Non-instructional Spending Improves Non-cognitive Outcomes: Discontinuity Evidence from a Unique Elementary School Counselor Financing System

Draft: June, 2009

Randall Reback*
Barnard College, Columbia University
rr2165@columbia.edu

Abstract: Children's non-cognitive 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 non-cognitive skill acquisition. This paper 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.

* This research was supported by the National Science Foundation and the U.S. Department of Education's National Center for Education Statistics via a grant from the American Educational Research Association, which receives funds for its "AERA Grants Programs" under NSF Grant #RED-0310268. Opinions reflect those of the author and do not necessarily reflect those of the granting agencies. I am grateful for this funding and for the valuable research assistance of Linda Carrion, Angelica Ortega, Alex Perl, Heather Schwartz, and Sarie Smolley. I also thank the National Center for Education Statistics for restricted-use access to the Early Childhood Longitudinal Survery-Kindergarten Cohort. I appreciate valuable suggestions from two anonymous referees, Julie Cullen, Janet Currie, Elizabeth Dhuey, David Figlio, Jim Heckman, Miguel Urquiola, Jane Waldfogel, Lei Zhang, and participants in conferences of the Society of Labor Economists, American Education Finance Association, and Southern Economics Association.

1. Introduction

Young children's non-cognitive 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, forthcoming). While internalized disorders (e.g., anxiety, depression) do not necessarily have a direct negative influence on peer achievement (Neidell and Waldfogel, forthcoming), 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 non-cognitive skill differentials as a source of the pay gap between GED recipients and traditional high school graduates (Heckman & Rubinstein, 2001; Heckman, Stixrud, and Urzua, 2006) and the gap in college enrollment between men and women (Jacob, 2002). Differences in non-cognitive traits may also substantially contribute to socio-economic gaps in school achievement and in future earnings.[1]

In light of the potentially enormous social returns to investments improving children's non-cognitive traits, behavior, and mental health, researchers have become increasingly interested in learning which educational interventions influence these outcomes. Changes in non-cognitive traits and behavior have been cited as potentially important effects from various schooling options, including charter schools (Imbermann, 2008), public schools of choice (Cullen, Jacob, & Levitt, 2006), private schools (Figlio &

[1] Examining data from the 1958 and 1970 British birth cohorts, Blanden, Gregg, and Macmillan (2006) find that a growing return to non-cognitive traits can partially explain rising income inequality in Britain.

Ludwig, 2000), and pre-Kindergarten schools (Neidell & Waldfogel, forthcoming).[2] Relatively little is known, however, about how traditional public schools can improve students' 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 8th grade class sizes improve students' engagement in the subject. Carrell and Carrell (2006) find that inter-temporal 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 non-cognitive 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 40,000 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,

---

[2] Imbermann (2008) 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, & 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 less likely to use birth control when they are sexually active. Neidell and Waldfogel (forthcoming) find that pre-school attendance is associated with worse non-cognitive 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 pre-school attendance outweigh the negative effects associated with more frequent externalized problem behaviors.

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 & 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 paper 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 & Lavy, 1999; Hoxby, 2000; Urquiola, 2006),[3] this is the first regression discontinuity study to examine the importance of student to staff ratios for non-instructional 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 U.S. to directly subsidize elementary school counselors and the only state to base these subsidies on discrete student enrollment ranges.[4] 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

---

[3] Researchers have also employed regression discontinuity analyses to examine other topics related to education. Some examples include Jacob & Lefgren's (2004) study of the impact of summer school and grade repetition, Ludwig & 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 Elder & Lubotksty, 2007, Black, Devereaux, & Salvanes, 2008, and McEwan & Shapiro, 2008, for recent examples and discussions of other entry age studies).

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

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 non-cognitive 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" (U.S. Dept. of Health & Human Services, 1999), but that 80 percent of children needing mental health services fail to receive them (Kataoka et al., 2002). This potential under-provision 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 non-cognitive 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 non-cognitive skills are important predictors of both future educational attainment and future earnings. Controlling for educational attainment, they find that non-cognitive skill measures are

even better than cognitive measures for predicting the future wages of all but the 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 U.S. and Canada reveal substantial negative effects of the disorder on both test scores and educational attainment (Currie & 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 1 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 and 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 Waldogel (forthcoming) 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 6th grade. A greater representation of these types of boys in 6th 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 grademates 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 which 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 non-clinical 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 non-cognitive skills and mental health needs (Atkins et al., 2003; Weist et al., 2003), and young children's non-cognitive 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 non-instructional resources.[5] There is a growing

[5] A few notable recent studies have identified the causal effects of public school resources on behavioral outcomes and/or examined the impact of non-instructional resources. Examing 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 8th 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

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 & Prout, 1998; Whitson & 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 & Campbell, 2003) and Washington (Sink & Stroh, 2003) base their findings on comparisons of students in treated and non-treated 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 only tracking outcomes shortly after the conclusion of the program (e.g., Gerler et al., 1985; Larkin & Thyler, 1999; Manning, 1988; Russell & Roberts, 1979).

Carrell and Carrell (2006) conduct one of the most rigorous non-experimental 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 student previously involved in a disciplinary incident, especially among Black male students and students from low income

---

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 Conuty, Florida.

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, 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 (Reback, 2009). 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 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 their standards for certifying schools. Throughout this paper's sample period, Alabama maintained foundation funding at the SACS' 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, then the district would receive greater funds from the state because the district would 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 40 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 1 unit for librarians, then the school is allowed to use these additional librarian units to either hire either part-time librarians or to hire aides. For the other three types of staff positions—principals, assistant principals, and counselors— the school can only use the money towards 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 (*Requirements for Certification…*, 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 a certified school counselor. 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 to be a certified public school teacher (*Requirements for Certification…*, 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. 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 (ASCA, 2003). This new emphasis on academics, along with the beginning of the *No Child Left Behind* school accountability program in the 2002-03 school year, may have influenced Alabama counselors' behavior, especially in the later years of this paper'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 2005-06 school years. These data come primarily from Alabama's publicly available school report cards, which report school-level information concerning 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 which were either atypical in size or represented large changes for the same school from one year to the next. The 2001-02 report cards also included retrospective data concerning ADM and attendance rates from 1986-87 through 2001-02, so I am able to 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.[6] 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

[6] It is not possible to know which report cards accurately report the full-time-equivalent 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 2001-2002, 21 percent in 2002-03, 18 percent in 2003-04, and then 4 percent in 2004-05, 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 inter-temporal variation in school-level reporting practices.

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 student and mean fourth grade student scores on Stanford Achievement Tests in math, language arts, and reading for the 2001-02 through 2004-05 school years.[7] The 2003-04 and 2004-05 test score data come from electronic files downloaded off the Alabama Department of Education website 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 the federal *No Child Left Behind Act*. 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.[8] Section 6.2 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 NCES' annual Common Core of Data. In particular, the Common Core of Data provides school-level information concerning the fraction of students whose ethnicity is nonwhite, the distribution of enrollments across

---

[7] Alabama switched from the 9th edition of the Stanford Achievement Test to the 10th edition after the 2003-04 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 2001-02 and 2002-03 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%, 94.6%, and 95.5% respectively in 2001-02 and 97.3%, 97.8%, and 98.0% for 2002-03. Among fourth grade students, these percentages were 92.3%, 94.9%, 95.5% in 2001-02 and 97.9%, 98.2%, and 98.3% in 2002-03.

grades, and the school's pupil-teacher ratio, as well as district-level data concerning operating expenditures per student.

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 either be 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 & 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 the likelihood of treatment rises sharply at the discontinuity. Given that the counselor subsidies are non-fungible, the rates of compliance for employing counselors should be very high; there should be hardly any cases where 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 & Lemiuex, 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.[9]

---

[9] NCES' Early Childhood Longitudinal Survey-Kindergarten Cohort (ECLS-K) tracked a nationally representative sample of students who were typically in third grade during the 2003-04 school year, at

As long as schools just above the threshold are not systematically different than schools just below the threshold, then 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 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, then 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

---

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.

treatment effect is either the number of counselors at the school or students' receipt of counseling services, one can alternatively view these estimates 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 (average daily membership) 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 non-counselor staff positions remain constant within each ADM range. The baseline sample pools observations surrounding three different counselor subsidy cutoffs (500, 750, and 1000 ADM),[10] 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 to 560, 690 to 810, or 940 to 1060). The main regression model is thus:

$$\begin{aligned} Y_{i,t} &= Range_{i,t-1} * ADM_{i,t-1}\beta_1 + Range_{i,t-1} * ADM_{i,t}\beta_2 + \beta_3 HalfTimeCounselorSub_{i,t}(ADM_{i,t-1}) \\ &+ \beta_4 \%Grades_Kto2_{i,t} + \beta_5 \%Grades_3or4_{i,t} + Range_{i,t-1}\beta_6 + \beta_7 \%LowIncome_{i,t} + \beta_8 \%Nonwhite_{i,t} \\ &+ \beta_9 PupilsPerTeacher_{i,t} + \beta_{10} SpendingPerPupil_{i,t} + T_t + \varepsilon_{i,t} \end{aligned} \tag{1}$$

where T_t is a vector of year effects, $ADM_{i,t}$ is the average daily membership during the first 40 school days of year *t*, $\%Grades_Kto2_{i,t}$ is the fraction of school *i*'s students in Kindergarten through 2nd grade and $\%Grades_3or4_{i,t}$ is the fraction in 3rd or 4th 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

[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.

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 ± 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

6.1 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 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.[11] 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 re-assigning students across schools to gain additional counselors (personal communication, 10/25/2007). As shown below, his assertion is consistent with the data. He believes that the only way in which some districts have responded to this financial structure is by closing 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).[12]

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, $X_{i,t-1}$:

$$X_{i,t-1} = Range_{i,t-1} * ADM_{i,t-1}\lambda_1 + \lambda_2 HalfTimeCounselorSub_{i,t}(ADM_{i,t-1}) + \lambda_3 \%Grades_Kto2_{i,t-1} + \lambda_4 \%Grades_3or4_{i,t-1} + Range_{i,t-1}\lambda_5 + T_t + e_{i,t-1} \quad (2)$$

[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.

[12] This sample restriction only eliminates 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' ADM's 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 5 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 decline in density at these cutoffs appear to be at least as large as the declines in density approaching the cutoffs from further away. For the main sample used below, there are 92 observations within 5 students below a counselor subsidy cutoff and 90 observations within 5 students above a cutoff. For a smaller bandwidth of $\pm$ 3 students, these counts are 35 and 34 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.

6.2 Main regression discontinuity results

Table 3 displays the estimates of the impact of counselor subsidies (β_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.

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 21 questions on a survey form given to instructional staff to assess their schools' counselors, 3 questions relate to how well the counselors assist with the administration and analysis of student test score data ("Counselor Survey for Instructional Staff", 2002).[13] If a policy increases rates of student test-taking, then this could decrease

[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,"

"trains teachers to administer tests, when necessary." The other 18 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

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.[14] 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 100 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 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

---

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 3rd grade math participation rates, -.4 on third grade language, -0.1 on third grade reading, 0.7 percentage points on 4th grade math, 0.1 on 4th grade language, and -0.7 on fourth grade reading.

participation.[15] 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 theses effects. However, even a very generous upper bound estimate of the potential downward bias is less than .07 standard deviations for any school's 4th grade math Z-score, only about .04 standard deviations for the median-performing school in 4th grade math, and even less for the other grades and subjects.[16] Test-taking selection bias alone thus cannot explain why the estimates in the first panel of Table 3 are statistically insignificant.[17]

Table 4 displays estimates of β_3 in equation 1 with students' behavioral outcomes as dependent variables, again using the sample with the $\pm$ 60 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 2000-01 report cards. The results in column 1 of the first panel suggest that one additional half-time counselor subsidy is

---

[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 4th grade math scores, a school's Z-score would decline by less than .07 standard deviations if one replaces 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 are statistically significant, nor would they be statistically significant if they were increased by .04.

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.[18]

The remaining columns of Table 4 examine the effect of counselor subsidies on the likelihood that schools experience at least one occurrence of various 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 that 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.[19]

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

---

[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.

[19] Given a bandwidth of ± 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).

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 non-cognitive traits, such as a lack of self-control and an oppositional attitude, that are linked to future problem behaviors.

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

6.3 Sensitivity checks for the regression discontinuity results

6.3.1 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, then one would reject the null hypothesis of zero effect in favor of the alternative hypothesis of a beneficial effect at the .038 level for models using the full set of control variables. Given that all of 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 re-assigned to a new place along the spectrum of the running variable, (prior year enrollment), within each range. For 1,000 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 re-estimate 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 both 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.

6.3.2 Different bandwidths

While Figures 1a through 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 of 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 which do not restrict the sample based on bandwidths and instead control for cubic terms in the running variable, prior year ADM.[20]

6.3.3 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 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

[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.

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 Section 6.3.1, 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 since, 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/aid subsidies on the same outcomes. At various cutoff points, schools receive an additional 0.25 full-time-equivalent (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 due to a minor increase in 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 full-time-equivalent 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 fewer student suspension among median-sized elementary schools.[21] If researchers are correct that physical aggression and other misbehavior in the early years puts youth on track for future cases of

[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*425; see footnote 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 2005-06 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% 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.)

juvenile delinquency, then 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 & 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 conseling 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 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, then 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 (ASCA, 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 paper'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.

References

Adelman, H.S. & Taylor, L. 2003. Toward a comprehensive policy vision for mental health in schools. *Handbook of School Mental Health: Advancing Practice and Research*, edited by Weist M.D., Evans, S.W., & N.A. Lever. New York: Kluwer Academic/Plenum.

Aizer, Anna. 2008. Peer effects and human capital accumulation. *NBER Working Paper 14354*.

Angrist, Joshua D. & Lavy, Victor. 1999. Using Maimonides' Rule to estimate the effect of class size on scholastic achievement. *The Quarterly Journal of Economics* 114(2): 533-575.

ASCA (American School Counselor Association). 2003. *The ASCA national model: A framework for school counseling programs.* Available www.schoolcounselor.org/files/Natl%20Model%20Exec%20Summary_final.pdf. Accessed 5 June 2009.

ASCA (American School Counselor Association). 2006. *State certification requirements*. Available www.schoolcounselor.org/content.asp?pl=325&sl=133&contentid=242. Accessed 18 October 2006.

Atkins, M.S., P.A. Graczyk, S.L. Frazier, & J. Abdul-Adil. 2003. Toward a new model for promoting urban children's mental health: Accessible, effective, and sustainable school-based mental health services. *School Psychology Review* 32(4): 503-514.

Black, Sandra E., Devereaux, Paul J., and Salvanes, Kjell G. 2008. Too young to leave the nest: The Effects of school starting age. *NBER Working Paper 13969.*

Blanden J., Gregg P, and Macmillan L. 2006. Accounting for intergenerational income persistence: Noncognitive skills, ability and education. *Economic Journal* 117 (519): C43-C60.

Brigman, G. & Campbell, C. 2003. Helping students improve academic achievement and school success behavior. *Professional School Counseling 7*(2): 91-98.

Carrell, S. & Carrell, S. 2006. Do lower student to counselor ratios reduce school disciplinary problems? *Contributions to Economic Analysis & Policy 5*(1): 1463-1463.

Carrell, S. & Hoekstra, M.L. 2008. Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. *NBER Working Paper 14246.*

Cellini, Stephanie R., Ferreira, Fernando, and Rothstein, Jesse. 2008. The Value of School Facilities: Evidence from a Dynamic Regression Discontinuity Design. *NBER Working Paper 14516*.

Common Core of Data (CCD) for 2001-2002. 2005. National Center for Education Statistics.

Counselor Survey for Instructional Staff. 2002. *Alabama professional education personnel evaluation program*. Available http://www.alabamapepe.com/specialty/alcnslrsurveys102.pdf. Accessed 18 October 2006.

Cullen, Julie B., Jacob, Brian A., & Levitt, Steven. 2006. The effect of school choice on participants: Evidence from randomized lotteries. *Econometrica* 74(5): 1191-1230.

Currie, Janet & Stabile, Mark. 2006. Child mental health and human capital accumulation: The case of ADHD. *Journal of Health Economics* 25: 1094-118.

Currie, Janet & Stabile, Mark. 2007. Mental health in childhood and human capital. *NBER Working Paper 13217*.

Dee, Thomas & West, Martin. 2008. The non-cognitive returns to class size. *NBER Working Paper 13994*.

Elder, Todd E. & Lubotsky, Darren H. *forthcoming*. Kindergarten entrance age and children's achievement: Impacts of state policies, family background, and peers. *Journal of Human Resources*.

Figlio, David N. 2007. Boys Named Sue: Disruptive Children and Their Peers. Education Finance and Policy 2(4): 376-394.

Figlio, David N., & Ludwig, Jens. 2000**.** Sex, Drugs, and Catholic Schools: Private Schooling and Adolescent Non-Market Behavior**.** *NBER Working Paper 7990*.

Gerler, E. R., Kinney, J., & Anderson, R. F. 1985. The effects of counseling on classroom performance. *Journal of Humanistic Education and Development 23*: 155-165.

Heard, Dennis. Office of LEA Funding and Accountability, Alabama Department of Education, personal communication, 10/25/2007.

Heckman, James J. 2000. Policies to foster human capital. *Research in Economics* 54: 3-56.

Heckman, James J. & Rubinstein, Yora. 2001. The importance of non-cognitive skills: Lessons from GED Testing Program, *American Economic Review* 91(2): 145-149.

Heckman, James J., Stixrud, J., & Urzua, S. 2006. The Effects of cognitive and noncognitive abilities on labor market outcomes and social behavior. *Journal of Labor Economics* 24(3): 411-482.

Hoxby, Caroline M. 2000. The Effects of class size on student achievement: New evidence from population variation. *The Quarterly Journal of Economics* 115(4): 1239-1285.

Imbens, Guido W. & Lemieux, Thomas. 2008. Regression discontinuity designs: A guide to practice. *Journal of Econometrics* 142: 615-635.

Imbermann, Scott A. 2008. Achievement and behavior in charter schools: Drawing a more complete picture. mimeo, University of Houston.

Jacob, Brian A. 2002. "Where the Boys Aren't: Non-Cognitive Skills, Returns to School and the Gender Gap in Higher Education" *Economics of Education Review*. 21: 589-598.

Jacob, Brian A. & Lefgren, Lars. 2004. Remedial education and student achievement: A regression-discontinuity analysis. *Review of Economics and Statistics* 86(1): 226-244.

Kane, Thomas J. 2003. A Quasi-experimental estimate of the impact of financial aid on college-going. *NBER Working Paper 9730*.

Kataoka, S. H., L. Zhang & Wells, K.B. 2002. Unmet need for mental health care among U.S. children: Variation by ethnicity and insurance status. *The American Journal of Psychology 159*(9): 1548-1555.

Larkin, R. & Thyler, B.A. 1999. Evaluating cognitive- behavioral group counseling to improve elementary school student's self- esteem, self control, and classroom behavior. *Behavioral Interventions 14*: 147- 161.

Lazear, Edward. 2001. Educational Production. *Quarterly Journal of Economics* 116(3): 777-803.

Ludwig, Jens, & Miller, Douglas L. 2007. Does head start improve children's life chances? Evidence from a regression discontinuity design. *Quarterly Journal of Economics* 122(1): 159-208.

Manning, B.H. 1988. Application of cognitive behavior modification: First and third grader's self management of classroom behavior. *American Educational Research Journal 25*: 193- 212.

McEwan, Patrick J. & Shapiro, Joseph S. 2008. The benefits of delayed primary school enrollment: Discontinuity estimates using exact birth dates. *Journal of Human Resources* 43(1): 1-29.

Nagin, D., & Tremblay, R.E. 1999. Trajectories of boys' physical aggression, opposition, and hyperactivity on the path to physically violent and nonviolent juvenile delinquency. *Child Development* 70 (5): 1181-1196.

Neidell, Matthew & Waldfogel, Jane. *forthcoming*. Cognitive and non-cognitive peer effects in early education. *The Review of Economics and Statistics*.

Prout, S.M. & Prout, H.T. 1998. A meta-analysis of school-based studies of counseling and psychotherapy: An update. *Journal of School Psychology 36*(2): 121-136.

Reback, Randall. 2009. Schools' mental health services and young children's emotions, behavior, and learning. *Barnard College Economics Department Working Paper 09-04.*

Requirements for Certification of Teachers, Counselors, Librarians, Administrators for Elementary and Secondary Schools, 66th edition, 2001-2002. (2001). edited by Elizabeth A. Kaye. Chicago, IL: University of Chicago Press.

Russell, M. L.& Roberts, S. M. 1979. Behaviorally based decision making training for children. *The Journal of School Psychology 17*: 264- 269.

Sink, C. A., & Stroh, H. R. 2003. Raising achievement test scores of early elementary school students through comprehensive school counseling programs. *Professional School Counseling 6*: 352-364.

Stage, S.A. , & Quiroz, D.R. 1997. A meta-analysis of interventions to decrease disruptive classroom behavior in public education settings. *School Psychology Review* 26 (3): 333-368.

Urquiola, Miguel. 2006. Identifying class size effects in developing countries: Evidence from rural schools in Bolivia. *Review of Economics and Statistics* 88(1).

Urquiola, Miguel, & Verhoogen, Eric A. 2009. Class size caps, sorting, and the regression-discontinuity design. *American Economic Review* 99(1): 179-215.

U.S. Department of Health and Human Services. 1999. *Mental Health: A Report to the Surgeon General*.

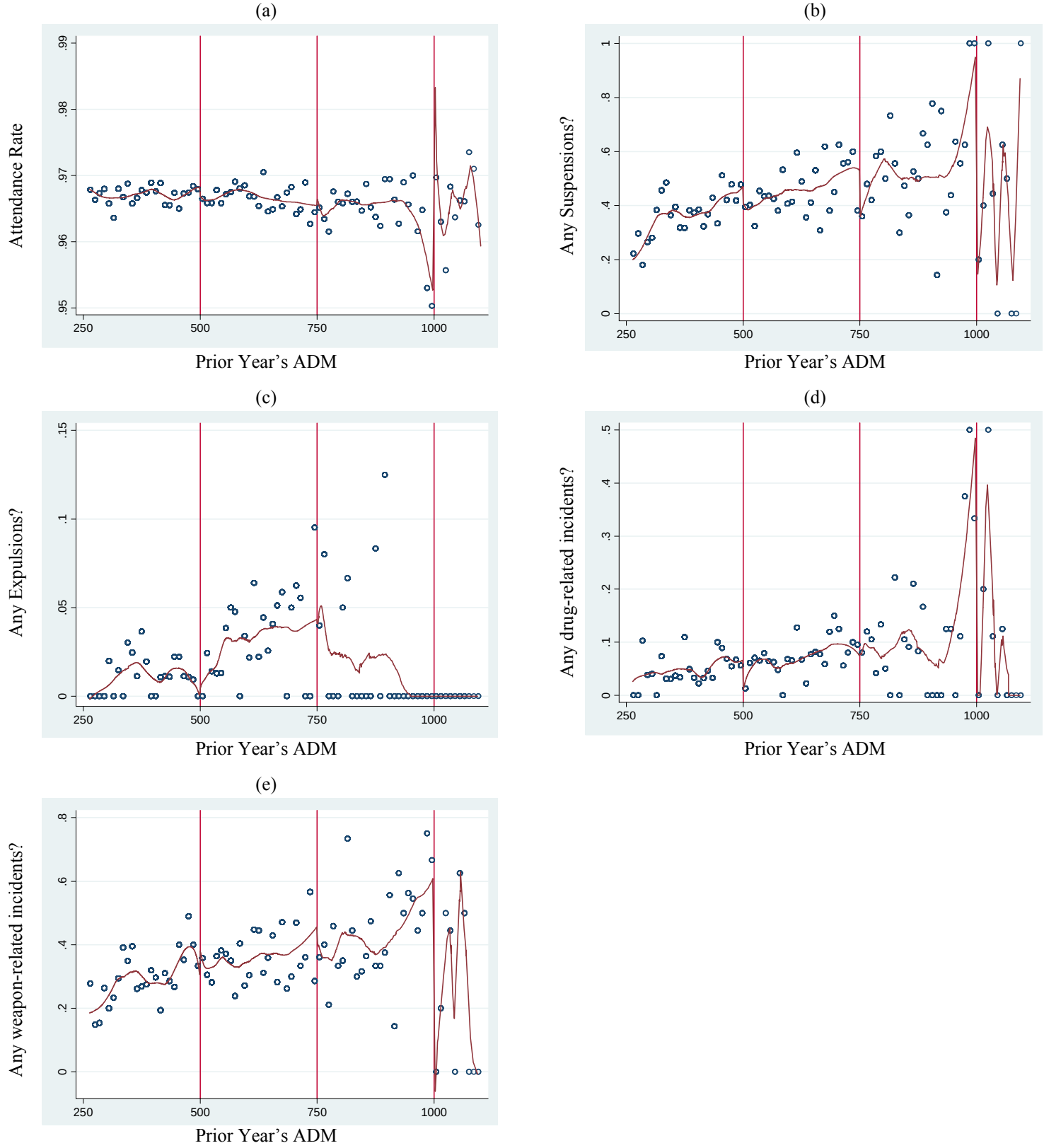
van der Klaauw, Wilbert. 2002. Estimating the effect of financial aid offers on college enrollment: A regression discontinuity approach. *International Economic Review* 43(4): 1249–1287.

Weist, M.D., S.W. Evans, & N.A. Lever. 2003. Advancing mental health practice and research in schools. *Handbook of School Mental Health: Advancing Practice and Research*, edited by S.W. Evans & N.A. Lever (ed.), New York: Kluwer Academic/Plenum.

Whitson, S.C., & Sexton, T.L. 1998. A review of school counseling outcome research: implications for practice. *Journal of Counseling and Development 76*: 412-426.

Zins, J., R. Weissberg, M. Wang, & Walberg, H.J. eds. 2004. *Building academic success on social and emotional learning: What does the research say?* New York City: Teachers College Press.

Figures 1a-1e: Discontinuity Patterns Across the Full Sample



Notes to Figures 1a-e: The vertical lines represent discontinuity points, with schools receiving an additional subsidy for a 0.5 full-time equivalent counselor as the prior year's average daily membership (ADM) exceeds each threshold.

Figures display circles for the variable-cell means of the dependent variable with cells that are 10 ADM wide, as well as locally weighted regression curves for intervals below and above each vertical discontinuity line.

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

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

		Indicator for whether the school experiences at least one…			
	Attendance Rate	**Suspension**	**Expulsion**	**Drug-related incident**	**Weapon-related incident**
MODERATE BANDWIDTH (± 60 ADM) AROUND ANY OF THE 3 POINTS OF DISCONTINUITY					
Mean	.968	0.46	0.020	0.080	0.38
Standard Deviation	.009	0.50	0.14	0.27	0.49
N	1305	1305	1228	1305	1305
MODERATE BANDWIDTH (± 60 ADM) AROUND FIRST DISCONTINUITY POINT					
Mean	.967	0.44	0.018	0.068	0.37
Standard Deviation	.011	0.50	0.13	0.25	0.48
N	1226	970	823	970	970
MODERATE BANDWIDTH (± 60 ADM) AROUND 2nd DISCONTINUITY POINT					
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
MODERATE BANDWIDTH (± 60 ADM) AROUND 3rd DISCONTINUITY POINT					
Mean	.965	0.52	N/A	0.19	0.49
Standard Deviation	.011	0.50		0.40	0.50
N	95	69		67	77

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

	Test Score Sample (N=947)		Behavioral Outcomes Sample (N=1305)	
	Mean	Stand. Dev.	Mean	Stand. Dev.
Lagged ADM for schools in the first lagged ADM range (440 to 559)	497	35	497	34
Current ADM for schools in the first lagged ADM range (440 to 559)	494	52	494	57
Lagged ADM for schools in the second lagged ADM range (690 to 809)	748	34	747	33
Current ADM for schools in the second lagged ADM range (690 to 809)	745	70	741	74
Lagged ADM for schools in the third lagged ADM range (940 to 1059)	985	38	988	38
Current ADM for schools in the third lagged ADM range (940 to 1059)	931	130	952	123
% of students at school enrolled in grades K-2	46.5%	13.9%	46.2%	14.0%
% of students at school enrolled in grades 3-4	32.8%	8.6%	32.8%	8.4%
% of students at school eligible for free or reduced-price lunches	55.8%	25.9%	54.7%	25.7%
% 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	$7485	$1250	$7261	$1287

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

SUBJECT	MATH		LANGUAGE		READING	
GRADE	3rd	4th	3rd	4th	3rd	4th
MODERATE BANDWIDTH (± 60 ADM) AROUND ANY OF THE POINTS OF DISCONTINUITY						
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[a]	894
R-squared	.603	.621	.633	.627	.726	.760
MODERATE BANDWIDTH (± 60 ADM) AROUND 1st DISCONTINUITY POINT						
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[a]	680
R-squared	.635	.635	.660	.639	.744	.770
MODERATE BANDWIDTH (± 60 ADM) AROUND 2nd DISCONTINUITY POINT						
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-squared	.583	.671	.594	.655	.722	.758
MODERATE BANDWIDTH (± 60 ADM) AROUND 3rd DISCONTINUITY POINT						
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-squared	.323	.475	.431	.635	.493	.680

Notes to Table 3 : 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.

[a] One school, Vance Elementary, has a missing value for the mean 3rd grade reading score for 2003-04.

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

	Attendance Rate	Likelihood that the school experiences at least one Suspension	Expulsion	Drug-related incident	Weapon-related incident
MODERATE BANDWIDTH (± 60 ADM) AROUND ANY OF THE 3 POINTS OF DISCONTINUITY					
Models with Fewer Control Variables					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0005	-.122**	.006	-.011	-.115**
(Standard error)	(.0012)	(.054)	(.011)	(.026)	(.052)
N	1738	1370	1288[b]	1370	1370
Models with Full Set of Control Variables					
Estimated effect of an additional subsidized 0.5 FTE counselor	-.0008	-.095*	.004	-.014	-.100*
(Standard error)	(.0009)	(.053)	(.011)	(.026)	(.052)
N	1305	1305	1228[b]	1305	1305
MODERATE BANDWIDTH (± 60 ADM) AROUND FIRST DISCONTINUITY POINT					
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[b]	970	970
MODERATE BANDWIDTH (± 60 ADM) AROUND 2nd DISCONTINUITY POINT					
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[b]	223[c]	258
MODERATE BANDWIDTH (± 60 ADM) AROUND 3rd DISCONTINUITY POINT					
Estimated effect of an additional subsidized 0.5 FTE counselor	.0048	-.602**	N/A[b]	-.056	-.162
(Standard error)	(.0053)	(.203)		(.201)	(.303)
N	77	69[a]		67[c]	77

Notes to Table 4: 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 of 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

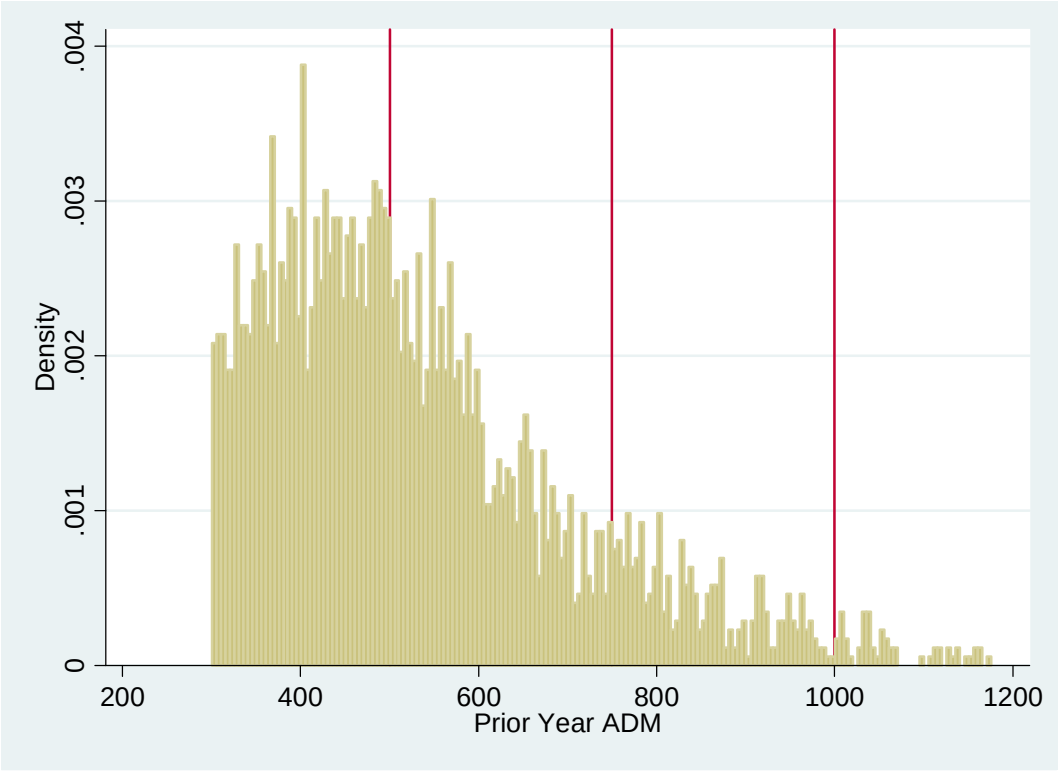
errors for the mean estimated marginal effects were found using the delta method. All standard errors are adjusted for clustering by school.

[a]All schools in the third discontinuity range had at least one suspension during the 2000-2001 school year. [b] None of the schools in the third discontinuity range had any student expulsions, and none of the schools in the first discontinuity in 2003-04 or the second range in 1999-2000 had any expulsions. [c] None of the schools in the second or third ranges had any drug-related incidents in the 1999-2000 school year. ** statistically significant at the .05 level * statistically significant at the .10 level

Appendix

Figure A.1

The Distribution of School-level Observations by Prior Year ADM



Notes to Figure A.1: To facilitate viewing and to ensure that bins do not overlap the cutoff points, this histogram uses bins that are 5 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 (prior year ADM is available for the 1997-98 through 2005-06 school years).

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

	Attendance Rate	Likelihood that the School Experiences at Least One…			
		Suspension	Expulsion	Drug-related Incident	Weapon-related Incident
± 40 STUDENT BANDWIDTH AROUND ANY OF THE 3 POINTS OF DISCONTINUITY (N=915[a])					
Estimated Effect of an Additional Subsidized 0.5 FTE Counselor	-.0006	-.108	.015	-.040	-.065
(Standard Error)	(.0015)	(.065)	(.014)	(.036)	(.065)
± 50 BANDWIDTH AROUND ANY OF THE 3 POINTS OF DISCONTINUITY (N=1138[a])					
Estimated Effect of an Additional Subsidized 0.5 FTE Counselor	-.0010	-.082	.022	-.028	-.069
(Standard Error)	(.0013)	(.059)	(.014)	(.031)	(.058)
± 60 BANDWIDTH AROUND ANY OF THE 3 POINTS OF DISCONTINUITY (SAME AS TABLE 4)					
Estimated Effect of an Additional Subsidized 0.5 FTE Counselor	-.0005	-.122	.006	-.011	-.115
(Standard Error)	(.0012)	(.054)	(.011)	(.026)	(.052)
± 70 BANDWIDTH AROUND ANY OF THE 3 POINTS OF DISCONTINUITY (N=1611[a])					
Estimated Effect of an Additional Subsidized 0.5 FTE Counselor	-.0004	-.126	-.004	-.004	-.121
(Standard Error)	(.0012)	(.052)	(.011)	(.024)	(.049)
± 80 BANDWIDTH AROUND ANY OF THE 3 POINTS OF DISCONTINUITY (N=1825[a])					
Estimated Effect of an Additional Subsidized 0.5 FTE Counselor	-.0007	-.088	-.003	-.013	-.076
(Standard Error)	(.0011)	(.049)	(.010)	(.023)	(.048)

Notes to Table A.1 : Models are analogous to those in the first panel in Table 4. All standard errors are adjusted for clustering by school.

[a] 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 1164 for the ± 40 bandwidth model, 1447 for the ± 50 bandwidth model, 2039 for the ± 70 bandwidth model, and 2300 for the ± 80 bandwidth model. The expulsion models have smaller sample sizes, with samples sizes of 745 for the ± 40 bandwidth model, 918 for the ± 50 bandwidth model, 1515 for the ± 70 bandwidth model, and 1719 for the ± 80 bandwidth model. The drug-related incident models have sample sizes of 797 and 1138 for the ± 40 and ± 50 bandwidth models respectively.

Table A2: The Impact of Subsidies for 0.25 Full Time Equivalent Librarian/Aide Positions on Students' Behavioral Outcomes

Bandwidth Around Cutoffs	Attendance Rate	Likelihood that the School Experiences at Least One…			
		Suspension	Expulsion	Drug-related Incident	Weapon-related Incident
± 40 (N=1022[a])	**.0012**	**-.032**	**.005**	**.058**	**-.010**
	(.0013)	**(.063)**	**(.016)**	**(.031)**	**(.054)**
± 50 (N=1289[a])	**.0004**	**-.018**	**.001**	**.050**	**-.002**
	(.0012)	**(.058)**	**(.017)**	**(.026)**	**(.050)**
± 60 (N=1542[a])	**.0004**	**-.032**	**.003**	**.054**	**.009**
	(.0011)	**(.053)**	**(.013)**	**(.025)**	**(.046)**
± 70 (N=1782[a])	**.0005**	**-.040**	**.006**	**.059**	**-.002**
	(.0010)	**(.050)**	**(.012)**	**(.023)**	**(.044)**
± 80 (N=2037[a])	**.0006**	**-.034**	**.002**	**.042**	**.004**
	(.0009)	**(.048)**	**(.011)**	**(.020)**	**(.043)**

Notes to Table A.2: Models are analogous to those in the first panel of Table 4.

[a] The sample sizes are as noted, except the expulsion models have slightly smaller sample sizes of 875 and 1102 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.