RESEARCH REPORT

Round 4 of the Partnership Model: 2023-24 Outcomes Report

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October 2025

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ACKNOWLEDGMENTS

The authors wish to acknowledge the many people who graciously gave of their time in support of this effort. We are especially grateful to our partners for their collaboration and thoughtful feedback.

In particular, we would like to thank Gloria Chapman, Dr. Delsa Chapman, Dr. Sue Carnell, and Dr. Michael Rice from the Michigan Department of Education; Tom Howell, Mike McGroarty, and Lauren Paluta from the Center for Educational Performance and Information; Kyle Kwaiser and Nicole Wagner from the Michigan Education Data Center at the University of Michigan; and Don Dailey and Kevin Bullard from the Michigan Data Hub.

At Michigan State University, we thank Wei-Chu Chen and Ariell Bertrand for their assistance with data collection and analysis. We also thank Emily Mohr, Tara Kilbride, and Erin Conley for ideas and comments. Finally, we thank Michelle Huhn for her assistance with graphic design and Bridgette Redman for her excellent copy-editing.

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Section One: Introduction

The Partnership Model of School and District Turnaround is Michigan's policy for improving student outcomes in its lowest performing schools. In compliance with the federal Every Student Succeeds Act (ESSA), the Michigan Department of Education (MDE) identifies as Partnership schools those in the bottom 5% of the Michigan School Index System and with graduation rates below 67%. Districts with at least one Partnership school enter into agreements with MDE and their Intermediate School Districts (ISDs) and are designated as Partnership Districts. These Partnership Agreements outline improvement goals, accountability measures, and the support that MDE and the ISD will provide to the district and its schools. (See Singer & Cullum, 2023, for additional details about ESSA, the Partnership Model, the Michigan School Index System, and the identification process for Partnership districts and schools).

The underlying theory of change for the Partnership Model is that a combination of supports and accountability can help districts build capacity for school improvement, which in turn allows them to support their lowest performing schools, leading to improved instructional quality, teacher retention, and ultimately student outcomes (Burns et al., 2023; Strunk et al., 2019, 2020). In the initial rounds of the Partnership Model (Rounds 1, 2, and 3), Partnership schools made greater gains in math and ELA and had higher teacher retention rates than comparable non-Partnership schools, including through the COVID-19 pandemic (Burns et al., 2023; Cullum & Harbatkin, 2023; Harbatkin et al., 2023b; Harbatkin et al., 2025; Strunk et al., 2022).

MDE identified the current round of Partnership schools and districts in November 2022 (Singer & Cullum, 2023), with the remainder of 2022-23 school year serving as the planning year. During this first year, districts developed their Partnership Agreements, which serve as the basis for three-year improvement plans that exist as living documents in the Michigan Integrated Continuous Improvement Platform (MICIP). The agreements and MICIP plans outline academic and non-academic improvement goals and the specific initiatives to achieve those goals. Academic goals focused on student proficiency rates and benchmark growth, whereas non-academic goals dealt

with more proximate outcomes such as student attendance, chronic student absenteeism, and, to a lesser extent, staffing. To achieve those goals, districts laid out strategies related to curriculum and instruction, human capital, data systems, and external partnerships (Cullum et al., 2024).

Two process-related themes were consistent across these improvement plans, both of which are consistent with a large literature on school turnaround. The first is that providing students with an adequate education first requires consistently bringing them into the school building (Henry et al., 2020). Indeed, greater student absences lead directly to lower test scores (Gershenson et al., 2017; Gottfried, 2014a, 2014b). More generally, high levels of absenteeism may disrupt organizational coherence in schools, from instructional coordination (Goodman, 2014) to lower teacher satisfaction and worse teacher-student relationships (Gottfried et al., 2024; Gottfried et al., 2025). The second is that human resources, in the form of stable, highly qualified instructional staff, are at the center of any school improvement strategy (Harbatkin et al., 2025; Henry et al., 2020; Malen & Rice, 2016; Pham, 2023; Strunk et al., 2016).

During the second year, districts began implementing their improvement plans in earnest. They were eligible to apply for and receive state 21h funding for school improvement. This funding totals about $6 million per year to be shared across all Partnership districts and is intended to help fund improvement efforts. Districts used these funds for activities and strategies related to staffing, professional development, data systems, and curriculum (Cullum et al., 2024).

In this report, we examine the effect of the Partnership Model on student achievement in Round 4 Partnership schools. We then move to the intervention's effect on two intermediate outcomes that districts targeted in their improvement plans—teacher turnover and student attendance. We leverage teacher and principal survey data to further explore potential mechanisms underlying our causal findings, but point interested readers to the 2023-24 Implementation Report for more on Partnership implementation in the first two intervention years. We conclude with implications for policy and practice.

Key findings include:

- Round 4 Partnership had no overall effect on math or ELA achievement in grades 4–8; however, we do find evidence of moderately sized math score gains in schools newly identified for Partnership for the first time in Round 4.
- Although Round 4 Partnership had no overall effect on student attendance-related outcomes, there were pockets of improvements that may have translated to elementary and middle school math score gains. Among 4th- through 8th-graders in newly identified schools, we find increased attendance, decreased absences, and decreased chronic absenteeism.
- Teacher turnover ticked upward across Partnership schools in the first implementation year. This increase was driven by within-district transfers across all schools and between-district transfers in newly identified Partnership schools.

In the remainder of this report, we use "Partnership schools" to refer to Round 4 Partnership schools. Therefore, any reference to Partnership schools refers to Round 4 unless explicitly stated otherwise.

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Section Two: Data and Methods

This report draws largely on statewide administrative data from the Michigan Department of Education (MDE) and Center for Educational Performance and Information (CEPI). In some places, we refer to Partnership district educator survey data to help explain potential mechanisms underlying our administrative data findings. We point interested readers to the Round 4 2023-24 Implementation Report for more details on these data. In this section, we describe our data sources and analytic strategies. Table 2.1 overviews our data sources and the outcomes we draw from each.

TABLE 2.1. Data Sources

Data	Outcomes of Interest	Source	Year(s)	Sample Size
Teacher administrative records	Teacher turnover	MDE and CEPI	2022-23 through 2023-24	122,049 teacher-year observations, including 69,628 unique teachers
Student administrative records	Math and English Language Arts (ELA) achievement, attendance, absenteeism	MDE and CEPI	2022-23 through 2023-24	2,619,350 student-year observations, including 1,461,398 unique students

SAMPLE AND DATA

Michigan identified 109 Round 4 Partnership schools across 51 districts. Most (99) of these schools were designated for Partnership because their Michigan School Index placed them in the bottom 5% of schools in the state. The other 10 were identified because they were high schools with graduation rates below 67%. Thirteen of the 99 in the bottom 5% also had graduation rates below 67%. Just under half of the Round 4 schools (46%) had been Partnership schools in one of the prior identification rounds (i.e., Round 1, 2, or 3, all of which were identified before the pandemic). Two-thirds are traditional public schools while one-third are charter schools. For more information on the sample of Round 4 Partnership schools, please refer to our Round 4 Identification Report.

Statewide administrative data include longitudinal data on students and teachers in Michigan public schools. Analyses using administrative data in this report focus largely on students and teachers in Round 4 Partnership schools and those in a set of similar comparison schools in 2022-23 and 2023-24. In our econometric analyses, we also estimate separate effects for schools that were reidentified from prior Partnership rounds compared with schools that were newly identified in this round.

In descriptive analyses, we draw on statewide data from 2017-18 onward and look across the state. We break state schools into six subgroups for these descriptive analyses in order to paint a picture of Partnership school trajectories relative to others throughout the state. Specifically, our descriptive analyses show:

- **Round 4 Partnership schools** (in some cases separately by reidentified vs. newly identified Partnership schools)
- **Non-Partnership Comprehensive Support and Improvement (CSI) schools**, which are schools that were designated for CSI but not for Partnership. This can occur if schools did not fall into the bottom 5% on the state index but either failed to meet exit criteria from the prior round of CSI or had a persistently low-performing subgroup of students (i.e., transitioned from Additional Targeted Support (ATS) in the prior round to CSI).
- **Non-Partnership schools in Partnership districts**, which are those schools that were not identified for Partnership but may benefit if the Partnership Model successfully builds district capacity. Alternatively, if district leaders redirect resources from non-Partnership to Partnership schools, these schools could experience declines.
- **Non-Partnership low-performing schools**, which were relatively low performing schools but still above the Partnership designation threshold. We split these into two subcategories based on their percentile rank in the Partnership designation year:
  - **Schools with an index score in the 6th to 10th percentile**, which overlap with the regression discontinuity comparison groups.
  - **Schools with an index score in the 11th to 15th percentile**, which are slightly higher performing schools that are not identified for Partnership. The lower performing of these also overlap with the regression discontinuity comparison groups.
- **All other public schools in the state**, including average to higher achieving schools.

We draw these categories from performance in the 2021-22 school year, which is the year that Round 4 Partnership and CSI assignments were based on. We provide a descriptive table based on data-driven samples for the econometric analyses that we detail below. Specifically, we focus on a narrow sample of Partnership and non-Partnership schools just around the cutoff for Partnership. This is the "optimal bandwidth" for the regression discontinuity models that we describe below.

To examine teacher turnover in Partnership schools, we use administrative data records on Michigan public school teachers provided by MDE and CEPI from 2013-14 through fall 2025. We define public school teachers as public-school employees (both TPS and charter) whose primary position is as a teacher. To examine student achievement and attendance/absenteeism, we draw on records from Michigan K-12 student test scores, days attended and days enrolled, race/ethnicity, gender, school placement, special education status, English learner status, and socioeconomic status.

Because Partnership schools were identified based on their Michigan School Index, our analytic sample for the econometric analyses described below begins from all 2,699 schools in the state that met inclusion criteria in the 2021-22 school year.[1] In order to obtain a clean estimate of Round 4 Partnership's effect, we then exclude from the sample any school that was designated as CSI but

not as Partnership. This includes schools that either were previously designated as ATS or CSI and did not improve their proficiency levels or schools previously designated as ATS that had the same demographic sub-group identified as low-performing and therefore were converted to CSI. Any eligible school from that group with an index score that fell below the state threshold (23.88 on the 100-point index) was designated as a Partnership school, while any school above that threshold was not (with the exception of high schools identified due to low graduation rates). To that end, our comparison group is made up of non-CSI schools just above the Partnership threshold. We provide more detail on the data-driven process we used to select the comparison group in the Analyses subsection below.

In total, the 109 Partnership schools had 2,263 teachers in 2022-23 and 2,310 teachers in 2023-24. While our analytic strategy involves examining the robustness of our findings by varying the comparison group, our preferred teacher comparison group includes 2,589 non-Partnership schools with 60,104 teachers in 2022-23 and 61,578 teachers in 2023-24.

Our student outcomes analyses include two nested samples. The first, for which we can observe attendance and absenteeism, includes students in all grades and consists of 658,267 unique students in 2022-23 and 661,154 in 2023-24. Nested within that sample, we can observe math and ELA achievement for students in grades 4-8.[2] This sample includes 449,744 unique students in 2022-23 and 455,880 unique students in 2023-24.

MEASURES

Outcomes

Teacher turnover. We examine four different measures of teacher turnover—three mutually-exclusive categories and a catch-all that captures turnover for any pathway out. The three mutually exclusive categories are within-district transfer, out-of-district transfer, and leaving Michigan education.

Student attendance/absenteeism. We examine three different measures of student attendance/absenteeism. We measure attendance using attendance rate, which reflects the share of enrolled days that a student was recorded as having attended school. We create two measures of absenteeism—number of days absent (a simple sum of days absent), and a binary indicator for chronic absenteeism. The chronic absenteeism variable takes a value of one if a student is absent for 10% or more of enrolled days and zero otherwise.

Student achievement. We measure student achievement using math and ELA test scores from the Michigan Student Test of Educational Progress (M-STEP, grades 3-7) and the Preliminary Scholastic Achievement Test (PSAT, grade 8). We standardize the scale scores by subject, grade, and year, to have a mean of zero and a standard deviation of one.

Table 2.2 provides summary statistics on Partnership schools relative to non-Partnership non-CSI schools in the first two columns—the starting point for our econometric analyses. Panel A shows that students in Partnership schools have lower test scores and attendance than non-Partnership, non-CSI schools. This is expected as test scores and attendance are part of the index score that determines Partnership designation. Panel B shows that teacher turnover is higher in Partnership schools, with a particularly pronounced difference in within-district transfers.

TABLE 2.2. Descriptive Statistics					
Panel A. Students					
	Partnership	**Non-Partnership, non-CSI**	**Non-Partnership CSI[1]**	**Partnership within preferred bandwidth**	**Non-Partnership, non-CSI within preferred bandwidth**
OUTCOMES					
Math score	-1.028 (0.768)	0.079 (0.978)	-0.741 (0.863)	-1.033 (0.765)	-0.899 (0.786)
ELA score	-0.904 (0.774)	0.068 (0.985)	-0.622 (0.899)	-0.909 (0.763)	-0.819 (0.786)
Attendance rate	0.813 (0.149)	0.924 (0.078)	0.877 (0.141)	0.826 (0.136)	0.851 (0.125)
Number of absences	31.5 (24.9)	13.0 (13.1)	20.5 (23.2)	29.5 (22.9)	25.2 (20.5)
Chronic absenteeism	0.678 (0.467)	0.235 (0.424)	0.439 (0.496)	0.655 (0.476)	0.579 (0.494)
COVARIATES					
Economically disadvantaged	0.921 (0.269)	0.513 (0.500)	0.808 (0.394)	0.925 (0.264)	0.918 (0.275)
Black	0.789 (0.408)	0.143 (0.350)	0.463 (0.499)	0.792 (0.406)	0.783 (0.412)
Hispanic/Latine	0.072 (0.258)	0.088 (0.284)	0.102 (0.303)	0.065 (0.247)	0.107 (0.309)
White	0.082 (0.274)	0.673 (0.469)	0.360 (0.480)	0.078 (0.267)	0.063 (0.243)
Special education	0.149 (0.356)	0.129 (0.335)	0.145 (0.352)	0.150 (0.357)	0.141 (0.348)
English learner	0.070 (0.256)	0.068 (0.252)	0.089 (0.285)	0.082 (0.275)	0.067 (0.250)
Panel B. Teachers					
OUTCOMES					
Turnover (any pathway out)	0.233 (0.423)	0.107 (0.309)	0.169 (0.375)	0.228 (0.420)	0.203 (0.402)
Within-district transfer	0.084 (0.278)	0.035 (0.183)	0.048 (0.214)	0.074 (0.263)	0.096 (0.294)
Out-of-district transfer	0.047 (0.212)	0.022 (0.146)	0.047 (0.212)	0.042 (0.202)	0.047 (0.211)
Leave Michigan education	0.101 (0.302)	0.050 (0.218)	0.074 (0.261)	0.110 (0.313)	0.066 (0.248)
COVARIATES					
Male	0.279 (0.448)	0.221 (0.415)	0.244 (0.430)	0.268 (0.443)	0.227 (0.419)
Black	0.470 (0.499)	0.051 (0.221)	0.245 (0.430)	0.447 (0.497)	0.385 (0.487)
Hispanic/Latine	0.021 (0.144)	0.016 (0.125)	0.021 (0.143)	0.024 (0.153)	0.029 (0.167)
Experience (years)	11.22 (10.60)	13.13 (9.72)	9.75 (9.46)	11.38 (10.83)	11.21 (10.13)

[1] Non-Partnership CSI schools are excluded from the comparison group in the econometric analyses.

Notes: Table shows means with standard deviations in parentheses. Figures based on 2022-23 school year. Columns showing sample within the bandwidth are restricted to 3.45 (the optimal bandwidth based on math test scores in 2022-23) in Panel A and 6.00 (the optimal bandwidth for the leaving for any pathway out outcome in 2022-23) in Panel B.

The third column provides these figures for non-Partnership CSI schools, which we exclude from the comparison group in our analyses. On average, non-Partnership CSI schools fall between Partnership schools and non-Partnership, non-CSI schools on these measures. Though they are more observably similar to Partnership schools than the schools in Column 2, we do not use them as a comparison group because they receive some degree of "treatment" due to their CSI designation.[3]

The last two columns compare just those schools within an optimal bandwidth around the CSI threshold of 23.88 as determined by the regression discontinuity analyses described below. The figures shown use a bandwidth of 3.82 points for students and 6.00 points for teachers. In Panel A, we show descriptive statistics for students in schools with index scores ranging from 20.06 to 27.70. Column 3 lists those below the threshold (Partnership), while Column 4 lists those above it (non-Partnership). By narrowing our sample to students and teachers in schools just around the cutoff, we home in on a more comparable group than shown in either Column 2 or 3. That said, while the two groups remain observably different on some measures, these differences are small after controlling for the Michigan Index Score.

Other Variables

We use a rich set of covariates in our analyses. At the teacher level, these include indicators for male, teacher race/ethnicity (Black, Hispanic/Latine, and other racial identity, with White as the reference category), and years of teaching experience. At the student level, these include race/ethnicity, as well as indicators for male, economic disadvantage, special education, and English learner status. Analyses also include school-level means of the student demographic variables and a logged function of student enrollment.

ANALYSES

We begin with simple descriptive analyses displaying our outcomes over time for Partnership schools relative to all other non-CSI schools. These provide a visual depiction of teacher turnover, student attendance/absenteeism, and student achievement in Partnership schools relative to non-Partnership schools before and after Partnership was implemented.

Next, we run regression discontinuity models, exploiting the Michigan School Index cutoff that determines which schools are assigned to Partnership and which are not. To do so, we compare the outcomes of students and teachers in schools just below the eligibility threshold with those just above the threshold. This approach can identify a causal effect of Partnership as long as the threshold is (a) endogenously determined (i.e., it was not determined in order to intentionally place some schools in or out of Partnership), and (b) schools are unable to manipulate their index scores in order to find their way into or out of Partnership (Cattaneo et al., 2019; Imbens & Lemieux, 2007). We conduct a variety of robustness checks to assure the validity of our estimates, including statistical tests for manipulation, placebo tests (i.e., checking to see whether we find "effects" at fake thresholds, which would call into question any effects we found at the true threshold), and examinations of total and differential attrition. We provide the results of these tests in the appendix but note here that our analyses provide evidence in support of using regression discontinuity to identify a causal effect.

Our regression discontinuity model takes the form:

$$y_{is}=\beta_1 I(IndexScore<0)_s+\beta_2 f(IndexScore)_s+\beta_3[I(IndexScore<0)\times f(IndexScore)]_s+\gamma \boldsymbol{X}'_s+\sigma \boldsymbol{K}'_i+\varepsilon_{is},$$

where y is the outcome for student or teacher i in school s, and $IndexScore$ represents the Michigan School Index Score centered at 23.88 (i.e., a school with a score below 23.88 would have a negative centered score and a school with a score above 23.88 would have a positive centered score). $I(IndexScore<0)$ denotes a binary variable that takes a value of one if a school is below the Partnership threshold and zero otherwise, and $f(IndexScore)$ represents a flexible function of the distance from the eligibility threshold. We determine the function of the index score using a mean square error-optimal bandwidth selection procedure that accounts for the clustering of students (teachers) within schools (Calonico et al., 2014, 2017). $\boldsymbol{X}'$ is a vector of school covariates as described above, and $\boldsymbol{K}'$ is a vector of teacher covariates for the teacher turnover models and student covariates for the models predicting student outcomes. For the student achievement models, the $\boldsymbol{X}'$ vector also includes the student's lagged test score in the relevant subject area. In the models predicting number of days absent, the $\boldsymbol{X}'$ vector includes the number of days the student was enrolled in the school. In the models predicting teacher turnover, the $\boldsymbol{X}'$ vector also includes school-level fixed effects (i.e., elementary, middle, high).

Our estimate of primary interest is represented by β_1, which provides the estimated effect of being below the eligibility cutoff after controlling for the Michigan Index Score. Mechanically, this estimate denotes the vertical difference between Partnership and non-Partnership schools at the cutoff. Intuitively, the estimate provides the difference in outcome between students or teachers in schools that were very similar with respect to student outcomes when Partnership schools were designated.

We run these models on our full sample (i.e., students/teachers in schools that met the state's inclusion criteria for a Michigan School Index and were not designated as CSI but were not identified for Partnership). We then estimate separate models that (a) exclude newly identified Partnership schools (i.e., only estimate an effect for schools that were reidentified), and (b) exclude reidentified schools (i.e., only estimate an effect for schools that are newly identified). We show these subgroup results in the main text when the groups show meaningful differences and provide all results in the appendix.

We present results as coefficient plots, which show the point estimates as markers and the 95% confidence intervals as capped spikes. We provide more traditional regression discontinuity plots in the appendix along with the regression tables.

Of particular importance, the regression discontinuity identifies a local average treatment effect (LATE) rather than an average treatment effect. This means that the estimates we report are the effects for schools closest to the Partnership eligibility threshold. The benefit is that we are comparing very similar schools—i.e., schools with an index score of 23.8 are similar in expectation to those with an index score of 23.9—so the two groups are what we call "equal in expectation" *but for* the treatment. In other words, if we observe a difference between Partnership and non-Partnership schools closest to the threshold, that difference is very likely driven by Partnership itself and not other differences between the two groups. The primary drawback is that the results are generalizable to schools within a narrow band of only school index scores.

SECTION TWO NOTES

1. We began with 3,367 schools that received an index score, then restricted the sample to schools that were eligible for CSI designation based on enrollment (i.e., meeting minimum N-sizes in tested grades and not serving 100% special or alternative education students). For teachers, we then further restricted the sample to teachers in schools that had at least five teachers.

2. While Michigan tests all students in math and ELA in grades 3-8, we use grade 3 scores as a lagged outcome to account for baseline differences in student achievement by measuring year-to-year growth.

3. CSI schools do not receive support through the Office of Partnership Districts or 21h funds. Instead, they are supported through the state's Office of Educational Supports (OES). They submit and can receive state feedback on a CSI Plan Report to guide improvement efforts. They can also request technical assistance and professional development from OES. Such supports are less intensive than those provided through OPD.

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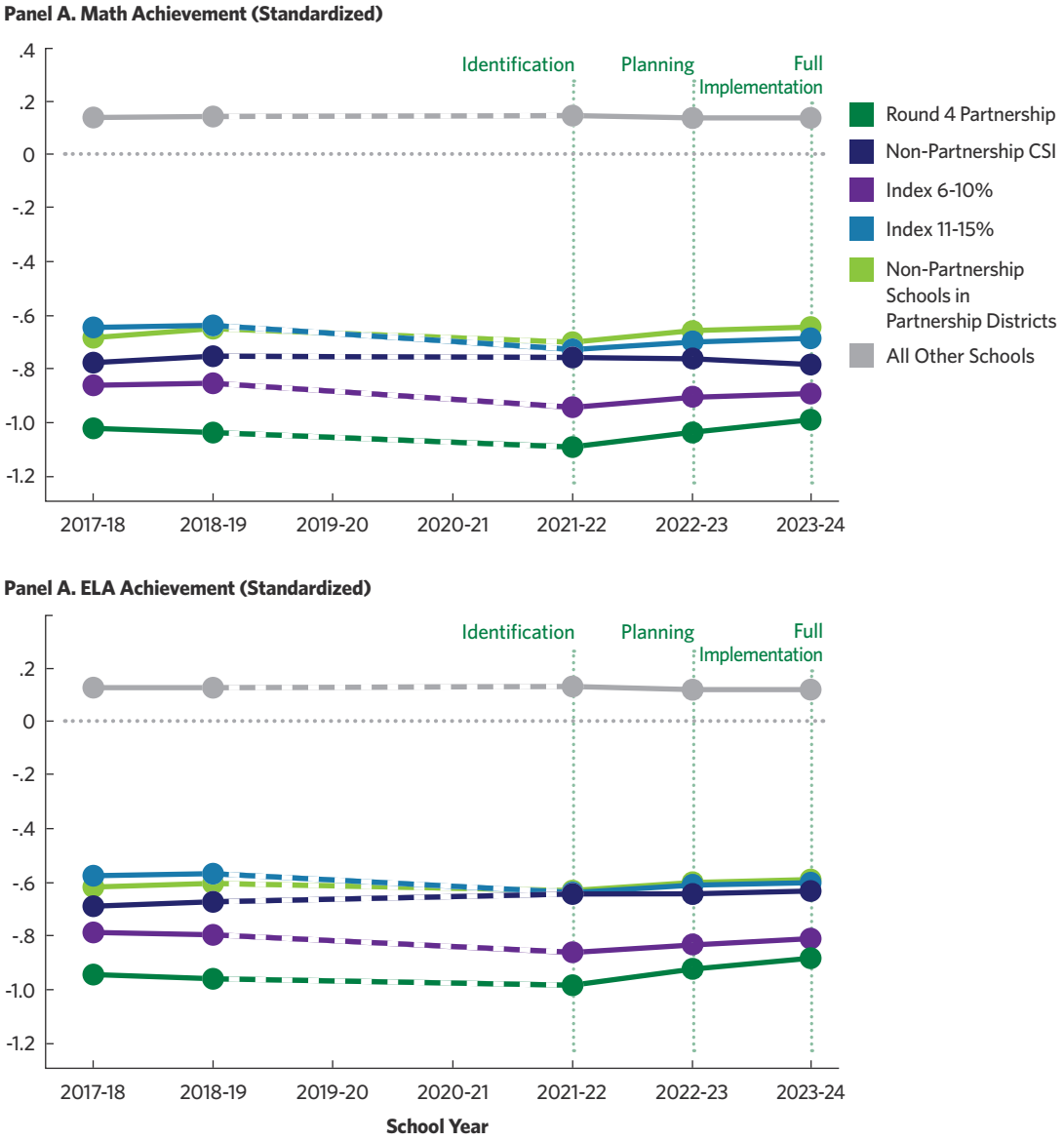
Section Three: Student Achievement

Figure 3.1 shows test scores over time for grade 3–8 students in Round 4 Partnership schools relative to the five other subgroups of schools described in Section Two. Because the scores are standardized to have a mean of zero in each year within grade and subject, groups should be interpreted relative to one another.

As expected, the "all other schools" group is the highest performing, just above the zero line that denotes the mean. Partnership schools were the lowest performing. They followed a downward trajectory before and through the pandemic, but rebounded after they were designated for Partnership, demonstrating improvements in 2022-23 and 2023-24. While this upturn points to the possibility of a positive Partnership effect, it may also be driven by a statistical phenomenon known as "mean regression." Mean regression occurs when, for instance, scores become so low that they can only increase from where they are (or so high that they can only decrease). These descriptive graphs suggest that at least some of the increase is driven by mean regression. In particular, the 6-10% group (i.e., the next lowest performing schools) also increased—though to a slightly lower degree than Partnership schools— in 2023-24 in math. Non-Partnership schools in Partnership districts also increased slightly, providing some suggestive evidence that districtwide capacity building might benefit all schools within a district rather than just Partnership schools. CSI schools that did not receive Partnership supports remained relatively stagnant.

Consistent with Figure 3.1, which suggests that Partnership schools made similar gains to non-Partnership schools just above the assignment threshold, our regression discontinuity analyses do not show strong evidence that Partnership affected grade 4-8 math or ELA achievement—though we do find some limited evidence of math gains in newly identified Partnership schools. Figure 3.2 displays these results graphically within our preferred bandwidth, with math in Panel A and ELA in Panel B. In our regression discontinuity estimates, we also split Partnership schools into two groups based on whether they were reidentified from one of the first three Partnership rounds or newly identified in this round. Green circular markers show overall results, blue square markers show results for reidentified schools, and teal diamond markers show results for newly identified schools. Capped spikes are 95% confidence intervals. When these spikes overlap with the horizontal zero line, an estimate is not statistically distinguishable from zero.

FIGURE 3.1. Student Achievement Over Time



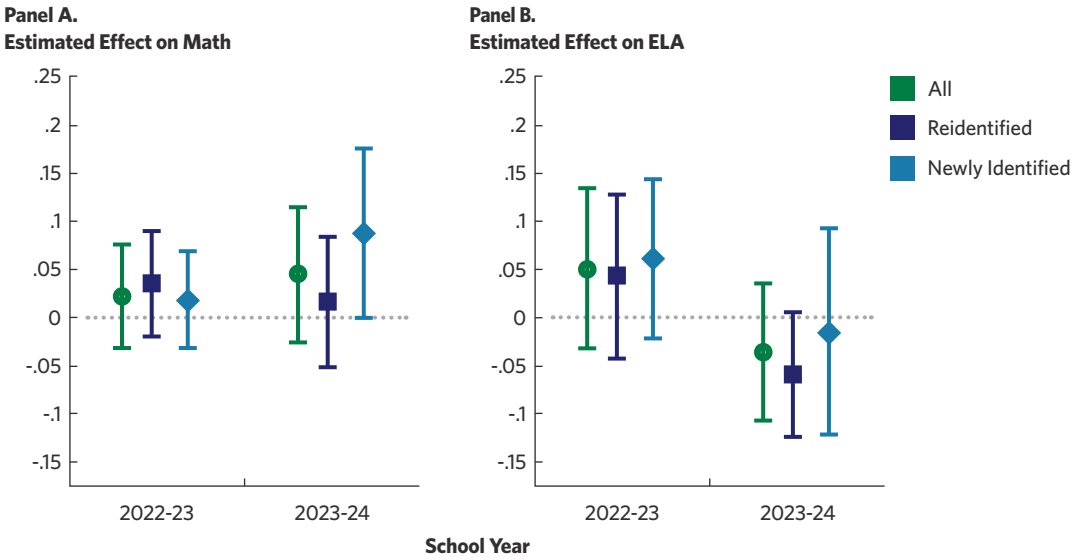
Notes: Markers show average standardized test scores for math (Panel A) and ELA (Panel B). Line is dashed between 2018-19 and 2021-22 because there was no testing in 2019-20 and minimum participation thresholds were suspended in 2020-21 due to the COVID-19 pandemic. Scores are standardized by subject, grade, and year, to have a mean of zero and a standard deviation of one. Subgroups are based on Partnership Round 4 identification year (2021-22).

Each math estimate is descriptively positive, suggesting that students in Partnership schools performed slightly better in math than their peers in comparison schools. However, because the confidence intervals all intersect with the zero line, we can say with 95% confidence that Partnership did not affect math achievement in either the 2022-23 planning year or the 2023-24 implementation year.

We turn next to separate results for reidentified versus newly identified schools. The implementation year estimate for newly identified schools is positive and marginally significant ($p<.10$), though the overlapping confidence intervals across groups in Figure 3.2 mean that the newly identified

estimate is not significantly different from the reidentified estimate. Specifically, math score gains were about 0.09 standard deviations higher in newly identified Partnership schools than in comparison schools. This estimate is consistent within the narrower alternative bandwidth but attenuates in the bandwidth that extends further from the cutoff (see Appendix A). Together, these findings provide some limited evidence that Partnership may have improved math achievement in newly identified schools but not reidentified schools. We do not find significant effects in ELA in either year, and, in fact, our implementation year estimates are descriptively negative.

FIGURE 3.2. Regression Discontinuity Estimates of Student Achievement



Notes: Estimates are from regression discontinuity models predicting grade 4-8 standardized test scores. All models include lagged test score as a covariate, so these estimates can be interpreted as the difference in test score growth. Spikes are 95% confidence intervals. When these confidence intervals intersect with the horizontal zero line, the estimate is not statistically different from zero (i.e., not statistically significant at conventional levels). Estimates are from models using preferred bandwidth for student test scores in math in 2022-23 (3.45) and 2023-24 (3.42). These estimates are provided with standard errors in tabular form in Appendix A, along with estimates excluding lagged test score in the model and across a variety of other bandwidths.

In sum, student achievement increased in Partnership schools after Partnership designation, but at least some of that increase appears to stem from statistical regression to the mean and the regression discontinuity analyses do not point to statistically significant effects of Partnership overall. Our preferred model can rule out overall effects as small—about .06 standard deviations in math and .05 standard deviations in ELA—which would be classified as small effect sizes in studies of educational policies.

This finding is different from the results of the Partnership intervention that preceded the pandemic. The first Partnership cohort made large increases in student achievement in the first two years before the pandemic's onset, whereas the second cohort made smaller gains in its single pre-pandemic implementation year (Burns et al., 2023; Harbatkin et al., 2025; Strunk et al., 2020, 2021). In the sections that follow, we investigate two mechanisms that could contribute to meaningful school turnaround—student attendance/absenteeism and teacher turnover. However, we note here that turnaround may need to look different in a post-pandemic context.

The pandemic's effects were especially severe in communities served by low-performing schools like Partnership schools—with greater economic losses, health implications, and more COVID-19 deaths. In turn, Partnership schools experienced greater disruptions, including longer periods under fully remote instruction and more student absenteeism (Harbatkin et al., 2023a; Hatch & Harbatkin, 2021). Thus, Partnership schools may need more supports in this new context.

We do find some suggestive evidence of a moderately sized positive math effect in newly identified schools closest to the assignment threshold in the first full implementation year. Of particular importance, an regression discontinuity provides a *local* average treatment effect rather than a simple average treatment effect, which means the estimated effect is for those schools closest to the assignment threshold. Here, that means the Partnership schools with the highest baseline achievement levels. In other words, we find a potential positive effect for schools that are higher performing by two measures: those that were not previously designated and those that had higher school performance indexes at the time of designation. This points to a possibility that the Partnership Model may be sufficient to effect meaningful improvement in some schools, but not in the most persistently low-performing schools.

04

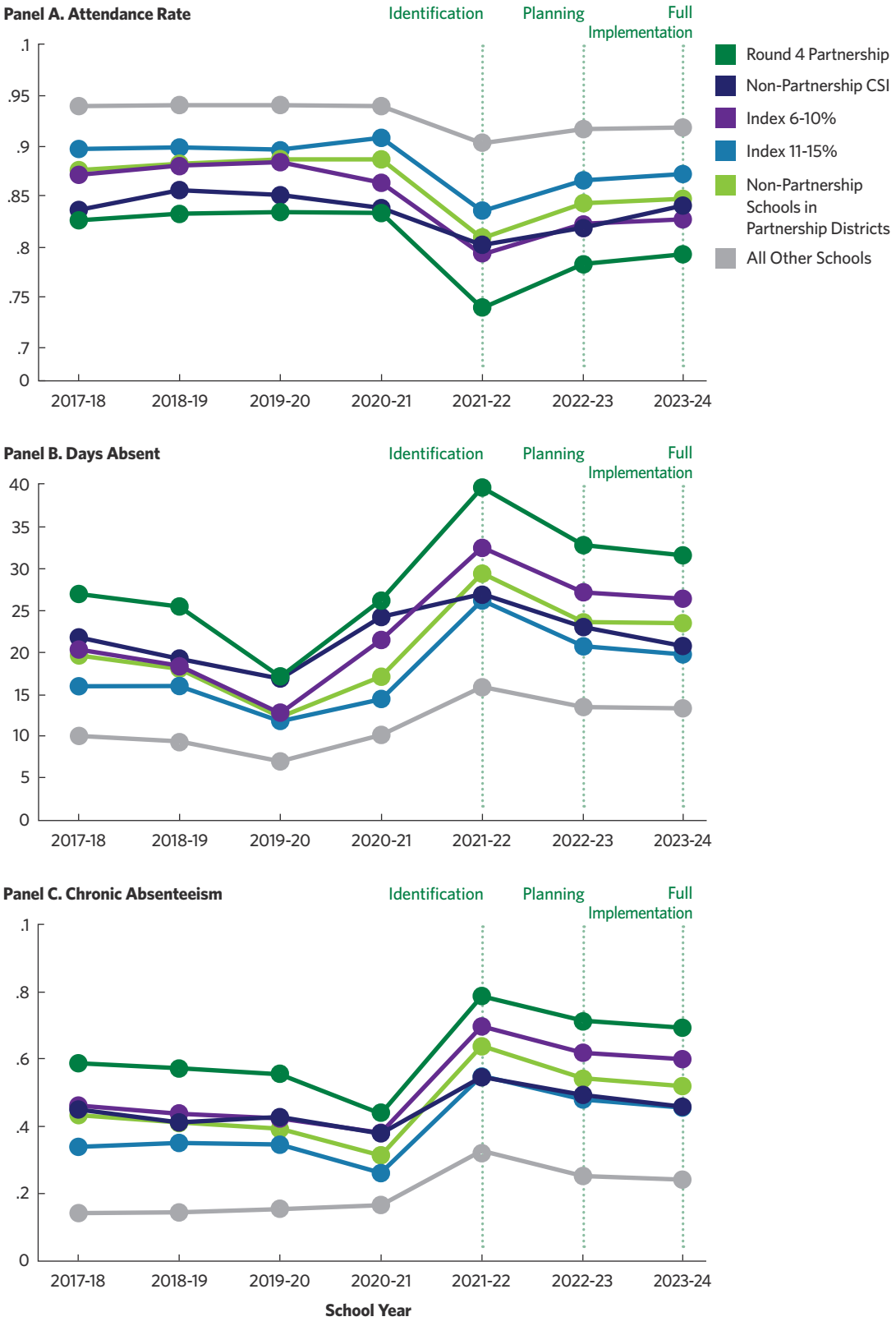
Section Four: Student Attendance and Absenteeism

Figure 4.1 shows average attendance rate (Panel A), total days absent (Panel B), and chronic absenteeism (Panel C) over time for students in Partnership and five other subgroups of schools. In line with schools nationwide (Dee, 2024), Michigan school attendance plunged and absences and chronic absenteeism surged during the 2021-22 school year.[1] This occurred in all school subgroups—but Partnership schools were already starting from the lowest attendance and highest absenteeism rates of any group. In particular, Partnership schools dropped to below 75% attendance, with students missing an average of about 40 days and 8 in 10 students counting as chronically absent. This amounts to approximately a 40% increase in chronic absenteeism from the last pre-pandemic school year in Partnership schools.

In the past two years, attendance rebounded and absenteeism declined in Partnership schools throughout the state—but did not return to pre-pandemic levels. By the 2023-24 school year, students in Partnership schools were attending just below 80% of enrolled days and were absent for an average of 31 days. While students with a large number of absences could inflate the average, the median student in Partnership schools was absent for 25 days in 2023-24, compared with 20 days in 6th- to 10th-percentile schools, and 17 days in non-Partnership schools in Partnership districts. By comparison, the median number of absences in the "all other schools" school group was nine. Seventy percent of students in Partnership schools were chronically absent.

Although attendance in Partnership schools increased and absenteeism decreased during Partnership, the largely parallel trajectories of schools in the 6th to 10th percentiles suggest these improvements may not be the result of Partnership but rather a partial recovery across lower-performing schools. Our regression discontinuity results shed light on these descriptive findings by comparing attendance and absenteeism in schools just around the Partnership threshold.

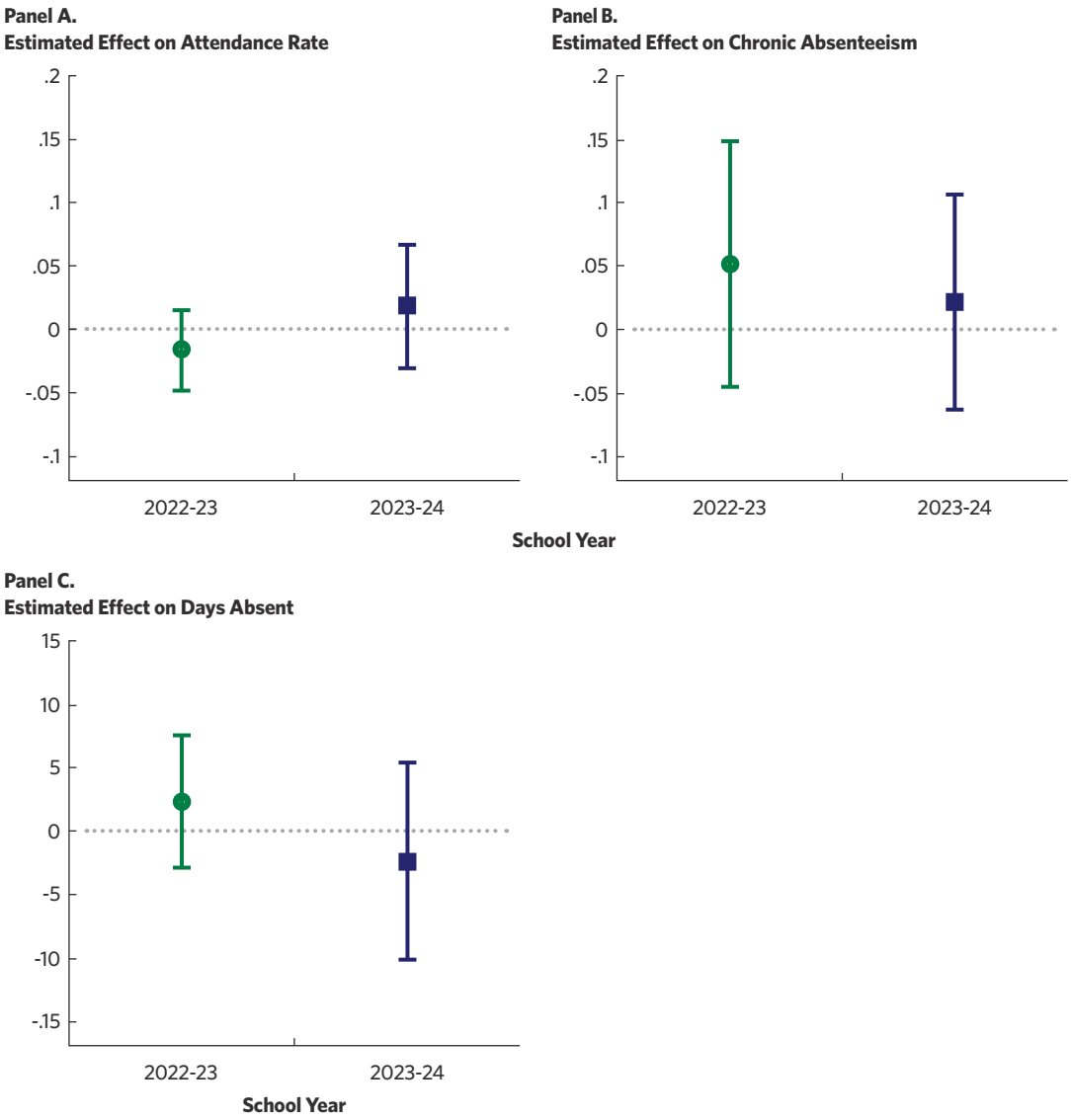
FIGURE 4.1. Student Attendance, Days Absent, and Chronic Absenteeism Over Time



Notes: Markers show average attendance rate (Panel A), days absent (Panel B), and chronic absenteeism (Panel C). Subgroups are based on Partnership Round 4 identification year (2021-22).

Figure 4.2 displays the regression discontinuity results, showing no effects of Partnership on attendance, days absent, or chronic absenteeism. Estimates are very close to zero in both years for all three outcomes. It is worth noting that there are wide confidence intervals for chronic absenteeism and days absent in particular, which means our models are not well powered to detect small effects. For example, while our point estimates suggest that Partnership is associated with a decrease of 2.7 absences in 2023-24, our preferred model can detect effects as small as 4.2 absences. However, we note that we do not detect statistically significant effects within the larger bandwidths we use either. We also do not find meaningful differences between newly identified and reidentified schools. These results, along with those using alternative bandwidths, are provided in Appendix A.

FIGURE 4.2. Regression Discontinuity Estimates of Student Attendance and Absenteeism Outcomes, All Grades



Notes: Estimates are from regression discontinuity models predicting attendance rate (Panel A), chronic absenteeism (Panel B), and days absent (Panel C). Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator

that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in Panels A and B can be interpreted in percentage point differences, whereas results from Panel C can be interpreted as number of days. Spikes are 95% confidence intervals. When these confidence intervals intersect with the horizontal zero line, the estimate is not statistically different from zero (i.e., not statistically significant at conventional levels). Estimates are from models using preferred bandwidth for each outcome in 2022-23 (2.51 for attendance, 4.08 for chronic absenteeism, and 2.63 for days absent) and 2023-24 (4.39 for attendance, 2.72 for chronic absenteeism, and 4.81 for days absent). These estimates are provided with standard errors in tabular form in Appendix A, along with estimates within a variety of other bandwidths.

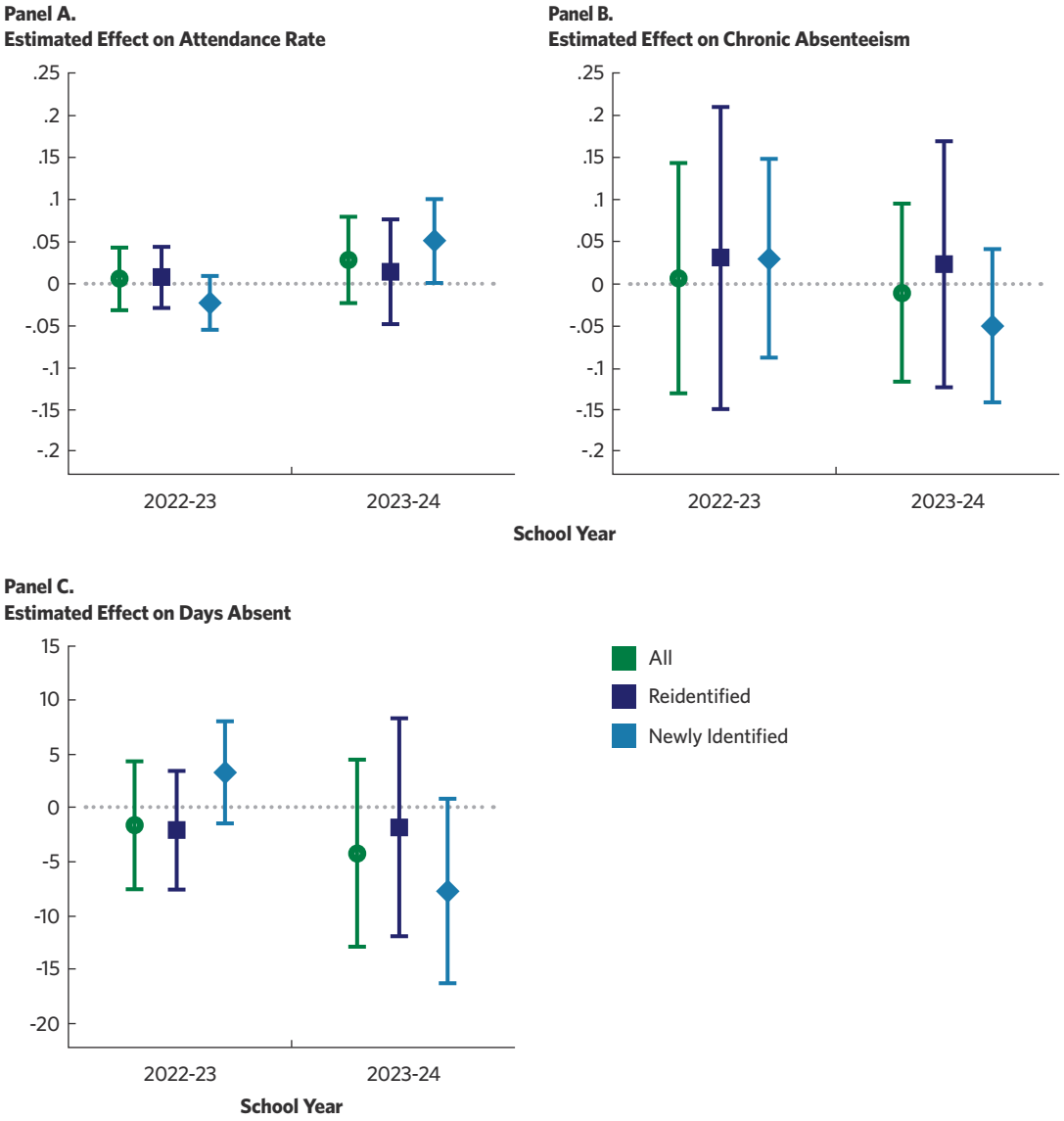
We run these models separately for grades 4-8 to make connections between the student achievement results reported above and some of the potential contributing mechanisms. Here, we find a descriptive increase in attendance and decrease in absenteeism, driven by improvements in newly identified schools. Figure 5.4 shows these results overall and then separately for reidentified and newly identified schools to highlight differences. In grades 4–8 in newly identified schools in the implementation year, attendance increased (Panel A), absences decreased (Panel C), and chronic absenteeism decreased (Panel B), though none of these estimates are statistically significant at conventional levels.

The estimates for attendance rate and days absent are marginally statistically significant, providing suggestive evidence that attendance increased by about 5 percentage points and absences decreased by just under eight days. To place these estimates in context, the attendance rate among 4th- to 8th-graders in newly identified schools was 82% the year they were identified for Partnership, so this finding represents a 6% increase in attendance. The decline in days absent represents a 25% change from the baseline year. We do not observe significant effects in reidentified schools.

These results resonate with evidence from Partnership district plans, case studies, and survey data. Partnership districts planned to ramp up the implementation of attendance strategies over time, mostly focusing on planning and development of new systems and strategies in 2022-23, and then implementing those in 2023-24 (Cullum et al., 2024), which may help to explain pockets of improvements beginning to emerge in 2023-24. Survey data, meanwhile, showed a substantial increase from 2022-23 to 2023-24 in the number of Partnership school principals identifying student attendance as a top focus, though the specific strategies that schools used did not differ significantly from one year to the next (Singer et al., 2024).

A further analysis of our survey data provides suggestive evidence that the focus on attendance may have been greater in newly identified schools. Though principal responses about their focus on attendance did not differ between reidentified and newly identified schools, we did find substantial differences in teacher responses on the same question. Specifically, the share of teachers in reidentified schools reporting attendance as a top focus did not change meaningfully from 2022-23 (47%) to 2023-24 (50%). Yet, in newly identified schools, the share of teachers reporting attendance as a top focus did increase substantially and statistically significantly, from 36% in 2022-23 to 52% in 2023-24. The increase in focus on attendance among newly identified schools was even greater for elementary and middle schools (20pp increase) than for high schools (14pp increase). These results may reflect a stronger translation of attendance plans into action in newly identified schools, especially in elementary and middle schools, relative to reidentified schools.

FIGURE 4.3. Regression Discontinuity Estimates of Student Attendance and Absenteeism Outcomes, Grades 4-8 Only, Overall and by Reidentified vs. Newly Identified



Notes: Estimates are from regression discontinuity models predicting attendance rate (Panel A), chronic absenteeism (Panel B), and days absent (Panel C) in grades 4–8 only (i.e., the student achievement sample). Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in Panels A and B can be interpreted in percentage point differences, whereas results from Panel C can be interpreted as number of days. Spikes are 95% confidence intervals. When these confidence intervals intersect with the horizontal zero line, the estimate is not statistically different from zero (i.e., not statistically significant at conventional levels). Estimates are from models using preferred bandwidth for each outcome in 2022-23 (2.62 for attendance, 4.24 for chronic absenteeism, and 2.61 for days absent) and 2023-24 (4.93 for attendance, 5.08 for chronic absenteeism, and 5.34 for days absent). These estimates are provided with standard errors in tabular form in Appendix A, along with estimates within a variety of other bandwidths.

In sum, Partnership did not affect attendance or absenteeism on average, though we observe potential improvements in grades 4–8 in newly identified Partnership schools. These improvements could have contributed to math gains in newly identified schools, though Section Three shows they did not translate into ELA gains. On the other hand, continued low attendance and high absenteeism in reidentified schools may help to explain the lack of Partnership effects on student achievement.

SECTION FOUR NOTES

1. While attendance appeared to increase and absenteeism decrease during 2020-21 school year, this is likely driven by differences in how absences were recorded during pandemic schooling. Partnership schools in particular are located in districts that spent more time under remote instruction, which would have led to artificially high attendance in the case of low thresholds for counting a student as present.

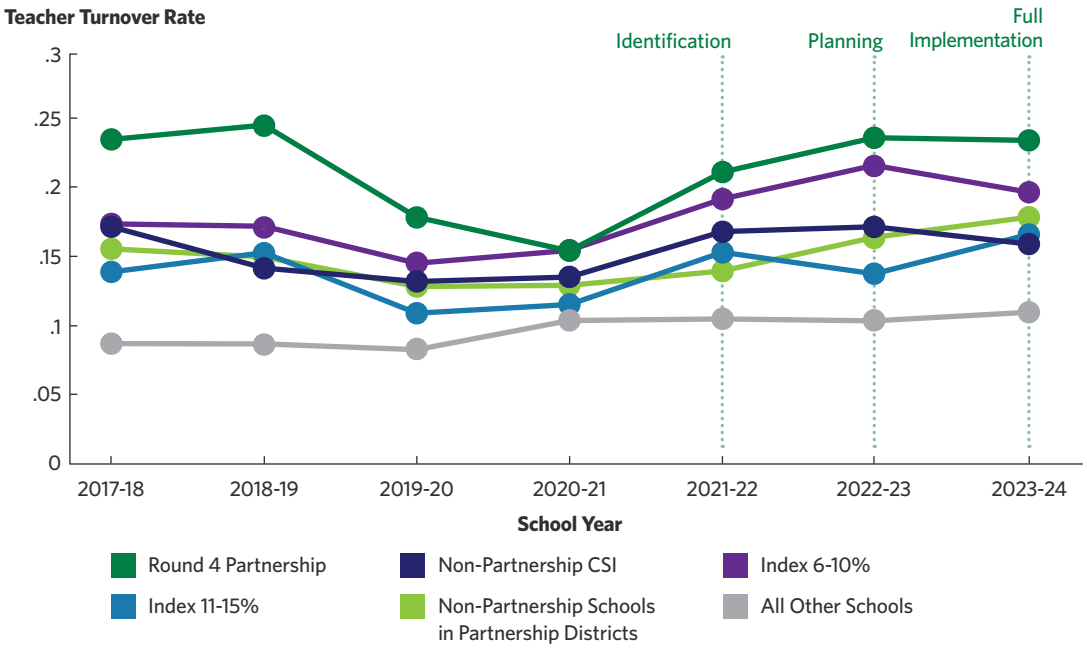
05

Section Five: Teacher Turnover

A stable and effective teacher workforce is central to school turnaround (Harbatkin et al., 2024; Henry et al., 2020; Malen & Rice, 2016). High rates of turnover among teachers in Partnership schools—especially in the planning year—could disrupt improvement efforts in the implementation year. On the other hand, teachers might transfer schools in pursuit of a better fit where they can be more effective (Dhaliwal et al., 2023), or districts might reshuffle teachers as part of their turnaround efforts (Kho et al., 2022; Pham, 2022; Schueler et al., 2021; Strunk et al., 2016). Here, we might expect an increase in teacher transfers in the planning year followed by a stabilization in the implementation year. These efforts might precipitate "growing pains," which could explain the null effects in 2023-24. In this section, we test those theories through an examination of teacher turnover in the planning and implementation years.

Figure 5.1 shows teacher turnover over time by school subgroup. Across all years, Partnership schools had the highest turnover rates, ranging from a low of about 15% to a high of 25%. By comparison, the "all other schools" group had turnover ranging from 8% to about 10%. Similar to the earlier rounds of Partnership schools that preceded them (Harbatkin et al., 2023b), Round 4 Partnership schools experienced an especially pronounced pandemic-era dip in teacher turnover relative to higher-performing schools and took longer to fully climb to pre-pandemic turnover rates. Still, by the end of 2022-23, Round 4 Partnership schools were losing nearly 1 in 4 teachers—similar to before the pandemic's onset.

FIGURE 5.1. Teacher Turnover Over Time, Any Pathway Out



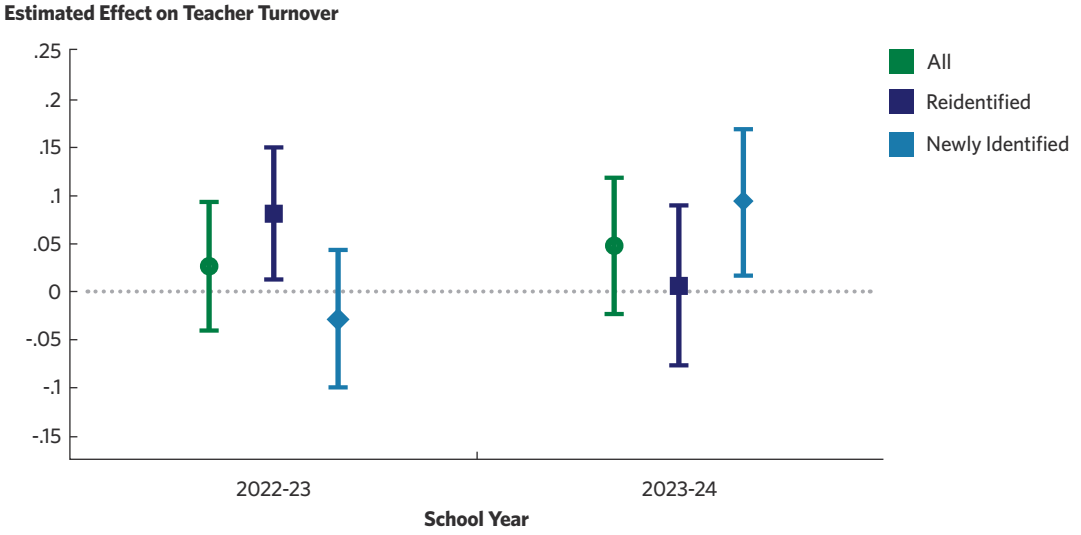
Notes: Markers show rate of teacher turnover for any pathway out. Subgroups are based on Partnership Round 4 identification year (2021-22).

Of particular note, the post-pandemic climb receded in 2023-24 as Partnership was being fully implemented. This could suggest a Partnership effect—or a natural stabilization. Our regression discontinuity results, shown in Figure 5.2, shed light on this descriptive trend. Because turnover is a dichotomous outcome, these can be interpreted in terms of change in probability; for example, an estimate of 0.15 would mean a 0.15 (or 15 percentage point) increase in the probability of turnover, whereas an estimate of –0.15 would represent a 0.15 (or 15 percentage point) decrease in the probability of turnover.

Beginning with estimates for all Partnership schools (green circular markers), we find that Partnership school teachers were descriptively more likely to turn over than their comparison school counterparts in both years, though neither estimate was statistically significant by conventional standards.

The blue square and teal diamond markers show that the overall effects mask heterogeneity. Specifically, turnover increased by about 8 percentage points in the planning year in reidentified schools before restabilizing—but did not increase until the first implementation year (by about 9 percentage points) in newly identified schools. In other words, teachers who were already in Partnership schools chose to exit when they found out their schools were reidentified, whereas teachers who were new to Partnership may have delayed their exits until they better understood what the designation meant. These estimates were consistent across alternative bandwidths in magnitude and statistical significance (Appendix B), providing strong evidence for their validity.

FIGURE 5.2. Regression Discontinuity Estimates of Teacher Turnover for Any Pathway Out

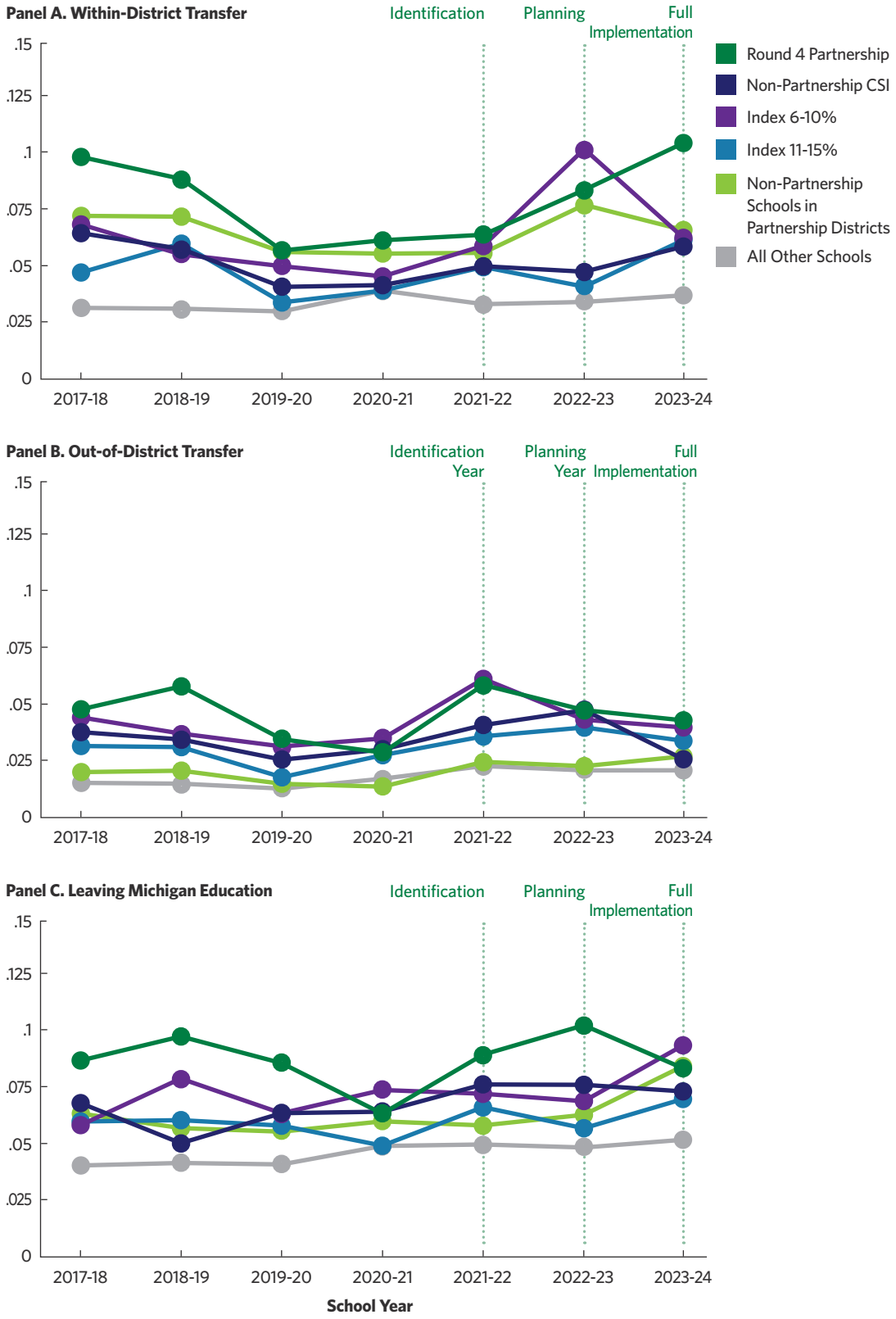


Notes: Estimates are from regression discontinuity models predicting teacher turnover for any pathway out. Because turnover is a dichotomous outcome, estimates can be interpreted in terms of difference in probability of turnover between Partnership and comparison schools. Spikes are 95% confidence intervals. When these confidence intervals intersect with the horizontal zero line, the estimate is not statistically different from zero (i.e., not statistically significant at conventional levels). Estimates are from models using preferred bandwidths. These estimates are provided with standard errors in tabular form in Appendix B, along with estimates within a variety of other bandwidths.

We turn next to different types of turnover. While unplanned turnover undercuts school improvement for the schools that teachers are leaving, different types of turnover may have different implications for the broader teacher workforce. We therefore break the above turnover measure into three mutually exclusive types of turnover—within-district transfer, out-of-district transfer, and leaving Michigan public schools entirely.

Figure 5.3 displays these three outcomes descriptively over time by subgroup. Panel A shows that within-district transfer from Partnership schools climbed steadily in Partnership schools from 2021-22 through 2023-24, even as overall turnover stabilized. Out-of-district transfer (Panel B) and leaving Michigan public schools (Panel C), on the other hand, decreased. In particular, Partnership schools experienced a monotonic decline in out-of-district transfer since Partnership designation, from 5.8% in 2021-22 to 4.9% in 2022-23, and finally 4.3% in 2023-24. Leaving Michigan public schools decreased in 2023-24 after reaching a high of 10% in the prior year for Partnership schools. In other words, teachers continued shuffling around Partnership districts even after the first implementation year, but attrition away from Partnership districts and the profession eased. To the extent that this latter trajectory continues, Partnership may be able to build human capital within Partnership districts and Michigan public schools more broadly. However, the possibility of continued rising within-district transfers is less sanguine, as teacher turnover has a disruptive effect on school processes and improvement.

FIGURE 5.3. Teacher Mobility and Attrition Over Time

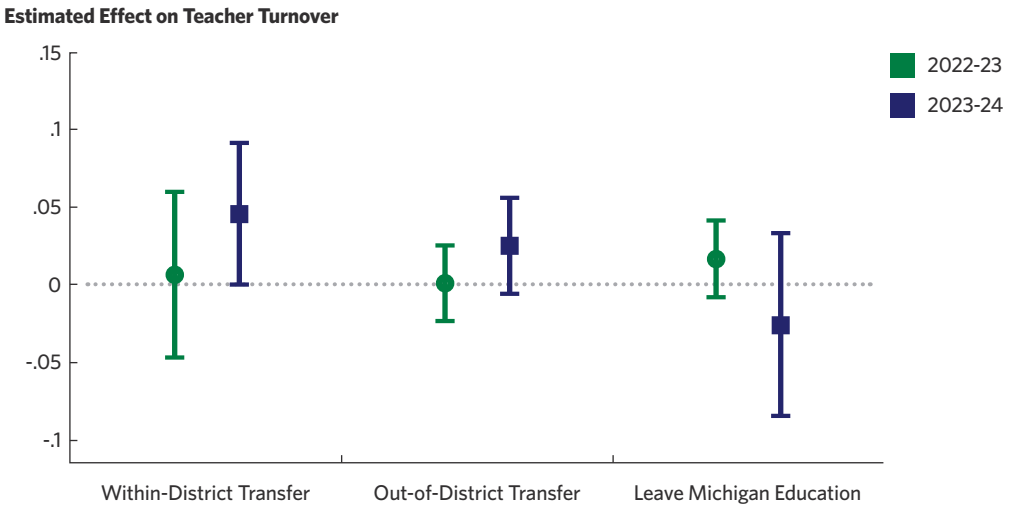


Notes: Markers show rate of each type of teacher turnover. Subgroups are based on Partnership Round 4 identification year (2021-22).

Our regression discontinuity estimates provide insight into whether any changes stem from the Partnership Model. Here, we focus only on average estimates for all Partnership schools because we do not observe meaningful differences between reidentified and newly identified schools on these more granular measures. The green circular markers in Figure 5.4 are all close to zero and intersect with the zero line, suggesting no significant differences between Partnership and comparison schools in the planning year. Then, in the full implementation year, we find a statistically significant increase in within-district transfers. Specifically, these results suggest that Partnership led to a nearly 5 percentage point increase in within-district transfers in the implementation year. Using alternative bandwidths, the estimates range from 5 to 7 percentage points and remain statistically significant. This reflects a 78% increase over the identification year within-district transfer rate of 6.4%. We do not observe significant effects on between-district transfer or leaving Michigan public schools.

These patterns could reflect a positive trend for Partnership schools despite the overall descriptive increase in teacher turnover if Partnership district leaders were working to intentionally reassign teachers to improve fit with school assignments. However, neither our survey nor interview data provide evidence of this sort of strategic teacher assignment. In particular, neither districts nor school leaders in case study interviews described plans for intentional teacher transfers. Additionally, based on prior turnaround research (Harbatkin et al., 2024; Zimmer et al., 2017), we would expect intentional reassignment to occur after the planning year rather than after the first implementation year. Instead, within-district transfer is close to zero in the planning year and then jumps in the implementation year.

FIGURE 5.4. Regression Discontinuity Estimates of Teacher Mobility and Attrition



Notes: Estimates are from regression discontinuity models predicting each of three types of teacher turnover. Because turnover is a dichotomous outcome, estimates can be interpreted in terms of difference in probability of turnover between Partnership and comparison schools. Spikes are 95% confidence intervals. When these confidence intervals intersect with the horizontal zero line, the estimate is not statistically different from zero (i.e., not statistically significant at conventional levels). Estimates are from models using preferred bandwidth for each outcome (6.00 for all 2022-23 models and 3.71 for all 2023-24 models). These estimates are provided with standard errors in tabular form in Appendix B, along with estimates within a variety of other bandwidths.

Finally, though not shown here, the increase in overall turnover in newly identified schools in 2023-24 is driven in part by increased transfers *out of* district—not just within district. In particular, out-of-district transfers increased by 5 percentage points in newly identified schools in the implementation year.

In one bright spot, although the estimate on leaving Michigan public schools is not statistically significant, it is negative in the implementation year. This shows that, descriptively at least, Partnership school teachers were about 2.5 percentage points less likely to leave the teaching profession than comparison school teachers in 2023-24 and the estimates are consistent across alternative bandwidths. It is possible again that there was a small decrease that our analysis does not have sufficient power to detect. Though not shown here, models restricted to only elementary and middle schools show a 7 percentage point decrease in leaving Michigan public schools that is marginally statistically significant at the preferred bandwidth ($p<.10$) and significant at conventional levels in larger bandwidths.

In sum, we find that overall teacher turnover increased in reidentified schools in the planning year and in newly identified schools in the first full implementation year. The implementation year upturn was driven by a meaningful and statistically significant increase in within-district transfers. While it is possible that these within-district transfers reflected intentional movement or reassignment, our data suggest that is unlikely. On the other hand, fewer Partnership elementary and middle school teachers left the profession than those from comparison schools—suggesting that Partnership may have improved teachers' view of the profession.

06

Section Six: Recommendations

Recommendation 1: Post-pandemic turnaround schools may need more supports.

In contrast to the pre-pandemic Partnership schools, this most recent round of Partnership schools did not make statistically significant gains in student achievement or teacher retention (although we do observe pockets of positive effects, specifically in math achievement and student absenteeism/attendance in newly identified schools). While there are a variety of possible explanations for the inconsistent effects in this round, one is that turnaround schools need more resources and support than they are receiving to make meaningful improvements. Amplified needs may stem in part from pandemic-era learning disruptions (Harbatkin et. al., 2023a; Hatch & Harbatkin, 2021), may simply reflect growing achievement gaps between the lowest performing students, schools, and their peers that have been documented nationwide (Aldeman, 2025; Wyckoff, 2025), or may stem in part from both. The upshot, either way, is that Partnership districts may need more resources and supports to make up for opportunity gaps that translate into growing achievement gaps.

Recommendation 2: Communicate what Partnership is (and what it isn't) to teachers.

Survey data from both pre- and post-pandemic Partnership cohorts show that teacher knowledge of and buy-in to Partnership and school improvement goals begins relatively low and then grows throughout the life of the intervention (Singer et al., 2024; Strunk et al., 2021, 2022). This could help to explain why we observe an immediate spike in teacher turnover among reidentified schools and a lagged increase in newly identified schools. A large body of educational research finds that schools can benefit from intentional teacher turnover, i.e., exits among teachers with lower effectiveness or who do not buy into a turnaround effort (e.g., Dee & Wyckoff, 2015; Grissom et al., 2013). To that end, we might consider an initial spike in teacher turnover to be a positive sign if the teachers coming in have higher buy-in for the reform and are at least as effective as the teachers they are replacing. However, our interview and survey data do not suggest that the turnover that occurred was strategic or intentional. To build teacher buy-in and reduce damaging teacher turnover, district and school leaders can make intentional early efforts to inform teachers about Partnership and engage them in the improvement process. Without this information, teachers may make their own assumptions and choose to depart based on incomplete or inaccurate information. Communication should clarify on what Partnership is in their school, what it means for teachers, and, importantly, what it does not mean for teachers.

Recommendation 3: Consider differential supports for reidentified vs. newly identified schools.

The Office of Partnership Districts (OPD) already has in place three tiers of supports for Partnership districts, in which districts with at least one reidentified school receive the most intensive supports. This type of tiered system is employed in other states as well and is grounded in research underscoring that different schools and districts have different needs. However, continued challenges in newly identified schools suggest that these schools may have unique needs that are not fully met by Partnership—even at its Intensive Level Support Tier. Ideally, turnaround interventions will build and reinforce the educational infrastructure so that schools and districts no longer need supplemental turnaround supports. Reidentified schools may need immersive intervention intended to create and sustain a culture of improvement. Evidence suggests one useful strategy toward this end is to build leadership teams through distributed leadership.

Recommendation 4: Implement evidence-based strategies for improving attendance and reducing chronic absenteeism.

Students cannot learn even from the most effective teacher if they are not in the classroom with that teacher. Partnership district and school leaders may benefit from explicit guidance and support related to attendance. Through 2023–24, schools were focused on building new organizational infrastructure—like attendance teams, data systems, and multi-tiered systems of support frameworks—and hiring attendance staff or family liaisons (Singer, 2025). It is possible that these schools have made progress with implementation in 2024-25, and that such progress will translate into improved in attendance rates. Partnership schools focused heavily on student and family communication strategies; attendance incentives; and some efforts address barriers to attendance, for example through clean clothing, transportation, or social services (Singer, 2025). As Partnership districts continue their attendance improvement efforts, they should streamline their efforts and prioritize effective practices: monitoring the efficacy of new systems, avoiding overreliance on low-impact strategies, using communication judiciously, linking attendance initiatives to broader improvements in school climate and instruction, and (when possible) directly addressing student-specific barriers (Singer & Lenhoff, 2025). OPD can draw upon the Michigan Department of Education's forthcoming guidance on school attendance strategies to support districts. Finally, it is important to note the need for greater external resources and cross-sector collaboration, given the sheer magnitude of chronic absenteeism that these schools face (Lenhoff & Singer, 2025). OPD can make efforts to bring together other regional and state agencies to jointly address the root causes of chronic absenteeism in Partnership schools and districts. This might involve partnerships related to healthcare, transportation, housing, employment, and poverty alleviation.

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APPENDIX A. REGRESSION DISCONTINUITY RESULTS, STUDENT OUTCOMES

TABLE A1. Estimated effects on student achievement, 2022-23						
	100% BW (+/- 2.24)		150% BW (+/- 3.36)		200% BW (+/- 4.48)	
	No lag	Lag	No lag	Lag	No lag	Lag
Std. Math	0.085+ (0.047)	0.022 (0.028)	0.063 (0.045)	0.016 (0.026)	0.011 (0.046)	-0.009 (0.026)
Std. ELA	0.111* (0.054)	0.052 (0.043)	0.071 (0.054)	0.033 (0.038)	0.026 (0.053)	0.010 (0.035)
Student N [School N]	436,277 [2107]		436,277 [2107]		436,277 [2107]	
N Bandwidth [School N]	8,217 [56]		10,762 [76]		13,453 [95]	
Treated in BW [School N]	3,771 [28]		4,817 [35]		5,484 [41]	
Comparison in BW [School N]	4,446 [28]		5,945 [41]		7,969 [54]	

*Note: Estimates from regression discontinuity models predicting grades 4-8 math (row 1) and ELA (row 2) achievement, respectively. Models exclude non-Partnership CSI schools. Math and ELA are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The optimal bandwidth was calculated using the math M-STEP outcome from the 2022-23 school year. * p<0.05, ** p<0.01, *** p<.001*

TABLE A2. Estimated effects on student achievement, 2023-24						
	100% BW (+/- 2.59)		150% BW (+/- 3.89)		200% BW (+/- 5.18)	
	No lag	Lag	No lag	Lag	No lag	Lag
Std. Math	0.152** (0.053)	0.044 (0.036)	0.091+ (0.054)	0.025 (0.036)	0.006 (0.059)	-0.007 (0.035)
Std. ELA	0.075 (0.048)	-0.036 (0.037)	0.048 (0.050)	-0.026 (0.037)	-0.011 (0.052)	-0.041 (0.035)
Student N [School N]	441,839 [2113]		441,839 [2113]		441,839 [2113]	
N Bandwidth [School N]	9,250 [64]		12,861 [89]		15,546 [110]	
Treated in BW [School N]	4,227 [30]		5,572 [41]		6,244 [48]	
Comparison in BW [School N]	5,023 [34]		7,289 [48]		9,302 [62]	

*Note: Estimates from regression discontinuity models predicting grades 4-8 math (row 1) and ELA (row 2) achievement, respectively. Models exclude non-Partnership CSI schools. Math and ELA are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The optimal bandwidth was calculated using the math M-STEP outcome from the 2023-24 school year. * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

TABLE A3. Estimated effects on student achievement by school designation group

	2022-23				2023-24			
	Reidentified		Newly identified		Reidentified		Newly identified	
	Lag	*No Lag*	*No Lag*	*Lag*	*No Lag*	*Lag*	*No Lag*	*Lag*
Std. Math	0.071+ (0.039)	0.035 (0.029)	0.152 (0.093)	0.018 (0.026)	0.118** (0.041)	0.016 (0.035)	0.225** (0.082)	0.088+ (0.046)
Std. ELA	0.067 (0.046)	0.043 (0.044)	0.207* (0.094)	0.062 (0.043)	0.036 (0.034)	-0.059+ (0.034)	0.124+ (0.073)	-0.014 (0.055)
Student N [School N]	431,815 [2066]		431,722 [2076]		437,251 [2071]		437,183 [2082]	
N Bandwidth [School N]	6,757 [39]		5,906 [44]		7,331 [45]		6,942 [52]	
Treated in BW [School N]	2,311 [11]		1,460 [17]		2,308 [11]		1,919 [19]	
Comparison in BW [School N]	4,446 [28]		4,446 [27]		5,023 [34]		5,023 [33]	

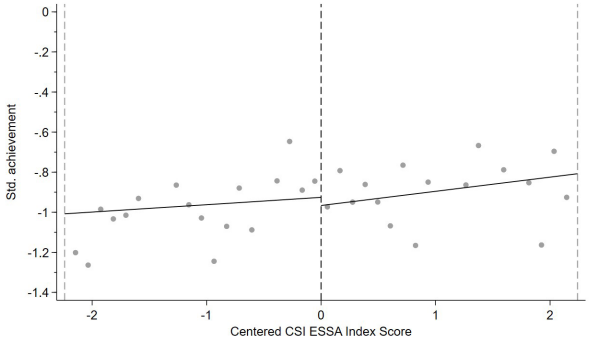
*Notes: Estimates from regression discontinuity models predicting grades 4-8 math (row 1) and ELA (row 2) achievement, respectively. All models exclude non-Partnership CSI schools. The reidentified models exclude Partnership schools that were newly identified in Cohort 3. The newly identified models exclude Partnership schools that were previously identified for Partnership in Cohort 1 or 2. Math and ELA are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The optimal bandwidths were calculated using the math M-STEP outcome from the 2022-23 school year for the 2022-23 estimates (+/- 2.24) and the 2023-24 school year for the 2023-24 estimates (+/- 2.59). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

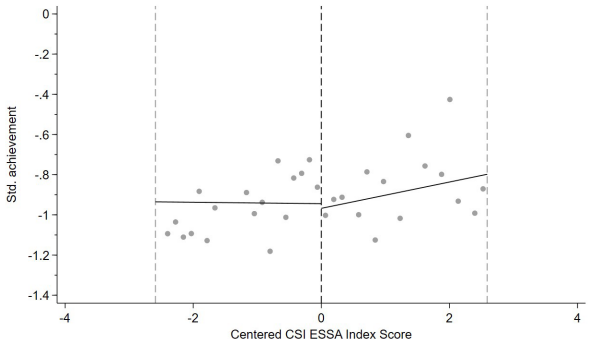
FIGURE A1. Estimated effects on student achievement by school identification group

Panel A. Math

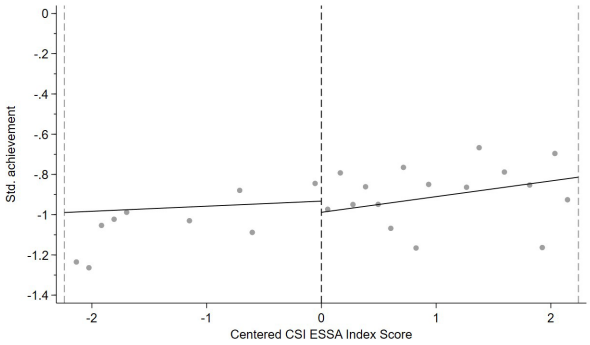
A1. Overall, 2022-23



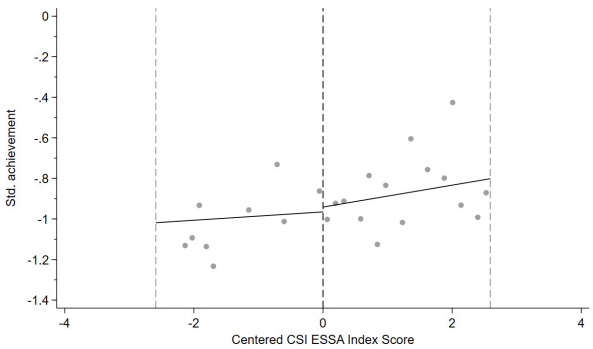
A2. Overall, 2023-24



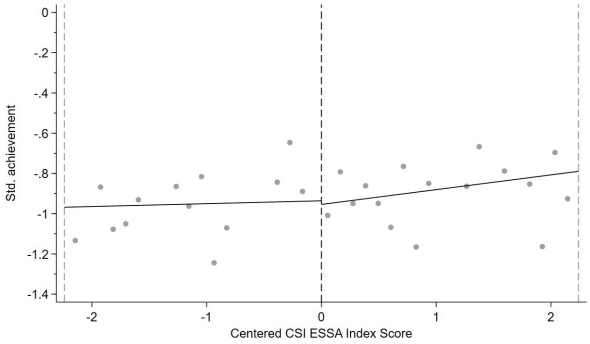
A3. Reidentified, 2022-23



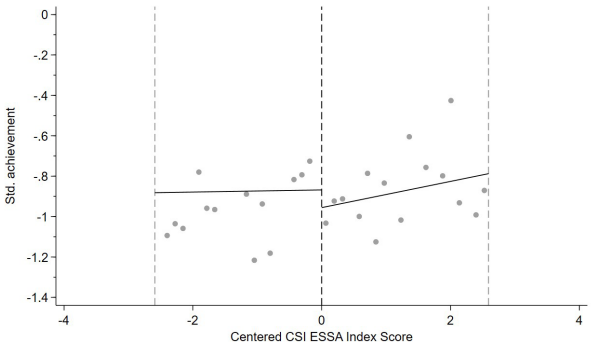
A4. Reidentified, 2023-24



A5. Newly Identified, 2022-23



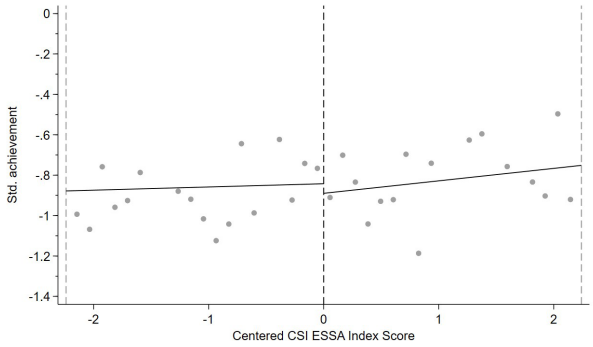
A6. Newly Identified, 2023-24



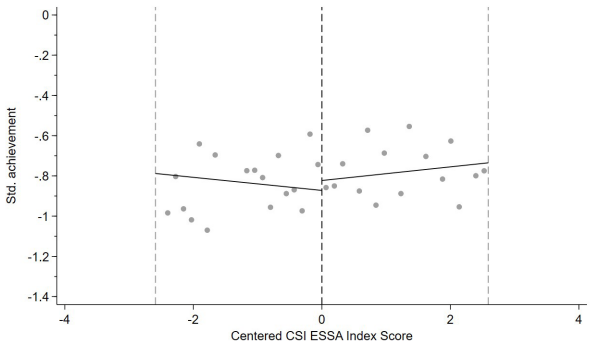
APPENDIX A. *(continued)*

Panel B. ELA

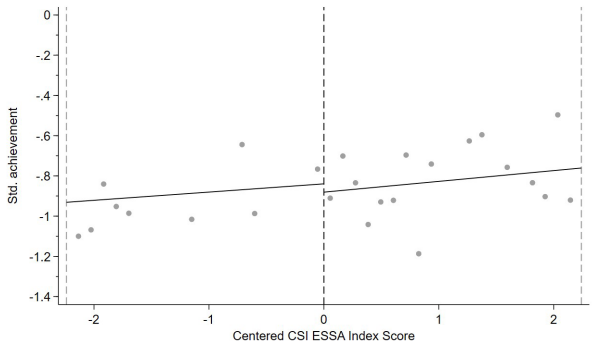
B1. Overall, 2022-23



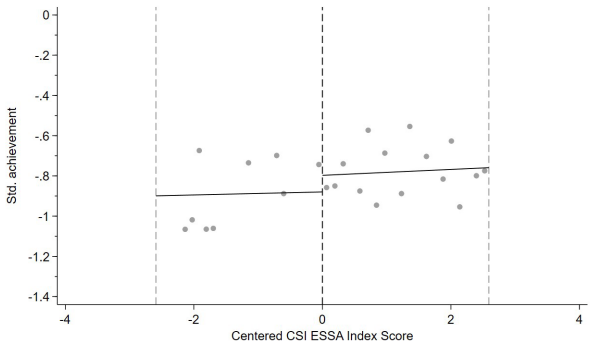
B2. Overall, 2023-24



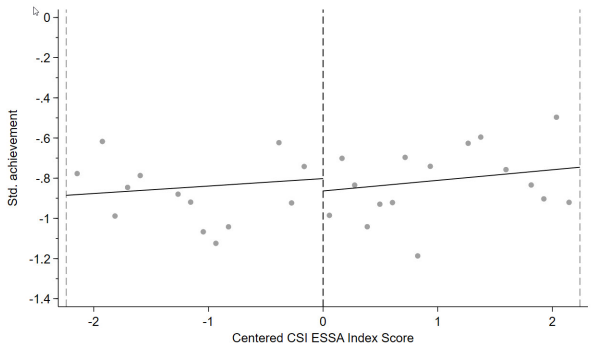
B3. Reidentified, 2022-23



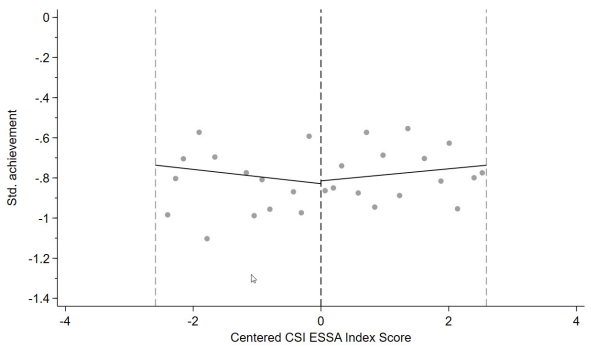
B4. Reidentified, 2023-24



B5. Newly Identified, 2022-23



B6. Newly Identified, 2023-24



*Notes: Estimates from regression discontinuity models predicting grades 4-8 math (Panel A) and ELA (Panel B) achievement, respectively. All models exclude non-Partnership CSI schools. The overall sample includes all Partnership schools (row 1 of each panel). The reidentified models exclude Partnership schools that were newly identified in Cohort 3 (row 2). The newly identified models exclude Partnership schools that were previously identified for Partnership in Cohort 1 or 2 (row 3). Math and ELA are estimated in separate models, but we show them in a single figure for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The optimal bandwidths were calculated using the math M-STEP outcome from the 2022-23 school year for the 2022-23 estimates (+/- 2.24) and the 2023-24 school year for the 2023-24 estimates (+/- 2.59). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

TABLE A4. Estimated effects on student attendance and absenteeism outcomes, all grades, 2022-23			
	100% BW	150% BW	200% BW
Attendance rate	-0.010 (0.019)	-0.020 (0.018)	-0.015 (0.018)
Chronic absenteeism	0.053 (0.056)	0.052 (0.050)	0.052 (0.044)
Days absent	1.695 (3.048)	2.866 (2.870)	2.243 (2.798)
Student N [School N]	636,670 [2590]	636,670 [2590]	636,670 [2590]

*Notes: Estimates from regression discontinuity models predicting attendance rate (row 1), chronic absenteeism (row 2), and days absent (row 3), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in rows 1 and 2 can be interpreted in percentage point differences, whereas results from row 3 can be interpreted as number of days. Models exclude non-Partnership CSI schools. All three attendance outcomes are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2022-23 school year (2.51 for attendance rate, 4.08 for chronic absenteeism, and 2.63 for days absent). * p<0.05, ** p<0.01, *** p<.001*

TABLE A5. Estimated effects on student attendance and absenteeism outcomes, all grades, 2023-24			
	100% BW	150% BW	200% BW
Attendance rate	0.020 (0.026)	0.016 (0.024)	0.014 (0.022)
Chronic absenteeism	0.002 (0.051)	0.019 (0.046)	0.024 (0.043)
Days absent	-2.816 (4.183)	-1.886 (3.830)	-1.853 (3.423)
Student N [School N]	639,063 [2587]	639,063 [2587]	639,063 [2587]

*Notes: Estimates from regression discontinuity models predicting attendance rate (row 1), chronic absenteeism (row 2), and days absent (row 3), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in rows 1 and 2 can be interpreted in percentage point differences, whereas results from row 3 can be interpreted as number of days. Models exclude non-Partnership CSI schools. All three attendance outcomes are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2023-24 school year (4.39 for attendance rate, 2.72 for chronic absenteeism, and 4.81 for days absent). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

TABLE A6. Estimated effects on student attendance and absenteeism by school designation group, all grades

	2022-23		2023-24	
	Reidentified	**Newly identified**	**Reidentified**	**Newly identified**
Attendance rate	-0.003 (0.017)	-0.028+ (0.014)	0.013 (0.034)	0.019 (0.027)
Chronic absenteeism	0.057 (0.070)	0.070 (0.044)	0.004 (0.074)	0.015 (0.045)
Days absent	-0.274 (2.700)	4.817* (2.195)	-1.880 (5.420)	-2.704 (4.428)
Student N [School N]	628,923 [2534]	629,055 [2544]	631,152 [2531]	631,697 [2541]

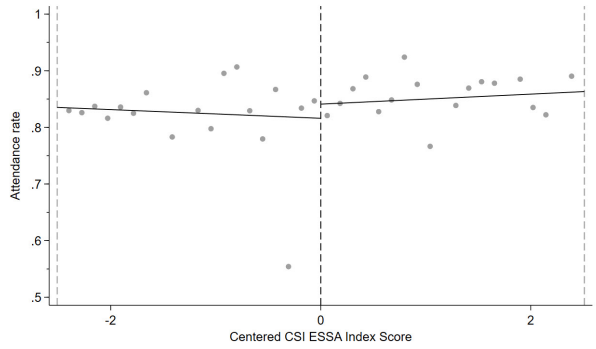
*Notes: Estimates from regression discontinuity models predicting attendance rate (row 1), chronic absenteeism (row 2), and days absent (row 3), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in rows 1 and 2 can be interpreted in percentage point differences, whereas results from row 3 can be interpreted as number of days. Models exclude non-Partnership CSI schools. The reidentified models exclude Partnership schools that were newly identified in Cohort 3. The newly identified models exclude Partnership schools that were previously identified for Partnership in Cohort 1 or 2. All three attendance outcomes are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2022-23 school year for the 2022-23 estimates (2.51 for attendance rate, 4.08 for chronic absenteeism, and 2.63 for days absent) and the 2023-24 school year for the 2023-24 estimates (4.39 for attendance rate, 2.72 for chronic absenteeism, and 4.81 for days absent). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

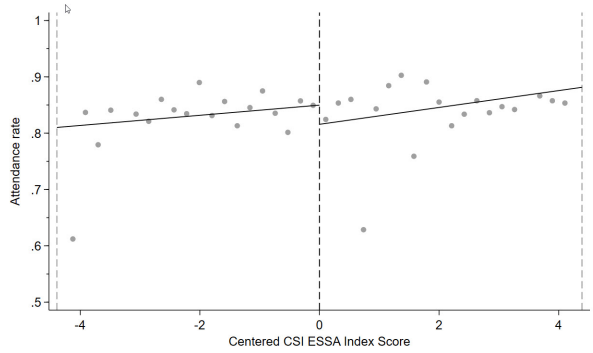
FIGURE A2. Estimated effects on student attendance and absenteeism by school identification group, all grades

Panel A. Attendance Rate

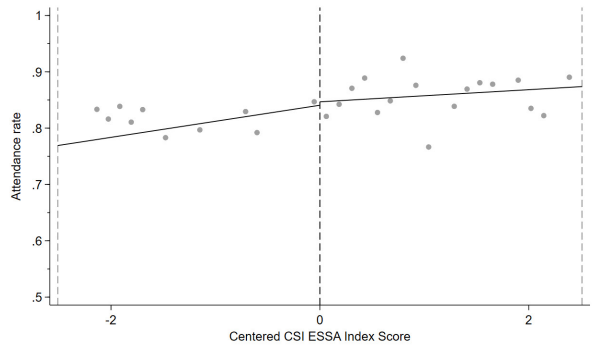
A1. Overall, 2022-23



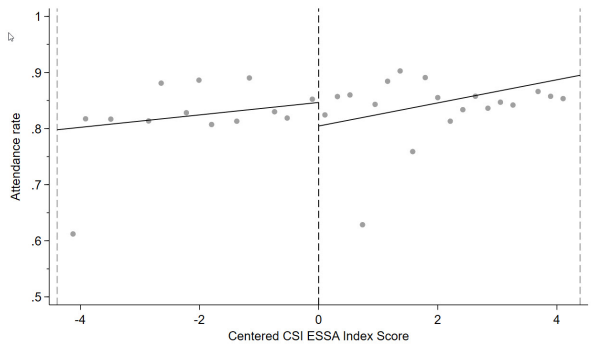
A2. Overall, 2023-24



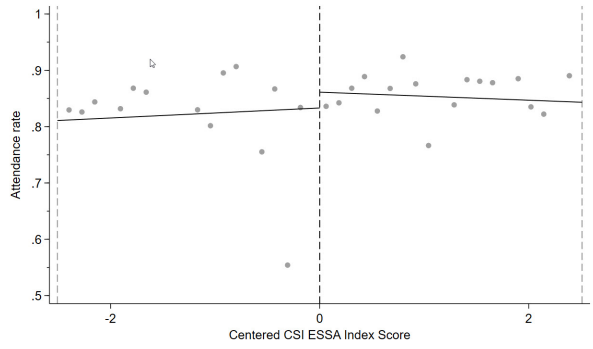
A3. Reidentified, 2022-23



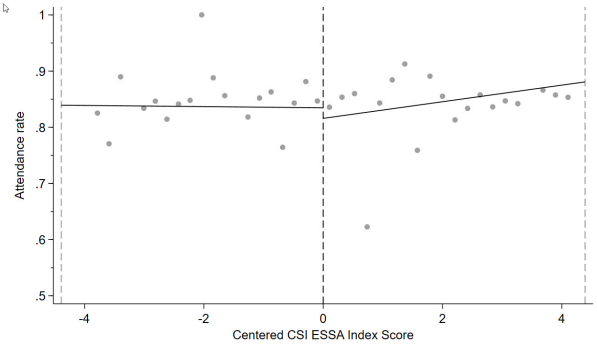
A4. Reidentified, 2023-24



A5. Newly Identified, 2022-23



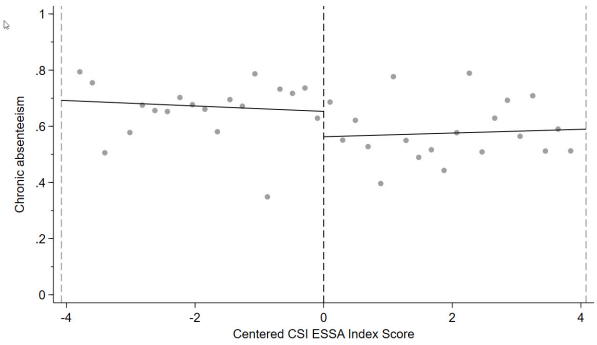
A6. Newly Identified, 2023-24



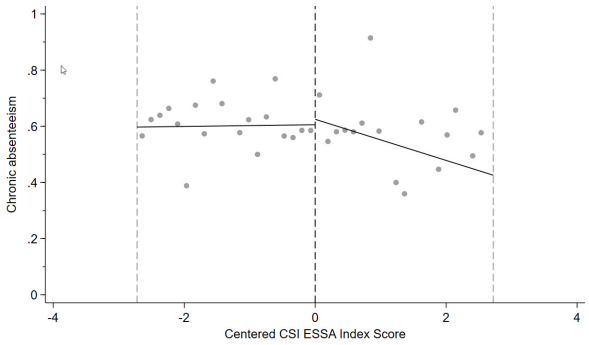
APPENDIX A. *(continued)*

Panel B. Chronic Absenteeism

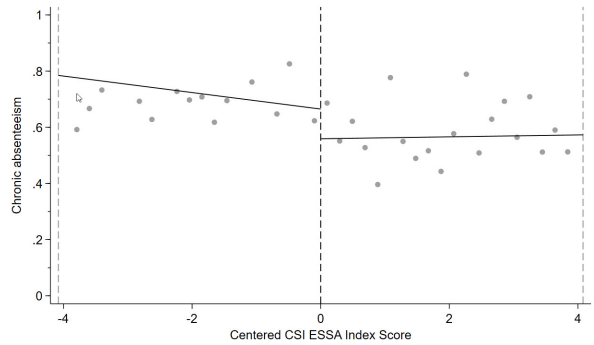
B1. Overall, 2022-23



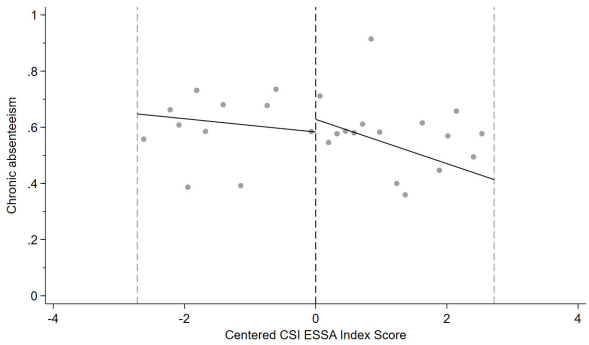
B2. Overall, 2023-24



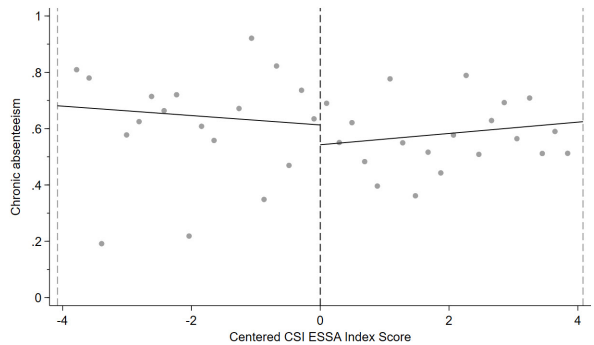
B3. Reidentified, 2022-23



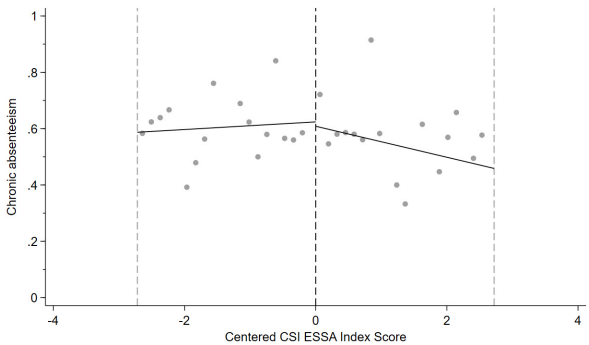
B4. Reidentified, 2023-24



B5. Newly Identified, 2022-23



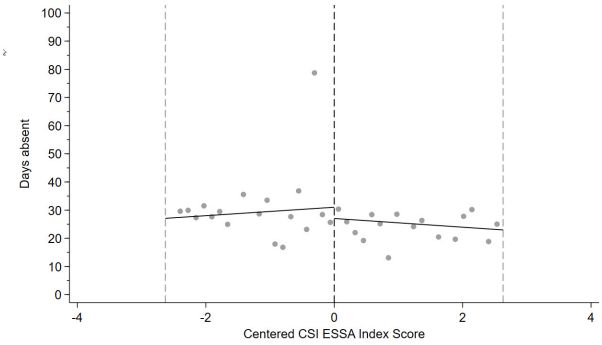
B6. Newly Identified, 2023-24



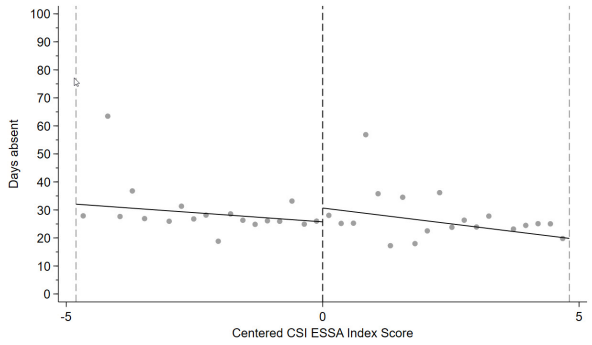
APPENDIX A. *(continued)*

Panel C. Days Absent

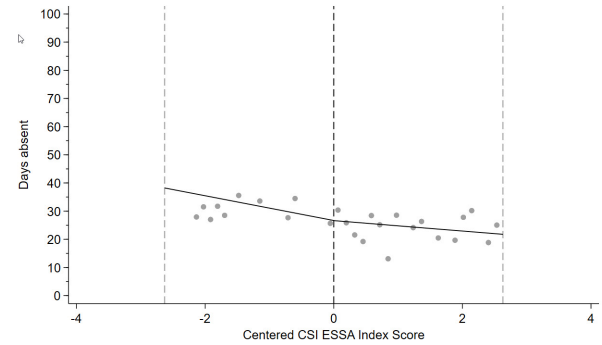
C1. Overall, 2022-23



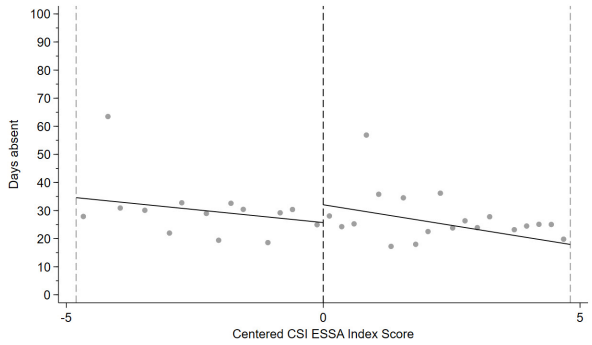
C2. Overall, 2023-24



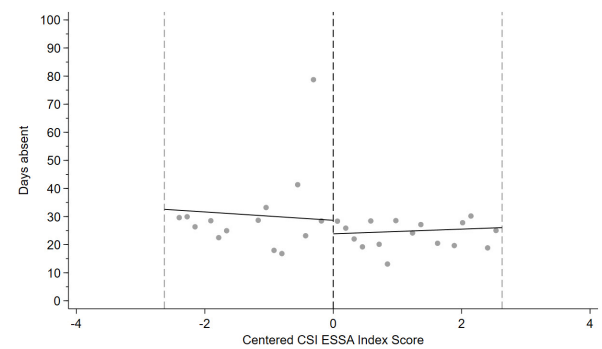
C3. Reidentified, 2022-23



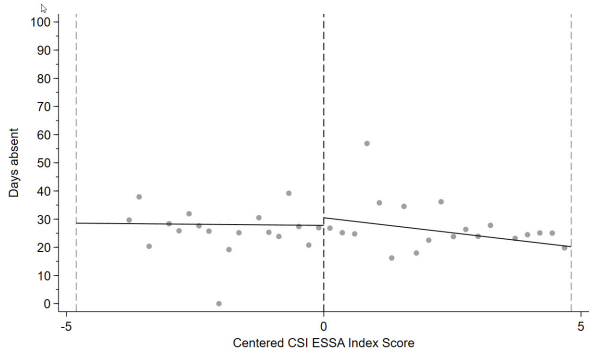
C4. Reidentified, 2023-24



C5. Newly Identified, 2022-23



C6. Newly Identified, 2023-24



*Notes: Estimates from regression discontinuity models predicting attendance rate (Panel A), chronic absenteeism (Panel B), and days absent (Panel C), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in Panel A and B can be interpreted in percentage point differences, whereas results from Panel C can be interpreted as number of days. Models exclude non-Partnership CSI schools. The overall sample includes all Partnership schools (row 1 of each panel). The reidentified models exclude Partnership schools that were newly identified in Cohort 3 (row 2). The newly identified models exclude Partnership schools that were previously identified for Partnership in Cohort 1 or 2 (row 3). All three attendance outcomes are estimated in separate models, but we show them in a single figure for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2022-23 school year for the 2022-23 estimates (2.51 for attendance rate, 4.08 for chronic absenteeism, and 2.63 for days absent) and the 2023-24 school year for the 2023-24 estimates (4.39 for attendance rate, 2.72 for chronic absenteeism, and 4.81 for days absent). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

TABLE A7. Estimated effects on student attendance and absenteeism outcomes, grades 4-8 only, 2022-23			
	100% BW	150% BW	200% BW
Attendance rate	0.005 (0.020)	-0.003 (0.020)	-0.001 (0.021)
Chronic absenteeism	0.006 (0.071)	0.014 (0.063)	0.026 (0.055)
Days absent	-1.600 (3.087)	-0.017 (3.220)	-0.170 (3.379)
Student N [School N]	436,230 [2107]	436,230 [2107]	436,230 [2107]

*Notes: Estimates from regression discontinuity models predicting attendance rate (row 1), chronic absenteeism (row 2), and days absent (row 3) in grades 4–8 only (i.e., the student achievement sample), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in rows 1 and 2 can be interpreted in percentage point differences, whereas results from row 3 can be interpreted as number of days. Models exclude non-Partnership CSI schools. All three attendance outcomes are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2022-23 school year (2.62 for attendance rate, 4.24 for chronic absenteeism, and 2.61 for days absent). * p<0.05, ** p<0.01, *** p<.001*

TABLE A8. Estimated effects on student attendance and absenteeism outcomes, grades 4-8 only, 2023-24			
	100% BW	150% BW	200% BW
Attendance rate	0.028 (0.027)	0.018 (0.025)	0.013 (0.023)
Chronic absenteeism	-0.012 (0.055)	0.006 (0.048)	0.014 (0.043)
Days absent	-4.196 (4.479)	-2.747 (4.121)	-2.160 (3.723)
Student N [School N]	441,812 [2113]	441,812 [2113]	441,812 [2113]

*Notes: Estimates from regression discontinuity models predicting attendance rate (row 1), chronic absenteeism (row 2), and days absent (row 3) in grades 4–8 only (i.e., the student achievement sample), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in rows 1 and 2 can be interpreted in percentage point differences, whereas results from row 3 can be interpreted as number of days. Models exclude non-Partnership CSI schools. All three attendance outcomes are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2023-24 school year (4.93 for attendance rate, 5.08 for chronic absenteeism, and 5.34 for days absent). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

TABLE A9. Estimated effects on student attendance and absenteeism by school designation group, grades 4-8 only

	2022-23		2023-24	
	Reidentified	**Newly identified**	**Reidentified**	**Newly identified**
Attendance rate	0.007 (0.019)	-0.024 (0.017)	0.013 (0.033)	0.050+ (0.026)
Chronic absenteeism	0.030 (0.092)	0.030 (0.061)	0.022 (0.075)	-0.051 (0.047)
Days absent	-2.077 (2.855)	3.306 (2.461)	-1.798 (5.207)	-7.736+ (4.402)
Student N [School N]	437,227 [2071]	437,159 [2082]	431,780 [2066]	431,677 [2076]

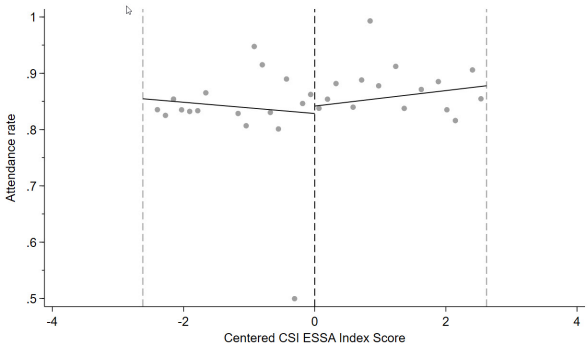
*Notes: Estimates from regression discontinuity models predicting attendance rate (row 1), chronic absenteeism (row 2), and days absent (row 3) in grades 4–8 only (i.e., the student achievement sample), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in rows 1 and 2 can be interpreted in percentage point differences, whereas results from row 3 can be interpreted as number of days. Models exclude non-Partnership CSI schools. The reidentified models exclude Partnership schools that were newly identified in Cohort 3. The newly identified models exclude Partnership schools that were previously identified for Partnership in Cohort 1 or 2. All three attendance outcomes are estimated in separate models, but we show them in a single table for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2022-23 school year for the 2022-23 estimates (2.62 for attendance rate, 4.24 for chronic absenteeism, and 2.61 for days absent) and the 2023-24 school year for the 2023-24 estimates (4.93 for attendance rate, 5.08 for chronic absenteeism, and 5.34 for days absent). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX A. *(continued)*

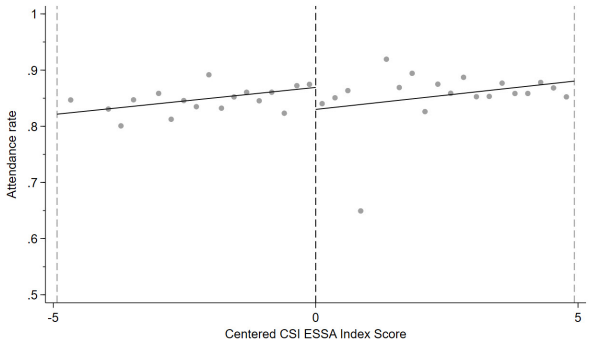
FIGURE A3. Estimated effects on student attendance and absenteeism by school identification group, grades 4-8 only

Panel A. Attendance Rate

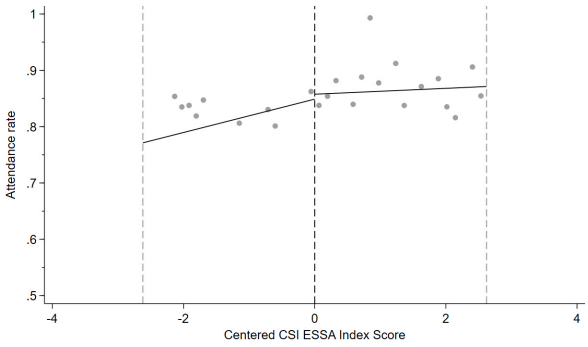
A1. Overall, 2022-23



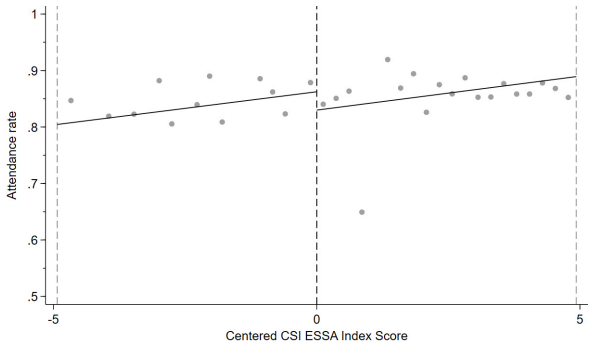
A2. Overall, 2023-24



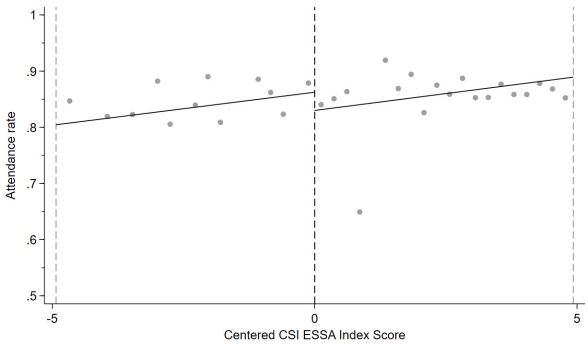
A3. Reidentified, 2022-23



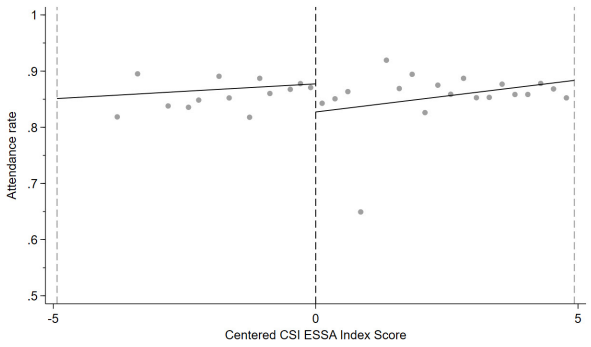
A4. Reidentified, 2023-24



A5. Newly Identified, 2022-23



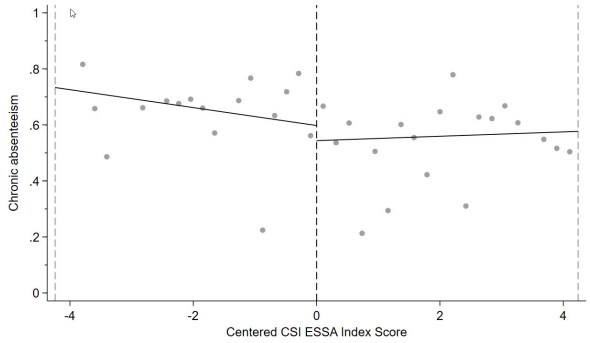
A6. Newly Identified, 2023-24



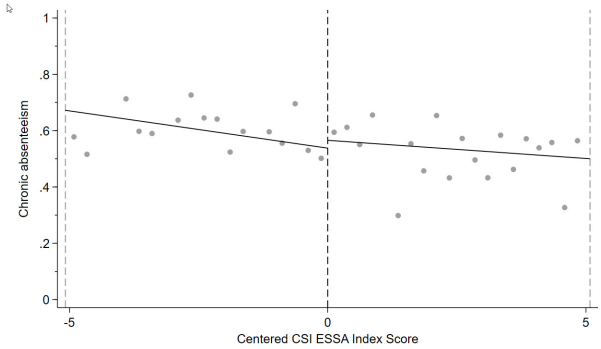
APPENDIX A. *(continued)*

Panel B. Chronic Absenteeism

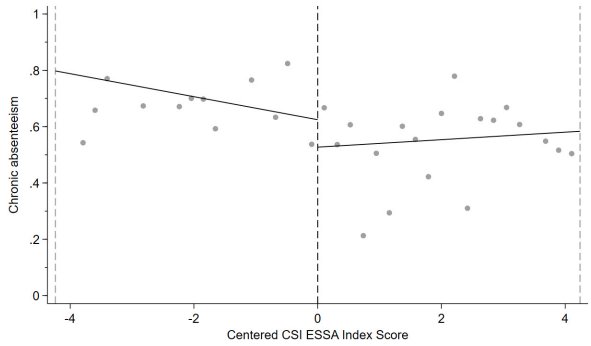
B1. Overall, 2022-23



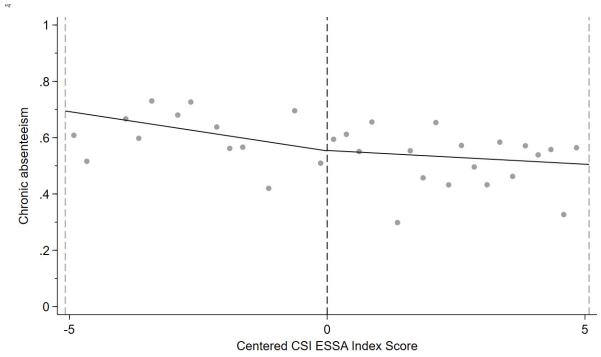
B2. Overall, 2023-24



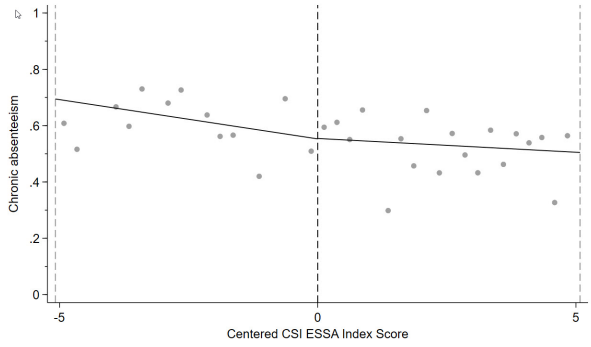
B3. Reidentified, 2022-23



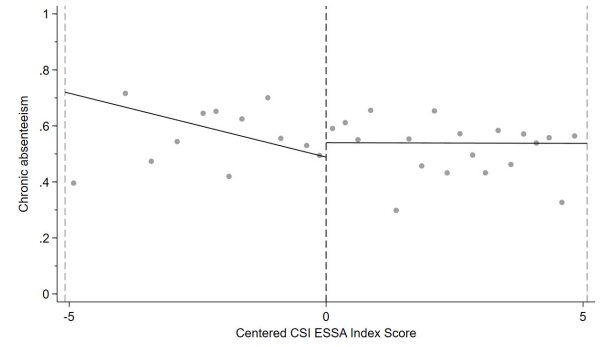
B4. Reidentified, 2023-24



B5. Newly Identified, 2022-23



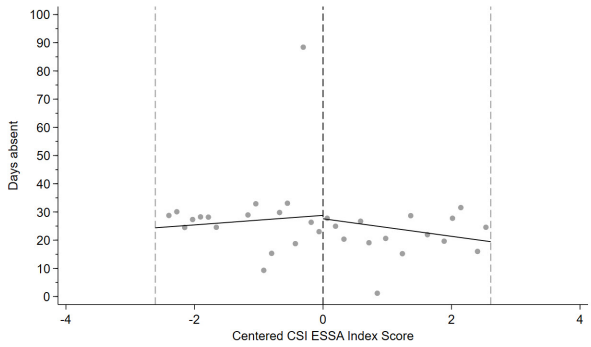
B6. Newly Identified, 2023-24



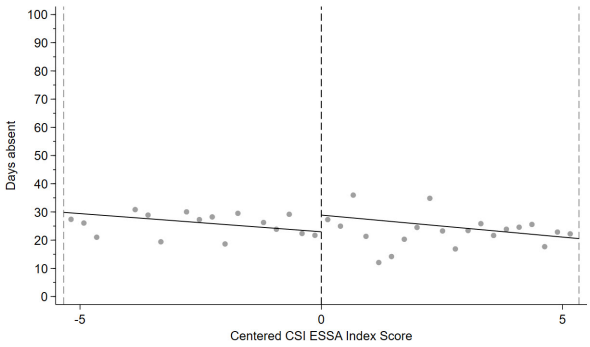
APPENDIX A. *(continued)*

Panel C. Days Absent

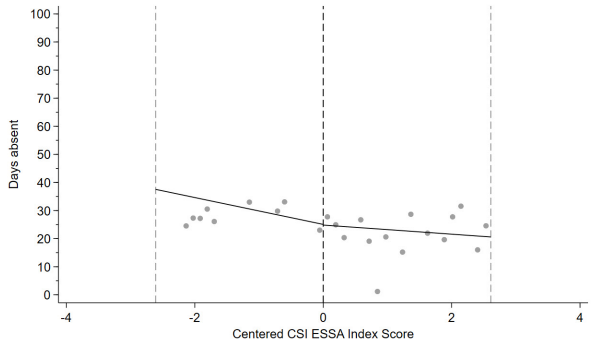
C1. Overall, 2022-23



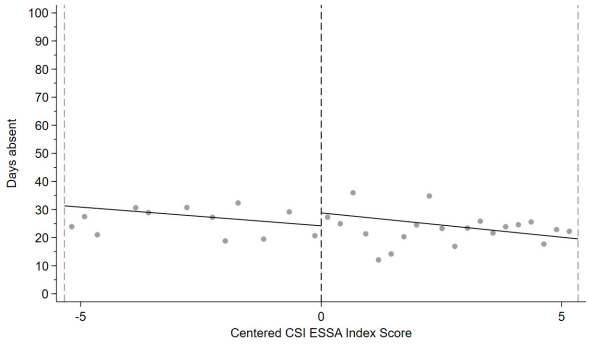
C2. Overall, 2023-24



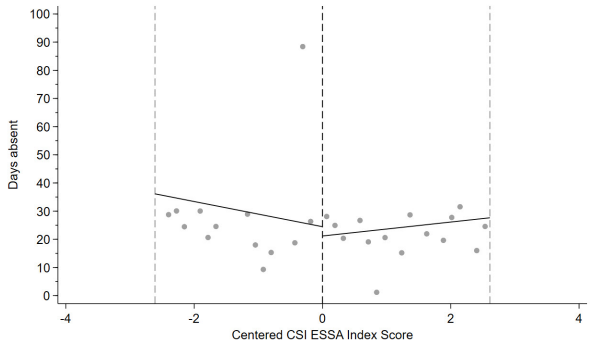
C3. Reidentified, 2022-23



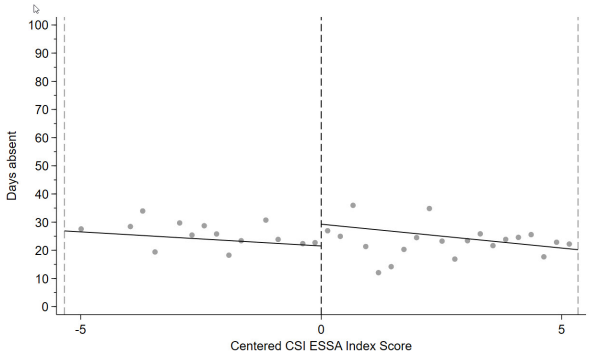
C4. Reidentified, 2023-24



C5. Newly Identified, 2022-23



C6. Newly Identified, 2023-24



*Notes: Estimates from regression discontinuity models predicting attendance rate (Panel A), chronic absenteeism (Panel B), and days absent (Panel C) in grades 4–8 only (i.e., the student achievement sample), respectively. Attendance rate is measured as a rate that can range from zero (having attended no enrolled days) to one (having attended 100% of enrolled days). Chronic absenteeism is an indicator that takes a value of one if a student was chronically absent and zero if not. Days absent is measured in days. Thus, results in Panel A and B can be interpreted in percentage point differences, whereas results from Panel C can be interpreted as number of days. Models exclude non-Partnership CSI schools. The overall sample includes all Partnership schools (row 1 of each panel). The reidentified models exclude Partnership schools that were newly identified in Cohort 3 (row 2). The newly identified models exclude Partnership schools that were previously identified for Partnership in Cohort 1 or 2 (row 3). All three attendance outcomes are estimated in separate models, but we show them in a single figure for brevity. All models control for student covariates (Black, Hispanic, other nonwhite, male, economic disadvantage, EL status, and special education) and school-level covariates measured in the 2021-22 baseline school year (% race/ethnicity, % economic disadvantage, %EL, % special education, and logged enrollment). The days absent model has an additional student-level covariate: the possible days a student could have attended school. The optimal bandwidth was calculated separately for each attendance outcome from the 2022-23 school year for the 2022-23 estimates (2.62 for attendance rate, 4.24 for chronic absenteeism, and 2.61 for days absent) and the 2023-24 school year for the 2023-24 estimates (4.93 for attendance rate, 5.08 for chronic absenteeism, and 5.34 for days absent). * p<0.05, ** p<0.01, *** p<.001*

APPENDIX B. REGRESSION DISCONTINUITY RESULTS, TEACHER OUTCOMES

Table B1. Estimated effects on teacher turnover, 2022-23

	100% BW (+/- 6.00)	150% BW (+/- 9.01)	200% BW (+/- 12.01)
Any Turnover	0.027 (0.035)	0.027 (0.030)	0.009 (0.030)
Teacher N [School N]	58,297 [2000]	58,297 [2000]	58,297 [2000]
N Bandwidth [School N]	2,543 [108]	4,098 [163]	5,021 [204]
Treated in BW [School N]	1,089 [46]	1,431 [59]	1,522 [66]
Comparison in BW [School N]	1,454 [62]	2,667 [104]	3,499 [138]

Table B2. Estimated effects on teacher turnover, 2023-24

	100% BW (+/- 6.00)	150% BW (+/- 9.01)	200% BW (+/- 12.01)
Any Turnover	0.048 (0.037)	0.054 (0.037)	0.057 (0.037)
Teacher N [School N]	59,340 [2,498]	59,340 [2498]	59,340 [2498]
N Bandwidth [School N]	2,033 [96]	2,694 [134]	3,342 [166]
Treated in BW [School N]	970 [45]	1,171 [57]	1,309 [63]
Comparison in BW [School N]	1,063 [51]	1,523 [77]	2,033 [103]

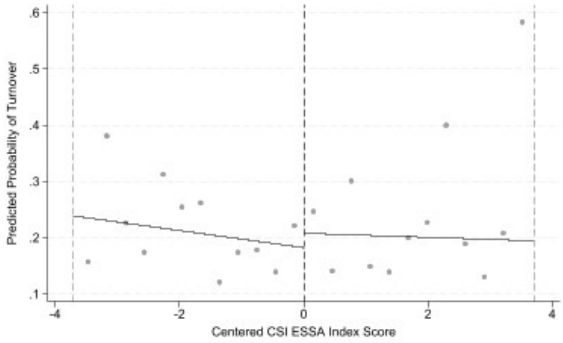
Table B3. Estimated effects on teacher turnover by school designation group

	2022-23		2023-24	
	Reidentified	Newly identified	Reidentified	Newly identified
Any Turnover	0.081* (0.036)	-0.028 (0.037)	0.007 (0.043)	0.093* (0.039)
Teacher N [School N]	57,462 [1962]	57,355 [1,963]	58,414 [2447]	58,382 [2454]
N Bandwidth [School N]	1,656 [62]	1,595 [66]	1,280 [57]	1,981 [101]
Treated in BW [School N]	509 [18]	499 [24]	405 [15]	591 [31]
Comparison in BW [School N]	1,147 [44]	1,096 [42]	875 [42]	1,390 [70]

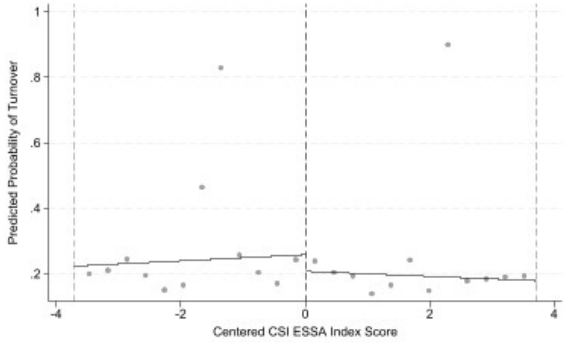
APPENDIX B. *(continued)*

Figure B1. Estimated effects on teacher turnover by school identification group

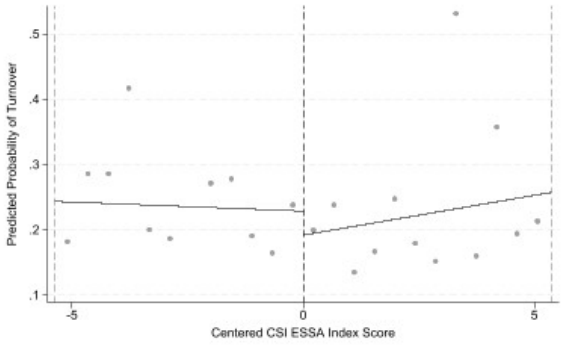
Panel A. Full, 2022-23



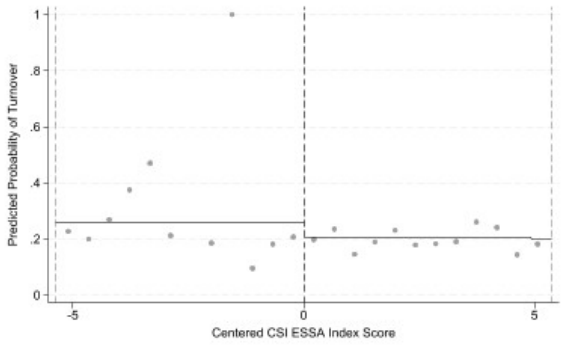
Panel B. Full, 2023-24



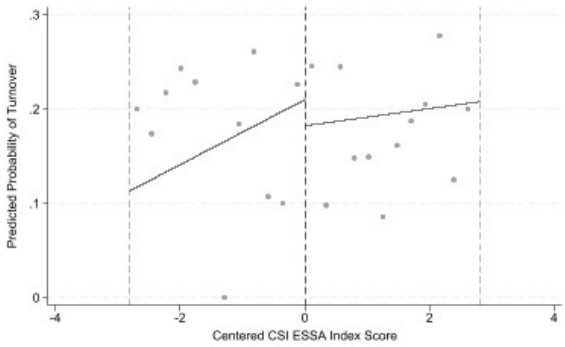
Panel C. Reidentified, 2022-23



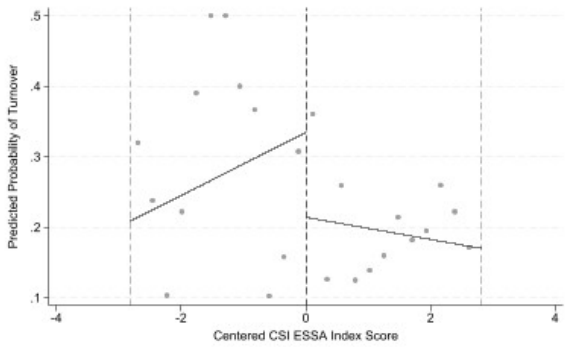
Panel D. Reidentified, 2023-24



Panel E. Newly Identified, 2022-23



Panel F. Newly Identified, 2023-24



APPENDIX B. *(continued)*

Table B4. Estimated effects on teacher turnover, 2022-23			
	100% BW (+/- 6.00)	**150% BW (+/- 9.01)**	**200% BW (+/- 12.01)**
Within-District Transfer	0.007 (0.028)	-0.007 (0.024)	-0.022 (0.025)
Between-District Transfer	0.002 (0.013)	-0.002 (0.012)	-0.006 (0.011)
Leave MI Education	0.017 (0.013)	0.036** (0.012)	0.037** (0.012)
Teacher N [School N]	58,297 [2000]	58,297 [2000]	58,297 [2000]
N Bandwidth [School N]	2,543 [108]	4,098 [163]	5,021 [204]
Treated in BW [School N]	1,089 [46]	1,431 [59]	1,522 [66]
Comparison in BW [School N]	1,454 [62]	2,667 [104]	3,499 [138]

Table B5. Estimated effects on teacher turnover, 2023-24			
	100% BW (+/- 6.00)	**150% BW (+/- 9.01)**	**200% BW (+/- 12.01)**
Within-District Transfer	0.047* (0.024)	0.059* (0.028)	0.067* (0.030)
Between-District Transfer	0.026 (0.016)	0.018 (0.016)	0.015 (0.015)
Leave MI Education	-0.025 (0.031)	-0.024 (0.026)	-0.025 (0.024)
Teacher N [School N]	59,340 [2,498]	59,340 [2498]	59,340 [2498]
N Bandwidth [School N]	2,033 [96]	2,694 [134]	3,342 [166]
Treated in BW [School N]	970 [45]	1,171 [57]	1,309 [63]
Comparison in BW [School N]	1,063 [51]	1,523 [77]	2,033 [103]

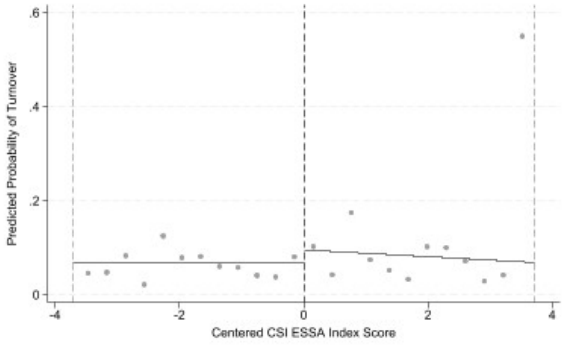
APPENDIX B. *(continued)*

Table B6. Estimated effects on teacher turnover by school designation group				
	2022-23		2023-24	
	Reidentified	Newly identified	Reidentified	Newly identified
Any Turnover	0.081* (0.036)	-0.028 (0.037)	0.007 (0.043)	0.093* (0.039)
Teacher N [School N]	57,462 [1962]	57,355 [1,963]	58,414 [2447]	58,382 [2454]
N Bandwidth [School N]	1,656 [62]	1,595 [66]	1,280 [57]	1,981 [101]
Treated in BW [School N]	509 [18]	499 [24]	405 [15]	591 [31]
Comparison in BW [School N]	1,147 [44]	1,096 [42]	875 [42]	1,390 [70]

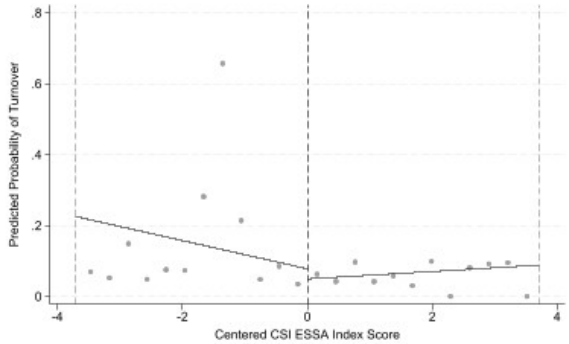
APPENDIX B. *(continued)*

Figure B2. Estimated effects on within-district transfer by school identification group

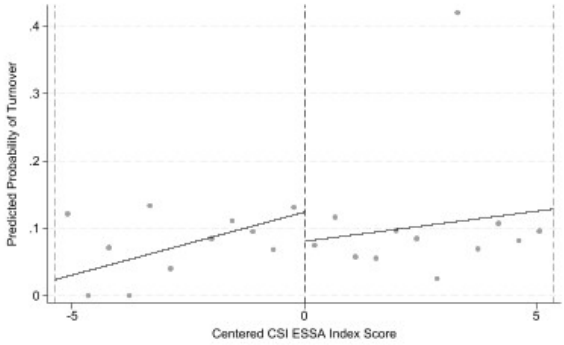
Panel A. Full, 2022-23



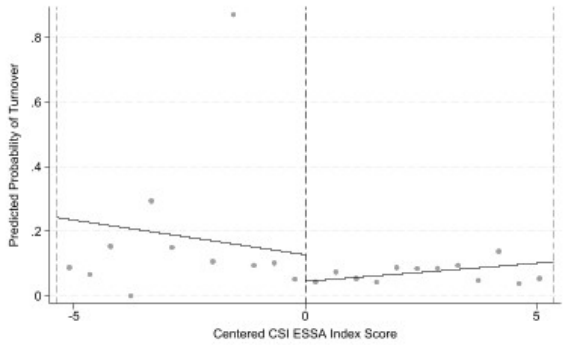
Panel B. Full, 2023-24



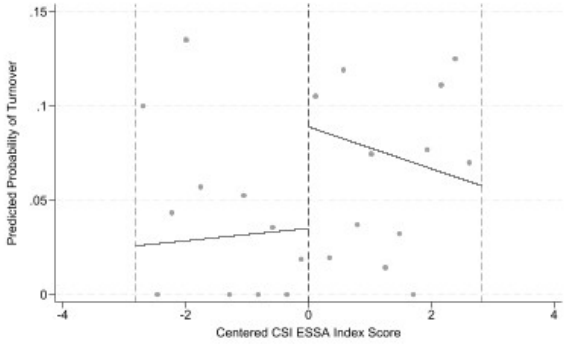
Panel C. Reidentified, 2022-23



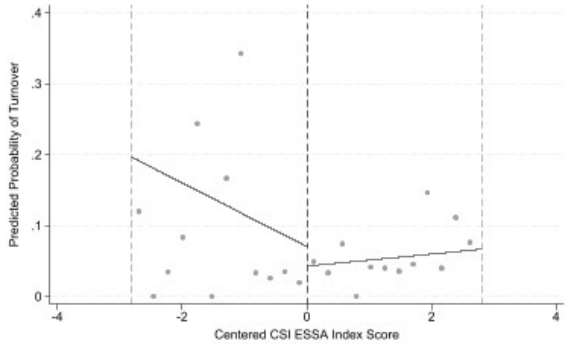
Panel D. Reidentified, 2023-24



Panel E. Newly Identified, 2022-23



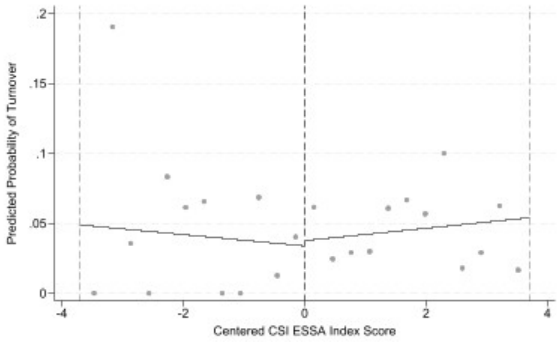
Panel F. Newly Identified, 2023-24



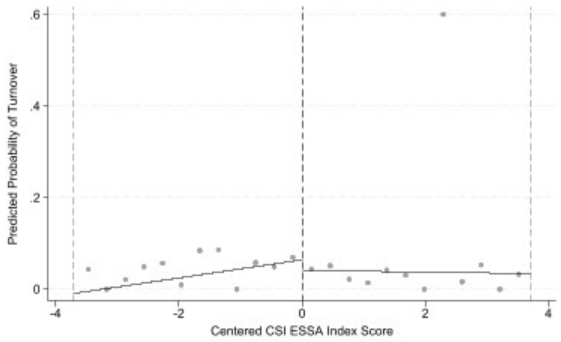
APPENDIX B. *(continued)*

Figure B3. Estimated effects on between-district transfer by school identification group

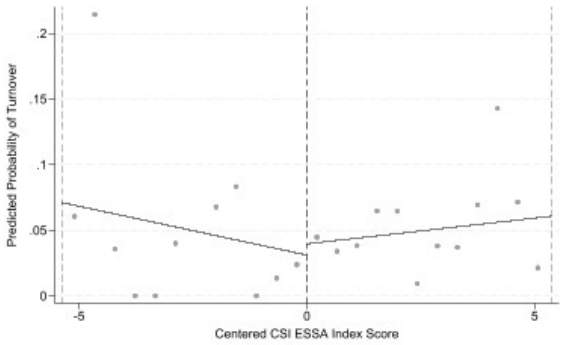
Panel A. Full, 2022-23



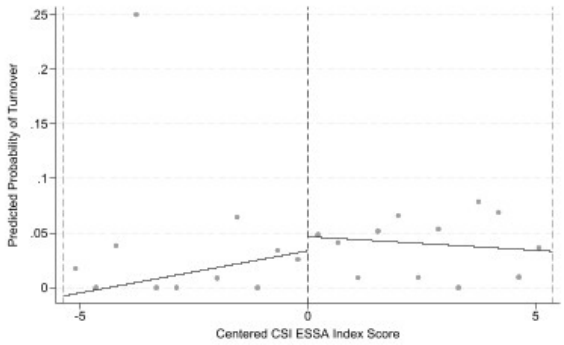
Panel B. Full, 2023-24



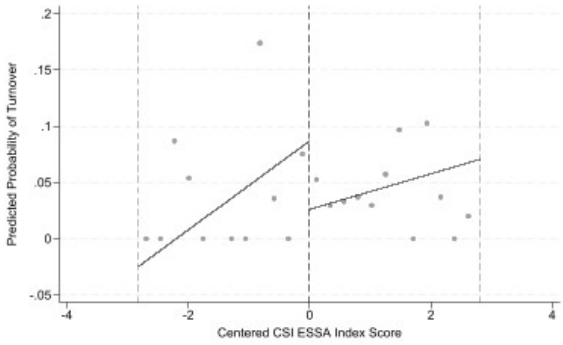
Panel C. Reidentified, 2022-23



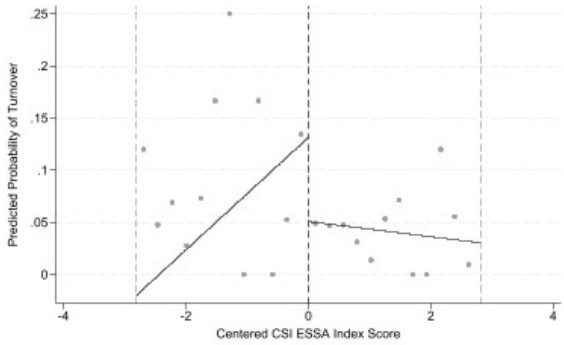
Panel D. Reidentified, 2023-24



Panel E. Newly Identified, 2022-23



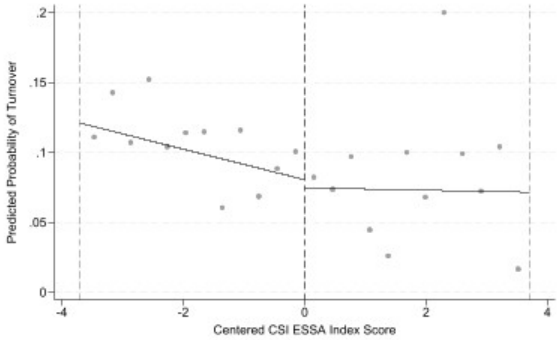
Panel F. Newly Identified, 2023-24



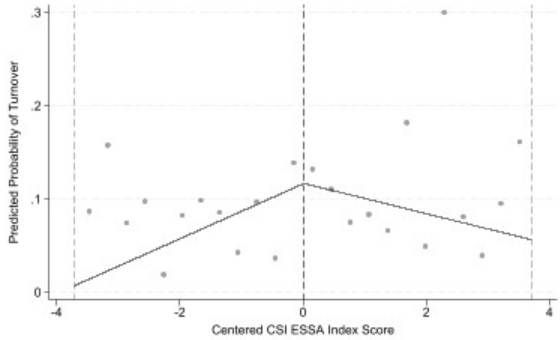
APPENDIX B. *(continued)*

Figure B4. Estimated effects on leaving Michigan education by school identification group

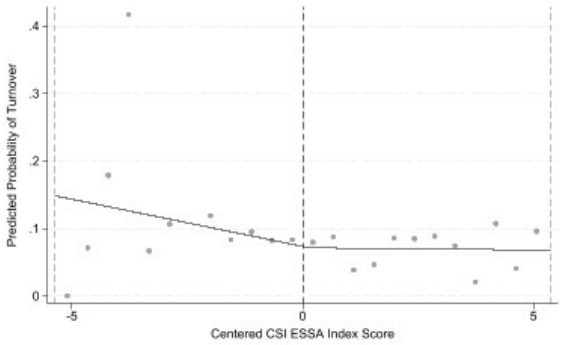
Panel A. Full, 2022-23



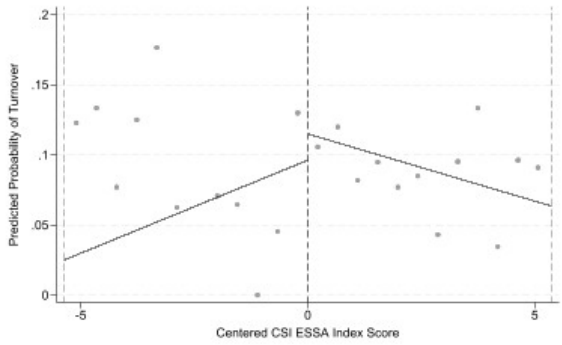
Panel B. Full, 2023-24



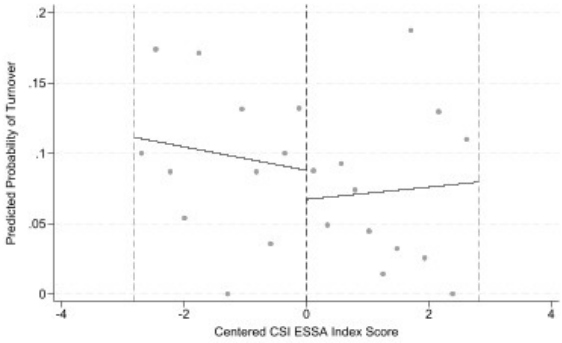
Panel C. Reidentified, 2022-23



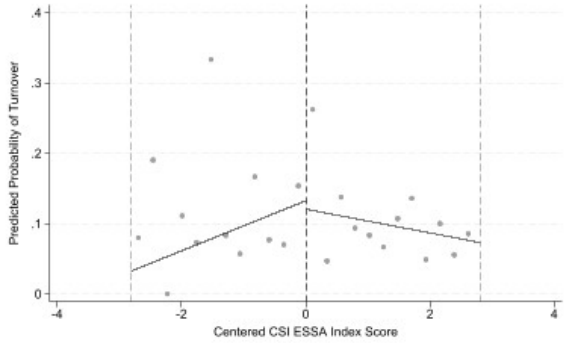
Panel D. Reidentified, 2023-24



Panel E. Newly Identified, 2022-23



Panel F. Newly Identified, 2023-24



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