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EdWorkingPaper No. 24-970

Staffing Interventions to Support Students Experiencing Homelessness: Evidence from New York City

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VERSION: May 2024

Suggested citation: O'Hagan, Kaitlyn G., and Zitsi Mirakhur. (2024). Staffing Interventions to Support Students Experiencing Homelessness: Evidence from New York City. (EdWorkingPaper: 24-970). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/vj6v-zn25

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Kaitlyn G. O'Hagan was supported by Institute of Education Sciences (IES) Pre-doctoral Interdisciplinary Training Program (PIRT) Grant #R305B200010 while conducting this research. The opinions expressed are those of the authors and do not represent views of New York City Schools. We are grateful to participants at the SREE 2022 annual conference, APPAM 2023 annual conference, and the NYU Education Policy Work in Progress group for feedback on this work, as well as assistance from Kristin Black and Kathryn Hill at the Research Alliance for New York City Schools. We have no conflicts of interest to disclose.

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Abstract

There is limited empirical evidence about educational interventions for students experiencing homelessness, who experience distinct disadvantages compared to their low-income peers. We explore how two school staffing interventions in New York City shaped the attendance outcomes of students experiencing homelessness using administrative records from 2013-2022 and a difference-in-differences design. We find suggestive evidence that one intervention, which placed social workers in schools, increased the average attendance rates of students in shelter by 1-3 percentage points after 3-5 years. We discuss implications for the importance of non-instructional school staff and strategies to serve homeless students.

Keywords: counseling/student services, educational policy, educational equity

Staffing Interventions to Support Students Experiencing Homelessness: Evidence from New York City

Large numbers of children in the United States experience housing insecurity. Data from the 2019-2020 school year indicate that 1.2 million students, or 2.5% of all students enrolled in public schools, experienced homelessness (National Center for Homeless Education [NCHE], 2022). Other analyses suggest that this number could be higher, as pre-pandemic as many as 2.9 million children were affected by an eviction filing each year (Graetz et al., 2023). Children who experience homelessness face disruptions to their lives during a developmentally critical period, contributing to well-established gaps in behavioral and health outcomes between homeless students and their housed peers (Dwomoh & Dinolfo, 2018; Weckesser, 2022). They also face disadvantages in their academic endeavors, as homeless students are more likely to switch schools and be absent from school than their classmates who are housed (Cowen, 2017; Deck, 2017; Hill & Mirakhur, 2019). They also struggle with engagement at school (Brumley et al., 2015), and have lower achievement on standardized tests (Cowen, 2017; DeGregorio et al., 2022; Deck, 2017; Tobin, 2016).

Practically and from a policy perspective, in part due to guidelines outlined in the federal McKinney-Vento Act (MVA), schools are important institutions in the lives of children who experience homelessness (Aviles de Bradley, 2015). Schools are often where students are identified as experiencing homelessness and where they and their families are connected to resources and services to meet their housing and educational needs. Schools are also the institutions where children experiencing homelessness often find support, consistency, and stability (e.g., Aviles de Bradley, 2011; Ingram et al., 2017; Murphy & Tobin, 2011).

Implicit in the discussion about the importance of schools for students experiencing homelessness is the crucial role played by school *staff*. Both instructional and non-instructional staff within school buildings identify students experiencing homelessness and serve as important sources of information and support for these students as well as their families (e.g., Groton et al., 2013; Miller, 2011). However, the literature on if and how school support staff might be effective at meeting the needs of students who experience homelessness remains sparse. In this paper, we aim to help fill that gap, exploring how, if at all, staffing interventions in New York City (NYC) shaped the attendance outcomes of students experiencing homelessness. We use detailed data on the universe of traditional public elementary and middle schools in NYC from 2013-2022 and a difference-in-differences study design to examine the impact of two school staffing interventions focused on serving homeless students. We find suggestive evidence that one intervention, which placed social workers in schools specifically to serve these students, increased the average attendance rates of students in shelter, one of the most at-risk subgroups of homeless students, by 1-3 percentage points after 3-5 years. Drawing on the literature on the importance of non-pedagogical school staff, our work has important implications for how we continue to serve students experiencing homelessness.

Background

Non-Pedagogical School Staff

Existing evidence suggests that non-pedagogical staff in schools, often referred to as school support staff (e.g., guidance counselors, social workers, and service coordination staff), are important for students. For example, Sorensen (2016) finds that $100 per pupil toward school support services (e.g., spending on school social workers, guidance counselors, and health workers) translates to 0.58 fewer absences per student per year; this investment also improves student achievement. Research that explores the role of school counselors, specifically, finds greater counselor subsidies reduce the frequency of disciplinary incidents and improve teachers’ perceptions of school climate; the evidence is mixed on whether they influence achievement (Carrell & Hoekstra, 2011; Reback 2010a; 2010b). More recent work on school counselors at the high school level finds they can increase high school graduation and college attendance, and they are particularly important for low-income and low-achieving students (Mulhern, 2023). A study of “natural” mentors in high schools (teachers, counselors, and coaches) finds having a mentor lowers rates of course failure, increases credits earned, improves GPA, and increases likelihood of college-going; it also found these relationships are most beneficial for students from low-income families (Kraft et al., 2023). While the inferential literature focusing on social workers is more limited, a recent study found that social workers in schools improved students’ mental health outcomes (e.g., outpatient mental health service use, suicide attempts), but did not affect academic outcomes such as attendance rate (Golberstein et al., 2023).[1] However, other studies of mental health services in schools have found positive impacts on outcomes such as attendance and suspension (e.g., Ballard et al., 2014; Farahmand et al., 2011).

There is also research that looks at the impact of community schools on students. Community schools are “a place-based strategy in which schools partner with community agencies and allocate resources to provide an integrated focus on academics, health and social services, youth and community development, and community engagement” (Maier et al., 2017, p. v). While community school programs are a distinct intervention from staffing supports, they do share similar features. In particular, community school interventions help to coordinate services for students and families from external partners. Trauma-informed care is another element of integrated student support found in many community schools (Maier et al., 2017, p. 22) and in the programs we study. Therefore, research on community schools can provide useful

[1] Of note, in this intervention, the clinical mental health staff were employed by community mental health services agencies, not the schools.

benchmarks of impacts we might expect from staffing interventions similarly focused on service coordination. In a review of research on community schools, Maier et al. (2017) find community schools are an effective intervention to support low-achieving students in high-poverty schools and to help close opportunity and achievement gaps for students from low-income families; many of the studies they review find positive impacts on students' attendance, behavior, social functioning, and academic achievement. In an examination of community schools in NYC, Covelli et al. (2022) find immediate reductions in chronic absenteeism, and improvements in math and ELA test scores by the third year after implementation. Notably, impacts on academic achievement take longer to manifest than the effects on attendance, suggesting improved attendance is a leading indicator of success. An earlier study of community schools in NYC similarly found positive effects on attendance for students in all grades and all years, while impacts on achievement took longer to materialize (Johnston et al., 2020).

The McKinney-Vento Act and School Support Staff

School support staff shape the experiences of homeless students in school, in part because of guidelines listed in the MVA. At its core, the MVA addresses the educational needs of children and youth who experience homelessness. It does so in part by defining homelessness: K-12 students are identified as homeless if they lack a fixed, regular, and adequate night-time residence. This includes students who are doubled-up with family or friends, living in shelters, or living unsheltered (e.g., in parks, in cars, etc.). The MVA also mandates that children are entitled to remain in their school of origin for the duration of an academic year, whether students lose or gain stable housing during or between school years (MVA, 2015).

In addition, the MVA mandates that each local educational agency designates an individual to serve as a *liaison* for homeless children in their district. This individual, by law, has numerous responsibilities: 1) Identify students who are experiencing homelessness by conducting outreach to families and “coordination activities with other entities and agencies”; 2) Ensure that homeless students have a full and equal opportunity to succeed in that district’s schools, including accessing and receiving the educational services for which they are eligible (e.g., transportation services to get to and from school, free- or reduced-price meals, supports for learning English, special education services, etc.); 3) Provide referrals to services that might benefit homeless students and their families such as health care services, dental services, mental health and substance abuse services, housing services; and 4) Support and help with the professional development of school-based staff in their district who are also tasked with supporting students experiencing homelessness (MVA, 2015).

Although social workers are often designated as MVA liaisons (Groton et al., 2013), this role is also played by individuals trained as school counselors, administrators, or special education directors (Havlik et al., 2020). Importantly, individuals often serve as MVA liaisons while serving in other roles for their districts (Aviles de Bradley, 2019; Sulkowski & Joyce-Beaulieu, 2014). In fact, survey data suggest that 77% of MVA liaisons focus only part of their time (0-10 hours per week) on this role (US Department of Education et al., 2015). Further, Havlik et al. (2020) find that MVA liaisons serve varying numbers of schools within their districts: Among the 10 participants they interviewed for their study, respondents were responsible for anywhere from 5 to 200 schools. That being said, even if their knowledge of the statute is limited (Canfield et al., 2012), the empirical literature suggests that MVA liaisons are working towards meeting their responsibilities as outlined in the federal law. In particular, researchers find that MVA liaisons spend much of their time connecting homeless students and their families with other individuals and agencies or organizations who can help meet their needs

(Havlik et al., 2020).

However, MVA liaisons do report facing challenges in meeting students' basic needs (e.g., transportation, food, clothing, shelter) in order to facilitate their academic success (Havlik et al., 2020). Service coordination is challenging for schools (Murphy & Tobin, 2011); it requires school staff to collaborate internally with one another, as well as with an often complex web of external service providers, beyond their school including shelters, public assistance agencies, and community-based organizations (CBOs). Access to CBO partnerships is often limited (Edwards, 2023). MVA liaisons also experience challenges in identifying students or families as experiencing homelessness, in part due to the stigma associated with a lack of housing (Havlik et al., 2020). As we highlight below, NYC builds on the MVA mandates to support students who experience homelessness.

NYC Context

Educational Policy for Homeless Students

Consistent with national trends, a large proportion and number of students in NYC experience homelessness (per the MVA definition)--pre-pandemic estimates found that 12% of students experience homelessness during their elementary years (Hill & Mirakhur, 2019). Given the district's size, in recent years, upwards of 100,000 students have been identified as homeless during each academic year (Closson, 2023). Our data–which cover the subset of the district's students in traditional public schools–confirm that students experiencing homelessness are a significant portion of the population and their numbers have grown over time. Not only do many students in NYC experience homelessness, the characteristics of these students reflect broader and persistent inequities along racial and socioeconomic lines. For instance, Black and Hispanic students, those who qualify for free- or reduced-price meals (FRM), as well as students who are eligible for special education and English language learning services are over-represented among NYC's elementary aged homeless students (Hill and Mirakhur, 2019). In NYC, homeless students also tend to have lower test scores than their low-income stably housed peers (McDermott, 2022). The COVID-19 pandemic exacerbated these disparities, especially for students from Black and Latinx communities (Iosso & Rein, 2022).

The district has taken a number of steps to meet the needs of students experiencing homelessness. The NYC Department of Education (NYC DOE) employs 10 borough-based staff who supervise and support approximately 120 *shelter*-based family assistants, who help families living in shelters understand their educational rights, enroll in a school closer to the shelter, and/or arrange transportation between student's school and shelter (NYC DOE, 2017; see also Tregalia et al., 2023). Though not targeted to support homeless students, NYC DOE also views the community school model as an additional way to support these students (NYC DOE, 2017).

In addition, all schools in NYC are required to designate a "Students in Temporary Housing"[2] (STH) liaison. This individual (typically a school guidance counselor or social worker) helps to ensure school-based compliance with NYC DOE policies and procedures around STH, such as collecting completed housing questionnaires. In rare instances, this person might be a staff member who exclusively works with homeless students, but typically the role of STH liaison is one of many "hats" the staff wears. In all cases, STH liaisons in NYC are expected to identify students affected by homelessness, help assess their needs, and refer them to supports in the school and community. In addition, they are tasked with helping the school leadership plan, budget, and spend Title I funding allocations to meet the needs of students who

[2] The NYC Department of Education uses the terminology "Student in Temporary Housing" to indicate that a student is experiencing homelessness in accordance with MVA guidelines. We use the same language here interchangeably with "students experiencing homelessness" and "homeless students."

experience homelessness.[3]

Staffing Supports for Homeless Students

In addition to the efforts described above, the NYC DOE has implemented two staffing programs to specifically address the needs of students experiencing homelessness. Using the mandate for an STH liaison laid out in the MVA as a basis, both of these initiatives provide schools that have high proportions of homeless students with *additional* staff who are responsible for meeting their mental health needs and leading service coordination efforts in order to support this vulnerable group of students.

The first program, *the Bridging the Gap (BTG) Social Worker Program*, which was launched during the 2016-17 school year, places social workers who are dedicated to supporting homeless students in schools. These social workers have a clinical focus and offer trauma-informed counseling to students; in other words, BTG social workers offer mental health and wellness support to students affected by homelessness in schools. Along with managing a student caseload and providing individual and group counseling, these social workers also build relationships with local shelters, city agencies, and other community-based organizations to connect students and their families with mental health supports as well as other resources. BTG social workers often serve as the STH liaison for their school. The first year that this program was launched, 32 BTG social workers were placed in schools with high proportions of students who lived in shelters. BTG expanded over time to 100 social workers in 2019-20 and, as of the 2021-22 school year, the City still allocated funding for 100 BTG social workers across the district at a cost of $10.8 million. Of note, the funding for this program was year-to-year (rather than guaranteed to continue in future years) until the 2019-20 school year.

[3] School districts set aside Title I funding to support students experiencing homelessness, however, no minimum amount is required.

The second program, the STH *Community Coordinator (CC) Program*, which was launched during the 2018-19 school year, places service coordinators in schools. Unlike BTG social workers, individuals in this role do not have to be trained mental health professionals. Rather, based on a needs assessment that they conduct, CCs work to streamline school-based services (e.g., connecting students experiencing homelessness to tutoring services), improve coordination between shelters and schools, ensure that students' transportation needs are met, and develop partnerships with community service providers (e.g., food banks, healthcare providers, housing agencies). CCs also often serve as the STH liaison for their school. In the first year of implementation, the 2018-19 school year, the City allocated funding for 107 school-based CCs across the district to be placed in schools where high proportions of students experience homelessness at a cost of $10.6 million; as of the 2021-22 school year the program continues to fund 107 CCs at a cost of $11.2 million. The role of CCs might seem similar to that of a community school director, however, they also differ in significant ways: Community school directors are typically employees of the non-profit partner, and there is a literature on best practices for community schools and the role of a community school director specifically (Maier et al., 2017). In addition, while community schools receive additional funding to support the work of the non-profit partner as part of the community school model, there are no such dedicated resources to fund community partnerships for CCs. As previously mentioned, access to external partnerships is often a key challenge in supporting students experiencing homelessness (Edwards, 2023) and a recent report from the NYC Comptroller (2023) suggests there are inequities in access to relevant municipal facilities, with both homeless shelters and social services facilities heavily concentrated in certain geographic areas.

How School Staffing Supports Might Affect Attendance Outcomes

The implementation of these two positions in schools serving high proportions of homeless students are important to study for a number of reasons. First, they are systematic, rather than ad-hoc programs instituted by individual staff members (e.g., a principal who chooses to direct funding to support STH). Both the BTG and CC programs were initiated by the district. Second, these are targeted supports: Unlike programs such as community schools, which serve low-income students regardless of their housing status, these programs specifically target STH.[4] Third, these programs are *school*-based: The staff is based at the school, rather than borough, district, and/or shelter-based staff who serve students across many schools. This allows school staff to have direct knowledge of students' academic needs. Further, it allows schools to engage in cycles of continuous improvement, drawing on examples and data from their school to improve their school-based service (Hill & Mirakhur, 2019). The fact that these policies are school-based also makes them distinct from MVA mandates, which are systematic and targeted but are focused on providing appropriate state- and district-level support and systems for STH. Finally, both of these programs are staffing interventions: Each provides schools with a full-time staff member, rather than push-in or pull-out supplementary services (e.g., after school tutoring, health screenings, or materials such as school supplies, food, or clothing). Ultimately, both NYC programs provide schools with staff to meet students' non-instructional needs.

Like other large, urban districts, the NYC DOE views attendance as a key proximal outcome for its students. Attendance outcomes include both continuous attendance rate, but also chronic absenteeism (missing 10% or more of the school year) and severe chronic absenteeism (missing 20% or more of the school year). These attendance measures are key metrics of school performance for all students (Zimmerman, 2023), but particularly so for students experiencing

[4] However, there may still be positive spillovers on permanently housed students, who may end up being directly served by these staff or benefitting from the positive effects on STH through peer or teacher effects.

homelessness, and improving attendance is considered a first-order challenge in serving these students across all school districts (NCHE, 2022). As written in the NYC DOE's policies for supporting STH: "Regular attendance of homeless children is of **paramount importance** [emphasis added], and the DOE must make every effort to ensure that the student regularly attends school." (NYC DOE, 2019). In addition, the NYC DOE's theory of change for BTG social workers and CCs explicitly highlights their ability to "remove barriers to attendance", as well as emphasizes their role providing services to families: "families are given tools to effectively navigate multiple interagency systems" and "families are referred to nonprofits and other partners to supplement or complement city agency supports" (NYC DOE, personal communication, January 2021). That is, NYC DOE believes if the BTG social workers and CCs are able to undertake their responsibilities, students and families will have the tools, resources, and desire to prioritize consistent school attendance. Attendance is an important outcome because it underscores students' connectedness to school; signals that they are present to receive other services (e.g., mental health services from a school's BTG social worker); and enables them to receive academic instruction that will, in time, improve their achievement outcomes. In addition, the BTG social worker and/or CC free up instructional staff to focus on the work of teaching academic content and skills.

Our Paper's Focus and Contribution

Although the literature on MVA liaisons is growing, there is no quantitative evidence which examines the *effectiveness* of this role and/or other similar roles. Further, to our knowledge, there are few formal policies or practices to support students who experience homelessness that exist beyond the MVA. In this paper, we aim to address both of these weaknesses. We provide a description of NYC policy that provides staffing solutions beyond those mandated by the MVA—BTG social workers and CCs—and we assess their effectiveness at shifting the outcomes of students who experience homelessness. In this way, our research also contributes to the body of evidence on impacts of non-pedagogical school staffing positions, which is relatively limited.

Although not experimental, we see our work as a feasibility study (DiPrete & Fox-Williams, 2021) because our paper takes stock of a policy intervention implemented to reduce a known disparity between groups of students. More specifically, we see this study as a chance to assess whether an institutional response (by the NYC DOE) facilitated the reduction of disparities between children who experienced homelessness and those who remained housed. In our view, this paper provides information to researchers and stakeholders within and outside NYC about the efficacy of staffing schools with non-pedagogical staff and, in doing so, helps us better understand the relationship between educational policy and practice.

Data and Sample

Data

To answer our research question, we create a school-level analytic dataset using 10 years of data for the 2012-13 through 2021-22 school years from two sources: school-level records from the NYC Department of Education about the placement of BTG social workers and CCs, and student-, and staff-, and school-level administrative records from the Research Alliance for NYC Schools (hereafter we refer to school years by the Spring calendar year). The student-level data include an indicator for STH and detailed data on their nighttime residence: doubled-up, shelter, hotel/motel, and other unsheltered housing.[5] These data do not capture if/when students change housing status during the school year, rather, the STH indicator captures if students were

[5] "Awaiting foster care" was also a category of STH until 2017; it was removed as a result of change in federal law that removed this from the federal definition of homelessness (MVA, 2015).

homeless at any point during the school year. We aggregate these student-level data to the school level to get the number and percentage of STH and of the two largest subsets of STH: students who are doubled-up and students in shelter, as well as other school-level student sociodemographic characteristics: the percentage of students by race/ethnicity, hereafter race (Hispanic, Black, White, Asian, and other race; these are the categories used by NYC DOE), students who are English language learners, students with disabilities, and students eligible for free- or reduced-price meals (FRM). We also use student-level data to determine total enrollment and grade span at each school in our dataset.

We combine student-level data with data from the American Community Survey to calculate the average median household income for each school based on each students' census tract. Although this is still a limited measure of income because it is not based on individual students' actual family income, it is a better proxy than FRM eligibility alone (Domina et al., 2018; Fazlul et al., 2023). Finally, we aggregate staff-level data to the school level to get average years of experience for teachers, calculate the pupil-teacher ratio using total enrollment and the total teacher count, and create an indicator for whether the school has a social worker.

Our key outcome measure is attendance rate, which we also aggregate from student-level data, for three mutually exclusive groups: stably housed students (i.e., students not flagged as a STH), students who are doubled-up, and students in shelter. We do the same for two other attendance rate measures: the percentage of students chronically absent (absent at least 10% of the school year) and the percentage of students severely chronically absent (absent at least 20% of the school year). We note that attendance rate captures the average proportion of days attended by students in a given school, so an increase in average attendance rate is a positive impact (that is, normatively good), while for the portion of students who are chronically or severely chronically absent, a *decrease* is a positive impact. Attendance data for 2020 captures September 2019-March 2020 (that is, attendance data for 2020 is from before COVID-19 significantly impacted attendance). Attendance data was not collected in a consistent way across NYC schools in the 2021 school year due to COVID-19 disruptions, so we do not use data from that year.

Sample Restrictions

We limit our sample to traditional public schools; neither program serves charter schools, and only three schools served by either program were special education-only schools (these schools serve students with severe disabilities and differ significantly from traditional public schools in funding, grade span, structure, and educational programming).

Both the BTG and CC programs served schools with various grade spans. However, both programs were heavily targeted to elementary/middle schools: 98 of the 111 traditional public schools that ever had funding for a BTG social worker are elementary or middle schools (i.e., they did not serve students in grades 9-12) and 100 of the 107 schools that ever had funding for a community coordinator are elementary or middle schools. The existing research suggests that STH at the high school level can experience homelessness differently than younger students (Darolia & Sullivan, 2023; Stone & Uretsky, 2016). Given differences in the population and outcome of interest, the interventions studied, and their impact, likely differed at the high school level. To avoid inappropriate comparisons across schools with different grade spans, we limit our sample to schools that never serve students in Grade 9 or higher over our sample period.

In the years of our sample, 10 schools that initially receive funding for a BTG social worker and four schools that receive funding for a CC leave the program (i.e., lose funding in a later year), for reasons that are unknown. It is possible that the staff are never placed at the school (i.e., the funding is not drawn down), though we do not observe this. Since selection *out* of the program is likely not random, to avoid biasing results by dropping these schools, we take an intent-to-treat (ITT) approach. That is, treatment "turns on" in the first year a school receives funding for a BTG social worker or CC, and stays on, even if they leave the program. Lastly, there are five schools that first receive BTG in the 2022 school year, and four schools that first receive CC in the 2022 school year; we drop these schools from the sample because we cannot estimate post-treatment effects given we are missing 2021 outcome data.[6] The result is a sample of 129 total treated elementary/middle schools: 33 schools that receive BTG only, 36 schools that receive CC only, and 60 schools that receive both BTG and CC. Table 1 breaks down the treatment timing for each of these three treatments: BTG had a staggered rollout, so schools joined the program each school year from 2017-2020, while all schools in our sample first receive CC in the 2019 school year. Most schools that get both interventions receive BTG concurrently with CC, or receive BTG after CC (i.e., 39 out of 60 schools that have BTG and CC receive funding for both positions at the same time, or receive BTG funding *after* CC funding).[7]

There is significant selection into the BTG social worker and CC programs, as funding for these positions was not assigned randomly. However, schools with very few or no STH were unlikely to receive these interventions, and therefore are not appropriate comparison schools. Therefore, we limit our sample to schools that served at least 10 STH in 2017. This limits our sample of comparison schools–schools that never received funding for either staff position–to 747 schools that could have plausibly been included in either or both programs. We base this

[6] Our estimator uses the year before treatment as the base year for comparison. For schools first treated in 2022, 2021 is the base year, but since we do not observe 2021 outcomes we cannot estimate effects for these schools..

[7] One school that receives both BTG and CC receives CC in 2020, not 2019 (because funding for the one school that loses CC in 2020 is reallocated). We consider 2020 this school's first year of treatment (determining treatment timing for schools that receive both BTG and CC is discussed further in methods).

sample restriction on STH counts in 2017 for two reasons. First, the identification of STH improved significantly from 2015 to 2016, in part due to improvements in information sharing between the Department of Homeless Services and the NYC DOE (Hill & Mirakhur, 2019). That is, STH counts prior to 2016 were likely artificially low because identification of homeless students remains a significant first-order challenge in serving these students (Ingram et al., 2017). Second, even though 2016 is the last year before either program was implemented, 2017 projected enrollment data is likely to have been used to determine where to place the first set of BTG social workers, because NYC DOE projects enrollment information before the beginning of the school year. That is, they would have projected STH counts for the 2017 school year in spring/summer 2016; in the first year of the program, funding for BTG social workers was allocated to schools in mid-August (NYC DOE, 2016).

Despite this sample restriction, we have an unbalanced sample for two of our outcome measures, average attendance rate for doubled-up students and average attendance rate for students in shelter, because some school-year observations (primarily in the years prior to 2016) have no doubled-up students or students in shelter. In a sensitivity analysis, we impose stricter sample inclusion criteria: We include only schools that had at least 10 students in shelter in every year of the panel (we require at least 10 students to minimize variability in outcome measures based on very small numbers of students). This reduces our set of comparison schools to 207–though this set of schools might be considered a better counterfactual, given they have persistently significant populations of STH. However, this more stringent sample inclusion criteria also eliminates 14 treated schools from our sample (five BTG only schools, five CC only schools, and four BTG and CC schools), which is why we do not present it as our main sample. Reassuringly, results are similar, suggesting sample selection does not significantly affect our estimates. We discuss our sensitivity analyses further in Results.

Summary Statistics

Table 2 presents summary statistics for our sample, disaggregated by each of the three treatments: BTG only, CC only, or BTG and CC, as well as summary statistics for the comparison schools that never receive either program. For the treated schools, summary statistics reflect the year before treatment (while this varies for BTG only schools, and schools that receive both BTG and CC, this reflects 2018 for the CC only treatment group). For the comparison (never treated) schools, summary statistics reflect averages across all school-year observations. As a reminder, comparison schools still provide all services to homeless students mandated by the MVA, and designate a STH liaison as required by NYC DOE.

As expected, schools that receive one or both staffing interventions have higher numbers and proportions of both students experiencing homelessness and students in shelter specifically, than the comparison schools. On average, schools that receive BTG only or CC only serve approximately 100 STH, who comprise 17-18% of the student population at each school. Just over half of the STH in these schools are doubled-up students, and just under half are students in shelter (each comprise 8-9% of all students in these schools). Schools that receive *both* BTG and CC, compared to schools that receive just one of the interventions, are larger (average total enrollment of 725), and serve more students experiencing homelessness (149 on average), including both more students doubled-up (71 on average) and students in shelter (72 on average).

Schools that receive either intervention also have lower average median household incomes, higher portions of FRM-eligible students, and higher portions of Hispanic and Black students (and lower portions of White and Asian students). Given known correlations between poverty, homelessness, and race/ethnicity, the over-representation of Hispanic and Black students at these schools reflects the racialized nature of inequality in our country; as in other places, the majority of families experiencing homelessness in NYC are Black or Hispanic (Coalition for the Homeless, 2023). The portion of students with diagnosed disabilities and the portion of students who are English language learners is also slightly higher in schools that receive any intervention than in comparison schools, suggesting a higher-need population in general. Average teacher experience is comparable across the four categories of schools, however, pupil-teacher ratio (PTR) is actually slightly lower in schools with one intervention. Also as expected, schools that receive BTG only are less likely than comparison schools to have a social worker prior to the intervention, and schools that receive CC only (or BTG and CC) are less likely than comparison schools to be a community school (NYC DOE considered schools' existing resources when selecting schools to receive either or both programs).

Average attendance rates are similar in schools that receive just one initiative or both interventions: Average attendance rate for stably housed students is 91-92% (that is, on average , stably housed students miss approximately 15 days out of a 180-day school year), for students doubled-up is 90.7% (approximately 17 days absent), and for students in shelter is 84-85% (approximately 28 days absent). Notably, the average attendance rate for students in shelter in treated schools means they are, on average, chronically absent: missing 10% or more of the school year. Rates of chronic absenteeism and severe chronic absenteeism are high in schools that receive one or both interventions, and particularly high for students in shelter. Attendance rate outcomes for all students are higher in the comparison schools without any intervention.

Methods

The introduction of these school staff was not random, so we use quasi-experimental methods to estimate the effects of these staff on student outcomes. The purposeful selection of schools is a *feature* of both of these initiatives. Put differently, policymakers would not want to place BTG social workers or CCs in schools with no homeless students, which is why we limit our sample of comparison schools as previously described. Even so, comparing outcomes across schools with and without the programs does not establish causal impacts–as reflected in Table 2, attendance rate outcomes are higher in our group of comparison schools than schools that receive any treatment. We use a difference-in-differences (DID) approach to estimate treatment effects, which exploits variations within a school over time.

A traditional dynamic DID estimator to examine the effects of BTG, which had a staggered rollout, would take the following form:

$$y_{st} = \alpha_s + T_t + X_{st}\theta + \sum_{k=-7}^{5} \beta_k \times BTG_{sk} + \varepsilon_{st} \quad (1)$$

Where y is the outcome of interest (e.g., average attendance rate) for school s in year t, α_s is a school fixed effect, T_t is a year fixed effect, X is a vector of school-level controls[8], and BTG_{sk} are a set of event-time indicators: $k = 0$ in the first year a school receives BTG and $k = -1$ is the omitted category. The coefficients of interest are $\boldsymbol{\beta}_0$-$\boldsymbol{\beta}_5$ (the impact after 1-6 years of BTG, respectively).

When there are multiple periods and variation in treatment timing (staggered implementation)—as is the case for BTG—traditional DID estimators include "forbidden comparisons," in particular, the use of early-treated units as controls for later-treated units (see Roth *et al.*, 2023 for a review). Therefore, we implement the estimator proposed by Callaway

[8] Specifically, this vector includes: total enrollment, percent students doubled-up, percent students in shelter, percent students with disabilities, percent eligible for free or reduced-price lunch, percent English language learners, percentages of students by race (Hispanic, Black, Asian, and other race), a set of indicators for grade span (K-5, 6-8, K-8, or other), average median household income, pupil-teacher ratio (PTR), average teacher experience, an indicator for whether a school has a social worker, and an indicator for whether a school is a community school.

and Sant'Anna (2021) to estimate effects in the presence of staggered treatment timing.[9] As reflected in the summary statistic data, schools selected for BTG differ from the comparison group on various observable characteristics. Therefore, we also control for a set of observable school characteristics in estimating the effect of the BTG program (as in Equation 1), though results are similar without these controls.

The assumptions for causal interpretation in a traditional difference-in-differences model still apply. We are assuming parallel trends: The attendance rate *trends* in schools that never received a BTG social worker are a valid counterfactual for the schools that do receive a BTG social worker, after accounting for fixed differences between schools and years, and the observable school-level characteristics. It is common to gauge the plausibility of the parallel trends assumption by assessing whether outcome trends in the years before treatment are parallel between treatment and control groups. That is, for $k < 0$, nonzero coefficients would suggest differing trends prior to treatment, and therefore it is possible that trends would not have been parallel in the post-treatment period in the absence of treatment. Put differently, it suggests the counterfactual–the outcome trends of schools that did not receive BTG–may not be valid. Even in the absence of differing pre-trends, if there are other contemporaneous changes in schools when they receive BTG, effect estimates may be biased. However, we are not aware of other

[9] It estimates all sensible two-by-two difference-in-differences, using the never-treated schools as the comparison group, for each group g (i.e., the year in which the schools were first treated) and each year t. These estimates are the group-time average treatment effect on the treated, or the ATT(g, t). In this case, the first year in which a school receives BTG determines their group ($g = \{2017, 2018, 2019, 2020\}$) and the times are the years before and after treatment ($t = \{2013, \ldots, 2022\}$). Because schools are treated in each year from 2017-2020 (four groups) and we have data from 2013-2020 and 2022 (nine time periods), we are estimating 32 group-time treatment effects. These are then aggregated to estimate effects at each event time. As an example, the treatment effect for $k = 0$, the first year a school has BTG, is the weighted average of four group-time treatment effects (ATT(g, t)): ATT(2017, 2017), ATT(2018, 2018), ATT(2019, 2019), and ATT(2020, 2020). We implement this using the csdid command in Stata (Rios-Avila *et al.*, 2021). We use the "long2" option to set the year before treatment (event time -1) as the comparison year for pre-treatment estimates, so that pre-treatment estimates are constructed symmetrically to post-treatment estimates, which is comparable to traditional dynamic difference-in-differences estimators (Roth, 2024).

changes in schools that receive BTG at the same time they first receive the program.

Even though all schools receive CCs in the same year, 2019 (i.e., there is *not* staggered treatment timing), for consistency, we use the same estimator used to estimate impacts for the BTG program (Callaway & Sant'Anna, 2021).[10] Again, we rely on the parallel trends assumption: Conditional on fixed differences between schools and years, and observable school characteristics, the *trend* in attendance rate for schools that never receive CC provides a valid counterfactual for schools that receive CC in 2019.

To estimate the effects of having both the BTG and CC program, we use the same estimator, but we define the first year of treatment as the first year of *either* treatment. This means 18 schools have only BTG in the first 1-2 years of treatment (as we define it) and 21 schools have only CC in the first year of treatment (as we define it). The other 21 schools receive BTG and CC in the same year; see Table 1 for a breakdown of treatment timing for schools that receive both BTG and CC.[11] In estimating the impacts of the programs on the set of schools that receive both BTG and CC, we are not trying to disentangle the effects of each program, or whether the two programs have additive or compounding effects. We only seek to demonstrate whether these programs together–in whatever dosage schools received them–had positive effects on the outcomes of interest. We do not attempt to disentangle the effects of the two programs for this treatment group because we have four sub-groups in this treatment (as schools may have received BTG social workers in any year from 2017-2020); considering each of these sub-groups separately would leave us with very small treated samples. While our findings from this analysis

[10] For the CC program, we only have one treatment group, since all schools are treated in 2019. We are therefore estimating seven group-time treatment effects, and the group-time treatment effects of interest are ATT(2019, 2019) and ATT(2019, 2020)--that is, treatment effects in the first and second years of the CC program.

[11] While 22 of the BTG and CC schools receive BTG in 2020, one of the schools that receives BTG in 2020 is also the school that receives CC in 2020 (one of two exceptions to CC's starting treatment timing). Therefore, it receives BTG and CC in the same year (as do the 20 schools that receive BTG in 2019), and for this school we define treatment as beginning in 2020 (see note 8).

will generate less definitive impacts, we still believe it is valuable to try to understand effects in the context of real-world policy implementation, which is often a combination of varying programs and/or “dosages” of programs.

Results

Figure 1A shows the results for the analysis of the effect of BTG only on the school-level average attendance rate for three populations: stably housed students, doubled-up students (the largest portion of STH), and students in shelter, who were particularly targeted by the BTG program (these three groups of students are mutually exclusive). The results suggest that the BTG program had a positive impact on attendance rates for *students in shelter* after the program had been in place for 3-6 years (event times 2-5). Specifically, after five years of the BTG program (event time 4) we observe a statistically significant 2.4 pp increase in the average attendance rate of students in shelter (see Appendix Table A1 for point estimates and standard errors). This is equivalent to an additional four days of school attended per year, assuming a 180-day school year. Estimates for event times 2, 3, and 5, while not statistically significant, suggest a 1.4 pp increase in average attendance rates of students in shelter (an additional 2.5 days of school per year). The average post-treatment effect for students in shelter is 1.2 pp, and statistically significant ($p < 0.10$). Estimates of impacts for students doubled-up are smaller and are also only statistically significant in event time 4 (the average post-treatment effect is 0.5 pp, and not statistically significant). Effects on attendance for stably housed students are smaller still, which is unsurprising, since stably housed students were unlikely to be directly served by the BTG social workers.[12]

[12] We observe a statistically significant ($p < 0.10$) increase in attendance rate for stably housed students after four years of the BTG program, though it is not practically significant. If this reflects a true impact of the program, rather than a statistical artifact, it may be due to spillover effects of positive impacts on stably housed students’ peers, and/or because BTG social workers are providing services to stably housed students in some capacity.

We present results for effects of BTG only on the percentage of students who are chronically absent and severely chronically absent in Appendix Figures A1A and A2A; these results suggest a statistically significant decline in the portion of both doubled-up students and students in shelter who are chronically absent in some years post-intervention (there are no significant effects for severe chronic absenteeism). Specifically, after five years (event time 4), there is an 11 pp reduction in the portion of doubled-up students chronically absent and a 13 pp reduction in the portion of students in shelter chronically absent. However, average post-treatment effects are smaller, and only statistically significant for students in shelter (a 6 pp reduction in the portion of students in shelter chronically absent). Taken with the results for average attendance rate, it is possible the program most effectively served STH on the cusp of chronic absenteeism.

We are cautious in interpreting this result as causal evidence of the impact of the BTG program because the trend in attendance rate prior to treatment (i.e., event times before event time 0) does appear to differ between comparison schools and schools that receive BTG. While the differences in the event times preceding the intervention are not statistically significant, standard tests of statistical significance can be underpowered (Roth et al., 2023), and the pre-trend differences for the attendance rate of students in shelter are significant in magnitude (e.g., 1 pp in event time -2). Put differently, the attendance outcome trends of students in shelter in comparison schools may not be a valid counterfactual for the attendance outcome trends of students in shelter in BTG schools.

Figure 1B shows the results for the analysis of the effect of CC only on the school-level average attendance rate for the same three populations: stably housed students, doubled-up students, and students in shelter (see Appendix Table A2 for point estimates and standard errors).

We find no statistically significant impacts for any subgroup in any years post-treatment. We similarly do not find any significant effects on the portion of students (in any subgroup) who are chronically absent or severely chronically absent (presented in Appendix Figures A1B and A2B).

Finally, Figure 1C shows the results for the analysis of the effect of BTG and CC on school-level average attendance rates, again for stably housed students, doubled-up students, and students in shelter (see Appendix Table A3 for point estimates and standard errors). Results are somewhat similar to the results for the BTG only program. Specifically, the average post-treatment effect on attendance rate of students in shelter is 1.1 pp (and statistically significant). Estimated effects are smaller for doubled-up students, and smaller still for stably housed students (and not statistically significant). Statistically significant effects for students in shelter do appear in event time 1 (after two years), and effects in event times 4-5, for students in shelter, are also larger (2+ pp, though only the event time 4 estimate is statistically significant). As with the estimates for BTG only schools, we do not find consistent reductions in the portions of students doubled-up or students who shelter who are chronically absent or severely chronically absent across post-treatment event times, and most average post-treatment results for these outcomes are not statistically significant (see Appendix Figures A1C and A2C).

As with estimates for the BTG only program, the precision of the event time estimates differs because we observe fewer schools in later event times (see Table 1). As previously discussed, our estimates for schools that receive both BTG and CC do not attempt to disentangle the effects of the two programs, or program durations, on outcomes. Rather, we view this as a supplementary analysis to the analysis of the BTG and CC programs alone, and interpret the estimates as *suggestive* that BTG may have been similarly effective when combined with CC.

Our results are generally similar when we exclude school-level controls and use the alternate sample, presented alongside the main results in Appendix Tables A1, A2, and A3. The alternate sample is limited to schools that always have at least 10 students in shelter, so that the same set of schools contributes to all estimates (in the main sample there are some school-year observations with no students doubled-up or students in shelter, resulting in an unbalanced sample for these outcomes). As previously discussed, this means we lose some treated schools from the sample as well, and the overall sample size makes these results less precise.

We conduct an additional robustness check for the BTG only analysis, where we limit the sample to K-5 schools, since they comprise 27 out of the 33 schools in our BTG only sample but are a smaller portion of the comparison schools (see Table 2). This robustness check, presented alongside the main results in Appendix Table A4, also shows similar results.

Discussion

We find suggestive evidence that the BTG program, which placed a social worker in schools with high portions of homeless students–and particularly students in shelter–to specifically support these students, had a positive impact on attendance outcomes for these students. However, the impacts took 3-5 years to materialize, and it is unclear if they are large enough to be practically significant: An increase in average attendance rate of 1.4 pp for students in shelter is equivalent to 2.5 additional school days per year (assuming a 180-day school year). While our estimates for the BTG program suggest impacts may have been modest, our estimates for all three sets of treated schools are imprecise. Results from the analysis of schools that receive both programs suggest the potential positive effects of BTG were neither diminished nor enhanced by the presence of CC. It is possible there are additive or complementary effects for BTG and CC, but we are unable to determine them. Finally, we are cautious in interpreting these as causal impacts, given baseline differences between BTG schools and the group of comparison schools, as well as evidence of differences in attendance rate outcome *trends*.

While we do not find any statistically significant impacts of the CC program, we caution against interpreting our results as evidence the CC program does not or cannot work. Research focusing on staff in similar roles (e.g., Community School Managers) suggests that coherence in their organizational positioning within schools is important (Hine et al., 2023). Given that the NYC DOE employs *shelter*-based staff who play similar roles, important questions about the positioning of CC in schools remain. Indeed, when considering the impact of school support staff in schools in general, it is important to note that their roles may overlap and differ depending on the context and target population (Rodriguez et al., 2024).

Both programs faced implementation challenges that may have affected their efficacy. Funding for the BTG program was year-to-year (rather than guaranteed to continue in future years) until the 2019-2020 school year. This likely created challenges for staff in those roles as well as school and district leaders--especially when it came to planning and implementing longer-term supports for students. It may also explain why the BTG program did not have significant impacts in the first few years, given that similar interventions that have been studied find a more immediate impact on attendance (e.g., Covelli et al., 2022).

For staff in both positions--BTG social workers and CCs--no additional funding was provided for service provision (e.g., to bring in a tutoring program or provide meals for students and families) leaving room for ad hoc solutions and variation across schools. We must also acknowledge the challenges wrought by COVID-19 on students experiencing homelessness (e.g., Roberts et al., 2021) as well as for BTG social workers, CCs, and other staff who serve students experiencing homelessness. We expect, in line with other research, that the pandemic required staff respond to increased demands in student and/or family needs (including around access to technology) while navigating the illness and protocols for managing COVID-19 (e.g., distancing) in the face of limited resources for service provision and delivery (Roberts et al., 2021). As we have seen for other school staff, these challenges likely contributed to anxiety, burnout, and attrition of the BTG social workers and CC staff themselves (Pressley, 2021).

Limitations

Future research on programs to support students experiencing homelessness with school-based staff, including these programs in NYC, may be able to extend our understanding by overcoming the limitations of this study. First, we cannot speak definitively to *why* the BTG program may have been effective at improving attendance outcomes–that is, what underlying mechanisms are most important to replicate, if the program was to be expanded in NYC and/or implemented in other districts. More robust qualitative work schools are doing to support homeless students could better inform the implementation details related to school-based staffing for this population, though we acknowledge there is already some work in this area (e.g., Havlick et al., 2020; Hill & Mirakhur, 2019; Pavlakis, 2018). Given the trauma experienced by children during the COVID-19 pandemic (Cenat & Dalexis, 2020; Kira et al., 2021), we hypothesize that the mental health training of BTG social workers could have been crucial in helping students.

Second, while we aim to demonstrate that the effects we estimate are indeed impacts of the BTG and CC programs themselves, selection into these programs is not random, so there are threats to causal interpretation. As previously discussed, the selection into the program based on the number/portion of students in temporary housing is by design. In addition, since we measure school-level outcomes, it is possible that our estimates are affected by changes in school population, rather than capturing average effects on individual students. Future work could use student-level data to estimate the impacts of school-based staffing interventions, though such analysis will be complicated by student mobility across schools, which is particularly high for the homeless student population (Hill & Mirakhur, 2019). Student-level analyses may also improve the precision of the results.

Third, future research on school-based supports for homeless students should use additional outcome measures. As the effect of the COVID-19 pandemic on our ability to collect and use data student outcomes wanes, it may be possible to see if impacts are realized in academic outcomes that are downstream from attendance, such as achievement and graduation. Outcomes beyond those measured within the education system may also better reflect program impact–for example, there may be impacts on students' health outcomes, especially given the focus of BTG social workers on clinical mental health services. Improvements in health outcomes may improve education outcomes, but even if they do not, non-academic outcomes are an important consideration on their own.

The BTG and CC programs have continued; as of the 2024 school year, the programs fund the same number of staff as they did in 2020 (NYC DOE, 2023). While these two specific programs have not expanded, similar non-pedagogical staffing programs have been introduced and/or expanded, though not necessarily targeting students in temporary housing. For example, some American Rescue Plan Act (ARPA) funding has been used to place social workers in "high need" schools, defined as schools previously lacking a full-time social worker or mental health professional and with high rates of behavioral incidences and/or poor ratings on school climate surveys (NYC DOE, 2022). In addition to the long-run impacts of BTG and CC, these programs may be a fruitful area for future research.

Conclusion

We provide evidence on novel school-based staffing supports for students experiencing homelessness; few programs targeting this population have been rigorously studied. There is growing acknowledgement that homeless students are falling behind their peers in academic outcomes, but little existing support beyond the McKinney-Vento Act, and there is limited evidence about what education interventions are most effective for this group. There is also limited evidence about non-teacher staffing interventions in schools more broadly. We find suggestive evidence that *targeted* staffing supports for students in shelter–particularly social workers, the staff placed in schools as part of NYC's BTG program--may have a positive impact on their attendance, a key outcome for this group, after three years in a school. Districts with schools serving significant portions of students experiencing homelessness could consider implementing similar targeted staffing interventions. While modifications to the MVA might be considered to require more support to students experiencing homelessness, without additional federal funding, changes to federal law could simply expand what is already an underfunded mandate. Only 23% of school districts receive funding through the MVA (NCHE, 2021), and even districts that do receive this funding often spend far more on required services to homeless students (e.g., transportation) than they receive (Cunningham et al., 2010). It is more likely states and districts will need to lead on funding and implementing programs that support homeless students in schools. Further research should be done to establish what kind of education interventions are most effective for students in temporary housing, and the cost-effectiveness of programs like BTG relative to other potential interventions.

Finally, we acknowledge that the BTG and CC programs--and our study--are housed within the educational system (see Moje, 2022). However, homelessness is likely to impact children in a variety of domains. If our attempts to address challenges in one of those domains shifts students' outcomes, there are reasons to be optimistic that targeted interventions across other systems can collectively help improve the experiences and outcomes of homeless students. We encourage researchers to continue studying a range of student outcomes that might shift with concurrent changes across other systems, such as housing, nutrition, and social welfare.

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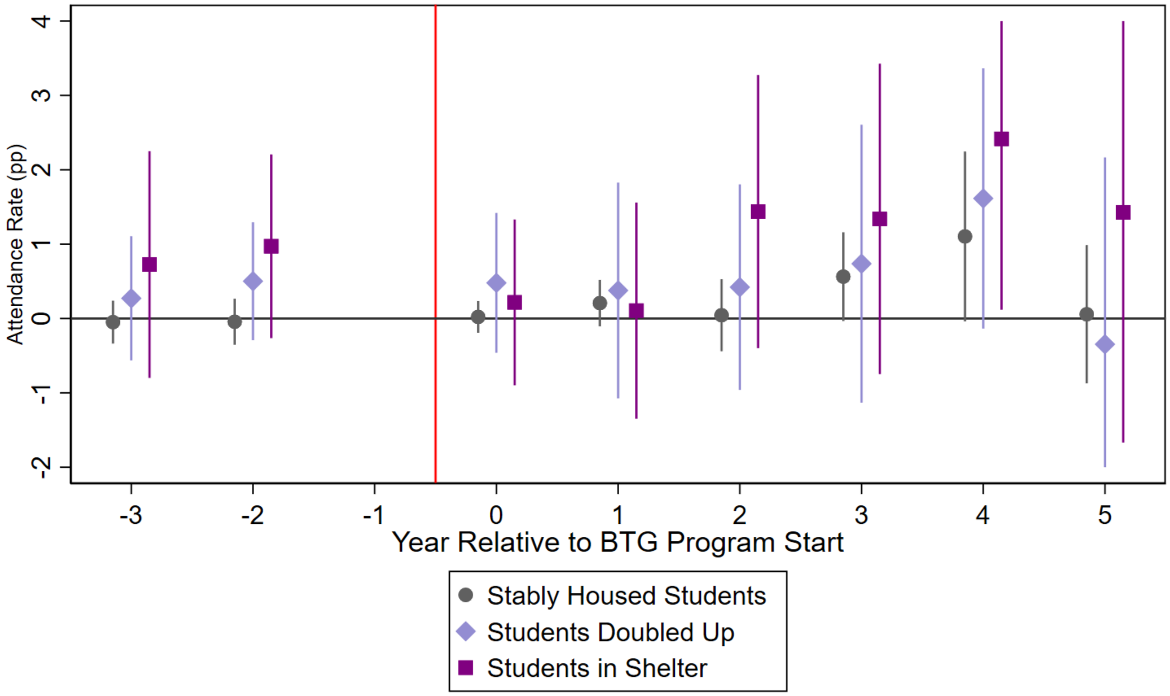
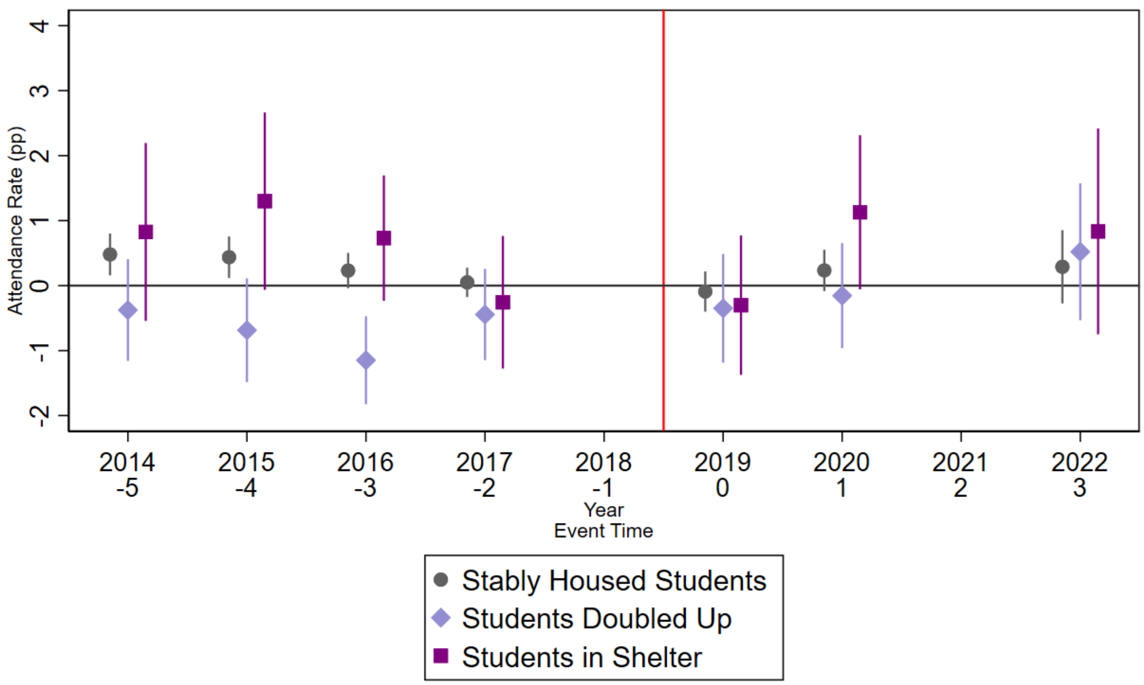
Figure 1. Impacts of the Interventions

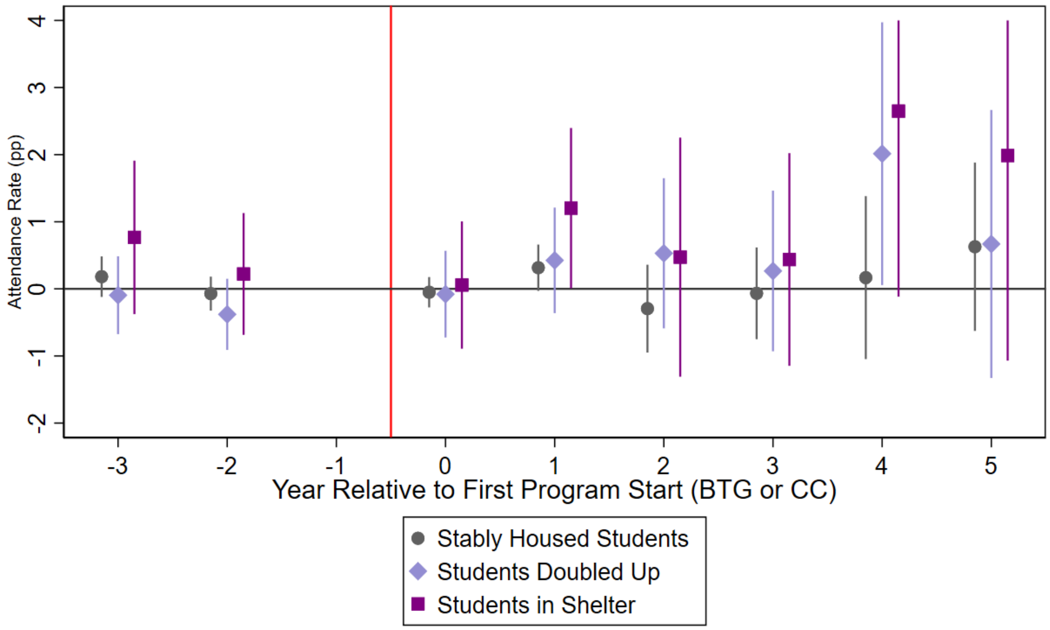
A. BTG Only Schools



B. CC Only Schools



C. BTG+CC Schools



Note. These figures present estimates from the Callaway & Sant'Anna (2021) estimator; point estimates and standard errors are presented in Appendix Tables A1, A2, and A3 (in the highlighted columns). Vertical bars reflect 95% confidence intervals, which are trimmed to the dimensions of the graph where necessary.

Table 1. Treated Schools, First Year in Program

	BTG Only	CC Only	BTG and CC
2017	17	0	13
2018	8	0	5
2019	2	36	20
2020	6	0	22
2021	0	0	0
Total	**33**	**36**	**60**

Note: The table shows the number of traditional elementary/middle schools (i.e., schools that do not serve Grade 9 or higher) that first received BTG, CC, or both, between 2017 and 2021. Schools that lost either program are included. For BTG and CC schools, the year reflects the first year the school received BTG (all schools received CC in 2019, except two, one of which received BTG in 2018, so 2018 is counted as the first year of treatment, and one of which received both BTG and CC in 2020). Schools that first receive BTG or CC in 2022 are excluded from the sample (see discussion of sample restrictions).

Table 2. Summary Statistics: Elementary/Middle Schools in NYC, 2013-2020

	Never Treated (all obs.)	BTG Only (t-1)	CC Only (t-1; 2018)	BTG and CC (t-1)
N Unique Schools	747	33	36	60
Total Enrollment	628	635	656	725
% (#) K-5 Schools	55% (415)	82% (27)	72% (26)	77% (46)
% (#) 6-8 Schools	27% (94)	3% (1)	14% (5)	3% (2)
% (#) K-8 Schools	12% (209)	15% (5)	14% (5)	20% (12)
% (#) Schools with other grade span	5% (29)	0% (0)	0% (0)	0% (0)
# STH	50	101	105	149
% STH	10	18	17	22
# Students Doubled-up	34	54	59	71
% Students Doubled-up	6	9	9	10
# Students in Shelter	14	44	42	72
% Students in Shelter	3	9	8	11
Avg. Median Household Income	$60,205	$44,649	$52,218	$44,426
% FRM-eligible	74	86	82	87
% Hispanic	43	49	54	55
% Black	28	41	35	37
% Asian	14	3	5	4
% White	12	5	4	2
% Other race	2	2	2	2
% Students with disabilities	20	20	22	21
% English language learners	16	17	18	17
Average teacher experience (Years)	11	11	11	11
Pupil-teacher ratio (PTR)	13.6	13.3	13.4	13.7
School has a Social Worker	50%	24%	72%	53%
School is a Community School	24%	30%	17%	8%
AttRt - Stably Housed Students	92.9	91.9	91.3	91.4
AttRt - Students Doubled-Up	91.7	90.8	90.7	90.7
AttRt - Students in Shelter	85.5	84.8	84.3	84.5
% CA - Stably Housed Students	20	24	29	28

Table 2. Summary Statistics: Elementary/Middle Schools in NYC, 2013-2020

	Never Treated (all obs.)	BTG Only (t-1)	CC Only (t-1; 2018)	BTG and CC (t-1)
% CA - Students Doubled-up	26	29	31	30
% CA - Students in Shelter	52	58	57	58
% SA - Stably Housed Students	5	6	7	7
% SA - Students Doubled-up	7	9	9	8
% SA - Students in Shelter	22	24	25	25

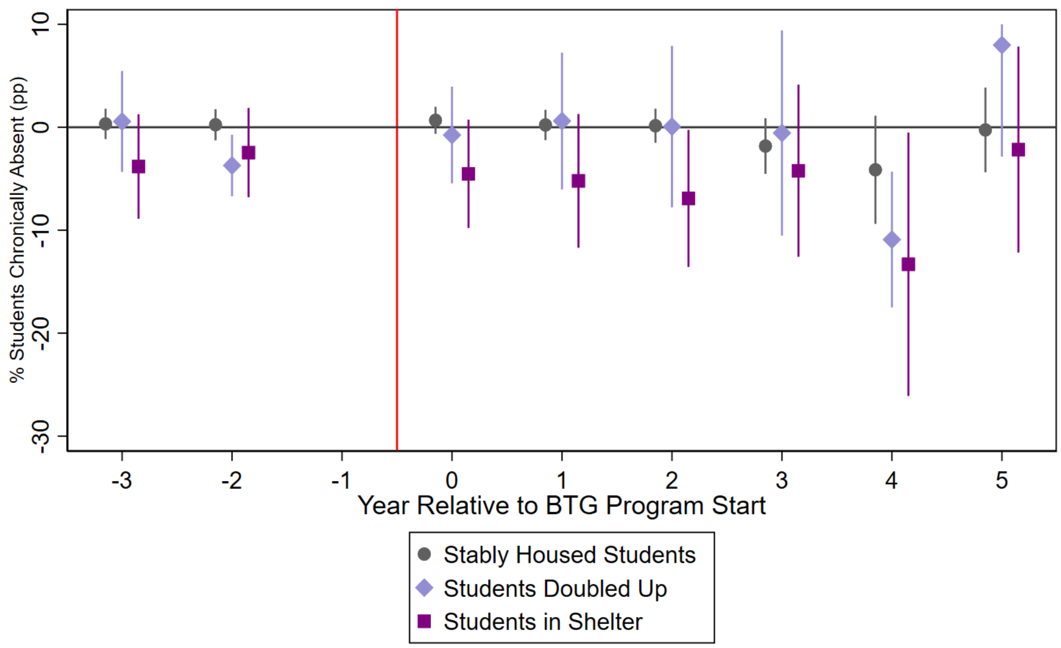
Note. Never treated schools include all traditional elementary/middle schools (i.e., schools that do not serve Grade 9 or higher) with at least 10 students in temporary housing (STH) in 2017. Summary statistics for never treated schools reflect the average of all school-year observations. BTG Only, CC Only, and BTG and CC include all elementary/middle schools that receive the treatment(s) identified. For BTG only schools, the summary statistics are for the year prior to treatment, which varies across schools (see Table 1). For CC only schools, the summary statistics are also for the year prior to treatment, but this is 2018 for all schools. For BTG and CC schools, the summary statistics are for the year prior to *first* treatment. First treatment may be either BTG or CC, so this also varies across schools (see Table 1). AttRt=Average Attendance Rate. CA=Chronically Absent. SA=Severely Chronically Absent. FRM=free- or reduced-price meal.

Online Appendix

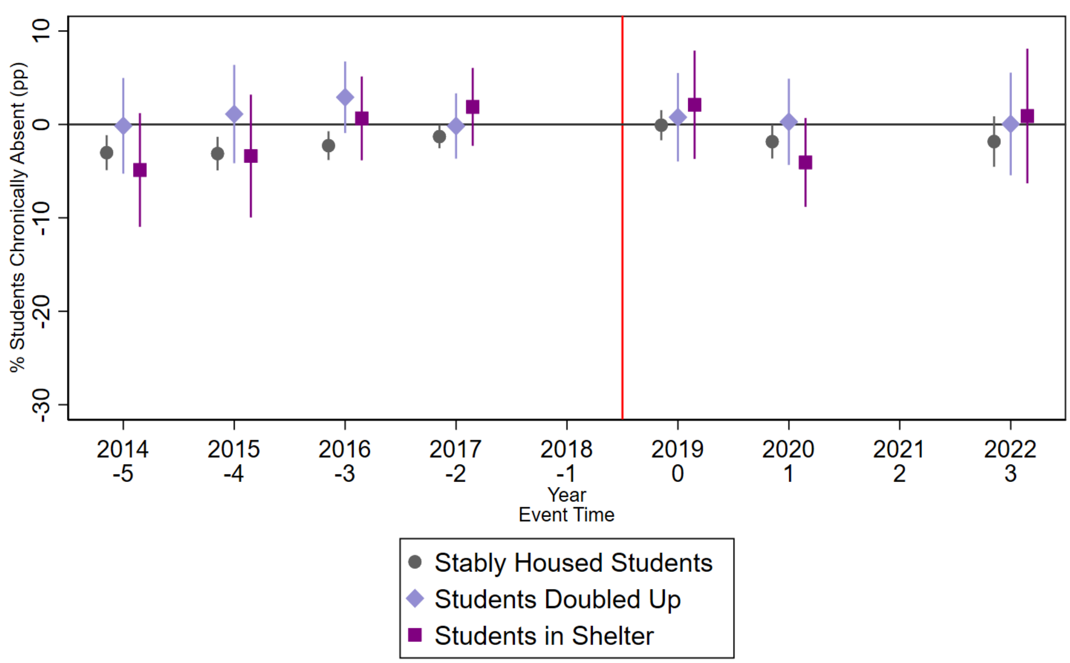
Staffing Interventions to Support Students Experiencing Homelessness: Evidence from New York City

Figure A1: Results for Chronic Absenteeism

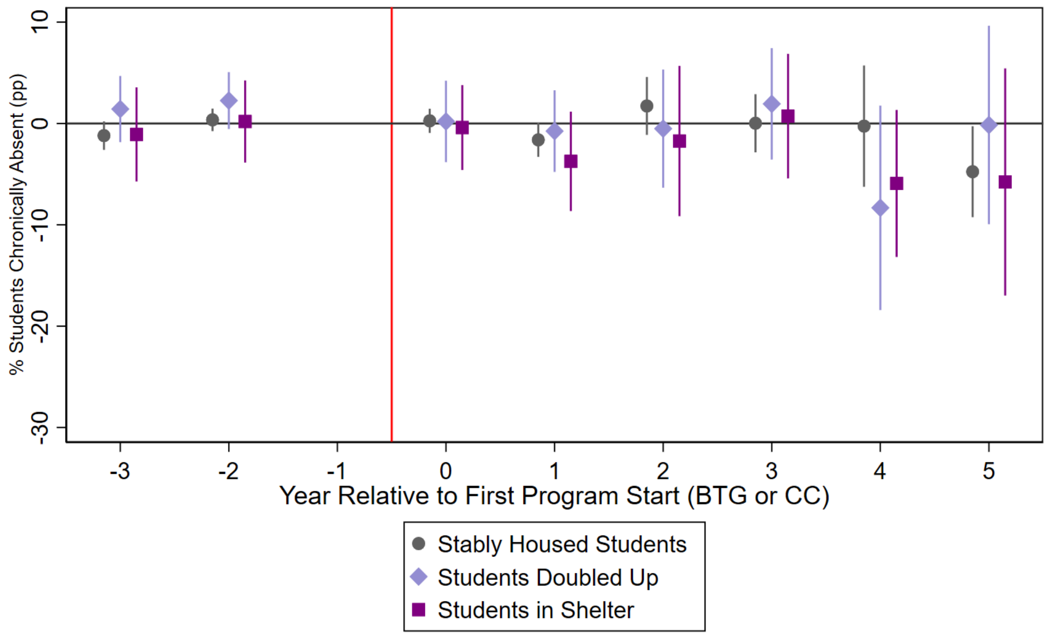
A. BTG Only Schools



B. CC Only Schools



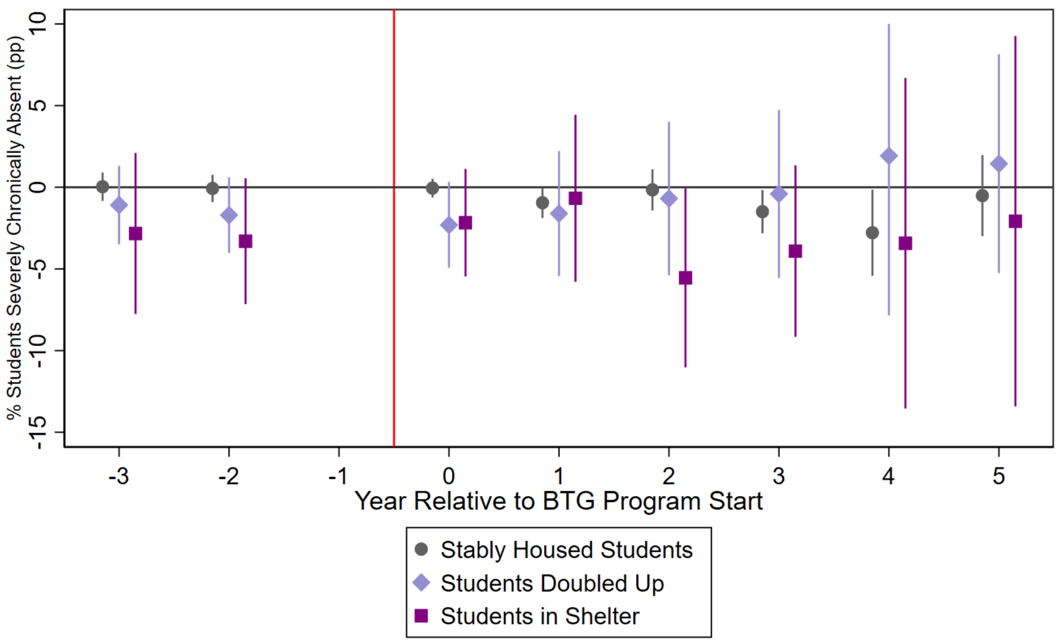
C. BTG+CC Schools



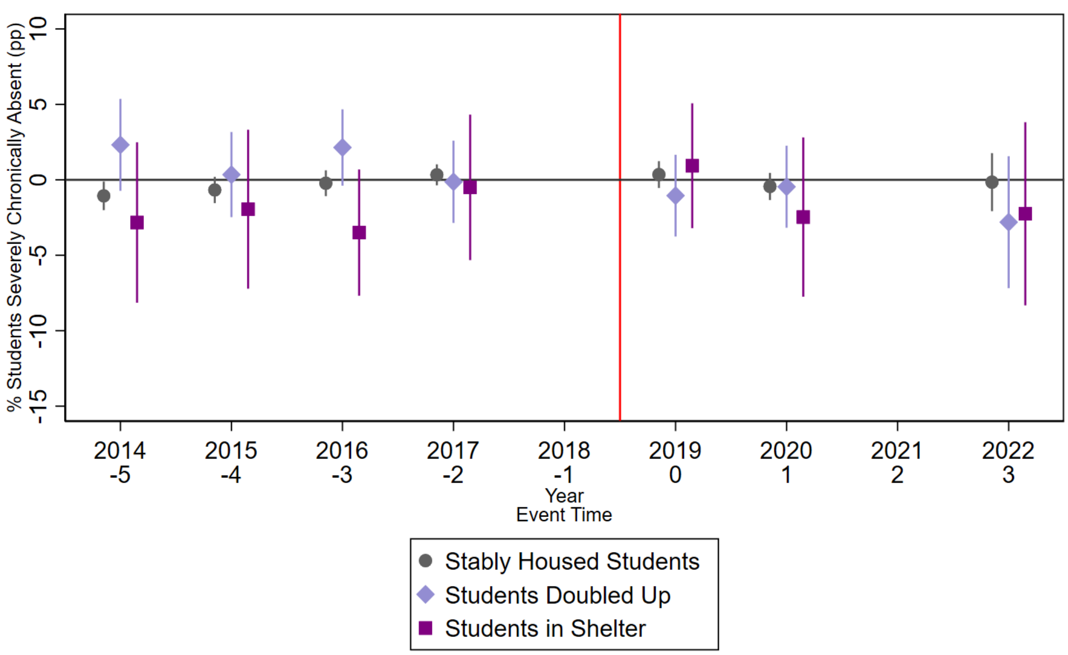
Note. These figures present estimates from the Callaway & Sant'Anna (2021) estimator. Vertical bars reflect 95% confidence intervals, which are trimmed to the dimensions of the graph where necessary.

Figure A2: Results for Severe Chronic Absenteeism

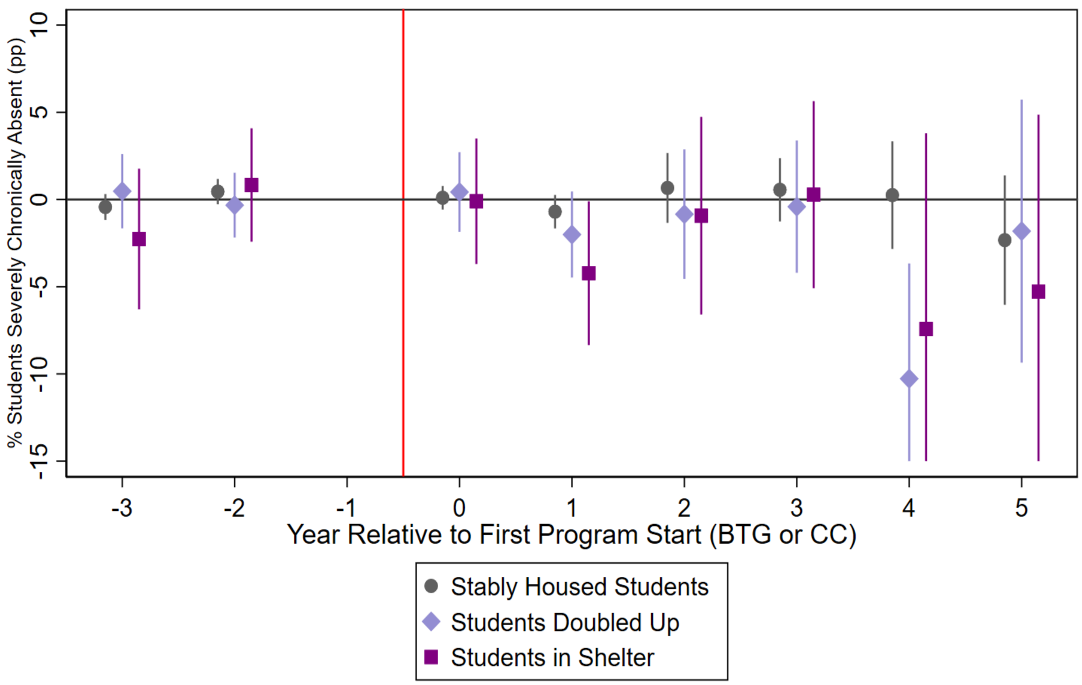
A. BTG Only Schools



B. CC Only Schools



C. BTG + CC Schools



Note. These figures present estimates from the Callaway & Sant'Anna (2021) estimator. Vertical bars reflect 95% confidence intervals, which are trimmed to the dimensions of the graph where necessary.

Table A1.

Attendance Rate Results for BTG only schools (w/ robustness)

	Stably Housed Students			Students Doubled-Up			Students in Shelter		
	Main Results	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**
Pre-treat. avg.	-0.011	0.047	-0.030	0.204	0.042	0.403	-0.120	-0.124	0.387
	(0.280)	(0.257)	(0.346)	(0.435)	(0.385)	(0.445)	(0.877)	(0.806)	(0.627)
Post-treat. avg.	0.333*	0.047	-0.030	0.547	0.340	0.767	1.157*	1.088**	0.732
	(0.199)	(0.257)	(0.346)	(0.576)	(0.561)	(0.696)	(0.634)	(0.548)	(0.610)
Event time									
-6	-0.350	-0.474	-0.321	0.002	-0.150	0.850	-1.059	-1.411	-0.320
	(0.664)	(0.558)	(0.838)	(1.116)	(1.050)	(1.000)	(1.260)	(1.257)	(1.117)
-5	0.117	0.104	0.038	0.359	0.181	0.452	-0.548	-0.306	-0.264
	(0.290)	(0.348)	(0.309)	(0.750)	(0.634)	(0.660)	(0.921)	(0.989)	(0.859)
-4	0.121	0.242	0.029	0.552	0.281	0.865	0.855	0.742	0.480
	(0.156)	(0.158)	(0.202)	(0.541)	(0.494)	(0.643)	(0.782)	(0.686)	(0.662)
-3	-0.049	-0.020	-0.150	0.272	0.147	0.464	0.726	0.898	0.402
	(0.147)	(0.131)	(0.214)	(0.426)	(0.381)	(0.573)	(0.778)	(0.694)	(0.756)
-2	-0.043	-0.067	-0.058	0.501	0.275	0.406	0.972	0.950*	0.768
	(0.158)	(0.144)	(0.181)	(0.405)	(0.370)	(0.487)	(0.630)	(0.561)	(0.553)
-1	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.
0	0.022	-0.027	0.007	0.479	0.369	0.705	0.217	0.412	0.056
	(0.109)	(0.104)	(0.134)	(0.480)	(0.460)	(0.509)	(0.569)	(0.510)	(0.624)
1	0.207	0.085	0.167	0.377	0.107	0.625	0.106	0.153	-0.424
	(0.160)	(0.150)	(0.216)	(0.740)	(0.710)	(0.824)	(0.742)	(0.619)	(0.773)
2	0.044	-0.212	-0.114	0.422	0.219	1.071	1.438	1.643**	0.462
	(0.247)	(0.250)	(0.302)	(0.705)	(0.674)	(0.706)	(0.937)	(0.820)	(0.933)
3	0.563*	0.384	0.702**	0.737	0.548	1.202	1.339	1.270	0.380
	(0.304)	(0.270)	(0.350)	(0.953)	(0.934)	(1.200)	(1.065)	(0.910)	(1.061)
4	1.104*	0.342	0.826	1.615*	1.192	1.107	2.415**	1.760	2.013*

	Stably Housed Students			Students Doubled-Up			Students in Shelter		
	Main Results	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**
	(0.583)	(0.549)	(0.570)	(0.893)	(0.952)	(1.264)	(1.172)	(1.182)	(1.207)
5	0.058	-0.465	0.184	-0.346	-0.392	-0.108	1.426	1.291	1.906
	(0.475)	(0.450)	(0.613)	(1.282)	(1.257)	(1.492)	(1.579)	(1.475)	(1.219)

Note. Estimates of impacts on attendance rate for BTG only schools, using the Callaway and Sant'Anna (2021) estimator described in the text. Estimates from the highlighted columns ("Main Results") are presented in Figure 1A. Estimates from the columns labeled "No Controls" use the same estimator but without student-level controls described in the text. Estimates from the columns labeled "Balanced Sample" use a smaller sample fully balanced for all subgroups of students, as described in the text. Standard errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2.

Attendance Rate Results for CC only schools (w/ robustness)

	Stably Housed Students			**Students Doubled-Up**			**Students in Shelter**		
	Main	**No Controls**	**Balanced Sample**	**Main**	**No Controls**	**Balanced Sample**	**Main**	**No Controls**	**Balanced Sample**
Pre-treat avg.	[illegible]	0.335*	0.260	[illegible]	-0.686**	-0.431	[illegible]	1.001*	0.910
	[illegible]	(0.177)	(0.183)	[illegible]	(0.343)	(0.406)	[illegible]	(0.539)	(0.619)
Post-treat. avg.	[illegible]	0.013	0.242	[illegible]	-0.037	-0.536	[illegible]	0.805	0.492
	[illegible]	(0.175)	(0.227)	[illegible]	(0.391)	(0.461)	[illegible]	(0.571)	(0.653)
2014	[illegible]	0.427**	0.495**	[illegible]	-0.450	0.009	[illegible]	1.303*	0.931
	[illegible]	(0.203)	(0.237)	[illegible]	(0.439)	(0.530)	[illegible]	(0.765)	(0.866)
2015	[illegible]	0.370*	0.400*	[illegible]	-0.618	-0.380	[illegible]	1.531**	1.859**
	[illegible]	(0.197)	(0.226)	[illegible]	(0.441)	(0.485)	[illegible]	(0.766)	(0.820)
2016	[illegible]	0.358*	0.121	[illegible]	-0.846**	-0.880*	[illegible]	0.905	0.650
	[illegible]	(0.184)	(0.198)	[illegible]	(0.368)	(0.505)	[illegible]	(0.617)	(0.763)
2017	[illegible]	0.153	0.013	[illegible]	-0.223	-0.481	[illegible]	-0.116	0.104
	[illegible]	(0.159)	(0.189)	[illegible]	(0.406)	(0.436)	[illegible]	(0.597)	(0.674)
2018		.	.		.	.		.	.
		.	.		.	.		.	.
2019	[illegible]	-0.122	-0.055	[illegible]	-0.466	-0.917**	[illegible]	-0.103	-0.353
	[illegible]	(0.177)	(0.223)	[illegible]	(0.475)	(0.457)	[illegible]	(0.604)	(0.757)
2020	[illegible]	0.323*	0.304	[illegible]	-0.084	-0.557	[illegible]	1.106	0.786
	[illegible]	(0.175)	(0.247)	[illegible]	(0.441)	(0.584)	[illegible]	(0.680)	(0.877)
2021		.	.		.	.		.	.
		.	.		.	.		.	.
2022	[illegible]	-0.162	0.476	[illegible]	0.439	-0.133	[illegible]	1.413	1.043
	(0.342)	(0.344)	(0.407)	(0.640)	(0.599)	(0.696)	(0.963)	(0.926)	(0.881)

Note. Estimates of impacts on attendance rate for BTG only schools, using the Callaway and Sant'Anna (2021) estimator described in the text. Estimates from the highlighted columns ("Main Results") are presented in Figure 1B. Estimates from the columns labeled

“No Controls” use the same estimator but without student-level controls described in the text. Estimates from the columns labeled “Balanced Sample” use a smaller sample fully balanced for all subgroups of students, as described in the text. Standard errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3.

Attendance Rate Results for BTG and CC schools (w/ robustness)

	Stably Housed Students			**Students Doubled-Up**			**Students in Shelter**		
	Main Results	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**
Pre-treat. avg.	0.291*	0.405***	0.204	0.484	0.377**	0.811*	0.305	0.686**	0.430
	(0.154)	(0.118)	(0.196)	(0.357)	(0.179)	(0.452)	(0.477)	(0.309)	(0.474)
Post-treat. avg.	0.116	-0.216	0.205	0.638*	0.487*	0.261	1.134**	1.539***	0.996
	(0.210)	(0.174)	(0.267)	(0.380)	(0.272)	(0.486)	(0.573)	(0.420)	(0.644)
Event time									
-6	0.418	0.485**	0.377	0.915	0.541	1.676*	0.574	1.228**	0.694
	(0.285)	(0.246)	(0.352)	(0.704)	(0.390)	(0.927)	(0.892)	(0.603)	(0.826)
-5	0.578**	0.433**	0.367	-0.260	-0.297	-0.042	0.265	1.166**	0.574
	(0.235)	(0.184)	(0.304)	(0.471)	(0.356)	(0.571)	(0.738)	(0.501)	(0.717)
-4	0.223	0.202	-0.014	0.261	-0.014	0.331	0.970	1.277***	0.580
	(0.188)	(0.140)	(0.240)	(0.358)	(0.258)	(0.497)	(0.593)	(0.446)	(0.585)
-3	0.182	0.314**	-0.019	-0.095	0.106	-0.141	0.767	0.702*	0.250
	(0.154)	(0.137)	(0.187)	(0.296)	(0.208)	(0.453)	(0.584)	(0.395)	(0.564)
-2	-0.071	0.018	-0.208	-0.380	-0.260	-0.317	0.222	0.422	0.468
	(0.129)	(0.115)	(0.170)	(0.270)	(0.196)	(0.380)	(0.463)	(0.330)	(0.499)
-1	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.
0	-0.050	-0.099	-0.037	-0.079	-0.051	-0.304	0.057	0.405	0.156
	(0.116)	(0.087)	(0.156)	(0.329)	(0.216)	(0.460)	(0.484)	(0.341)	(0.595)
1	0.315*	0.308**	0.317	0.425	0.231	-0.470	1.202**	1.192***	1.063
	(0.176)	(0.148)	(0.223)	(0.401)	(0.287)	(0.518)	(0.609)	(0.390)	(0.677)
2	-0.295	-0.535	-0.373	0.530	0.334	0.113	0.472	1.166*	0.358
	(0.334)	(0.364)	(0.382)	(0.571)	(0.481)	(0.726)	(0.909)	(0.666)	(1.079)
3	-0.067	-0.348	-0.197	0.267	0.236	-0.070	0.437	1.598***	0.460
	(0.349)	(0.297)	(0.394)	(0.611)	(0.440)	(0.751)	(0.809)	(0.568)	(0.911)
4	0.167	-0.438	0.560	2.014**	1.693**	1.637	2.649*	2.397*	1.552

	Stably Housed Students			**Students Doubled-Up**			**Students in Shelter**		
	Main Results	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Main Results**	**No Controls**	**Balanced Sample**
	(0.619)	(0.503)	(0.847)	(0.999)	(0.822)	(1.284)	(1.411)	(1.299)	(1.358)
5	0.627	-0.186	0.962	0.669	0.477	0.658	1.986	2.474**	2.388
	(0.640)	(0.606)	(0.780)	(1.019)	(0.899)	(1.161)	(1.559)	(1.241)	(1.522)

Note. Estimates of impacts on attendance rate for BTG only schools, using the Callaway and Sant'Anna (2021) estimator described in the text. Estimates from the highlighted columns ("Main Results") are presented in Figure 1C. Estimates from the columns labeled "No Controls" use the same estimator but without student-level controls described in the text. Estimates from the columns labeled "Balanced Sample" use a smaller sample fully balanced for all subgroups of students, as described in the text. Standard errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0$.

Table A4.

Attendance Rate Results for BTG only schools (w/ additional robustness)

	Stably Housed Students		**Students Doubled-Up**		**Students in Shelter**	
	Main Results	**K-5 Schools Only**	**Main Results**	**K-5 Schools Only**	**Main Results**	**K-5 Schools Only**
Pre-treat. avg.	-0.011	-0.036	0.204	0.194	-0.120	-0.273
	(0.280)	(0.330)	(0.435)	(0.524)	(0.877)	(1.094)
Post-treat. avg.	0.333*	0.302	0.547	0.690	1.157*	1.752**
	(0.199)	(0.200)	(0.576)	(0.697)	(0.634)	(0.780)
Event time						
-6	-0.350	-0.595	0.002	-0.581	-1.059	-1.712
	(0.664)	(0.820)	(1.116)	(1.483)	(1.260)	(1.515)
-5	0.117	0.154	0.359	0.081	-0.548	-0.258
	(0.290)	(0.307)	(0.750)	(0.808)	(0.921)	(0.972)
-4	0.121	0.126	0.552	0.878	0.855	0.536
	(0.156)	(0.180)	(0.541)	(0.574)	(0.782)	(0.919)
-3	-0.049	0.067	0.272	0.525	0.726	0.983
	(0.147)	(0.168)	(0.426)	(0.475)	(0.778)	(0.877)
-2	-0.043	0.080	0.501	0.934**	0.972	0.926
	(0.158)	(0.185)	(0.405)	(0.393)	(0.630)	(0.740)
-1	.	.	.	.	.	.
	.	.	.	.	.	.
0	0.022	-0.023	0.479	0.634	0.217	0.355
	(0.109)	(0.117)	(0.480)	(0.532)	(0.569)	(0.656)
1	0.207	0.148	0.377	0.636	0.106	-0.075
	(0.160)	(0.188)	(0.740)	(0.719)	(0.742)	(0.894)
2	0.044	0.028	0.422	0.208	1.438	1.676
	(0.247)	(0.264)	(0.705)	(0.833)	(0.937)	(1.109)
3	0.563*	0.388	0.737	0.217	1.339	0.990
	(0.304)	(0.298)	(0.953)	(1.247)	(1.065)	(1.322)
4	1.104*	1.153**	1.615*	1.749*	2.415**	3.523**
	(0.583)	(0.579)	(0.893)	(1.057)	(1.172)	(1.603)

	Stably Housed Students		**Students Doubled-Up**		**Students in Shelter**	
	Main Results	**K-5 Schools Only**	**Main Results**	**K-5 Schools Only**	**Main Results**	**K-5 Schools Only**
5	0.058	0.117	-0.346	0.698	1.426	4.040***
	(0.475)	(0.445)	(1.282)	(1.471)	(1.579)	(1.470)

Note. Estimates of impacts on attendance rate for BTG only schools, using the Callaway and Sant'Anna (2021) estimator described in the text. Estimates from the highlighted columns ("Main Results") are presented in Figure 1A and Appendix Table A1. Estimates from the columns labeled "K-5 Schools Only" use the same estimator but limit the sample—both treated and comparison—to schools with a K-5 grade span. For treated schools, we determine inclusion based on grade span in the year before treatment (i.e., if the school is a K-5 school in the year before treatment, they are in the sample). For comparison schools, we determine inclusion based on grade span in all years (i.e., if the school is a K-5 school in every year they appear in our data (2013-2022), they are included in the sample). Standard errors in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.