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

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Abstract

Political theorists have long argued that the average citizen's lack of information and lack of clear policy preferences provide the rationale for public policy to be guided by experts and elites. Others counter that it is precisely the practice of deference to elites that perpetuates and even exacerbates the problem of apathetic and uninformed citizens. According to them, requiring citizens to take responsibility for political decisions and procedures motivates them to obtain the information and training necessary to become effective citizens.

Here we look at school choice programs as an environment poised to provide insight into this debate. Theories of school choice suggest that parents need to and can make informed decisions that will tend to situate their students in appropriate schools. School choice, in a sense, brings elements of participatory democracy into the world of compulsory education, and thus brings the same potential benefits and problems that have long challenged democratic theorists. Increasing choices to parents may give them an incentive to raise their information levels about the schools their children attend. Akin to the information gathering of consumers in a marketplace, choice parents should have more reasons to gather more information about their schools than parents without options. Alternatively, a lack of any increase in information levels amongst school choosers would suggest that despite the increased incentives to gather information, having choices per se is not sufficient to overcome the costs of information gathering.

To test whether the availability of school choice increases parent information about schools, we analyze data from the second year experimental evaluation of the Washington Scholarship Fund, a privately-funded partial-tuition voucher program. We find that presenting parents with choices does lead to higher levels of accurate school-based information on measures of important school characteristics. Specifically, parents in the school choice treatment group provided responses that more closely matched the school-reported data about school size and class size than did parents of control group members.

I. THEORY

A. 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 and McPhee 1954; Campbell et al 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 and Keeter 1996). Factual policy-specific information is especially important for citizens to realize policy-specific preferences (Schneider et al, 1998; Gilens 2001; Kuklinski et al 1982). Our analysis here is a twist on this line of research. While 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.

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; Guttman 1987). Mark Warren (1992) captures this argument best, writing:

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

From the perspective of these political theorists and analysts, citizen information is a precious good that is enhanced by community participation that itself is motivated by a feeling of responsibility.

B. Information and School Choice

From a policy perspective, citizen/customer information is central to theoretical arguments about the application of market mechanisms to education. A major claim of supporters of school choice policies is that choice enables parents to place their children in schools that are a good "fit" for their particular needs (Wolf 2005, p. 167; Godwin and Kemerer 2002, p. 20; Chubb and Moe 1990; Coons and Sugarman 1978). 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 choice 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 and Meier 1995, p. 126). A consumer is not likely to be satisfied with a purchase if they don't 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).

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 upon data from a three-city experimental evaluation of voucher-like programs in New York City; Dayton, Ohio; and Washington, DC; Howell and Peterson (2002, p. 106) 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, Mark Schneider and his colleagues (1997, p. 1202) 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.

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 choosing it? Schneider and his colleagues 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 (Schneider, Teske and Marschall 2000, Chapter 7). 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 results with measures of the actual characteristics obtained from the schools and calculated the percent 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 non-choice 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 concluded that the responsibilities associated with parental school choice increased social capital (Schneider et al 1997; Schneider and Teske 1997).

This pioneering research by Schneider and his colleagues 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 non-choice 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, since school choice is a politically controversial policy reform, a wealth of compelling evidence will be necessary to convince the skeptics that parents actually become better informed about education as a direct result of being offered educational choices.

This paper draws upon data from an experimental evaluation of school choice in Washington, DC to determine whether low-income parents who are exercising choice are better informed about key characteristics of their child's school than are their non-choice peers.

Parental responses to survey questions about school size and class size are correlated 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 correlated 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 non-choosers, that result would call into question a major component of market theory as applied to education.

II. HYPOTHESES, DATA AND METHODS

A. 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 H_0 null hypothesis. Thus:

> H_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.*

Since our data are longitudinal and we are Bayesians at heart, we also test a second hypothesis:

> H_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.

B. Data

The core of the data that we use in the study comes from the second year evaluation of the Washington Scholarship Fund (WSF) privately-funded voucher program (Wolf, Peterson and West 2001), now called the Signature Scholarship Program (SSP). This voucher-type program pre-dates and is distinct from the federally-funded Opportunity Scholarship Program (OSP) that also is administered by the WSF under a contract with the U.S. Department of Education (Wolf et al 2005; 2006). 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.[1] 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 1998-2001. WSF has been awarding Signature Scholarships to D.C. students since 1993. In 2001, the final year of the 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 Signature Scholarship program in 1998. Since demand greatly exceeded even the supply of 1,000 new scholarships, 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 via a Randomized Field Trial (RFT). In the spring of 1998 the families of 1,582 students in grades 1-7 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

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[1] 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 percent of poverty).

study. 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 (Wolf, Howell and Peterson 2000).

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 a response rate of 63 percent. Two years after the voucher offer, in the spring of 2000, the remaining members of the treatment and control groups were invited to data collection sessions in which their children were tested and the parents and older students were surveyed about their educational experiences. Since 125 members of the control group had won the turnout incentive lottery in the first re-test year of 1999, a total of 1,457 students remained in the study population. The second-year turnout of 730 students comprised 50% of that population, for both the treatment and control groups. The respondent data are weighted to re-balance each year's outcome sample to reflect the baseline characteristics of participants (Howell and Peterson 2002, pp. 209-216).

The second-year SSP evaluation data were generated via 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 were about school size and class size (Wolf, Peterson and West 2001). Since 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 non-parochial 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 over 80% of the private schools in the sample that we then matched to the observations of students in our database who had attended those schools.

C. Methodology

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:

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

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

In the parent survey the answers to both questions were recorded using ordinal categories. For the school size question, parents chose from a range of 5 ordered categorical ranges and a "Don't know option. The class size question incorporated 8 categorical ranges and also included a "Don't Know" option. Descriptive statistics for the actual school measures are summarized in Table 1.

Table 1: Treatment and Control Comparison: School Data				
	Treatment Group		Control Group	
Characteristic	**Mean**	**Standard Deviation**	**Mean**	**Standard Deviation**
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

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 it 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.[2]

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, Teske, and Marschall, 2000, p. 152). This newly constructed measure of differences had a range of 0-4 for the measure of school size and a range of 0-6 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

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[2] While this approach is less prone to bias, it has the effect of reducing the range of our dependent variable and thus in some ways discards important information in the data received about the schools. 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 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.

additional value on this scale represents missing the "correct" category by an additional unit. Since 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 0-3.

Next, we incorporated an option into our design to potentially screen out any programmatic impacts that may have been a product of the different ranges of 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 either for parents of students in the more standardized sector to guess correctly even if they didn't possess more accurate information about the school. Schneider and his colleagues 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 in order to provide a biased but "strong" test of their hypothesis (Schneider, Teske and Marschall 2000, p. 161).

In our case, the dispersion of the data is larger on the public school side of the comparison for some measures and years, but smaller for others. 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 because the measure of the treatment and control groups' "correctness" has been standardized by the respective group's actual degree of variability in the 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, since the randomization of large populations of participants approximately equalizes the treatment and control group on all measured and unmeasured characteristics.[3] 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 in order 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, since not all of the treatment students offered a voucher actually used it to attend a private school, and some members of the control group attended private schools without a voucher (Howell et al 2002).[4] Because treatment non-users and control group members who "cross over" to the treatment-like condition are likely to be atypical members of their randomized groups,

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[3] For our own curiosity and in some ways to satisfy the expected concerns about potentially unknown selection effects, we also ran the analysis while controlling 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.

[4] The treatment usage rate was 68 percent for one year and 47 percent for two years. The control rate of "crossover" to private schooling was 11 percent for one year and 8 percent for two years (Howell and Peterson 2002, p. 44).

structuring the analysis as an ITT preserves the purely experimental structure of the evaluation.[5]

Since the treatment presumably had no impact on the members of the treatment group who did

not use it, but the outcomes for treatment non-users are included into 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.[6]

III. RESULTS

The analysis proceeded as a set of 8 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 two

years. The single independent 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 for the eight regression estimates

are summarized in Table 2.

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 8 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 8 of the models, the impact was significant at or beyond the 95% confidence level,

and in 6 of the 8 models the coefficient on the treatment variable was statistically significant at

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[5] 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.

[6] 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, since presumably these students would
have gone to private schools even without the treatment voucher, since in fact they did just that.

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 resoundingly confirmed by these experimental data.

Dependent Variable: Distance from Actual	Treatment Effect-Ordered Logit	p-value	Treatment Effect-Standardized Ordered-Logit Regression	p-value	N
Table 2: Ordered Logit Regression-Based Estimates of Treatment Impact on School Size and Class Size Accuracy, 1999 and 2000					
School Size 1999	-.51** (.23)	.02	-.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

***   Statistically significant at .01
**   Statistically significant at .05
*   Statistically significant at .10
Two-tailed test conducted

Because the results of ordered logit regression are difficult to interpret and to compare across the different models, we also present the results in terms of predicted probabilities, shown in Table 3.

Table 3: Probability of Answering Correctly (Y=0)				
Parent Estimates:	Treatment Group (Standard Error)	Control Group (Standard Error)	First Difference (Standard Error)	95% Confidence Interval
School Size 1999	48% (.04)	36% (.04)	12% (.05)	.01, .23
School Size 2000	53% (.03)	40% (.04)	13% (.05)	.03, .21
Class Size 1999	28% (.03)	19% (.03)	8% (.03)	.01, .15
Class Size 2000	29% (.03)	19% (.02)	10% (.03)	.04, .16

Note: Probabilities for predicted values obtained through Stata's "Clarify" software simulation package.

In substantive terms (see the third results column of Table 3), 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. 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 the data.

IV. DISCUSSION

These analytic results from a school voucher experiment provide a limited but important contribution to our understanding of what happens when public policies provide school choice opportunities and responsibilities to populations of parents that previously lacked them. In contrast to the somewhat inconsistent pattern of results that Schneider and his colleagues reported based on their matched sample of districts with and without public school choice, the results from this Randomized Field Trial of a voucher-type intervention indicate unequivocally that parental reports of objective school characteristics are more accurate if the parent had been given the option of choosing their child's school. Since accurate information is important for consumers 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. While 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. Schneider and his colleagues (1998) reason that a modest number of well-informed "marginal consumers" are sufficient to signal to less-informed parents which schools are more desirable. The decisions and actions of marginal choosers therefore pressure all schools to improve. Other analysts (e.g. Smith and Meier 1995; Wells 1996) are less sanguine about the willingness and ability of an adequate number of disadvantaged parents to garner the information

needed to avoid being taken advantage of 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.

This study also does not identify the specific mechanisms whereby the offer of a voucher produced better-informed parents. It is possible, as theorists in both the school choice (e.g. Chubb and 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. Alternatively, is 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 and Peterson 2002, p. 106). 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. As Warren (1992, p. 8) writes:

> These transformations would improve the workings of higher-level representative institutions, as well as mitigate—if not remove—the threats democracy is held to pose to rights, pluralism, and governability.

Ironically, such citizen empowerment advocated so forcefully by democratic theorists could be advanced by providing more parents with educational options beyond their assigned government-run public school.

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Appendix Table 1: Robustness Check Using Covariates in Ordered Logit

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

***   Statistically significant at .01
**   Statistically significant at .05
*   Statistically significant at .10
Two-tailed test conducted. Some missing data created through multiple imputation techniques.