

**NONINSTRUCTIONAL SPENDING  
IMPROVES NONCOGNITIVE  
OUTCOMES: DISCONTINUITY  
EVIDENCE FROM A UNIQUE  
ELEMENTARY SCHOOL  
COUNSELOR FINANCING SYSTEM**

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**Abstract**

Children's noncognitive skills, mental health, and behavior are important predictors of future earnings and educational attainment. Their behavior in the classroom also affects their peers' behavior and achievement. There is limited prior evidence, however, concerning the impact of school resources on student behavior. Some elementary schools employ counselors whose primary purpose is to help improve students' behavior, mental health, and noncognitive skill acquisition. This article estimates regression discontinuity models exploiting Alabama's unique financing system for school counselors. Alabama fully subsidizes counselor appointments for all elementary schools, with the number of appointments based on schools' prior year enrollments using discrete enrollment cutoffs. The results suggest that greater counselor subsidies reduce the frequency of disciplinary incidents but do not strongly influence mean student achievement test scores. Increases in counselors moderate relatively severe behavioral problems without necessarily improving systemic behavior affecting classroom learning.

## 1. INTRODUCTION

Young children's noncognitive traits, behavior, and mental health are extremely important, as they are closely linked to their chance of future success. Ratings of children's self-control and self-esteem are strong predictors of academic success as well as future earnings conditional on schooling (Heckman, Stixrud, and Urzua 2006). Young children's behavioral disorders have strong negative effects on their test scores (Currie and Stabile 2006, 2007) and are related to future cases of juvenile delinquency (Nagin and Tremblay 1999). Disruptive students also negatively influence peers' behavior and learning (Figlio 2007; Aizer 2008; Carrell and Hoekstra 2008; Neidell and Waldfogel 2010). While internalized disorders (e.g., anxiety, depression) do not necessarily have a direct negative influence on peer achievement (Neidell and Waldfogel 2010), these disorders can be very costly because they substantially increase the probability that a student will repeat a grade (Currie and Stabile 2007). Researchers have attributed noncognitive skill differentials as a source of the pay gap between general educational development (GED) recipients and traditional high school graduates (Heckman and Rubinstein 2001; Heckman, Stixrud, and Urzua 2006) and the gap in college enrollment between men and women (Jacob 2002). Differences in noncognitive traits may also substantially contribute to socioeconomic gaps in school achievement and in future earnings.<sup>1</sup>

In light of the potentially enormous social returns to investments improving children's noncognitive traits, behavior, and mental health, researchers have become increasingly interested in learning which educational interventions influence these outcomes. Changes in noncognitive traits and behavior have been cited as potentially important effects from various schooling options, including charter schools (Imbermann 2007), public schools of choice (Cullen, Jacob, and Levitt 2006), private schools (Figlio and Ludwig 2000), and prekindergarten schools (Neidell and Waldfogel 2010).<sup>2</sup> Relatively little is known, however, about how traditional public schools can improve students'

1. Examining data from the 1958 and 1970 British birth cohorts, Blanden, Gregg, and Macmillan (2006) find that a growing return to noncognitive traits can partially explain rising income inequality in Britain.
2. Imbermann (2007) analyzes data from a large urban school district and finds that students shifting from traditional public schools to newly created charter schools increase their attendance and incur fewer disciplinary infractions. Cullen, Jacob, and Levitt (2006) find that students winning lotteries allowing them to attend Chicago public high schools of their choice report lower rates of disciplinary incidents and are arrested less frequently than losers of these lotteries. Figlio and Ludwig (2000) find that most behavioral differences between public and private school children are related to differences in personal and family characteristics, though, controlling for these characteristics, religious private school students are less likely to be sexually active but are less likely to use birth control when they are sexually active. Neidell and Waldfogel (2010) find that preschool attendance is associated with worse noncognitive outcomes during elementary school, such as externalized problem behaviors; even so, they find that, for both individual elementary students and their peers, the cognitive benefits of preschool attendance outweigh the negative effects associated with more frequent externalized problem behaviors.

mental health and behavior. Lazear (2001) theorizes that small class sizes may be particularly important because they reduce the frequency of episodes of disruptive student behavior, and Dee and West (2008) find evidence that smaller eighth-grade class sizes improve students' engagement in the subject. Carrell and Carrell (2006) find that intertemporal variation in the number of counselors per student in elementary schools influenced rates of disciplinary infractions in a Florida school district. Aside from these studies, there is remarkably little empirical evidence concerning the causal influence of educational policies on children's noncognitive development and behavior. It may be particularly important to better understand the impact of resources and policies specifically designed to address students' behavior and mental health.

There are more than forty thousand elementary school counselors employed in the United States, trained mental health professionals who are employed directly by public school districts and typically possess certification and graduate degrees in psychology, school psychology, or school counseling (Reback 2009). Unlike high school counselors (who spend much of their time advising students on their course selection, college applications, and career choices), elementary counselors spend the vast majority of their time dealing with students' behavioral and mental health issues, usually interacting with students either one on one or in small groups (Adelman and Taylor 2003). Unlike special education psychologists, elementary school counselors serve the general student population. A recent national study finds that there is enormous variation in elementary schools' employment of counselors and that state-level policies strongly influence the provision of counseling services (Reback 2009).

This article employs a regression discontinuity approach to investigate the effects of elementary school counselor subsidies on students' test scores and behavior. While past studies have used a regression discontinuity approach to examine the importance of class size in schools (e.g., Angrist and Lavy 1999; Hoxby 2000; Urquiola 2006),<sup>3</sup> this is the first regression discontinuity study to examine the importance of student-to-staff ratios for noninstructional staff. The analyses below exploit Alabama's unique statewide policy of subsidizing elementary counselors based on discrete enrollment cutoffs. As of 2005, Alabama was one of only four states in the continental United States to directly subsidize elementary school counselors and the only state to base these

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3. Researchers have also employed regression discontinuity analyses to examine other topics related to education. Some examples include Jacob and Lefgren's (2004) study of the impact of summer school and grade repetition, Ludwig and Miller's (2007) study of the effects of the Head Start program, van der Klaauw's (2002) and Kane's (2003) studies of the impact of financial aid on college enrollment, and several studies examining the importance of primary school entry age (see Black, Devereaux, and Salvanes 2008, McEwan and Shapiro 2008, and Elder and Lubotsky 2009 for recent examples and discussions of other entry age studies).

subsidies on discrete student enrollment ranges.<sup>4</sup> For example, Alabama elementary schools automatically receive state funding for a half-time counselor if their prior year enrollments were below 500 but receive funding for a full-time counselor if enrollments were between 500 and 749. This enrollment cutoff is not used for any other subsidy, and variation in school enrollments does not appear to be influenced by the cutoffs for counselor subsidies; observations just above or below the cutoffs are similar along observable dimensions, and schools are no more likely to be just above a cutoff than to be just below. Using panel data for every Alabama elementary school, the analyses explore whether student outcomes are influenced by the resulting arbitrary variation in the number of subsidized counselors at the school. The results suggest that additional counselor subsidies reduce the likelihood of disciplinary incidents, such as weapon-related incidents and student suspensions, but do not strongly affect mean student test scores. Increases in counselors moderate relatively severe behavioral problems without necessarily improving systemic behavior affecting classroom learning.

The next section reviews the literature related to children's noncognitive skills and mental health needs, their impact on classmates' performance, and schools' interventions targeting these areas. Section 3 provides background information on Alabama's school counseling policies, section 4 describes the school-level Alabama panel data set, section 5 describes the empirical models, section 6 presents the results, and section 7 concludes.

## 2. RELATED LITERATURE

Researchers have estimated that as many as 20 percent of young children “have mental disorders with at least mild functional impairment” (USDHHS 1999) but that 80 percent of children needing mental health services fail to receive them (Kataoka, Zhang, and Wells 2002). This potential underprovision of mental health services is not simply due to the lack of universal private health insurance; young children and adolescents who lack private health insurance are just as likely to receive mental health services as those with private insurance (Glied et al. 1998). Children's mental health problems and noncognitive traits can have a profound impact on short-run and long-run outcomes. Examining data from the National Longitudinal Survey of Youth–1979, Heckman, Stixrud, and Urzua (2006) find that noncognitive skills are important predictors of both future educational attainment and future earnings. Controlling for educational attainment, they find that noncognitive skill measures are even better than cognitive measures for predicting the future wages of all but the

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4. The other three states were Delaware, Georgia, and Nevada (see Reback 2009).

least educated women (high school dropouts) and the most educated men (four-year college graduates). Recent studies of attention-deficit/hyperactivity disorder (ADHD) among children in the United States and Canada reveal substantial negative effects of the disorder on both test scores and educational attainment (Currie and Stabile 2006, 2007). Currie and Stabile (2007) find that untreated externalized disorders (e.g., hyperactivity) have substantial effects on test scores, while untreated internalized disorders (e.g., anxiety, depression) are associated with as much as a one percentage point increase in the probability that a student will repeat a grade. Using longitudinal data tracking boys who attended schools in low socioeconomic areas of Montreal, Canada, Nagin and Tremblay (1999) find that physical aggression and “oppositional behavior” during one’s youth are relatively strong predictors of future cases of juvenile delinquency.

Reducing disruptive behavior could also be very important if a student’s disruptive behavior substantially affects peers’ achievement and behavior. Four recent studies find compelling evidence that disruptive student behavior has substantial negative peer effects. These studies all find large negative effects on peers’ academic achievement, yet each study uses a very different underlying source of variation in student disruptions: students’ ADD (attention-deficit disorder) diagnoses, their disruptive home lives, their nomenclatural disadvantages, or their teachers’ survey responses concerning behavioral problems in the classroom. Neidell and Waldfogel (2010) find that elementary school teachers’ negative ratings of their students’ social skills and behavior, particularly externalized behavior problems, are negatively related to classmates’ academic growth in both reading and math. Examining data from a large county in Florida, Figlio (2007) identifies peer effects of problematic behavior caused by males with first names that are relatively more common among females (e.g., as in the Johnny Cash song, boys named Sue). While these boys do not have systematically different outcomes during elementary grades, boys with typically female names get into more disciplinary trouble than other boys during sixth grade. A greater representation of these types of boys in sixth-grade classes leads to lower academic performance for other students in the classroom and leads to increased disciplinary incidents among other boys. Examining data from Alachua County, Florida, public schools, Carrell and Hoekstra (2008) find that greater exposure to grade mates whose families have reported domestic violence incidents adversely affects both students’ test scores and their behavior. They find that exposure to students from these families is especially harmful to the behavior of students from lower income families, male students, and black female students. Examining a nationally representative sample of elementary students and exploiting variation in the timing of the expansion of insurance programs that help promote ADD diagnoses, Aizer (2008) finds that

greater exposure to undiagnosed students who later become diagnosed with ADD has a negative effect on students' academic performance. The majority of students diagnosed with ADD receive medications to treat their conditions, and many of them also receive counseling in clinical or nonclinical settings, so the beneficial peer effects of diagnoses might arise from medication, counseling, or integrated treatments. Given that ADD diagnoses generally improve the diagnosed student's behavior but not the quality of this student's academic work, Aizer's findings suggest that it is the disruptive behavior of the student that leads to negative peer effects for academic achievement.

Schools may be a relatively easy place to target children's noncognitive skills and mental health needs (Atkins et al. 2003; Weist, Evans, and Lever 2003), and young children's noncognitive skills may be particularly malleable (Heckman 2000). There is limited evidence, however, concerning whether school-based interventions actually improve student behavior. Empirical studies of the impact of teachers, class size, and school expenditures tend to estimate effects on test scores rather than behavioral outcomes, and there have been relatively few studies examining noninstructional resources.<sup>5</sup> There is a growing literature discussing the merits of specific types of emotional learning programs (see Zins et al. 2004), but reviews of the impact of elementary school counseling on students' academic outcomes reveal that it is difficult to make strong, generalizable conclusions (Prout and Prout 1998; Whitson and Sexton 1998). It is challenging to identify the impact of counseling on student outcomes because students in these studies are typically not randomly assigned to receive counseling. Recent studies showing connections between student test score outcomes and specific elementary counseling programs in the states of Florida (Brigman and Campbell 2003) and Washington (Sink and Stroh 2003) base their findings on comparisons of students in treated and nontreated schools, even though the intervention was not randomly assigned. Even the best studies utilizing random assignment generally suffer from other limitations such as small sample sizes, treatment via the researchers rather than counselors, potentially biased survey responses determining treatment success, short duration of the intervention (often two weeks or less), and/or

5. A few notable recent studies have identified the causal effects of public school resources on behavioral outcomes and/or examined the impact of noninstructional resources. Examining evidence from the Project STAR class size experiment and from a nationally representative data set, Dee and West (2008) find evidence that elementary school class size reductions can improve short-run measures of students' initiative, and eighth-grade class size reductions can improve students' engagement with the relevant subject matter. Using a dynamic regression discontinuity model, Cellini, Ferreira, and Rothstein (2008) find evidence that the passage of bonds funding capital construction in California school districts leads to increases in local house values without large effects on student test scores. As described below, Carrell and Carrell (2006) find a reduction in disciplinary incidents given a greater supply of counselors per pupil in elementary schools in Alachua County, Florida.

tracking outcomes only shortly after the conclusion of the program (see, e.g., Gerler, Kinney, and Anderson 1985; Larkin and Thyer 1999; Manning 1988; Russell and Roberts 1979).

Carrell and Carrell (2006) conduct one of the most rigorous nonexperimental studies of the impact of counselors and the only prior study that has specifically examined the effects of counselor-student ratios. Examining school-level variation in student-counselor ratios in a Florida school district, variation partially driven by the internship and practicum assignments of counselors in training at the University of Florida, Carrell and Carrell find that greater elementary school counselor availability reduced rates of student disciplinary problems. Exploiting within-school variation in counselor-student ratios over various semesters, they find that greater availability of counselors reduces both the share of students involved in any disciplinary incident and the likelihood of recidivism among students previously involved in a disciplinary incident, especially among black male students and students from low-income families. Carrell and Carrell's study may be viewed as complementary to the analyses below: they were able to identify student-level effects for the recurrence of major disciplinary incidents of an unspecified nature, whereas the analyses below identify school-level effects but can do so for a variety of outcomes.

In another recent study (Reback 2009), I examine how states' adoptions of policies subsidizing or requiring elementary school counselors are related to student outcomes and teachers' survey responses concerning school climate. Cross-state evidence suggests that, controlling for students' prior test scores and a host of other factors, fifth-grade students earn higher test scores and self-report lower rates of emotional/behavioral problems if their states' policies increase the provision of elementary school counseling. Triple differences estimates exploiting both the timing and the targeted grade level of counseling policy changes suggest that more aggressive statewide elementary counseling policies reduce teachers' concerns about students' behavior. Those analyses are also complementary to the analyses below, as they are based on nationally representative samples but either rely on cross-state evidence or examine changes in subjective attitudes rather than changes in concrete outcomes.

### **3. BACKGROUND ON ALABAMA'S FINANCING AND REQUIREMENTS FOR ELEMENTARY SCHOOL COUNSELORS**

The origins of Alabama's current counselor subsidy policy date back to the state's foundation aid program for "instructional support units" (principals, assistant principals, counselors, and librarians), which began in the 1988–89 school year. The state began to reimburse all public schools for the salaries

**Table 1.** Prior Year Average Daily Membership (ADM) Ranges for State Subsidies of Instructional Support Units in Alabama Elementary Schools, 2000–2007

Prior Year ADM	Principals	Assistant Principals	Counselors	Librarians
<263	0.5	0	0.5	0.5
263–439.99	1	0	0.5	1
440–499.99	1	0	0.5	1.25
500–659.99	1	0	1	1.25
660–749.99	1	0.5	1	1.5
750–879.99	1	0.5	1.5	1.5
880–999.99	1	1	1.5	1.5
1000–1099.99	1	1	2	1.5
1100–1249.99	1	1.5	2	1.5
1250–1319.99	1	1.5	2.5	1.5
1320–1499.99	1	2	2.5	1.5
≥1500	1	2	3	1.5

of enough of these staff members so that each school could satisfy the recommended staff-student ratios of the Southern Association of Colleges and Schools (SACS). The SACS requires these ratios as part of its standards for certifying schools. Throughout this article's sample period, Alabama maintained foundation funding at the SACS's recommended levels as of the year 2000, even though the SACS has more recently loosened some of these standards. Specific schools receive the staff allocations, districts cover the full cost of these positions based on a statewide salary schedule, and the state fully reimburses districts for these expenses. If a school hires a more experienced staff member, the district will receive greater funds from the state because the district will have to cover a larger salary for this staff member.

To allow schools to plan for the upcoming year, the instructional support units are awarded based on the schools' average daily membership (ADM) from the first forty days of the *prior* school year. Table 1 displays the ADM ranges for elementary schools associated with various levels of instructional support units, which translate into full-time-equivalent staff positions. If a school earns more than one unit for librarians, the school is allowed to use these additional librarian units to hire either part-time librarians or aides. For the other three types of staff positions—principals, assistant principals, and counselors—the school can only use the money toward the salaries of that particular type of staff member; if a school hires fewer counselors than its entitled counselor units, the district simply forfeits the opportunity to receive those funds.



Alabama's requirements for certified elementary school counselors have generally been similar to other states' requirements, though Alabama's requirements are slightly stricter in terms of testing and graduate course grade point average (Kaye 2001; ASCA 2006). Like almost every other state, Alabama requires a master's degree from an accredited postsecondary institution in a subject related to counseling. Like the majority of states, Alabama also requires individuals to complete an internship and a practicum before becoming certified school counselors. Unlike most other states, Alabama requires certified counselors to have earned a minimum grade point average of 3.0 for their graduate courses related to counseling and to either have passed a general knowledge teacher certification exam or be a certified public school teacher (Kaye 2001).

Many schools in Alabama and elsewhere in the country currently try to follow the American School Counselor Association (ASCA) national model for school counselors, which was adopted in 2003 (see Bowers and Hatch 2003). One notable feature of this model is that it calls on counselors to play a direct role in students' academic development and ties the role of the counselor to their schools' academic missions (Bowers and Hatch 2003). This new emphasis on academics, along with the beginning of the No Child Left Behind (NCLB) school accountability program in the 2002–3 school year, may have influenced Alabama counselors' behavior, especially in the later years of this article's sample period.

#### **4. DESCRIPTION OF THE DATA**

The Alabama regression discontinuity analyses below use a panel of school-level data from the 1999–2000 through 2005–6 school years. These data come primarily from Alabama's publicly available school report cards, which report school-level information on student enrollments, rates of student disciplinary incidents, attendance rates, the percent of students eligible for free or reduced price lunches, and student performance on various statewide exams. My research assistants manually coded these data from PDF report cards for each school in each year, and I carefully checked for typing errors by reexamining values that were either atypical in size or represented large changes for the same school from one year to the next. The 2001–2 report cards also included retrospective data on ADM and attendance rates from 1986–87 through 2001–2, so I can use a longer sample for models predicting attendance rates. I include schools in the sample if they were classified as elementary schools (serving students in third grade or lower and not serving any students above eighth grade) and were in existence throughout the sample period. Although the Alabama school report cards provide staffing information, accurate records for the number of counselors employed each year are unavailable because

schools do not consistently report part-time counselor assignments; schools frequently round up or round down to the nearest whole number.<sup>6</sup> In light of this measurement error, the regression discontinuity analyses below do not use the staffing variables from the school report cards and instead focus on schools' eligibility for subsidized staff positions. As discussed in the next section, this limits the scope of interpretation but does not in any way limit the methodological approach, especially because external data confirm that counselor subsidies strongly affect counselor provision.

The test score data include the mean third-grade and mean fourth-grade student scores on Stanford Achievement Tests in math, language arts, and reading for the 2001–2 through 2004–5 school years.<sup>7</sup> The 2003–4 and 2004–5 test score data come from electronic files downloaded from the Alabama Department of Education Web site and are not reported in the school report cards, as the report cards began to instead report data from the Alabama Reading and Mathematics Tests, exams administered under compliance with NCLB. The Alabama Reading and Mathematics Test data are not yet available for a sufficient number of years to serve as dependent variables in the analyses below. Some students did not contribute to the school mean Stanford Achievement Test scores, with participation rates ranging from about 92 percent to about 98 percent of students in the tested grades.<sup>8</sup> Section 6 below includes an analysis of how incomplete student participation in testing might bias the estimated effects of counselor subsidies on mean test scores.

I combine the panel data from school report cards with variables from the National Center for Education Statistics (NCES) annual Common Core of Data. In particular, the Common Core of Data provides school-level information on the fraction of students whose ethnicity is nonwhite, the distribution of enrollments across grades, and the school's pupil-teacher ratio, as well as district-level data concerning operating expenditures per student.

6. It is not possible to know which report cards accurately report the FTE number of counselors employed at the school. An example of inconsistent reporting is that 4 percent of elementary schools reported a non-integer number of counselors in 2001–2, 21 percent in 2002–3, 18 percent in 2003–4, and 4 percent in 2004–5, even though there was not any relevant policy change across these years and there was not a substantial change in the overall number of full-time or part-time counselors employed statewide. State officials could not think of an explanation for this intertemporal variation in school-level reporting practices.
7. Alabama switched from the ninth edition of the Stanford Achievement Test to the tenth edition after the 2003–4 school year, but the use of z-scores for test score variables below should limit the impact of this change on any of the estimates.
8. Only the 2001–2 and 2002–3 school year report cards include rates of student participation on these statewide exams. Statewide, the percentage of third-grade students tested in reading, language, and mathematics was 93.8 percent, 94.6 percent, and 95.5 percent, respectively, in 2001–2 and 97.3 percent, 97.8 percent, and 98.0 percent for 2002–3. Among fourth-grade students, these percentages were 92.3 percent, 94.9 percent, and 95.5 percent in 2001–2 and 97.9 percent, 98.2 percent, and 98.3 percent in 2002–3.

## 5. METHODOLOGY

The regression discontinuity analyses exploit the discrete prior year enrollment cutoffs for elementary school counselor subsidies displayed in table 1. If one considers the “treatment” in this analysis to be either whether schools offer additional counseling services or whether students receive counseling, then, in the language of regression discontinuity studies, the model employed here is a “fuzzy regression discontinuity” (Imbens and Lemieux 2008). This means that treatment is not a deterministic function of whether the school lies above or below the point of discontinuity, but rather that the likelihood of treatment rises sharply at the discontinuity. Given that the counselor subsidies are nonfungible, the rates of compliance for employing counselors should be very high; there should be hardly any cases in which schools employ fewer counselors than they could receive from the state at no cost to the school or district. However, there are likely to be some “always-takers”—schools that would use their own local funds to give themselves the “treatment” of an additional half-time counselor regardless of whether they fall above or below the cutoff for a subsidy. To the extent that schools compensate using local funds, the results may be muted. Even if data concerning the number of counselors employed were available in the main data set, it would be inappropriate to compare compliers in the treatment group just above the discontinuity point with schools below the discontinuity point, because this would create a selection bias of indeterminate form (Imbens and Lemieux 2008). These data would simply have been useful for precisely determining the average rates of always-taking. External data confirm that the rates of always-taking are fairly low, as the likelihood that students receive counseling services increases precipitously as schools cross cutoffs for additional subsidies.<sup>9</sup>

As long as schools just above the threshold are not systematically different from schools just below the threshold, the analyses below produce unbiased estimates of the average local treatment effects of a marginal increase in counselor subsidies. Due to the presence of some always-takers, schools that would have hired as many counselors even with a smaller state subsidy, the average

9. The NCES Early Childhood Longitudinal Survey–Kindergarten Cohort (ECLS-K) tracked a nationally representative sample of students who were typically in third grade during the 2003–4 school year, at which time the ECLS-K asked their teachers whether the students met with a professional counselor at the school. Estimates from a probit model using counseling subsidies and the running variable (prior year ADM) to predict Alabama ECLS-K students’ receipt of counseling suggest that a school’s eligibility for an additional half-time counselor subsidy increases the likelihood that a student received counseling by 13 percentage points; one may reject the null hypothesis of zero effect of this subsidy in favor of a positive effect of this subsidy at the .039 level, even after adjusting the standard errors for clustering by school. This increase, roughly a doubling of the counseling rate, represents a large but reasonably sized increase, given that the number of subsidized counselors doubles at the most frequently surrounded cutoff point (the lowest one) and increases by 50 percent at the next cutoff point. It is possible that the impact of additional counselor subsidies varies depending on the specific subsidy cutoff, but these samples are too small to reliably differentiate these effects.

local treatment effect of the subsidy may partially reflect how these always-takers spend their increased state aid on resources other than counselors. Given that districts control the distribution of funds across their schools, it is likely that the additional funds would effectively be spread across all schools in the district. It is therefore possible that these local average treatment effects fail to capture the benefits of state counselor subsidies enjoyed by students in other schools in the same district. If always-takers tend to be schools hiring additional counselors who are especially effective, it is also possible that the local average treatment effect of counselor subsidies is less than the typical effect from increasing counselors.

The counselor subsidy variable is the appropriate variable from the standpoint of state policy, whereby a state would want to know whether additional subsidies influence student outcomes. Just as one can view these estimates as the results from a fuzzy regression discontinuity analysis in which the treatment effect is either the number of counselors at the school or students' receipt of counseling services, one can alternatively view them as the results of a sharp regression discontinuity analysis in which the treatment effect is the impact of additional state counselor subsidies.

The regression sample is restricted to school-level observations with prior year ADM within a certain bandwidth of the discontinuity points for counselor subsidies. For the baseline analyses, a bandwidth of  $\pm 60$  ADM is used because this yields the largest sample for which subsidized noncounselor staff positions remain constant within each ADM range. The baseline sample pools observations surrounding three different counselor subsidy cutoffs (500, 750, and 1000 ADM),<sup>10</sup> and the model controls for unique intercepts and unique linear effects of enrollments within each of these three ranges. Define  $Range_{i,t-1}$  as a vector of three indicator variables for whether school  $i$ 's prior year ADM was within one of these three ranges (i.e., 440–560, 690–810, or 940–1060). The main regression model is thus:

$$\begin{aligned}
 Y_{i,t} = & Range_{i,t-1} * ADM_{i,t-1} \beta_1 + Range_{i,t-1} * ADM_{i,t} \beta_2 \\
 & + \beta_3 HalfTimeCounselorSub_{i,t}(ADM_{i,t-1}) \\
 & + \beta_4 \%Grades\_Kto2_{i,t} + \beta_5 \%Grades\_3or4_{i,t} + Range_{i,t-1} \beta_6 \\
 & + \beta_7 \%LowIncome_{i,t} + \beta_8 \%Nonwhite_{i,t} \\
 & + \beta_9 PupilsPerTeacher_{i,t} + \beta_{10} SpendingPerPupil_{i,t} + T_i + \varepsilon_{i,t} \quad (1)
 \end{aligned}$$

10. The analyses utilize these first three cutoffs but ignore the higher cutoffs because there are not enough observations surrounding the highest two cutoffs of 1250 and 1500 ADM.

where  $T_t$  is a vector of year effects,  $ADM_{i,t}$  is the average daily membership during the first forty school days of year  $t$ ,  $\%Grades\_Kto2_{i,t}$  is the fraction of school  $i$ 's students in kindergarten through second grade, and  $\%Grades\_3or4_{i,t}$  is the fraction in third or fourth grade during year  $t$ .  $HalfTimeCounselorSub_{i,t}$  is the number of 0.5 full-time-equivalent (FTE) subsidized counselors that school  $i$  is eligible for during year  $t$  (which increases by one as a school's prior year ADM crosses one of the discontinuity points). Although analyses below document that observations slightly above and slightly below the discrete cutoffs have very similar observable characteristics, equation 1 includes controls for four additional variables: the fraction of students in the school eligible for free or reduced price lunches, the fraction of students in the school whose ethnicity is nonwhite, the school's pupil-teacher ratio, and the district's spending per pupil. Controlling for these variables ensures that the estimates are not influenced by small, randomly determined differences in sample composition above and below the discrete enrollment cutoffs, though in practice the estimates are not very sensitive to the inclusion of these control variables. In addition to estimates of equation 1, the analyses include estimates from models using the same control variables and dividing the sample based on observations near specific cutoffs. In all of the analyses, standard errors are adjusted for clustering by school.

Additional specifications test the sensitivity of the estimates to changing this bandwidth or removing the bandwidth limitation and instead controlling for cubic terms of the running variable, prior year ADM. These additional specifications add a control variable for the number of subsidized librarians if the bandwidth is greater than  $\pm 60$  ADM, given that this would imply within-range variation in eligibility for librarian subsidies. These analyses are followed by several falsification tests, including estimates from analogous models using counterfactual cutoff points or using subsidies for librarians rather than subsidies for counselors.

Tables 2a and 2b display the means and standard deviations of the behavioral dependent variables and the independent variables for the various samples used in the regressions below. (As discussed in the previous section, the test score samples are smaller than the other samples due to the lack of test scores in the first few years of report card data.)

## 6. RESULTS

### Verifying the Exogeneity of Prior Year School-Level Enrollments

Before discussing the main results, it is important to verify that schools' prior year student enrollments were exogenously determined. With instructional support units rewarded based on schools' prior year enrollment levels, it is

**Table 2a.** Summary Statistics of the Dependent Variables for the Main Regression Samples

	<b>Indicator for Whether School Experiences at Least One:</b>				
	<b>Attendance Rate</b>	<b>Suspension</b>	<b>Expulsion</b>	<b>Drug-Related Incident</b>	<b>Weapon-Related Incident</b>
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around any of the three points of discontinuity</b>					
Mean	.968	0.46	0.020	0.080	0.38
Standard deviation	.009	0.50	0.14	0.27	0.49
N	1,305	1,305	1,228	1,305	1,305
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around first discontinuity point</b>					
Mean	.967	0.44	0.018	0.068	0.37
Standard deviation	.011	0.50	0.13	0.25	0.48
N	1,226	970	823	970	970
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around second discontinuity point</b>					
Mean	.966	0.53	0.045	0.11	0.40
Standard deviation	.012	0.50	0.21	0.32	0.49
N	323	258	223	223	258
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around third discontinuity point</b>					
Mean	.965	0.52	NA	0.19	0.49
Standard deviation	.011	0.50		0.40	0.50
N	95	69		67	77

possible that some districts might have strategically altered school-level enrollments to meet certain support unit thresholds. Widespread manipulation of enrollments is unlikely because there are multiple cutoffs for various types of instructional support units, student enrollments are generally based on fixed attendance zones, and the amount of money per student involved with the instructional support units is small relative to total state aid.<sup>11</sup> Nonetheless, it is important to explore whether even a few districts may have strategically assigned students because this could substantially bias the main regression discontinuity estimates. Dennis Heard, an Alabama state official in charge of local education agency funding and accountability, could not think of any anecdotal examples of districts strategically reassigning students across schools to gain additional counselors (personal communication, 25 October 2007). As shown below, his assertion is consistent with the data. He believes that the only way some districts have responded to this financial structure is by closing

11. School-level student enrollments seem to be endogenously determined in other policy contexts, particularly when schools are not forced to serve all students residing in particular geographic areas. For example, Urquiola and Verhoogen’s (2009) regression discontinuity analyses using data from Chilean schools show that enrollments fluctuate in response to a nationwide limit on class sizes.

**Table 2b.** Summary Statistics of the Independent Variables for the Main Regression Samples

	Test Score Sample (N = 947)		Behavioral Outcomes Sample (N = 1,305)	
	Mean	Stand. Dev.	Mean	Stand. Dev.
Lagged ADM for schools in the first lagged ADM range (440–559)	497	35	497	34
Current ADM for schools in the first lagged ADM range (440–559)	494	52	494	57
Lagged ADM for schools in the second lagged ADM range (690–809)	748	34	747	33
Current ADM for schools in the second lagged ADM range (690–809)	745	70	741	74
Lagged ADM for schools in the third lagged ADM range (940–1059)	985	38	988	38
Current ADM for schools in the third lagged ADM range (940–1059)	931	130	952	123
Percent of students at school enrolled in grades K–2	46.5	13.9	46.2	14.0
Percent of students at school enrolled in grades 3–4	32.8	8.6	32.8	8.4
Percent of students at school eligible for free or reduced price lunches	55.8	25.9	54.7	25.7
Percent of students at school whose ethnicity is nonwhite	41.7	34.3	41.1	34.2
Pupils per teacher at school	14.8	1.50	14.9	1.50
District-level spending per pupil	\$7,485	\$1,250	\$7,261	\$1,287

very small elementary schools that would not be entitled to funding for a full-time principal. Given that there might be selection biases in terms of very small elementary schools, the analyses below do not include elementary schools that were ever too small to receive state funding for a full-time principal. These small schools are not a sensible fit for the regression discontinuity analyses anyway because the first discrete cutoff for counselors (500 students) is much higher than the discrete cutoff for a full-time principal (266 students).<sup>12</sup>

The following model is used to test for selection of the counselor subsidy level at school  $i$  based on manipulation of ADM in year  $t - 1$  associated with an observable characteristic,  $X_{i,t-1}$ :

$$\begin{aligned}
 X_{i,t-1} = & \text{Range}_{i,t-1} * \text{ADM}_{i,t-1} \lambda_1 + \lambda_2 \text{HalfTimeCounselorSub}_{i,t}(\text{ADM}_{i,t-1}) \\
 & + \lambda_3 \% \text{Grades\_Kto2}_{i,t-1} + \lambda_4 \% \text{Grades\_3or4}_{i,t-1} \\
 & + \text{Range}_{i,t-1} \lambda_5 + T_t + e_{i,t-1}.
 \end{aligned}
 \tag{2}$$

12. This sample restriction eliminates only one school from the main analyses.

Four dependent variables are used for different versions of equation 2: the fraction of students at the school receiving free or reduced price lunch, the fraction of students at the school whose ethnicity is nonwhite, the school district's operating expenditures per pupil, and the school's pupil-teacher ratio. Reassuringly, the counselor subsidy variable's estimated coefficient is statistically insignificant in each of these models, with  $p$ -values greater than .2 and coefficients (and standard errors) of  $-.008$  (.034),  $-.023$  (.044),  $-186$  (172), and  $.206$  (.166), respectively. These coefficients are even smaller in magnitude if the model controls for school-level fixed effects. The coefficients of the counselor subsidy variable are also statistically insignificant if one instead estimates a model predicting the year  $t$  value of the observed characteristic,  $X_{i,t}$ , using the first six independent variables in equation 1.

To further refute the idea that districts strategically manipulate schools' ADMs based on counselor subsidy cutoffs, one can examine the density of the sample for various prior year ADM ranges. Figure A.1 in the appendix displays a histogram with schools with prior year ADM between 299.99 and 1199.99 placed into bins based on ranges that are five ADM wide. There does not appear to be any sudden clumping of observations to the right of the various cutoff points. In fact, for the first two cutoff points, there are actually fewer observations immediately to the right of the cutoff point than immediately to the left, and the declines in density at these cutoffs appear to be at least as large as the declines in density approaching the cutoffs from farther away. For the main sample used below, there are ninety-two observations within five students below a counselor subsidy cutoff and ninety observations within five students above a cutoff. For a smaller bandwidth of  $\pm 3$  students, these counts are thirty-five and thirty-four, respectively. There is not any evidence to support the idea that many schools strategically manipulate prior year ADM to obtain a greater number of counselor subsidies.

### Main Regression Discontinuity Results

Table 3 displays the estimates of the impact of counselor subsidies ( $\beta_3$  in equation 1) on various student test variables, based on the sample using the aforementioned baseline bandwidth of  $\pm 60$  ADM. The dependent variables equal school-level z-scores for the mean student test score within a specific grade on the Stanford-9 or Stanford-10 Achievement Test in math, language arts, or reading. To facilitate interpretation of effects as a change in a school's place in the statewide distribution, the dependent variables were standardized using the full set of schools offering testing in that subject in that grade, regardless of whether the schools were in the appropriate moderate bandwidth around a discontinuity point. The means and standard deviations of these dependent variables for each sample are thus close to but not exactly equal to zero and one, respectively.



**Table 3.** Regression Discontinuity Evidence Concerning the Effects of Counselor Subsidies on Standardized Mean Student Test Scores

Subject	Math		Language		Reading	
	3rd	4th	3rd	4th	3rd	4th
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around any of the points of discontinuity</b>						
Estimated effect of an additional subsidized 0.5 FTE counselor	.0003	-.006	.0004	-.044	.005	.046
(Standard error)	(.088)	(.083)	(.087)	(.087)	(.078)	(.065)
N	947	894	947	894	946 <sup>a</sup>	894
R <sup>2</sup>	.603	.621	.633	.627	.726	.760
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around first discontinuity point</b>						
Estimated effect of an additional subsidized 0.5 FTE counselor	.048	.018	.084	.015	.059	.064
(Standard error)	(.104)	(.100)	(.101)	(.106)	(.093)	(.080)
N	716	680	716	680	715 <sup>a</sup>	680
R <sup>2</sup>	.635	.635	.660	.639	.744	.770
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around second discontinuity point</b>						
Estimated effect of an additional subsidized 0.5 FTE counselor	-.091	.094	-.195	-.141	-.155	.061
(Standard error)	(.142)	(.144)	(.186)	(.153)	(.123)	(.104)
N	177	167	177	167	177	167
R <sup>2</sup>	.583	.671	.594	.655	.722	.758
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around third discontinuity point</b>						
Estimated effect of an additional subsidized 0.5 FTE counselor	.053	.017	-.012	.170	.300	.426
(Standard error)	(.343)	(.434)	(.378)	(.261)	(.366)	(.309)
N	54	47	54	47	54	47
R <sup>2</sup>	.323	.475	.431	.635	.493	.680

Notes: Estimates are based on equation 1. The dependent variables are the schools' mean student test scores standardized by subject and grade (z-scores). Standard errors are adjusted for clustering by school.

<sup>a</sup>One school, Vance Elementary, has a missing value for the mean third-grade reading score for 2003-4.

Counselor subsidies are generally not related to mean student test scores at conventional levels of statistical significance. In a couple of cases, there is actually a negative relationship between counselor subsidies and test scores, though this relationship is never statistically significant. The analyses with moderate bandwidths around the first cutoff, which has the largest sample

sizes, produce positive but small estimates, suggesting that the counselor subsidies do not have much of an effect on test scores. These estimates remain statistically insignificant if the models omit the descriptive control variables.

Interpreting school-level mean test score results is typically complicated by the endogeneity of student participation rates. School counselors in Alabama might influence student test participation rates because assisting with student testing is actually part of their job description; out of twenty-one questions on a survey form given to instructional staff to assess their schools' counselors, three questions relate to how well the counselors assist with the administration and analysis of student test score data (Alabama PEPE Program 2002).<sup>13</sup> If a policy increases rates of student test taking, this could decrease the mean student test score if the students induced to take the test have relatively low ability. If one estimates models similar to those above using these participation rates as the dependent variable, there is evidence that counselor subsidies positively affect rates of student participation in testing. Given that only two years of data are available for analyzing testing participation rates, these models use a slightly different specification in order to find point estimates with reasonably small standard errors.<sup>14</sup> I estimate these models using the full set of Alabama elementary schools for these two years that were large enough to receive a full principal subsidy (last year's ADM was at least 266) but small enough to be within one hundred students of the third highest counselor subsidy cutoff (last year's ADM did not exceed 1,100). Rather than controlling for unique linear effects of current year and prior year ADM within a moderate range around each cutoff, I control for linear effects of these terms throughout the distribution.

Counseling subsidies have positive effects on student participation rates, with small and statistically insignificant effects for third-grade participation and larger effects for fourth-grade participation. The estimated coefficients on the half-time counselor subsidy variable are 0.25 percentage points (0.49 standard error) for third-grade math test participation, 0.01 (0.54) for third-grade

13. On a scale of one to five, instructional staff were asked to rate how well a counselor does each of the following: "serves as a resource to faculty in student assessment and analysis of standardized test data"; "assists teachers and administrators in communicating and interpreting standardized test data"; and "trains teachers to administer tests, when necessary." The other eighteen questions on this survey covered topics including how well counselors assist individual students, how well counselors work with teachers to integrate counseling into the curriculum, how well counselors assist with crisis planning and management, and how well counselors promote cooperation with parents/guardians. The responses to these surveys are not publicly available.

14. With the exact same specification used to analyze student test score performance, the standard errors are more than twice as large as those for the participation rate estimates reported in the text, and all confidence intervals include both zero and large effects. The point estimates for the effect of a half-time counselor subsidy would be 0.6 percentage points on third-grade math participation rates,  $-0.4$  on third-grade language,  $-0.1$  on third-grade reading,  $0.7$  on fourth-grade math,  $0.1$  on fourth-grade language, and  $-0.7$  on fourth-grade reading.

language participation, 0.15 (0.64) for third-grade reading participation, 1.05 (0.53) for fourth-grade math participation, 0.87 (0.60) for fourth-grade language participation, and 0.77 (0.66) for fourth-grade reading participation.<sup>15</sup> The two categories with negative point estimates for achievement effects in table 3, fourth-grade math and language, have the largest estimated positive effects on test participation. It is possible that counseling subsidies have mild, positive effects on students' academic progress but that the positive effect of subsidies on participation disguises these effects. However, even a very generous upper bound estimate of the potential downward bias is less than .07 standard deviations for any school's fourth-grade math z-score, only about .04 standard deviations for the median-performing school in fourth-grade math, and even less for the other grades and subjects.<sup>16</sup> Test-taking selection bias alone thus cannot explain why the estimates in the first panel of table 3 are statistically insignificant.<sup>17</sup>

Table 4 displays estimates of  $\beta_3$  in equation 1 with students' behavioral outcomes as dependent variables, again using the sample with the  $\pm 6\sigma$  ADM bandwidth. The first panel of table 4 shows estimates from models with larger samples because the models do not control for the last four independent variables in equation 1. The sample in the first panel is thus particularly large for the first column examining attendance rates because only attendance rate and ADM data are available starting with the 1996–97 school year, thanks to retrospective data for these variables in the 2000–1 report cards. The results in column 1 of the first panel suggest that one additional half-time counselor subsidy is associated with a statistically insignificant decline in the attendance rate. This estimate remains statistically insignificant in the models with the full set of control variables. The lack of a positive effect of counselor subsidies on overall attendance rates makes the test participation rate results above more remarkable; counselor subsidies increase some test participation rates even though they do not increase attendance on typical school days.<sup>18</sup>

The remaining columns of table 4 examine the effect of counselor subsidies on the likelihood that schools experience at least one occurrence of various

15. The means (and standard deviations) for these participation rates for schools in this sample are, respectively, 96.3 percent (4.4 percent), 95.8 percent (5.1 percent), 95.1 percent (6.0 percent), 96.2 percent (4.7 percent), 95.9 percent (5.4 percent), and 94.7 percent (5.9 percent).
16. For fourth-grade math scores, a school's z-score would decline by less than .07 standard deviations if one replaced the actual z-score with the sum of the reported z-score multiplied by .9895 and the very lowest statewide score multiplied by .0105. (The score distributions for individual schools are not available.)
17. A similar conclusion is reached if one examines test performance using the same sample and model used to analyze test score participation (based on only two years of data). None of the point estimates is statistically significant, nor would they be statistically significant if they were increased by .04.
18. The estimated effect of counselor subsidies remains negative ( $-.0012$ ) and statistically insignificant ( $p$ -value of .185) if one uses the same sample and specification as used for the test participation analyses.

**Table 4.** Regression Discontinuity Evidence Concerning the Effects of Counselor Subsidies on Student Behavior

	<b>Likelihood That the School Experiences at Least One:</b>				
	<b>Attendance Rate</b>	<b>Suspension</b>	<b>Expulsion</b>	<b>Drug-Related Incident</b>	<b>Weapon-Related Incident</b>
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around any of the three points of discontinuity</b>					
<i>Models with fewer control variables</i>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0005	-.122**	.006	-.011	-.115**
(Standard error)	(.0012)	(.054)	(.011)	(.026)	(.052)
N	1,738	1,370	1,288 <sup>a</sup>	1,370	1,370
<i>Models with full set of control variables</i>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0008	-.095*	.004	-.014	-.100*
(Standard error)	(.0009)	(.053)	(.011)	(.026)	(.052)
N	1,305	1,305	1,228 <sup>a</sup>	1,305	1,305
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around first discontinuity point</b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0009	-.091	.003	-.011	-.121**
(Standard error)	(.0010)	(.061)	(.012)	(.029)	(.057)
N	970	970	823 <sup>a</sup>	970	970
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around second discontinuity point</b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0013	-.064	.029	.029	-.009
(Standard error)	(.0019)	(.120)	(.047)	(.064)	(.116)
N	258	258	223 <sup>a</sup>	223 <sup>b</sup>	258
<b>Moderate bandwidth (<math>\pm 60</math> ADM) around third discontinuity point</b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	.0048	-.602**	NA <sup>a</sup>	-.056	-.162
(Standard error)	(.0053)	(.203)		(.201)	(.303)
N	77	69 <sup>c</sup>		67 <sup>b</sup>	77

Notes: Estimates are based on equation 1, with OLS estimates displayed for the attendance rate models and mean estimated marginal effects from probit estimation displayed for the other models. The first panel of estimates above omits the final four control variables in equation 1, while all the other panels above are based on models using the full set of controls. In the first panel, the attendance rate models have larger sample sizes due to additional years of available data for that particular outcome alone. Standard errors for the mean estimated marginal effects were found using the delta method. All standard errors are adjusted for clustering by school.

<sup>a</sup>None of the schools in the third discontinuity range had any student expulsions, and none of the schools in the first discontinuity in 2003–4 or the second range in 1999–2000 had any expulsions.

<sup>b</sup>None of the schools in the second or third ranges had any drug-related incidents in the 1999–2000 school year.

<sup>c</sup>All schools in the third discontinuity range had at least one suspension during the 2000–2001 school year.

\*significant at 10%; \*\*significant at 5%

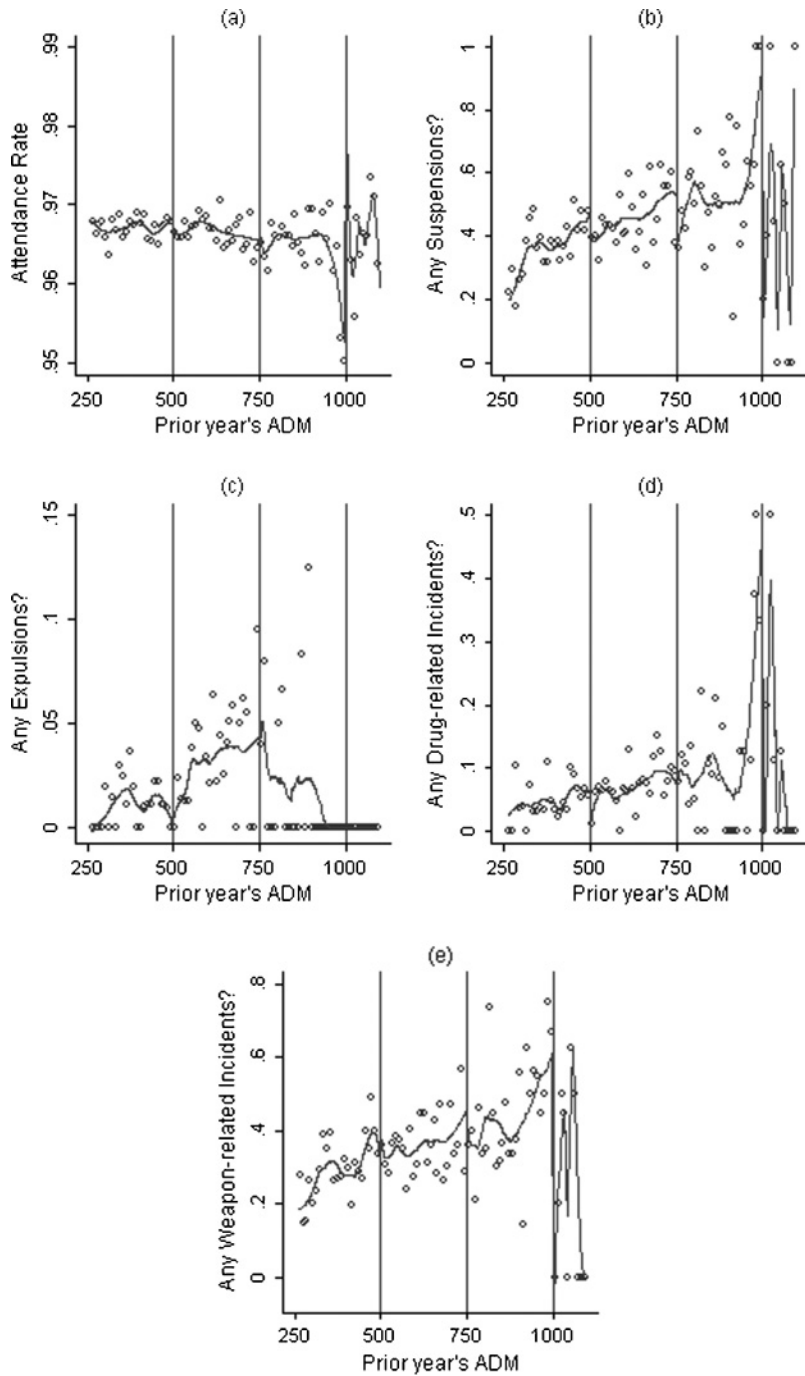
types of disciplinary incidents during that year: a suspension, an expulsion, a drug-related incident, or a weapon-related incident. These columns display the average marginal effects derived from estimation of probit models, with the same independent variables as in equation 1. Discrete dependent variables are used because these disciplinary events occur infrequently in elementary schools, so estimates of the effects of counselors using a continuous dependent variable would be strongly influenced by the very few observations of schools with numerous incidents of the same type of disciplinary event during the same year. For suspensions and weapon-related incidents, which have relatively high frequencies (as seen in the first row of table 2a), the estimated effects of counselor subsidies are very large though not always statistically significant if one changes the dependent variable to incidents per pupil.<sup>19</sup>

The second column of table 4 reveals that counselor subsidies decrease the likelihood that at least one student is suspended from school during the year. This relationship is fairly large and is statistically significant at the .05 level, with an additional half-time counselor subsidy leading to a 12.2 percentage point decrease in the likelihood of a suspension (or a 9.5 percentage point decrease based on the model including the full set of control variables). This finding could stem from a positive influence of counselors on students' behavior, but it could also be related to alternative ways that schools can handle disruptive students when they have additional counselors. The next two columns of table 4 suggest that counselor subsidies do not strongly influence rates of expulsions or drug-related incidents. These events occur relatively infrequently, so the lack of a statistically significant observed effect on expulsions or drug-related incidents may simply be due to the limited precision of the estimates.

The final column of table 4 suggests that counselor subsidies strongly reduce rates of weapon-related incidents among students. An additional half-time counselor subsidy leads to an 11.5 percentage point decline in the likelihood of a weapon-related incident (or a 10 percentage point decline based on the model including the full set of control variables). This positive effect on student behavior could have very large social benefits, given that a weapon-related incident as early as elementary school would typically be associated with negative noncognitive traits, such as a lack of self-control and an oppositional attitude, that are linked to future problem behaviors.

Figures 1a–1e illustrate some sources of the identification of policy effects in these models by plotting the various dependent variables against the prior year's ADM used to determine counselor subsidies. The figures display circles

19. Given a bandwidth of  $\pm 60$  ADM and including the full set of control variables, an additional 0.5 FTE counselor subsidy leads to a 0.00107 decrease in suspensions per pupil (.00065 standard error) and leads to a .00028 decrease in weapon-related incidents per pupil (.00025 standard error).



**Figures 1a–1e.** Discontinuity Patterns across the Full Sample. Schools receive additional counselor subsidies as their prior year’s enrollment (average daily membership) exceeds each threshold (vertical line). Circles are variable-cell means, 10 ADM wide. Locally weighted regression curves are based on intervals below and above each discontinuity line.

for “raw” ADM cell means of the dependent variable with ADM cells that are 10 ADM wide, as well as fitted locally weighted regression curves. The vertical lines indicate the three discontinuity points used for counselor subsidies. Naturally, the likelihood of at least one student having a disciplinary problem increases as the size of the student body increases, but these figures reveal the notable drops that tend to happen for suspensions and for weapon-related incidents.

### **Sensitivity Checks for the Regression Discontinuity Results**

#### ***Permutation Tests to Help Interpret the Joint Significance of the Results***

The greater the number of outcomes examined in a research study, the higher the likelihood that one would find statistically significant results in the absence of any true effect, simply due to random variation. In the main results displayed in tables 3 and 4 (i.e., those using the pooled sample of observations surrounding the three discontinuity points), two out of the five behavioral outcomes were statistically significant at the .05 level (or at the .075 level if the models use the full set of control variables), and none of the six test score outcomes were statistically significant at the .10 level. Furthermore, if one were testing the one-tailed hypothesis that additional counselor subsidies lead to *beneficial* effects on student outcomes, one would reject the null hypothesis of zero effect in favor of the alternative hypothesis of a beneficial effect at the .038 level for two models using the full set of control variables. Given that all these dependent variables are not independent, it is not immediately obvious how frequently one would find such statistically significant results for a subset of these dependent variables even in the absence of any true effects.

To investigate this issue, one may conduct permutation analyses using Monte Carlo simulations in which the actual school observations are randomly reassigned to a new place along the spectrum of the running variable (prior year enrollment) within each range. For one thousand trials, I randomly drew a counterfactual prior year enrollment value based on a uniform distribution surrounding the discontinuity point close to the school’s actual prior year enrollment level. For each trial, I used the counterfactual prior year enrollment values and the ensuing counterfactual level of counselor subsidies to reestimate the same regression model as equation 1. (While the prior year enrollments were based on the counterfactual values, I continued to control for actual current year student enrollment.) I tabulated the frequency of statistically significant counselor subsidy effects based on this random assignment of prior year enrollments.

These simulation results suggest that the main finding of some beneficial counselor subsidy effects was unlikely to occur simply due to random variation. Among the five behavioral outcomes alone (attendance, suspensions,

expulsions, drug-related incidents, weapon-related incidents), at least two estimates were statistically significant at the .05 level in only 3.7 percent of the cases. For models using the full set of control variables, only 3.5 percent of cases allowed one to accept the alternative hypothesis of a beneficial effect of counselors at the .038 level for at least two types of behavioral outcomes. Even when one considers the simulated regression results for the five behavioral outcomes and the six test score variables together, only 5.6 percent of these cases allowed one to accept the alternative hypothesis of a beneficial effect of counselors at the .038 level for at least two of these eleven outcomes.

### ***Different Bandwidths***

While figures 1a–1e paint a general picture of the patterns above and below the discontinuity points, table A.1 in the appendix shows how the size of the bandwidth matters using the same regressions as earlier. All the estimated slopes retain their sign when smaller or larger bandwidths are used around the discontinuity points, though their magnitude and statistical significance can fluctuate considerably when the sample sizes are small. Decreasing the bandwidth tends to reduce the size and statistical significance of the estimates for suspensions and weapon-related incidents, while increasing the bandwidth to  $\pm 70$  ADM slightly increases the magnitude and statistical significance of these estimates. For small bandwidths, the impact of counselor subsidies on weapon-related incidents is no longer statistically significant. Given that the standard errors are not small and the magnitudes of these estimates are fairly sensitive to the size of the bandwidth, it is difficult to make conclusions about the precise local average treatment effects of counseling subsidies. One can instead conclude that these subsidies have at least some effect on reducing suspensions and weapon-related incidents. In separate results not displayed here, I reach this same conclusion from models that do not restrict the sample based on bandwidths and instead control for cubic terms in the running variable, prior year ADM.<sup>20</sup>

### ***Do Counterfactual Cutoff Points or Librarian/Aide Subsidies Produce Similar Effects?***

To further refute the possibility that the counselor subsidy results were simply due to chance or typical changes associated with increases in the running variable (prior year ADM), I next replicate the main analyses using artificial

20. Using the full set of control variables and these cubic terms and using a sample of 3,250 observations of elementary schools with prior year ADM between 266 and 1,100, additional half-time counselor subsidies reduce the likelihood of a suspension and the likelihood of a weapon-related incident, with one-sided test  $p$ -values of .07 and .03, respectively.



cutoff points. The logical choice of fake cutoffs is prior year ADM of 380 or 940, as these are the closest points to the actual cutoffs that allow bandwidths of  $\pm 60$  ADM that do not produce within-range variation in the number of subsidies for any type of school staff. When the analogous models as in the first or second panel of table 4 are estimated using these counterfactual cutoff points and a bandwidth of  $\pm 60$  ADM, the sample sizes are almost as large, yet counterfactual increases in counselor subsidies are not associated with decreases in either suspensions or weapon-related incidents. For the model including the full set of control variables, counterfactual increases in counseling subsidies are associated with a .065 percentage point increase in the likelihood of suspensions ( $p$ -value = .250) and a .012 percentage point increase in weapon-related incidents ( $p$ -value = .827). Along with the permutation tests described in this section, these results provide convincing evidence that the main findings in table 4 are actually due to counselor subsidies.

Another important check of these findings is to compare the impact of counselor subsidies with the estimated impact of another type of subsidy. This check may be particularly important because, as discussed earlier, I do not know the precise extent to which counselor subsidies actually increase the provisions of counselors rather than simply displacing local funds that would have been used for these counselors. To the extent that schools would have hired the same number of counselors even without the increase in the subsidy, any beneficial estimated effect of counselor subsidies could be due to the inflow of revenues that are used for other purposes. While Alabama's staff subsidies are also awarded for principals and assistant principals, there is the aforementioned endogeneity concern regarding the very low ADM cutoff for receiving additional principal subsidy, and there are few observations surrounding the relatively high ADM cutoffs for receiving additional assistant principals. I therefore focus on the impact of librarian/aide subsidies on the same outcomes. At various cutoff points, schools receive an additional 0.25 FTE subsidy for extending the hours of a librarian or for hiring a part-time aide.

Table A.2 in the appendix displays estimates of the impact of these librarian/aide subsidies. The estimated effects of librarian subsidies tend to be statistically insignificant and do not suggest strong beneficial effects. Librarian subsidies are actually associated with a greater likelihood of drug-related incidents. This result may be due to coincidental random variation or to a minor increase in the detection of drug-related incidents given the presence of additional aides. It is reassuring that only counselor subsidies, and not librarian subsidies, reduce the likelihood of suspensions and of weapon-related incidents.

## 7. CONCLUSIONS

Counselor subsidies decrease the likelihood of elementary school students being suspended or having weapon-related incidents, with large estimated effect sizes. Elementary counselor subsidies do not substantially affect expulsions or drug-related incidents, both of which occur infrequently in elementary schools. While the reduction in suspensions might simply be due to a shift in disciplinary policies whereby misbehaving students are sent to a counselor rather than sent home, schools are obligated to report any weapon-related incident, so the reduction in weapon-related incidents is a clear indication that counselor subsidies reduce certain types of behavioral problems. Given that the minimum cost to the state of an additional 0.5 FTE counselor in Alabama equals about \$23,300, estimates imply that it could cost about \$51,200 (or about \$120 per pupil) in additional state-funded counselor subsidies to yield one less student suspension among median-sized elementary schools.<sup>21</sup> If researchers are correct that physical aggression and other misbehavior in the early years put youth on track for future cases of juvenile delinquency, the benefits to the state from additional elementary counselor subsidies may exceed their cost, especially given that some portion of the \$51,200 in subsidies may simply be a shift in tax burden from the local level to the state level.

The results do not suggest that counselor subsidies have large effects on student test scores. There is loose evidence indicating that subsidies increase student participation rates on some types of standardized tests, possibly complicating the identification of the true positive impact of counselor subsidies on student test scores. The lack of a strong positive effect of counselor subsidies on test scores may seem surprising for a couple of reasons. Counselor subsidies reduce disciplinary incidents, and prior studies have found that greater exposure to potentially disruptive peers decreases students' test scores (Aizer 2008; Carrell and Hoekstra 2008; Figlio 2007). In addition, there is evidence that students make greater test score gains in states that require or encourage greater provision of elementary counseling services (Reback 2009).

Even if counselors help students' own academic performance and behavior, however, there are several potential reasons why the Alabama regression discontinuity results might fail to suggest strong positive effects of counselor

21. A median-sized elementary school has about 425 students, and an additional half-time counselor reduces the mean number of suspensions per year at that school by about .454 ( $= .00107 \times 425$ ; see note 20), which means that about 2.2 ( $= 1/.454$ ) additional half-time counselors are required to eliminate one suspension. The \$23,300 minimum estimate for the cost of a half-time counselor subsidy is based on the fact that the statewide minimum salary in Alabama for the 2005–6 school year was \$38,796 for a full-time counselor with less than three years of experience, and fringe benefits, would cost roughly an additional 20 percent of the employee's salary. (The statewide minimum salaries are incrementally higher for more experienced counselors, slowly increasing up to \$49,226 for a full-time counselor with more than twenty-nine years of experience.)

subsidies on mean test scores. First, counselors might decrease the frequency of severe misbehavior without reducing the frequency of minor misbehavior that can disrupt classroom learning. Given that counselors reduce suspensions, students might actually face a greater number of classroom hours with the students who tend to be most disruptive. Second, mean student-level test scores could be noisy and incomplete measures of students' academic progress so that greater counseling subsidies improve students' academic performance in unobserved ways. Third, if Alabama school districts can correctly anticipate which schools' students' academic achievement is very sensitive to the marginal provisions of additional counselors, state counselor subsidies might not substantially affect student achievement; schools in most need of counseling services may tend to hire extra counselors even when they fall just short of qualifying for a state subsidy. Fourth, year-to-year variation in counselor provision might be far less important than whether a school consistently provides a greater counselor-student ratio. Finally, it is possible that the marginal school counselor employed due to an extra state subsidy is much less effective at improving students' academic performance than the average elementary counselor. Even if the mean quality of Alabama's counselors is on par with counselors across the nation, the marginal addition of a half-time counselor appointment in Alabama may not provide the same quality of counseling services as baseline, permanently provided counseling coverage. Given the increasing desire for elementary school counselors to also promote students' academic growth (Bowers and Hatch 2003), it is important for future research to determine whether permanent increases in counselor availability raise student achievement.

Along with the findings of Carrell and Carrell (2006), this article's results highlight the positive effects of elementary school counselors in terms of reductions in rates of behavior problems. More evidence is needed concerning the precise impact of counselor availability on various outcomes for specific types of students. For example, Nagin and Tremblay (1999) find that externalized disorders are usually temporary rather than persistent, and it is unclear which types of counseling services (if any) are effective at treating temporary problems as opposed to persistent problems. This research topic is complicated by the relationship between the previous availability of mental health services and the duration of mental disorders and behavioral problems. The importance of this topic merits a large-scale randomized experiment tracking students' external and internal problematic behaviors and students' receipt of various types of mental health services over a long period of time.

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## APPENDIX

**Table A.1.** The Effect of Counselor Subsidies on Student Behavior: Regression Discontinuity Results Using Alternative Bandwidths

	<b>Likelihood That the School Experiences at Least One:</b>				
	<b>Attendance Rate</b>	<b>Suspension</b>	<b>Expulsion</b>	<b>Drug- Related Incident</b>	<b>Weapon- Related Incident</b>
<b>±40 student bandwidth around any of the three points of discontinuity (N = 915)<sup>a</sup></b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0006	-.108	.015	-.040	-.065
(Standard error)	(.0015)	(.065)	(.014)	(.036)	(.065)
<b>±50 bandwidth around any of the three points of discontinuity (N = 1,138)<sup>a</sup></b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0010	-.082	.022	-.028	-.069
(Standard error)	(.0013)	(.059)	(.014)	(.031)	(.058)
<b>±60 bandwidth around any of the three points of discontinuity (same as table 4)</b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0005	-.122	.006	-.011	-.115
(Standard error)	(.0012)	(.054)	(.011)	(.026)	(.052)
<b>±70 bandwidth around any of the three points of discontinuity (N = 1,611)<sup>a</sup></b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0004	-.126	-.004	-.004	-.121
(Standard error)	(.0012)	(.052)	(.011)	(.024)	(.049)
<b>±80 bandwidth around any of the three points of discontinuity (N = 1,825)<sup>a</sup></b>					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0007	-.088	-.003	-.013	-.076
(Standard error)	(.0011)	(.049)	(.010)	(.023)	(.048)

Notes: Models are analogous to those in the first panel in table 4. All standard errors are adjusted for clustering by school.

<sup>a</sup>The sample sizes are as noted, with the following exceptions: The attendance models have larger sample sizes due to the availability of data for a greater number of years, with sample sizes of 1,164 for the ±40 bandwidth model, 1,447 for the ±50 bandwidth model, 2,039 for the ±70 bandwidth model, and 2,300 for the ±80 bandwidth model. The expulsion models have smaller sample sizes, with sample sizes of 745 for the ±40 bandwidth model, 918 for the ±50 bandwidth model, 1,515 for the ±70 bandwidth model, and 1,719 for the ±80 bandwidth model. The drug-related incident models have sample sizes of 797 and 1,138 for the ±40 and ±50 bandwidth models, respectively.

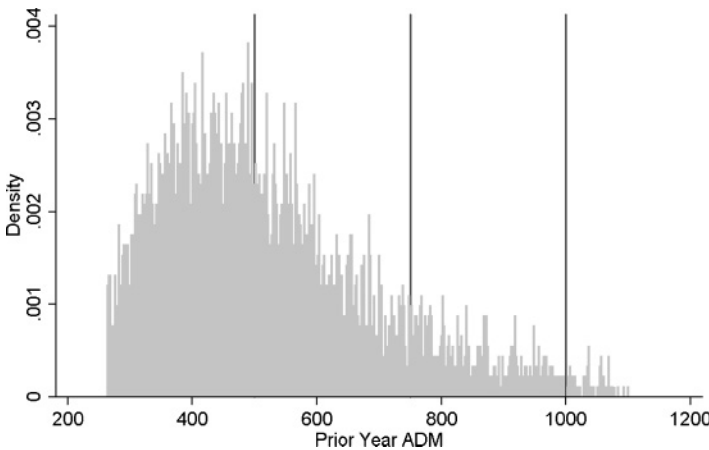


**Table A.2.** The Impact of Subsidies for 0.25 Full-Time Equivalent Librarian/Aide Positions on Students' Behavioral Outcomes

Bandwidth around Cutoffs	Likelihood That the School Experiences at Least One:				
	Attendance Rate	Suspension	Expulsion	Drug- Related Incident	Weapon- Related Incident
±40 (N = 1,022) <sup>a</sup>	.0012 (.0013)	-.032 (.063)	.005 (.016)	.058 (.031)	-.010 (.054)
±50 (N = 1,289) <sup>a</sup>	.0004 (.0012)	-.018 (.058)	.001 (.017)	.050 (.026)	-.002 (.050)
±60 (N = 1,542) <sup>a</sup>	.0004 (.0011)	-.032 (.053)	.003 (.013)	.054 (.025)	.009 (.046)
±70 (N = 1,782) <sup>a</sup>	.0005 (.0010)	-.040 (.050)	.006 (.012)	.059 (.023)	-.002 (.044)
±80 (N = 2,037) <sup>a</sup>	.0006 (.0009)	-.034 (.048)	.002 (.011)	.042 (.020)	.004 (.043)

Notes: Models are analogous to those in the first panel of table 4.

<sup>a</sup>The sample sizes are as noted, except the expulsion models have slightly smaller sample sizes of 875 and 1,102 for the first two rows above, and the attendance rate models have larger sample sizes due to the availability of additional years of data for attendance rates.



**Figure A.1.** The Distribution of School-Level Observations by Prior Year ADM. This histogram uses bins that are five ADM wide and limits the sample to schools with prior year ADM between 299.99 and 1,199.99. The sample includes observations from all available years (1997–98 through 2005–6 school years).