Ohio School Turnaround Interventions: Impacts on Achievement, Attainment, and Administration

A Report Prepared for the Ohio Department of Education

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I. Executive Summary

The Every Student Succeeds Act (ESSA) of 2015 requires states to identify and improve their lowest performing schools. This study assesses the impact of recent school improvement initiatives in Ohio to inform the state's plans for meeting this ESSA requirement. Specifically, the analysis estimates the impacts of the federal School Improvement Grant (SIG) program and Priority school interventions Ohio implemented as part of its No Child Left Behind Act (NCLB) waivers. Both initiatives can be characterized as "school turnaround" efforts because they sought to produce rapid and lasting improvements in school quality by requiring significant changes to many aspects of schools' educational delivery, such as the replacement of principals and teachers and the use of data to drive instructional and managerial decision-making. Schools qualified for these interventions if they ranked in the bottom 5 percent of eligible schools in terms of student proficiency rates or if they were high schools with graduation rates below 60 percent.

This study evaluates the impact of the first two rounds of the SIG program and the first round of Priority school identification. The state identified the first two cohorts of schools eligible for SIG awards in 2009 and 2010, respectively, and districts could apply for these awards to help them implement one of four federally-approved school improvement models in the eligible schools. The awards were distributed over three years (state fiscal years 2011-2013 for cohort 1 and fiscal years 2012-2014 for cohort 2) as schools implemented the models—primarily the SIG Transformation and SIG Turnaround models.[1] The state identified the first set of Priority schools in 2012. All Priority schools were required to implement a turnaround model much like the SIG Turnaround and SIG Transformation models if they had not previously received a SIG award. Efforts to turn around the first wave of Priority schools lasted from the 2012-13 through the 2014-15 school years.

Consistent with the goals of these interventions, the analysis focuses on their impacts on math and reading achievement and graduation rates. The analysis also considers the mechanisms that might explain changes in school quality, such as the principal and teacher turnover that the SIG and Priority models require. The results of the analysis are as follows:

1) *SIG awards had a positive impact on student achievement and graduation rates.*

SIG eligibility and, more specifically, the receipt of a SIG award had a large positive impact on school quality as measured by annual student achievement growth in math and reading. The analysis indicates that students in schools that received SIG awards experienced achievement advantages of around 0.10-0.15 standard deviations annually, which is the equivalent of approximately 60 extra "days of learning" each year if one assumes a 180-day school year. These improvements to annual school "value added" occurred primarily during the three years of the interventions and taper off afterward, but we generally cannot rule out substantively large advantages in school quality in those later years.

The annual achievement gains accumulate over time such that student achievement levels ultimately are much higher than they would have been without the interventions. By one estimate, by 2014 the average test scores of a SIG school's students were around 0.55 standard deviations higher than they would have been without the intervention. That is the equivalent of

[1] Both of these models are considered school "turnaround" models in the more general sense of the term.

students moving from the 5th percentile (the cutoff to identify low-performing schools) to approximately the 14th percentile on the achievement distribution.

There is also evidence that SIG awards had a positive impact on high school graduation rates. Our most conservative and credible estimates indicate that graduation rates for the first SIG cohort were 7 to 9 percentage points higher than they would have been without the interventions. Estimates for the second SIG cohort ranged from negligible to positive.

2) *SIG awards generally led to less principal and teacher turnover in the long term.*

Personnel turnover was already so great in these low-achieving schools that SIG eligibility often failed to measurably increase overall turnover rates. Contrary to what one might expect, actually receiving an award led the first cohort of SIG schools to experience significantly less turnover than other SIG-eligible schools that did not receive an award. Although there is some evidence that the second cohort of SIG schools receiving an award experienced more principal turnover, on average SIG awards led to less principal and teacher turnover.

3) *The SIG Turnaround model was more disruptive in the short term than the SIG Transformation model.*

The SIG Turnaround model required more personnel replacement than the SIG Transformation model. Accordingly, the analysis indicates that the SIG Turnaround model led to annual principal and teacher turnover rates that were 20 to 30 percentage points higher. This disruption corresponded to a negative impact on the achievement of students in attendance when schools were granted the awards. On the other hand, there is also some evidence that implementing the SIG Turnaround model may have had a more positive initial impact on school "value added", which also captures the annual achievement growth of students who were not there when a school was initially identified as SIG eligible.

4) *Priority school identification had no clear impact on school quality as measured by student achievement, but there is some evidence of a positive impact on graduation rates.*

Priority school interventions generally did not have discernable impacts on school quality as measured by student achievement growth. On the other hand, the few Title I-served high schools included in the analysis reveal that Priority designations had positive impacts on graduation rates of between 3 and 8 percentage points.

5) *Priority school identification led to more principal and teacher turnover, but it did not have a negative impact on the achievement of students who experienced the disruption.*

Students who attended schools at the time they were identified as Priority schools generally did not experience achievement declines. Indeed, the achievement effects we detected were sometimes positive for these students.

The research designs we employed provide confidence that these were the causal impacts of the SIG and Priority school interventions. That is because the methods we used entail comparing the performance trajectories of schools that were similar in every way except whether or not they were eligible for or required to implement the interventions. Consequently, we were able to observe how schools receiving the interventions would likely have performed had they not received them.

There are many possible reasons for the results above. For example, the research we review in the report indicates that whether or not replacing principals and teachers leads to improvements in school quality depends on the relative quality of the incoming personnel. Turnover is generally harmful to student achievement—at least in the short term—unless incoming teachers are of sufficiently greater quality to compensate for the negative disruptive effects. Similarly, there is some evidence that providing districts and schools with technical assistance can help, but that surely depends on the nature of the assistance, the needs of particular schools and districts, and the extent to which the assistance imposes an administrative burden that distracts from a school's core mission. It is conceivable that SIG's large positive impact (particularly relative to Priority interventions) is due to the funding provided or the fact that districts could decide to apply for a grant and participate in the program if they anticipated a marginal benefit from doing so. For example, schools and districts identified as SIG eligible could forgo obtaining a grant and implement their own strategies if they did not think SIG models would help them get out of the bottom five percent of schools.

Overall, the study provides convincing evidence that interventions such as the SIG turnaround models have the potential to improve school quality very quickly, which is consistent with the theory underlying school turnaround reforms as well as research in other contexts. We also find, however, that initial positive impacts dissipated after the first 2-3 years of implementation, which is inconsistent with the hope that turnaround interventions lead to long-term improvements in school quality. There is suggestive evidence that some more modest positive effects persisted 4-5 years later, but we are unable to discern whether or not that is truly the case. Beyond that, the report is necessarily limited to describing some of the differences in the nature of these interventions and, via a literature review, providing some insights as to how they might affect school quality. We leave it to administrators and policymakers to determine which mechanisms are likely to play out in a particular context.

II. Introduction

The Every Student Succeeds Act (ESSA) of 2015 requires states to identify and improve their lowest performing schools. This study evaluates the impact of recent school "turnaround" initiatives in Ohio to inform the state's plans for meeting this ESSA requirement. Specifically, the analysis estimates the impacts of the federal School Improvement Grant (SIG) program and Priority school interventions Ohio implemented as part of its No Child Left Behind Act (NCLB) waivers. Both initiatives sought to produce rapid and lasting improvements in school quality by requiring significant changes to many aspects of schools' educational delivery—particularly their leadership and staffing, as well as their use of data to drive instructional and managerial decision-making. Additionally, in both cases, eligible schools qualified for interventions if they ranked in the bottom 5 percent of eligible schools in terms of student proficiency or if they were high schools with graduation rates below 60 percent. [2]

The primary purpose of SIG and Priority school interventions in Ohio was to improve school quality as captured by student achievement and attainment. Thus, the analysis operationalizes school quality primarily in terms of schools' contributions to student performance on math and reading tests and, to a lesser extent, school-level graduation rates.[3] Additionally, because the interventions emphasized rapid and substantial changes to school operations—particularly by requiring changes in leadership and staff—the analysis assesses the extent to which the interventions brought about such changes. Finally, in part to examine the extent to which changes in the composition of students might explain some of the school-level educational outcomes we report, the analysis examines the impact of SIG and Priority school interventions on student mobility and school closure.

The strict performance cutoffs determining SIG eligibility and priority school identification (i.e., the 5^{th} percentile in terms of student proficiency or a 60 percent graduation rate for high schools) allowed us to estimate the causal impact of these programs on school administration and educational outcomes using a Regression Discontinuity (RD) design. The RD design essentially entails comparing schools that were very close to but on either side of the performance threshold determining SIG eligibility or Priority school status. For example, we compared the performance trajectories of schools with 2012 proficiency rates that put them just above the 5^{th} percentile to those with proficiency rates that put them just below the 5^{th} percentiles (i.e., those schools identified as Priority schools). Provided that the assumptions of the design are met (something that we test as part of the analysis) schools close to but on either side of the performance cutoff should be essentially identical in every respect except whether they received Priority school interventions.

Additionally, because not all schools eligible for SIG ultimately applied for and received grants, we compared the performance trajectories of SIG-eligible schools that did and did not receive SIG awards. We emphasize the results of those comparisons only when the statistical analysis indicates that the schools are sufficiently comparable. Most notably, we do not report these results for Priority schools in the main text, as all Priority schools were required to implement the turnaround model and, thus, there was no set of similar, low-performing schools to which we could compare them.

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[2] The pool of eligible schools differed between the first SIG cohorts and the identification of "priority" schools associated with the NCLB waiver.

[3] The report de-emphasizes the latter in part because it is difficult to attribute changes in graduation rates to the efforts of a particular school as opposed to changes in the composition of students over time, for example. Additionally, the samples of high schools for which we can implement the RD design is small.

The report is organized as follows. First, it provides context with a very brief history of school turnaround interventions and a review of research examining their impacts (Section III). Second, it provides a thorough description of the interventions that are the focus of this study, beginning with the first two rounds of SIG (Section IV) and then the first round of Priority school identification (Section V). The report then provides an overview of the research design, including the primary data and statistical modeling strategies (Section VI), followed by a review of the results (Section VII) and concluding thoughts on the implications of these results (Section VIII).

It is important to note that our research design and results sections focus on providing intuition for the methodologies we employed and the results we obtained. Readers who want more technical details and a more thorough description of the results should consult the technical appendix (Section IX).

III. Research Relevant to School Turnaround Initiatives

"Whole-school" or "comprehensive" school reform (CSR) programs steadily gained popularity late in the 20th century. They were based on the notion that coordinated efforts to improve schools as a whole are more likely to have an impact than piecemeal efforts targeting particular aspects of educational delivery. Beginning in the late 1980s, the federal government shifted its efforts from targeted programs aimed at improving the achievement of impoverished students to CSR programs targeting entire schools serving impoverished students. These federal grant programs were quite specific about which interventions qualified as CSR, stipulating that they must involve evidence-based strategies for improving everything from school management and instruction to fostering parental and community involvement. Research examining the impact of various CSR programs on student achievement found mixed results. Though many studies were of limited quality (Herman et al., 2008), there is good evidence that some CSR models had a positive impact on achievement (e.g., see Bifulco, Duncombe, and Yinger, 2005; Borman et al., 2003; Gross, Booker, and Goldhaber, 2009).

The federal government subsequently stepped up these efforts by incentivizing the implementation of more aggressive "turnaround"[4] models of reform, most notably by distributing billions of dollars via a revamped School Improvement Grants (SIG) program and state Race to the Top (RttT) grants, as well as offering waivers from the accountability provisions of NCLB. Unlike CSR, what has come to be called school turnaround initiatives are meant to produce rapid improvements in school quality via dramatic changes to school operations. In particular, the four federally defined SIG models entailed closing a school completely (the Closure model), restarting a school as a charter school or one managed by an independent management organization (the Restart model), or, to various degrees, reconstituting a school's leadership and instructional staff through mandatory and data-driven hiring and firing processes (the SIG Transformation and SIG Turnaround models). For example, the Turnaround and Transformation models both required replacing a school's principal, and the Turnaround model goes further by allowing a school to rehire no more than 50 percent of teachers.

Research indicates that these turnaround models could lead to improvements in student achievement. Research on school closure, for example, indicates that closure can benefit students in

[4] "Turnaround" is the school improvement strategy that SIG and Priority school models sought to implement. Confusingly, it also is the label assigned to a specific SIG model. We capitalize all referends to the specific SIG model and use lower-case "turnaround" to refer to the more generic reform strategy.

failing schools provided that affected students switch to schools of sufficiently higher quality to compensate for the disruption that closure can introduce (Brummet, 2014; Carlson and Lavertu, 2015; Carlson and Lavertu, 2016). The SIG Closure model is consistent with this research in that it requires that students be directed to higher-performing schools. The required leadership and managerial changes in the other three SIG models could also yield benefits. Principal quality is particularly variable in high poverty schools and appears to have a substantively significant impact on student achievement (Branch, Hanushek, & Rivkin, 2012; Grissom et al., 2015). Thus, schools with poor-performing principals could benefit significantly. Additionally, providing new principals with greater managerial discretion could enable them to respond to their organizational environments, which research suggests is particularly important in the education sector (Bloom et al., 2015). For example, more discretion might enable principals to recruit and retain high-quality teachers (Ladd, 2011), which is the most important known school-based factor determining student achievement (Hanushek, 2011). Indeed, teacher turnover has been shown to increase student achievement if new teachers are of sufficiently greater quality than the teachers they replace (Adnot et al., 2016)

Some other strategies that the SIG models require have also been shown to be effective in some contexts. For example, extended instructional time and data-driven managerial and instructional decision-making seem to correlate with school quality both domestically and internationally (Angrist et al., 2013; Bloom et al., 2015; Dobbie and Fryer, 2013). And providing schools and districts with technical assistance to implement school improvement strategies—such as data-driven decision-making—was found to improve the achievement of students in low-performing California districts (Strunk and McEachin, 2014; Strunk, McEaching, and Westover, 2014).

On the other hand, the effectiveness of principal and teacher replacement depends on the supply. Replacing experienced principals with novices could be problematic, as principal inexperience has been shown to have a significant negative impact on educational outcomes (Clark, Martorell, & Rockoff, 2009). Similarly, there is increasing evidence that teacher experience can have a significant impact on student educational outcomes (e.g., Harris and Sass 2011). Thus, if the supply of quality teachers is low or recruitment is difficult, as tends to be the case in low-achieving, high-poverty urban and rural districts (e.g., see Boyd, Lankford, & Wyckoff, 2007; Cowen et al., 2012; Jackson, 2009; Clotfelter, Ladd, & Vigdor, 2007, 2010), new teachers could very well be of comparable or lower quality than those they replace. Or perhaps schools obtain higher quality teachers from elsewhere in their districts, thereby having a negative effect on other district schools. Moreover, even if teachers are replaced with new teachers of comparable quality, such teacher “churn” has itself been shown to have a negative impact on student achievement (Atteberry et al., 2016). Finally, although the SIG Turnaround model introduces more managerial discretion for principals, researchers have suggested that working with districts rather than specific schools within them is more likely to be effective in part because districts have more managerial discretion (e.g., see Schueler et al, 2016).[5]

Although the work reviewed above is undoubtedly relevant to turnaround initiatives, there is little published research that actually estimates the impact of recent federal school improvement programs using rigorous research designs. There have been some in-depth case studies of the implementation of all SIG models (e.g., see Le Floch et al., 2016), but the U.S. Department of Education’s Institute for Education Sciences has not yet released its report of SIG’s impact on student educational outcomes. And independent researchers have only begun to publish their analyses. To our knowledge, there are no published papers that examine the impact of closure in

[5] It is unclear the extent to which SIG schools coordinate well with the districts.

the context of SIG, RttT, or NCLB waivers. There is one published study that examines the SIG Restart option in Boston and New Orleans that identifies substantively significant positive achievement effects in math and reading when schools convert to charter schools (Abdulkadiroğlu et al., 2016), but it is worth keeping in mind that the study focuses on relatively high-performing charter sectors (e.g., see CREDO, 2015). Indeed, unpublished papers that examine the Restart option alongside other school improvement models in California and Tennessee find that there were no positive achievement effects when school management is taken over by charter management organizations (Dee, 2012; Zimmer et al., 2016).

More relevant to this report, Abdulkadiroğlu et al. (2016) also found that SIG Turnaround interventions in Boston yielded achievement gains comparable to those they found from charter conversion provided that there was sufficient staff reconstitution. Schools that implemented turnaround strategies that involved less staff turnover yielded smaller achievement gains. This latter finding is consistent with a published study of Los Angeles turnaround interventions (Strunk et al., 2016a) and a working paper focused on the impact of SIG grants in California as a whole (Dee, 2012). They found that the more disruptive SIG Turnaround model, which mandated that at least 50 percent of teachers be dismissed, was the only SIG model with a positive effect on student achievement. It is important to note, however, that these studies typically employ few years of data, so it is unclear whether the achievement effects persist after the initial 2-3 years after implementation. Additionally, although Dee (2012) employs a regression discontinuity design, he employs a school-level achievement index. Thus, changes in school-level achievement could be due to changes in student composition or the manner in which the index is compiled, as opposed to changes in school effectiveness.

Heissel and Ladd's (2016) evaluation of turnaround efforts in North Carolina is perhaps the most rigorous study available. They employ a regression discontinuity design, student- and staff-level administrative data, and, importantly, teacher survey data that enables them to estimate the causal impact of federal school improvement models. Like Dee (2012), and as per the federal SIG models, they found that the implementation of SIG Turnaround models indeed led to principal and teacher turnover. They also found that teachers reported more professional development, more communication with parents, a greater focus on tests, and more administrative burdens in terms of required meetings and paperwork, for example. And they found that receiving a SIG grant led buildings to have a higher concentration of students receiving free- and reduced-price lunches. Finally, they found that the interventions had a negative average impact on school-level proficiency rates (which could be due to the student compositional changes mentioned in the previous paragraph). It is important to note, however, that in a separate analysis of these efforts in North Carolina, Henry and Guthrie (2015) found immediate and significant positive impacts on school quality as measured by student achievement growth, though these initial effects diminished over time.

There are a number of additional unpublished papers in circulation that utilize regression discontinuity and other quasi-experimental designs to estimate the causal impact of SIG and similar turnaround models, such as those used for Priority schools as part of NCLB waivers. On balance, they seem to find null or positive achievement effects.[6] Like the studies we describe above, however, these studies are generally limited to no more than three years of post-treatment effects. This limitation is particularly important because research in many contexts has found that initial

[6] For example, Dougherty and Weiner (2015), Papay (2015), and Ruble (2015) recently presented such papers at the Association for Public Policy Analysis and Management (APPAM) 2015 annual conference. There were also papers in very early stages presented at the 2016 conference of the Association for Education Finance & Policy.

positive or negative achievement effects tend to dissipate (e.g., see Strunk et al., 2016b; Favero and Rutherford, 2016). On the other hand, there is some evidence that a turnaround model implemented in many Cleveland and Cincinnati schools might have had lasting impacts on achievement (see Player and Katz, 2016). These studies employ good research designs, but those designs are generally less convincing than those we employ in this report. To our knowledge, this study is the first to examine the long-term achievement impacts of turnaround models in Ohio using the rigorous RD design. And this is the first study to estimate the impact on school quality as measured by the achievement gains of students whether or not they directly experienced the reforms.

IV. Ohio SIG Schools[7]

School turnaround efforts intensified in the fall of 2009 when the Obama Administration revamped the federal SIG program. Eligible districts could apply for SIG funds to turn around poor-performing schools using one of the four new SIG models. There were three types of schools eligible for the funds: Title I-served schools under NCLB's "school improvement" process that were either among the lowest achieving five percent or secondary schools with a five-year graduation rate less than 60 percent ("Tier 1"); Title I-eligible secondary schools that were either among the lowest achieving five percent or had a five-year graduation rate less than 60 percent ("Tier 2"); and Title I-served schools under NCLB's "school improvement" process that were not identified as Tier 1 ("Tier 3"). The Tier 1 and Tier 2 schools were those labeled as "persistently low-achieving" and those required to implement one of the four SIG turnaround models if their districts applied for and received SIG funds on their behalf. However, districts receiving SIG funds could also spend them in Tier 3 schools. ODE awarded the SIG grants of between $50,000 and $2 million per building through a competitive application process that required districts to demonstrate their commitment and capacity to implement the models in the identified buildings. District applications included detailed budgets and narratives for each building.

Consistent with federal guidelines, the Ohio Department of Education (ODE) determined the lowest five percent of schools under NCLB's "school improvement" status using an average of two proficiency calculations: a weighted proficiency rate in math and reading for each building as of the most recent school year—2008-09 (state fiscal year 2009, or FY09) for Cohort I and 2009-10 (FY10) for Cohort II—and a five-year average of this weighted proficiency rate over the last five school years (FY05-FY09 for Cohort I and FY06-FY10 for Cohort II). ODE then rank-ordered schools based on a "combined proficiency rate" that weighted these two proficiency calculations equally. Additionally, ODE also ranked all high schools under NCLB's "school improvement" status based on an average graduation rate across five years of data (FY04-FY09 for Cohort I and FY05-09 for Cohort II). These ranking rules were applied separately for the Tier 1 and Tier 2 schools in each cohort. Most of the analysis below focuses on estimating the effect of SIG on Tier 1 schools because too few Tier 2 schools were identified for the analysis. It also focuses on SIG Turnaround and Transformation interventions because very few districts chose the Restart and Closure models.

Table 1 describes the SIG Transformation and Turnaround models that Tier 1 and Tier 2 schools implemented. It is worth noting that because many of the eligible schools were going through NCLB's "school improvement" process at the time, many likely had begun implementing features of

[7] Unless otherwise noted, all information in sections III and IV comes from internal documentation provided by the Ohio Department of Education.

the SIG models. Indeed, the "other" improvement option under NCLB very much resembled the Transformation model. Additionally, it is worth noting that federal rules stipulated that a district that had nine or more Tier 1 and Tier 2 schools was not permitted to implement the transformation model in more than 50 percent of those schools.

Table 1. SIG Turnaround vs. SIG Transformation Model

SIG Models	Count	Model Description
Transformation	58 Schools	▪ Replace principal, provide managerial flexibility (over staffing, budget, and curriculum), and develop leadership. ▪ Use data to design and implement instructional programs aligned to state standards. ▪ Provide high quality professional development. ▪ Develop new teacher evaluations and retain only those who are deemed to be effective. ▪ Engage with families and community. ▪ Direct Title I funds to expanded learning time and professional development activities. ▪ A range of other optional activities.
Turnaround	16 Schools	▪ All of the above (with minor differences) ▪ Rehire no more than 50 percent of current teachers ▪ Create a district office focused on school turnaround

Note. Counts reported above are for cohort I and II schools included in our statistical analysis. The contents of the table are based on documentation ODE provided.

The most dramatic change in schools implementing the Transformation model might have been the receipt of SIG funds, provided that districts distributed those funds above and beyond the funds those schools would have received in the absence of SIG. If this indeed was the case, the average SIG school would have received over $2,000 more per pupil over the course of three years. As Table 2 indicates, over 50 percent of this funding was dedicated to salaries and benefits, and over 25 percent was dedicated to contracting for services.

Table 2. SIG Spending

SIG Model	Median Annual School SIG Budget	Median Annual School SIG Budget Per Pupil	Allocation of SIG Funds: Salaries	Retirement/ Fringe Benefits	Purchased Services	Supplies	Capital Outlays
Transformation	$772,000	$2,234.04	43.25%	9.88%	28.22%	12.06%	5.90%
Turnaround	$809,200	$2,237.81					

NOTE. Above stats are for all SIG Tiers. Tier 1 spending per building is a bit higher ($897,215.10 and $818,400, for "transformation" and "turnaround" respectively), but Tier 1 spending per pupil is similar. Figures in the table were calculated by the authors using data ODE provided.

Schools received funding for three years—FY11-FY13 for Cohort I and FY12-FY14 for Cohort II, although a handful of schools refused funding or closed before they could receive all of it. But it is important to consider that many SIG-eligible schools (i.e., those deemed "persistently low achieving") either did not apply for SIG funds or applied and did not receive them (at least not initially); and some districts applied for and received funding for schools that were not labeled as "persistently low achieving" ("Tier 3" schools described above). Table 3 provides counts of schools that were and were not identified as "persistently low achieving" and counts of schools in each category that applied for and received SIG awards.

Table 3. SIG Schools Identified and Included in Analysis

SIG Round	Tier 1 Schools Identified as Persistently Low Achieving	Number Funded in Corresponding SIG Round
I	55 of 724 schools in pool	25 of 55 low performers (12 additional schools eventually funded in round 2, for a total of 37 of 55 schools) 6 non-low performers (Tier 3 schools)
II	47 of 695 schools in pool (excludes schools awarded SIG grants in first round)	21 of 47 low performers 0 non-low performers (Tier 3 schools)

Note. Figures in the table were calculated by the authors using data ODE provided.

It is important to note that all schools that applied for but did not receive SIG funds in the first round were awarded SIG funds in the second round. Thus, a much larger proportion of SIG I-eligible schools received funds than SIG II-eligible schools. It is also worth noting that the vast majority of the SIG I-eligible schools that received funding in the first round were district schools, whereas most charter schools that had applied in the first round received funding in the second round.

V. Ohio Priority Schools

In 2011 President Obama announced that states could receive waivers from some of the accountability requirements of NCLB if they developed alternative systems to hold schools and districts accountable and intervened to turn around the lowest performing schools. States submitted plans to identify and turn around low-performing "Priority" schools as part of their waiver applications. Ohio submitted its plans to identify and intervene in Priority schools—including a list of the schools it identified with its proposed procedure—as part of its waiver application in February, 2012.

The methods Ohio used to identify and turn around Priority schools mirrored those from the SIG program. The first wave of Ohio Priority schools—those identified in 2012—consisted of Title I-served schools ("Tier 1") and Title I-eligible secondary schools ("Tier 2") that had a combined proficiency rate that placed them in the bottom five percent of eligible schools, had an average graduation rate below 60 percent, or that had received School Improvement Grant (SIG) funds beginning in FY2011 (cohort 1) or FY2012 (cohort 2). Using FY2007-FY2011 data, ODE identified 167 Priority schools, just over half of which had received SIG funds. Specifically, ODE identified 77

Priority schools that would undergo turnaround interventions but that did not previously receive SIG grants. These schools eventually were given the option of applying for SIG funding that would begin in FY2015.

Table 4. Priority School Included in the Analysis and Description of Interventions

Tier 1 Schools ID'd	Highlights of Model Used
72 of 1,904 eligible schools that had not previously received a SIG grant	▪ Replace principal *or justify to state that keeping current principal is appropriate,* provide managerial flexibility (over staffing, budget, and curriculum), and develop leadership. ▪ Use data to design and implement instructional programs aligned to state standards. ▪ Provide high quality professional development. ▪ Develop new teacher evaluations and retain only those teachers deemed to be effective. ▪ Engage with families and community. ▪ Direct Title I funds to expanded learning time and professional development activities. ▪ A range of other optional activities.
Note. Counts above are based on the sample of schools that enter the analysis below. The contents of the table are based on documentation ODE provided.	

The Tier 1 Priority schools that had not previously received SIG funds—those implementing the model in Table 4—are the focus of this analysis. Like all Priority and SIG schools, they were required to implement a series of interventions for at least three years. As Table 4 indicates, the Priority schools identified in 2012 were required to implement a model much like the SIG Transformation model. The most notable difference between the two models is that Priority schools could retain their principals if they demonstrated to the state that s/he should be retained. ODE also provided significant technical assistance and put processes in place to monitor and ensure fidelity of implementation, which began in fall 2012 (FY2013).

VI. Research Design

The ideal research design would entail randomly assigning a subset of persistently poor-performing schools to receive the SIG or Priority intervention (the treatment group) and then comparing their performance over time to schools that did not implement the turnaround models (the control group). If a sufficient number of schools were randomly assigned, the two groups would be composed of schools that are, on average, similar in every imaginable way except whether they implemented a turnaround model. If that were the case, the control group would provide an excellent proxy for how SIG or Priority schools would have performed had they not been required to implement a turnaround model.

The turnaround policies were not implemented in this way. However, the strict performance cutoff by which schools were identified as SIG-eligible or Priority school nonetheless provided an opportunity to estimate the causal effects of these interventions for a sample of schools. Specifically, the strict performance cutoff allowed us to employ a Regression Discontinuity (RD) design. The RD design entails comparing the outcomes of schools with proficiency or graduation rates that placed them close to but on either side of the performance threshold used to identify SIG-eligible and Priority schools (i.e., a proficiency rate at the 5th percentile or a graduation rate of 60 percent). The logic of the design is that these schools (and their students) should be nearly identical in every respect except for their exposure to the turnaround intervention. In other words, if the assumptions of the RD design hold, it is as if schools near the performance cutoffs were randomly assigned to the treatment and control conditions. We tested this assumption by comparing the characteristics of schools near the cutoff and found that the assumption holds.[8]

A limitation of the RD design is that it focuses on schools near the performance threshold determining SIG eligibility and priority school identification—that is, schools with composite proficiency rates in math and reading near the threshold demarcating the 5th percentile (specifically, 29.175 percent for SIG cohort I, 33.66 percent for SIG cohort II, and 40.6 percent for the first wave of Priority schools), or graduation rates near 60 percent. Another potential problem is that there are sometimes too few schools near the threshold to detect differences in outcomes. To address these potential issues, we also estimated models that compare the performance trajectories of all schools that were and were not treated. For example, we calculated the change in school quality before and after schools were identified as SIG-eligible, before and after they received a SIG award, and before and after they received a Priority designation, and we compared those changes to the trajectories we observed in schools that did not receive these treatments. A potential advantage of this approach is that it entails estimating the average effect across all schools receiving a treatment, as opposed to the small sample near the performance cutoffs.

The assumption of this "difference in differences" (DID) approach, however, is that the performance trajectories of schools that ultimately received the treatment would have been the same as those that did not receive treatment. At times the results of our analyses lead us to question whether this assumption holds, particularly when it comes to estimating the effect of SIG eligibility and the Priority designation on student achievement.[9] Consequently, we emphasize the results of this approach only when it appears that the assumption should hold. In particular, we rely on this approach when looking at the impact of actually receiving a SIG award (as opposed to merely being SIG-eligible) and when estimating the impact of the interventions on graduation rates. Although the design is particularly problematic for estimating the impact of the Priority designations, we still employ it for graduation rates because it enabled us to generate estimates for the small sample of Tier 1 Priority high schools that had not previously received a SIG award.

The analysis focuses on two measures of school quality: ODE's estimates of annual student growth in math and reading achievement (i.e., school "value added") and school-level graduation rates. We use the former because it takes into account the characteristics of the student body a school educates, which could change after the treatment is administered. We also include the analysis of graduation rates in the body of the report because it is the best measure available for high schools. But it is important to keep in mind that it is sensitive to changes in the student body. Additionally, we examined the impacts on the achievement levels of a school's students using student-level test scores. Finally, we estimated the impact of these interventions on school administration, focusing primarily on principal and teacher turnover because other measures of teacher characteristics and mobility largely yielded negligible results (see Appendix F).

The remainder of this section describes our data and sample of schools, as well as our modeling strategies. We point the reader to the technical appendix at the end of this report for a more detailed description of our methodological approach and the results for each of the analyses.

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[8] See Appendix A for details.
[9] See Appendix B for details.

Data

The ODE provided the combined proficiency and graduation rates used to identify persistently low achieving schools for the first two rounds of SIG and the first round of Priority schools. The remaining FY2007-FY2015 building-level data we used in the analysis are generally publicly available on the ODE website, although ODE provided some of these data in a format we requested. We obtained FY2007-FY2014 student- and staff-level data from the Ohio Education Research Center (OERC).

The measure of school quality on which we focus most is ODE's annual, school-level "value added" estimates in math and reading. These building-level measures are arguably the most valid estimates of school quality because they control for multiple prior years of student test scores, thereby accounting for differences in the students that schools educate. These estimates capture one-year achievement gains in grades 4-8 and are reported in "normal curve equivalent" (NCE) units, but we converted them to standard deviation units when presenting the results in the body of the report.[10] Additionally, we estimated student achievement levels on state math, reading, and science tests in grades 3-8 using student-level achievement data, which also allow us to control for changes in student populations over time.

The analysis also examines schools' graduation rates[11] and their scores on the state's performance index, which captures the performance level of a school's students across multiple subjects (math, reading, writing, science, and social studies) on all state assessments (the OAA in grades 3-8 and the Ohio Graduation Test) on a 0-120 scale. Graduation rates and performance index scores may not accurately reflect differences in school quality because they do not account for changes in the characteristics of students schools educate. Additionally, performance index scores aggregate a number of censored measures (counts of students meeting certain thresholds), as opposed to a continuous measure of achievement for all students, and the tests included in the metric change over time. We present some results using these measures because they capture dimensions of achievement and attainment that school value-added estimates miss. The performance index captures performance in more grades and subjects—and it is available for more schools—than the value-added measures. And both graduation rates and the performance index enable us to examine educational outcomes in high schools. However, because we have the more valid value-added achievement estimates available—as well as continuous measures of average school-level achievement that we calculated using student-level data—we present the results for the performance index only in the appendix.

Finally, we examine a number of building-level characteristics, including student and staff characteristics and mobility rates. We review only some of these results in this report because school staffing measures generally yielded negligible results. However, we report some of these additional analyses in the appendix.[12]

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[10] See appendix H for details of this conversion. It is important to note that the value-added estimates we use are gain scores, as opposed to the gain "index" used to grade schools. It also is important to note that the 2013 and 2014 value-added estimates publicly available are three-year averages that must be converted to annual gains.
[11] It is important to note that the measurement of graduation rates changed in FY2012. We use the "four year" graduation rate in subsequent years.
[12] In particular, see Appendix F.

Description of Schools Included in the Analysis

Table 5 below presents some statistics summarizing the characteristics of the Tier 1 schools that were and were not identified as being below the 5th percentile in terms of their combined proficiency rates as of FY2009 (SIG I), FY2010 (SIG II), and FY2011 (first wave of non-SIG Priority schools). The table reveals that, compared to other schools in their respective pools, schools identified as "persistently low achieving" (PLA--i.e., below the 5th percentile) had much smaller enrollments, had more economically disadvantaged and minority students, had far more teacher turnover, and were far more likely to be charter schools. Indeed, about 50 percent of SIG-eligible schools, and over one quarter of Priority schools, were charter schools.

Table 5. Comparing Tier 1 Schools that Were and Were Not Identified as Being Below the 5th Percentile in terms of a Combined Proficiency Rate in Math and Reading

	SIG I (FY2009)		SIG II (FY2010)		Priority (FY2011)	
	PLA	Not PLA	PLA	Not PLA	PLA	Not PLA
Average Enrollment	230.11	473.16	234.18	480.03	337.64	435.01
Percent "Economically Disadvantaged" Students	84.34	72.63	85.68	73.81	91.80	56.13
Percent Black Students	70.29	42.33	66.03	38.17	76.21	19.32
Percent Charter Schools	50.00	14.10	57.89	11.72	25.76	8.56
Combined Proficiency Rate (Math & Reading)	24.33	58.83	27.74	61.99	35.14	77.03
Average Performance Index Score	54.41	79.97	57.00	82.54	64.78	94.10
Average Value-Added Estimate in Reading (SDs)	-0.05	0.02	-0.04	0.01	-0.05	0.01
Average student-teacher ratio	13.45	15.80	12.63	15.77	18.35	16.71
Average Annual Principal Turnover Rate	0.17	0.17	0.14	0.09	0.10	0.06
Average Annual Educator Turnover Rate	0.43	0.21	0.38	0.17	0.25	0.14
Proportion of Baseline Principals Gone by FY2014	0.95	0.79	1.00	0.78	0.76	0.52
Proportion of Schools Closed by FY2014	0.31	0.14	0.18	0.08	0.08	0.02

Note. SIG II and Priority statistics exclude schools that previously received a SIG award.

The schools identified as low performers also had somewhat lower value-added estimates, which capture annual student achievement growth. Whereas a school with students whose annual achievement growth is average should have a value-added estimate of 0 standard deviations (SDs), schools identified as low performers have students whose annual achievement growth was between 0.05 and 0.07 standard deviations lower. That puts these schools' students at around the 48th percentile in terms of student achievement growth, whereas students in non-PLA schools were at around the 51st percentile. If one assumes that students get about 180 days' worth of learning in a school year, these statistics suggest that students in schools below the 5th percentile in terms of proficiency rates receive the equivalent of around 30 fewer days of learning each year than students in schools above the 5th percentile.

Statistical Modeling

The RD strategy entails comparing the outcomes of schools that were close to but on either side of the performance thresholds for SIG eligibility and Priority identification. There are a number of ways to do this statistically. One way is to use data from all schools in the sample and to model the relationship between the assignment or "running" variable (e.g., the "combined proficiency rate" used to rank schools) and the outcome of interest using a flexible functional form that accounts for the relationship completely. Alternatively, one can restrict the schools in the analysis to those within a narrow bandwidth of the threshold—for example, schools with a combined proficiency rate within 20 percentage points of the performance cutoff—and compare their outcomes. We report the results of models that employ both approaches at once because they yield little evidence of differences in the pre-treatment characteristics of treated and untreated schools.[13]

We also employed the difference-in-differences (DID) design we describe above on its own (e.g., see Appendix B) as well as to enhance our RD analysis (see Appendix C). As we detail in the appendix, the DID design entails comparing a school's outcomes after treatment to its outcomes in the latest pre-treatment year (FY2009 for SIG I, FY2010 for SIG II, and FY2011 for Priority), and comparing those changes between treated and untreated schools. We examine these differences through the 2014-15 school year, as well as up to three years prior to the interventions. We do the latter to look for differences in pre-treatment trajectories between treated and untreated schools, which would invalidate the DID and RD designs. As we note above, we primarily emphasize results in which there are no significant pre-treatment trends.

Conducting the RD analysis within a DID framework helps increase confidence in our estimates. In particular, it should help minimize pre-treatment differences between the treatment and control groups, and it should enhance our ability to detect statistically significant effects. That said, it is important to reiterate that comparing treated and untreated schools using the RD design is as if comparing identical schools that were randomly assigned to the treatment condition. As we note above, treated and un-treated schools near the performance thresholds for SIG eligibility and Priority identification do not reveal statistically significant differences across the numerous characteristics we consider. For example, the results reveal that the differences in pre-treatment trends that invalidated some of the difference-in-differences analyses[14] generally disappear in our RD analysis. Thus, it is reasonable to interpret the results we emphasize below as causal effects of SIG eligibility and Priority school identification.

Finally, as we noted above, estimating the impact of SIG models is not as straightforward as estimating the impact of Priority identification because not all SIG-eligible schools applied for and received SIG awards. We estimated a number of models that account for this fact. Ultimately, we typically chose to report the results of DID models comparing SIG-eligible schools that did and did not receive SIG awards because of the desirable properties we detail above. However, we report in

[13] See Appendix A and Appendix C for more details.
[14] See Appendix B.

the appendix additional analyses that account for the fact that only a fraction of SIG-eligible schools ultimately received awards.[15]

VII. Results

The results below are organized into five sections: the impacts of SIG eligibility and Priority identification on school quality; impacts of receiving SIG awards on school quality; comparisons of the SIG Turnaround and Transformation models; impacts of SIG awards and Priority school identification on principal and teacher turnover; and impacts of SIG and priority school identification on students attending schools at the time they were identified. These results are meant to provide a general summary of those we report in the appendixes. It is important to note that we found little evidence that SIG and priority school designations and interventions had an impact on the probability of school closure.[16] Thus, the estimates of the impacts we review below are unlikely to be due to treated or untreated schools leaving the sample. Additionally, although we did not find statistically significant changes over time in the composition of students in these schools, it is important to keep in mind that changes in student composition could still affect analyses involving graduation rates or achievement levels (as opposed to growth).

Impact of SIG Eligibility and Priority School Identification on School Quality

Figure 1 on the following page presents the impact of identifying schools as being in the lowest achieving five percent—that is, the impact of SIG eligibility (whether or not schools ultimately received a SIG award) or Priority identification—on school-level annual student achievement growth in math and reading (i.e., school "value added"). Treatment effects are reported in standard deviation units. Positive numbers indicate that the treatments had a positive impact, and solid bars indicate that the estimate is statistically different from zero ($p<0.10$ for a two-tailed test). In light of evidence we review later in this report, one can reasonably interpret empty bars as revealing a more suggestive estimate of the intervention's impact in a given year.

Figure 1a reveals that by the end of the 2011-12 school year (FY2012), identifying a school as SIG eligible—or, put differently, identifying a school as being in the bottom 5 percent of Title I-served schools under NCLB's "school improvement" program—led to improvements in achievement of 0.28 standard deviations in reading and 0.21 standard deviations in math. Assuming a 180-day school year and based on estimates of average achievement growth, that translates to schools imparting almost a full year of additional reading content (the equivalent of 159 extra days of learning) and about 89 days of extra math content in FY2012 than they would have without being identified as being a low-performer.[17] However, the positive impact in reading seems to go away completely by FY2015. Similarly, the effect in math is nearly halved by FY2015, although the statistically insignificant effect still equates to achievement gains of about 53 extra days of learning per year.

[15] See Appendix G.
[16] See Appendix F.
[17] See Appendix H for a description of the procedure used to calculate "days of learning."

Figure 1. Impact of SIG Eligibility & Priority School Designation on Schools' Annual Student Achievement Growth

Figure 1a. Impact of SIG I Eligibility

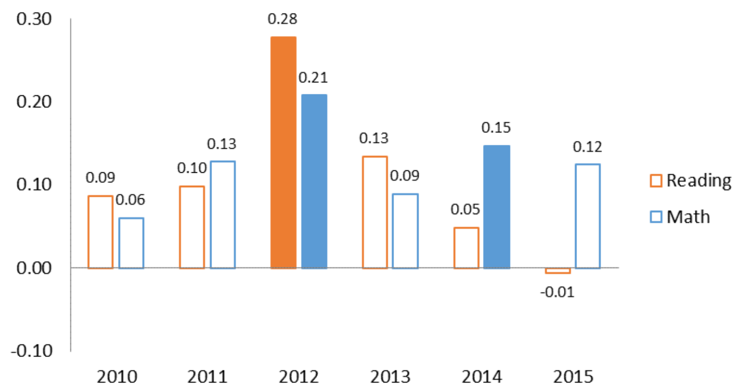


Figure 1b. Impactof SIG II Eligibility

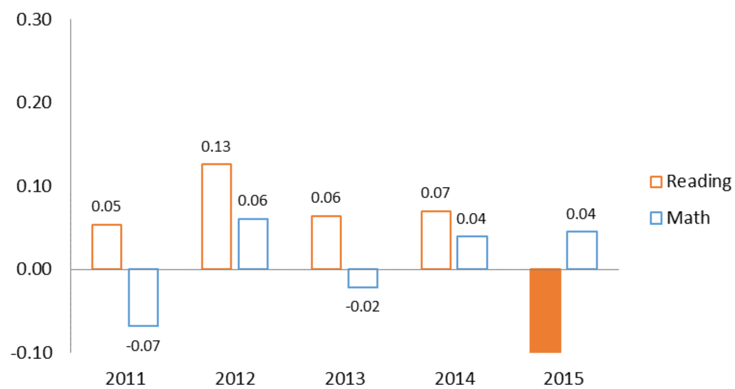
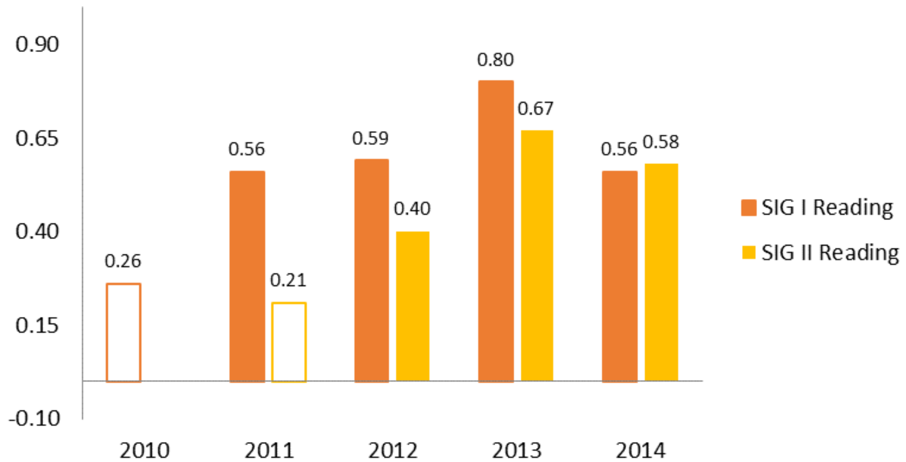


Figure 1c. Impactof Priority School Identification

Note. The figures report the results of a regression discontinuity analysis of the impact of SIG eligibility and Priority identification on schools' annual student gains in reading and math. The effects are reported in standard deviations. Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix C (Tables C1-C3) provides more details on the modeling strategy and results.

The second wave of SIG eligibility determinations and the first wave of Priority school identification did not have statistically significant positive impacts on annual value-added estimates, and there is some evidence that they had some negative effects by FY2015. However, the statistical insignificance of the estimates does not mean that the interventions did not have lasting impacts. We lack the statistical power to make that determination. Also note that annual value-added estimates reveal how much more a school's students learned over a single year. Thus, achievement gains can accumulate over time, resulting in schools having significantly higher achievement levels, even if the size of annual value-added advantages declines. Figure 2 illustrates such increases in reading achievement levels based on SIG eligibility. Results for math are similar.[18]

Figure 2. Impact of SIG Eligibility on Student Achievement Levels



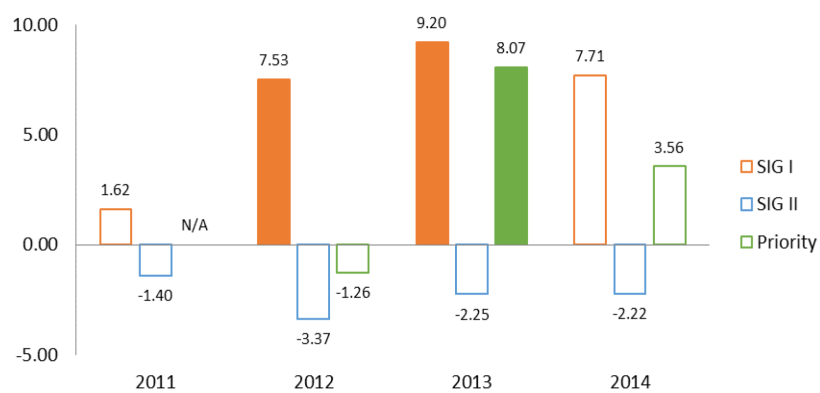
Note. The figure reports the results of an RD analysis of the impact of SIG eligibility on achievement levels in reading. Results are reported in standard deviation units. Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix C (Table C4) provides more details on the modeling strategy and results.

Figure 2 illustrates just how large the improvement in reading achievement these SIG eligible schools (i.e., the lowest 5 percent) experienced in absolute terms. By FY2013, SIG eligibility had given schools' students the equivalent of over two extra "years of schooling." By 2014, even with a dip from FY2013, the average test scores of a SIG school's students were around 0.55 standard deviations higher than they would have been without the intervention, which is the equivalent of students moving from the 5th percentile (the cutoff to identify low-performing schools) to approximately the 14th percentile on the achievement distribution. Thus, even statistically insignificant annual value-added estimates of 0.05-0.15 standard deviations may capture real annual benefits that accumulate into substantial achievement advantages at the school level. Nevertheless, annual value-added remains the superior measure of school quality. Value-added estimates approaching zero or that become negative in later years (as in the case of reading for SIG II) indicate that a SIG-eligible school's students are learning the same or less from year to year than they would have without SIG eligibility.

[18] See Table C4 in Appendix C.

Finally, Figure 3 provides estimated impacts on graduation rates using a difference-in-differences design (for the reasons we discuss above). Specifically, Figure 3 focuses on Tier 1 schools that could qualify for SIG based on their graduation rates. In this case, positive numbers can be interpreted as SIG eligibility or Priority identification having a positive impact on graduation rates, reported as percentage point increases. Consistent with the pattern of results reported in Figure 1, the results in Figure 3 indicate that SIG I eligibility led to improvements in school graduation rates of around 7-9 percentage points, whereas SIG II eligibility is not associated with statistically significant impacts. However, it appears that Priority school identification led graduation rates to increase by about 8 percentage points in FY2013 and about 3.5 percentage points in FY2014, although the latter estimate does not reach statistical significance. Finally, it is worth noting that this analysis includes only Tier 1 high schools that qualified for SIG or received a Priority designation based on graduation rates. Thus, there are few high schools included in the analysis: 19 treated schools for SIG I, 9 for SIG II, and 6 for priority school identification.

Figure 3. Impact of SIG Eligibility & Priority School Designation on School Graduation Rates



Note. The figure reports the results of a difference-in-differences analysis of the impact of SIG eligibility and priority school identification on school graduation rates. Results are reported in percentage points. Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix B (Table B2) provides more details on the modeling strategy and results.

Impact of Receiving a SIG Award on School Quality

To calculate the average achievement effect of actually receiving a SIG award across both rounds, we conducted a difference-in-differences analysis that compares achievement growth in SIG-eligible schools that did and did not receive a grant to implement a SIG model. This analysis also has the benefit of providing a single estimate of SIG's impact from the first two cohorts.

Figure 4. Impact of Receiving a SIG Award on Schools' Annual Student Achievement Growth

Note. The figure reports the results of a difference-in-differences analysis of the impact of SIG eligibility and priority school identification on schools' annual student achievement growth. Effects are reported in standard deviation units. Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix B (Table B3) provides more details on the modeling strategy and results.

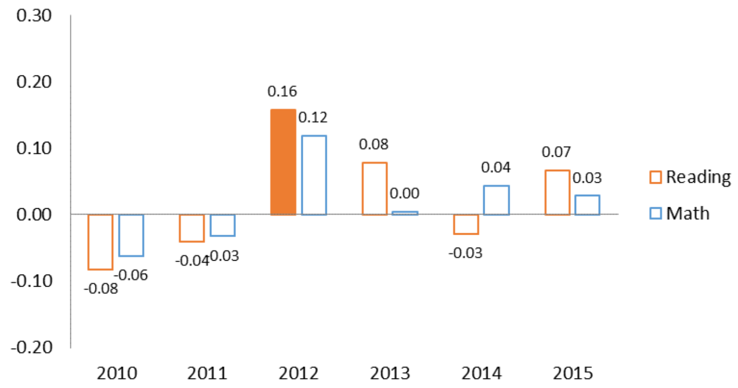
The results in Figure 4 indicate that the average improvements in school performance due to SIG awards peaked at around 0.10-0.15 standard deviations in additional student learning. That corresponds to students receiving the equivalent of 60 extra days of learning in a single year.[19] Again, however, these positive effects decline and no longer reach statistical significance by 2015. It could be that the 2015 effects of 0.05-0.08 standard deviations—corresponding to around 30 extra days of learning—are quite real but that we lack the sample size necessary to confirm it. These would remain substantively significant impacts if that were the case.

Comparing the Impacts of SIG Turnaround and SIG Transformation on School Quality

Finally, Figure 5 compares the impacts of the SIG Transformation and Turnaround models, the latter of which is meant to be more aggressive and potentially more disruptive. This analysis is limited to comparing changes in schools' value-added estimates between the few that implemented the SIG Turnaround model to the many that implemented the SIG Transformation model. Again, although this design requires arguably more untestable assumptions than the RD analysis, we found that pre-treatment trends were generally similar between these two sets of schools.[20] Thus, the results in Figure 5 can be considered reasonable estimates of the differences in the causal impact of the two SIG models.

[19] See Appendix H for the "days of learning" calculation.
[20] See Appendix E.

Figure 5. Impact of Implementing a SIG Turnaround Model (as opposed to a SIG Transformation Model) on Schools' Annual Student Achievement Growth



Note. The figure reports the results of a difference-in-differences analysis of the impact of the SIG Turnaround model as opposed to a SIG Transformation model on schools' annual student achievement growth. Effects are reported in standard deviation units. Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix E provides more details on the modeling strategy and results.

Figure 5 reveals that SIG Turnaround schools had somewhat inferior performance before they received the SIG awards (during FY2010 and FY2011), although these results do not reach statistical significance. However, consistent with some of the studies we reviewed above, by FY2012 the Turnaround model outperformed the Transformation model. These advantages dissipate and fail to attain statistical significance in later years, however.

Impacts on School Administration

The SIG and Priority models required administrative changes that could affect the quality of school leadership and instruction. Thus, we estimated the impact of SIG awards and Priority school interventions on the displacement of principals and teachers. The analyses are done using school-level measures of turnover, some of which we created using staff-level data, and the results we present below in Figure 6 are based on RD designs.[21]

[21] See Appendix F, Table F3, and Appendix G, Table G1.

Figure 6. Impact of Receiving a SIG Award or Priority School Designation on the Turnover Rates of Principals and Teachers

Figure 6a. Impact of Receiving a SIG Award on the Turnover Rates of Principals and Teachers Present in 2009 (SIG I elegible schools)

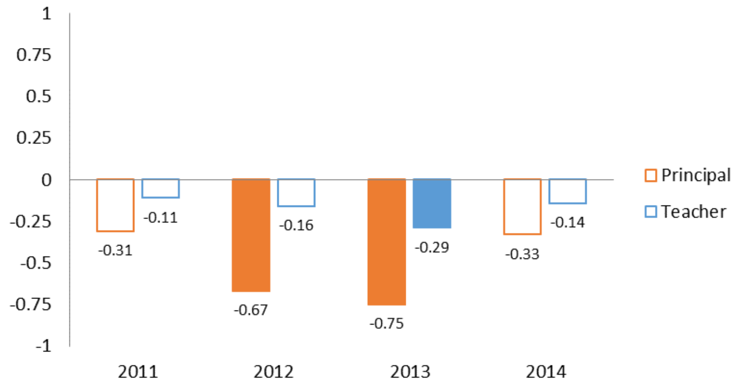


Figure 6b. Impact of Receiving a SIG II Award on the Turnover Rates of Principals and Teachers Present in 2009 (SIG II eligible schools)

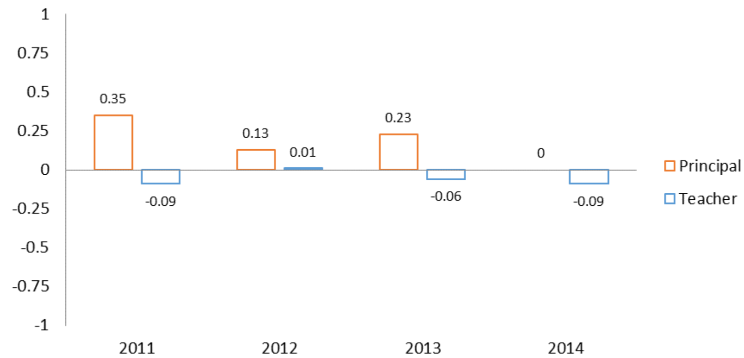
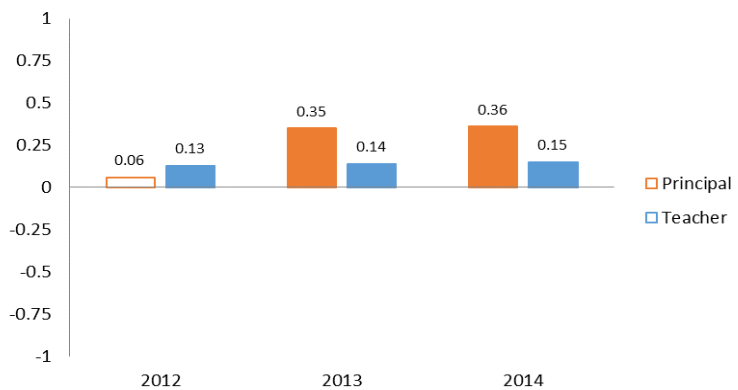


Figure 6c. Impactof Receiving a Priority School Designation on the Turnover Rates of Principals and Teachers Present in 2011



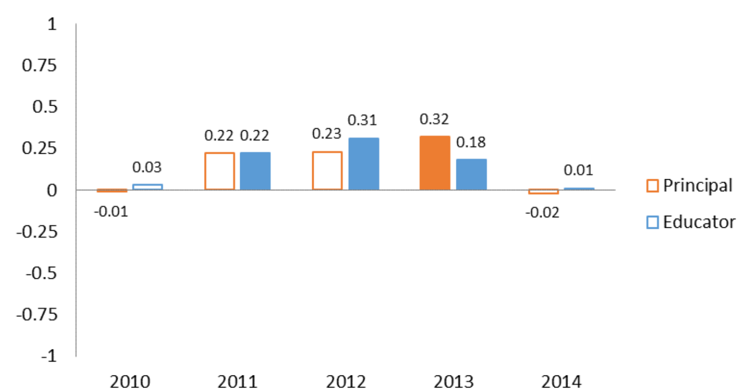
Note. The figures report the results of a regression discontinuity analysis of the impact of receiving a SIG award or a priority school designation on principal and teacher attrition since 2009 (for SIG I and II) and since 2011 (for Priority schools). Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix F (Tables F3 and F4) and Appendix G (Table G1) provide more details on the modeling strategies and results.

As Figure 6 reveals, schools that received SIG awards based on SIG I eligibility had lower principal and teacher turnover rates than they would have had otherwise. Specifically, the proportion of principals in place in 2009 that were gone by 2013 was 75 percentage points lower. Similarly, the proportion of a school's 2009 teachers who were not present in 2013 was 0.30 percentage points lower. On the other hand, schools that received awards based on SIG II eligibility appear to have had higher rates of turnover, though none of the results reaches statistical significance. Although SIG schools seem to have complied with the staff replacement provisions of the SIG turnaround models (see Table 5 above), staff turnover was already so great in these low-achieving SIG schools that turnaround-induced turnover may have failed to have a statistically significant impact on these schools. Indeed, if one combines both SIG cohorts, the results indicate that receiving a SIG award significantly reduced turnover among a school's staff.[22]

Priority school interventions had the effects one would expect, however. The turnover rate among principals in place when the schools were identified (FY2011) was substantially higher in FY2013 (the first year of the reforms) than it would have been had schools not been subjected to the interventions. Indeed, the turnover rate of principals—which was already high in these schools, though not nearly as high as schools in the SIG pools—was 35 percentage points higher than it would have been without the interventions. Similarly, the turnover rate of teachers was about 15 percentage points higher than it would have been otherwise.

Although both SIG Turnaround and Transformation models required the replacement of the principal, only the Turnaround model mandated that at least 50 percent of teachers be replaced. Thus, one might expect more teacher attrition in schools implementing Turnaround. Figure 7 confirms that schools that implemented Turnaround experienced greater annual turnover among staff labeled "education professionals"—most of whom are classroom teachers. Specifically, Turnaround schools were around 20-30 percentage points more likely to replace educators in a given year while implementing the models. Interestingly, Turnaround had greater annual principal turnover rates by a comparable margin.

Figure 7. Impact of Implementing a SIG Turnaround Model (relative to a SIG Transformation Model) on Annual Principal and Educator Turnover Rates



Note. The figure reports the results of a difference-in-differences analysis of the impact of the SIG Turnaround model relative to a SIG Transformation model on the annual attrition rates of school principals and education professionals. Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix E provides more details on the modeling strategy and results.

[22] See Table F4 in Appendix F.

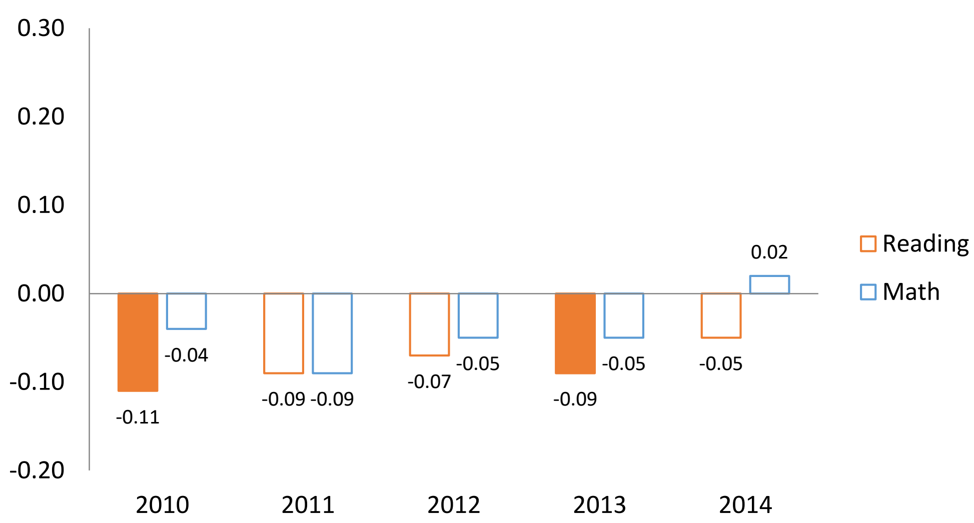
In spite of the clear evidence that these interventions affected staff turnover, we found little evidence that the interventions led to significantly less experienced teachers, fewer "high quality" teachers (as defined by the State of Ohio), or lower student-teacher ratios.[23]

Impacts on Students Attending at the Time of Identification

We found little evidence that the implementation of SIG and Priority turnaround models increased student mobility or otherwise disrupted the learning of students attending schools at the time they were identified. Among these students, there is some evidence that the Priority school interventions had a positive impact on math achievement and some evidence that SIG models had a negative average effect on reading achievement, but the results generally do not reach statistical significance. We also found that these interventions had little impact on student mobility.

There is one effect worth reporting, however. As Figure 8 indicates, SIG Turnaround appears to have had an immediate negative impact on the achievement of students who attended SIG schools when they were identified, although students whom we observe for five years (up to grade 8) appear to recover. Note that unlike the effects reported in previous figures, these effects are not annual achievement gains. They are cumulative.

Figure 8. Impact of Implementing a SIG Turnaround Model (relative to a SIG Transformation Model) on the Achievement of Students Attending SIG Schools at the Time Schools Were Identified



Note. The figure reports the results of a difference-in-differences analysis of the impact of the SIG Turnaround model relative to a SIG Transformation model on the achievement of students who attended schools the year in which they were identified. Effects are reported in standard deviation units. Negative coefficients indicate that SIG Turnaround is associated with lower student achievement. Solid bars indicate that the effects reach statistical significance (p<0.10 for a two-tailed test). Appendix E (Table E2) provides more details on the modeling strategy and results.

[23] See Appendix F for details.

VIII. Conclusion

The analysis employed regression discontinuity and difference-in-differences methods to estimate the impact of the SIG and Priority turnaround programs on school administration and quality. The purpose was to determine whether prior experiences with school turnaround efforts might provide insights about how Ohio should pursue school improvement under ESSA. The analysis provides convincing evidence that, on average, SIG interventions led to improvements in school quality as measured by annual student achievement growth in math and reading. It also indicates that both SIG and Priority school interventions had positive impacts on the graduation rates of Title I-served high schools. However, the analysis reveals that the effects generally diminished over time until they became statistically insignificant. That does not mean in all cases that the statistically insignificant positive effects (which are sometimes substantively large) did not persist. It may just be that the effects were too small to distinguish them from zero using available data.

The analysis also examined mechanisms that might explain these effects. Contrary to what one might expect given SIG's focus on school reconstitution, schools that received a SIG award generally experienced less principal and teacher turnover than they likely would have without the award. Priority school interventions, on the other hand, caused significant principal and teacher turnover. It is tempting, therefore, to conclude that staff churn undermined Ohio's Priority School interventions. But it is also important to keep in mind that the more disruptive SIG Turnaround model—which had a significant disruptive effect on the staff and students attending the schools when reforms were implemented—appears to have had an edge (at least initially) over the SIG Transformation model when it comes to school quality as captured by annual value-added.

There are many possible reasons for the results above. For example, the research we reviewed indicates that whether or not replacing principals and teachers leads to improvements in school quality depends on the relative quality of the incoming personnel. Turnover is generally harmful to student achievement—at least in the short term—unless incoming teachers are of sufficiently greater quality to compensate for the negative disruptive effects. Similarly, there is some evidence that providing districts and schools with technical assistance can help, but that surely depends on the nature of the assistance, the needs of particular schools and districts, and the extent to which the assistance imposes an administrative burden that distracts from a school's core mission. It is conceivable that SIG's large positive impact (particularly relative to Priority interventions) is due to the relatively low performance of its schools, the significant amount of funding provided, or the fact that districts could decide to apply for a grant and participate in the program if they anticipated a marginal benefit from doing so.

Overall, the study provides convincing evidence that interventions such as the SIG turnaround models have the potential to improve school quality very quickly, which is consistent with the theory underlying school turnaround reforms as well as research in other contexts. We also find, however, that initial positive impacts dissipated after the first 2-3 years of implementation, which is inconsistent with the notion that turnarounds lead to long-term improvements in school quality. There is suggestive evidence that some more modest positive effects persisted 4-5 years later, but we have too few observations to discern whether or not that is truly the case. Beyond that, the report is necessarily limited to describing some of the differences in the nature of these interventions and, via a literature review, providing some insights as to how they might affect school quality. We leave it to administrators and policymakers to determine which mechanisms are likely to play out in a particular context.

IX. Technical Appendix

This appendix assumes knowledge of econometric methods. It provides the details of the specific models we used to generate the estimates presented in the main body of the report, as well as the results of additional analyses we performed but chose not to emphasize in the main body (sometimes because tests indicated that a research design was questionable in a particular context).

The appendices are in the order in which the analyses were conducted. Appendix A tests the assumption of the regression discontinuity (RD) design that, near the cutoff determining assignment to treatment, schools receiving a treatment (SIG eligibility or Priority identification) were similar to those that did not receive treatment. Appendix B introduces the difference-in-differences (DID) framework underlying all of our analyses and reports the results of models we used to examine whether the pre-treatment trends of treated and untreated schools were similar. For example, Appendix B reveals that the pre-treatment trends in school value-added are not comparable if one compares schools that qualified for SIG or Priority status to those that did not (Table B1), but it also reveals that pre-treatment trends in value-added are comparable if one compares SIG-eligible schools that did and did not receive a SIG award (Table B3).

After testing the basic RD assumption and outlining and examining the assumptions of the DID framework, we begin the RD analysis of school value-added and graduation rates by showing how we embedded the RD design into the DID framework to generate the first set of results presented in the body of the report (Appendix C). The next few sections are ordered based on the substantive topics they explore—including the impact of SIG and Priority identification on students attending schools when they were identified (Appendix D), the comparison of the two SIG models (Appendix E), and impacts on school closure and staffing (Appendix F). Finally, Appendix G supplements the DID analysis estimating the impact of actually getting a SIG award using a "fuzzy RD" design, and Appendix H presents our calculation for converting value-added scores from Normal Curve Equivalent (NCE) scores, to standard deviation units (which are reported in the tables and figures in the main body of the report), to "days of learning."

APPENDIX A. Covariate Balance Tests

The regression discontinuity design assumes that there is no discontinuous change in pre-treatment building characteristics at the performance threshold that determines whether or not a building was SIG-eligible or qualified as a Priority school. One can test this assumption by comparing differences in observed building characteristics at the threshold. To do so, we tested for differences in the FY2009 (SIG I), FY2010 (SIG II), and FY2011 (Priority school) building characteristics using the same modeling techniques we used to implement our RD design. (As we note below, the panel methods we employed in the main analysis also provide tests of other pre-treatment building characteristics—namely pre-treatment trends in those building characteristics.) Specifically, we report the results of a series of covariate balance tests based on the following OLS model:

$$O_i = \tau Lowest5pct_i + \beta_1 X_i + \beta_2 X_i^2 + \beta_3(Lowest5pct_i \times X_i) + \beta_4\left(Lowest5pct_i \times X_i^2\right) + \alpha + \epsilon_i$$

where O is an FY2009, FY2010, or FY2011 characteristic of building i, $Lowest5pct_i$ is a variable indicating whether or not a building qualified for SIG or was identified as a Priority school due to a proficiency rate that placed them in the bottom five percent of schools, and X is the weighted proficiency rate used to determine eligibility (known as the "running" or "forcing" variable capturing distance from the threshold). Note that the proficiency rate is modeled as a quadratic polynomial and interacted with the "lowest 5 percent" indicator to allow separate functional forms on either side of the performance threshold. We selected the quadratic polynomial based on Gelman and Imbens (2014), and because it provides better balance than a linear specification. Importantly, X is centered at the performance threshold (29.175 percent for SIG I, 33.66 percent for SIG II, and 40.6 percent for priority schools) so that the coefficient τ captures the difference in the observable characteristics at the cutoff. Finally, α is the intercept for each regression model.

Table A1 below reports the results of a series of OLS models for each characteristic. In the interest of space—and because the results we present are based on models with restricted bandwidths—we provide the covariate balance estimates based on samples that include only those schools with combined proficiency rates that place them within 20 percentage points of the respective performance thresholds (i.e., $X > -20$ and $X < 20$). That includes all treated schools (or nearly all, depending on the year) but only a subset of untreated schools. We selected this bandwidth because the bandwidth selection procedure suggested by Imbens and Kalyanaraman (2012) usually indicated a bandwidth of around that size. We focus on results based on a restricted sample because pre-treatment covariate balance was superior to what we obtained using the full sample.

The results in Table A1 indicate that schools near the cutoffs generally are similar, as the RD design requires. The SIG-eligible schools in the first round have a smaller percentage of Black students and a greater number of teachers with bachelor's degrees, and treated schools in the second round of SIG have higher value-added scores in reading. But balance is quite good across the other cohorts and characteristics. One important fact to note, however, is that the value-added estimates are very noisy. Thus, we also estimated differences in the prior year score (FY-1), and a three-year average of value-added scores leading up to the fiscal year of identification. As the table illustrates, when a three-year average is used the coefficients reduce dramatically in size and the value-added estimate in reading no longer approaches substantive or statistical significance. This is important to note, as our RD analysis below often uses three prior years of value-added estimates as a baseline to minimize bias and improve the precision of our estimates of treatment impacts on school quality.

Table A1. Covariate Balance Using Quadratic Specification and Restricted Sample						
	SIG I (FY2009)		SIG II (FY2010)		Priority (FY2011)	
	N	Coeff./(SE)	N	Coeff./(SE)	N	Coeff./(SE)
Math Value-Added (t)	232	-2.01	254	1.58	330	-1.70
		(1.31)		(1.26)		(1.05)
Math Value-Added (FY-1)	224	0.20	241	0.80	330	0.37
		(2.18)		(1.47)		(1.15)
Math Value-Added (3-yr avg.)	213	-1.81	229	0.40	312	0.08
		(1.06)		(0.81)		(0.73)
Reading Value-Added	232	-2.61	254	2.24*	330	-1.22
		(1.59)		(1.31)		(1.09)
Reading Value-Added (t-1)	224	-0.08	242	-0.01	330	0.83
		(2.30)		(1.47)		(1.16)
Reading Value-Added (3-yr avg.)	213	-0.62	229	0.44	313	0.43
		(1.25)		(0.86)		(0.53)
Performance Index	224	-3.41	261	-0.17	372	-0.57
		(2.19)		(1.14)		(1.27)
Performance Index (t-1)	237	-2.95	256	-3.88	358	3.95
		(3.43)		(2.78)		(1.84)
Pct Econ Disadvantaged	244	-0.05	261	-0.02	373	0.05
		(0.06)		(0.06)		(0.04)
Pct Disabled	244	0.13	261	-0.12	373	-0.07
		(0.11)		(0.10)		(0.08)
Pct Limited English Prof.	244	-0.003	261	-0.01	373	0.03
		(0.05)		(0.05)		(0.04)
Percent Asian	244	-0.01	261	0.001	373	-0.01
		(0.01)		(0.003)		(0.01)
Percent Black	244	-0.20*	261	-0.07	373	0.02
		(0.11)		(0.12)		(0.10)
Percent Hispanic	244	0.04	261	0.05	373	0.04
		(0.05)		(0.06)		(0.04)
Enrollment	243	-1.40	261	78.85	373	-81.19
		(71.35)		(68.98)		(115.42)
Attendance Rate	244	-1.30	261	-1.10	373	-0.92
		(1.20)		(1.40)		(0.86)
Charter (0,1)	244	0.20	261	0.17	373	-0.07
		(0.20)		(0.19)		(0.14)
Teacher Count	243	-3.07	260	2.09	372	-7.22
		(5.04)		(4.65)		(4.58)
Teacher Attendance Rate	243	-9.02	260	6.29	372	3.06
		(8.41)		(9.41)		(3.37)
Teacher Experience (Yrs)	243	-1.43	260	-3.27	372	-0.49
		(2.60)		(2.76)		(2.25)
Percent Teachers Certified	242	4.59	259	2.32	373	2.33
		(2.84)		(3.35)		(1.80)
Percent Teachers w/ BA	243	1.52**	260	-1.06	372	-11.77
		(0.69)		(1.04)		(7.71)
Percent Teachers w/ MA	243	-5.77	260	-5.00	372	0.46
		(8.06)		(8.00)		(7.59)
Avg Teacher Salary (dollars)	243	-4,012.75	260	-8,123.81	372	-6,375.11
		(6,896.75)		(5,593.82)		(6,283.27)
Percent Teachers Certified	242	0.26	259	-0.02	373	-2.80
		(0.24)		(0.02)		(1.88)
Pct Teachers "High Quality"	242	2.63	259	-0.95	373	1.28
		(6.03)		(3.98)		(3.47)
Pct Staff Turnover Since t-1	242	-0.04	259	0.11	358	-0.01
		(0.06)		(0.09)		(0.05)

Note. The table presents coefficient estimates and standard errors for the indicator of scoring below the threshold for priority designation from OLS models. Each coefficient is from a separate regression. Robust standard errors are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Appendix B. DID Analysis

The difference-in-differences analysis compares treated schools' pre- and post-treatment performance to the changes in performance over the same time period in buildings that did not receive treatment. The first treatment we consider is whether or not schools were identified as being in the lowest five percent, therefore making them eligible to apply for SIG funds or requiring them to implement priority school interventions. Specifically, we estimated the following OLS model:

$$Y_{it} = \alpha_i + \theta_t + \tau^t(Lowest5pct_i \times \theta_t) + \epsilon_{it}$$

where the school performance measure Y for building i in fiscal year t is a function of building fixed effects (α_i), fiscal year fixed effects (θ_t), and an interaction between a variable indicating whether or not a building was in the bottom 5 percent ($Lowest5pct_i$). The building fixed effects are differenced out. The fiscal year fixed effects (θ_t) are captured through the inclusion of indicator variables for all years except the last pre-treatment year (FY2009 for SIG I, FY2010 for SIG II, and FY2011 for Priority). Thus, the model captures differences relative to FY2009, FY2010, or FY2011. The coefficient vector τ^t captures differences in trends between buildings that did and did not receive the "lowest five percent" designation. Finally, we clustered the standard errors at the building level to account for within-building correlations over time.

Note that samples of Tier 1 schools are limited to schools that did not receive SIG grants in a prior round. Also note that our preferred school quality measure is ODE's value-added measure, which is in Normal Curve Equivalent (NCE) units. Conversations to standard deviations—on which figures in the report are based—and annual "days of learning" are described in Appendix H. Finally, as we note in the main body, Table B1 reveals pre-treatment trends and, thus, invalidates the DID approach when it comes to estimating the impact of SIG eligibility and Priority status on VA.

Table B1. Impact of SIG Eligibility and Priority Identification on School-Level Value-Added Estimates

	SIG I		SIG II (No Prior SIG Award)		Priority (No Prior SIG)	
Fiscal Year	Reading VA (NCEs)	Math VA (NCEs)	Reading VA (NCEs)	Math VA (NCEs)	Reading VA (NCEs)	MathVA (NCEs)
2007	0.31	-0.31	-0.19	0.01	0.60	2.31***
	(1.05)	(1.06)	(1.17)	(0.94)	(0.61)	(0.80)
2008	1.14	2.44**	1.86*	2.93***	1.71**	0.64
	(1.08)	(1.03)	(1.07)	(1.11)	(0.67)	(0.63)
2009	___	___	0.35	0.49	-0.14	2.08***
			(1.09)	(0.91)	(0.58)	(0.53)
2010	2.27**	3.15***	___	___	1.70**	1.51**
	(0.97)	(0.85)			(0.67)	(0.62)
2011	1.11	3.53***	2.08**	2.58***	___	___
	(1.00)	(0.10)	(0.94)	(0.81)		
2012	1.89*	2.97***	0.48	2.02**	-0.35	0.28
	(1.07)	(1.12)	(0.86)	(0.95)	(0.56)	(0.48)
2013	1.75**	2.44***	1.50	2.03***	1.00*	2.05***
	(0.89)	(0.91)	(0.94)	(0.64)	(0.55)	(0.56)
2014	1.38	3.07***	1.73**	2.37***	0.94**	3.37***
	(1.09)	(0.71)	(0.82)	(0.68)	(0.39)	(0.52)
2015	0.07	0.35	-1.71	-1.00	1.72**	-0.56
	(1.06)	(1.06)	(1.23)	(1.00)	(0.84)	(0.59)
N	5,471	5,471	5,488	5,487	15.193	15,190
Bldg Count	681	681	662	662	1,794	1,794
Method/Model	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel
Reference Year	2009	2009	2010	2010	2011	2011

Note. The table presents coefficient estimates for the indicator of scoring below the 5 percent proficiency threshold. Standard errors clustered by building are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

We also estimated similar models for graduation rates. Specifically, we identified Tier 1 high schools that could qualify as persistently low-achieving based only on graduation rates and conducted the same analysis as above. Importantly, note that pre-treatment differences in graduation rates are minimal for SIG I (see first column). Thus, the DID method may in fact allow us to identify the causal impact of the intervention on graduation rates. To retrieve more precise estimates, we re-estimated all three models using graduation rates from 2007 through 2009 as the baseline for SIG and 2009 through 2011 as a baseline for the priority school analysis (see last three columns). Note that we once again removed schools that received SIG grants in a prior round.

$$Y_{it} = \alpha_i + \theta_t + \tau^t(GradRateBelow60pct_i \times \theta_t) + \epsilon_{it}$$

Table B2. Impact of SIG-eligibility or Priority Identification on Graduation Rates

	SIG I	SIG II	Priority 2012	SIG I	SIG II	Priority 2012
Fiscal Year	Grad Rate (Pct)	Grad Rate (Pct)	Grade Rate (Pct)	Grad Rate (Pct)	Grad Rate (Pct)	Grade Rate (Pct)
2007	-1.94	11.95	5.02	_	_	_
	(3.21)	(9.48)	(5.60)			
2008	-2.26	8.19**	7.16	_	_	_
	(2.04)	(3.86)	(4.81)			
2009	_	_	0.86	_	_	_
			(4.06)			
2010	1.64	1.61	1.87	3.01	-4.65	_
	(3.80)	(5.44)	(3.17)	(4.02)	(6.27)	
2011	0.25	4.82	_	1.62	-1.40	_
	(4.99)	(4.90)		(5.16)	(5.49)	
2012	6.14*	2.82	-0.95	7.53*	-3.37	-1.26
	(3.60)	(5.67)	(2.84)	(3.94)	(6.00)	(2.21)
2013	7.82*	3.94	8.38***	9.20**	-2.25	8.07**
	(4.14)	(5.95)	(2.39)	(4.50)	(6.69)	(3.48)
2014	6.33	3.97	4.22	7.71	-2.22	3.56
	(4.71)	(5.94)	(4.81)	(4.94)	(6.69)	(4.02)
N	482	378	738	482	378	565
Bldg Count	66	50	96	66	50	96
Method/Model	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel
Reference Year	2009	2011	2011	2007-09	2007-09	2009-11

Note. The table presents coefficient estimates for the indicator of scoring below the 60 percent graduation rate threshold. Standard errors clustered by building are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Finally, among schools eligible for SIG, we compared the outcomes of those that did and did not receive SIG grants. Note that there is likely selection bias, as districts that applied for and received SIG grants might have been more motivated to improve their schools and more committed to implementing the prescribed models. On the other hand, this comparison involves schools with similar characteristics, which addresses the concern with comparing SIG-eligible schools with those that were not SIG eligible. Specifically, we estimated the following OLS model

$$Y_{it} = \alpha_i + \theta_t + \tau^t(SIGRecipient_i \times \theta_t) + \epsilon_{it}$$

where $SIGRecipient_i$ indicates whether or not a SIG-eligible school received a SIG grant in either cohort I or II. Note the minimal differences in pre-treatment trends in the results presented in Table B3. Thus, at least for building value-added measures, the DID approach might provide us with a plausibly causal estimate of SIG's impact.

Table B3. Impact of Receiving a SIG Award (SIG-Eligible Schools Only)

Fiscal Year	Bldg VA Math (NCEs)	Bldg VA Reading (NCEs)	Bldg Grad Rate (Pct)	Bldg VA Math (NCEs)	Bldg VA Reading (NCEs)	Bldg Grad Rate (Pct)
2007	2.18	-0.82	13.69**	_	_	_
	(2.04)	(1.64)	(6.61)			
2008	0.86	-0.55	6.77	_	_	_
	(1.90)	(1.78)	(4.83)			
2009	_	_	_	_	_	_
2010	2.00	-1.58	15.30**	1.05	-1.14	9.37
	(1.57)	(1.52)	(7.41)	(1.39)	(1.30)	(7.85)
2011	3.42**	1.80	25.51***	2.50**	2.22*	19.59**
	(1.64)	(1.30)	(7.85)	(1.22)	(1.14)	(8.29)
2012	3.14	1.24	22.07**	2.22	1.66	16.43*
	(2.11)	(1.44)	(9.11)	(1.61)	(1.35)	(9.26)
2013	4.04*	1.60	20.88**	3.13**	2.02*	15.24
	(2.07)	(1.22)	(9.60)	(1.52)	(1.06)	(9.41)
2014	2.93*	1.25	18.76**	2.01	1.67	13.12
	(1.75)	(1.48)	(8.68)	(1.38)	(1.17)	(8.47)
2015	2.00	1.19	_	1.08	1.61	_
	(2.09)	(1.52)		(1.82)	(1.44)	
N	507	507	193	507	507	193
Bldg Count	74	74	32	74	74	32
Stdnt Count	N/A	N/A	N/A	N/A	N/A	N/A
Method/Model	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel
Ref. Year	2009	2009	2009	2007-09	2007-09	2007-09

Note. The table presents coefficient estimates for the indicator of implementing a SIG model and receiving SIG funding (1) as opposed to being SIG eligible but not applying for and receiving the grant. Standard errors clustered at the building level are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

APPENDIX C. RD Analysis of Building Value-Added & Achievement Levels

The RD design uses the strict cutoff determining SIG eligibility and Priority designations to estimate the causal impact of these treatments on school quality. (See Lee and Lemieux [2010] for a description of the RD design.) In particular, we employ a combination of the RD modeling strategy we used for the covariate balance test (see Appendix A) and the DID modeling strategy (see Appendix B), as per Cellini et al (2010). The reason for this is that building value-added estimates are noisy are reveal some potential imbalance, so examining pre- and post-treatment changes in building value-added should help minimize bias from any remaining pre-treatment covariate imbalances and enhance the precision of our estimates. Modeling in this way also provides the added benefit of allowing us to easily check balance in pre-treatment trends associated with these buildings.

Specifically, we report the results of the following OLS model for each outcome:

$$Y_{it} = \alpha_i + \theta_t + \tau^t(Lowest5pct_i \times \theta_t) + \beta_1^t(X_i \times \theta_t) + \beta_2^t(X_i^2 \times \theta_t)$$
$$+\beta_3^t(Lowest5pct_i \times (X_i \times \theta_t)) + \beta_4^t\left(Lowest5pct_i \times (X_i^2 \times \theta_t)\right) + \epsilon_{it}$$

where the performance measure Y for building i in fiscal year t is a function of building fixed effects (α_i), fiscal year fixed effects (θ_t), and an interaction with a variable indicating whether or not a building was in the bottom 5 percent based on the combined proficiency rate ($Lowest5pct_i$). Once again, the building fixed effects are differenced out. The fiscal year fixed effects (θ_t) are captured through the inclusion of indicator variables for all years except the baseline pre-treatment year. As in the covariate balance tests, we include a quadratic specification for the running variable that is allowed to differ on each side of the threshold. The coefficient vector τ^t captures differences in performance trends between buildings that did and did not reside in the bottom 5 percent but that were near the proficiency cutoff.

In some specifications, we use FY2009 (for SIG), FY2010 (for SIG II), and FY2011 (for Priority) as the omitted pre-treatment baselines, but in our preferred specifications we constrain to zero multiple pre-treatment years (FY07-FY09, FY07-FY10, or FY09-FY11) in order to minimize bias and increase precision. As the tables below reveal, the method generally (though not always) reveals no discernable pre-treatment differences between treated and untreated schools. There are some instances of such imbalances, however, which is why we generally preferred to feature results from models that use three pre-treatment years as a baseline.

Table C1. Impact of SIG I Eligibility on Annual Building Value-Added and Performance Index

	Reading Value-Added (NCEs)		Math Value-Added (NCEs)		Performance Index	
2007	0.94	–	-2.93	–	3.46	–
	(2.47)		(2.72)		(3.23)	
2008	1.91	–	2.07	–	0.41	–
	(2.96)		(2.49)		(2.88)	
2009	–	–	–	–	–	–
2010	2.77	1.82	0.96	1.27	1.22	-0.06
	(2.14)	(2.31)	(1.54)	(2.01)	(2.27)	(2.64)
2011	3.02	2.07	2.37	2.69	-1.39	-2.66
	(2.22)	(2.23)	(2.24)	(2.28)	(3.06)	(2.84)
2012	6.77***	5.85**	4.04*	4.37*	3.28	2.02
	(2.44)	(2.62)	(2.39)	(2.39)	(5.16)	(4.89)
2013	3.79*	2.83	1.44	1.87	-6.19	-7.46
	(1.93)	(2.00)	(2.03)	(1.94)	(8.87)	(8.52)
2014	2.02	1.02	2.66	3.10**	-1.63	-2.97
	(1.95)	(1.71)	(1.62)	(1.34)	(4.56)	(4.30)
2015	0.84	-0.13	2.17	2.62	7.18	5.89
	(3.21)	(2.96)	(2.61)	(2.40)	(4.44)	(4.07)
N	1,836	1,836	1,836	1,836	1,896	1,896
Bldg Count	242	242	242	242	244	244
Method/Model	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel
Reference Year	2009	2007-09	2009	2007-09	2009	2007-09
Rest. Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates for the indicator of scoring below the 5 percent proficiency threshold. Standard errors clustered by building are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Table C2. Impact of SIG II Eligibility on Building Value-Added and Performance Index (no prior SIG)

	Reading Value-Added (NCEs)		Math Value-Added (NCEs)		Performance Index	
2007	-3.16*		-1.23		-4.85	
	(1.75)		(2.36)		(3.56)	
2008	-2.44	–	-1.03	–	-3.51	–
	(1.89)		(2.07)		(2.55)	
2009	-1.87	–	-0.27	–	-2.66	–
	(2.28)		(2.53)		(2.59)	
2010	–	–	–	–	–	–
2011	-0.50	1.12	-1.98	-1.45	-1.73	0.81
	(1.79)	(1.26)	(2.12)	(1.57)	(2.19)	(2.35)
2012	1.03	2.65	0.76	1.28	4.52	7.03**
	(1.93)	(1.76)	(1.72)	(1.77)	(3.59)	(3.23)
2013	-0.26	1.34	-0.98	-0.46	3.45	5.95*
	(1.93)	(1.64)	(1.45)	(1.58)	(3.47)	(3.21)
2014	-0.14	1.47	0.30	0.82	3.28	5.77**
	(1.46)	(1.21)	(1.40)	(1.47)	(2.73)	(2.56)
2015	-5.15**	-3.58*	0.45	0.94	1.61	4.14
	(2.36)	(2.11)	(2.45)	(2.04)	(4.64)	(4.70)
N	1,992	1,992	1,993	1,993	2,052	2,052
Bldg Count	246	246	246	246	247	247
Method/Model	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel
Reference Year	2010	2007-10	2010	2007-10	2010	2007-10
Rest. Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates for the indicator of scoring below the 5 percent proficiency threshold. Standard errors clustered by building are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Table C3. Impact of Priority Identification on Building Value-Added and Performance Index (no prior SIG)

	Reading Value-Added (NCEs)		Math Value-Added (NCEs)		Performance Index	
2009	-0.22	_	2.99**	_	2.44	_
	(1.23)		(1.30)		(1.83)	
2010	0.95	_	1.63	_	3.08*	_
	(1.52)		(1.42)		(1.64)	
2011	_	_	_	_	_	_
2012	1.20	0.86	2.35*	0.63	0.48	-2.33*
	(1.35)	(1.16)	(1.34)	(1.23)	(1.58)	(1.38)
2013	0.53	-0.02	1.99	-0.21	-0.77	-3.00
	(1.12)	(0.99)	(1.34)	(1.10)	(1.83)	(1.83)
2014	0.69	0.07	2.90**	1.53	-0.18	-2.54
	(1.10)	(1.01)	(1.32)	(1.22)	(2.27)	(2.22)
2015	-0.52	-1.54	-1.01	-1.91	-1.80	-3.58
	(2.19)	(2.21)	(1.64)	(1.44)	(3.05)	(2.97)
N	2,063	2,063	2,062	2,062	2,166	2,166
Bldg Count	315	315	315	315	325	325
Method/Model	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel
Specification	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.
Reference Year	2011	2009-11	2011	2009-11	2011	2009-11
Rest. Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates and standard errors for the indicator of scoring below the 5 percent proficiency threshold. Standard errors clustered by building are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Table C4. Impact of SIG Eligibility on Building Achievement Levels in Math and Reading

	SIG I Eligibility		SIG II Eligibility (no prior SIG award)	
	Reading (SDs)	Math (SDs)	Reading (SDs)	Math (SDs)
2010	0.26	0.12	_	_
	(0.19)	(013)		
2011	0.56**	0.30	0.21	-0.05
	(0.24)	(0.20)	(0.17)	(0.12)
2012	0.59**	0.25	0.40*	0.27*
	(0.28)	(0.19)	(0.22)	(0.15)
2013	0.80*	0.42	0.67**	0.44**
	(0.41)	(0.36)	(0.28)	(0.22)
2014	0.56**	0.23	0.58***	0.23
	(0.27)	(0.19)	(0.21)	(0.16)
N	1,209	1,209	1,350	1,350
Bldg Count	243	243	246	246
Method/Model	RD/Panel	RD/Panel	RD/Panel	RD/Panel
Reference Year	2009	2009	2009-10	2009-10
Rest. Bandwidth	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates for the indicator of scoring below the 5 percent proficiency threshold. Standard errors clustered by building are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

APPENDIX D. Analysis of Student-Level Data

We also conducted DID and RD analyses using student-level data that enable us to examine how interventions affected students attending schools at the time they were identified. Specifically, we focused on the achievement of students who attended the treated and untreated schools during the baseline years used to allocate the treatment: FY2009 for SIG I, FY2010 for SIG II, and FY2011 for the Priority school analysis. The logic of this analysis is that whereas schools might improve their ability to educate children as a result of these interventions, the disruption or excitement associated with the intervention might affect existing students differently than students who enter later.

First, we conducted the RD analysis using the same models as in the Appendix C. The primary difference is that observations are now at the student level instead of the building level, and students, as opposed to buildings, are identified during the baseline years. Thus, the analysis follows treated students if they transition to other schools. Table D1 presents the results of analyses of student achievement (standardized by grade, subject, and year and reported in standard deviation units) in reading, math and science.

Table D1. Impact of SIG Eligibility on Students Attending when Schools Identified

	SIG I			SIG II (no prior SIG award)			Priority (no prior SIG award)		
	Reading (SDs)	Math (SDs)	Science (SDs)	Reading (SDs)	Math (SDs)	Science (SDs)	Reading (SDs)	Math (SDs)	Science (SDs)
2010	-0.03	0.02	0.05	_	_	_	_	_	_
	(0.05)	(0.06)	(0.07)						
2011	0.02	0.08	0.03	0.07	0.08	-0.02	_	_	_
	(0.09)	(0.08)	(0.09)	(0.06)	(0.08)	(0.08)			
2012	-0.01	0.04	0.02	0.07	0.08	0.08	-0.01	0.01	-0.03
	(0.07)	(0.07)	(0.08)	(0.08)	(0.10)	(0.08)	(0.03)	(0.04)	(0.07)
2013	-0.01	0.06	0.01	0.11	0.05	-0.02	-0.07	0.03	-0.07
	(0.07)	(0.07)	(0.09)	(0.08)	(0.09)	(0.11)	(0.05)	(0.04)	(0.09)
2014	-0.01	0.01	0.05	0.14	0.10	0.02	-0.00	0.10*	0.04
	(0.07)	(0.08)	(0.11)	(0.09)	(0.08)	(0.11)	(0.06)	(0.05)	(0.08)
N	233,601	233,410	10,540	234,868	234,651	10.604	261,484	261,229	14,280
Stdnt Cnt	60,869	60,839	10,540	66,636	66,592	10,604	83,188	83,102	14,280
Bldg Cnt	244	244	228	247	247	234	325	325	302
Mthd/Mdl	RD/Panel	RD/Panel	RD/Yrly	RD/Panel	RD/Panel	RD/Yrly	RD/Panel	RD/Panel	RD/Yrly
Specif.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.
Ref. Yr	2007-09	2007-09	N/A	2008-10	2008-10	N/A	2009-11	2009-11	N/A
Rest. Bndwdth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates for the "lowest five percent" indicator variable for each year. The coefficients for science are estimated separately for each year, whereas the coefficients for math and reading are estimated using panel methods. The science achievement models include controls for a student's math and reading achievement in FY09 for SIG I, FY10 for SIG II, and FY11 for priority school identification. Observation counts for the science achievement models are from the earliest listed model. Robust standard errors clustered at the building level are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Table D2 examines whether the intervention led students to switch buildings and districts (since treatment identification) or whether it affected annual mobility rates. For example, "Diff Bldg" indicates whether a student is in a different building than in 2009 for SIG I, 2010 for SIG II, and 2011 for priority identification. "Switched building" on the other hand, identifies whether a student is in a different building than in the previous year. Thus, the estimates present the impacts of the treatments on student mobility rates.

Table D2. Impact of SIG Eligibility on Student Mobility

	SIG I			SIG II (no prior SIG award)			Priority (no prior SIG award)		
	Diff Bldg (2009)	Diff Dist (2009)	Switched Bldg (Annual)	Diff Bldg (2010)	Diff Dist (2010)	Switched Bldg (Annual)	Diff Bldg (2011)	Diff Dist (2011)	Switched Bldg (Annual)
2010	-0.03	-0.03	-0.03	_	_	_	_	_	_
	(0.07)	(0.06)	(0.07)						
2011	0.05	-0.03	0.02	0.15	0.35**	0.15	_	_	_
	(0.09)	(0.10)	(0.09)	(0.13)	(0.14)	(0.13)			
2012	0.00	-0.03	-0.01	0.13	0.39**	0.10	-0.05	-0.07*	-0.05
	(0.05)	(0.10)	(0.06)	(0.11)	(0.17)	(0.08)	(0.10)	(0.04)	(0.10)
2013	-0.01	-0.07	0.07	0.05	0.46**	0.06	-0.02	-0.08	0.00
	(0.03)	(0.12)	(0.06)	(0.06)	(0.16)	(0.11)	(0.09)	(0.05)	(0.06)
2014	0.002	-0.15	0.14**	0.04	0.45***	0.15**	-0.05	-0.08	-0.06
	(0.003)	(0.12)	(0.05)	(0.03)	(0.15)	(0.08)	(0.05)	(0.06)	(0.08)
2015	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
N	20,167	20,167	20,167	19,860	19,860	19,860	25,860	25,860	25,860
Stdnt Cnt	20,167	20,167	20,167	19,860	19,860	19,860	25,860	25,860	25,860
Bldg Cnt	230	230	230	237	237	237	305	305	305
Mthd/Mdl	RD/Yrly	RD/Yrly	RD/Yrly	RD/Yrly	RD/Yrly	RD/Yrly	RD/Yrly	RD/Yrly	RD/Yrly
Specif.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.
Rest. Bndwdth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates and standard errors for the "lowest 5 percent" indicator variable for each year. Mobility is for students in non-terminal grades. Robust standard errors clustered at the building level are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Finally, we conducted the DID analysis (see Appendix B for model specification) at the student level to compare students attending SIG-eligible schools whose schools did and did not receive a SIG award. Table D3 presents the results.

Table D3. Impact of SIG Award on Students Attending when Schools Identified

	Reading (SDs)	Math (SDs)
2010	-0.02	0.02
	(0.03)	(0.03)
2011	0.03	0.05
	(0.04)	(0.03)
2012	-0.07*	0.03
	(0.04)	(0.04)
2013	-0.04	0.05
	(0.07)	(0.08)
2014	-0.08	0.05
	(0.08)	(0.08)
N	64,591	64,532
Stdnt Cnt	20,751	20,742
Bldg Cnt	75	75
Method/Model	DID/Panel	DID/Panel
Ref. Year	2007-09	2007-09
Rest. Bndwdth	Yes	Yes

Note. The table presents coefficient estimates for the indicator of implementing a SIG model and receiving SIG funding (1) as opposed to being SIG eligible but not applying for and receiving the grant. Standard errors clustered at the building level are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Appendix E. Comparison of SIG Models

The table below presents the results of a "difference-in-differences" analysis (see Appendix B for model specification) comparing schools that implemented the SIG Turnaround model as opposed to the SIG Transformation model. As before, we estimated models that use 2009 as a baseline (so that we can look for pre-treatment differences in trends) and that use 2007-2009 as a baseline to minimize bias and enhance precision. The first six columns examine building-level school quality measures, whereas the last two examine the impact on standardized achievement scores of students attending these schools in 2009.

Table E1. Comparison of SIG "Turnaround" vs. "Transformation" Models (SIG I & II combined)

Fiscal Year	Bldg VA Math (NCEs)	Bldg VA Reading (NCEs)	Bldg Grad Rate (Pct)	Bldg VA Math (NCEs)	Bldg VA Reading (NCEs)	Bldg Grad Rate (Pct)
2007	1.19	0.69	7.29	_	_	_
	(1.37)	(2.25)	(5.59)			
2008	-2.39	-2.11	5.10	_	_	_
	(1.51)	(1.40)	(6.55)			
2009	_	_	_	_	_	_
2010	-1.72	-2.23	4.54	-1.30	-1.74	0.46
	(2.23)	(1.90)	(5.45)	(1.95)	(1.47)	(4.68)
2011	-1.10	-1.34	4.90	-0.69	-0.85	0.83
	(1.97)	(1.61)	(5.86)	(1.83)	(1.31)	(4.91)
2012	2.13	2.88	9.71	2.50	3.33**	5.64
	(1.90)	(1.80)	(7.06)	(1.87)	(1.59)	(5.87)
2013	-0.26	1.19	15.31*	0.10	1.64	11.24
	(1.46)	(1.35)	(8.50)	(1.18)	(1.06)	(7.17)
2014	0.55	-1.04	26.13***	0.91	-0.60	22.06***
	(1.51)	(1.33)	(6.87)	(1.26)	(1.17)	(6.17)
2015	0.18	0.92		0.59	1.40	
	(2.21)	(2.81)	N/A	(1.98)	(2.69)	N/A
N	404	404	252	404	404	252
Bldg Count	57	57	36	57	57	36
Stdnt Count	N/A	N/A	N/A	N/A	N/A	N/A
Mthd/Mdl	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel	DID / Panel
Ref. Year	2009	2009	2009	2007-09	2007-09	2007-09

Note. The table presents coefficient estimates for the indicator of implementing a "turnaround" model (1) as opposed to a "transformation" mode (0). Standard errors clustered at the building level are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Table E2. Impact of SIG Turnaround model relative to SIG Transformation model (SIG I & II combined; Student-level data)

Fiscal Year	Math (SDs)	Reading (SDs)	Math (SDs)	Reading (SDs)
2007	-0.02	-0.05	_	_
	(0.08)	(0.05)		
2008	-0.05	-0.01	_	_
	(0.06)	(0.04)		
2009	_	_	_	_
2010	-0.06	-0.12**	-0.04	-0.11**
	(0.05)	(0.06)	(0.05)	(0.06)
2011	-0.11*	-0.10	-0.09	-0.09
	(0.07)	(0.07)	(0.06)	(0.06)
2012	-0.06	-0.08	-0.05	-0.07
	(0.07)	(0.05)	(0.07)	(0.04)
2013	-0.06	-0.10**	-0.05	-0.09**
	(0.09)	(0.05)	(0.08)	(0.04)
2014	0.00	-0.07	0.02	-0.05
	(0.10)	(0.06)	(0.09)	(0.05)
2015	N/A	N/A	N/A	N/A
N	47,233	47,286	47,233	47,286
Bldg Count	48	48	48	48
Stdnt Count	14,472	14,478	14,472	14,478
Mthd/Mdl	DID / Panel	DID / Panel	DID / Panel	DID / Panel
Ref. Year	2009	2009	2009	2009

Note. The table presents coefficient estimates for the indicator of implementing the Turnaround model (1) as opposed to the Transformation model (0). Standard errors clustered at the building level are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

APPENDIX F. School Closure and Staffing

We estimated numerous models examining building closure and staffing changes. We report below the results of models that illustrate the types of changes we observed in schools' staffs. We do not report models of salaries, part-time vs. full-time staff, and temporary vs. certificated teachers because they did not yield significant results. Note that we indicate the analysis type (RD vs. DID) and whether coefficients were estimated simultaneously across all years (panel) or whether they were estimated year by year (yearly) at the bottom of each table. Note that the first four columns reveal turnover or closure rates since the baseline year. Thus, the closure estimates capture the impact on the proportion of schools present in the baseline year that were no longer open in a later year. Similarly, the principal turnover measure captures the proportion of principals in place in 2009 that were no longer in place in a given year.

Table F1. Impact of SIG I Eligibility on Staffing

	Closure (since 2009)	Principal Change Rate (relative to 2009)	Teacher Change Rate (relative to 2009)	Staff Change Rate (relative to 2009)	Staff Retirement Rate (annual)	Average Teacher Experience (years)	Percent of Teachers that are "HQ"	Student – Teacher Ratio	Ed Prof Turnover Rate (annual)
2010	0.00	0.26	-0.02	-0.05	0.00	-0.77	-10.11	-1.78	-0.01
	(0.003)	(0.24)	(0.05)	(0.05)	(0.01)	(0.87)	(7.19)	(3.42)	(0.10)
2011	-0.02	0.30	-0.08	-0.09	0.03**	-1.08	-2.71	2.07	0.06
	(0.16)	(0.26)	(0.07)	(0.06)	(0.01)	(1.05)	(5.84)	(5.53)	(0.14)
2012	-0.06	-0.19	-0.04	-0.11*	-0.04	-0.49	-7.94	-4.47	-0.08
	(0.16)	(0.22)	(0.07)	(0.07)	(0.04)	(1.14)	(4.97)	(4.11)	(0.12)
2013	-0.09	-0.26	-0.04	-0.10	0.00	-1.12	-4.09	-3.01	-0.06
	(0.17)	(0.23)	(0.08)	(0.07)	(0.01)	(1.26)	(5.52)	(3.62)	(0.12)
2014	-0.05	-0.10	-0.04	-0.09	0.00	-1.92	-3.66	-1.42	0.03
	(0.18)	(0.17)	(0.08)	(0.08)	(0.01)	(1.97)	(6.14)	(3.41)	(0.14)
2015						1.00	4.39	-0.18	
	N/A	N/A	N/A	N/A	N/A	(2.07)	(9.93)	(4.07)	N/A
Mthod/Mdl	RD/Yearly	RD/Yearly	RD/Yearly	RD/Yearly	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel
Specification	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.
Ref. Year	N/A	N/A	N/A	N/A	2009	2007-09	2007-09	2007-09	2009
Rest. Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates for the indicator of scoring below the combined proficiency threshold. Standard errors clustered by building are in parentheses for the panel models, whereas robust standard errors are reported in parentheses for the OLS models estimate by year. *p<0.10; **p<0.05; ***p<0.01

Table F2. Impact of SIG II Eligibility (no prior SIG award) on Staffing

	Closure (since 2009)	Principal Change Rate (relative to 2009)	Teacher Change Rate (relative to 2009)	Staff Change Rate (relative to 2009)	Staff Retirement Rate (annual)	Average Teacher Experience (years)	Percent of Teachers that are "HQ"	Student – Teacher Ratio	Ed Prof Turnover Rate (annual)
2011	0.01	0.01	-0.07	-0.03	-0.02	-0.52	-3.48	2.05	-0.14
	(0.01)	(0.23)	(0.07)	(0.07)	(0.02)	(0.86)	(4.95)	(2.37)	(0.09)
2012	-0.02	0.29**	-0.01	0.06	0.02	-0.17	-4.58	1.68	-0.07
	(0.12)	(0.12)	(0.08)	(0.08)	(0.09)	(1.18)	(6.59)	(1.51)	(0.11)
2013	0.04	0.27**	-0.04	0.00	-0.05**	-0.10	1.68	-1.91	-0.23
	(0.16)	(0.12)	(0.09)	(0.09)	(0.02)	(1.36)	(5.64)	(1.94)	(0.17)
2014	0.02	0.07	-0.06	-0.02	-0.02	2.36	4.27	-4.28***	-0.24**
	(0.17)	(0.07)	(0.08)	(0.09)	(0.02)	(1.9)	(5.36)	(1.48)	(0.11)
2015						6.68***	-4.78	2.97	
	N/A	N/A	N/A	N/A	N/A	(2.12)	(11.00)	(3.45)	N/A
Mthod/Mdl	RD/Yearly	RD/Yearly	RD/Yearly	RD/Yearly	RD/Panel	RD/Panel	RD/Panel	RD/Panel	RD/Panel
Specification	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.	Quad.
Ref. Year	N/A	N/A	N/A	N/A	2009-10	2008-10	2008-10	2008-10	2009-10
Rest. Band.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates for the indicator of scoring below the combined proficiency threshold. Standard errors clustered by building are in parentheses for the panel models, whereas robust standard errors are reported in parentheses for the OLS models estimate by year. *p<0.10; **p<0.05; ***p<0.01

Table F3. Impact of Priority Identification on Staffing (no prior SIG award)

	Closure (since 2011)	Principal Change Rate (relative to 2011)	Teacher Change Rate (relative to 2011)	Staff Change Rate (relative to 2011)	Staff Retirement Rate	Average Teacher Experience (years)	Percent of Teachers that are "HQ"	Student – Teacher Ratio	Ed Prof Turnover Rate (annual)
2012	0.06	0.06	0.13**	0.12**	-0.01	0.70	-3.20	-0.17	0.08
	(0.06)	(0.15)	(0.06)	(0.06)	(0.01)	(1.12)	(2.25)	(2.76)	(0.07)
2013	0.09	0.35*	0.14**	0.13**	-0.01	-0.20	-4.46*	-0.87	0.10
	(0.08)	(0.20)	(0.06)	(0.06)	(0.01)	(1.30)	(2.51)	(2.96)	(0.08)
2014	0.09	0.36*	0.15**	0.17***	-0.01	-0.98	-3.94	-0.95	0.14
	(0.11)	(0.19)	(0.06)	(0.06)	(0.01)	(1.54)	(4.00)	(3.17)	(0.10)
2015						-1.23	-0.01	0.20	
	N/A	N/A	N/A	N/A	N/A	(1.51)	(3.16)	(3.26)	N/A
Mthod/Mdl Specification	RD/Yearly Quad.	RD/Yearly Quad.	RD/Yearly Quad.	RD/Yearly Quad.	RD/Panel Quad.	RD/Panel Quad.	RD/Panel Quad.	RD/Panel Quad.	RD/Panel Quad.
Ref. Year	N/A	N/A	N/A	N/A	2009-11	2009-11	2009-11	2009-11	2009-11
Rest. Bandwidth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table presents coefficient estimates for the indicator of scoring below the combined proficiency threshold. Standard errors clustered by building are in parentheses for the panel models, whereas robust standard errors are reported in parentheses for the OLS models estimate by year. *p<0.10; **p<0.05; ***p<0.01

Table F4. Impact of Receiving a SIG Award (SIG-eligible buildings only)

	Principal Turnover Rate (yearly)	Ed Prof Turnover Rate (yearly)	Staff Retirement Rate	Average Teacher Experience	Pct "High Quality" Teachers	Student-Teacher Ratio
2010	-0.10	-0.16**	-0.00	-1.13	7.05	-1.73
	(0.16)	(0.08)	(0.01)	(0.77)	(6.54)	(1.88)
2011	-0.35*	-0.19**	0.00	0.64	-0.75	-9.84
	(0.19)	(0.08)	(0.01)	(1.84)	(3.23)	(8.27)
2012	-0.53***	-0.15	0.00	1.10	2.16	-0.47
	(0.20)	(0.10)	(0.02)	(1.63)	(3.92)	(1.86)
2013	-0.71***	-0.18	0.01	2.82	4.70	1.37
	(0.22)	(0.11)	(0.01)	(3.25)	(3.86)	(3.10)
2014	-0.40**	-0.11	-0.01	2.98	1.48	3.11
	(0.19)	(0.10)	(0.007)	(3.58)	(4.13)	(3.54)
2015	_	_	_	-0.75	-2.04	-0.67
				(1.66)	(4.28)	(4.49)
N	284	463	530	660	641	643
Bldg Count	69	85	85	85	85	85
Method/Model	DID/Panel	DID/Panel	DID/Panel	DID/Panel	DID/Panel	DID/Panel
Reference Yrs	2009	2009	2009	2007-09	2007-09	2007-09

Note. The table presents coefficient estimates comparing SIG eligible schools that did and did not receive a SIG award. Robust standard errors clustered at the building level are in parentheses below coefficient estimates: ^p<0.15; *p<0.10; **p<0.05; ***p<0.01

Table F5. Turnaround vs. Transformation (Buildings with SIG awards only)

	Principal Turnover Rate (annual)	Ed Prof Turnover Rate (annual)	Teacher Retirement Rate	Average Teacher Experience	Pct "High Quality" Teachers	Student-Teacher Ratio
2010	-0.01	0.03	-0.02	0.61	0.39	-4.31
	(0.14)	(0.05)	(0.02)	(0.64)	(2.33)	(4.42)
2011	0.22	0.22***	-0.01	0.39	3.28	-4.79
	(.019)	(0.06)	(0.02)	(0.75)	(3.31)	(4.51)
2012	0.23	0.31***	0.07	0.47	5.72	-4.73
	(0.18)	(0.06)	(0.05)	(0.90)	(3.55)	(4.48)
2013	0.32**	0.18**	0.00	0.45	6.03**	-3.58
	(0.12)	(0.07)	(0.02)	(1.13)	(3.00)	(4.48)
2014	-0.02	0.01	0.00	0.86	4.90	-3.23
	(0.14)	(0.09)	(0.01)	(1.31)	(4.84)	(4.45)
2015				-0.34	0.67	-4.40
	N/A	N/A	N/A	(1.41)	(8.78)	(4.75)
N	312	422	490	621	614	614
Bldg Count	68	73	73	73	73	73
Method	DID/Panel	DID/Panel	DID/Panel	DID/Panel	DID/Panel	DID/Panel

Note. The table presents differences-in-differences (DID) estimates and standard errors for models comparing SIG schools that implemented the "turnaround" model relative to those that implemented the "transformation" model. Robust standard errors clustered at the building level are in parentheses below coefficient estimates: ^p<0.15; *p<0.10; **p<0.05; ***p<0.01

APPENDIX G. Impact of Receiving a SIG Award (Fuzzy RD)

We also estimated the impact of SIG awards using an instrumental variables approach. Specifically, we used a Two-stage Least Squares model to conduct a "fuzzy RD" analysis. The first stage estimates the impact of the FY2009 or FY2010 proficiency rate threshold for SIG eligibility on the probability of schools receiving a SIG award using a quadratic specification of the running variable interacted with the treatment indicators, and the second stage employs the predicted probability of the SIG award indicator on various outcomes. The models include baseline value-added and achievement levels in the regressions to minimize bias and enhance precision.

First stage results indicate that the SIG I and SIG II thresholds are good predictors of receiving SIG funding in 2011, 2012, 2013, and 2014. The SIG I threshold indicator has coefficients (and significance levels) of 0.59 (p=0.001) for 2011 and 0.40 (p=.015) for 2012-2014. The SIG II threshold indicator has a coefficient (and significance level) of 0.23 (p=0.078) for 2011 and 0.72 (p=0.000) for 2012-2014. We do not review these results thoroughly because of the relative imprecision of the estimates. The tables below present second-stage results for some key covariates using models that exclude charter schools, which yield comparable but somewhat stronger effects than models that include charter schools.

Table G1. Impact of SIG Funding (Instrument is SIG I indicator; Charter Schools Excluded)

	Reading VA	Math VA	Closure	Principal Change Since 2009	Staff Change Since 2009	Teacher Change Since 2009
2011	2.27	2.90	0.09	-0.31	-0.12*	-0.11
	(2.33)	(2.79)	(0.33)	(0.29)	(0.06)	(0.08)
2012	5.62**	3.30	-0.08	-0.67*	-0.21**	-0.16
	(2.51)	(2.67)	(0.37)	(0.35)	(0.08)	(0.11)
2013	0.19	-1.55	-0.20	-0.75**	-0.22**	-0.29**
	(1.80)	(1.47)	(0.34)	(0.33)	(0.10)	(0.11)
2014	-2.51	-1.34	-0.21	-0.33	-0.14	-.14
	(1.53)	(1.30)	(0.24)	(0.33)	(0.09)	(0.12)
Method/Model Specification	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.

Note. The table presents coefficient estimates for a variable capturing the probability that a school received a SIG award. The estimate for each year is from a separate regression. Robust standard errors are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

Table G2. Impact of SIG Funding (Instrument is SIG II indicator; Charter Schools Excluded)

	Reading VA	Math VA	Closure	Principal Change Since 2009	Staff Change Since 2009	Teacher Change Since 2009
2011	-2.87	-9.39	0.01	0.35	-0.10	-0.09
	(8.68)	(14.60)	(0.04)	(0.68)	(0.28)	(0.30)
2012	2.18	1.62	-0.08*	0.13	0.05	0.01
	(1.69)	(1.70)	(0.04)	(0.14)	(0.07)	(0.09)
2013	-2.20	-1.72	0.08	0.23	-0.04	-0.06
	(1.65)	(1.78)	(0.21)	(0.17)	(0.09)	(0.10)
2014	-0.75	-0.99	0.08	0.00	-0.08	-0.09
	(0.68)	(1.64)	(0.25)	(0.10)	(0.06)	(0.07)
Method/Model Specification	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.	Fuzzy RD/ 2SLS Quad.

Note. The table presents coefficient estimates for a variable capturing the probability that a school received a SIG award. The estimate for each year is from a separate regression. Robust standard errors are in parentheses below coefficient estimates: *p<0.10; **p<0.05; ***p<0.01

APPENDIX H. Converting Estimates to Student-Level SDs and Days of Learning

All of the school-level value-added impact estimates in the body of the report are reported in standard deviation units. However, the analyses themselves—which we report in the appendix—were done using ODE's value-added measures, which are in Normal Curve Equivalent (NCE) units. To characterize effects in terms of standard deviation units, we converted the results in the appendix from NCE units to standard deviations by dividing the NCE estimates by 21.063. Those are the estimates of impacts on student achievement growth that we report in the figures. When discussing the results in those figures, we frequently refer to them in terms of extra "days of learning." To obtain this estimate, we divided the standard deviation estimates by the average annual achievement growth of students in those grades (providing us with a proportion in terms of an annual year of learning) and multiplied by 180 days to get "days of learning" based on an 180-day school year. Specifically, based on Hill et al. (2008), we assumed average learning gains in grade 4-8 of 0.314 in reading and 0.422 in math. We provide the calculations below for each figure reported in the main body of the report.

Figure 1a. SIG I Eligibility

	RD - Bldg Data (NCE)			**RD - Bldg Data (SD)**			**Days of Learning**	
	Reading	Math		Reading	Math		Reading	Math
2010	1.82	1.27	2010	0.09	0.06	2010	49.53	25.72
2011	2.07	2.69	2011	0.10	0.13	2011	56.34	54.47
2012	5.85	4.37	2012	0.28	0.21	2012	159.21	88.50
2013	2.83	1.87	2013	0.13	0.09	2013	77.02	37.87
2014	1.02	3.10	2014	0.05	0.15	2014	27.76	62.78
2015	-0.13	2.62	2015	-0.01	0.12	2015	-3.54	53.06

Figure 1b. SIG II Eligibility

	RD - Bldg Data (NCE)			**RD - Bldg Data (SD)**			**Days of Learning**	
	Reading	Math		Reading	Math		Reading	Math
2011	1.12	-1.45	2011	0.05	-0.07	2011	30.48	-29.36
2012	2.65	1.28	2012	0.13	0.06	2012	72.12	25.92
2013	1.34	-0.46	2013	0.06	-0.02	2013	36.47	-9.32
2014	1.47	0.82	2014	0.07	0.04	2014	40.01	16.61
2015	-3.58	0.94	2015	-0.17	0.04	2015	-97.43	19.04

Figure 1c. Priority School Identification

	RD - Bldg Data (NCE)			**RD - Bldg Data (SD)**			**Days of Learning**	
	Reading	Math		Reading	Math		Reading	Math
2012	0.86	0.63	2012	0.04	0.03	2012	23.41	12.76
2013	-0.02	-0.21	2013	0.00	-0.01	2013	-0.54	-4.25
2014	0.07	1.53	2014	0.00	0.07	2014	1.91	30.98
2015	-1.54	-1.91	2015	-0.07	-0.09	2015	-41.91	-38.68

Figure 2. Impact of SIG Eligibility on Achievement Levels

	RD - Bldg Data (SD)			**Days of Learning**	
	SIG I Reading	SIG II Reading		Reading (SIG I)	Reading (SIG II)
2010	0.26	N/A	2010	149.04	N/A
2011	0.56	0.21	2011	321.02	120.38
2012	0.59	0.40	2012	338.22	229.30
2013	0.80	0.67	2013	458.60	384.08
2014	0.56	0.58	2014	321.02	332.48

Figure 4. Impact of SIG Award (SIG Eligible Only)

	Reading	Math		Reading	Math		Reading	Math
2010	-1.14	1.05	2010	-0.05	0.05	2010	-31.03	21.26
2011	2.22	2.50	2011	0.11	0.12	2011	60.42	50.63
2012	1.66	2.22	2012	0.08	0.11	2012	45.18	44.96
2013	2.02	3.13	2013	0.10	0.15	2013	54.98	63.38
2014	1.67	2.01	2014	0.08	0.10	2014	45.45	40.70
2015	1.61	1.08	2015	0.08	0.05	2015	43.82	21.87

Figure 5. SIG Turnaround vs. SIG Transformation

	Reading	Math		Reading	Math			
2010	-1.74	-1.30	2010	-0.08	-0.06	2010	-47.36	-26.33
2011	-0.85	-0.69	2011	-0.04	-0.03	2011	-23.13	-13.97
2012	3.33	2.50	2012	0.16	0.12	2012	90.63	50.63
2013	1.64	0.10	2013	0.08	0.00	2013	44.63	2.03
2014	-0.6	0.91	2014	-0.03	0.04	2014	-16.33	18.43
2015	1.4	0.59	2015	0.07	0.03	2015	38.10	11.95

Figure 8. SIG Turnaround vs. SIG Transformation (DID; Student-Level Achievement)

	Reading	Math			
2010	-0.11	-0.04	2010	-63.06	-17.06
2011	-0.09	-0.09	2011	-51.59	-38.39
2012	-0.07	-0.05	2012	-40.13	-21.33
2013	-0.09	-0.05	2013	-51.59	-21.33
2014	-0.05	0.02	2014	-28.66	8.53

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