shed light on our understanding of the potential benefits and problems that have long challenged democratic theorists of political participation.

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Information and School Choice

From a policy perspective, citizen/customer information is central to theoretical arguments about the application of market mechanisms to education. Thus, a major claim of supporters of school choice policies is that choice can motivate parents to place their children in schools that are a good "fit" for their particular needs (Chubb & Moe, 1990; Coons & Sugarman, 1978; Godwin & Kemerer, 2002; Wolf, 2005). That important assertion rests on the assumption that low-income parents become reasonably well-informed educational consumers when they participate in school choice programs.

A furious debate rages over this question of whether the disadvantaged parents who are the target of most school choice initiatives become sufficiently informed about schools to choose confidently and effectively. If new educational consumers are uninformed or misinformed about private schools, then such an information asymmetry would undermine the potential benefits of parental choice (Smith & Meier, 1995). A consumer is not likely to be satisfied with a purchase if they do not really know what they have bought. Based on a qualitative study of a voluntary urban–suburban busing program, Amy Stuart Wells (1996) reports that, "Transfer parents and students lacked information about the 16 county districts and about particular schools, suggesting they were not making the best choice" (p. 36).[4]

Parents who participate in school choice programs certainly *say* that they are better informed about their new schools of choice. Weidner and Herrington (2006) find that parents whose children participate in Florida's McKay Scholarship Program for students with disabilities report higher levels of satisfaction with the amount of information that they receive from their new schools. Drawing on data from a three-city experimental evaluation of voucher-like programs in New York City; Dayton, Ohio; and Washington, D.C.; Howell and Peterson (2002) report that parents of students attending private schools describe a much more intensive school–home information exchange than do parents of students in the comparison group. In a review of the early literature on school choice and parent information, Schneider, Teske, Roch, and Marschall (1997) report that evaluations of the Alum Rock and Milwaukee school choice programs established a positive connection between the availability of school choice and higher levels of information about schools. Schneider et al. (1998) had similar findings in an examination of school choosers in New York City.

School choice programs may induce schools to share more descriptive information, and parents may be more satisfied with what they are receiving, but do parents actually know more about their child's school as a result of

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choosing it? Schneider, Teske, and Marschall (2000) are the only researchers so far to test whether the higher amounts of school information that school choice parents report actually result in choosers having more accurate information about the school their child attends. They matched New Jersey and New York public school districts that require all parents to choose their child's school with comparison districts in their respective states that lacked school choice. The researchers asked parents for the name of the principal of their child's school as well as estimates of the average reading test score, class size, percent Hispanic, and percent African American at the school. They compared the parental responses with measures of the actual characteristics obtained from the schools and calculated the percentage of correct answers for the case of principal name and distance scores for the four school-level characteristics. They reported that the suburban parents in the New Jersey school choice district "in fact are more accurate about test scores and the percentage of Hispanic children" in their child's school than the comparison parents (pp. 161-162). The urban parents in the New York City choice district were only more accurate than the nonchoice comparison parents regarding the name of their child's principal. They conclude that, "central-city parents with choice do not have consistently higher levels of accurate information than other urban parents" (p. 163). In related research, Schneider and his team (Schneider et al., 1997; Schneider & Teske, 1997) concluded that the responsibilities associated with parental school choice increased social capital.

This pioneering research by Schneider et al. (2000) has made an important contribution to the literature on school choice and parent information. However, it should not be the last word regarding the long-running dispute of whether or not the availability of school choice results in parents becoming better informed about their child's school. Schneider's team compared the results from school choice and nonchoice school districts that were comparable on many important observable characteristics; yet it is entirely possible that the choice and comparison populations differed significantly on unobservable characteristics associated with their decisions to live in their respective school districts, generating a selection effect that could have biased their findings either in favor of or against identifying a school choice advantage. Moreover, because school choice is a politically controversial policy reform, a wealth of compelling evidence will be necessary to convince skeptics that parents actually become better informed about education as a direct result of being offered educational choices.

This study draws on data from an experimental evaluation of school choice in Washington, D.C. to determine whether low-income parents who

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are exercising choice are better informed about key characteristics of their child's school than are their nonchoice peers. Parental responses to survey questions about school size and class size are compared with actual administrative data from the schools. If educational markets tend to produce better informed consumers, then the parental responses of the treatment group should be more closely associated with the school data than the parental responses of the control group. If the data suggest that school choosers are *not* better informed about their children's schools than are comparable nonchoosers, that result would call into question major components of both market theory as applied to education and the citizen empowerment movement in political science.

Analyzing parental responses to survey questions about school size and class size is important for several reasons. Verifiable questions of fact, such as the size of things, avoid the inherent unreliability of self-reported assessments of knowledge (Price & Zaller, 1993). Additionally, school size and class size rate high on measures of school qualities that parents find important (Armor & Peiser, 1998; Howell, 2006; Schneider et al., 2000). Both of these school characteristics have been shown to affect student achievement (Mosteller, 1995; Sander, 1999) and are also associated with higher levels of school safety and parental satisfaction (Nathan & Thao, 2007). And, perhaps most important, parental accuracy on measures of school size and class size signal a general familiarity with their child's school and are likely correlated with knowledge about other more subtle school qualities that parents also rely on to make informed decisions. Previous research has found that even limited measures of information levels are highly useful in measuring the sophistication of survey respondents (Delli Carpini & Keeter, 1996).

Hypotheses, Data, and Method

Hypotheses

At this early stage in the research on the information and involvement effects of school choice, we treat the hypothesis of a causal relationship as the alternative to the null hypothesis. Thus,

> *Hypothesis 1:* Parents provided with the opportunity to choose their child's school will be better informed about the characteristics of that school than comparable parents who are not provided access to school choice.

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Our first hypothesis tests the theory that new consumers of a public good such as education will seek and obtain accurate information about the purveyors of that service before making their selection. The responsibility of selecting a school for one's child could, however, also result in a more gradual accretion of accurate information over time. This would be the case if private schools, because they operate in a more competitive market environment than public schools, provide more information to parents about their schools even after parents have selected them. A lack of any increase in information levels over time would suggest that the process of gathering information is primarily consumer driven and occurs at the point of sale, much like the case of voters who collect a great deal of information about candidates just before elections and then move on to other activities after their selections are made. Because our data are longitudinal, we also can test this second hypothesis:

Hypothesis 2: The impact of the school choice opportunity on the accuracy of parent information about schools will increase over time.

It is these two questions regarding the immediate and longer term effects of school choice on parent information that we test using extensive data and "gold standard" evaluative methods.

Data

The core of the data that we use in the study comes from the 2nd-year evaluation of the Washington Scholarship Fund (WSF) privately funded voucher program (Wolf, Peterson, & West, 2001), now called the Signature Scholarship Program (SSP). This voucher-type program predates and is distinct from the federally funded Opportunity Scholarship Program, which also is administered by the WSF under a contract with the U.S. Department of Education (Wolf et al., 2009). The privately funded SSP provides partial tuition scholarships of up to $3,000 to families in the District of Columbia with household income at or below 270% of the federal poverty line. Families with income below the poverty line are eligible for the maximum scholarship amount, whereas families at 270% of poverty are eligible for about half of the maximum.[5] The scholarships are like vouchers, which can be redeemed at any of the more than 100 D.C. private schools that participated in the program during the evaluation period of 1998 to 2001. WSF has been awarding Signature Scholarships to D.C. students since 1993. In 2001, the final year of the

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evaluation that produced these data, the WSF was supporting 1,325 elementary and secondary school students with Signature Scholarships.

The WSF experienced a dramatic expansion of their SSP in 1998. Because demand greatly exceeded even the supply of about 1,000 new scholarships to students who previously attended public schools, the vouchers were awarded by lottery. Because only the luck of the draw determined which family would or would not receive a voucher, the effect of the voucher on student and family outcomes could be studied experimentally by a randomized field trial (RFT). In the spring of 1998, the families of 1,582 public school students in Grades 1 to 7 were entered into a purely random lottery and 811 of the students were awarded scholarships. The remaining 771 students comprised the control group for the study.[6] The treatment and control groups in the D.C. evaluation did not differ significantly on any of nearly 50 educationally relevant characteristics measured at baseline (Peterson, Greene, Howell, & McCready, 1998).[7] The lottery effectively randomized the two study groups.

The participants in this school choice experiment were highly disadvantaged in various significant ways. At baseline, only 16.5% of families included a married mother, whereas another 3.8% reported that they were living with a partner. Only 9.2% of mothers reported having obtained a college degree. The average household income was $18,583. Such qualities have generally been found to hinder the potential for citizens' information gathering. Thus, the data from this program provide a hard test for our hypotheses of empowerment leading to significantly higher levels of relevant knowledge.

One year after random assignment, the families were invited to attend any of several initial outcome data collection events. A total of 998 students responded, for an overall response rate of 63% that was the same for both the treatment and the control groups.[8] Two years after the voucher offer, in the spring of 2000, the members of the treatment and control groups again were invited to data collection sessions in which their children were tested, and the parents and older students were surveyed about their educational experiences. The 2nd-year turnout of 730 students represented an overall response rate of 50% of the 2nd-year outcome study population.[9] Again the response rates were the same for both the treatment and control groups. The respondent data are weighted to rebalance each year's outcome sample to reflect the baseline characteristics of participants to prevent nonresponse from biasing the analysis (Howell & Peterson, 2002).

The SSP evaluation data were generated through a number of measurement instruments, including a parental survey of their child's experiences at school. Two very specific questions that were asked of parents each year were about

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school size and class size (Wolf et al., 2001). Because those two questions are concrete and unambiguous, they provide the basis for our empirical test here.

These core data from the RFT were then supplemented by information collected from and about the various public and private schools that the students attended during the 1998-1999 and 1999-2000 academic years. The supplemental data included information about per pupil spending on students as well as statistics regarding student body characteristics, enrollments, and class sizes. For the public schools in the sample, these data were provided to us by the district's Office of Public Accountability. For the private schools in the sample, the data were obtained from two sources, depending on the type of private school involved. Information about the Catholic parochial schools in the sample was obtained from the Office of the Superintendent of Schools for the Catholic Archdiocese of Washington, D.C. Statistics regarding the nonparochial religious and independent private schools in the sample were obtained from responses to a survey we mailed to those schools. The response to the mail survey was high, thanks in part to our persistence in following up with private school administrators, as we obtained at least some information from more than 80% of the private schools in the sample. We then matched each school's administrative data to the observations of students in our database who had attended that particular school.

The administrative data from the schools provide some interesting contrasts between the treatment and control groups (Table 1). Students in the treatment (i.e., scholarship) group tended to receive their education in schools that were about .8 standard deviations smaller than those of members of the control group. Treatment group students experienced class sizes that, on average, were statistically similar to those of the control group. For three of the four measures, the standard deviations for the treatment group were somewhat larger than those of the control group, confirming the conventional wisdom that the population of private schools tends to be more variegated than the population of public schools, which tends to be more standardized.

Method

In this analysis, we rely specifically on two measures of information recorded from the parent surveys that we then matched against the supplemental data collected about the schools their children were attending. In 1999 and 2000 the parent surveys asked the following questions:

- Approximately how many students attend this child's school?
- Approximately how many students are in this child's class?

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Table 1. Treatment and Control Comparison: School Data

	Treatment Group		Control Group	
Characteristic	*M*	*SD*	*M*	*SD*
Actual school size, 1999	348.03	194.36	488.00	192.09
Actual school size, 2000	323.33	183.97	470.89	196.89
Actual class size, 1999	17.11	4.14	16.53	2.08
Actual class size, 2000	17.50	6.20	17.00	4.96

In the parent survey, the answers to both questions were recorded using ordinal categories. For the school size question, parents chose from among five ordered categorical ranges (e.g., 1-150, 151-300, etc.) and a "don't know" option. The class size question incorporated eight categorical ranges and also included a "don't know" option.[10]

Because the school-supplied data were reported in terms of real continuous numbers, there was an inherent mismatch to the parent-generated and school-generated measures of the characteristics. Importantly, this mismatch is consistent across the treatment and control groups and therefore is unlikely to bias the results of the analysis. Still, the parent and school data needed to be transformed into a common metric. For our primary model we recoded the schools' actual data by assigning them to the respective ordinal category that corresponded to the parents' survey options. This conversion facilitates a valid comparison of the accuracy of responses though it will introduce some inefficiency into the analysis by collapsing the continuous school data into discrete categories.[11]

We then created our dependent variable of interest by taking the absolute value of the difference between the parent estimate and the recoded school values (Schneider et al., 2000). This newly constructed measure of differences had a range of 0 to 4 for the measure of school size and a range of 0 to 6 for class size, with 0 in this case representing a "perfect" score or exact match between parental perceptions and school reports. In other words, a parent who chose the same category that the school data fell into had a score of zero, and each additional value on this scale represents missing the "correct" category by an additional unit. Because only a very few parents had scores of 4 or more, we collapsed these outliers into the category with a score of 3, reducing the overall range of our dependent variable to four ordinal categories 0 to 3.[12]

Next, we incorporated an option into our design to screen out any programmatic impacts that may have been a product of the different ranges of

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values that the treatment and control groups contain. If either private or public schools, as a class, are more standard in their school or class size, then it would be easier for parents of students in the more standardized sector to guess correctly, even if they did not possess more accurate information about the particular school. Schneider et al. (2000) mentioned that variations in the dispersions of their data across their comparison groups might have influenced their results. In their case, the patterns of dispersion mitigated against a finding of a school choice effect, so they left the data as they were to provide a biased but "strong" test of their hypothesis.[13]

In our case, the dispersion of the data is larger on the private school side of the comparison for three measures and years but smaller for one. Because of this inconsistency, it is better for us to eliminate any potential bias in this case instead of merely interpreting the results in the presence of likely bias. To control for differing dispersions of the data, we standardized our dependent variable by dividing the treatment and control differences from the true values by the standard deviations of the actual school data. Thus, our second specification is sensitive to the spread of values in the underlying data because the measure of the treatment and control groups' "correctness" has been standardized by the respective group's actual degree of variability in school conditions within the two sectors.

Our analytic method is both simple and powerful. The impact of the school choice opportunity on the accuracy of parent information is simply the difference in the means between the treatment and control group regarding the "distance from the correct answer" measures. Because the treatment (i.e., offered a voucher) and control (not offered a voucher) groups were randomized at baseline, any group-level differences observed post-randomization can be ascribed with known confidence to the treatment intervention. There is no need to infer causality when conducting successful experimental evaluations—one simply observes outcome differences or does not. If differences are observed, the treatment is the cause. One also need not control for any other variables in the analysis, because the randomization of large populations of participants approximately equalizes the treatment and control group on all measured and unmeasured characteristics.[14] As Boruch, De Moya, and Snyder (2002) have argued, because of these characteristics of RFTs, "they provide the best possible evidence of relative program effects" (p. 51).

Although it is not necessary to estimate regression models to determine the impacts of an experimental treatment such as we have here, we do so for this analysis merely as a convenience. This analysis is structured as an "intention-to-treat" (ITT) assessment, because not all the treatment students offered a voucher actually used it to attend a private school, and some

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members of the control group attended private schools without a voucher (Howell & Peterson, 2002).[15] Because treatment nonusers and control group members who "cross over" to the treatment-like condition are likely to be atypical members of their randomized groups, structuring the analysis as an ITT preserves the purely experimental structure of the evaluation.[16] Because the treatment presumably had no impact on the members of the treatment group who did not use it, but the outcomes for treatment nonusers are included in the treatment-wide averages, the ITT approach generates a conservative estimate of what the voucher impact would be if all members of the treatment group had used their vouchers.[17]

Results

The analysis proceeded as a set of eight separate estimations of the "distance from the correct answer" dependent variables, as there were two different outcome variables (school size and class size) operationalized two different ways—unstandardized and standardized—over 2 years (Table 2). The single explanatory variable was coded 1 for the treatment group and 0 for the control group. The longitudinal dimension to the data is crucial for testing our second hypothesis regarding treatment-induced learning over time.

The results of the regression analysis make a strong case for the notion that school choice leads to higher levels of accurate parental information about schools. In all eight of the models, being in the treatment group had a negative and statistically significant relationship to the dependent variable, which measured how far off parents' estimates were from the school-supplied data. For all the models, the impact was statistically significant at or beyond the 95% confidence level. In six of the eight models, the coefficient on the treatment variable was statistically significant at or beyond the 99% confidence level. Additionally, the results are not sensitive to whether or not the dependent variable was standardized. Hypothesis 1, that the opportunity to exercise school choice would produce higher levels of accurate parental information about schools, is confirmed by these experimental data.

Because the results of ordered logit regression are difficult to interpret and compare across the different models, we also present the results in terms of predicted probabilities (Table 3). In substantive terms, being offered a voucher produced parental estimates of school size that were, on average, 12% more likely to match the correct school-reported sizes in 1999 and 13% more likely to be correct in 2000. The voucher offer generated parental class size estimates that were 8% more likely to match the school-reported figures in 1999 and 12% more likely to match in 2000.

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Table 2. Ordered Logit Regression-Based Estimates of Treatment Impact on School Size and Class Size Accuracy, 1999 and 2000

Dependent Variable: Distance From Actual	Treatment Effect: Ordered Logit	*p* Value	Treatment Effect: Standardized Ordered-Logit	*p* Value	*N*
School size, 1999	−.51* (.23)	.02	−0.99** (.24)	.00	279
School size, 2000	−.54** (.19)	.00	−1.10** (.21)	.00	435
Class size, 1999	−.47* (.21)	.03	−1.43** (.23)	.00	338
Class size, 2000	−.57** (.18)	.00	−1.34** (.20)	.00	462

*$p < .05$, two-tailed test. **$p < .01$, two-tailed test.
Note: Standard errors are in parentheses.

Table 3. Probability of Answering Correctly (Y = 0)

Parent Estimates:	Treatment Group (Standard Error)	Control Group (Standard Error)	First Difference (Standard Error)
School size, 1999	48% (.04)	36% (.04)	12% (.05)
School size, 2000	53% (.03)	40% (.04)	13% (.05)
Class size, 1999	28% (.03)	19% (.03)	8% (.03)
Class size, 2000	29% (.03)	19% (.02)	10% (.03)

Note: Probabilities for predicted values obtained through Stata's "Clarify" procedure.

As is suggested by the pattern of results across the two analysis years, the treatment benefit of improving the accuracy of parent information about schools appears to increase slightly over time. Parents who are offered school choice initially possess more accurate information about their child's school than do control group parents, and their accuracy advantage appears to grow at least somewhat over time as they extend their experience with school choice. Still, the small increase in the treatment impact over time is not statistically significant. Hypothesis 2, that the positive impact of school choice on parental information about schools will increase over time, is not confirmed conclusively by these data.

Discussion

These analytic results from a school voucher experiment provide a limited but important contribution to our understanding of what happens when public policies extend decision-making responsibilities to populations of citizens

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that previously lacked them, especially in the area of school choice. In contrast to the somewhat inconsistent pattern of results that Schneider et al. (2000) reported, based on their matched sample of districts with and without public school choice, the results from this RFT of a voucher-type intervention indicate that parental reports of objective school characteristics are more accurate if the parent had been given the option of choosing their child's school. Because accurate information is important for citizens to make effective choices, these results are generally consistent with the claims made by school choice supporters that low-income inner-city parents can and will become informed educational consumers. That, alone, is a significant finding.

This study does, however, have important limitations. For one, the scholarships only partially covered tuition expenses. Parents had "skin in the game" because of their own tuition contributions, and this may have provided them with an added incentive to make an informed choice—an information-seeking incentive that might be lessened in full–tuition voucher programs such as the Milwaukee Parental Choice Program. Moreover, the population of parents we examined is different from the general population in at least two important ways. First, the self-selected members of both the treatment and control group had a desire to participate in a school choice program; as a result there is reason to doubt whether or not our results would be as strong if a school choice program was made universally available to parents who were not actively seeking options. At the same time, the parents in our sample are highly disadvantaged in ways that makes them atypical of the average American citizen. The fact that we find positive results among these disadvantaged citizens, who typically maintain low levels of civic and consumer information, suggests that more expansive school choice policies could have even stronger informational effects than the ones we observe in this targeted experiment.

Although it is an important finding that parents given choice are more informed about school and class size, this study does not address other important questions such as knowledge of school quality or academic performance. Also, although we have shown that increasing school choices does lead to higher levels of accurate information, questions remain about what level of information is sufficient for a consumer-driven school marketplace to operate effectively. Some scholars reason that a modest number of well-informed "marginal consumers" are sufficient to signal to less informed parents which are the more desirable schools (Teske et al., 1993). The decisions and actions of these "market mavens" therefore pressure all schools to improve (Buckley & Schneider, 2007). Other analysts (e.g., Smith & Meier, 1995; Wells, 1996) are less sanguine about the willingness and ability of an adequate number of

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disadvantaged parents to garner the information needed to avoid being scammed in the education marketplace. Our evidence here cannot resolve that debate, except to repeat the clear and simple finding that parents offered school choices know more about the conditions of their child's school as a direct result of being given the opportunity to choose it.

This study also does not identify the specific mechanisms whereby the offer of a voucher produced better informed parents. It is reasonable to suggest, as theorists in both the school choice (e.g., Chubb & Moe, 1990) and citizenship (e.g., Warren, 1992) fields have claimed, that the responsibility attached to the act of choosing schools motivates parents to actively seek more and better information about schools. Recent focus group studies of parents in the Opportunity Scholarship Program report that parents certainly say that they actively seek such information when suddenly presented the responsibility to choose their child's school (Stewart, Wolf, & Cornman, 2007; Stewart, Wolf, Cornman, McKenzie-Thompson, & Butcher, 2009). Alternatively, it could be that public and private schools of choice initiate a process of more effectively advertising the condition of their school to existing and potential customers, leading parental choosers to be better informed about schools (Howell & Peterson, 2002).[18] Our lack of strong evidence in support of our second hypothesis, that parents' information levels will continue to grow over time, suggests that information gains are more demand-driven at the point of decision and less a result of suppliers routinely providing information. Because participating private schools were not told who won the WSF lottery, the first contact between WSF parents seeking school information and private schools seeking students would have been initiated by the parents. Whether initiated by producers or consumers, the incentives surrounding school choice policies appear to have the practical effect of leaving parents with more accurate information about a critical institution in their lives—their child's school.

These results have important implications for policy design, both beyond and within school choice. When government services incorporate mechanisms that give recipients a way to actively participate in the process, citizens have a greater incentive to increase their knowledge on important issues. And, although we are unable to directly test any potentially positive spillover effects in this case, there is good reason to believe that the benefits experienced by these parents are not confined solely to their experiences with their child's schooling. Other important research by Soss (1999) has found that when citizens receive public services, the way in which those services are delivered is perceived as a microcosm of government itself. As a result, their experiences with government services have consequences regarding their

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attitudes toward participation in other veins of citizenship. Soss finds that citizens' experiences with programs that are designed to be more participatory have transformative effects on participants and their attitudes toward political participation broadly speaking, whereas programs that are less participatory can have a negative effect on an individual's overall desire to participate. Our findings coincide nicely with those of Soss, and both studies support the pluralist–democratic school of thought over the elitist view. When adequately incentivized, citizens appear to be willing to put forth the efforts required to become more informed. The burden of information gathering required by school choice may be sour medicine that busy, underresourced parents reluctantly swallow. Still, the empowering effects of that medicine could cure some of the ills of our republic.

Appendix

Robustness Check Using Covariates in Ordered Logit

	School Size		Class Size	
Variable	1999	2000	1999	2000
Treatment	−.54* (.23)	−.54* (.21)	−.48* (.22)	−.80** (.21)
Family size	.07 (.06)	.11 (.06)	−.09 (.06)	.01 (.05)
Mother's employment	.25 (.25)	.47* (.23)	−.49* (.23)	.12 (.21)
Mother's education	−.01 (.07)	−.15* (.07)	.11 (.06)	.08 (.05)
Family income	.00 (.00)	−.00 (.00)	.00 (.00)	.00 (.00)
Constant: Cut1	−.00 (.55)	−.88 (.47)	−1.28** (.45)	−1.00* (.43)
Constant: Cut2	1.45** (.56)	.98* (.48)	.49 (.45)	.86* (.42)
Constant: Cut3	2.86** (.62)	2.98** (.63)	2.19** (.46)	2.14** (.44)

Note: Some missing data created through multiple imputation techniques.
*$p < .05$, two-tailed test. **$p < .01$, two-tailed test.

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Notes

1. This research received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors.
2. To our knowledge, the closest area of research to our own that explores the notion that the presence of more choices will lead citizens to gather more information can be found in a handful of studies that have found that greater use of state ballot initiatives can result in higher political information levels among citizens (see, e.g., Smith, 2002; Smith & Tolbert, 2004).
3. It should be noted that Barber (1984) has some reservations concerning the individualistic approach toward citizen motivation.
4. For an alternative rebuttal of Wells's (1996) argument see Teske, Schneider, Mintrom, and Best (1993) who suggest that an effective market for public goods can be driven by a "subset of informed consumers who shop around between alternate suppliers and produce pressure for competitive outcomes from which all consumers benefit" (p. 702).
5. The OSP, in contrast, is government funded and provides more generous vouchers (up to $7,500) to a more disadvantaged population (at or below 185% of poverty).
6. Although about 350 private school students also applied for the program and nearly 200 of them won the scholarship lottery, the evaluation team chose not to include these students in the evaluation, because they were seeking a voucher to remain in private school and not to switch to private school. That way, the treatment group remained a set of new school choosers; whereas the control group represented a set of families that sought to be new school choosers but were denied that opportunity by the lottery.
7. The treatment and control groups were compared on a variety of educationally relevant characteristics measured at baseline, including the following: Parents' education levels, parents' employment status, parents' religious affiliation, parents' ethnicity, parents' marital status, family receipt of various government assistance programs (food stamps, Welfare, Social Security, Medicaid, HUD housing vouchers, and Earned Income Tax Credits), and students' grade level. None of these differences were statistically significant.
8. Families received $50 as compensation for their participation in the data collection phase. Members of the control group also were offered entry into a "second chance" lottery if they complied with data collection.

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9. Because 125 members of the control group had won the turnout incentive lottery in the 1st retest year of 1999, and were therefore excluded from the study in the 2nd year, a total of 1,457 students remained in the study population.
10. Response categories for class size were as follows: *1 to 10 students*, *11 to 15 students*, *16 to 20 students*, *21 to 25 students*, *26 to 30 students*, *31 to 35 students*, *36 to 40 students*, and *41 or more students*. Response categories for school size were as follows: *1 to 150 students*, *151 to 300 students*, *301 to 450 students*, *451 to 600 students*, and *601 or more students*. Descriptively speaking, both the treatment group and the control group were more prone to overestimate class size and underestimate school size.
11. In a separate analysis, we "reversed" the described process and created continuous data for the parents by assigning them the midpoint value of the ordinal category they chose in the survey. We then took the absolute value of this parent approximation subtracted from the continuous school data to create a dependent variable that could be analyzed with linear regression methods. Our results were entirely consistent regardless of the approach used.
12. Preserving the original values of "missed by 4 or more" observations would have resulted in those aberrant cases exerting inordinate influence over the results. If they are retained, the results of the analysis do not change significantly, though the standard errors of the estimates grow somewhat because of the increased noisiness of the data.
13. We also ran the regressions with White-corrected standard errors to control for heteroscedasticity. Our results were unaffected.
14. We also ran the analysis with control variables for family size, mother's education, employment status, and household income. The inclusion of these covariates did not alter our results. For a breakdown of this analysis, see the appendix.
15. The treatment usage rate was 68% for 1 year and 47% for 2 years. The control rate of "crossover" to private schooling was 11% for 1 year and 8% for 2 years (Howell & Peterson, 2002).
16. All students were matched up with the characteristics of their actual schools—public or private—regardless of whether they were in the treatment or control group. Thus, some of the treatment observations in the analysis are of parents estimating the conditions of their child's public school and some of the control observations are of parents estimating the conditions of their child's private school.
17. The outcomes from the control group "crossovers" to private schooling also are averaged into the control group means. Doing so preserves the ideal counterfactual that is the control group, because presumably these students would have gone to private schools even without the treatment voucher, which in fact they did.
18. For example, we conducted a limited exploratory analysis on our data that looked at whether or not enrollment at a Catholic school was related to more accuracy

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on our measure of parental knowledge. Catholic schools have a reputation for actively engaging parents (see, e.g., Bryk, Lee, & Holland, 1993) and half of the students in our treatment group attended Catholic schools. We found that parents whose children attended Catholic schools were, on average, better informed than other parents on our measure of school size accuracy, but there was no significant relationship between Catholic schooling and our measure of class size accuracy. Such findings, however, are nonexperimental and cannot be distinguished from selection effects. Additional statistical tests allow us to conclude that our findings are not driven by the effect of Catholic schools.

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