The Impact of No Child Left Behind's Accountability Sanctions on School Performance: Regression Discontinuity Evidence from North Carolina

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Abstract

Comparisons of schools that barely meet or miss criteria for adequate yearly progress (AYP) reveal that some sanctions built into the No Child Left Behind accountability regime exert positive impacts on students. Estimates indicate that the strongest positive effects associate with the ultimate sanction: leadership and management changes associated with school restructuring. We find some positive incentive effects in schools first entering the NCLB sanction regime, but no significant effects of intermediate sanctions. Further analysis shows that gains in sanctioned schools are concentrated among low-performing students, with the exception of gains from restructuring which are pervasive. We find no evidence that schools achieve gains among low-performing students by depriving high-performing students of resources.

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1. Introduction

The No Child Left Behind Act of 2001 (NCLB) required schools receiving Federal Title I funding to track student performance, and to implement an escalating series of negative sanctions in the event performance fell below a state-established threshold. This paper analyzes the impact of these sanctions on student performance. We use school- and student-level data covering North Carolina public schools to compare student test score growth in schools exposed to varying sanctions. Since the NCLB sanction regime treated schools very differently if the performance of a subgroup of students lay on either side of an arbitrary threshold, we use regression discontinuity (RD) techniques to achieve econometric identification.

This paper contributes to a large and growing literature on educational accountability. Accountability regimes are intended to improve student performance by introducing incentives that some argue are absent in traditional public education systems. They rely on the existence of a suitable measure of student performance – ideally one correlated with long-run student well-being and difficult to manipulate by means other than providing high-quality education to all students – and sanctions, whether positive or negative, that sufficiently motivate educators to improve the quality of education provided. Previous literature, summarized below, has found both encouraging and cautionary evidence regarding the overall effects of educational accountability.

While previous literature has carefully considered the incentive effects of local, state, and federal sanction regimes, less attention has been paid to evaluating what happens when responses to incentives are not sufficient to evade punitive sanctions. Our contribution assesses whether the sanctions incorporated into the nation's most comprehensive accountability regime are themselves positive or negative interventions, when assessed by their ability to yield significant

year-over-year improvements in student test scores. These sanctions are important to study for two reasons. First, each of the sanctions could be – and in some cases have been – implemented as stand-alone interventions, often in manners that do not invite rigorous evaluation. Knowledge of the treatment effect of each sanction might inform future discussion over whether the intervention is useful or wasteful. Second, reasonable models of school administrator behavior suggest that responses to the threat of sanctions will evolve over time as knowledge regarding the impact of those sanctions is disseminated. Sanctions that prove to be irrelevant may be increasingly ignored over time.

Our results indicate that the sanctions incorporated into NCLB had widely varying effects. On average, students attending schools that barely missed the cutoff for Adequate Yearly Progress (AYP) in a given year posted test score improvements at most slightly greater than those in schools that barely made the cutoff. Subsequent analyses stratifying schools by the sanction for which they are at risk reveals a significant mix of estimated effects. Consistent with prior literature, we find modest evidence of improvements for schools facing the threat of the first NCLB sanction. We find more significant evidence of improvements among schools that become subject to the ultimate sanction in the system, restructuring incorporating leadership or management change. Intermediate sanctions, including mandatory tutoring services for low-income students, have no demonstrable effect.

Previous literature has raised concerns that a focus on proficiency will lead schools to reallocate resources away from higher-performing students, or more generally that incentives to focus on one group will result in a redirection of resources away from other groups. We assess the importance of these concerns in two ways. First, we determine whether sanctions for low proficiency result in poorer outcomes for students well above the proficiency threshold. While

we do find that the beneficial impacts of early-stage sanctions are concentrated among lower-performing students, we find no evidence of countervailing negative impacts on higher-performing students. In fact, the estimated impacts of restructuring appear to accrue throughout the test score distribution. North Carolina's independent accountability system, which focuses on test score growth rather than accountability, may have restrained any impulse to deprive high-performing students.

Second, we examine whether test score improvements in schools sanctioned for the performance of one specific subgroup are concentrated among members of that subgroup. A concentration of resources along these lines would be a rational response to the incentive. We find evidence of particularly strong effects within the targeted subgroup, with the exception of the restructuring sanction, which appears to generate improvements across the board.

Overall, our results suggest that accountability systems can have modest impacts on student performance, and if properly designed can in fact improve the performance of some students without harming others. The association of strongest, and broadest, effects with restructuring indicates that management and leadership issues are the most significant obstacles to improvement in public schools marked by persistent low performance.

2. How To Think About Accountability Sanctions: Theory and Prior Evidence

2.1 The theoretical rationale for accountability

The market for primary and secondary education is not likely to conform to the assumptions underlying the standard prediction of economic efficiency in competitive markets. Producers – schools – enjoy some degree of market power, and consumers – children and their parents – may not be perfectly informed about the quality of the education they receive; even if

they are perfectly informed, acting on that information generally introduces significant transaction costs – exchanging one house for another in a different attendance zone.

A variety of public school reform initiatives can be conceptualized within this framework. Efforts to introduce school choice via vouchers and similar mechanisms aim to reduce transaction costs, which by enabling well-informed consumers to act on differences in quality reduce the potential role of market power. Requiring schools to report basic information on student performance improves the information available to consumers.[1]

Accountability sanctions – rewards or punishments meted out to schools on the basis of student performance data – are rationalized by the recognition that introducing choice and information may be insufficient to motivate schools to operate efficiently. They attempt to substitute punitive incentives for market incentives. Following basic principal-agent theory, punishments or rewards can align agent (school) behavior with principal (government) objectives, so long as they are in fact consequential to the agent and the principal has access to an informative signal of the agent's effort.

In a traditional principal-agent model, the incentive itself is not expected to have any impact on production ex post. Accountability sanctions may depart from this model, in that they often involve interventions that may have treatment effects on students in a school. Thus even in the absence of incentive effects, accountability sanctions could improve educational outcomes.

There are potentially significant obstacles to the successful implementation of an accountability sanction system. While it is reasonable to think that sanctions such as bonus payments or dismissal would be consequential to agents, sanctions more akin to educational interventions might have weaker incentive effects. The signals used to determine whether to

[1] To be sure, the quality of this information may be limited, to the extent that non-school factors determine student achievement or that standardized test scores are noisy signals (Kane and Staiger 2002).

apply sanctions might correlate poorly with principal objectives, because they are statistically noisy or influenced by extraneous factors. When sanctions take the form of educational interventions, those interventions may have null or negative treatment effects.

The No Child Left Behind Act introduced a system of escalating sanctions into all American public schools receiving Title I funding. A summary of the system appears in Table 1. For schools serving students in grades 3-8, evaluation is based on standardized test score performance in reading and math. To be identified as making "Adequate Yearly Progress" in a subject, and thus avoid progressing further in the escalating sanction regime, the proportion of all students meeting a state-defined proficiency threshold must exceed a discrete cutoff. The same criterion is further applied to a series of subgroups in the school, defined by race, ethnicity, program participation, English proficiency status, or disability. [2] Under some circumstances, schools may qualify for limited exemptions to the proficiency requirements.[3]

The set of sanctions applied to a school in any given year depends on that school's recent track record of performance. The sanctions threatened or imposed by the system include permitting students transfers to other public schools, offering supplemental education services (tutoring) to disadvantaged students, "corrective action," and the formulation or implementation of a restructuring plan.

2.2 Existing evidence on the impact of specific sanctions

[2] The AYP standard also incorporates a participation requirement, mandating that at least 95% of enrolled students are tested.

[3] The *confidence interval* exception effectively allows subgroups to be considered as meeting the target if their observed proficiency rate is close enough to the predetermined threshold that the statistical hypothesis that the "population" rate equals or exceeds the threshold cannot be rejected. The *safe harbor* exception exempts schools from negative sanctions if the subgroups with subpar proficiency have demonstrated a tangible improvement from one year to the next.

Many of the sanctions incorporated into NCLB had been elements of state- or district-level accountability programs in earlier years. Since its implementation, several studies have examined the impact of the entry-level sanctions incorporated into the law. This brief literature review will examine existing evidence on four categories of sanctions: school choice sanctions, personnel-focused sanctions, instructional interventions, and positive rewards.[4]

2.2.1 School choice sanctions

Sanctions involving the introduction of school choice, either through the use of vouchers for private school attendance or transfers to other public schools, are motivated by the theory of market incentives. School choice serves as the first layer of sanction in the NCLB regime and in Florida's state-specific accountability program.

Florida's A+ Accountability and School Choice Program identified low-performing schools and prioritized them for a range of educational interventions, while offering vouchers to students in schools that received multiple low-performing ratings in a short timespan. The criteria for Florida's school ratings permits the use of regression discontinuity methods to identify the impact of exposure to the voucher threat. Several studies have documented positive effects of the voucher threat on student performance (Greene and Winters 2003; West and Peterson 2006; Chiang 2009). Schools exposed to the threat implement meaningful changes in instructional practices (Rouse et al. 2013), leading to broad improvements that carry over to low-stakes subjects (Winters, Trivitt and Greene 2010). Craig et al. (2013) find positive effects of accountability threats on per pupil expenditure in a Texas system where the primary sanction was

[4] In addition to these studies of individual accountability sanctions and threats, there have been two noteworthy studies comparing regimes with and without accountability systems to infer their total impact. Hanushek and Raymond (2005) report better student performance in states with accountability systems using pre-NCLB data. Dee and Jacob (2011) exploit the introduction of NCLB to perform a difference-in-difference analysis and similarly report positive results.

a choice threat. On the more negative side, Florida's voucher threat appears to lead schools to suspend students more frequently around standardized testing time (Figlio, 2006).

Prior studies of the school choice threats embedded in NCLB have found mixed results. Chakrabarti (forthcoming) and Springer (2008) report positive effects in Wisconsin and an unidentified state respectively, concentrated in the subject areas or among the students where improvements would most strongly mitigate the choice threat. By contrast, Hemelt (2011) finds negative effects of choice threats on performance in Maryland, and West and Peterson (2006) conclude that NCLB's choice threats are inconsequential in Florida given the state's use of vouchers in its own accountability system.

The mixed results in the literature on school choice threats is complemented by a contentious and similarly mixed literature on the effects of school choice itself (Rouse 1998; Hoxby 2000; Bettinger 2005; Bifulco and Ladd 2006; Angrist, Bettinger, and Kremer 2006; Sass 2006; Rothstein 2007; Chakrabarti 2008a, 2008b). Null effects of school choice might reflect the low take-up of offered transfers, or unsophisticated choice behavior rooted in the complexity of education as a consumer good.

2.2.2 Personnel-focused sanctions

School restructuring-type sanctions, involving the replacement of personnel, or in some cases the complete closure or reconstitution of a school, comprise the ultimate sanction in the NCLB regime. The sanction that precedes the development of a restructuring plan, corrective action, can be thought of as a "light" version of the sanction. Restructuring sanctions were also a component of some state- or district-level accountability systems predating NCLB. Of these, the system introduced in Chicago Public Schools in 1996 has received the most attention. The

introduction of Chicago's system has been associated with significant improvements in test scores (Jacob, 2005). At the same time, studies of Chicago Public Schools under accountability have unearthed a wide range of unintended and possibly detrimental side effects, ranging from outright cheating (Jacob and Levitt 2003) to diverting instructional attention from students far above or below the proficiency threshold (Neal and Schanzenbach, 2010).[5] Analysis of a comparable reconstitution threat in Virginia showed significant changes in school lunch menus during standardized test season (Figlio and Winicki, 2005).

2.2.3 Instructional Interventions

There have been several studies of the impact of supplemental education services (SES)– NCLB-mandated tutoring – on student performance, arriving at widely varying conclusions (Chatterji et al. 2006; Burch 2007; Zimmer et al. 2007; Heinrich et al., 2010; Munoz et al. 2012). One consistent pattern emerging from the literature is that take-up rates tend to be low, in the single digits in some cases, and that positive effects are conditional on extensive use of SES (Ryan and Fatani, 2005; Rickles and Barnhart 2007). From a causal inference perspective, this pattern introduces concerns of self-selection. One qualitative study of SES in multiple settings concludes that instructional shortcomings may also plague many programs, which are provided by a wide array of contractors (Good et al. 2011). The literature, in summary, suggests that the potential exists for positive impacts of SES, but that a variety of issues might limit this potential in practice.

2.2.4 Rewards in Place of Negative Sanctions

[5] Neal and Schanzenbach also report negative effects of NCLB-style sanctions, which similarly focus on the proportion of students attaining a proficiency standard.

Accountability sanctions need not focus on punishing underperforming schools; in some cases systems have been designed to deliver rewards at the other end of the performance spectrum. The use of bonus payments for outstanding teaching has been associated with stronger student performance in India (Muralidharan and Sundararaman 2011) and the United States (Ladd 1999; Figlio and Kenny 2007). Cash bonuses have been shown to reduce teacher turnover in disadvantaged schools (Clotfelter et al., 2008); however performance-based bonus programs may exacerbate turnover problems if disadvantaged schools receive them less often (Clotfelter et al., 2004). In North Carolina, the setting for this study, schools that barely fail to receive a performance-based bonus in one year demonstrate significant improvements in the next (Ahn and Vigdor, 2013). At the other end of the spectrum, other studies have shown no positive impact of individual- or group-level performance incentives in rigorous randomized trials (Springer et al., 2012; Yuan et al., 2013).

In summary, the evidence base for effectiveness of any form of accountability sanction on improving student performance is decidedly mixed. Most existing studies focus on a single sanction in isolation, removed from an NCLB-style system where the imposition of one sanction is conflated with the threat of the next. While there is considerable heterogeneity in methodological rigor across studies, there is no general pattern of more rigorous studies – those employing randomized trials, regression discontinuity, and like methods – reporting more or fewer positive effects.

3. Data and Methods

We seek to analyze whether the imposition or threat of NCLB sanctions significantly alters student performance. To do so, we make use of student and school-level data from North

Carolina, covering the period from inauguration of the NCLB system in the 2002/03 school year to 2009/10. These data are a mix of public records and anonymized data made available to researchers by the North Carolina Educational Research Data Center (NCERDC). To analyze the impact of sanctions on student performance, we use longitudinally linked data on students who attend the same school in consecutive years. This requires us to focus on schools that serve students in grades 3-8.

At present, AYP results from five school years are combined with test score results from those years and the following years to form the heart of our dataset. The dataset consists of over 8,000 school-by-year observations and over 1.7 million individual-level computations of test score gains.[6]

3.1 Regression Discontinuity

A simple comparison of student performance across schools subject or not subject to sanctions in a given year will be prone to yielding misleading results, since the sanctions are applied exclusively to schools in which proficiency rates are relatively low. To overcome this obstacle, we take advantage of the structure of the NCLB sanction system, in which schools with very similar patterns of student proficiency might be subject to very different sanctions if their small differences place them on either side of the AYP threshold. Under certain assumptions, this regression discontinuity analysis reveals an estimate of the average treatment effect of interventions applied at the AYP threshold, local to the set of schools that find themselves close to that threshold.

[6] Sample sizes for reading and math analyses vary because in some small schools in some years we are unable to match test score performance for any students from one year to the next.

In practice, it is not trivial to identify the set of schools whose performance places them within an arbitrarily small neighborhood around the AYP threshold. Schools can miss AYP in a subject if even one student subgroup posts a sufficiently low proficiency rate. Moreover, because of the confidence interval and safe harbor exemptions, schools can make AYP even when numerous subgroups fail to post proficiency rates that place them above the state-defined standard.

To identify schools that barely meet or miss the AYP standard, we first compute for each subgroup in each school an "effective" proficiency threshold, defined as follows:

Effective threshold = min(boundary for CI exemption, boundary for SH exemption)

Where CI and SH stand for the two exemptions made available to schools. The boundary for the CI exemption is determined by the confidence interval formula employed by the state and the number of students assessed. The boundary for the SH exemption is determined by the subgroup's performance in the prior academic year.

We then compare the actual proficiency rate exhibited by a subgroup within a school with the effective threshold for that subgroup:

Subgroup gap = actual proficiency – effective threshold

The assignment variable for our RD analysis is defined as the minimum subgroup gap, across that set of subgroups with a sufficiently large number of tested students. In North Carolina, the threshold is set at 40 students. When the assignment variable is positive, the school

qualified for AYP; when it is negative the school fails to make AYP. The cutoff for AYP, which we refer to as the minimum effective threshold, is zero.

Note that the assignment variable is not necessarily determined by the subgroup that performs worst in absolute terms. The worst-performing subgroup may qualify for AYP by an exemption in situations where the school as a whole fails to qualify. This would occur when the worst-performing group is numerically small and qualifies under the confidence interval exemption (and the next-worst performing group is large and does not), or when the worst-performing group has shown significant improvement over the prior year, thereby qualifying under safe harbor (and the next-worst performing group does not).

To assess the impact of exposure to NCLB sanctions and threats, we evaluate the performance of students in the affected school in the subsequent academic year. The dependent variable of interest is the change in standardized test score over the period in which the treatment is administered. Thus, when a school meets or misses the AYP cutoff in year *t*, we examine the effects by observing growth in test scores between the end of year *t* and the end of year *t*+1.[7] In our baseline specifications, we include only those students observed attending the school in question in both years. In alternate specifications, we include students who attend the school in year *t* regardless of the school attended in year *t*+1, so long as the students remain enrolled in any North Carolina public school. This will permit us to include the hypothesized treatment effect of offering transfers, and other family school choice behavior occasioned by the failure of a child's school to make AYP. We caution, however, that these short run effects may be dominated by disruption effects that dissipate over time (Hanushek, Kain and Rivkin 2004).

[7] Note that our use of test score gains varies from NCLB's focus on levels. Research has established a strong link between test score gains and longer-run outcomes including earnings (Chetty, Friedman and Rockoff, 2011).

Regression discontinuity analysis can be performed either parametrically or nonparametrically. For our analyses, we use the Hahn, Todd, and van der Klaauw (2001) nonparametric specification, which entails the estimation of local linear regressions to fit a smooth function to either side of the discontinuity. While there is no explicit functional form choice associated with the local linear regression, in practice the shape of the smooth function is heavily influenced by choice of bandwidth in the local linear regression – put simply, the number of data points used to estimate a slope in the neighborhood of each individual data point. We report results for a variety of bandwidths centered around the "optimal" bandwidth as defined by Imbens and Kalyanaraman (2009).[8]

The nonparametric analysis is complicated by the nature of the data: we have individual-level records for purposes of computing treatment effects, but assignment to the treatment occurs at the school rather than individual level. In our analyses below, we address this problem by collapsing the individual-level data to the school level, weighting observations in the school-level estimation by the number of individual-level observations used in computing school-specific means of the dependent variable and covariates.[9]

3.2 Analyzing the Impact of Specific Sanctions

Our primary goal is to assess the impact of individual NCLB sanctions. As such, a simple RD analysis on the entire dataset is unlikely to be informative, as it represents the average effect of a number of different threats and sanctions. For most of our analyses, then, we focus

[8] We replicate each reported nonparametric RD estimate using a more traditional OLS-based parametric procedure and report the results in footnotes below. The OLS regressions control for either a linear, quadratic, cubic or quartic function of the assignment variable, with terms permitted to vary on either side of the point of discontinuity, as well as controls for student gender, English proficiency status, race, disability status, and school enrollment.

[9] We also estimate models using individual-level data, using a bootstrapping procedure to approximate clustered standard errors. While point estimates are comparable with both procedures, the clustered standard errors are larger, consistent with the notion that clustering is a conservative solution to the problem of grouped data. We additionally estimated unweighted school-level models which yield qualitatively similar results.

our attention on sets of school/year observations at risk for the same sanction or threat. For example, to analyze the impact of being threatened with the requirement to offer transfers, we will begin with the set of schools with no history of missing AYP prior to year *t*, and compare student performance in year *t*+1 across schools that make or miss AYP in year *t*.

While restriction to schools with similar AYP histories can help refine the sanction or threat under analysis, we are still left with the difficulty that each failure to make AYP after the first results in the simultaneous imposition of all previous sanctions and the threat of the next sanction in the sequence shown in Table 1. Thus in the absence of additional information it is impossible to separate the effect of offering transfers from the effect of the threat of offering tutoring.

In practice, we take advantage of a waiver to standard NCLB policy that was offered to seven North Carolina districts beginning in the 2006/07 school year, and expanded statewide beginning in 2008/09.[10] The waiver permitted affected districts to reverse the order of the first two NCLB sanctions, implying that schools would be required to offer supplemental services first and transfers second. Thus, under the assumption that the effects of each threat and sanction are time invariant, we can uniquely identify not only the impact of being threatened with offering transfers, but the threat of offering supplemental services, the transfers themselves, the supplemental services themselves, and the threat of corrective action.

Of course, there is no guarantee that these effects will be time-invariant. Revelation of information about the effects of sanctions might well alter responses to the threat of those sanctions.

[10] The seven districts are Burke County, Cumberland County, Durham County, Guilford County, Moore County, Northhampton County, and Pitt County. Three of these districts are among the state's ten largest.

3.3 RD diagnostics

A standard concern with regression discontinuity analysis is that agents may have some capacity to manipulate the assignment variable. This would appear to be a valid concern in this application, as the effective thresholds for each subgroup are in theory calculable at the beginning of each school year. There is at least some evidence that school personnel might manipulate testing data in a high-stakes setting (Jacob and Levitt, 2003).

To assess this and other potential threats to validity, Figures 1-3 perform basic diagnostics appropriate to RD analysis (Imbens and Lemieux 2008; Lee and Lemieux, 2010). Figure 1 begins by showing a basic density plot of our data, pooled across all available years, for reading and math. In these plots, we would be particularly concerned if we found evidence of a mass on the right hand side of the minimum effective threshold relative to the left hand side.[11] The figure demonstrates that the AYP cut point falls very nearly in the middle of the observed data, and that the density is in fact slightly greater to the immediate right of the cut point. The histogram bar to the immediate right is about 20% higher than the one immediately to the left. A formal statistical test for a significant break in density at the cut point fails to reject the null hypothesis of no difference (test statistic 0.094, standard error 0.079, see McCrary 2008).

To further investigate, Figure 2 plots average student characteristics for schools as a function of the minimum subgroup gap. Included characteristics include percent nonwhite, percent receiving free or reduced lunch, and percent limited English proficiency. While it is clear that schools with lower performance tend to serve more disadvantaged students, there is no

[11] It should be noted that this analysis differs from a typical RD analysis in one important respect: some of the schools "at risk" for sanctions have already had sanctioned applied. To the extent that there is a positive treatment effect of any sanction, this may lead to a scenario where the density to the right of the discontinuity exceeds that on the left. Coupled with the insignificant McCrary test statistic reported below, this increases our confidence that manipulation is not a significant concern in these data.

evidence of a significant trend or break in trend in any of these student characteristics at the AYP threshold.

Finally, Figure 3 verifies that there is indeed a discontinuous change in the probability of treatment at the point where schools cross the AYP threshold. The magnitude of the probability, revealed in nonparametric specifications based on local linear regression, is 0.854 for reading and 0.828 for math. The "fuzziness" of the discontinuity, or the imperfections in our ability to forecast which schools will make or miss AYP in a given year, stems from the fact that factors other than test score performance are incorporated into AYP designations. Schools must meet attendance and test participation benchmarks.

Following our discussion of results below, we address an additional potential concern with regression discontinuity analysis in this application. Given that schools must exhibit low performance to miss AYP, there is some concern that simple mean reversion may lead us to estimate spurious positive impacts on test score growth in the following year. In theory, the RD design should address this concern, as we have no reason to think that the degree of mean reversion would jump discontinuously at the AYP threshold. Nonetheless, we report the estimates below of pseudo-RD analyses designed to determine whether mean reversion is a concern.

4. Results

In all results tables below, the signs obtained from RD analysis have been reversed, so that we might interpret the reported effects as the impact of being exposed to NCLB sanctions or threats, rather than the impact of avoiding them.

4.1 The main effect of failure to make AYP

Table 2 reports regression discontinuity estimates of the effect of meeting AYP criteria using all school-year observations. As noted above, these estimates conflate a wide variety of NCLB sanctions and threats. We report estimates derived from local linear regressions using the "optimal" bandwidth (Imbens and Kalyanaraman 2009), but note in the table whether results are statistically significant in specifications using twice or half this bandwidth. These specifications, as well as those reported below, condition on student race, free/reduced price lunch participation, and English proficiency status.

For reading test scores, estimation with optimal bandwidth yields an exceptionally small point estimate, on the order of two-hundred-thousandths of a standard deviation. The effect is estimated precisely enough to rule out any positive or negative impacts greater than one percent of a standard deviation. Estimation using alternate bandwidths from 50% to 300% of the optimal value yields similarly insignificant results. The point estimate remains extremely small when we include students who transfer away from the school in the sample.[12]

When analyzing math test scores, evidence is suggestive of a positive impact of failure to make AYP. Using the optimally selected bandwidth, the point estimate suggests that students in schools that miss AYP post test score gains of 2% of a standard deviation relative to students that do not make AYP. The effect is marginally significant, with a *p*-value of 0.057. Estimation with larger bandwidths yields larger and more significant results, while estimation with smaller bandwidths produces smaller estimates – in some cases reverse-signed – that are statistically

[12] Estimation using parametric OLS regressions similarly yields insignificant results, with point estimates on either side of zero and at most 0.013 in absolute value.

insignificant. Introducing students who transfer out in the sample increases the point estimate slightly. Ultimately, then, we consider the math results suggestive but fragile.[13]

4.2 Effects of specific sanctions and threats

The consequences of failing to make AYP vary dramatically depending on a school's history, and for this reason aggregate effects such as those reported above are quite likely to misrepresent the impact of exposure to particular sanctions, or the threats of those sanctions. This section summarizes the results of a number of additional nonparametric RD specifications, which by restricting the sample to students in schools with comparable AYP histories report the local average treatment effect of exposure to a certain combination of sanctions and/or threats of sanctions.

The estimates in Table 3 are based on sample restrictions to those schools not currently subject to any threats or sanctions in the NCLB regime, because they have consistently met the AYP criteria since the inception of the policy or for two consecutive years. Among these schools, the failure to make AYP exposes them to the threat of the first sanction in the regime, which varies by year and district in North Carolina. These schools face an incentive to make AYP in the following year in order to avoid the imposition of this first sanction.

Across the board, point estimates suggest modest positive impacts of failing to meet the AYP standard for the first time. For reading test scores, the estimated effects are 2% of a standard deviation or below, and fail to reach statistical significance at standard bandwidth whether considering exposure to the tutoring or transfer threats. The point estimates are slightly larger in magnitude with *p*-values of 0.074 when using a large bandwidth. Including students

[13] Estimation using parametric OLS regressions yields positive and significant point estimates ranging from 0.021 in a quadratic specification to 0.061 in a cubic specification. Estimates using a quartic functional form are not statistically significant.

who transfer away from the school in the sample leaves point estimates within the range already established in other specifications.[14]

Math test scores appear to improve more substantially when exposed to the first NCLB threat. The effect is most pronounced with exposure to the transfer threat, where point estimates indicate a statistically significant 4.7% of a standard deviation improvement in test scores. This particular result is sensitive to bandwidth choice; larger and more significant at higher levels, but reverse-signed and insignificant at half the optimal bandwidth. By contrast, the impact of exposure to the tutoring threat is somewhat smaller at 2.8% of a standard deviation, but statistically significant and robust to bandwidth choice.[15] In both cases, including students who transfer away from the school in the sample yields larger point estimates, suggesting that the failure to make AYP – even when not attached to a particular sanction – leads some families to make school choices resulting in better math instruction for their children. Intriguingly, as we will see below, this effect is not apparent in specifications when the failure to make AYP leads to the transfer sanction.

The estimates in Table 4 focus on schools that are under threat of the first sanction in the AYP regime. These schools have missed the AYP criteria in a particular subject in a recent year. Comparison of those who make or miss AYP in the reference year reveals the local treatment effect of being exposed to the first sanction and threatened with the second, relative to being threatened with that sanction.

[14] Estimation by parametric OLS yields a similar mix of insignificant results, with the exception of linear or quadratic specifications in the sample of schools exposed to the SES threat, where point estimates are on the order of 0.04.

[15] Estimation by parametric OLS yields a mix of positive and negative insignificant coefficients among schools exposed to the transfer threat; estimates are on the order of 0.05, significant and positive in most specifications for schools exposed to the SES threat. The OLS and nonparametric results thus are not entirely consistent with one another, which suggests caution in interpreting the impact of initial failure to make AYP on subsequent test score gains.

Normally, the first sanction applied in the NCLB system is a requirement to offer students transfers to higher-performing public schools in the same district. Students in schools required to offer transfers – and threatened with further sanctions – post modest improvements in standardized reading test scores, relative to students in schools that barely avoid this sanction. The effect is not statistically significant using the standard bandwidth selection, with a *p*-value of 0.149. Across numerous alternate bandwidths, the point estimate is similar in magnitude while never attaining statistical significance. In math, point estimates are of a similar order of magnitude, but attain significance at the 10% level when using bandwidths at least twice the optimal level. Evidence thus suggests some possible modest positive effects of being exposed to the transfer sanction and supplemental education services threat. As noted above, introducing transferred students into the sample does not produce a significant change in point estimates. This suggests that the mechanism of improving student performance through access to better public schools is at best canceled out by the negative impact of switching schools in the short run. Together with the results in Table 3, they suggest that savvy parents exercise school choice options in response to the initial information that their school has failed to make AYP, rather than the explicit offer of transfers.

In district/year observations where the order of sanctions was reversed, failure to make AYP a second time exposed schools to the supplemental education services sanction and the threat of offering transfers. Exposure to the tutoring sanction and transfer threat has no discernible impact on reading or math test scores, with or without transferred students in the sample. The associated standard errors are relatively large, as by the time the sanction reversal

was implemented most schools at high risk for entry into the NCLB sanction system were already beyond the level of the first sanction.[16]

Table 5 show estimates derived from a sample of schools that are already subject to one NCLB sanction and whose exposure to a second sanction, along with the threat of the third in the sequence, depends on their performance in the reference year. The RD estimates thus reveal the local average treatment effect of the second sanction, along with the threat of the third, relative to persisting in a state where only the first sanction is in place.

In the standard NCLB sequence, requirements to offer supplemental education services are layered on top of transfers when schools miss AYP a third time. Estimates of the effect of introducing this sanction (and the threat of further action) are statistically insignificant in both math and reading, with or without transferred students included in the sample. In districts and time periods with a reversed sanction sequence, exposure to transfers as a second sanction associates with negative effects, marginally significant and robust in the case of math test scores, less robust in the case of reading. At the optimal bandwidth, point estimates suggest that introducing the transfer sanction, along with the threat of further action, leads to a 4.4% of a standard deviation reduction in math test scores. Including transferred students in the analysis actually produces more negative, and statistically significant, point estimates – indicating once again that the offer of transfers fails to yield short-term improvements among the students who take them up.

To summarize results to this point, the strongest association between failure to make AYP and subsequent test score performance occurs among those schools not yet exposed to any actual sanctions. There is some modest evidence of positive impacts associated with introducing

[16] Parametric estimates of the effect of exposure to the first NCLB sanction, using four functional forms, two different dependent variables, and two different initial sanctions, for a total of 16 specifications, yield exactly one coefficient significant at the 5% level.

transfers as a first sanction, but in districts that implemented transfers as a second sanction the impact is strongly negative. Put together, the results indicate that the threat of exposure to the tutoring sanction – rather than the implementation of any sanction per se – has the strongest effect on student test scores. The absence of any effects of SES on test score growth is consistent with much of the existing literature, and might reflect either low take-up or poor average quality of implemented services.

Table 6 reports the results of exposure to higher-order sanctions in the NCLB regime. Beyond the first two years, it is no longer possible to distinguish the impact of exposure to one sanction from the impact of exposure to the threat of the next sanction. Nonetheless, these specifications produce intriguing findings.[17]

Upon failing to make AYP a fourth time, schools are required to take "corrective action." Point estimates associate this sanction with test score improvements in both reading and math, but neither are statistically significant using either the default bandwidth or close alternatives.[18]

Schools that fail to make AYP a fifth time are required to formulate a restructuring plan, which they will be required to implement in the event they are unsuccessful a sixth time. Results indicate that schools exposed to this sanction exhibit declines in reading test score performance of nearly 6% of a standard deviation, a result that is both close to the standard statistical significance threshold (p=0.053 using the optimal bandwidth) and fairly robust across bandwidth selections. Results for math test scores are statistically insignificant across a wide range of bandwidths.

[17] We do not report the results of alternate specifications including transferred students here, as there are no noteworthy deviations from the included results.

[18] Separate estimation for those schools that implemented sanctions in the standard order – transfers first, then tutoring – reveals larger, statistically significant impacts on both reading and math test score gains, with coefficients on the order of 7-8% of a standard deviation. In the full sample, these gains are offset by more modest and in some cases reverse-signed impacts among those schools that implemented tutoring first. This may in fact be more of a timing effect than a sanction order effect – those schools that reached the corrective action stage earlier in the process may have been better able to forecast which corrective actions would improve test scores.

Finally, schools that fail to make AYP a sixth time are required to implement their restructuring plans. The estimated effects of restructuring on reading scores are sensitive to bandwidth choice, with the optimal bandwidth yielding an estimated 2.6% of a standard deviation improvement (p=0.066), and smaller bandwidths producing larger and more significant estimates. Math results are more consistently robust, with point estimates indicating a statistically significant 5.5% of a standard deviation improvement in test scores at the optimal bandwidth, and only modest variation at alternate bandwidths.[19]

In summary, across all the sanctions in the NCLB regime, the ultimate penalty, implementation of a restructuring plan, shows the strongest evidence of positive test score impacts across the board.[20] No other sanction yields estimates of the same magnitude in both subjects; the threat of exposure to the sanction system comes closest.

4.3 Inspecting a possible mechanism for restructuring effects: staff turnover

Restructuring plans constitute a heterogeneous treatment. It is not clear from the reduced-form results above what components of restructuring are responsible for the observed positive effects. The specifications in Table 7 attempt to shed some light on the mechanism underlying the effect, by examining how exposure to restructuring influences staff turnover. Since the outcomes analyzed here are based on school-level measures and not individual test scores, the RD analyses are not weighted here.

[19] Replicating each of the Table 6 specifications with parametric OLS regressions yields a similar pattern: there are no consistently significant results except in the case of the restructuring sanction. The OLS coefficients in math specifications for restructuring are similar in magnitude, but fail to attain statistical significance when using functional forms involving at least a quadratic term. The parametric specifications thus appear to suffer from a power problem relative to the nonparametric estimates.

[20] As noted above, exposure to the corrective action sanction appears to have even stronger positive impacts among schools that proceed through the sanction system in the standard order.

The first specification examines the rate of teacher turnover, defined as the proportion of teachers observed in year *t* who do not return in year *t*+1. Coefficients indicate that the turnover rate in schools exposed to restructuring increase by three to five percentage points. The baseline rate of turnover for schools at risk for restructuring is around 20%, so these effects indicate a 15 to 25 percent increase in the turnover rate associated with the restructuring sanction.

In defining principal turnover, we are faced with the issue that the baseline rate of principal turnover is very high in part because principals move between schools. It is unclear whether to attribute a move between schools as an involuntary effect of restructuring. We therefore define principal turnover more narrowly to be a scenario where the principal in year *t* is not observed as a principal in any North Carolina public school in year *t*+1. Estimates in this case span a broad range and are estimated somewhat imprecisely, attaining significance at the 10% level with smaller bandwidths. The estimates suggest a 6-18 percentage point increase in principal turnover. The baseline rate of turnover in at-risk schools is on the order of 30 percent, indicating that these estimates point to a 20-60 percent increase in the turnover rate.

While these results do not exhaustively probe all possible mechanisms for the improvements associated with restructuring, they are consistent with the notion that leadership and personnel change are an important component.

4.4 Effect heterogeneity: by initial student test performance

One concern with school accountability systems based on proficiency rates is that they provide incentives for schools to focus only on those students near the proficiency threshold, focusing less attention on those either well above or below the cutoff (Reback 2008; Neal and Schanzenbach 2010). In theory, the treatment effect estimates presented above could represent a

mix of very positive impacts for students near the threshold and very negative impacts for students far from it. Table 8 assesses this concern by reporting treatment effect estimates specific to students' initial test score quartile.[21] An exclusive focus on moving students above the proficiency threshold would lead schools to focus on the bottom two quartiles, shifting resources away from students in higher quartiles.

There is at least some evidence to indicate that schools respond to accountability sanctions by focusing more resources on lower-performing students. In schools barely missing the AYP criteria for the first time, students with below-average initial test scores post math test score increases 5% of a standard deviation greater, relative to below-average students in schools barely meeting the AYP criteria. Point estimates suggest a diminished effect among students in the second-highest quartile, and no effect whatsoever among the highest-performing students.

There is also evidence that below-average students post significant reading test score increases in schools that barely miss the AYP criteria for a second consecutive year, relative to those that miss the first time but barely make it the second. These estimates are even greater in magnitude, up to 11% of a standard deviation, and once again diminish at higher initial achievement levels, to the point where top-quartile students have comparable performance on either side of the discontinuity.

Table 6 above showed that students post significantly higher test score gains in schools required to undergo restructuring, after missing AYP for a sixth consecutive year. The final row in Table 8 shows that the beneficial effects of restructuring, estimated somewhat imprecisely here, appear to be spread fairly evenly throughout the initial test score distribution. Math test score gains, in particular, accrue at the rate of 4-6% of a standard deviation across all four

[21] Table 7 omits analysis of school/year observations subject to reversed order of the first two accountability sanctions. Results in this subsample are uniformly insignificant.

quartiles. Point estimates are in fact slightly larger for students with higher initial test scores; estimates for the lower quartiles are not statistically distinguishable from zero. Point estimates for reading test score gains by quartile are positive across the board, but never significantly different from zero.

There is one set of anomalous results worth mentioning. Lower-performing students in schools required to formulate, but not implement, a restructuring plan post significantly worse test score improvements than their counterparts in schools that avoid this sanction. This pattern replicates the more basic results in Table 6 above. It is possible that schools tasked with formulating a restructuring plan must devote resources to that activity that would otherwise be productive in the instruction of reading to low-performing students. And of course, it is also possible that this anomaly is just that – attributable to an outlier in the data. Aside from this anomaly, the remaining results in Table 8 reveal no significant patterns of note.

Overall, then, our results partly support those of existing literature showing that schools focus resources on students near the proficiency threshold when subject to accountability pressure. In contrast to this prior literature, we find no evidence that accountability leads schools to "leave behind" their highest-performing students; results for these students are near zero but never significantly negative. Moreover, we find that school restructuring leads to broad-based improvements in math test scores.

The lack of negative effects for inframarginal students might reflect the simultaneous operation of North Carolina's own accountability system, which is based on test score increases rather than proficiency and offers cash bonuses to personnel in high-performing schools. Given tradeoffs between qualifying for AYP by increasing proficiency and qualifying for a bonus by promoting test score growth across the initial test score distribution, school personnel may have

responded rationally by reallocating resources only to the point where high-performing students were not harmed on the margin. High-performing students may also have avoided relative test score declines to the extent their parents substituted for resources allocated away from them within the school system. To the extent that this occurred, the lack of test score declines among high-performers should not be considered evidence that the gains to low-performing students came at no cost.

4.5 Effect heterogeneity: among critical subgroups

For many schools in our sample, the failure to make AYP in a given year can be attributed to the performance of a single subgroup, such as Limited English Proficiency students or students receiving free or reduced price lunch. In these schools, there is a clear incentive to focus efforts on improving the performance of just that subgroup. If the improvement is sufficiently large, the group's proficiency rate does not even need to make the NCLB target, under the safe harbor exemption. Table 9 examines the impact of failure to make AYP, using regression discontinuity analysis, on a sample comprised exclusively of students belonging to the subgroup responsible for the school's failure to make AYP.

Consistent with a pattern of rational focus on the problematic subgroup, and with earlier studies of NCLB, point estimates of the effects of failure to make AYP, estimated using the Imbens-Kalyanaraman optimal bandwidth, tend to be more positive than analogous estimates for the full sample. The pattern holds in 11 of 14 cases, comparing Table 9 point estimates to those in earlier tables. The overall impact of failure to make AYP on the math scores of the problematic subgroup, for example, is twice the magnitude of the effect in the overall population. Despite the larger magnitudes of these coefficients, they only rarely achieve statistical

significance. The underlying sample size of students in the problematic subgroup is, of course, considerably smaller than the sample of students in schools overall, leading to larger standard errors.

The three exceptions to the larger-effect-size pattern in Table 9 include one case where the Table 9 coefficient is wrong-signed relative to its earlier counterpart. In a second case, both the original coefficient and the Table 9 coefficient are negative-signed, with the Table 9 version bearing a larger magnitude. The final case pertains to the effect of the ultimate sanction, school restructuring, on math achievement. Here, the point estimate in the full population is larger than the effect in the problematic subgroup – the only case where point estimates have the same sign and the point estimate in the problematic subgroup is closer to zero. While it may be inappropriate to assign much interpretation to this pattern, given the imprecision of the underlying estimates, this is consistent with the notion that mandated changes in school leadership among schools that have persistently failed to make AYP yields benefits to all students, and not only those who would be targeted by a rational administrator.

4.6 Effect heterogeneity: SES effects among eligible students

Supplemental education services were required to be offered to students eligible for free or reduced price lunch in schools that had missed AYP in the same subject for three consecutive years (two years following North Carolina's reversal of sanction order). Tables 4 and 5 above failed to report any evidence that SES led to improved test score outcomes; in fact the preponderance of point estimates are negative. Table 10 re-examines the effects of SES by restricting attention to those students actually eligible to receive the services. Even in this subsample, there is no evidence of a significant impact of SES on student performance.

4.7 Falsification Tests

As noted above, our procedure may yield spurious positive effects of missing the AYP cutoff if the tendency to mean revert towards higher performance is, for any reason, significantly greater on the left-hand side of the discontinuity point. To assess this possibility, we estimated two sets of pseudo-regression discontinuity analyses. In the first set, we estimated our models using a cut point 15 percentage points above the actual proficiency cutoff in each school. This point lies towards the upper tail of the proficiency distribution as shown in Figure 1, implying that a pattern of discontinuous mean reversion would produce spurious negative results. The second set uses a cut point 15 percentage points below the actual cutoff, closer to the lower tail of the distribution. Discontinuous mean reversion would produce spurious positive results in this sample.

Table 11 presents our basic optimal-bandwidth results from preceding tables, along with results using an identical procedure at the pseudo-discontinuity points. These point estimates will tend to be large in absolute magnitude, because they are derived from a Wald estimator with a relatively small denominator. That is to say, there is not much evidence of a discontinuity in treatment at either of these points, so any effect observed in the outcome variable will be increased substantially. In analyzing these results, then, we are more interested in patterns of sign and significance than the actual value of the point estimates *per se*.

Significance patterns reveal few signs of concern. Across forty falsification tests, we obtain 5 point estimates significant at the 10% level, 3 at the 5% level, and one at the 1% level. In each case, these exceed the expected number of false positives under the null hypothesis of no effects, but by no more than one. Moreover, the signs of the significant coefficients do not

always concord with the hypothesized effects of mean reversion. In the first set of tests, where we would be concerned about spurious negative mean reversion, only two of three significant coefficients are in fact negative. In the second test, where our concern is positive mean reversion, both significant coefficients are in fact negative.

Looking more broadly, the first set of falsification tests yield 12 negative point estimate in 20 tests, a plausible outcome under the null hypothesis of no effect in any test. The second set provides an even split across positive and negative point estimates. In summary, using two sets of regression discontinuity analyses where we might suspect mean reversion to be an even greater concern, we find no evidence to validate these concerns.

5. Conclusions

Previous research has raised a number of concerns about school accountability regimes. Attaching high stakes to test scores, and in particular to proficiency rates on tests, may lead to a number of resource allocation decisions that have the short-term impact of avoiding sanctions but no impact, or in some cases even a negative impact, on long-term measures of student productivity or well-being. While this study is incapable of providing a holistic estimate of the impact of accountability relative to a counterfactual of no accountability (see Hanushek and Raymond 2005 or Dee and Jacob 2011 for such exercises), we assess whether the sanctions associated with the nation's most comprehensive accountability system impact a fundamental measure of student learning – year-over-year improvement in test scores.

We find evidence that schools respond to the threat of initial sanctions in ways that contribute to student learning, and that the ultimate sanction in the NCLB system – restructuring – yields even greater contributions to learning. Consistent with prior research, we find some

evidence that these gains are concentrated among students at or below the proficiency threshold. Unlike prior research, we find that these gains are not accompanied by significant losses among students well above the proficiency threshold – which may illustrate the importance of a secondary accountability system measuring growth rather than accountability. We also find evidence that schools focus in particular on improving the performance of the subgroup that caused the school to miss AYP.

The strong positive effects of restructuring – which appear to be broad, rather than focused on the lowest-performing students – indicate that school management or leadership problems constitute the single greatest obstacle to improved student performance. Indeed, the existence of management or leadership problems might help explain why lesser sanctions – including a targeted tutoring intervention associated with no positive effects even among the targeted students – have no demonstrable effect. School leaders who cannot formulate strategies to improve performance cannot be expected to react constructively to incentives to do so. The association of positive effects with entry into the NCLB sanction system might also be interpreted as evidence of effective leadership reactions – among those schools at the threshold of AYP, the ones with effective leadership are the ones who find ways to increase proficiency rates and exit the sanction system, leaving only schools with less-effective leaders behind.

Even with this admittedly speculative interpretation of the results, one should not jump to the conclusion that a No Child Left Behind-style sanction regime is an effective way to identify schools in need of leadership change and implement that change. Presumably, more targeted strategies could achieve the same results without the need to wait for six years' worth of standardized test results. These results do highlight the need for further research into the nature of effective school leadership and management.

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Table 1: The NCLB sanction regime

Number of consecutive years missed AYP in same subject	Sanction
1	None; placement on watch list, develop school improvement plan
2	District must offer transfers (with transportation) to higher-performing public schools in the same district. School listed as "needs improvement."
3	District must offer supplemental education services to students qualifying for free or reduced price lunch
4	School must undertake "corrective action." Corrective actions may include staff/leadership changes, curriculum changes, instructional time changes, or appointment of outside advisors.
5	School must formulate a restructuring plan.
6	School must implement the restructuring plan. Restructuring must involve either conversion to a charter school, replacement of the principal and most staff, state takeover, contracting with another entity to manage the school, or similar major change to school governance.

Table 2: Regression Discontinuity Estimates of the Effect of Missing AYP

	Reading (n=8,264)	Math (n=8,266)
At optimal bandwidth	-0.00002	0.0180*
	(0.0056)	(0.0095)
Half optimal bandwidth	0.0061	-0.0117
	(0.0071)	(0.0137)
Twice optimal bandwidth	-0.0025	0.0282 ***
	(0.0053)	(0.0081)
At optimal bandwidth,	-0.0004	0.022**
including transferred students	(0.431)	(0.011)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. Bandwidth determination is by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.
*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 3: RD estimates of the impact of exposure to the threat of the first NCLB sanction

First sanction is transfers	Reading (n=3,241)	Math (n=3,244)
At optimal bandwidth	0.0203	0.0473**
	(0.0140)	(0.0204)
Half optimal bandwidth	0.0086	-0.0197
	(0.0218)	(0.0322)
Twice optimal bandwidth	0.0200*	0.0681***
	(0.0111)	(0.0170)
At optimal bandwidth,	0.0160	0.0759***
including transferred students	(0.0118)	(0.017)
First sanction is SES	Reading (n=958)	Math (n=957)
At optimal bandwidth	0.0161	0.0285*
	(0.0136)	(0.0163)
Half optimal bandwidth	0.0119	0.0350*
	(0.0170)	(0.0204)
Twice optimal bandwidth	0.0227*	0.0241*
	(0.0127)	(0.0147)
At optimal bandwidth,	0.0221*	0.0338**
including transferred students	(0.0131)	(0.0153)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. Bandwidth determination is by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.
*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 4: RD estimates of the impact of exposure to the first NCLB sanction/threat of second

First sanction is transfers (second SES)	Reading (n=1,486)	Math (n=1,486)
At optimal bandwidth	0.0178	0.0144
	(0.0124)	(0.0181)
Half optimal bandwidth	0.0139	0.0076
	(0.0176)	(0.0254)
Twice optimal bandwidth	0.0185	0.0256
	(0.0106)	(0.015)
At optimal bandwidth, including transferred students	0.0194	0.0092
	(0.0105)	(0.0200)
First sanction is SES (second transfers)	**Reading (n=177)**	**Math (n=177)**
At optimal bandwidth	-0.0064	-0.0069
	(0.0377)	(0.0337)
Half optimal bandwidth	-0.0291	-0.0184
	(0.0445)	(0.0423)
Twice optimal bandwidth	-0.0126	0.0069
	(0.0353)	(0.0306)
At optimal bandwidth, including transferred students	0.0055	-0.0056
	(0.0381)	(0.0314)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. Bandwidth determination is by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.
*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 5: RD estimates of the impact of exposure to the second NCLB sanction/threat of third

Second sanction is SES (transfers in place)	Reading (n=727)	Math (n=727)
At optimal bandwidth	-0.0244	-0.0219
	(0.0268)	(0.0368)
Half optimal bandwidth	-0.0519	-0.064
	(0.0398)	(0.0546)
Twice optimal bandwidth	-0.0234	-0.0409
	(0.0217)	(0.0394)
At optimal bandwidth, including transferred students	-0.0191	-0.0384
	(0.0259)	(0.0473)
Second sanction is transfers (SES in place)	**Reading (n=310)**	**Math (n=310)**
At optimal bandwidth	-0.0289	-0.0442
	(0.0244)	(0.0243)
Half optimal bandwidth	0.005	-0.0495
	(0.0327)	(0.029)
Twice optimal bandwidth	-0.0429**	-0.0394
	(0.0208)	(0.0234)
At optimal bandwidth, including transferred students	-0.0495**	-0.0528*
	(0.0216)	(0.0261)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. Bandwidth determination is by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.
*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 6: RD estimates of the impact of higher order sanctions and threats

Corrective action, threat of restructuring plan	Reading (n=549)	Math (n=549)
At optimal bandwidth	0.035	0.0231
	(0.0261)	(0.0247)
Half optimal bandwidth	0.0337	-0.0065
	(0.0392)	(0.0319)
Twice optimal bandwidth	0.0148	0.0311
	(0.0213)	(0.0215)
Restructuring plan, threat of restructuring	Reading (n=442)	Math (n=442)
At optimal bandwidth	-0.0558**	0.0278
	(0.0288)	(0.0258)
Half optimal bandwidth	-0.052	0.0486
	(0.0466)	(0.0339)
Twice optimal bandwidth	-0.0937*	0.0197
	(0.0516)	(0.0238)
Restructuring	Reading (n=367)	Math (n=367)
At optimal bandwidth	0.0264*	0.0547**
	(0.0144)	(0.0263)
Half optimal bandwidth	0.0562***	0.0664*
	(0.0202)	(0.035)
Twice optimal bandwidth	0.0205	0.0609**
	(0.0132)	(0.0242)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. Bandwidth determination is by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.
*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 7: RD estimates of the impact of restructuring on staff turnover

Teacher turnover	
At optimal bandwidth	0.049**
	(0.025)
Half optimal bandwidth	0.056*
	(0.029)
Twice optimal bandwidth	0.033*
	(0.019)
Principal turnover	
At optimal bandwidth	0.134*
	(0.071)
Half optimal bandwidth	0.187*
	(0.104)
Twice optimal bandwidth	0.059
	(0.058)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variables defined in the text. Bandwidth determination is by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.
*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 8: RD estimates of the effect of NCLB sanctions and threats on test score gains, by quartile of initial test score

Sanction/	Reading				Math			
Threat	Lowest	2nd	3rd	Highest	Lowest	2nd	3rd	Highest
General Impact of	-0.0023	-0.0137	-0.0026	0.0019	0.0363***	0.0146	0.0194*	0.0015
Missing AYP	(0.0130)	(0.0138)	(0.0103)	(0.0066)	(0.0116)	(0.0103)	(0.0103)	(0.0114)
None/	-0.0054	-0.0251	-0.0036	0.0014	0.0548**	0.0506**	0.0308	0.0039
Transfers	(0.0388)	(0.0375)	(0.0247)	(0.0173)	(0.0272)	(0.0236)	(0.0244)	(0.0261)
Transfers/	0.1045**	0.1127***	0.0506*	0.0195	0.0404	0.0237	-0.0077	0.0028
SES	(0.0448)	(0.0421)	(0.0277)	(0.0200)	(0.0322)	(0.0299)	(0.0308)	(0.0239)
SES/	-0.0897	-0.0808	0.0050	-0.0065	0.0007	0.0160	0.0282	0.0090
Corrective Action	(0.0707)	(0.0603)	(0.0348)	(0.0223)	(0.0382)	(0.0413)	(0.0363)	(0.0391)
Corrective Action/	0.0378	0.0427	0.0305	0.0222	0.0360	0.0042	0.0531*	-0.0021
Restructuring Plan	(0.0373)	(0.0444)	(0.0256)	(0.0182)	(0.0259)	(0.0294)	(0.0311)	(0.0299)
Restructuring Plan/	-0.0788**	-0.0936**	-0.0373	-0.0349	0.0424	-0.0000	0.0078	0.0308
Restructuring	(0.0379)	(0.0387)	(0.0290)	(0.0231)	(0.0346)	(0.0300)	(0.0341)	(0.0380)
Restructuring/	0.0346	0.0232	0.0087	0.0172	0.0418	0.0424	0.0598**	0.0605*
None	(0.0217)	(0.0194)	(0.0179)	(0.0206)	(0.0299)	(0.0298)	(0.0298)	(0.0313)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. All estimates utilize the optimal bandwidth as determined by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.

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Table 9: RD estimates of the effect of NCLB sanctions on students in the critical subgroup

Sanction/ Threat	Reading	Math
General Impact of Missing AYP	0.009	0.038**
	(0.025)	(0.015)
None/	0.043**	0.053
Transfers	(0.022)	(0.036)
Transfers/	0.029	-0.043
SES	(0.049)	(0.045)
SES/	0.025	0.040
Corrective Action	(0.032)	(0.037)
Corrective Action/	0.068	0.044
Restructuring Plan	(0.062)	(0.046)
Restructuring Plan/	-0.203	0.095**
Restructuring	(0.138)	(0.038)
Restructuring/	0.044	0.043
None	(0.075)	(0.042)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. All estimates utilize the optimal bandwidth as determined by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.

*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 10: RD estimates of the impact of SES, Free/Reduced Lunch Eligible Students Only

	Reading	Math
SES implemented as second sanction	-0.0393	0.0437
	(0.0501)	(0.0382)
SES implemented as first sanction	-0.0040	0.0083
	(0.0454)	(0.0357)

Note: Standard errors in parentheses. Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. All estimates utilize the optimal bandwidth as determined by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.

*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level.

Table 11: Summary of Falsification Tests

Sanction/ Threat	Actual result		Pseudo-RD at 15% above threshold		Pseudo-RD at 15% below threshold	
	Math	Reading	Math	Reading	Math	Reading
Overall	0.018*	0.0002	-1.09***	0.061	0.0097	0.130
None/ Transfers	0.047**	0.021	-13.26	-0.864	-0.040	-0.036
None/ SES	0.029*	0.016	-0.042	-0.844	0.083	0.032
Transfers/ SES	0.014	0.018	0.004	-0.049	0.068	0.258
SES/ Transfers	-0.007	-0.006	0.328	0.333	-0.586	-0.628**
SES/ Corrective Action	-0.022	-0.024	-0.090	0.007	0.125	-0.111
Transfers/ Corrective Action	-0.044	-0.029	0.132*	0.046	-0.077	0.121
Corrective Action/ Restructuring Plan	0.023	0.035	-0.038	-0.038	-0.132	-0.114
Restructuring Plan/ Restructuring	0.028	-0.056**	1.86	-0.065	-.423*	-0.392
Restructuring/ None	0.055**	0.026*	-0.125**	-0.048	0.034	0.293

Note: Unit of observation is school/year, weighted by number of students used to compute dependent variable. Dependent variable is mean year-over-year change in standardized test score in the indicated subject between the year of AYP status determination and the following year. All estimates utilize the optimal bandwidth as determined by the Imbens-Kalyanaraman algorithm. Specifications factor out school-level aggregate characteristics including percent minority, percent limited English proficient, and percent free/reduced lunch.
*** denotes an estimate significant at the 1% level; ** the 5% level; * the 10% level

Figure 1: Density plot by assignment variable

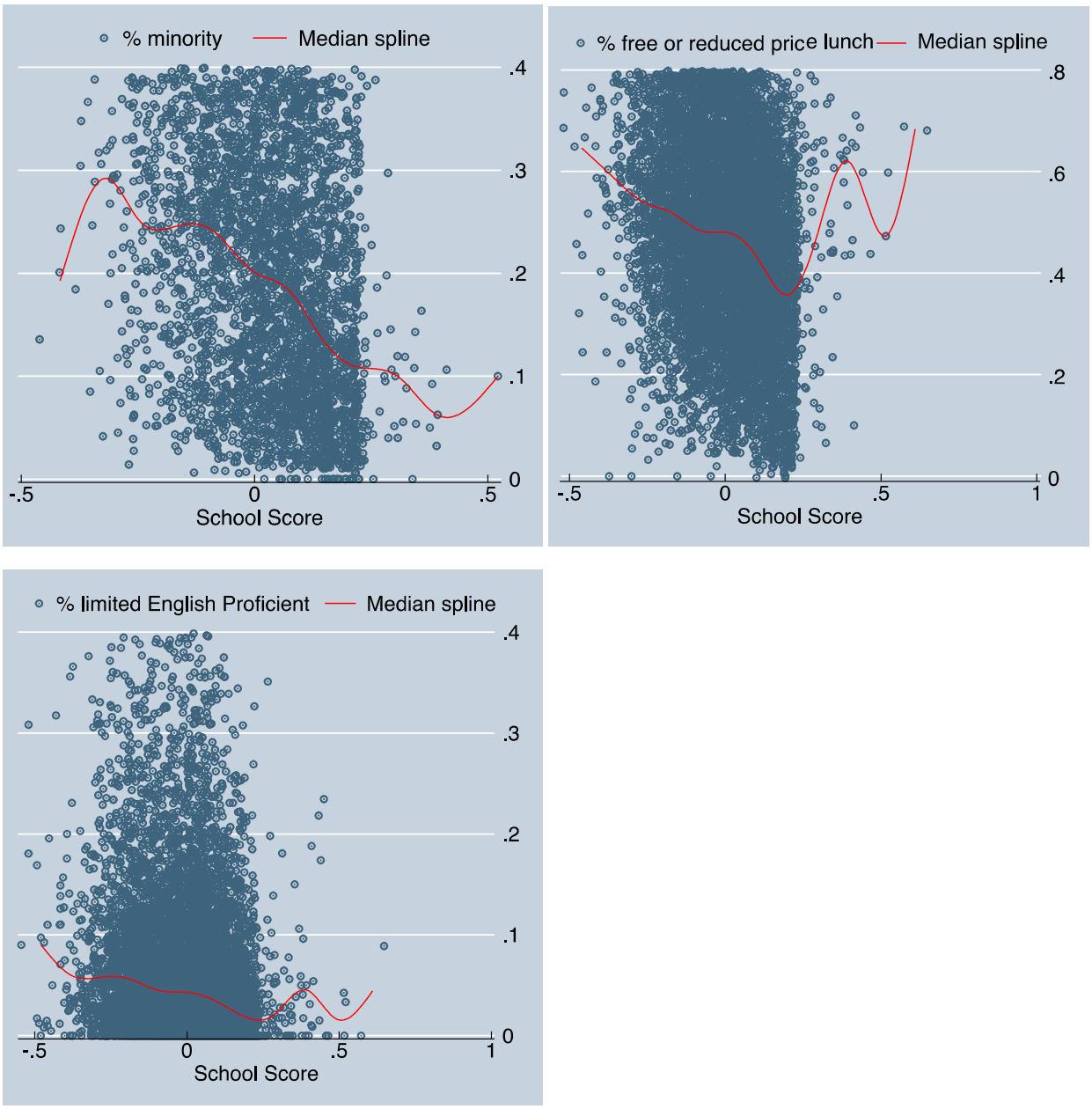
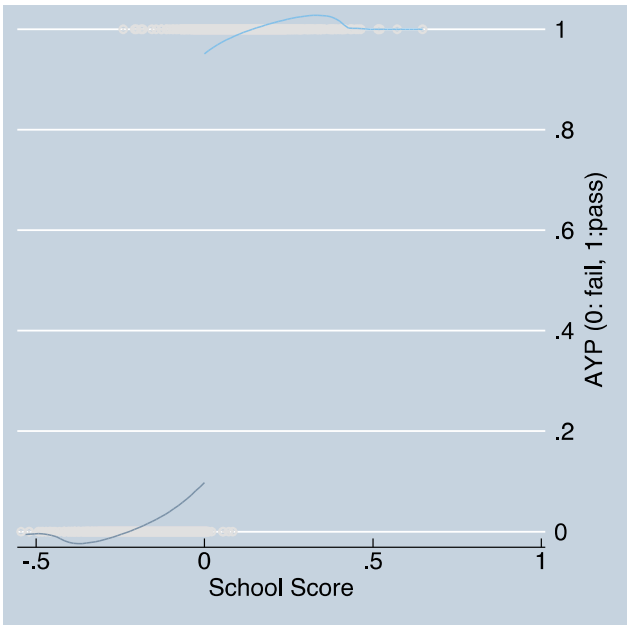
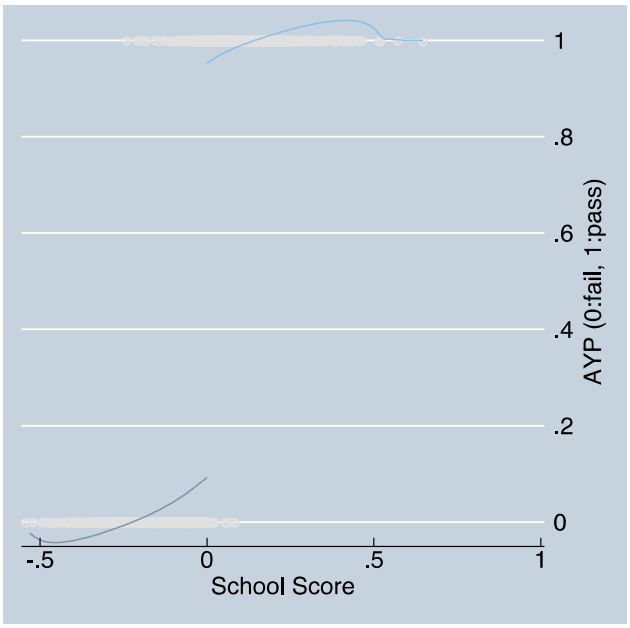


Figure 2: Covariates by assignment variable



A. Reading



B. Math

Figure 3: Probability of meeting the AYP criteria by assignment variable