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

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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 attendance outcomes of students experiencing homelessness using administrative records from 2013-2022 and a difference-in-differences estimator. We find suggestive evidence that an intervention that placed social workers in schools to serve students experiencing homelessness is associated with a 1.2 percentage point increase in average attendance rates of students in shelter. We discuss this small association relative to program costs and implications for education policies targeting 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 2021-2022 school year indicate that 1.2 million students, or 2.4% of all students enrolled in public schools, experienced homelessness (National Center for Homeless Education [NCHE], 2024). 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 important periods of development, 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 less likely to consistently attend the same school and struggle to engage when they do attend school (Brumley et al., 2015; Cowen, 2017; Deck, 2017; DeGregorio et al., 2022; Hill & Mirakhur, 2019; 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 can also be 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 non-instructional school 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) are associated with the attendance outcomes of students experiencing homelessness. Using data from 2013-2022 and a difference-in-differences estimator, we find suggestive evidence that one intervention, which placed social workers in schools (specifically to serve students experiencing homelessness) is associated with a 1.2 percentage point increase in the average attendance rates of students in shelter. We raise questions about the costs of the program relative to its impact in the discussion, as well as potential limits in the ability of school-based staff to address attendance challenges driven by factors external to the school environment. Drawing on the literature on the importance of non-pedagogical school staff, our work has 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, school nurses, family liaisons, etc.), shape students' school experiences and outcomes. For example, Mulhern (2023) finds that school counselors can increase high school graduation and college attendance, and they are particularly important for low-income and low-achieving students. While the inferential literature focusing on social workers is more limited, a recent study finds that social workers in schools improve students' mental health outcomes (e.g., outpatient mental health service use, suicide attempts), but do not affect academic outcomes such as attendance rate (Golberstein et al., 2023).[1]

[1] In this intervention, the mental health staff were employed by community health services agencies, not schools.

However, other studies of mental health services in schools document positive impacts on outcomes such as attendance and suspension (e.g., Ballard et al., 2014; Farahmand et al., 2011). Similarly, research that focuses on school counselors finds greater counselor subsidies reduce the frequency of disciplinary incidents and improve teachers' perceptions of school climate, although the evidence is mixed on whether they influence achievement (Carrell & Hoekstra, 2011; Reback 2010a; 2010b). Importantly, the existing literature highlights the potential efficiency of these staff: 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 and improves student achievement.

In addition, while community school programs are a distinct intervention from the staffing supports we study (Maier et al., 2017), they share similar features. In particular, community schools often have staff who coordinate services for students and families with support 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. As such, the research on community schools can provide useful benchmarks for our work. In their review, 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. Studies of community schools in NYC (Covelli et al., 2022; Johnston et al., 2020) find reductions in absenteeism, with improvements in math and ELA test scores in later years of implementation, suggesting improved attendance is a leading indicator of success.

The McKinney-Vento Act and School Support Staff

School support staff are particularly important in shaping the experiences of homeless students, 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: Pre-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 (the largest group of homeless students), as well as those living in emergency or transitional shelters. In addition, the MVA mandates that each local educational agency designate an individual to serve as a *liaison* for homeless children in their district (NCHE, 2018). This individual, by law, has numerous responsibilities around the identification and service provision for students experiencing homelessness (MVA, 2015).

The existing empirical literature on MVA liaisons is small but growing. Research on this group typically relies on surveys or interviews with individuals in specific geographic regions. Importantly, we note that survey response rates for liaisons appear to be between 30 and 40 percent (e.g., Groton et al., 2013; Mullins et al., 2016; Wilkins et al., 2016), which is likely not representative of all individuals who serve in this role. Furthermore, individuals often serve as MVA liaisons while also playing in other roles in their districts including special education directors, administrators, and counselors (Aviles de Bradley, 2019; Havlik et al., 2020; Sulkowski & Joyce-Beaulieu, 2014). Along these lines, 77% of MVA liaisons report that they 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 (e.g., five to 200) of schools within their districts. While MVA liaisons report spending much of their time connecting homeless students with other individuals and agencies or organizations who can help meet their needs (Havlik et al., 2020), it is unclear how effective such efforts are.

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 borough-based staff who supervise and support approximately *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). Notably, these positions have been in place since at least 2013 (NYC

DOE, 2017). 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, and have been since at least 2013. This individual (typically a school guidance counselor or social worker) helps to ensure school-based compliance with NYC DOE policies around STH, such as collecting completed housing questionnaires. In some 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 other 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 homeless students.[3]

Staffing Supports for Homeless Students

In addition to the efforts described above, the NYC DOE 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.

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

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

homeless students in schools. These social workers have a clinical focus and offer trauma-informed mental health and wellness supports to students affected by homelessness. Along with managing a student caseload and providing 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 schools with high proportions of students who lived in shelters received funding to hire BTG social workers. BTG funding expanded over time to 100 schools in 2019-20 and, as of the 2021-22 school year, the City still allocated funding for 100 schools to hire BTG social workers across the district at a cost of $10.8 million. The BTG program did face implementation challenges–principals did not have guarantees funding would continue year-over-year until 2019, and funding may not have translated into staff hired (and/or staff may not have been hired for the full year), though we do not directly observe this.[4]

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

[4] Even for schools that received funding every year after they initially joined the program, in the early years of the program, the funding was not baselined—that is, guaranteed to continue. So each year, principals did not find out whether they had funding to continue the program until June. This may have disincentivized principals from filling the position, made it difficult to find high-quality staff, and/or made it difficult to retain staff.

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 continued 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. 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.

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. 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. 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 students who experience homelessness. 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 additional staff to meet students' non-instructional needs.

How School Staffing Supports Might Affect Attendance Outcomes

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 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). Put differently, 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; food, personal care items, and other supplies to take home; supplemental academic supports, etc.); 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.

In NYC, students experiencing homelessness have lower rates of attendance than their peers who remain permanently housed (Hill & Mirakhur, 2019; McDermott, 2022). Furthermore, there is heterogeneity in attendance rates *among* students experiencing homelessness: Students in shelters tend to have the worst attendance rates of all students experiencing homelessness (Hill & Mirakhur, 2019; McDermott, 2022). In fact, pre-pandemic estimates indicate that almost three-quarters of students who were a part of the shelter system were chronically absent from school, and missing over three weeks of instruction (Hill & Mirakhur, 2019). Notably, in focus groups, respondents reported that non-instructional staff were important assets to help boost student attendance because they could help create and enforce routines for examining trends in attendance and have the time and capacity to connect with families as well as staff in shelters and other agencies serving these students (Hill & Mirakhur, 2019). However, they also noted that these non-instructional staff operate with limited resources, including confronting broader structural challenges of poverty (e.g., Desmond, 2023).

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 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 provide a description of NYC DOE policy that provides staffing solutions beyond those mandated by the MVA—BTG social workers and CCs—and we assess their association with attendance outcomes of students who experience homelessness. In this way, our research also contributes to the body of evidence on the role(s) of non-pedagogical school staff, 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

For our study, we create a school-level dataset using 10 years of data, for the 2012-13 through 2021-22 school years (hereafter, we refer to school years by the spring calendar year), from two sources: school-level records from the NYC DOE 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. 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 are captured through housing questionnaires administered by schools; since 2017, there is also a direct data link between city-run homeless shelters and NYC DOE that also captures whether a student is in shelter (Hill & Mirakhur, 2019). As in many districts, we note that doubled-up students, in particular, may be under-identified (Cutuli et al., 2024). In addition, these data do not capture if or when students change housing status during the school year, rather, the STH indicator measures if students were 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 all STH. We also aggregate, to the school level, the number and percentage of the two largest subsets of STH: students who are doubled-up and students in shelter. Finally, our dataset includes student sociodemographic characteristics at the school level, including 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.[6]

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

[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).

[6] While students may change schools within a school year, our student-level data reflects the school a student attends in June (that is, the student-level data that are aggregated to the school level are unique at the student-year level).

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 the 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 any social worker.

Our key outcome measure is attendance rate, which we also aggregate from student-level data to the school-level, 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 the 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. Attendance rate reflects the number of days a student was present divided by the number of days a student was enrolled, so accounts for the possibility that students may not be enrolled for the entire school 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. This is unsurprising, given that existing research suggests that older children experience homelessness differently than younger students (Darolia & Sullivan, 2023; Stone & Uretsky, 2016) and need other supports (at school). Given differences by age, the interventions we study likely differ 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 received funding for a BTG social worker and four schools that received 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, we take an intent-to-treat (ITT) approach to avoid biasing results by dropping these schools. 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 received BTG in the 2022 school year, and four schools that first received 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.[7] The result is a sample of 129 total treated elementary/middle schools: 33 schools that receive BTG only, 36 schools

[7] 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.

that receive CC only, and 60 schools that receive both BTG and CC. Table 1 breaks down the 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 got both interventions received BTG concurrently with CC, or received 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).[8] In Table 1, for schools with both programs, the year reflects the first year the school received BTG.

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.[9] We require at least 10 students to minimize variability in outcome measures based on very small numbers of students. We understand that the cutoff of 10 students is somewhat arbitrary, and for this reason, we explore the sensitivity of our analysis to this sample restriction, running the analysis on a sample where the comparison group is not restricted, and imposing a more restrictive sample inclusion criteria. Our decision to focus on schools with at least some STH 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.

[8] 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).

[9] We base this 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 NYC's 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).

Summary Statistics

Table 2 presents summary statistics for our comparison schools, which never receive either program, as well as information for treatment schools, disaggregated by each of the three treatments: BTG only, CC only, or BTG and CC. 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 one staff member as an 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, on average, just over 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, the over-representation of Hispanic and Black students at these schools reflects the racialized nature of poverty and 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).

Our examination of our dependent variables indicate that attendance rate outcomes for all students are higher in the comparison schools without any intervention relative to schools that received one or both programs. In contrast, 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 91% (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. 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.

Methods

The purposeful placement of BTG social workers and CCs is a feature of both programs, so we use quasi-experimental methods to estimate the association of these staff with student outcomes. 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, we reiterate that 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 associations with the program(s), which exploits variations within a school over time. The DID estimator allows us to control for school-level factors associated with selection into the program and attendance rate outcomes, as well as time trends common to all schools. Therefore, these estimates provide more insight than, for example, simple cross-sectional comparisons would. Even though we interpret our results as descriptive, we seek to generate estimates of the associations between the programs of interest and attendance rate outcomes that control for as many confounds as possible.

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[10], 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 association after 1-6 years of BTG, respectively).

[10] 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.

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 and Sant’Anna (2021).[11] 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 to the Callaway & Sant’Anna estimator. In order to interpret our estimates as the causal effect of the BTG program, we would have to assume 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

[11] The Callway and Sant’Anna method allows us to estimate 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, . . . , 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).

trends would not have been parallel in the post-treatment period in the absence of treatment. In other words, it would suggest 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 the BTG program, effect estimates may be biased. While we are not aware of other changes in schools at the same time they first receive the BTG program–the only new STH-related positions introduced during the study period are the BTG social workers and CCs–we remain cautious and interpret our results as descriptive.

Even though all schools that receive *only* CCs receive them in the same year, 2019 (i.e., there is *not* staggered treatment timing), for consistency, we use the same strategy to estimate associations as we do for the BTG program (Callaway & Sant'Anna, 2021).[12]

Finally, to calculate the associations with attendance rate for schools that have 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.[13] In estimating the attendance rate associations in 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 associations with the outcomes of interest.

[12] 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.

[13] 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).

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) and considering each of these sub-groups separately would leave us with very small treated samples. While our findings from this analysis will generate less definitively interpretable estimates, we still believe it is valuable to try to understand outcomes in the context of this real-world policy implementation.

Results

Figure 1A shows the results for the analysis of the association between BTG only and school-level average attendance rate for our three mutually exclusive focal populations: stably housed students, doubled-up students, and students in shelter, who were particularly targeted by the BTG program. We note here that our estimates in the time periods prior to the implementation of the BTG program give us additional reason to avoid interpreting our results as causal. In addition to the selection issues previously discussed, 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 relatively large (e.g., 1 percentage point 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.

Turning to the estimates post-treatment, none of the results are statistically significant (see Appendix Table A1 for point estimates and standard errors and see Appendix Table A4 for the number of schools contributing to each event time estimate). However, our confidence intervals are large. Looking at the magnitude of the estimates, the BTG program may have been positively associated with attendance rates for *students in shelter*, particularly after the program had been in place for 3-6 years (event times 2-5). The average post-treatment association for students in shelter, across all years, is 1.2 percentage points (pp). Again, this association is not statistically significant, but we return to the magnitude of this estimate in the discussion and attempt to benchmark it against comparable school investments. Estimates of BTG's association with the attendance rate of students doubled-up are smaller (the average post-treatment effect is 0.5 pp, and again, not statistically significant). Associations with 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.

We present results for the association of BTG only with the percentage of students who are chronically absent and severely chronically absent in Appendix Figures A1A and A2A. Average associations with chronic absenteeism rates post-treatment are only statistically significant for students in shelter: a 6 pp reduction in the portion of students in shelter who are chronically absent ($p < 0.05$).

Figure 1B shows the results for the analysis of the association between CC only and school-level average attendance rate for the same three populations (see Appendix Table A2 for point estimates and standard errors). These estimates suggest the trend in attendance rate prior to treatment (i.e., event times before event time 0) are statistically significantly different between comparison schools and schools that receive CC. Post-treatment, we also find no statistically significant associations between the CC program and attendance rates for any subgroup. In addition to not being statistically significant, the magnitude of the estimates for CC only schools are much smaller than the magnitude of the estimates from BTG only schools (e.g., 0.5 pp overall post-treatment estimate for students in shelter). We similarly do not find any significant associations with 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 association between BTG and CC and 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 and see Appendix Table A5 for the number of schools contributing to each event time estimate). Results are somewhat similar to the results for the BTG only program. Estimates suggest the trend in attendance rate prior to treatment may differ between comparison schools and schools that receive BTG and CC, which is why we apply the same caution in interpretation as with the BTG only results. That being said, the average post-treatment effect on attendance rate of students in shelter is 1.1 pp and statistically significant ($p < 0.05$). Estimated effects are smaller for doubled-up students, and smaller still for stably housed students (and neither are 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 differ because we observe fewer schools in later event times (see Table 1).

Sensitivity Analyses

Our results are generally similar when we exclude school-level controls presented alongside the main results in Appendix Tables A1, A2, and A3. Of greater consideration is our sample restriction criteria. To demonstrate robustness, we present results for two alternate samples (again, presented alongside the main results in Appendix Tables A1, A2, and A3). The first alternate sample is more restricted, and limited to schools that have at least 10 students in shelter in every year of the panel. In the main sample there are some school-year observations with no students doubled-up or students in shelter (primarily in the years prior to 2016), resulting in an unbalanced sample for these outcomes. In the alternate sample, the same set of schools contributes to all estimates (all schools with at least 10 students in shelter also have at least 10 doubled up students). The alternate sample 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. The alternate sample 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 our sample selection for our main results does not significantly affect our estimates. In a second alternate sample that is less restrictive, and includes all schools in the comparison group, regardless of the number of STH or students in shelter served, we find that the results are similar, suggesting our sample restriction for our main results is not arbitrarily influencing findings.

Discussion

Our aim in this paper is to assess the association between two non-instructional positions with the attendance outcomes of students who experience homelessness. While none of our results for the BTG program's association with attendance rates are statistically significant, we find suggestive evidence that it has a positive association with attendance outcomes for students in shelter. However, this association may take 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 1.2 pp for students in shelter is equivalent to 2 additional school days per year (for a student enrolled for all 180 days of the school year). However, the results show a 6 pp reduction in chronic absenteeism among students in shelter after the introduction of BTG, suggesting the program may have targeted STH on the cusp of chronic absenteeism. We do not find any statistically significant associations between the CC program and any of our outcomes (attendance rates, chronic absenteeism, and severe chronic absenteeism).

As previously discussed, our estimates for schools that receive both BTG and CC do not attempt to disentangle the associations of the two programs, or program durations, with attendance 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 the potential positive associations between BTG and attendance outcomes 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.

Implications

Although a feasibility study, this work has important implications in terms of understanding the costs and benefits of these programs in meeting homeless students' needs. For this reason, we do a back-of-the-envelope cost-effectiveness analysis, comparing the cost of each program with the estimated associated attendance improvements for students in shelter (who have the worst attendance rates of all students experiencing homelessness), and benchmarking that against previous literature. The cost of the BTG program for each school is $105,660 as of the 2023-2024 school year (NYC DOE, 2023–this is the funding for one social worker). Based on the average number of students in shelter attending schools with the BTG program (44, see Table 2), the average cost per student is $26,415. While our estimate of the association between the BTG program and attendance rate outcomes for students in shelter–1.2 pp, or 2 additional days of school–is not statistically significant, we use it for this informal analysis as a *potential* impact of the program. That is, the cost-effectiveness of the program based on this estimate is approximately $12,229 per one fewer absence per student per year. This is 70 times as large as the estimates from Sorenson (2016), which found a $172 per pupil investment in school support staff would result in one fewer absence per student per year (we assume linearity in these estimates). That is, even if the BTG program improved attendance rates by the amount we estimate, it has done so inefficiently. There may be limits to the ability of school-based staff to improve attendance among students in shelter when absenteeism is driven by factors external to schools. It is also possible that there are benefits of the program not captured in attendance outcomes.

Additionally, while we note that while we do not find any statistically significant association between the CC program and attendance rate, 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 studying school support staff 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).

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). These programs may be a fruitful area for future research.

Implementation Challenges

Because we see this as a feasibility study, it is important to take into account implementation challenges, which may have affected the programs’ efficacy. As described earlier, 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 was not significantly associated with attendance 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).

Finally, we do not have detailed data on *how* the BTG and CC programs were implemented in schools, and therefore cannot speak definitively to *why* the programs were not more clearly associated with attendance improvements for all STH–that is, what underlying mechanisms may have helped or hindered program success. 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.

Study 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, selection into these programs is not random, and we are not able to identify the causal impact of these programs on attendance outcomes. As previously discussed, the selection into the program based on the number/portion of students in temporary housing is by design. We find both baseline differences between BTG schools and the group of comparison schools, as well as evidence of differences in attendance rate outcome *trends*. Relatedly, it is possible that the larger positive results observed in specific event times are driven by school-specific effects, if the program was more successful in the cohorts of schools contributing to those estimates (e.g., our largest estimates for the BTG program are in event time 4, and only schools in the 2018 cohort are contributing to this estimate—see Table A4).

Second, since we measure school-level outcomes, it is possible that our estimates are affected by changes in school population, rather than capturing average associations for individual students. These changes in school population could be true changes in students' housing status, and/or changes around identification of STH. While the identification of students in shelters is near-complete, due to a data sharing agreement between the city-run shelter system and the NYC DOE, doubled-up students are likely under-identified in NYC schools, as they are in many school districts. This means stably housed students as defined in our data may include doubled-up students. If under-identification is consistent across schools that do and do not receive the interventions we study, this is unlikely to significantly affect our estimates. However, if these interventions directly affect practices around the identification of STH, our estimates may be capturing this effect. For example, if there are newly identified doubled-up students in schools with an intervention, and these students have relatively high attendance rate, this could upwardly bias the associations we measure for doubled-up students–though as discussed, our findings for this population are small and not statistically significant.

The use of school-level outcomes also means our estimates for all three sets of treated schools are imprecise. Future work could use student-level data to estimate the impacts of school-based staffing interventions, which may also improve the precision of the results. However, such analyses are complicated by student mobility across schools, which is particularly high for the homeless student population (Hill & Mirakhur, 2019). It is also complicated by the dynamic nature of student homelessness. Students may enter or leave stable housing throughout the school year (which we do not observe) and across school years. Properly accounting for the dynamic nature of homelessness and the mobility of these students in the context of school-level interventions may require more detailed data on implementation and services as well as on student-level movements across schools and housing circumstances.

Third, future research on school-based supports for homeless students should use additional outcome measures. Outcomes beyond those measured within the education system may better reflect program effects–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.

Conclusion

There is growing acknowledgement that homeless students are falling behind their peers in academic outcomes, and receive little existing support beyond the MVA. Further, there is limited evidence about what interventions are most effective for this group and about non-instructional staffing interventions in schools more broadly. We fill both of these gaps in this study. Our results indicate that there may be limits to the ability of school-based staff to improve attendance among students in shelter when absenteeism is driven by factors external to schools. However, it is also possible that there are benefits of the program not captured in attendance outcomes. 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 school-based staffing programs relative to other potential interventions. The BTG and CC programs—and our study—are housed within the educational system (see Moje, 2022). However, homelessness impacts children in a variety of domains. Targeted interventions across other systems may be necessary to 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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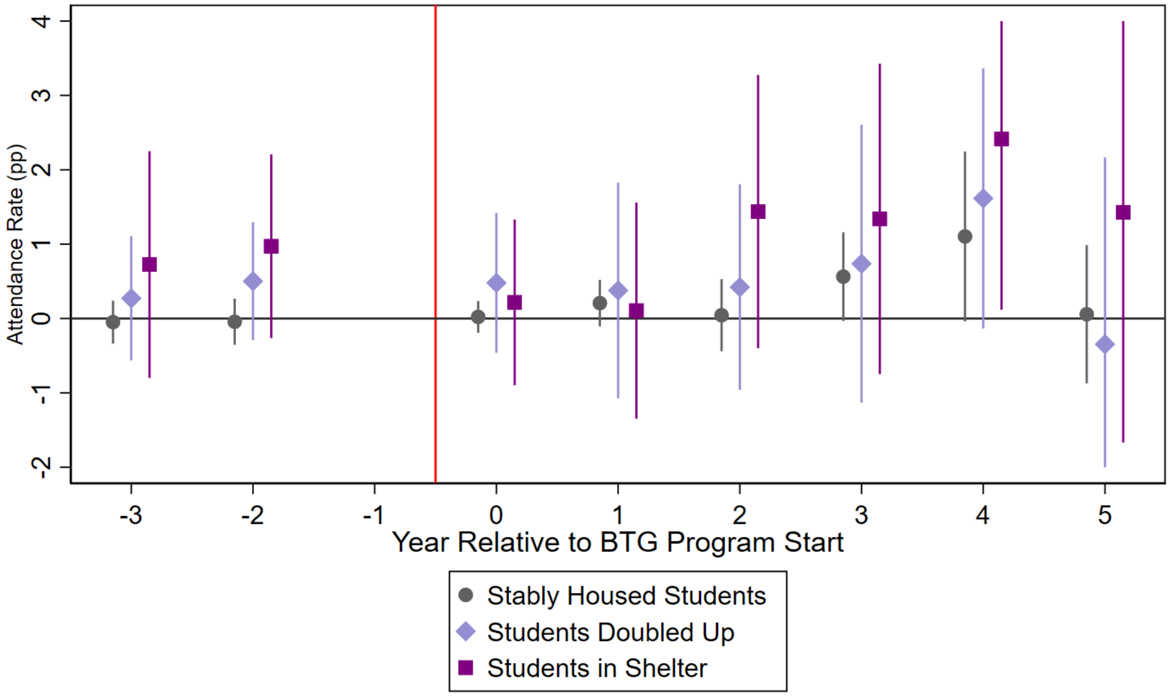
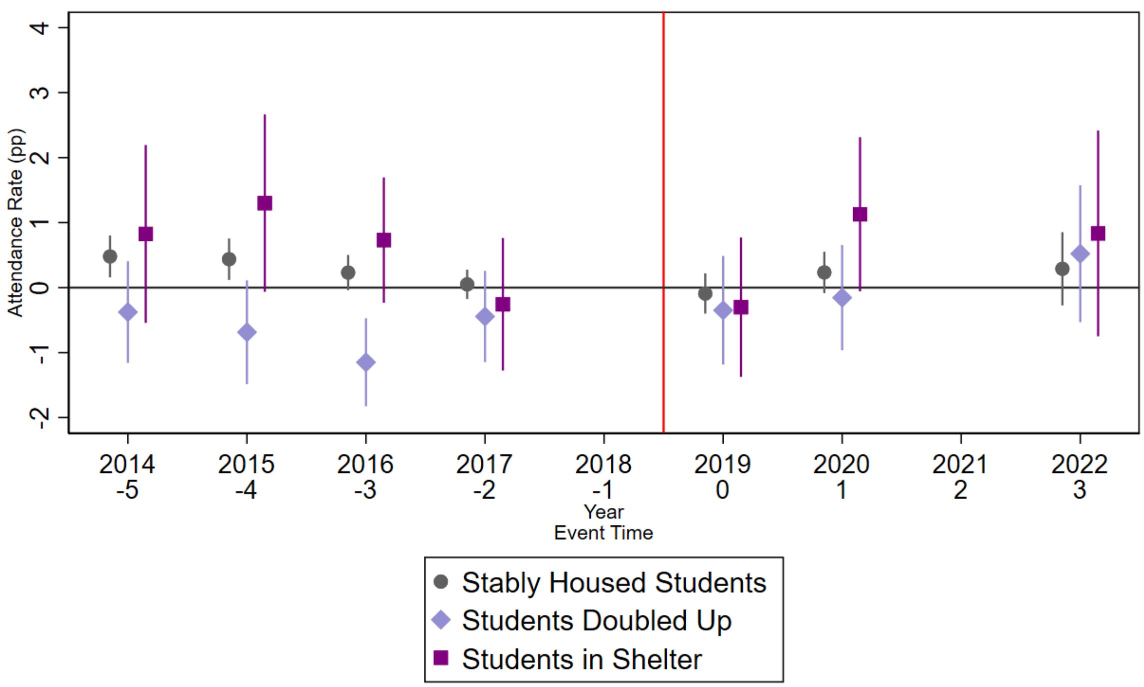
Figure 1. Attendance Outcomes, by Interventions

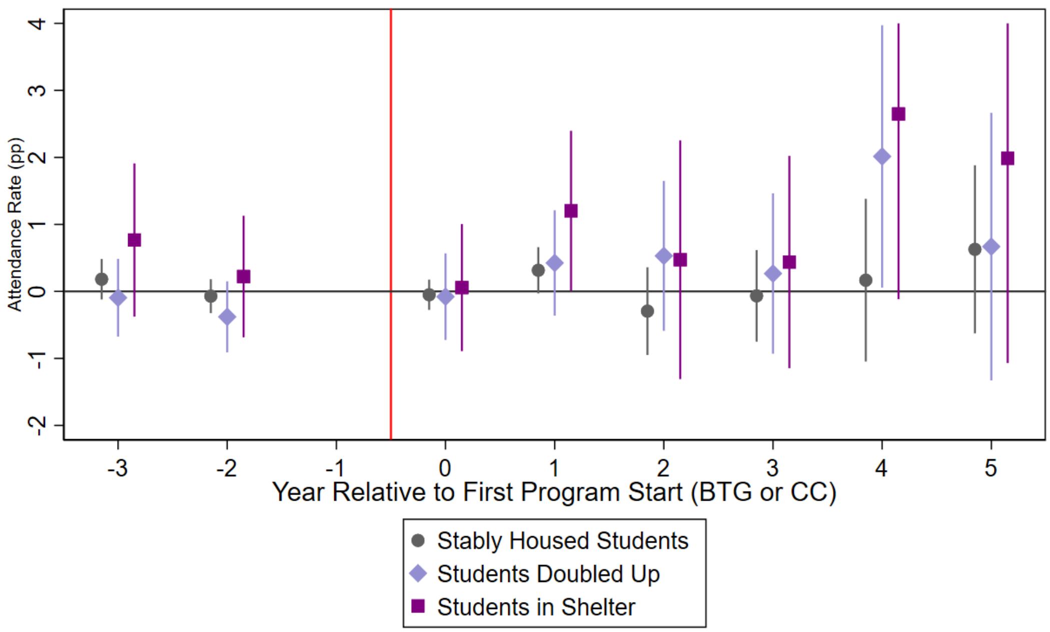
A. BTG Only Schools



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. See Tables A4 and A5 for the number of schools contributing to each event time estimate panels A and C, respectively (the same number of schools contribute to each event time estimate in panel B, see Table 1) .

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. Average Characteristics of Elementary/Middle Schools in NYC, 2013-2020, by BTG & CC Status

	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
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%
Attendance Rate (%)				
Stably Housed Students	92.9	91.9	91.3	91.4
Students Doubled-Up	91.7	90.8	90.7	90.7
Students in Shelter	85.5	84.8	84.3	84.5

Table 2. Average Characteristics of Elementary/Middle Schools in NYC, 2013-2020, by BTG & CC Status

	Never Treated (all obs.)	BTG Only (t-1)	CC Only (t-1; 2018)	BTG and CC (t-1)
% Chronically Absent				
Stably Housed Students	20	24	29	28
Students Doubled-up	26	29	31	30
Students in Shelter	52	58	57	58
% Severely Chronically Absent				
Stably Housed Students	5	6	7	7
Students Doubled-up	7	9	9	8
Students in Shelter	22	24	25	25

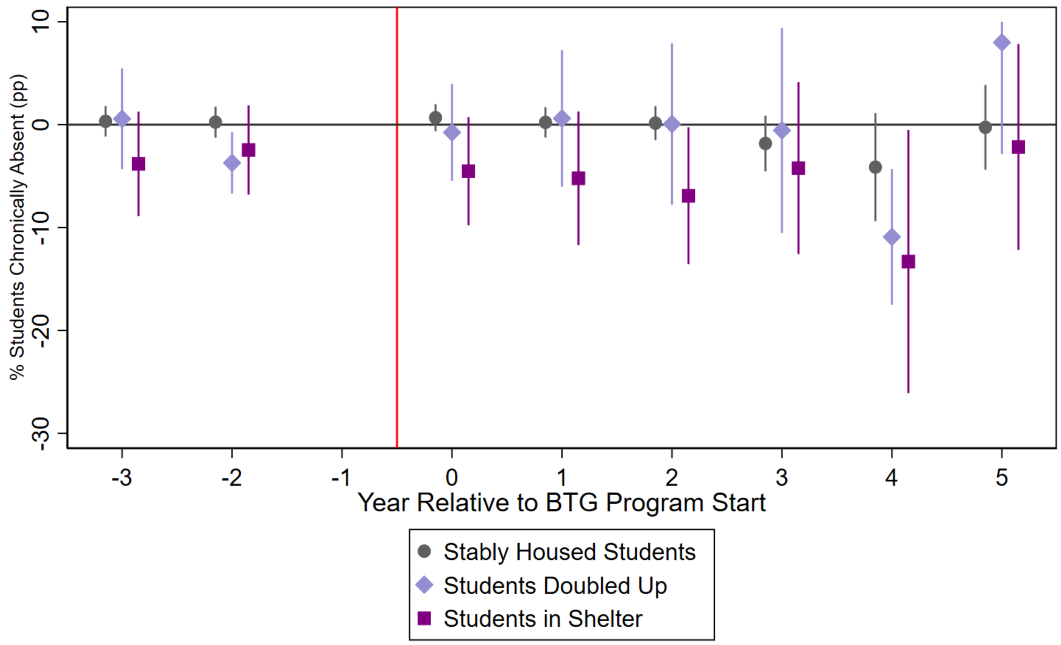
Note. Never treated schools include all traditional elementary/middle schools (i.e., schools that do not serve Grade 9 or higher) with at least 10 students in temporary housing (STH) in 2017. Summary statistics for never treated schools reflect the average of all school-year observations. BTG Only, CC 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). FRM=free- or reduced-price meal.

Appendix

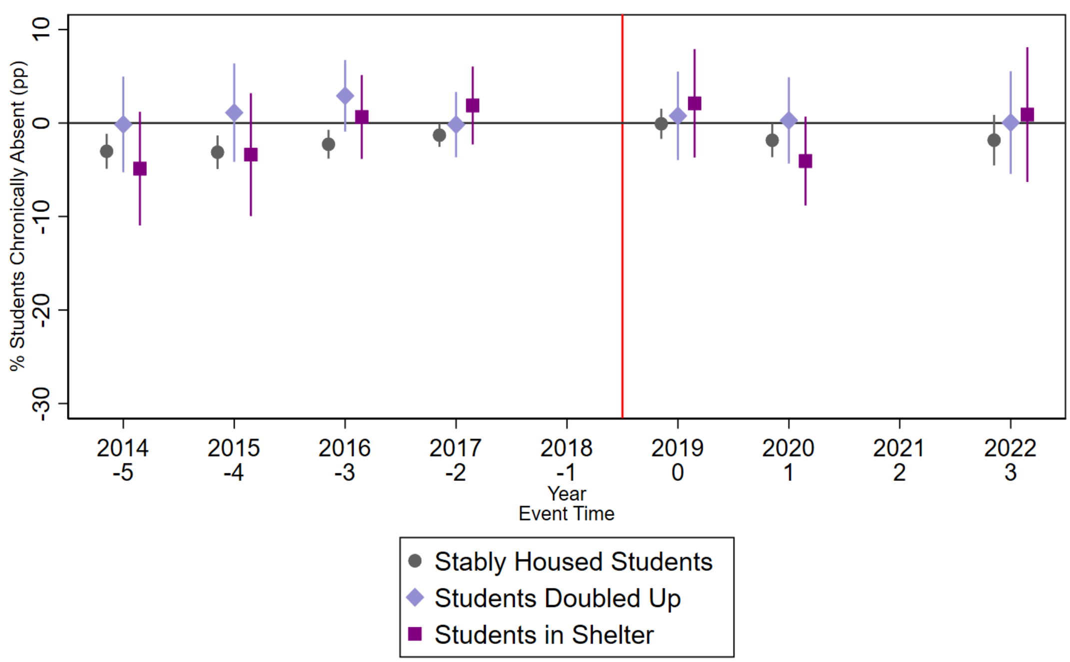
Staffing Interventions to Support Students Experiencing Homelessness: Evidence from New York City

Figure A1: Results for Chronic Absenteeism

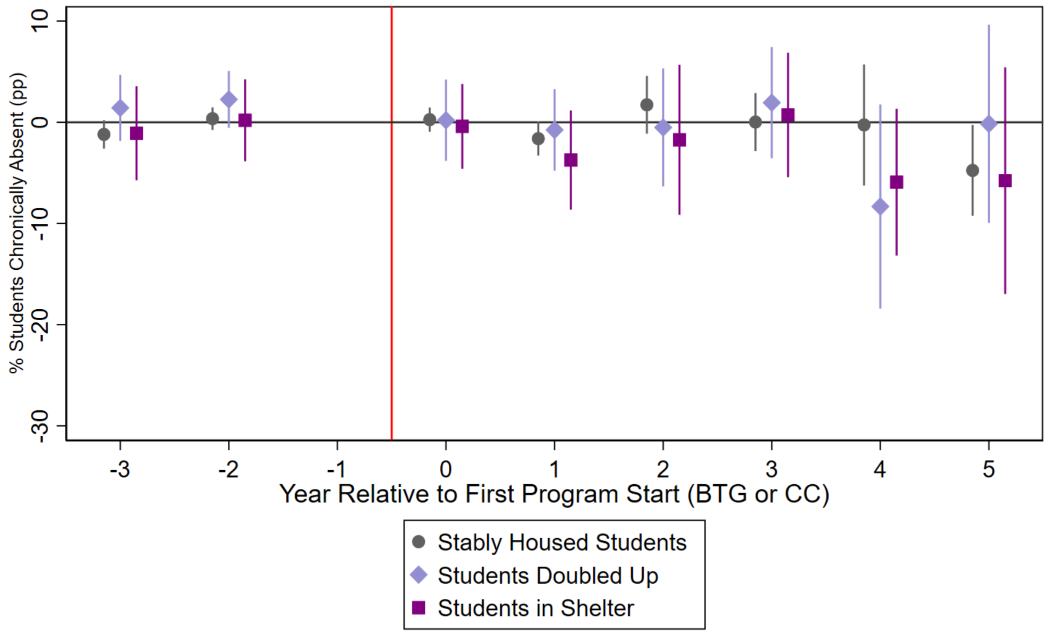
A. BTG Only Schools



B. CC Only Schools



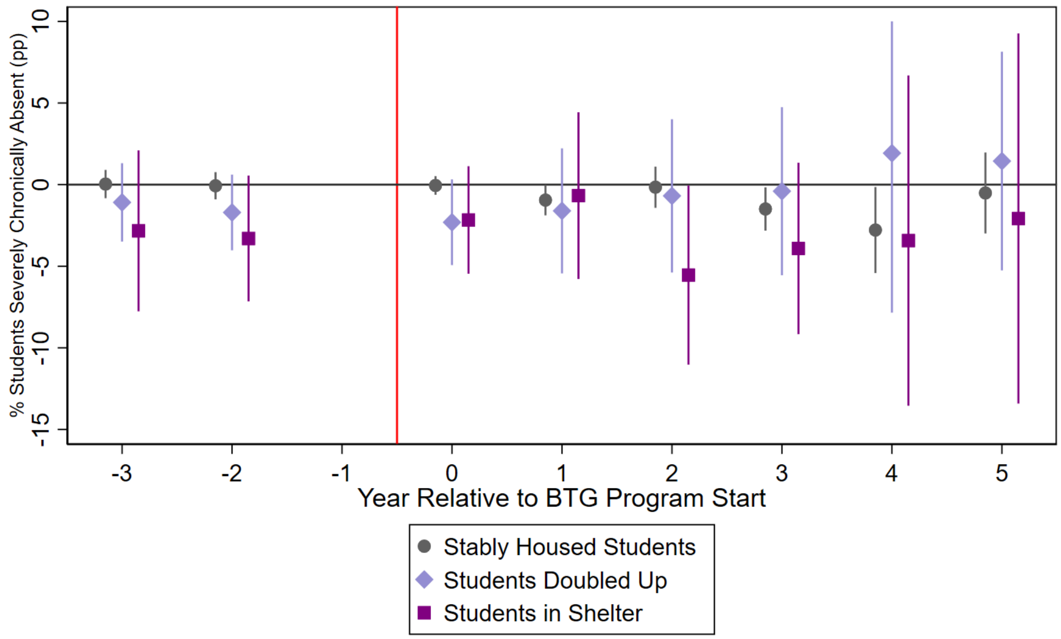
C. BTG+CC Schools



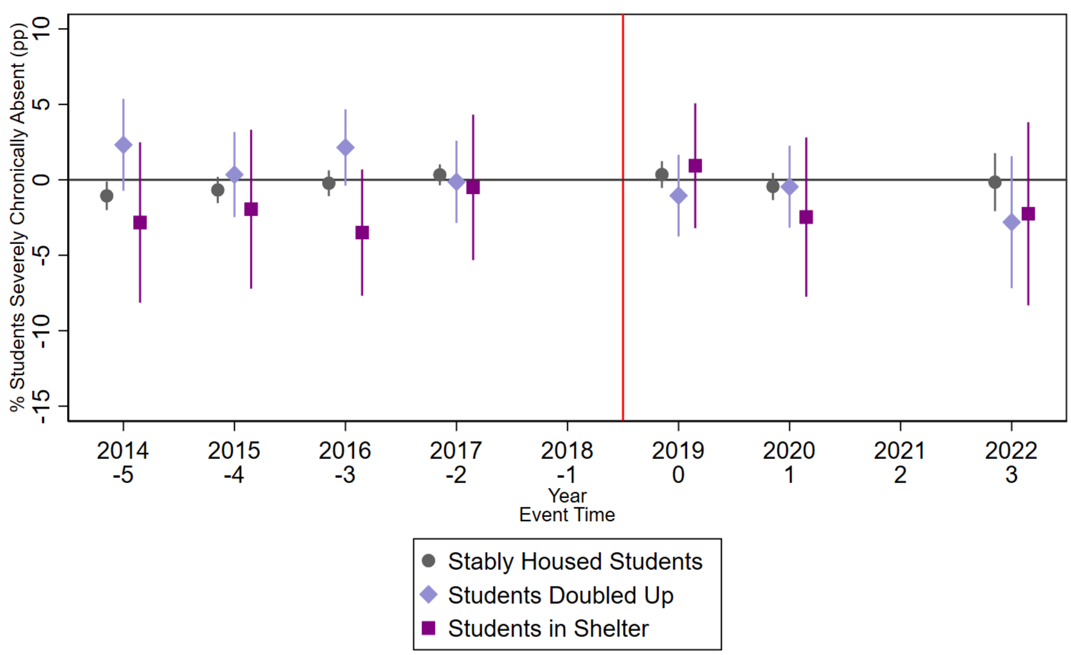
Note. These figures present estimates from the Callaway & Sant'Anna (2021) estimator. Vertical bars reflect 95% confidence intervals, which are trimmed to the dimensions of the graph where necessary.

Figure A2: Results for Severe Chronic Absenteeism

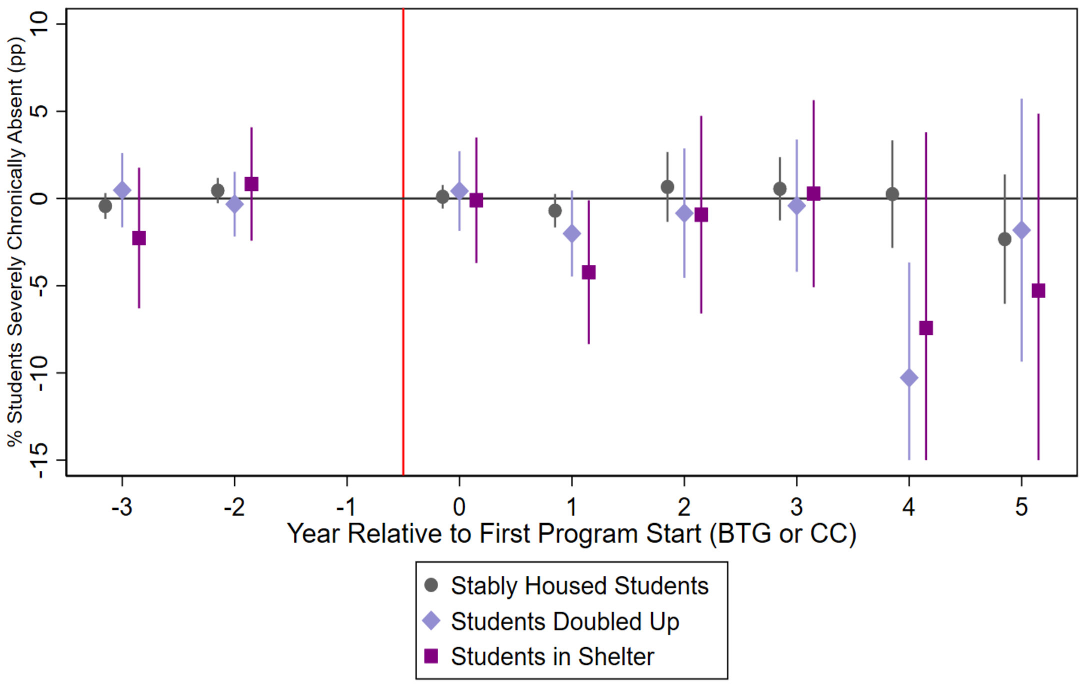
A. BTG Only Schools



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	**No Controls**	**Balanced Sample**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**
Pre-treat.	[illegible]	0.047	-0.030	-0.010	0.204	0.042	0.403	0.197	-0.120	-0.124	0.387	-0.158
avg.	[illegible]	(0.257)	(0.346)	(0.277)	(0.435)	(0.385)	(0.445)	(0.431)	(0.877)	(0.806)	(0.627)	(0.861)
Post-treat.	0.333	0.047	-0.030	0.337*	0.547	0.340	0.767	0.645	1.157	1.088**	0.732	1.127
avg.	(0.199)	(0.257)	(0.346)	(0.196)	(0.576)	(0.561)	(0.696)	(0.577)	(0.634)	(0.548)	(0.610)	(0.640)
Event time												
-6	-0.350	-0.474	-0.321	-0.344	0.002	-0.150	0.850	0.052	-1.059	-1.411	-0.320	-0.961
	(0.664)	(0.558)	(0.838)	(0.652)	(1.116)	(1.050)	(1.000)	(1.131)	(1.260)	(1.257)	(1.117)	(1.274)
-5	0.117	0.104	0.038	0.100	0.359	0.181	0.452	0.348	-0.548	-0.306	-0.264	-0.560
	(0.290)	(0.348)	(0.309)	(0.286)	(0.750)	(0.634)	(0.660)	(0.741)	(0.921)	(0.989)	(0.859)	(0.921)
-4	0.121	0.242	0.029	0.121	0.552	0.281	0.865	0.530	0.855	0.742	0.480	0.699
	(0.156)	(0.158)	(0.202)	(0.153)	(0.541)	(0.494)	(0.643)	(0.568)	(0.782)	(0.686)	(0.662)	(0.784)
-3	-0.049	-0.020	-0.150	-0.037	0.272	0.147	0.464	0.215	0.726	0.898	0.402	0.656
	(0.147)	(0.131)	(0.214)	(0.146)	(0.426)	(0.381)	(0.573)	(0.435)	(0.778)	(0.694)	(0.756)	(0.775)
-2	-0.043	-0.067	-0.058	-0.052	0.501	0.275	0.406	0.432	0.972	0.950*	0.768	0.920
	(0.158)	(0.144)	(0.181)	(0.157)	(0.405)	(0.370)	(0.487)	(0.409)	(0.630)	(0.561)	(0.553)	(0.655)
-1	.	.	.	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.	.	.	.
0	0.022	-0.027	0.007	0.011	0.479	0.369	0.705	0.624	0.217	0.412	0.056	0.178
	(0.109)	(0.104)	(0.134)	(0.107)	(0.480)	(0.460)	(0.509)	(0.476)	(0.569)	(0.510)	(0.624)	(0.573)
1	0.207	0.085	0.167	0.202	0.377	0.107	0.625	0.456	0.106	0.153	-0.424	0.048
	(0.160)	(0.150)	(0.216)	(0.157)	(0.740)	(0.710)	(0.824)	(0.743)	(0.742)	(0.619)	(0.773)	(0.755)
2	0.044	-0.212	-0.114	0.033	0.422	0.219	1.071	0.474	1.438	1.643**	0.462	1.392
	(0.247)	(0.250)	(0.302)	(0.244)	(0.705)	(0.674)	(0.706)	(0.696)	(0.937)	(0.820)	(0.933)	(0.933)
3	0.563	0.384	0.702**	0.552*	0.737	0.548	1.202	0.894	1.339	1.270	0.380	1.158
	(0.304)	(0.270)	(0.350)	(0.297)	(0.953)	(0.934)	(1.200)	(0.954)	(1.065)	(0.910)	(1.061)	(1.076)
4	1.104	0.342	0.826	1.152**	1.615*	1.192	1.107	1.467	2.415**	1.760	2.013	2.516**

	Stably Housed Students				**Students Doubled-Up**				**Students in Shelter**			
	Main Results	**No Controls**	**Balanced Sample**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**
	(0.583)	(0.549)	(0.570)	(0.575)	(0.893)	(0.952)	(1.264)	(0.927)	(1.172)	(1.182)	(1.207)	(1.208)
5	0.058	-0.465	0.184	0.072	-0.346	-0.392	-0.108	-0.042	1.426	1.291	1.906	1.472
	(0.475)	(0.450)	(0.613)	(0.467)	(1.282)	(1.257)	(1.492)	(1.284)	(1.579)	(1.475)	(1.219)	(1.601)

Note. Estimates here demonstrate attendance rate shifts 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 school-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. Estimates from the columns labeled "Full Sample" include all schools, with no comparison sample restrictions, as described in the text. Standard errors in parenthesis. ** $p < 0.05$, *** $p < 0.01$.

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**	**Full Sample**	**Main**	**No Controls**	**Balanced Sample**	**Full Sample**	**Main**	**No Controls**	**Balanced Sample**	**Full Sample**
Pre-treat	[illegible]	0.335	0.260	0.299*	[illegible]	-0.686**	-0.431	-0.812**	[illegible]	1.001	0.910	0.776
avg.	[illegible]	(0.177)	(0.183)	(0.155)	[illegible]	(0.343)	(0.406)	(0.367)	[illegible]	(0.539)	(0.619)	(0.545)
Post-treat.	0.144	0.013	0.242	0.159	0.006	-0.037	-0.536	0.011	0.554	0.805	0.492	0.589
avg.	(0.186)	(0.175)	(0.227)	(0.186)	(0.428)	(0.391)	(0.461)	(0.424)	(0.604)	(0.571)	(0.653)	(0.599)
2014	[illegible]	0.427**	0.495**	0.485**	[illegible]	-0.450	0.009	-0.428	[illegible]	1.303	0.931	0.950
	[illegible]	(0.203)	(0.237)	(0.194)	[illegible]	(0.439)	(0.530)	(0.483)	[illegible]	(0.765)	(0.866)	(0.818)
2015	[illegible]	0.370	0.400	0.433**	[illegible]	-0.618	-0.380	-0.752	[illegible]	1.531**	1.859**	1.310
	[illegible]	(0.197)	(0.226)	(0.192)	[illegible]	(0.441)	(0.485)	(0.487)	[illegible]	(0.766)	(0.820)	(0.826)
2016	[illegible]	0.358	0.121	0.227	[illegible]	-0.846**	-0.880	-1.154***	[illegible]	0.905	0.650	0.852
	[illegible]	(0.184)	(0.198)	(0.165)	[illegible]	(0.368)	(0.505)	(0.421)	[illegible]	(0.617)	(0.763)	(0.588)
2017	[illegible]	0.153	0.013	0.057	[illegible]	-0.223	-0.481	-0.470	[illegible]	-0.116	0.104	-0.183
	[illegible]	(0.159)	(0.189)	(0.137)	[illegible]	(0.406)	(0.436)	(0.432)	[illegible]	(0.597)	(0.674)	(0.620)
2018		.	.	.		.	.	.		.	.	.
		.	.	.		.	.	.		.	.	.
2019	[illegible]	-0.122	-0.055	-0.073	[illegible]	-0.466	-0.917**	-0.233	[illegible]	-0.103	-0.353	-0.234
	[illegible]	(0.177)	(0.223)	(0.187)	[illegible]	(0.475)	(0.457)	(0.527)	[illegible]	(0.604)	(0.757)	(0.642)
2020	[illegible]	0.323*	0.304	0.245	[illegible]	-0.084	-0.557	-0.133	[illegible]	1.106	0.786	1.183
	[illegible]	(0.175)	(0.247)	(0.193)	[illegible]	(0.441)	(0.584)	(0.491)	[illegible]	(0.680)	(0.877)	(0.728)
2021		.	.	.		.	.	.		.	.	.
		.	.	.		.	.	.		.	.	.
2022	[illegible]	-0.162	0.476	0.306	[illegible]	0.439	-0.133	0.399	[illegible]	1.413	1.043	0.817
	(0.342)	(0.344)	(0.407)	(0.342)	(0.640)	(0.599)	(0.696)	(0.656)	(0.963)	(0.926)	(0.881)	(0.984)

Note. Estimates here demonstrate attendance rate shifts for CC 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 school-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. Estimates from the columns labeled "Full Sample" include all schools, with no comparison sample restrictions, as described in the text. Standard errors in parenthesis. ** $p < 0.05$, *** $p < 0.01$.

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**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**
Pre-treat.	0.291	0.405***	0.204	0.285*	0.484	0.377**	0.811	0.489	0.305	0.686**	0.430	0.305
avg.	(0.154)	(0.118)	(0.196)	(0.154)	(0.357)	(0.179)	(0.452)	(0.356)	(0.477)	(0.309)	(0.474)	(0.476)
Post-treat.	0.116	-0.216	0.205	0.137	0.638	0.487	0.261	0.684	1.134**	1.539***	0.996	1.199**
avg.	(0.210)	(0.174)	(0.267)	(0.208)	(0.380)	(0.272)	(0.486)	(0.394)	(0.573)	(0.420)	(0.644)	(0.571)
Event time												
-6	0.418	0.485**	0.377	0.414	0.915	0.541	1.676	0.932	0.574	1.228**	0.694	0.592
	(0.285)	(0.246)	(0.352)	(0.282)	(0.704)	(0.390)	(0.927)	(0.718)	(0.892)	(0.603)	(0.826)	(0.879)
-5	0.578**	0.433**	0.367	0.585**	-0.260	-0.297	-0.042	-0.317	0.265	1.166**	0.574	0.398
	(0.235)	(0.184)	(0.304)	(0.234)	(0.471)	(0.356)	(0.571)	(0.472)	(0.738)	(0.501)	(0.717)	(0.734)
-4	0.223	0.202	-0.014	0.231	0.261	-0.014	0.331	0.241	0.970	1.277***	0.580	0.874
	(0.188)	(0.140)	(0.240)	(0.187)	(0.358)	(0.258)	(0.497)	(0.361)	(0.593)	(0.446)	(0.585)	(0.593)
-3	0.182	0.314**	-0.019	0.191	-0.095	0.106	-0.141	-0.143	0.767	0.702	0.250	0.859
	(0.154)	(0.137)	(0.187)	(0.153)	(0.296)	(0.208)	(0.453)	(0.303)	(0.584)	(0.395)	(0.564)	(0.588)
-2	-0.071	0.018	-0.208	-0.063	-0.380	-0.260	-0.317	-0.406	0.222	0.422	0.468	0.238
	(0.129)	(0.115)	(0.170)	(0.127)	(0.270)	(0.196)	(0.380)	(0.283)	(0.463)	(0.330)	(0.499)	(0.470)
-1	.	.	.	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.	.	.	.
0	-0.050	-0.099	-0.037	-0.033	-0.079	-0.051	-0.304	0.071	0.057	0.405	0.156	0.080
	(0.116)	(0.087)	(0.156)	(0.114)	(0.329)	(0.216)	(0.460)	(0.351)	(0.484)	(0.341)	(0.595)	(0.483)
1	0.315	0.308**	0.317	0.317	0.425	0.231	-0.470	0.458	1.202**	1.192***	1.063	1.219**
	(0.176)	(0.148)	(0.223)	(0.176)	(0.401)	(0.287)	(0.518)	(0.404)	(0.609)	(0.390)	(0.677)	(0.614)
2	-0.295	-0.535	-0.373	-0.300	0.530	0.334	0.113	0.633	0.472	1.166*	0.358	0.486
	(0.334)	(0.364)	(0.382)	(0.333)	(0.571)	(0.481)	(0.726)	(0.577)	(0.909)	(0.666)	(1.079)	(0.907)
3	-0.067	-0.348	-0.197	-0.065	0.267	0.236	-0.070	0.178	0.437	1.598***	0.460	0.325
	(0.349)	(0.297)	(0.394)	(0.346)	(0.611)	(0.440)	(0.751)	(0.620)	(0.809)	(0.568)	(0.911)	(0.802)
4	0.167	-0.438	0.560	0.245	2.014**	1.693**	1.637	1.818*	2.649	2.397	1.552	2.828**

	Stably Housed Students				**Students Doubled-Up**				**Students in Shelter**			
	Main Results	**No Controls**	**Balanced Sample**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**	**Main Results**	**No Controls**	**Balanced Sample**	**Full Sample**
	(0.619)	(0.503)	(0.847)	(0.613)	(0.999)	(0.822)	(1.284)	(1.017)	(1.411)	(1.299)	(1.358)	(1.441)
5	0.627	-0.186	0.962	0.659	0.669	0.477	0.658	0.945	1.986	2.474**	2.388	2.257
	(0.640)	(0.606)	(0.780)	(0.643)	(1.019)	(0.899)	(1.161)	(1.067)	(1.559)	(1.241)	(1.522)	(1.548)

Note. Estimates here demonstrate attendance rate shifts for BTG and CC 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 school-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. Estimates from the columns labeled "Full Sample" include all schools, with no comparison sample restrictions, as described in the text. Standard errors in parenthesis. ** $p < 0.05$, *** $p < 0$

Table A4. Years and Schools Contributing to BTG Event Time Estimates							
		Event Time					
BTG Cohort	**# of Schools in Cohort**	**0**	**1**	**2**	**3**	**4**	**5**
2017	17	2017	2018	2019	2020	2021	2022
2018	8	2018	2019	2020	2021	2022	2023
2019	2	2019	2020	2021	2022	2023	2024
2020	6	2020	2021	2022	2023	2024	2025
# of schools contributing to event time estimate		**33**	**27**	**31**	**19**	**8**	**17**

Note. Cells highlighted in green are the years of data contributing to each event time estimate. We do not use attendance data for 2021 (red cells) given inconsistencies in data collection due to schooling disruptions from the COVID-19 pandemic. Our data includes years 2013-2022, so we do not have data from 2023, 2024, or 2025 (gray cells).

Table A5. Years and Schools Contributing to BTG+CC Event Time Estimates

		Event Time					
BTG+CC Cohort	**# of Schools in Cohort**	**0**	**1**	**2**	**3**	**4**	**5**
2017	13	2017	2018	2019	2020	2021	2022
2018	5	2018	2019	2020	2021	2022	2023
2019	41	2019	2020	2021	2022	2023	2024
2020	1	2020	2021	2022	2023	2024	2025
# of schools contributing to event time estimate		**60**	**59**	**19**	**54**	**5**	**13**

Note. Cells highlighted in green are the years of data contributing to each event time estimate. We do not use attendance data for 2021 (red cells) given inconsistencies in data collection due to schooling disruptions from the COVID-19 pandemic. Our data includes years 2013-2022, so we do not have data from 2023, 2024, or 2025 (gray cells).