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School-Based Support Staff and Chronic Absenteeism: Evidence from North Carolina

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In this paper, I investigate the causal impact of school-based support staff on student absences and chronic absenteeism using the introduction of the Child and Family Support Team (CFST) program in North Carolina. Introduced during the 2006/07 school year, the CFST program funded two-person teams composed of a nationally-certified school nurse and a state-licensed school social worker in 43 elementary schools. The teams worked in schools to identify and intervene with students who faced economic, health, and social barriers to academic success. Taking advantage of two sources of variation, I estimate impacts for students in directly treated schools, which received CFST teams, and indirectly treated schools, which experienced staffing increases through district reallocation and hiring responses to CFST. Using event-study and difference-in-differences approaches, I find that high-risk students in directly treated schools experienced reductions in absences of 0.4 days per year (6%) and a 0.9 percentage point reduction in the likelihood of chronic absenteeism (12%). Students in indirectly treated schools experienced smaller declines in absences of 0.2 days per year (3%) but no statistically significant effects on the likelihood of chronic absenteeism. Although I interpret the estimates from indirectly treated schools with caution, the pattern and magnitude of effects corroborate the main findings from directly treated schools. Overall, the main results indicate that intensive, team-based school-based support staffing models are effective for identifying and supporting high-risk students. The findings highlight the role of health and social services in reducing chronic absenteeism and inform education policy related to school-based support staff in K-12 public schools.

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School-Based Support Staff and Chronic Absenteeism: Evidence from North Carolina*

Sarah Komisarow†

Abstract

In this paper, I investigate the causal impact of school-based support staff on student absences and chronic absenteeism using the introduction of the Child and Family Support Team (CFST) program in North Carolina. Introduced during the 2006/07 school year, the CFST program funded two-person teams composed of a nationally-certified school nurse and a state-licensed school social worker in 43 elementary schools. The teams worked in schools to identify and intervene with students who faced economic, health, and social barriers to academic success. Taking advantage of two sources of variation, I estimate impacts for students in directly treated schools, which received CFST teams, and indirectly treated schools, which experienced staffing increases through district reallocation and hiring responses to CFST. Using event-study and difference-in-differences approaches, I find that high-risk students in directly treated schools experienced reductions in absences of 0.4 days per year (6%) and a 0.9 percentage point reduction in the likelihood of chronic absenteeism (16%). Students in indirectly treated schools experienced smaller declines in absences of 0.2 days per year (3%) but no statistically significant effects on the likelihood of chronic absenteeism. Although I interpret the estimates from indirectly treated schools with caution, the pattern and magnitude of effects corroborate the main findings from directly treated schools. Overall, the main results indicate that intensive, team-based school-based support staffing models are effective for identifying and supporting high-risk students. The findings highlight the role of health and social services in reducing chronic absenteeism and inform education policy related to school-based support staff in K–12 public schools.

Keywords: K–12 Public Schools, Chronic Absenteeism, School-Based Support Staff
JEL codes: I20, I21, J18, H75

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1 Introduction

Student absences, and particularly chronic absenteeism, are pressing and policy-relevant challenges for K–12 public schools in the United States (U.S.). Persistently high levels of chronic absenteeism (defined as missing more than 10% of school days each year) are concerning for policymakers because of their substantial consequences for student outcomes in both the short- and long-run. Causal evidence suggests that each additional 10 days of absence reduces test scores by approximately 2.9–6.7% of a standard deviation and that school absences negatively impact outcomes in adulthood, including educational attainment and employment (Fitzpatrick et al., 2011; Aucejo and Romano, 2016; Liu et al., 2021; Cattan et al., 2023). Emerging research further suggests that chronic absenteeism continues to slow post-pandemic learning recovery, which underscores its system-wide consequences (Council of Economic Advisers, 2023; Dee, 2024; Guinan and Hill, 2026).

Recent estimates indicate that around 24% of students in the U.S. are chronically absent each year (Malkus, 2025). Although this rate is lower than the post-pandemic peak from the 2021/22 school year, chronic absenteeism remains well above pre-pandemic levels, which hovered around 15% (Dee, 2024; Diliberti et al., 2025; Malkus, 2025). Explanations for this rise include more relaxed attendance expectations, greater parental concern about child illness, and worsening student mental health (Diliberti et al., 2025), but no single factor can explain both the sharp increase and persistence. The complex, multifaceted nature of the problem suggests that effective strategies to reduce chronic absenteeism will need to address educational, social, and health-related factors simultaneously.

One promising yet underexplored strategy is to engage school-based support staff, including school nurses and social workers, to address student needs directly. School-based support staff have long played a critical role in addressing non-academic barriers to student learning, including physical health, mental health, and family issues (National Association of School Social Workers, 2012; American School Counselor Association, 2019; Davis et al., 2021), all of which are factors that are increasingly linked to chronic absenteeism (Jacob and Lovett, 2017; Allison et al., 2019). Unlike traditional absence interventions that focus on monitoring and information (Rogers and Feller, 2018; Heppen et al., 2020; Rhode Island Public Expenditure Council, 2026) or community-based interventions that address neighborhood-level barriers to school attendance (Komisarow and

Gonzalez, 2023), school-based support staff provide direct, individualized support to students and families. Correlational and case study evidence increasingly suggests the potential of school-based support staff in reducing chronic absenteeism (Allen, 2003; Yoder, 2020; Rankine et al., 2023).

This paper studies increased exposure to school-based support staff following the introduction of the Child and Family Support Team (CFST) program in North Carolina. This state-level program fully funded two-person teams composed of a nationally-certified school nurse and a state-licensed school social worker in 100 disadvantaged K–12 schools in North Carolina beginning in the 2006/07 school year. These teams worked with students and families to address barriers to learning arising from health, social, and other family challenges. The school nurse and social worker teams identified, evaluated, and intervened with students by providing services such as assessment, case management, and connections to community resources.

Prior to CFST, most K–12 schools in North Carolina lacked a full-time school nurse or social worker; the program thus represented a substantial increase in school-based support staffing. I leverage the introduction of CFST teams in schools that received a two-person team (henceforth, "directly treated schools") to estimate the causal effects of increased support staff on student absences and chronic absenteeism. I then examine a secondary, complementary source of variation arising from school district responses to CFST. I define "indirectly treated schools" as schools located in the same districts as CFST schools that did not themselves receive a two-person CFST team. Although these indirectly treated schools did not receive CFST teams, they experienced changes in school-based support staffing levels as districts reallocated and hired personnel in response to the CFST program rollout. Because the staffing changes in indirectly treated schools reflect district-level responses, estimates from these schools should be considered potentially endogenous and interpreted with caution. Nonetheless, the pattern and magnitudes of effects observed in indirectly treated schools provide suggestive evidence—albeit weaker and secondary—that corroborates the main findings from directly treated schools.

Using event-study and difference-in-differences approaches, I conduct two separate analyses. The first compares absence-related outcomes for students in directly treated schools to those in comparison group schools. The second separately compares students in indirectly treated schools to those in the same comparison group. I first estimate CFST-induced changes in school staffing levels and find evidence of large increases in student exposure to social workers and school nurses.

In directly treated schools, the number of social workers increased by 0.63 FTEs (227%) and the number of school nurses increased by 0.34 FTEs (279%). In indirectly treated schools, staffing increases generated through district responses were smaller yet still meaningful; the number of social workers increased by 0.12 FTEs (53%) and the number of school nurses increased by 0.05 FTEs (47%).

These staffing increases translated into meaningful improvements in student outcomes: increased exposure to social workers and school nurses reduced both absences and the likelihood of chronic absenteeism. In directly treated schools, high-risk students experienced a decline of 0.4 days absent annually (6%) and a 0.9 percentage point reduction in chronic absenteeism (16%). In indirectly treated schools, where staffing increases were much smaller, days absent declined by 0.2 days annually (3%) for all students, although I do not detect any statistically significant effects on chronic absenteeism. Although estimates from indirectly treated schools should be interpreted with caution given the potentially endogenous district-led staffing changes, the pattern of effects is consistent with the main results from directly treated schools. Overall, my main findings from directly treated schools indicate that two-person social worker and school nurse teams were particularly effective at identifying and supporting high-risk students, while secondary findings from indirectly treated schools provide corroborating evidence for the role of school-based support staff in reducing absences.

This paper contributes to several strands of literature. First, this paper contributes to the literature on the relationship between children's physical and mental health status and their educational outcomes. Children's physical and mental health affects absences, grade repetition, behavior, and cognitive outcomes (Currie, 2005; Currie and Stabile, 2006). These relationships are reinforced by a small, policy-oriented literature that finds that public health insurance access during childhood has positive impacts on long-run educational attainment and employment (Cohodes et al., 2016; Brown et al., 2020; Goodman-Bacon, 2021).[1]

Second, this paper contributes to the small economics literature on the causal effects of school-based support staff (i.e., guidance counselors, school counselors, and school nurses) on student outcomes. These papers generally find that additional counselors decrease classroom misbehavior

[1]See Aizer (2017) for discussion of evidence on mechanisms other than medical care that could potentially explain the relationship between health status and outcomes in adulthood.

and reduce reported disciplinary incidents in schools, although the evidence on academic outcomes is mixed (Carrell and Carrell, 2006; Reback, 2010a,b; Carrell and Hoekstra, 2014). Abrahamsen et al. (2024) find that exposure to a school nurse in childhood reduces teenage pregnancy and welfare use and increases educational attainment and labor market earnings, although contemporaneous impacts are not examined. A limitation of these studies is that none examine student outcomes related to absences, which leaves open questions regarding how school-based support staff may influence student outcomes through physical and mental health channels.

Third, the paper relates to literature on the provision of healthcare and other support services in schools, including school-based health centers, telemedicine clinics, and other community-based programs. These studies are relevant because they examine services integrated into K–12 public schools. Lovenheim et al. (2016) find that access to a school-based health center reduces teen fertility, while Komisarow and Hemelt (2024) find that access to a school-based telemedicine clinic reduces chronic absenteeism among elementary and middle school students. Komisarow (2022) finds that access to comprehensive support services reduces the likelihood of suspension and increases grade point averages (GPAs) and credit accumulation in early high school. Golberstein et al. (2024) finds that school-based access to mental health services reduces suspensions but does not find evidence of impacts on absences or student achievement. These findings point to the potential to address students' unmet physical health, mental health, and other social support needs in K–12 schools.

The findings from this paper have implications for both research and education policy. First, they highlight the importance of addressing non-academic barriers to learning and reinforce that student absences are shaped by physical health, mental health, and social needs. Second, the results demonstrate that increased access to school-based support staff can be an effective strategy for reducing absences, particularly among students at highest risk of chronic absenteeism. The larger effects observed in directly treated schools further suggest that team-based models of support may be especially well-suited to identifying and addressing complex student needs. Finally, these findings highlight the value of integrating health and social services into K–12 school settings.

2 Background

2.1 Program Design and Implementation

The Child and Family Support Team (CFST) program was introduced beginning in the 2006/07 school year (North Carolina General Assembly, 2005). The program funded two-person teams composed of a nationally-certified school nurse and a state-licensed school social worker in 100 disadvantaged K–12 schools across North Carolina (North Carolina Child and Family Leadership Council, 2006). These teams were integrated into the existing ecosystem of school-level student supports to address barriers to student learning arising from economic, health, and social challenges (Troop and Tyson, 2008; Gifford et al., 2010). Before the introduction of CFST, less than 10% of public schools employed a full-time social worker and less than 5% employed a full-time school nurse.[2] Oversight of the program was provided by the North Carolina Child and Family Leadership Council, which included representatives from multiple state agencies, including the Departments of Public Instruction and Health and Human Services (North Carolina Child and Family Leadership Council, 2006).

Each team consisted of a nationally-certified school nurse and a state-licensed school social worker. National certification for school nurses requires state licensure, clinical hours, and examination, along with ongoing continuing education (National Board Certification for School Nurses, 2021). School social workers in North Carolina are licensed through the Department of Public Instruction (North Carolina Department of Public Instruction, 2022). In practice, these professionals provided complementary services addressing both the physical health, mental health, and family-based needs of students. School nurses addressed acute health needs, managed chronic conditions, and promoted health education and wellness for students. Social workers provided advocacy, case management, crisis intervention, counseling, family support, and coordination with community resources. Students were identified for CFST support through referrals from teachers, administrators, other school staff, and parents (Troop and Tyson, 2008; Gifford et al., 2010). The North Carolina Child and Family Leadership Council monitored the program and published reports on implementation outcomes. Case management and team meeting records showed that school absences and health-related concerns were consistently among the most common unmet needs among referred

[2] Author's calculations using data from the North Carolina Education Research Data Center (NCERDC).

students (North Carolina Child and Family Leadership Council, 2010).

2.2 School Selection and Program Duration

School participation in CFST was determined through a multi-stage selection process. In the first stage, a state-level working group identified 33 candidate school districts based on measures of student need, demographic composition, and institutional strengths (North Carolina Child and Family Leadership Council, 2006). Candidate school districts were then invited to apply for CFST and were allowed to nominate up to ten schools to receive teams, although in practice most nominated fewer (North Carolina Child and Family Leadership Council, 2006). Of the 33 candidate districts identified by the working group, 31 applied for CFST teams. In the second stage, 21 districts were selected to receive CFST teams for the schools they had identified.

Districts selected to receive CFST teams were similar to those that did not on observable characteristics, although selected districts exhibited somewhat higher shares of disadvantaged student populations (see Columns (4) and (5) of Appendix Table A1). My research design compares outcomes in directly treated schools (i.e., schools that received CFST teams) to schools in districts that applied but did not receive any CFST funding (comparison group). I also take advantage of broader changes in staffing driven by district responses and resource reallocation within selected districts and study impacts on students in schools within the same district as CFST schools (indirectly treated schools).

Annual program expenditures averaged approximately $17 million (in 2026 dollars) during the first four years of implementation, but subsequent state budget cuts beginning in the fifth year reduced both the number of schools and the intensity of services (North Carolina Child and Family Leadership Council, 2006, 2012). These changes complicate interpretation of treatment effects in later periods, so I restrict analysis of post-treatment impact to the first four years (i.e., 2006/07 to 2009/10) of the CFST program when funding, staffing, and operational structure were stable.

3 Data

3.1 Data Sources and Variables

Student- and staff-level data in this paper were obtained from the North Carolina Education Research Data Center at Duke University (see Appendix B). These data cover students in third through fifth grades in traditional public schools in North Carolina between the 2003/04 and 2009/10 school years. The data also include information on school staff for the positions of school nurses, social workers, guidance counselors, and school psychologists over the same period. Student-level variables include enrollment, demographic characteristics, and absences. Absences are reported in days (per year) and transformed into a binary indicator for chronic absenteeism (i.e., missing 10% or more of school days per year). Staff-level variables include school(s) served, position, full-time equivalent (FTE) allocation, and funding source. I aggregate the staff-level data to the school-by-year level and, in some analyses, further disaggregate by funding source (i.e., CFST-funded versus not CFST-funded). Information on school-level CFST teams came from public North Carolina Child and Family Leadership Council reports (see Appendix B).

3.2 Directly Treated, Indirectly Treated, and Comparison Group Schools

In this paper, I take advantage of the introduction of CFST during the 2006/07 school year, when two-person social worker and school nurse teams were placed in 43 elementary schools. I define these schools as "directly treated," as they received a state-funded CFST team beginning in 2006/07. In addition, I examine "indirectly treated" schools, defined as schools located in the same districts as directly treated schools that did not receive a CFST team. These indirectly treated schools experienced secondary effects from the CFST program, including the reallocation of existing school nurse and social worker time from directly treated schools and district-led efforts to increase staffing in non-CFST schools. Although these reallocations and hiring were not intended as part of CFST, they are present in the data and represent an important channel through which the program affected staffing levels more broadly.

Separately for both directly and indirectly treated schools, I compare staffing levels and student outcomes to those in a comparison group of elementary schools located in districts that applied for but did not receive the CFST program. The comparison group schools were not exposed to CFST and did not experience any staffing changes related to the introduction of the program. Figure 1 illustrates the geographic distribution of directly treated (triangles), indirectly treated (squares), and comparison schools (circles) in North Carolina. Directly and indirectly treated schools are geographically close to one another, as they are located within the same school districts by design. Comparison group schools, in contrast, are spread across similar regions throughout the state.

3.3 Program Targeting and Predicted Student Risk

Although any student enrolled in a directly treated school could be referred to the CFST team for evaluation or services, the teams were explicitly instructed to prioritize students who faced the highest risk of negative academic outcomes. To capture this targeting, I constructed a student-level index predicting risk of chronic absenteeism. Specifically, using data from the pre-CFST period (i.e., the 2003/04 to 2005/06 school years), I estimated a model predicting the likelihood of chronic absenteeism as a function of student- and school-level characteristics. I then used the estimated parameters from this model to compute each student's predicted probability of chronic absenteeism in the full sample. Finally, I classified students as either high- or low-risk based on whether their predicted probability fell above or below the sample median. I refer to this approach as the "in-sample" predicted risk measure. As a robustness check, I repeated the exercise using students enrolled in all other schools in North Carolina in the pre-CFST period (I refer to this as the "out-of-sample" approach); the resulting index is very similar, with a correlation of 0.98 between the two measures.

3.4 Summary Statistics

Appendix Table A2 reports summary statistics for students in my sample at baseline. Columns (1)–(4) report means for students in the full sample, directly treated, indirectly treated, and comparison schools, respectively, while Columns (5)–(7) report differences between groups. Students enrolled in directly treated schools are more likely to be Black (17.5 percentage points) or Hispanic (7.4 percentage points), less likely to be White (−24.9 percentage points), and more likely to be economically disadvantaged (25.6 percentage points). Students in directly treated schools also have 1.0 percentage point higher predicted risk of chronic absenteeism relative to students in comparison group schools. Baseline outcomes also differ: students in directly treated schools are absent 0.42 more days on average and are 1.2 percentage points more likely to be chronically absent. In contrast, differences between students in indirectly treated and comparison group schools are generally small and statistically insignificant. The only significant difference is that students in indirectly treated schools are more likely to be Hispanic (4.1 percentage points).

4 Empirical Methods

In this paper, I use two approaches to estimate the effect of the program on school staffing levels: event-study and difference-in-differences. I estimate the following equations at the school level:

$$Y_{st} = \sum_{\substack{k=-3 \\ k \neq -1}}^{3} \pi_k \times Treat_s \times \mathbf{1}\{t - T_s^* = k\} + Z_{st}\gamma + \alpha_s + \phi_t + \nu_{st} \tag{1a}$$

$$Y_{st} = \alpha + \beta \times Treat_{st} + Z_{st}\gamma + \alpha_s + \phi_t + \nu_{st} \tag{1b}$$

In Equation (1a), Y_{st} is a staffing-related outcome (i.e., position) for school s in year t, expressed in full-time equivalents (FTEs). $Treat_s$ is a binary variable that indicates whether school s was ever treated, and T_s^* is the school year in which treatment was introduced at school s (note: this is the 2006/07 school year across all schools; there is no staggered adoption). The sequence of π_k coefficients traces out the time path of relative differences in outcomes between treated and comparison group schools (k runs from -3 to $+3$, with $k = -1$ omitted). In Equation (1b), I replace the interacted treatment and event-time indicators with a single binary indicator, $Treat_{st}$, that equals one for school s in the year t in which treatment begins (and for all subsequent years). Both equations include α_s and ϕ_t, two-way fixed effects (school and year, respectively), and Z_{st}, a vector of time-varying school characteristics: shares by race/ethnicity, gender, and economic disadvantage, and the natural logarithm of enrollment. β is the difference-in-differences estimate of the treatment effect on school-level staffing outcomes. Standard errors are heteroskedasticity-robust and clustered at the school level.

I then present event-study and difference-in-differences estimates using student-level data. To assess the effects of treatment on student outcomes, I use the following estimating equations:

$$Y_{ist} = \sum_{\substack{k=-3 \\ k \neq -1}}^{3} \omega_k \times Treat_s \times \mathbf{1}\{t - T_s^* = k\} + X_{it}\gamma + \alpha_s + \phi_t + \varepsilon_{ist} \tag{2a}$$

$$Y_{ist} = \alpha + \theta \times Treat_{st} + X_{it}\gamma + \alpha_s + \phi_t + \varepsilon_{ist} \tag{2b}$$

In Equation (2a), Y_{ist} is an absence-related outcome for student i enrolled in school s in year t, such as the number of days absent or an indicator for chronic absenteeism. $Treat_s$ is a binary variable that indicates whether school s was ever treated, and T_s^* is the school year in which treatment was introduced (i.e., 2006/07). The sequence of ω_k coefficients traces out the time path of relative differences in average outcomes between treated and comparison schools (k varies from -3 to $+3$, with $k = -1$ omitted). As above, event time maps directly onto school years. X_{it} is a vector of student-level characteristics, including gender, race/ethnicity, and economic disadvantage. α_s and ϕ_t denote school and year fixed effects, respectively, which absorb time-invariant school characteristics and common shocks across schools. ε_{ist} is an error term assumed to be uncorrelated with other determinants of student outcomes. In Equation (2b), I again replace the event-time interactions with treatment with a single indicator, $Treat_{st}$, equal to one in treated schools in all years following the introduction of CFST. The coefficient θ captures the difference-in-differences estimate of the effect of treatment on student outcomes. Standard errors are heteroskedasticity-robust and clustered at the school level.

I estimate each model separately for two samples to distinguish between direct and indirect exposure to CFST. The first sample compares directly treated schools to comparison group schools, while the second compares indirectly treated schools to the same comparison group schools. For models estimated on the indirect exposure sample (i.e., indirectly treated schools versus comparison group), all treatment indicators (i.e., $Treat_s$ and $Treat_{st}$) are replaced with their indirect treatment analogues.

The main source of identifying variation in this paper comes from the introduction of CFST teams in directly treated schools. These schools received two-person teams as part of the CFST program, which represented large and plausibly exogenous shocks to school-based support staffing levels. Estimated effects for students in directly treated schools are identified by comparing directly treated schools to all other schools in districts that applied for CFST funding but did not receive a team. Because school districts selected which schools would receive CFST at the time of the application, the comparison group includes both schools in the district nomination set and schools outside of it (i.e., the nomination set is unobserved in non-recipient districts). A potential concern is that selection into the nomination set may have been correlated with time-varying shocks to student outcomes. Identification thus relies on the assumption that within-district nomination decisions were based on pre-determined school characteristics rather than contemporaneous changes. I assess the plausibility of this assumption using an event-study that tests for differential pre-trends between directly treated schools and comparison group schools. As an additional robustness check, I also construct a nearest-neighbor matched comparison group designed to more closely approximate the set of nominated schools and show that results are robust to this alternative.

In contrast, evidence from indirectly treated schools should be interpreted more cautiously. Although these schools are separately compared to the same set of comparison group schools (i.e., schools in districts that applied for but did not receive any CFST teams), changes in school-based support staffing levels in indirectly treated schools reflect district-level responses to CFST, including unobserved decisions to reallocate existing staff time and adjust staffing through hiring. As a result, estimated effects for students in indirectly treated schools capture the reduced-form impact of broader, and potentially endogenous, changes in school-based support staffing within affected districts. For these indirectly treated schools, I examine the plausibility of the parallel trends assumption by testing for differential pre-trends using an event study and present results using a nearest-neighbor matched comparison group. The identifying assumptions are weaker, and thus the estimates should be interpreted as supportive of the main findings rather than causal.

5 Results

5.1 School Staffing Levels

An event-study plot depicting staffing levels for social workers in directly treated schools appears in Panel (a) of Figure 2. This plot provides visual evidence of flat pre-trends followed by an immediate and constant increase in social worker staffing levels at time $t = 0$ (i.e., the 2006/07 school year). A corresponding difference-in-differences estimate is reported in Panel A of Table 1. The result in Column (1) indicates an overall increase of 0.63 FTEs in social worker staffing levels in directly treated schools, which is 227% relative to the baseline of 0.28. The second and third rows of the table disaggregate this staffing increase by funding source. The net increase of 0.63 FTEs was the result of the combination of an increase in CFST-funded social worker positions (+0.79 FTEs) and a decrease in social worker positions funded by other local, state, and federal sources (−0.16 FTEs) (see Panel (a) of Appendix Figure A1 for event-study estimates by funding source), as directly treated schools reallocated social worker time to other schools in the district (i.e., to indirectly treated schools). The CFST-funded position estimate is less than one because of partial-year vacancies and unfilled positions in some directly treated schools. Nonetheless, the social worker staffing level increases induced by CFST are large and persistent.

Social worker staffing levels also increased in indirectly treated schools, as social worker time devoted to directly treated schools was reallocated to indirectly treated schools (i.e., other schools in the district). An event-study plot for social workers in indirectly treated schools appears in Panel (b) of Figure 2 and shows flat pre-trends followed again by an immediate increase—albeit much smaller than in directly treated schools. The corresponding difference-in-differences estimate in Table 1 indicates an increase of 0.12 FTEs, which translates into a 53% increase relative to the baseline of 0.23. Although smaller than the increase in directly treated schools, this increase reflects a combination of within-district reallocation of social workers and new hires in indirectly treated schools.

The introduction of the CFST program also increased school nurse staffing levels in directly treated schools. An event-study plot in Panel (c) of Figure 2 shows flat pre-trends followed by an immediate increase. The difference-in-differences estimate (Panel (B) of Table 1, Column 2) indicates a net increase of 0.34 FTEs, a 279% increase relative to the baseline of 0.12. Once again this is driven by an increase in CFST-funded positions (+0.46 FTEs) and partially offset by a decrease in other-funded positions (−0.12 FTEs) (see Panel (b) of Appendix Figure A1 for event-study estimates by funding source). In the case of school nurses, the CFST-funded estimate being below one not only reflects partial-year vacancies or unfilled positions but also the common practice that school nurse positions were funded by local public health departments (and thus do not appear in the administrative staff data) (North Carolina Child and Family Leadership Council, 2006).

School nurse staffing levels also increased in indirectly treated schools, reflecting reallocation and district-driven staffing when no CFST-funded positions were assigned to the school. Panel (d) of Figure 2 shows an event-study plot with flat pre-trends followed by a large increase. Difference-in-differences estimates in Panel (B) of Table 1 indicate a net increase of 0.05 FTEs, a 47% increase relative to the baseline of 0.11. Although smaller than the increase in directly treated schools, this change again reflects both within-district reallocation and new hires.

To bolster the causal interpretation of my findings for staffing levels in directly treated schools, I present results from falsification, robustness, and placebo tests. Falsification tests examine staffing levels for two school-based staff positions unaffected by CFST: guidance counselors and school psychologists. Event-study plots in Panels (e)–(h) of Figure 2 provide visual evidence, and Columns (3) and (4) in Table 1 present difference-in-differences estimates. The event-study coefficients show flat pre-trends followed by null effects. The difference-in-differences estimates are similarly small and insignificant. Appendix Tables A3 and A4 demonstrate robustness by presenting results from models augmented with time-varying school-level covariates and weighted by enrollment; both are very similar to the main findings. Finally, Appendix Table A5 reports results from estimates generated using placebo (fake) treatment years (i.e., the 2004/05 and 2005/06 school years, respectively). The placebo treatment effect estimates are very small compared to the main results.

5.2 Student Outcomes

Panel (a) of Figure 3 presents event-study estimates from Equation (2a) for directly treated schools. Coefficient estimates in pre-CFST school years are close to zero and statistically insignificant, while estimates in post-CFST school years shift downward and suggest reductions in absences. Although individual post-CFST coefficients are not significant, the absence of pre-trends and the post-treatment pattern support the parallel trends assumption and suggest a negative effect on absences. Columns (1)–(3) of Panel A, Table 2 report difference-in-differences estimates from Equation (2b), with results in each column incorporating progressively richer controls and more flexible fixed effects. Estimates for directly treated schools range from approximately -0.31 to -0.39 days absent, although only one estimate is statistically significant at the 0.05 level (one is marginally significant at the 0.10 level). Column (4) reports heterogeneous effects by risk group: for high-risk students, absences decline by 0.41 days (a 6% reduction), while the estimated effect for low-risk students is small (0.14 days) and statistically insignificant (corresponding event-study estimates appear in Appendix Figure A2). I can reject the null of equal effects across high- and low-risk groups ($p<0.001$).

Panel (c) of Figure 3 presents event-study estimates for the binary outcome of chronic absenteeism. The figure again provides evidence in support of the parallel trends assumption: coefficients in the pre-period hover around zero and are insignificant, while those in the post-period move downward and suggest a reduction in the likelihood of chronic absenteeism. Although the year-by-year estimates are imprecise, the lack of pre-trends and the post-treatment pattern suggest program impacts. Columns (5)–(7) of Panel A, Table 2 report difference-in-differences estimates for chronic absenteeism. Point estimates range from around −0.6 to −0.8 percentage points, although only one is statistically significant. Column (8) presents estimates by risk group. For high-risk students, CFST reduces the probability of chronic absenteeism by 0.9 percentage points (16% effect), while the estimated effect for low-risk students is small and statistically insignificant (corresponding event-study estimates again appear in Appendix Figure A2). I can again reject the null of equal effects across groups (p<0.001).

Panel (b) of Figure 3 presents event-study estimates for indirectly treated schools. Although not all coefficient estimates in pre-period years are statistically insignificant, there is no clear pre-trend. In the post-period, coefficients shift downward and suggest a reduction in absences, although the magnitude is much smaller. Columns (1)–(3) of Panel B, Table 2 report difference-in-differences estimates, which range from approximately −0.20 to −0.22 days absent and are all statistically significant. Column (4) reports heterogeneous effects by risk group: absences decline by 0.22 days for high-risk students and by 0.18 days for low-risk students, with both estimates statistically significant (corresponding event-study estimates appear in Appendix Figure A2). I fail to reject the null hypothesis of equal effects across risk groups (p=0.600).

Panel (d) of Figure 3 presents event-study estimates for chronic absenteeism in indirectly treated schools. Pre-treatment coefficients are close to zero, while post-treatment coefficients are slightly negative although imprecise. Columns (5)–(7) of Panel B, Table 2 report difference-in-differences estimates that are small (around −0.3 percentage points) and statistically insignificant. Column (8) shows no meaningful heterogeneity by risk group, with small but insignificant estimates for both high- and low-risk groups (see Appendix Figure A2). I cannot reject the null of equal effects across groups (p=0.909).

5.3 Placebo Tests, Alternative Risk Measure, and Robustness Checks

To strengthen the causal interpretation of the main findings from directly treated schools, I report results from several additional analyses and robustness checks. First, I report results from placebo treatment estimates based on false CFST implementation years. These estimates are small and statistically insignificant (Appendix Table A6) and thus support the interpretation that the main results reflect the impact of CFST.

Second, as a check on the results by risk group, Appendix Tables A7 and A8 present heterogeneous treatment effect estimates by observable student characteristics, including gender, economic disadvantage, and race/ethnicity. Although these characteristics were included in the model predicting student risk of chronic absenteeism, I present them separately here for transparency and note that most of the patterns are similar. A key finding is that there is evidence of heterogeneous treatment effects in directly treated schools but not in indirectly treated schools. In directly treated schools, reductions in absences and chronic absenteeism are concentrated among girls, economically disadvantaged students, and students of color. The stronger effects for economically disadvantaged students and students of color are consistent with the concentration of treatment effects among high-risk students, as both groups are overrepresented in the high-risk group (see summary statistics by risk group in Appendix Table A9). By contrast, treatment effect estimates in indirectly treated schools are generally smaller in magnitude and show no clear evidence of heterogeneity across observable student characteristics.

Third, I re-estimate the main models using an alternative version of student-level predicted risk of chronic absenteeism based on out-of-sample prediction (see Appendix B). This yields qualitatively similar findings (see Appendix Table A10).

Finally, to address concerns that the comparison group includes schools outside the original nomination set, I re-estimate the main results using refined comparison groups constructed via nearest-neighbor matching. Specifically, I separately match directly and indirectly treated schools to comparison schools using propensity scores and select the three nearest neighbors for each school. Estimates based on these matched comparison groups (which differ for directly and indirectly treated schools) are qualitatively similar to the main findings and are reported in Appendix Table A11.

5.4 Discussion

Comparing estimated effects across directly and indirectly treated schools reveals important differences in both magnitude and distribution. In directly treated schools, reductions in absences and chronic absenteeism are larger but are concentrated among high-risk students. This pattern of findings is consistent with the mission of the CFST program, which explicitly instructed the social worker and school nurse teams to identify and intervene with students who faced the highest risk of negative academic outcomes. The 12% reduction in chronic absenteeism among high-risk students in directly treated schools is comparable in relative terms to impacts from lower-cost, information-based interventions that similarly target high-absence students (e.g. Rogers and Feller (2018) and Heppen et al. (2020)). While substantially more costly than information interventions, the intensive, team-based CFST model is designed for students facing complex, co-occurring health, social, and family challenges and for whom information alone would be unlikely to be effective. In contrast, estimated effects from indirectly treated schools are smaller but more diffuse, with reductions in absences present for both high- and low-risk students. Because staffing changes in indirectly treated schools reflect district-level responses to CFST, these estimates may be subject to endogeneity concerns and should be interpreted with caution. Nonetheless, the pattern and magnitudes of effects in indirectly treated schools provide suggestive evidence broadly consistent with the main findings from directly treated schools.

Although I do not observe measures of services provided to students by school-based support staff, potential mechanisms underlying these findings operate through the roles of school nurses and social workers. School nurses may reduce absences by identifying and assisting students (and parents) in the management of both acute and chronic health conditions. This could prevent missed school days altogether or facilitate earlier return to school. At the same time, social workers may reduce absences through mental health and social channels by supporting students and families in addressing specific mental health conditions (e.g., anxiety), family issues, and other factors that cause school absences. The estimated impact on days absent is about half the magnitude estimated for school-based telemedicine clinics (Komisarow and Hemelt, 2024), which is plausible given that telemedicine intervenes through the direct provision of healthcare, while CFST primarily operates through coordination, referral, and case management rather than direct care. Coordinated care and intervention from a two-person team may be particularly beneficial for high-risk students facing multiple barriers, which is consistent with the larger and more concentrated effects observed in directly treated schools. Qualitative and descriptive evidence from CFST reports is consistent with this interpretation and with these potential mechanisms. Absences and health-related concerns were consistently identified as unmet student needs (North Carolina Child and Family Leadership Council, 2010), and many students were documented to have multiple, co-occurring needs (North Carolina Child and Family Leadership Council, 2009).

6 Conclusion

Chronic absenteeism continues to pose a significant challenge for K–12 public schools. Despite recent improvements, rates of chronic absenteeism remain substantially higher than pre-pandemic levels and affect a large number of students. Against this contemporary backdrop, this paper examines the impact of expanding school-based support staff through the introduction of the CFST program. Leveraging both the direct placement of school nurse and social worker teams and indirect changes in staffing across schools, this paper provides causal evidence on how increased access to school-based support staff influences absence-related student outcomes and chronic absenteeism.

A limitation of this paper is that the CFST program was implemented as a bundled intervention, which prevents disentangling the individual contributions of school nurses and social workers. Accordingly, the estimates should be interpreted as the combined effect of increased access to school-based support staff rather than the impact of any single component.

Overall, the findings point to the importance of addressing students' health, social, and family-related needs as a means to decrease chronic absenteeism. Intensive support programs appear particularly effective for high-risk students, while broader staffing increases may yield smaller but more widespread improvements. The results also highlight the role of K–12 schools in supporting student well-being.

Tables and Figures

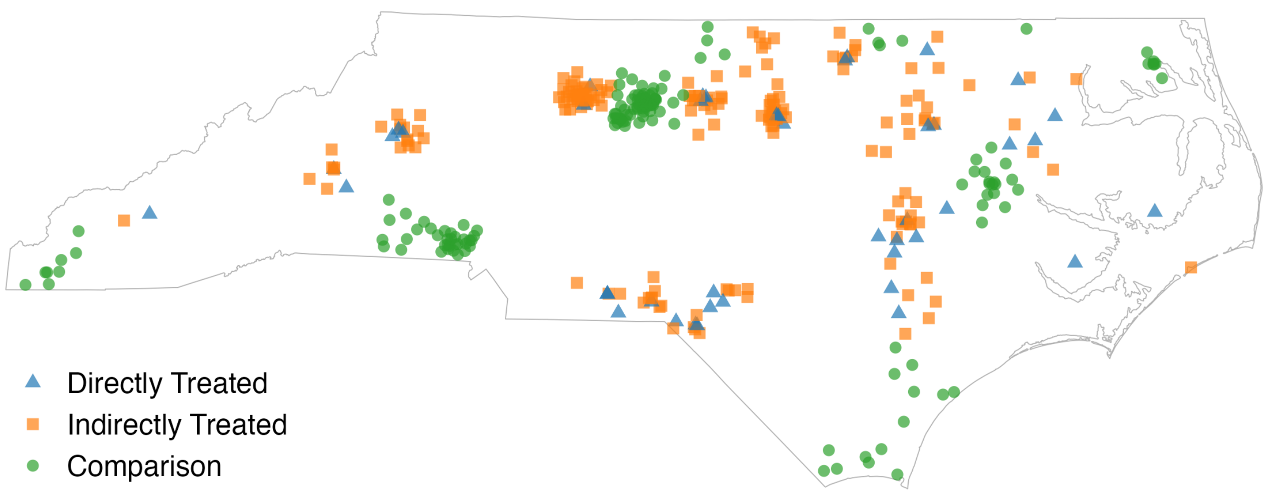
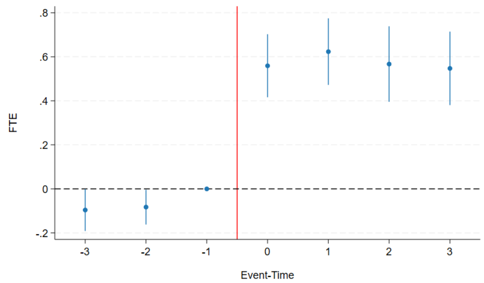
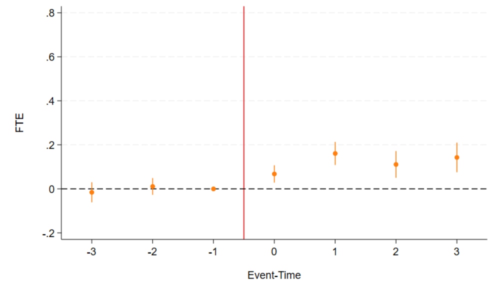


Figure 1: Directly Treated, Indirectly Treated, and Comparison Schools in North Carolina

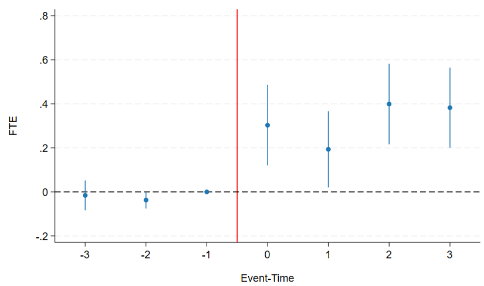
Notes: This figure shows the geographic distribution of schools. Triangles indicate directly treated schools, squares indicate indirectly treated schools, and circles indicate comparison group schools. Each marker corresponds to a school's location based on latitude and longitude coordinates.



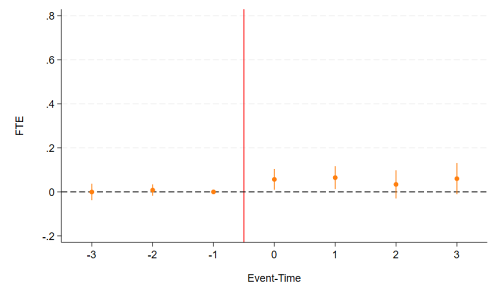
(a) Directly Treated: Social Workers



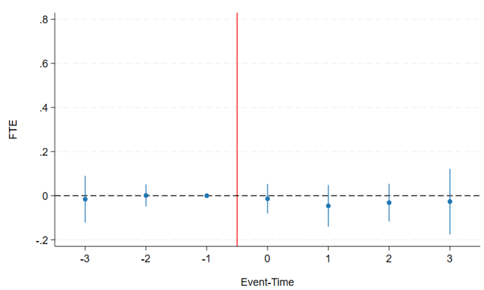
(b) Indirectly Treated: Social Workers



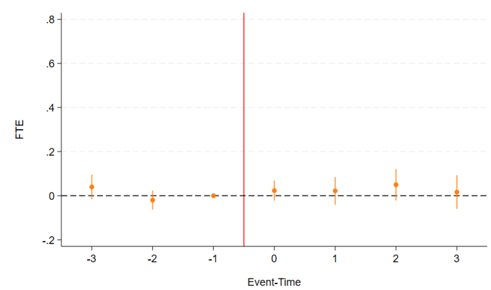
(c) Directly Treated: School Nurses



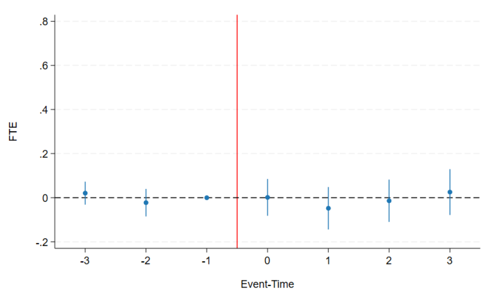
(d) Indirectly Treated: School Nurses



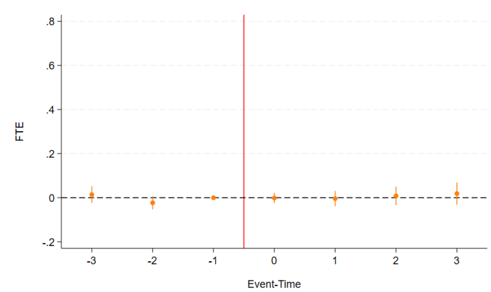
(e) Directly Treated: Guidance Counselors (Falsification)



(f) Indirectly Treated: Guidance Counselors (Falsification)



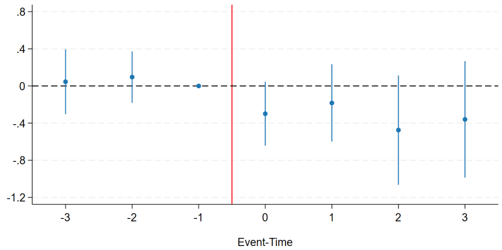
(g) Directly Treated: School Psychologists (Falsification)



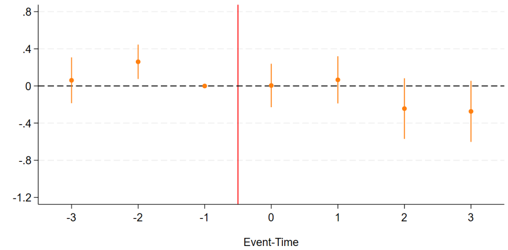
(h) Indirectly Treated: School Psychologists (Falsification)

Figure 2: Event-Study Estimates: School Staffing Levels

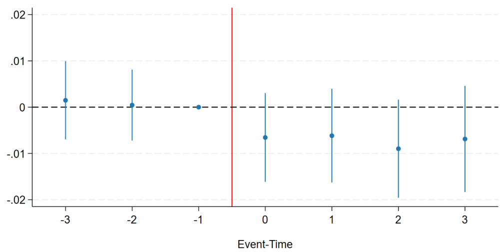
Notes: Panels (a)–(h) depict event-study estimates (circles) and associated 95% confidence intervals (vertical bars) obtained using Equation (1a). The dependent variable in each panel is measured in the unit of full-time equivalents (FTE) at the school-level. Event-time zero corresponds to the 2006/07 school year. Heteroskedasticity-robust standard errors are clustered at the school level.



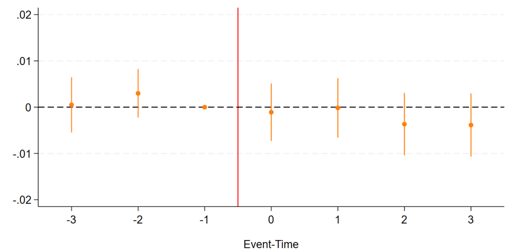
(a) Directly Treated: Days Absent



(b) Indirectly Treated: Days Absent



(c) Directly Treated: Chronic Absenteeism



(d) Indirectly Treated: Chronic Absenteeism

Figure 3: Event-Study Estimates: Student Outcomes

Notes: Panels (a)–(d) depict event-study estimates (circles) and associated 95% confidence intervals (vertical bars) obtained using Equation (2a). The dependent variable in Panels (a) and (b) is the number of days absent. The dependent variable in Panels (c) and (d) is a binary indicator for chronic absenteeism. Event-time zero corresponds to the 2006/07 school year. Heteroskedasticity-robust standard errors are clustered at the school level.

Table 1: Difference-in-Differences Estimates: School Staffing Levels

			Other Staff	
	(1) Social Workers	(2) School Nurses	(3) Guidance Counselors	(4) School Psychologists
Panel A. Directly Treated Schools				
Outcome = All Funded Positions (FTE)	0.634***	0.337***	-0.025	-0.008
	(0.080)	(0.082)	(0.045)	(0.037)
Obs.	1,379	1,379	1,379	1,379
Baseline Mean	0.279	0.121	1.094	0.166
Outcome = CFST Funded Positions (FTE)	0.791***	0.460***		
	(0.056)	(0.067)		
Obs.	1,379	1,379		
Baseline Mean	0.000	0.000		
Outcome = Fed/State/Local-Funded Positions (FTE)	-0.157**	-0.117***		
	(0.062)	(0.045)		
Obs.	1,379	1,379		
Baseline Mean	0.279	0.121		
Panel B. Indirectly Treated Schools				
Outcome = All Funded Positions (FTE)	0.122***	0.051**	0.021	0.008
	(0.023)	(0.025)	(0.028)	(0.015)
Obs.	2,303	2,303	2,303	2,303
Baseline Mean	0.232	0.109	1.117	0.211
School FE	X	X	X	X
Year FE	X	X	X	X

Notes: Each coefficient estimate was obtained from a separate regression using Equation (1b). The dependent variable in each regression is measured in the unit of full-time equivalents (FTE) at the school-level. The sample in Panel A includes directly treated and comparison group schools between the 2003/04 and 2009/10 school years. The sample in Panel B includes indirectly treated and comparison group schools between the 2003/04 and 2009/10 school years. Panel A includes three outcome rows: all funded positions; CFST funded positions; and federal, state, and local funded positions. Panel B includes only one outcome row (all funded positions) because funding disaggregation is not relevant for indirectly treated schools. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Difference-in-Differences Estimates: Student Outcomes

	Days Absent				Chronic. Abs. (0/1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Directly Treated Schools								
Treat × Post	-0.376**	-0.297	-0.300*		-0.008**	-0.006	-0.006	
	(0.178)	(0.180)	(0.180)		(0.004)	(0.004)	(0.004)	
Treat × Post × High Risk				-0.409**				-0.009**
				(0.181)				(0.004)
Treat × Post × Low Risk				0.142				0.006
				(0.221)				(0.005)
Observations	328,765	328,765	328,765	328,765	328,765	328,765	328,765	328,765
Baseline Mean	6.766	6.766	6.766	6.766	0.056	0.056	0.056	0.056
p-value: High = Low				0.000				0.000
Panel B. Indirectly Treated Schools								
Treat × Post	-0.219**	-0.205**	-0.203**		-0.003	-0.003	-0.003	
	(0.097)	(0.095)	(0.095)		(0.002)	(0.002)	(0.002)	
Treat × Post × High Risk				-0.222**				-0.003
				(0.112)				(0.002)
Treat × Post × Low Risk				-0.184**				-0.003
				(0.091)				(0.002)
Observations	542,263	542,263	542,263	542,263	542,263	542,263	542,263	542,263
Baseline Mean	6.479	6.479	6.479	6.479	0.047	0.047	0.047	0.047
p-value: High = Low				0.600				0.909
School FE	X	X	X	X	X	X	X	X
Student Covariates		X	X	X		X	X	X
Year FE	X	X			X	X		
Grade FE		X				X		
Grade X Year FE			X	X			X	X

Notes: Each coefficient was obtained from a separate regression using Equation (2b). The dependent variable for absences is measured in days and chronic absenteeism is binary (0/1). The sample in Panel A includes directly treated and comparison group students between the 2003/04 and 2009/10 school years. The sample in Panel B includes indirectly treated and comparison group students between the 2003/04 and 2009/10 school years. Columns (4) and (8) present results interacting treatment with student risk group (high and low). The p-values report results from tests of equality of effects across risk groups. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * p <0.10, ** $p < 0.05$, *** $p < 0.01$.

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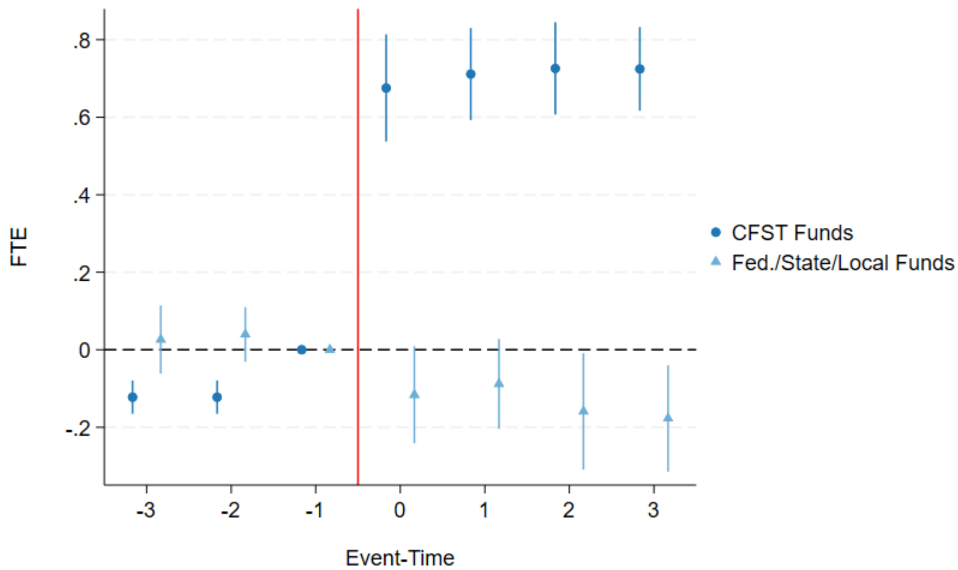
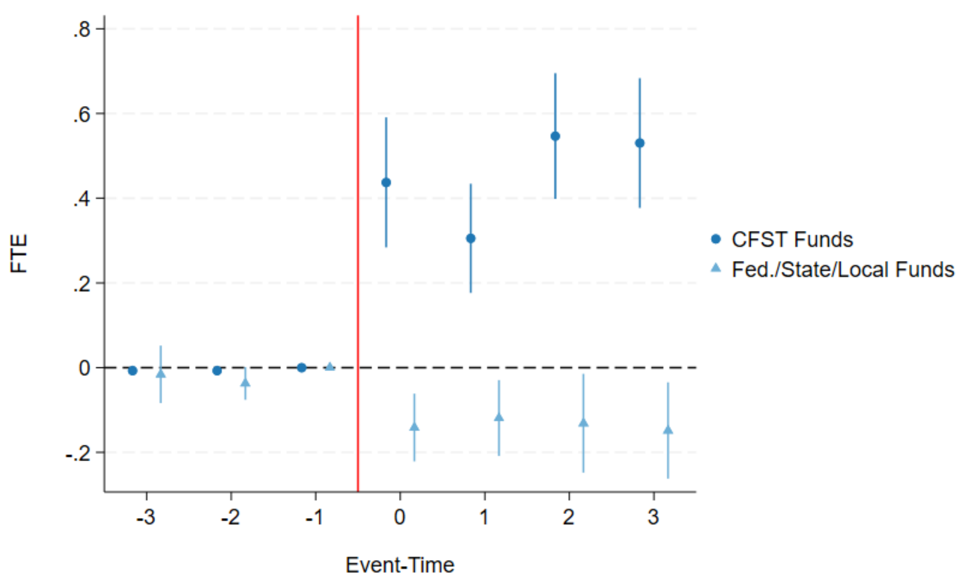
Appendix A: Supplemental Results



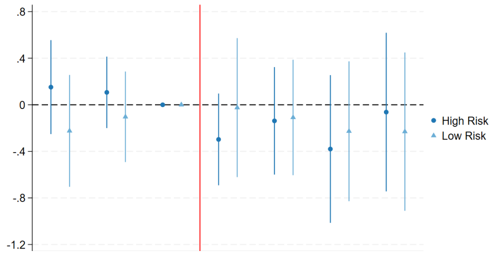
(a) Social Workers (FTE)



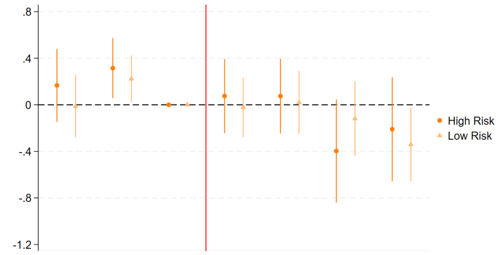
(b) School Nurses (FTE)

Figure A1: Event-Study Estimates, School Staff (FTE)

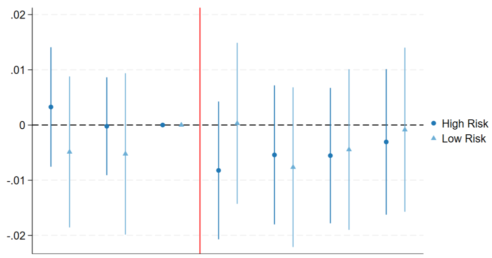
Notes: Panels (a) and (b) depict event-study estimates (circles for CFST funds; triangles for federal, state, and local funds) and associated 95% confidence intervals (vertical bars) obtained using Equation (1a). The dependent variable in each panel is measured in the units of full-time equivalents (FTE) at the school-level. The sample includes directly treated and comparison group schools between the 2003/04 and 2009/10 school years. Event-time zero corresponds to the 2006/07 school year (i.e., the first school year during which CFST was introduced). Heteroskedasticity-robust standard errors are clustered at the school level.



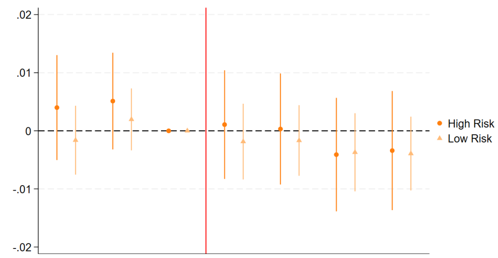
(a) Directly Treated: Days Absent



(b) Indirectly Treated: Days Absent



(c) Directly Treated: Chronic Absenteeism



(d) Indirectly Treated: Chronic Absenteeism

Figure A2: Event-Study Estimates: Student Outcomes, Separately by Risk Group

Notes: Panels (a)–(d) depict event-study estimates (circles for high-risk students; triangles for low-risk students) and associated 95% confidence intervals (vertical bars) obtained using Equation (2a). The dependent variable in Panels (a) and (b) is the number of days absent. The dependent variable in Panels (c) and (d) is a binary indicator for chronic absenteeism. Event-time zero corresponds to the 2006/07 school year. Heteroskedasticity-robust standard errors are clustered at the school level.

Table A1: Characteristics of School Districts Invited to Apply for CFST Program

		Status		Sample		Differences	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Invite	No Invite	Treat.	Comp.	Invite - No	Treat. - Comp.
Panel A. District Characteristics							
Urban Locale (0/1)	0.200	0.242	0.183	0.286	0.167	0.059	0.119
						(0.087)	(0.151)
Number of Schools	18.7	20.3	18.1	18.5	23.6	2.2	-5.1
						(4.4)	(9.0)
Population	73,618	78,283	71,741	68,866	94,762	6,542	-25,896
						(21,126)	(39,026)
Panel B. Demographics and Enrollment							
Enrollment	11,561	12,042	11,367	10,892	14,052	674	-3,160
						(3,185)	(5,929)
Share Children in Poverty	0.188	0.206	0.181	0.208	0.204	0.025***	0.004
						(0.009)	(0.013)
Share Amer. Ind./Al. Nat.	0.014	0.024	0.011	0.028	0.016	0.013	0.012
						(0.010)	(0.015)
Share Asian	0.012	0.007	0.014	0.007	0.007	-0.006**	0.000
						(0.003)	(0.004)
Share Black	0.292	0.410	0.244	0.435	0.365	0.166***	0.070
						(0.045)	(0.084)
Share Hispanic	0.058	0.047	0.063	0.058	0.028	-0.015	0.030**
						(0.010)	(0.012)
Share White	0.610	0.494	0.657	0.454	0.565	-0.162***	-0.111
						(0.045)	(0.081)
Share Spec. Ed.	0.147	0.151	0.146	0.154	0.144	0.005	0.010
						(0.005)	(0.007)
Share Eng. Lang. Learner	0.044	0.035	0.048	0.043	0.022	-0.013	0.020*
						(0.008)	(0.011)
Obs.	115	33	82	21	12	115	33

Notes: District-level data for the 2003/04 school year from the Common Core of Data (CCD) and the Small Area Income and Poverty Estimates (SAIPE) were accessed using the Urban Institute Education Data Portal. Column (1) presents mean estimates for all school districts in North Carolina. Columns (2) and (3) partition school districts into those invited to apply for CFST and those that were not invited. Columns (4) and (5) present mean estimates for treatment (invited, applied, and selected) and comparison (invited, applied, but not selected) school districts. Column (6) presents differences in means (and associated standard errors) between Columns (2) and (3). Column (7) presents differences in means (and associated standard errors) between Columns (4) and (5). Asterisks indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Summary Statistics for Student Characteristics and Outcomes at Baseline

	(1) Full Sample	(2) Directly Treated	(3) Indirectly Treated	(4) Comparison	(5) Diff.: (2)-(4)	(6) Diff.: (3)-(4)	(7) Diff.: (2)-(3)
Panel A. Student Characteristics							
Female (0/1)	0.488	0.490	0.490	0.485	0.005	0.005*	-0.000
					(0.004)	(0.003)	(0.004)
Black (0/1)	0.376	0.525	0.361	0.350	0.175***	0.011	0.163***
					(0.048)	(0.029)	(0.048)
Hispanic (0/1)	0.085	0.132	0.098	0.058	0.074***	0.041***	0.033
					(0.021)	(0.009)	(0.022)
Other Race/Eth. (0/1)	0.056	0.061	0.052	0.060	0.001	-0.008	0.009
					(0.017)	(0.005)	(0.016)
White (0/1)	0.483	0.283	0.488	0.532	-0.249***	-0.044	-0.205***
					(0.044)	(0.032)	(0.044)
Economic Disadvantage (0/1)	0.545	0.770	0.515	0.514	0.256***	0.001	0.255***
					(0.027)	(0.026)	(0.026)
Grade Level	4.005	3.970	4.017	4.002	-0.032	0.014	-0.047**
					(0.021)	(0.012)	(0.023)
Chron. Abs. Risk	0.047	0.057	0.044	0.046	0.010***	-0.002	0.012***
					(0.001)	(0.001)	(0.001)
Panel B. Student Outcomes							
Days Absent	6.456	6.766	6.479	6.345	0.421**	0.134	0.287*
					(0.168)	(0.119)	(0.155)
Chronically Absent (0/1)	0.047	0.056	0.047	0.043	0.012***	0.004	0.008**
					(0.004)	(0.002)	(0.004)
Observations	259664	30614	119114	109936	140550	229050	149728

Notes: The sample is restricted to third through fifth grade students enrolled in directly treated, indirectly treated, and comparison group schools during the 2003/04 to 2005/06 school years (i.e., pre-CFST school years). Column (1) reports means for the full sample. Column (2) reports means for students in directly treated schools. Column (3) reports means for students in indirectly treated schools. Column (4) reports means for students in comparison group schools. Column (5) reports the difference in means between directly treated and comparison group students (and associated standard errors). Column (6) reports the difference in means between indirectly treated and comparison group students (and associated standard errors). Column (7) reports the difference in means between directly treated and indirectly treated students (and associated standard errors). Asterisks indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Difference-in-Differences Estimates: School Staffing Levels (with School-Level Controls)

			Other Staff	
	(1) Social Workers	(2) School Nurses	(3) Guidance Counselors	(4) School Psychologists
Panel A. Directly Treated Schools				
Outcome = All Funded Positions (FTE)	0.635***	0.347***	-0.026	-0.003
	(0.082)	(0.082)	(0.046)	(0.038)
Obs.	1,379	1,379	1,379	1,379
Baseline Mean	0.279	0.121	1.094	0.166
Outcome = CFST Funded Positions (FTE)	0.785***	0.459***		
	(0.058)	(0.068)		
Obs.	1,379	1,379		
Baseline Mean	0.000	0.000		
Outcome = Fed/State/Local-Funded Positions (FTE)	-0.150**	-0.108**		
	(0.059)	(0.043)		
Obs.	1,379	1,379		
Baseline Mean	0.279	0.121		
Panel B. Indirectly Treated Schools				
Outcome = All Funded Positions (FTE)	0.115***	0.054**	0.016	0.009
	(0.023)	(0.026)	(0.028)	(0.016)
Obs.	2,303	2,303	2,303	2,303
Baseline Mean	0.232	0.109	1.117	0.211
School FE	X	X	X	X
Year FE	X	X	X	X

Notes: Each coefficient estimate was obtained from a separate regression using Equation (1b). The dependent variable in each regression is measured in the unit of full-time equivalents (FTE) at the school-level. The sample in Panel A includes directly treated and comparison group schools between the 2003/04 and 2009/10 school years. The sample in Panel B includes indirectly treated and comparison group schools between the 2003/04 and 2009/10 school years. School-level controls include proportion Black, proportion Hispanic, proportion other race/ethnicity, and the natural logarithm of enrollment. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * $p<0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Difference-in-Differences Estimates: School Staffing Levels (Weighted by Enrollment)

			Other Staff	
	(1) Social Workers	(2) School Nurses	(3) Guidance Counselors	(4) School Psychologists
Panel A. Directly Treated Schools				
Outcome = All Funded Positions (FTE)	0.702***	0.343***	-0.031	-0.005
	(0.086)	(0.079)	(0.054)	(0.041)
Obs.	1,379	1,379	1,379	1,379
Baseline Mean	0.279	0.121	1.094	0.166
Outcome = CFST Funded Positions (FTE)	0.812***	0.445***		
	(0.058)	(0.076)		
Obs.	1,379	1,379		
Baseline Mean	0.000	0.000		
Outcome = Fed/State/Local-Funded Positions (FTE)	-0.110*	-0.097*		
	(0.064)	(0.053)		
Obs.	1,379	1,379		
Baseline Mean	0.279	0.121		
Panel B. Indirectly Treated Schools				
Outcome = All Funded Positions (FTE)	0.109***	0.044*	0.024	0.014
	(0.022)	(0.027)	(0.037)	(0.017)
Obs.	2,303	2,303	2,303	2,303
Baseline Mean	0.232	0.109	1.117	0.211
School FE	X	X	X	X
Year FE	X	X	X	X

Notes: Each coefficient estimate was obtained from a separate regression using Equation (1b). The dependent variable in each regression is measured in the unit of full-time equivalents (FTE) at the school-level. The sample in Panel A includes directly treated and comparison group schools between the 2003/04 and 2009/10 school years. The sample in Panel B includes indirectly treated and comparison group schools between the 2003/04 and 2009/10 school years. All regressions are weighted by school-level enrollment. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * p <0.10, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Difference-in-Differences Estimates with Placebo Treatment Years: School Staffing Levels

	Placebo Year=2005		Placebo Year=2006	
	(1) Social Workers	(2) School Nurses	(3) Social Workers	(4) School Nurses
Panel A. Directly Treated Schools				
Treat × Post (Placebo)	0.055	-0.003	0.089**	0.027
	(0.037)	(0.027)	(0.041)	(0.026)
Obs.	591	591	591	591
Baseline Mean	0.279	0.121	0.279	0.121
Panel B. Indirectly Treated Schools				
Treat × Post (Placebo)	0.021	0.005	0.002	-0.004
	(0.018)	(0.016)	(0.019)	(0.014)
Obs.	987	987	987	987
Baseline Mean	0.232	0.109	0.232	0.109
School FE	X	X	X	X
Year FE	X	X	X	X

Notes: Each coefficient estimate was obtained from a separate regression using Equation (1b). The dependent variable in each regression is measured in the unit of full-time equivalents (FTE) at the school-level. The sample in Panel A includes directly treated and comparison group schools between the 2003/04 and 2005/06 school years. The sample in Panel B includes indirectly treated and comparison group schools between the 2003/04 and 2005/06 school years. Placebo "treatment" years are indicated in the column headings. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * p <0.10, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Difference-in-Differences Estimates with Placebo Treatment Years: Student Outcomes

	Days Absent				Chronic. Abs. (0/1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Directly Treated, Placebo Year = 2005								
Treat × Post (Placebo)	-0.016	-0.007	-0.007		-0.002	-0.001	-0.001	
	(0.158)	(0.159)	(0.159)		(0.004)	(0.004)	(0.004)	
Treat × Post (Placebo) × High Risk				-0.027				-0.002
				(0.164)				(0.004)
Treat × Post (Placebo) × Low Risk				0.074				0.001
				(0.180)				(0.005)
Observations	140,550	140,550	140,550	140,550	140,550	140,550	140,550	140,550
p-value: High=Low				0.422				0.507
Panel B. Directly Treated, Placebo Year = 2006								
Treat × Post (Placebo)	-0.083	-0.066	-0.066		-0.001	-0.001	-0.001	
	(0.135)	(0.130)	(0.130)		(0.003)	(0.003)	(0.003)	
Treat × Post (Placebo) × High Risk				-0.118				-0.002
				(0.138)				(0.004)
Treat × Post (Placebo) × Low Risk				0.128				0.006
				(0.178)				(0.006)
Observations	140,550	140,550	140,550	140,550	140,550	140,550	140,550	140,550
p-value: High=Low				0.132				0.241
Panel C. Indirectly Treated, Placebo Year = 2005								
Treat × Post (Placebo)	0.066	0.066	0.066		0.001	0.001	0.001	
	(0.102)	(0.101)	(0.101)		(0.002)	(0.002)	(0.002)	
Treat × Post (Placebo) × High Risk				0.185				0.003
				(0.113)				(0.003)
Treat × Post (Placebo) × Low Risk				-0.032				-0.001
				(0.102)				(0.002)
Observations	229,050	229,050	229,050	229,050	229,050	229,050	229,050	229,050
p-value: High=Low				0.003				0.062
Panel D. Indirectly Treated, Placebo Year = 2006								
Treat × Post (Placebo)	-0.166*	-0.176*	-0.176*		-0.002	-0.002	-0.002	
	(0.098)	(0.099)	(0.098)		(0.003)	(0.003)	(0.003)	
Treat × Post (Placebo) × High Risk				-0.132				-0.002
				(0.119)				(0.003)
Treat × Post (Placebo) X Low Risk				-0.211**				-0.002
				(0.098)				(0.002)
Observations	229,050	229,050	229,050	229,050	229,050	229,050	229,050	229,050
p-value: High=Low				0.365				0.760
School FE	X	X	X	X	X	X	X	X
Student Covariates		X	X	X		X	X	X
Year FE	X	X			X	X		
Grade FE		X				X		
Grade X Year FE			X	X			X	X

Notes: Each coefficient was obtained from a separate regression using Equation (2b). The dependent variable for absences is measured in days and chronic absenteeism is binary (0/1). The sample in Panel A includes directly treated and comparison group students between the 2003/04 and 2005/06 school years using the 2004/05 school year as the placebo “treatment” year. The sample in Panel B includes directly treated and comparison group students between the 2003/04 and 2005/06 school years using the 2005/06 school year as the placebo “treatment” year. The sample in Panel C includes indirectly treated and comparison group students between the 2003/04 and 2005/06 school years using the 2004/05 school year as the placebo “treatment” year. The sample in Panel D includes indirectly treated and comparison group students between the 2003/04 and 2005/06 school years using the 2005/06 school year as the placebo “treatment” year. Asterisks indicate statistical significance: * p <0.10, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Difference-in-Differences Estimates in Directly Treated Schools: Heterogeneity by Student Characteristics

	Gender			Economic Disadvantage			Race/Ethnicity		
	Female	Male	p-value	ED	Not ED	p-value	White	Nonwhite	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Days Absent	-0.417**	-0.196	0.042	-0.443**	0.167	0.000	-0.149	-0.359*	0.294
	(0.188)	(0.190)		(0.184)	(0.216)		(0.221)	(0.195)	
N	160,484	168,281		193,050	135,715		152,955	175,810	
Baseline Mean	6.273	6.655		7.004	5.763		7.014	5.965	
Panel B. Chron. Abs. (0/1)	-0.009**	-0.004	0.084	-0.010**	0.006	0.000	-0.003	-0.007*	0.329
	(0.004)	(0.004)		(0.004)	(0.005)		(0.006)	(0.004)	
N	160,484	168,281		193,050	135,715		152,955	175,810	
Baseline Mean	0.041	0.052		0.061	0.027		0.050	0.043	

Notes: Each coefficient was obtained from a separate regression using Equation (2b). The dependent variable for absences is measured in days and chronic absenteeism is binary (0/1). The sample includes directly treated and comparison group students between the 2003/04 and 2009/10 school years. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Difference-in-Differences Estimates in Indirectly Treated Schools: Heterogeneity by Student Characteristics

	Gender			Economic Disadvantage			Race/Ethnicity		
	Female (1)	Male (2)	p-value (3)	ED (4)	Not ED (5)	p-value (6)	White (7)	Nonwhite (8)	p-value (9)
Panel A. Days Absent	-0.188*	-0.216**	0.574	-0.231**	-0.167*	0.400	-0.190**	-0.213*	0.798
	(0.097)	(0.100)		(0.111)	(0.092)		(0.095)	(0.113)	
N	264,882	277,381		291,020	251,243		267,958	274,305	
Baseline Mean	6.234	6.600		7.056	5.754		6.882	5.936	
Panel B. Chron. Abs. (0/1)	-0.003	-0.003	0.722	-0.004	-0.002	0.396	-0.002	-0.003	0.636
	(0.002)	(0.002)		(0.002)	(0.002)		(0.002)	(0.002)	
N	264,882	277,381		291,020	251,243		267,958	274,305	
Baseline Mean	0.040	0.051		0.062	0.028		0.048	0.042	

Notes: Each coefficient was obtained from a separate regression using Equation (2b). The dependent variable for absences is measured in days and chronic absenteeism is binary (0/1). The sample includes indirectly treated and comparison group students between the 2003/04 and 2009/10 school years. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * p <0.10, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Summary Statistics by Risk Group, Directly Treated, Indirectly Treated, and Comparison Group Students

	Low Risk			High Risk		
	(1) Directly Treated	(2) Indirectly Treated	(3) Comparison	(4) Directly Treated	(5) Indirectly Treated	(6) Comparison
Panel A. Chron. Abs. Risk Generated In-Sample (IS)						
Female (0/1)	0.640	0.565	0.561	0.451	0.399	0.399
Black (0/1)	0.419	0.278	0.270	0.552	0.463	0.440
Hispanic (0/1)	0.100	0.075	0.047	0.140	0.127	0.070
Other Race/Eth. (0/1)	0.087	0.063	0.068	0.054	0.038	0.050
White (0/1)	0.394	0.584	0.615	0.254	0.372	0.440
Economic Disadvantage (0/1)	0.178	0.178	0.155	0.924	0.926	0.917
Grade Level	3.754	3.903	3.899	4.026	4.154	4.118
Chron. Abs. Risk (IS)	0.029	0.029	0.029	0.064	0.064	0.066
Chron. Abs. Risk (OOS)	0.022	0.024	0.025	0.054	0.056	0.058
Days Absent	5.682	5.776	5.593	7.047	7.335	7.189
Chronically Absent (0/1)	0.028	0.030	0.026	0.063	0.068	0.063
Observations	6,303	65,405	58,129	24,311	53,709	51,807
Panel B. Chron. Abs. Risk Generated Out-of-Sample (OOS)						
Female (0/1)	0.555	0.525	0.517	0.465	0.447	0.451
Black (0/1)	0.322	0.231	0.209	0.602	0.521	0.496
Hispanic (0/1)	0.132	0.083	0.038	0.132	0.117	0.079
Other Race/Eth. (0/1)	0.081	0.058	0.064	0.053	0.044	0.055
White (0/1)	0.466	0.628	0.689	0.213	0.318	0.370
Economic Disadvantage (0/1)	0.215	0.133	0.071	0.982	0.980	0.973
Grade Level	3.825	3.929	3.937	4.025	4.123	4.070
Chron. Abs. Risk (IS)	0.035	0.029	0.029	0.065	0.063	0.065
Chron. Abs. Risk (OOS)	0.025	0.024	0.024	0.056	0.056	0.058
Days Absent	5.973	5.819	5.628	7.068	7.283	7.088
Chronically Absent (0/1)	0.034	0.031	0.026	0.064	0.067	0.062
Observations	8,453	65,428	55,960	22,161	53,686	53,976

Notes: The sample is restricted to third through fifth grade students enrolled in directly treated, indirectly treated, and comparison group schools during the 2003/04 to 2005/06 school years (i.e., pre-CFST school years). Panel A uses the "in-sample" predicted risk measure, and Panel B uses the "out-of-sample" predicted risk measure. Columns (1)–(3) presents summary statistics for low-risk students, and Columns (4)–(6) present summary statistics for high-risk students.

Table A10: Difference-in-Differences Estimates: Student Outcomes, Alternative Predicted Risk

	Days Absent				Chronic. Abs. (0/1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Directly Treated Schools								
Treat × Post	-0.376**	-0.278	-0.281		-0.008**	-0.005	-0.006	
	(0.178)	(0.177)	(0.177)		(0.004)	(0.004)	(0.004)	
Treat × Post × High Risk				-0.389**				-0.008*
				(0.182)				(0.004)
Treat × Post × Low Risk				-0.054				-0.001
				(0.202)				(0.004)
Observations	328,765	328,765	328,765	328,765	328,765	328,765	328,765	328,765
Baseline Mean	6.766	6.766	6.766	6.766	0.056	0.056	0.056	0.056
p-value: High = Low				0.018				0.022
Panel B. Indirectly Treated Schools								
Treat × Post	-0.219**	-0.191**	-0.189**		-0.003	-0.003	-0.003	
	(0.097)	(0.094)	(0.094)		(0.002)	(0.002)	(0.002)	
Treat × Post × High Risk				-0.148				-0.002
				(0.111)				(0.003)
Treat × Post × Low Risk				-0.225**				-0.004*
				(0.091)				(0.002)
Observations	542,263	542,263	542,263	542,263	542,263	542,263	542,263	542,263
Baseline Mean	6.479	6.479	6.479	6.479	0.047	0.047	0.047	0.047
p-value: High = Low				0.291				0.222
School FE	X	X	X	X	X	X	X	X
Student Covariates		X	X	X		X	X	X
Year FE	X	X			X	X		
Grade FE		X				X		
Grade X Year FE			X	X			X	X

Notes: Each coefficient was obtained from a separate regression using Equation (2b). The dependent variable for absences is measured in days and chronic absenteeism is binary (0/1). The sample in Panel A includes directly treated and comparison group students between the 2003/04 and 2009/10 school years. The sample in Panel B includes indirectly treated and comparison group students between the 2003/04 and 2009/10 school years. All results were obtained using the "out-of-sample" predicted risk measure. Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * p <0.10, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Difference-in-Differences Estimates: Student Outcomes, Matched Comparison Group (3 Nearest Neighbors)

	Days Absent				Chronic. Abs. (0/1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Directly Treated Schools								
Treat × Post	-0.327*	-0.266	-0.269		-0.007*	-0.006	-0.006	
	(0.185)	(0.188)	(0.188)		(0.004)	(0.004)	(0.004)	
Treat × Post × High Risk				-0.365*				-0.008*
				(0.188)				(0.004)
Treat × Post × Low Risk				0.127				0.006
				(0.226)				(0.005)
Observations	253,405	253,405	253,405	253,405	253,405	253,405	253,405	253,405
Baseline Mean	6.766	6.766	6.766	6.766	0.056	0.056	0.056	0.056
p-value: High = Low				0.001				0.000
Panel B. Indirectly Treated Schools								
Treat × Post	-0.225**	-0.214**	-0.212**		-0.003	-0.003	-0.003	
	(0.104)	(0.102)	(0.102)		(0.002)	(0.002)	(0.002)	
Treat × Post × High Risk				-0.229*				-0.003
				(0.117)				(0.003)
Treat × Post × Low Risk				-0.195**				-0.003
				(0.099)				(0.002)
Observations	494,452	494,452	494,452	494,452	494,452	494,452	494,452	494,452
Baseline Mean	6.479	6.479	6.479	6.479	0.047	0.047	0.047	0.047
p-value: High = Low				0.638				0.869
School FE	X	X	X	X	X	X	X	X
Student Covariates		X	X	X		X	X	X
Year FE	X	X			X	X		
Grade FE		X				X		
Grade X Year FE			X	X			X	X

Notes: Each coefficient was obtained from a separate regression using Equation (2b). The dependent variable for absences is measured in days and chronic absenteeism is binary (0/1). The sample in Panel A includes directly treated and comparison group students between the 2003/04 and 2009/10 school years. The sample in Panel B includes indirectly treated and comparison group students between the 2003/04 and 2009/10 school years. All results were obtained using a matched comparison group (3 nearest neighbors). Heteroskedasticity-robust standard errors are clustered at the school-level. Asterisks indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B: Data Appendix

Data Source

I obtained administrative data from the North Carolina Education Research Data Center (NCERDC) at the Center for Child and Family Policy, Duke University. For more information, see: https://childandfamilypolicy.duke.edu/north-carolina-education-research-data/.

The data contained information on the universe of third through fifth grade students enrolled in public schools in North Carolina between the 2003/04 and 2009/10 school years. Student-level data contained information on enrollment, grade level, demographic characteristics (race/ethnicity, gender, and economic disadvantage), and absences. Staff-level data contained information on position, school/district, full-time equivalent (FTE), and funding source.

Identifying CFST Schools

To document the rollout of Child and Family Support Teams (CFST) across K–12 public schools in North Carolina, I relied on information in the following published reports from the North Carolina Child and Family Leadership Council:

- Invited Local Education Agencies (LEAs): January 2006 (Attachment B)
- 2006/07 School Year: July 2006 (Attachment F), January 2011 (Attachment 1)
- 2007/08 School Year: January 2008 (Attachment 2), July 2008 (Attachment 1)
- 2008/09 School Year: July 2009 (Attachment 1)
- 2009/10 School Year: July 2010 (Attachment 1)

From the reports listed above, I identified 106 unique K–12 public schools. Of these 106 schools, 5 were eliminated due to late entrance (i.e., after the 2006/07 school year). The remaining 101 schools were matched to NCERDC data on third through fifth grade (elementary school) students enrolled in public schools in North Carolina. 54 schools did not match to any third through fifth grade students due to being high schools (21 schools), middle schools (32 schools), or standalone primary schools serving students in pre-kindergarten through second grade (2 schools). Of the remaining 47 schools, 3 were dropped due to closure prior to 2009/10, and 1 was dropped due to opening after 2003/04. The final school-level panel included 43 elementary schools and was strongly balanced across the seven school years (i.e., from 2003/04 to 2009/10).

Predicting Chronic Absenteeism Risk

Using pre-CFST data from the 2003/04 to 2005/06 school years, I estimated the following linear probability model:

$$\Pr(Y_{it}|X_{it}, Z_{st}) = \beta_0 + X_{it}\boldsymbol{\beta} + Z_{st}\boldsymbol{\kappa} + \eta_{it}$$

Y_{it} indicates whether student i was chronically absent in year t. X_{it} is a vector of student characteristics, including race/ethnicity, gender, grade, economic disadvantage, and Z_{st} is a vector of school characteristics, including the share of students by race/ethnicity, the share of economically disadvantaged students, and the natural logarithm of enrollment. I obtained estimates of the $\boldsymbol{\beta}$ and $\boldsymbol{\kappa}$ parameters and then predicted the probability of chronic absenteeism for each student in all school years. I generated risk groups (high versus low) by splitting the sample at the median of this predicted risk.

I generated parameter estimates of $\boldsymbol{\beta}$ and $\boldsymbol{\kappa}$ using two different samples:

1. **In-Sample (IS):** Students enrolled in third through fifth grades during the 2003/04 to 2005/06 (i.e., pre-CFST) school years in directly treated, indirectly treated, and comparison schools.

2. **Out-of-Sample (OOS):** Students enrolled in third through fifth grades during the 2003/04 to 2005/06 (i.e., pre-CFST) school years in all other schools in North Carolina (i.e., schools that were neither directly treated, indirectly treated, nor in the comparison group).

A calibration plot in Appendix Figure B1 confirms that predicted probabilities from the model track observed chronic absenteeism across the majority of the predicted probability distribution. Because I use the predicted probabilities to classify students into high-risk and low-risk groups, observed chronic absenteeism rates across these two groups are the most direct indicator of model performance: the likelihood that students in the high-risk group are chronically absent is 0.060, while the likelihood that students in the low-risk group are chronically absent is 0.028.

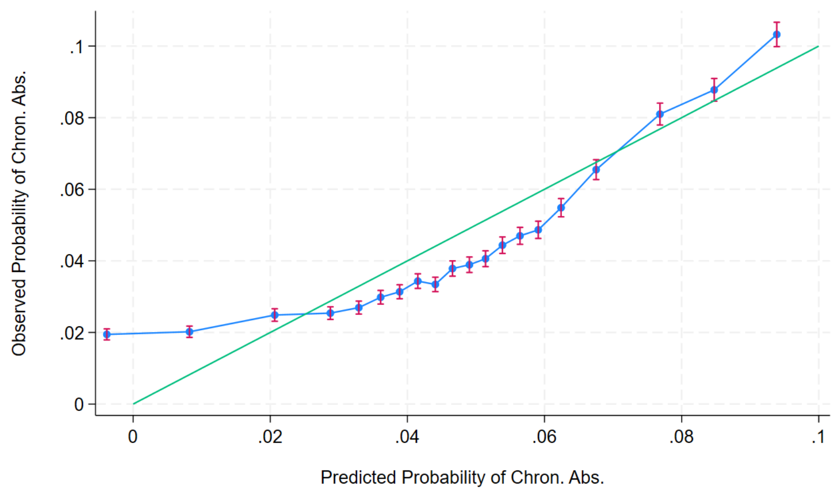


Figure B1: Calibration Plot for Chronic Absenteeism Risk Prediction Model

Notes: The blue circles show average observed probability of chronic absenteeism within each bin of predicted probabilities of chronic absenteeism (20 quantile bins). The red vertical bars illustrate 95% confidence intervals. The 45 degree-line (green) represents perfect calibration (i.e. where observed probability equals predicted probability).