Long-term effects of integrated student support: An evaluation of an elementary school
intervention on postsecondary enrollment and completion

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Abstract

This study examines the effect of an Integrated Student Support (ISS) intervention on postsecondary enrollment and completion for predominantly low-income students of color. We use propensity score weighting to estimate the effect of receiving ISS during elementary school on postsecondary outcomes for 2009-2017 high school graduates from a high-poverty urban district. Students who received ISS have higher probabilities of enrolling in and completing postsecondary education, and higher probabilities of enrolling in and graduating from four-year (versus two-year) institutions, than the comparison group. Together, results suggest ISS as an effective intervention to reduce barriers to postsecondary education and, thereby, to potentially reduce income- and race-based disparities in postsecondary outcomes.

Keywords: elementary schools, postsecondary education, poverty, program evaluation, student development

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Carnevale et al. (2013) predicted that by 2020, 65% of U.S. jobs would require postsecondary education. Beyond a necessity for employment, postsecondary education is associated with myriad benefits, including greater full time employment (McFarland et al., 2019) and lifetime earnings (Baum et al., 2013). Benefits extend beyond the labor market; they include greater chances of social mobility (Haskins et al., 2009); health insurance coverage (Baum et al., 2013); greater participation in retirement plans, less reliance on public assistance, and greater exercise, voting, and volunteering (Ma et al., 2016). Despite a general increase in postsecondary education enrollment (Baum et al., 2013; Ma et al., 2016; McFarland et al., 2019), students from low-socioeconomic (SES) households and students of color disproportionately face barriers that limit their postsecondary opportunities. Indeed, students from low-SES households are less likely to enroll in and complete postsecondary education (Bailey & Dynarski, 2011; Haskins et al., 2009; McFarland et al., 2019; McLaughlin & Van Eaton, 2018; National Student Clearinghouse Research Center [NSCRC], 2017), as are Black and Hispanic students (Ma et al., 2016; McFarland et al., 2019; NSCRC, 2017, 2018; Velez et al., 2019). Such disparities have myriad deleterious effects. They contribute to unrealized talent in the labor market (Carnevale et al., 2013); perpetuate the stratification of individuals by income and race; and deny low-SES and students of color the experience of being in higher education, access to social capital, and opportunities for upward social mobility (Perna, 2007). It is therefore crucial to remove barriers to postsecondary opportunities for these students.

A main barrier is adequate academic preparation (Long, 2017; Page & Scott-Clayton, 2016), often an aftereffect of persistent structural inequities. Students from low-income households more often have limited school resources (e.g., Orlofsky, 2002) and experience poor instructional quality (e.g., Lankford, Loeb, & Wyckoff, 2002). Students of color may face racial

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biases in school and lowered teacher expectations (Boser et al., 2014; Gregory et al., 2010; McKown & Weinstein, 2008; Peterson et al., 2016). Out-of-school stressors, including food insecurity and lack of quality childcare (Author, 2008; Coley & Baker, 2013), increased mobility rates (Gruman et al., 2008), and unsafe neighborhood environments (Evans, 2004) negatively influence students' readiness to learn and school achievement (Yoshikawa et al., 2012). These persistent in-school and out-of-school inequities begin early and accumulate throughout schooling (Cunha et al., 2006; Dearing, 2008; McLoyd, 1998; Noguera et al., 2019; Reeves & Halikias, 2017; Restuccia & Urrutia, 2004), which underscores the need to facilitate academic preparation by supporting students early, increasing access to resources, and supporting students holistically. Indeed, interventions that prevent or mitigate disparities that lead to later differences in postsecondary attainment may be more effective than intervening once disparities exist (Campbell & Ramey, 1994; Ramey & Ramey, 2004).

Elementary school-based integrated student support (ISS) interventions offer one such approach. In this study, we examine the effects of an ISS intervention that supports academic success for students who attend high-poverty urban schools. We examine differences in the probability of enrolling in and completing postsecondary education for students who received the ISS intervention in elementary school compared to their non-intervention peers.

Integrated Student Support

ISS focuses on supporting the whole child, and adopts ecological and life course perspectives (Author, 2002; Moore et al., 2017). ISS integrates developmental science with knowledge of best-practice frameworks for student support, which include situating student support at the core of the school, systematically addressing students' strengths and needs, and partnering with community agencies to provide access to services (Adelman & Taylor, 2005).

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ISS may be used as an umbrella term (e.g., Moore et al., 2017; Moore & Emig, 2014) to refer to initiatives that broadly address five core components: assessing needs, developing partnerships, coordinating student support, integrating into the school, and tracking data. However, when used this way, the term encompasses programs that vary widely in structure and implementation.

A few examples illustrate this heterogeneity. Communities in Schools (CIS) seeks to support students' academic achievement by building a community of support. Through organizational capacity building, CIS connects with partners to identify programs that will benefit groups of students across grades, and to integrate programs, enrichment opportunities, trainings, and workshops into the school day (Figlio, 2015). By bringing some community group opportunities into the school, CIS reaches most students; those with more intensive needs get targeted or individualized support (Communities in Schools, 2020). As another example, Dobbie and Fryer (2011) describe the Harlem Children's Zone (HCZ) as supporting student achievement by blending community and school programming. Community programs, such as early child care, tax help, foster care prevention, and a college success office, enrich the neighborhood environment. HCZ charter schools combine learning supports like an extended school day, extended school year, and Saturday school for math and ELA remediation, with more holistic supports inside the school, such as food baskets, bus fare, and medical, dental, and mental health services. As a final example, the Comer School Development Program (Comer, 1992, 2005) is a school-wide intervention that focuses on changing the school environment through building collaborative relationships among educational stakeholders. The goal is to address academic achievement by improving students' interpersonal relationships and creating a positive school environment, which in turn supports healthy development and learning. In sum, broadly conceived, ISS programs vary across several dimensions, including focus (neighborhood, school,

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students), degree of individualization of programming, and location of and responsibility for services (part of the school, brought into the school, provided by community agencies).

Emerging evidence suggests that ISS effectively supports students' development and academic achievement, but that effectiveness varies by program and approach (for a review, see Moore et al., 2017). In addition to looking across varied implementation to examine the effectiveness of ISS broadly, research on specific models is needed. Studying the effectiveness of specific ISS models can illuminate which approaches may be most effective, and whether effectiveness persists beyond the intervention to support long-term outcomes.

[Intervention]

In this study, we focus on [Intervention], an ISS approach that promotes academic achievement for students in high-poverty urban schools by addressing students' strengths and needs across multiple domains: academic, health, family, and social-emotional/behavioral (SEB). The intervention's premise is that, to thrive, every student needs tailored, comprehensive enrichment and support services that address both in-school and out-of-school factors related to learning. [Intervention] serves every student in a school and provides tailored services at the student-level, differing from school-based initiatives that focus on changing the school environment and climate (e.g., Cook, Murphy, & Hunt, 2000) or making the school a community hub to support youth, families, and the community (e.g., Coalition for Community Schools at the Institute for Educational Leadership, 2019). Tailored services address the necessity to support both strengths and needs as way to promote resilience (Masten & Tellegen, 2012), and the reality of child development as a unique trajectory of dynamic interactions between the child and their environment (Cicchetti & Sroufe, 2000; Sameroff, 2009).

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Services are comprehensive across domains and levels of need. As some examples, student and family services include academic support, summer programming, socio-emotional/behavioral lessons, violence prevention, counseling, language services, physical and mental health services, and crisis intervention. For example, a student who has good attendance and strong parental advocacy, but struggles with behavioral and hygiene challenges, family homelessness, and below-grade-level academic achievement, may receive parent support resources, counseling, clothing, and academic interventions (Author, 2020).

Schools implementing [Intervention] have a coordinator, a master's-trained, licensed school counselor or social worker, who promotes student achievement through three main tasks. First, early in the school year, the coordinator conducts a Whole Class Review, a meeting with every homeroom teacher to discuss every student. Using a semi-structured interview, the coordinator and teacher determine the strengths and needs of each student across academic, health, family, and SEB domains. Second, the coordinator and teacher collaboratively develop a support plan for every student based on their unique strengths and needs. The coordinator then arranges services, drawing from their extensive network of community partners, and communicates with each student's family to ensure the supports are in place. Third, students with the highest level of need (about 8-10% of students) receive an Individual Student Review, in which the coordinator and a larger team (e.g., school psychologists, teachers, administrators, community-based professionals) collaboratively develop a goal-based plan to ensure the student receives the needed supports.

Importantly, throughout the year the coordinator maintains contact with families and builds relationships with community partners. Coordinators also record student data into a student support information system, through which intervention implementers track students

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over time and track program fidelity – that students receive services and implementation is consistent across schools. Coordinators receive support from a district-level program manager, who leads coaching and professional development throughout the school year.

The Effectiveness of [Intervention]

Studies of [Intervention] suggest it effectively supports students' academic achievement. Prior studies have focused on student-level effects; the intervention is adopted at the school level, but through tailoring, consists of a set of student-level treatments that are clustered within schools. Additionally, the number of [Intervention] and comparison schools in participating districts is not large enough to power a school-level analysis. In one study, Author (2014) compared students in [Intervention] schools in a high-poverty urban district to students in a randomly-selected, closely-matched subset of comparison schools in the same district, to examine differences in report card and standardized test scores from third to eighth grade. The authors used propensity score methods to adjust for pre-treatment differences between groups. Students who received [Intervention] in elementary school had higher elementary school report card grades and higher standardized English language arts and math test scores in middle school, on average, compared to their non-[Intervention] peers.

Author (2016a) examined differences in fifth grade standardized math and reading performance between first generation immigrant students who did and did not receive the intervention. Using a within-schools design, the authors compared students who attended an [Intervention] school when it implemented the intervention to students in the same school when it was not implementing the intervention. Students who received [Intervention] had higher standardized reading and math scores compared to their peers who never received the intervention. Further, the authors found a dose-response relation in which students who received

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the intervention for two or more years had higher performance than students who received the intervention for one year. Authors (2016b) found a similar relation, in which [Intervention] students had higher sixth grade standardized math scores than their non-[Intervention] peers, and who demonstrated greater growth in reading, English language arts, and math from third to sixth grade than the comparison group. Extending prior studies, Authors (2018) used discrete event history analysis with propensity score weighting to examine potential differences in the probability of high school dropout between students in a high-poverty urban district who received [Intervention] in elementary school and their non-[Intervention] peers. Students who received the intervention were about half as likely as their non-[Intervention] peers to drop out of high school. In sum, prior research using varied samples, analytic approaches, and outcomes suggests positive effects of [Intervention] on student achievement through high school.

By extension, [Intervention] may promote enrollment in and completion of postsecondary education for students in high-poverty urban schools through several complementary mechanisms. Because the experience of living in poverty negatively impacts multiple domains related to academic success, including cognition, socioemotional development, and mental and physical health (Dearing, 2008; Noguera et al., 2019; Yoshikawa et al., 2012), comprehensive interventions that address multiple developmental domains and target both in-school and out-of-school factors may lead to longer-lasting impacts on academic achievement and educational attainment. With [Intervention], teachers feel more supported, better equipped to handle student behavior, and have an increased understanding of students' out-of-school challenges (Author, 2017), which may positively affect student-teacher relationships, learning environments, and the school community. Students' tailored supports may help reduce student and family stress, and support students' engagement and skill development. In turn, intrinsic motivation and learning

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efficiencies may facilitate further skill-development (Heckman, 2008). In short, by changing students' developmental and learning environments, [Intervention] may change students' academic trajectories to better position them to take advantage of postsecondary opportunities.

The Present Study

In this study, we examine the effect of [Intervention] on postsecondary outcomes. We merge 16 years of longitudinal data from a large, high-poverty urban school district with student-level intervention data and college-going data to estimate the effect of participation in [Intervention] on the probabilities of postsecondary enrollment and completion, and on the type of institution (four-year versus two-year) where students may enroll and complete. We use propensity score-weighted logistic regression to compare outcomes for students who participated in [Intervention] during at least one elementary school year to a comparison group of students who never participated. We ask four research questions:

- RQ1. Do [Intervention] students have a higher probability of enrolling in a postsecondary education institution than their non-[Intervention] peers?
- RQ2. For students who enroll, do [Intervention] students have a higher probability of enrolling in a four-year (versus a two-year) institution, compared to non-[Intervention] students?
- RQ3. For students who enroll, do [Intervention] students have a higher probability of completing postsecondary education than their non-[Intervention] peers?
- RQ4. For students who complete postsecondary education, do [Intervention] students have a higher probability of doing so from a four-year (versus a two-year) institution, compared to non-[Intervention] students?

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This study is the first to estimate the effect of an elementary-school ISS intervention on postsecondary outcomes, generating new knowledge of the downstream effects of elementary school interventions generally and on the ISS approach specifically. Such knowledge dovetails with recent attention and investments in ISS and comprehensive K-12 supports (e.g., Every Student Succeeds Act, 2015), illuminating whether ISS may also be an effective strategy to increase postsecondary opportunities for students of color from low-income backgrounds.

Method

Data

The analyses draw on three sources of data. A district-provided dataset contributes longitudinal, anonymized student-level school records from 2001-2017, for all students who graduated from a large, urban school district in the northeastern U.S. during the years 2009-2017 ($n = 26,182$). These data include students' school attendance records, demographic characteristics (e.g., race, gender), academic performance measures (e.g., GPA, scores on English language arts and mathematics state assessments), and student identifiers. We use intervention data to determine whether each student received the intervention (i.e., dose), the time frame for the intervention, and the number of school years that the student participated (i.e., dosage).

Postsecondary enrollment and completion data come from the National Student Clearinghouse (NSC), a non-profit organization that tracks enrollment and graduation at Title IV, degree-granting postsecondary institutions (e.g., Dundar & Shapiro, 2016). The NSC matches high school graduates to postsecondary enrollment and completion data using student names and dates of birth (NSCRC, 2014). According to the NSC Research Center (2018a), overall coverage rates (i.e., the percentage of all Integrated Postsecondary Education Data System enrollments at

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Title IV, degree-granting institutions) were high and increased from 92.5% to 97% from 2010-2017. Coverage rates during this time increased for the northeast U.S. region (i.e., 94.6-97.6%) and the state where the intervention took place (i.e., 97.6%-98.3%). These data suggest close to full coverage on postsecondary enrollment and completion patterns for the sample.

Sample

Study participants include students attending schools in a large, urban public school district that primarily serves students of color who live in high-poverty neighborhoods. During the years that students may have received the intervention, adoption in the district spread, and up to 21 schools implemented [Intervention] each year. Eight percent of 2009-2017 high school graduates in the district participated in [Intervention] during at least one elementary school year. Figure 1 shows the percent of all [Intervention] students that received the intervention in each grade. As the figure shows, students most often received the intervention when they were in later elementary school grades. All students received the intervention during at least one grade from kindergarten to fifth grade.

As we mention above, the sample includes all students in the district who graduated from high school. Since there is no evidence that students in the district who drop out of high school routinely get a GED and pursue postsecondary education, we exclude students who are not high school graduates. Given that prior research (Author, 2018) shows that more comparison students in the district do not graduate from high school, we acknowledge that conditioning on high school graduation has the potential to downward-bias the effect of [Intervention].

The analytic sample varies by research question. To examine the impact of [Intervention] on postsecondary enrollment (RQ1), we include high school graduates from the 2009-2017 cohorts ($n = 26,182$; this excludes students who received instruction in a substantially separate

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classroom due to potential non-standard requirements for high school graduation). To determine whether institution type for first enrollment differs based on [Intervention] treatment (RQ2), we use an analytic sample of high school graduates from the 2009-2017 cohorts who enrolled in a postsecondary institution; that is, students whose postsecondary record is in the NSC database ($n = 20,530$).

To examine the relation between [Intervention] and postsecondary completion (RQ3), we use an analytic sample of high school graduates from the 2009-2013 cohorts who enrolled in a postsecondary institution ($n = 12,733$). These sample restrictions ensure that students had at least four years after high school graduation to complete postsecondary education, and account for the fact that postsecondary completion is conditional on enrollment. To examine the impact of the intervention on graduation institution type (RQ4), we use an analytic sample of students who completed postsecondary education ($n = 5,824$). Table 1 provides descriptive statistics, by condition, for each analytic sample.

Measures

Participation in [Intervention].

We capture student participation in [Intervention] using a dichotomous indicator *TRT*. [Intervention] students belong to the treatment group if they ever received the intervention (participated = 1; did not participate = 0). Students participated in [Intervention] for an average of 2.6 years (SD = 1.7, range: 0.3-8). Table 1 summarizes the average participation in [Intervention] for each analytic sample (see also Figure 1).

Covariates. For all analyses, we include cohort fixed effects for high school graduation year: 2009-2017 for postsecondary enrollment and 2009-2013 for completion. We also control for key demographic variables, which include gender, race, special education status, eligibility

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for free or reduced-price lunch, and English learner (EL) status. We include a set of dichotomous indicators for students' most severe special education designation across all years, which is based on the amount of time students spend in a general education classroom (i.e., less than 25% time outside of GE; 25-65% of time out of GE). As a proxy for SES, we use two dichotomous variables to indicate whether students ever qualified for reduced-price or free lunch. For English learner status, we code a value of 1 if the student was ever enrolled in secondary English instruction. Table 1 provides descriptive statistics for covariates.

Outcomes.

Postsecondary enrollment. We use a dichotomous indicator, E_i , to represent whether a student enrolled in a postsecondary institution for at least one semester (enrollment = 1; no enrollment = 0); that is, if the student has an NSC record during any semester from 2009-2017 for any institution type (i.e., two-year, four-year). The majority (89%) of students enrolled within one year after graduating from high school. This limited the variability in time of first enrollment, precluding analyses of enrollment timing. Table 2 provides descriptive statistics for postsecondary education outcomes for the full sample.

Postsecondary completion. We use a dichotomous indicator, C_i , to represent whether a student attained a postsecondary credential (i.e., certificate, associate's degree, bachelor's degree) from any institution in four or more years after high school graduation (completed = 1; not completed = 0; see Table 2). While some reports may define time-to-degree as 150% of standard time to degree (i.e., six years after high school graduation; McFarland et al., 2019; McLaughlin & Van Eaton, 2018; NSCRC, 2017), we use a time frame of four or more years after high school graduation to maintain sufficient sample size since several cohorts had not yet been out of high school for six years. Nationally, about 44% of 2015-2016 bachelor's degree

recipients graduated within four years (Velez et al., 2019), suggesting that it is reasonable to include students who are four or more years post-high school graduation to estimate college completion effects.

Institution type. NSC provides institution type classifications for each institution that students attend. We use a dichotomous indicator, T_i , based on whether the postsecondary institution the student attended is a four-year or two-year institution (four-year = 1; two-year = 0; see Table 2). For enrollment analyses, we use the institution type in which the student first enrolled after high school. For the postsecondary completion analyses, we use the type of institution from which the student graduated. For students who first graduated from a two-year institution and subsequently graduated from a four-year institution, we code the latter.

Data Analysis

Propensity scores. Given the absence of random assignment of schools to intervention versus control conditions, selection bias is a potential concern. To address potential selection bias, we compare intervention and comparison groups across a range of observed student-level covariates associated with postsecondary outcomes and utilize student-level propensity score weights when estimating our outcome models (Guo & Fraser, 2010; Rosenbaum & Rubin, 1983).

We follow prior evaluations of [Intervention] (e.g., Author, 2014, 2018) and estimate student-level propensity scores, for several reasons. First, each student receives a tailored set of enrichments and supports, such that the intervention consists of student-level treatments within a school. Second, students must enter an [Intervention] school to receive the intervention; factors (e.g., family income, parent education, location of housing) that contribute to which school in the district a student enters are at the student-level rather than the school-level. Third, the estimand

of interest – the average difference between [Intervention] students and comparison students – is at the student-level, and so covariate balance is required at the student-level.

Although the propensity score model does not account for the nesting of students in schools (i.e., students entered the district at different times, and received the intervention across different grades, schools, and years, which precludes the ability to nest each [Intervention] and comparison student in one school for propensity score estimation), we account for the multilevel structure of the data. Specifically, we follow the recommendation made by Li et al. (2013) to use robust standard errors for all outcomes analyses.

We estimate propensity scores for each student in the full sample via generalized boosted regression (GBR) using the *twang* package (Ridgeway et al., 2020) in R. GBR creates a highly predictive model through an iterative process of combining regression models, by fitting each subsequent model to the errors of the previous one. Because the final predictive regression model is formed from the linear combination of regression trees, it has the same advantages as traditional regression tree approaches, such as flexibility in handling different data types and the ability to capture non-linear effects and interactions (McCaffrey et al., 2013). As a nonparametric procedure, GBR offers a significant advantage over the traditional logistic regression approach for estimating propensity scores. To estimate unbiased treatment effects using propensity scores, the propensity score model must specify the correct functional form of the relation between the covariates and treatment (Drake, 1993), but in practice the functional form is unknown. Propensity score estimation via logistic regression imposes a functional form on the data, whereas GBR does not, relaxing this assumption and protecting against propensity score model misspecification.

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We estimate propensity scores using available factors that predate the intervention: a dummy indicator of membership of student i in the intervention group (TRT_i); gender ($Male_i$), indicating the gender of student i (female = 0, male = 1); race ($Race_i$), which is a set of student-level dichotomous indicators including Black, Hispanic, White, Asian, and Multi-Racial/Other (non-group membership = 0, group membership = 1), in which White is the reference category; special education status ($SpecialEd_i$), which is a set of student-level dichotomous indicators (no = 0, yes = 1) as defined above; free lunch status ($Free\ lunch_i$), indicating if student i was eligible for free school meals; reduced lunch status ($Reduced\ lunch_i$), indicating if student i was eligible for reduced-price school meals; missing lunch ($Missing\ lunch_i$), indicating if a student was missing information regarding free and reduced-price school meal eligibility (no = 0, yes = 1); and English learner status (EL_i), a dummy covariate indicating if a student is an English language learner (no = 0, yes = 1).

These pre-intervention observables overlap with factors in prior non-experimental studies that examine long-term effects of interventions for children (e.g., Garcés et al., 2002; Palfrey et al., 2005; Reynolds et al., 2018). Some prior studies included additional observables unavailable to us like birth weight (Garcés et al., 2002), other measures of SES (e.g., maternal employment or education), or factors that are not applicable to the present analysis (e.g., resides in high-poverty neighborhood; Reynolds et al., 2018). This suggests that, while not without limitation, the available factors capture the major selection factors that may contribute to students' propensity to receive [Intervention]. Further, we include special education status because [Intervention] is not designed to impact students' special education diagnosis. However, to check for potential propensity score model misspecification, we also estimated propensity scores excluding special education status and refit outcomes models. Results were substantively

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unchanged; below we present results that include special education status. Figure 2 shows the distributional overlap of propensity scores for intervention students and students in the comparison group.

We calculate student-level unstabilized and stabilized Average Treatment Effect (ATE) propensity weights (Thoemmes & Ong, 2016) as follows:

$$Treatment = \frac{1}{P(TRT=1|X)}; Control = \frac{1}{1-P(TRT=1|X)} \quad (\text{Unstabilized Weights})$$

$$Treatment = \frac{P(TRT=1)}{P(TRT=1|X)}; Control = \frac{1-P(TRT=1)}{1-P(TRT=1|X)} \quad (\text{Stabilized Weights})$$

where TRT is defined as above and X represents a vector containing the following student-level covariates: *Male*, *Race*, *SpecialEd*, *Free lunch*, *Reduced lunch*, and *EL*. Given that subjects with very large or small propensity scores can result in extremely large weights, we calculate stabilized weights. Stabilized weights use the marginal probability of treatment and control to address extremely large propensity score weights resulting from very large or small propensity scores (Austin & Stuart, 2015; Thoemmes & Ong, 2016). Such an approach adjusts large propensity score weights more naturally than other approaches, such as trimming, because it obviates the need to arbitrarily choose cutoff points (Thoemmes & Ong, 2016).

To assess covariate balance and the success of the weighting, we compare [Intervention] students and non-[Intervention] students on demographic variables both before and after propensity score weighting. We use the standardized bias method for covariate balance, calculating standardized mean differences for each demographic variable.

Outcomes model. We incorporate ATE propensity weights into a series of fixed-effect logistic regression models, one for each outcome across research questions. We fit the following model to measure the impact of [Intervention] on postsecondary enrollment, completion, and institution type:

$$p(Y_i = 1) =$$

$$\frac{1}{1 + e^{-(\beta_0 + \beta_1 TRT_i + \beta_2 Cohort_i + \beta_3 Male_i + \beta_4 Race_i + \beta_5 SpecialEd_i + \beta_6 Free\ lunch_i + \beta_7 Reduced\ lunch_i + \beta_8 EL_i)}}$$

in which the outcome Y , represents E_i , C_i , or T_i for effects on enrollment, completion, and institution type, respectively. $Cohort_i$ is a set of student-level dichotomous indicators for high school graduation year for student i , with cohort 2009 as the reference group. All other variables are as defined above. To determine a significant [Intervention] treatment effect, we examine the regression coefficient associated with TRT_i and its associated p -value, with p -values less than 0.05 indicating a statistically significant relationship. We examine odds ratios via exponentiation of the TRT_i regression coefficient to provide the odds of experiencing each outcome relative to the comparison group.

Sensitivity analyses. A concern with analytic techniques that involve propensity scores is the omission of relevant variables from the propensity score model (e.g., Miller, Henry, & Votruba-Drzal, 2016). Such an omission may result in biased treatment effect estimates and therefore it is necessary to examine whether the results from models employing propensity scores are robust to omitted variable bias. In line with recent calls for such analyses in child development research (Dearing & Zachrisson, 2019), we conduct sensitivity analyses to examine the degree to which findings are robust to potential unmeasured confounding. In other words, sensitivity analyses help quantify how skeptical or confident we should be that our estimates are causal.

We examine sensitivity using VanderWeele and Ding's (2017) E-value. E-values are on risk ratio scales; an E-value of 1.5, for example, indicates that an unmeasured confounder would need to increase the relative risk of exposure to the treatment and the relative risk of experiencing the positive outcome by 1.5 times above and beyond what is explained by

measured covariates. A strength of the E-value is that it is designed specifically for logistic models. To compute E-values, we follow VanderWeele and Ding's (2017) recommendation for odds ratios for common outcomes, using the squared odds ratios from the weighted logistic models described above.

Results

Covariate Balance

In Table 3 we provide covariate balance statistics between intervention and comparison groups before and after propensity score weighting. Shaded cells denote standardized differences that are close to or larger than 0.25 standard deviations, a guideline for imbalance between treatment and comparison groups (Harder, Stuart, & Anthony, 2010; What Works Clearinghouse [WWC], 2017). As Table 3 shows, prior to ATE propensity score weighting, the two groups differ in their percentage of students who are Black and Asian. After weighting, all mean standardized differences between groups are less than 0.05 standard deviations, which satisfies WWC requirements for baseline group equivalence (WWC, 2017).

Effect of [Intervention] on Postsecondary Enrollment

In Table 4 we provide estimates of the treatment effect on enrollment, enrollment institution type, completion, and completion institution type, controlling for cohort and demographic characteristics. For each outcome, we provide estimates from unweighted models and propensity score weighted models with stabilized weights. We also provide the full set of parameter estimates including cohort fixed effects, covariates, and goodness-of-fit statistics for unweighted models and propensity score weighted models with unstabilized and stabilized weights in the appendix, in Tables A1-A4. Here, we focus interpretation on the magnitude and effect size of treatment coefficients in propensity score weighted models with stabilized weights.

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Figure 3 displays the average fitted probability for each outcome, separately for the intervention and comparison groups.

As Table 4 shows, there is a statistically significant effect of [Intervention] on postsecondary enrollment. The coefficient associated with [Intervention] treatment for postsecondary enrollment is positive across both unweighted and stabilized weighted analyses, indicating that [Intervention] students have greater odds of postsecondary enrollment than their non-[Intervention] peers, 1.18 times the odds of non-[Intervention] students. In other words, there is a 18% increase in the odds of enrolling in college for [Intervention] students compared to non-[Intervention] students. As Figure 3 shows, the intervention is associated with a three percentage point difference in the probability of postsecondary enrollment, controlling for cohort and demographics.

There is a positive, statistically significant effect of [Intervention] on the institution type in which students first enroll (Table 4). The regression coefficient associated with treatment is positive across both unweighted and weighted analyses, indicating greater log-odds of enrolling in a four-year (versus a two-year) college for [Intervention] students than for the comparison group. [Intervention] students have 1.54 times the odds of non-[Intervention] students for enrolling in a four-year college as opposed to a two-year college, or a 54% increase in odds of enrolling in four-year college. Panel (b) of Figure 3 shows that, conditional on enrollment, participation in the intervention is associated with an eight percentage point difference in the probability of first enrolling in four-year (versus two-year) institution, controlling for cohort and demographics.

Effect of [Intervention] on Postsecondary Completion

Table 4 displays the effect of [Intervention] on postsecondary completion, for students who enrolled. [Intervention] students have 1.49 times the odds of non-[Intervention] students for completing postsecondary education, which corresponds to a 49% increase in the odds of completion. For students who completed postsecondary education, [Intervention] students have 1.57 times the odds of graduating from a four-year institution (versus a two-year institution) than their non-[Intervention] peers, a 57% increase in the odds of completing college at a four-year institution. Figure 3, panels (c) and (d) display the average fitted probabilities for post-secondary completion and institution type of completion, controlling for cohort and demographics. As the figures show, the intervention is associated with a ten percentage point positive difference in postsecondary completion, and a four percentage point positive difference in graduation from a four-year (versus two-year) institution, conditional on enrollment, and controlling for cohort and demographics.

Sensitivity analyses

We calculated E-values to examine the robustness of the results to omitted variable bias. E-values were 1.39 for postsecondary enrollment, 1.79 for enrollment institution-type, 1.74 for postsecondary completion, and 1.82 for completion institution type. The E-value indicates the degree of association on the risk-ratio scale that an unmeasured confounder would need to have with both the treatment and the outcome above and beyond what is explained by measured covariates to reduce the observed treatment effect to zero (VanderWeele & Ding, 2017). For example, to reduce the effect of [Intervention] on postsecondary completion to null, each unmeasured confounder would need to be associated with receiving [Intervention] and with postsecondary completion by risk ratios of at least 1.74-fold. The strongest covariate in this

model ($OR_{Asian} = 1.61$, see Table A3) has a risk ratio of 1.16. Thus, the unmeasured confounder would have to be stronger than any of the measured covariates. This pattern holds for all four E-values; in each model, an unmeasured confounder would need to be stronger than the strongest measured confounder. The E-value approach suggests that a relatively strong causal omitted variable would be required to reduce the effect of [Intervention] to zero.

Discussion

Despite nationwide increases in postsecondary enrollment and completion, disadvantaged students both enroll in and complete college at lower rates than their more advantaged peers (Haskins et al., 2009; Ma et al., 2016; McFarland et al., 2019; McLaughlin & Van Eaton, 2018; NSCRC, 2017). These disparities are the aftereffect of persistent inequities that contribute to performance differences throughout schooling (Bohrnstedt et al., 2015; Hemphill & Vanneman, 2011) and earlier (Coley, 2002). Elementary school interventions that target the multiple domains that affect academic preparation (Dearing, 2008) – a key barrier to postsecondary opportunities (Adelman, 1999) – may have lasting effects that impact postsecondary enrollment and completion. Here, we examine whether one such intervention, [Intervention], affects postsecondary outcomes for students who are majority students of color who attend high-poverty schools. We find that students who received the intervention in elementary school have a higher probability of enrolling in and completing postsecondary education, and a higher probability of enrolling in and graduating from four-year versus two-year institutions, than students in the comparison group. Results are consistent across models with and without propensity score weighting, and appear robust to omitted variable bias.

Longitudinal Effects of Elementary-School Interventions

This study contributes to the growing literature on the effects of elementary school interventions on postsecondary outcomes. Evidence suggests that light-touch interventions focused on in-class factors such as reading instruction and development of students' behavior skills do not affect postsecondary outcomes (Hemelt et al., 2013). However, more intensive interventions may be more effective. As Tennessee's Project STAR shows, small class size in elementary school causes higher postsecondary enrollment and completion, with larger effects for Black students and students who attend low-income schools (Chetty et al., 2011; Dynarski, Hyman, & Schanzenbach, 2013). The Chicago Longitudinal Study shows positive effects of the Child-Parent Center program, a school-based program that provided academic, health, and family support to low-SES children of color through the third grade during the early- to mid-1980s in Chicago (Reynolds et al., 2007). By age 35, intervention participants had a 48% higher rate of degree completion (i.e., associate's or above) than the comparison group (Reynolds et al., 2018). We extend this work to show that, starting in elementary school, a school-based, tailored intervention that simultaneously addresses a range of out-of-school barriers to learning has positive effects on the probability of postsecondary enrollment and completion for low-SES students mostly of color.

Results further support the positive effects of the ISS approach broadly. Relatively few studies exist that examine long-term effects of ISS on postsecondary educational attainment or beyond. Extending their review, Moore et al. (2017) used the social genome model to simulate long-term outcomes using existing studies of ISS. Based on current evidence of ISS' effectiveness, they estimate that simulated attainment of a four-year degree by age 25 would be up to 1.9 percentage points higher for students who receive ISS than for comparison students,

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controlling for high school graduation, race, gender, SES, prior achievement, and socio-emotional-behavioral variables throughout development. With similar demographic controls and conditioning on high school graduation, we find a probability of postsecondary completion that is 10 percentage-points higher, and a probability of postsecondary completion at a four-year institution that is four percentage-points higher for [Intervention] students than comparison students. The different analytical approaches preclude directly comparable estimates, but provide encouragement that [Intervention]’s specific ISS approach may support postsecondary education opportunities more than anticipated.

Potential Mechanisms of ISS

What might drive the differences between [Intervention] and comparison students’ probabilities of postsecondary enrollment and completion? For students in high-poverty schools, tailored ISS across family, health, SEB, and academic domains may engage several interrelated mechanisms that support student learning over time. For example, supporting students across domains increases access to health care and myriad material resources, and may lessen family and student stress, factors that support cognitive development and academic achievement (Yoshikawa et al., 2012). Indeed, previous research suggests that [Intervention] students have lower academic achievement at baseline compared to students who never received the intervention (Author, 2014), but later evidence higher elementary school grades (Author, 2014), higher middle school standardized test scores in reading and math (Author, 2014; Author, 2016a; Author, 2016b), and a higher probability for high school graduation (Author, 2018) than non-intervention peers. [Intervention] may facilitate elementary school learning that is self-reinforcing, such that “abilities beget abilities” (Heckman, 2008), and may accordingly prevent or attenuate downstream disparities. This would result in better academic preparation and thus

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the removal of a main barrier to postsecondary education opportunities for students from low-SES backgrounds and for students of color. One might speculate that [Intervention] could also affect related factors more proximal to postsecondary education, such as performance on the SAT (Reeves & Halikias, 2017) or enrollment in remedial coursework (Attewell et al., 2006; Chen, 2016; Radford & Horn, 2012).

[Intervention] students' higher probability of first enrolling in and graduating from a four-year (versus two-year) institution suggests that [Intervention] may play a role in not just whether students pursue postsecondary opportunities, but which opportunities they pursue. As speculated above, by supporting academic preparation, [Intervention] could reduce the potential need for remedial coursework, which is associated with enrollment in two-year versus four-year colleges (Chen, 2016). Further, high school graduates who enroll in four-year postsecondary institutions have higher completion rates than students who enroll in two-year institutions (e.g., McLaughlin & Van Eaton, 2018), which suggests first enrollment in four-year institutions as a potential mechanism by which [Intervention] may support students' postsecondary completion.

ISS and Additional Factors for Post-Secondary Education Success

We view [Intervention] as one support among many that may facilitate postsecondary education success. The pathway to postsecondary enrollment and completion involves in-school, out-of-school, and social factors (Arbona & Nora, 2007; Barbatis, 2012; Byrd & Macdonald, 2005; Ward, 2006). While the goal of [Intervention] is to support students' academic achievement by reducing the impact of out-of-school barriers to learning, it does not explicitly focus on fostering a college-going mindset. Participation in the intervention does not remove larger structural barriers of college-going expectations or opportunities that low-SES and students of color may experience. It does not address a lack of in-school resources (e.g., in high

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school) that directly support the college-going process or account for individual differences that may factor into a student's decision not to pursue postsecondary opportunities.

For students who wish to enroll, the high school-to-college transition requires coordination among several moving pieces. Beyond academic preparation, students need access to information about the college-going process and how to secure financial aid, such as from a college counselor or other program (Bettinger et al., 2012; Castleman et al., 2012; Long, 2017; Page & Scott-Clayton, 2016). As such, this study aligns with the truism that academic preparation is necessary, but not sufficient, for postsecondary education opportunities. While [Intervention] may not address all barriers to enrollment, results suggest that [Intervention] facilitates the necessary academic preparation for students to enroll, persist, and graduate, compared to their non-[Intervention] peers.

Limitations

By design, [Intervention] provides tailored enrichments and services to every student in a participating school, which precludes student-level random assignment to treatment. At present, the number of schools in districts that participate in [Intervention] do not provide adequate power to estimate school-level treatment effects. In the absence of random assignment, we aim to estimate the impact of [Intervention] on postsecondary enrollment and completion using propensity score weighting to account for potential selection bias. Limitations of the propensity score approach include the inability to account for unobservable characteristics and the potential for omitted variables that introduce bias. To examine the latter, we conduct sensitivity analyses that suggest results of the present study are robust to omitted variables. Further support comes from situating this study alongside prior research (e.g., Author, 2014, 2016a, 2018) that likewise found positive effects of the intervention using different samples and analytical approaches.

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Due to data limitations, we use relatively crude measures of some covariates. For example, as a proxy for SES, we use measures of free- and reduced-price lunch. Unavailable to us are additional indicators of SES, such as parent education level and income. Also beyond the scope of the data are additional measures that relate to postsecondary enrollment and persistence, such as parental involvement (Perna, 2002), family life (Byrd & Macdonald, 2005), educational aspirations (Perna & Swail, 2001; Stokes & Somers, 2009), goal-orientation and determination (Barbatis, 2010), and remedial coursework and proximity to postsecondary institutions (Stokes & Somers, 2009). However, when we account for factors that predate the intervention that impact academic achievement and postsecondary outcomes, we find a higher probability of [Intervention] students enrolling in and completing postsecondary education, consistent across unweighted models and models using unstabilized and stabilized propensity score weights.

The estimates of the effect of [Intervention] on postsecondary outcomes rely on postsecondary data from NSC. Two areas of concern for measurement error in NSC data are institutional coverage and blocking through the Family Educational Rights and Privacy Act (FERPA; Dynarski, Hemelt, & Hyman, 2013). We define students without an NSC record as having never enrolled in a postsecondary institution. Because colleges are not required to participate in the NSC, it is possible that some students who we classify as having never enrolled did enroll in and/or complete postsecondary education from an institution that does not participate in the NSC. Based on coverage rates, we believe the number of cases this may affect is small. At the national, regional, and [State] levels, NSC coverage for public two- and four-year institutions is over 99% and coverage for not-for-profit private colleges is over 95% (NSCRC, 2018a). Coverage for private for-profit institutions is substantially lower (e.g., 71.2% nationwide, 62.5% northeast region, 95% [State] in 2017; NSCRC, 2018a). However, for-profit

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institutions are least relevant to our analyses. In 2008, less than one percent of first-time students enrolled in these institutions nationwide (NSCRC, 2014) and schools that do not participate in NSC account for only three percent of all students (Dundar & Shapiro, 2016). Alternatively, students without an NSC record may have blocked their information in accordance with FERPA. Available NSC estimates suggest that nationwide block rates decreased from 4.10-3.72% between 2012 and 2017, and [State] block rates decreased from 2.25-1.73% over the same time period. Thus, we cannot rule out a small amount of measurement error due to FERPA blocking.

Future directions

The present study shows for the first time that [Intervention] has a positive effect on students' educational attainment and success beyond K-12 schooling. As such, these results suggest several future avenues to examine the mechanisms by which [Intervention] may be effective. Our models show that there remains a large portion of unexplained variation in postsecondary enrollment and completion (see Tables A1-A4), which suggests there are additional important predictors of postsecondary outcomes unaccounted for here. For example, we exclude measures of academic achievement (e.g., high school grades, standardized test scores), which predict postsecondary success (Noble & Sawyer, 2002; Westrick et al., 2015) and whose inclusion would doubtless produce a larger R-square. However, prior research on [Intervention] (e.g., Author, 2014) suggests that academic achievement mediates the [Intervention] effect on postsecondary outcomes, and therefore is endogenous to the intervention. Here we estimate the overall intervention effect, rather than its unique contribution above and beyond the (mediating) effects of academic achievement, and so only include covariates that predate the intervention that may relate to postsecondary enrollment and completion. Covariates such as SES and race may not explain a lot of variance in our outcomes, since there are many

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factors beyond these that shape students' trajectories from elementary to postsecondary schooling. Future studies should test potential mediators of the effects we find here, including indicators of academic achievement (e.g., high school GPA, standardized test performance), SES (e.g., parental education, income), college-mindset (e.g., student aspirations, student self-efficacy), and their potential interactions.

As additional [Intervention] cohorts graduate from high school, a second avenue for future research is to examine the specific aspects of postsecondary enrollment, persistence, and completion that participation in [Intervention] supports. For example, Dynarski et al. (2013) find that students in a small class-size intervention shifted toward higher-earning degrees (e.g., in STEM). Future studies could similarly ask whether [Intervention] affects patterns of course taking or postsecondary fields of study. Likewise, future research could investigate differences between patterns of postsecondary enrollment and completion for [Intervention] and comparison students, to determine the relation between [Intervention] and enrollment status (i.e., part-time versus full-time), transfer or reverse-transfer, or stop-out.

Conclusion

In this study, we show [Intervention], an elementary school-based intervention, positively affects the probability of students enrolling in and completing postsecondary education, and that this ISS approach can have long term effects on students' academic success. Our results further the growing research base that suggests [Intervention] boosts academic achievement for disadvantaged students. Accordingly, [Intervention] may be one mechanism to reduce income- and race-based disparities in postsecondary outcomes by supporting students' academic preparation. Together with prior research, these results underscore the notion that both in-school and out-of-school factors facilitate students' academic success through postsecondary education.

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Results also inform national- and state-level discourse about how to support students during K-12 schooling and how to best position them to take advantage of postsecondary opportunities. As examples, our results suggest that [Intervention] may more effectively support students' postsecondary success than college-preparatory charter schools serving similar student populations that may boost postsecondary enrollment but not persistence (Coen et al., 2019), or programs closer to the high school-college transition that show mixed results (Bowman et al., 2018; Cabrera et al., 2006; Haskins & Rouse, 2013; Knaggs et al., 2015; Sanchez et al., 2018; Seftor et al., 2009). Further, analyses of [Intervention] (Blinded, 2015; Blinded, 2018) show an estimated benefit-to-cost ratio of 3:1, which suggests [Intervention] as a way to support students' long-term achievement in a cost-effective way. Results of this study may also be particularly timely, as state departments of education begin to enact policies to invest in programs that support the whole-child by drawing on the ISS approach (e.g., Ohio Code §3317.26). As suggested here, [Intervention] may be an example of how such investments can pay dividends throughout K-12 and postsecondary education, and provide increased opportunities for the students who most need them.

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EFFECTS OF INTEGRATED STUDENT SUPPORT

Figure 1.

Table 1

Student sample size, years of participation in [Intervention], race, special education status, free and reduced-price lunch status, and English learner status for [Intervention] students and the comparison group, across the four analytic samples.

	Full sample 2009-2017 (<i>n</i> = 26,182)		Enrolled 2009-2017 (<i>n</i> = 20,530)		Enrolled 2009-2013 (<i>n</i> = 12,733)		Completed 2009-2013 (<i>n</i> = 5,824)	
	[Intervention]	Comparison	[Intervention]	Comparison	[Intervention]	Comparison	[Intervention]	Comparison
<i>n</i> students	2,116 (8%)	24,066 (92%)	1,713 (8%)	18,817 (92%)	732 (6%)	12,001 (94%)	396 (7%)	5,428 (93%)
Years of [Intervention]								
Mean	2.65	-	2.66	-	2.17	-	2.16	-
SD	1.67		1.66		1.15		1.13	
Male	46%	46%	44%	43%	44%	43%	38%	36%
Race								
Black	28%	39%	27%	38%	27%	39%	19%	33%
Hispanic	31%	34%	29%	32%	33%	31%	27%	25%
Asian	29%	11%	33%	13%	29%	13%	41%	19%
White	11%	15%	10%	16%	11%	15%	12%	22%
Multi- Racial/Other	1%	1%	1%	1%	1%	1%	1%	1%
Special education								
GE ^a with modifications < 25% outside	< 1%	< 1%	< 1%	< 1%	< 1%	< 1%	0%	< 1%
GE ^a 25-60% outside	7%	4%	7%	4%	5%	4%	4%	3%
GE ^a	9%	6%	7%	5%	8%	5%	3%	2%
Free lunch	89%	84%	88%	82%	89%	82%	84%	74%
Reduced-Price lunch	3%	4%	4%	4%	4%	4%	5%	5%
EL	27%	30%	27%	28%	25%	28%	28%	25%

^aGeneral education classroom

EFFECTS OF INTEGRATED STUDENT SUPPORT

Table 2

Percent of students who enrolled in a postsecondary institution, overall and by institution type; and percent of students who completed postsecondary education, overall and by institution type.

	Full Sample 2009-2017 (<i>n</i> = 26,182)	
	[Intervention]	Comparison
Ever enrolled	81%	78%
Time of enrollment		
First year ^a	92%	89%
After first year ^a	8%	11%
First institution		
Two-year ^a	23%	34%
Four-year ^a	77%	66%
Completion	19%	23%
Graduating institution		
Two-year ^b	8%	12%
Four-year ^b	92%	88%

^a Percent reflects only students who enrolled

^b Percent reflects only students who completed

Figure 2.

EFFECTS OF INTEGRATED STUDENT SUPPORT

Table 3

Group equivalence before and after ATE propensity score weighting for the full sample (2009-2017, $n = 26,182$). Shaded cells indicate covariates with an imbalance between [Intervention] participants and the comparison group.

	Unweighted			Weighted		
	[Intervention] ($n = 2,116$)	Comparison ($n = 24,066$)	Standardized difference	[Intervention] ($n = 2,116$)	Comparison ($n = 24,066$)	Standardized difference
Male	0.46	0.46	0.01	0.45	0.46	-0.01
Black	0.29	0.39	-0.24	0.39	0.39	> -.01 ^b
White	0.11	0.15	-0.13	0.14	0.14	> -.01 ^b
Asian	0.29	0.11	0.39	0.13	0.13	< .01 ^b
Hispanic	0.31	0.34	-0.05	0.34	0.34	0.01
Multi-Racial/Other	0.01	0.01	-0.04	0.01	0.01	-0.03
< 25% outside of GE ^a	0.07	0.05	0.11	0.05	0.05	> -.01
25-60% outside of GE ^a	0.09	0.06	0.11	0.06	0.06	> -.01
Reduced lunch	0.03	0.04	-0.01	0.03	0.04	-0.02
Free lunch	0.89	0.84	0.18	0.85	0.84	0.02
EL	0.27	0.30	-0.06	0.29	0.30	-0.01

^a General education classroom ^bStandardized difference close to zero

EFFECTS OF INTEGRATED STUDENT SUPPORT

Table 4

Parameter estimates, robust standard errors, *p*-values, and odds ratios for the impact of [Intervention], by outcome, for unweighted models and models with stabilized propensity score weights. All models include cohort fixed effects and covariates of gender, race, special education status, free and reduced-price lunch, and EL status (see Appendix Tables A1-A4 for estimates of all predictors, separately for unweighted models, and propensity score weighted models with unstabilized and stabilized weights).

Outcome	Effect of [Intervention]			
	Log-odds	SE	<i>p</i>	Odds ratio
Enrollment (<i>n</i> = 26,182)				
Unweighted	0.196	0.061	0.001	1.216
Stabilized weighted	0.162	0.069	0.020	1.175
Enrollment: Institution type (<i>n</i> = 20,530)				
Unweighted	0.346	0.064	< .001	1.414
Stabilized weighted	0.431	0.073	< .001	1.539
Completion (<i>n</i> = 12,733)				
Unweighted	0.357	0.083	< .001	1.429
Stabilized weighted	0.396	0.094	< .001	1.486
Completion: Institution type (<i>n</i> = 5,695)				
Unweighted	0.434	0.199	0.029	1.544
Stabilized weighted	0.453	0.225	0.044	1.574

EFFECTS OF INTEGRATED STUDENT SUPPORT

Figure 3.

Figure Captions

Figure 1. Percent of students who received [Intervention] in each grade, out of the total number of students who ever received the intervention ($n = 2,116$). Any students who received the intervention after fifth grade also received the intervention for at least one elementary school year. Students who received the intervention in multiple grades are represented in the bar for each grade.

Figure 2. Frequency distribution of overlap in propensity scores for [Intervention] students (i.e., pink) and students in the comparison group (i.e., blue).

Figure 3. Average fitted probabilities for [Intervention] and non-[Intervention] students for a) postsecondary enrollment ($n = 26,182$), b) first enrollment in a four-year (versus two-year) institution ($n = 20,530$), c) postsecondary completion ($n = 12,733$), and d) postsecondary completion at a four-year (versus two-year) institution ($n = 5,824$), from models fit with stabilized propensity score weights.

SUPPLEMENTAL MATERIALS

Table A1

Parameter estimates, standard errors, *p*-values, and odds ratios for the effect of [Intervention] on students' postsecondary enrollment, controlling for gender, race, special education status, free and reduced-price lunch, and EL status (*n* = 26,182). Estimates are separate for an unweighted model, and propensity score weighted models with unstabilized and stabilized weights.

	Unweighted				Unstabilized weighted				Stabilized weighted			
	Log-odds	S.E.	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio
Parameter estimates												
[Intervention]	0.196	0.061	0.001	1.216	0.141	0.070	0.043	1.152	0.162	0.069	0.020	1.175
Intercept	2.639	0.078	< .001	14.004	2.214	0.178	< .001	9.153	2.594	0.080	< .001	13.381
Cohort 2010	0.168	0.069	0.015	1.182	0.124	0.136	0.359	1.132	0.168	0.068	0.014	1.182
Cohort 2011	0.045	0.067	0.507	1.046	0.029	0.128	0.821	1.029	0.051	0.067	0.445	1.052
Cohort 2012	-0.165	0.065	0.011	0.848	-0.175	0.124	0.157	0.839	-0.172	0.064	0.007	0.842
Cohort 2013	-0.255	0.065	< .001	0.775	-0.603	0.137	< .001	0.547	-0.273	0.066	< .001	0.761
Cohort 2014	-0.361	0.065	< .001	0.697	-0.344	0.128	0.007	0.709	-0.371	0.064	< .001	0.690
Cohort 2015	-0.302	0.067	< .001	0.739	-0.426	0.134	0.002	0.653	-0.316	0.067	< .001	0.729
Cohort 2016	-0.450	0.067	< .001	0.638	-0.590	0.133	< .001	0.554	-0.462	0.067	< .001	0.630
Cohort 2017	-0.768	0.065	< .001	0.464	-0.820	0.130	< .001	0.440	-0.768	0.065	< .001	0.464
Male	-0.532	0.031	< .001	0.588	-0.457	0.069	< .001	0.633	-0.524	0.031	< .001	0.592
Black	-0.325	0.057	< .001	0.723	-0.053	0.117	0.649	0.948	-0.309	0.058	< .001	0.734
Asian	0.792	0.080	< .001	2.208	1.151	0.137	< .001	3.161	0.784	0.083	< .001	2.191
Hispanic	-0.375	0.059	< .001	0.687	-0.090	0.117	0.443	0.914	-0.362	0.059	< .001	0.697
Multi-Racial/Other	-0.356	0.154	0.021	0.700	0.235	0.294	0.425	1.265	-0.331	0.153	0.030	0.718
< 25% outside of GE ^b	-0.250	0.068	< .001	0.779	-0.385	0.166	0.021	0.680	-0.266	0.072	< .001	0.767
25-60% outside of GE ^b	-0.647	0.056	< .001	0.524	-0.661	0.094	< .001	0.517	-0.650	0.056	< .001	0.522
Free lunch	-0.600	0.065	< .001	0.549	-0.369	0.156	0.018	0.691	-0.561	0.068	< .001	0.571
Reduced lunch	-0.228	0.110	0.039	0.796	0.162	0.236	0.493	1.176	-0.192	0.111	0.085	0.825
EL	-0.278	0.035	< .001	0.757	-0.245	0.081	0.003	0.782	-0.277	0.036	< .001	0.758
Goodness-of-fit												
Log-likelihood	-12880.420				-24612.870 ^c				-13010.080 ^c			
Tjur's <i>d</i>	.059				.054 ^c				.047 ^c			

^a Robust standard errors; ^b General education classroom; ^c Based on approximation of the linear probability model due to link function and weights

SUPPLEMENTAL MATERIALS

Table A2

Parameter estimates, standard errors, *p*-values, and odds ratios for the effect of [Intervention] on enrollment institution type, controlling for gender, race, special education status, free and reduced-price lunch, and EL status (*n* = 20,530). Estimates are separate for an unweighted model, and propensity score weighted models with unstabilized and stabilized weights.

	Unweighted				Unstabilized weighted				Stabilized weighted			
	Log-odds	S.E.	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio
Parameter estimates												
[Intervention]	0.346	0.064	0.000	1.414	0.422	0.072	<.001	1.524	0.431	0.073	<.001	1.539
Intercept	2.387	0.082	0.000	10.876	2.342	0.213	<.001	10.400	2.379	0.080	<.001	10.795
Cohort 2010	-0.032	0.061	0.595	0.968	0.096	0.130	0.462	1.101	-0.034	0.061	0.571	0.966
Cohort 2011	-0.055	0.061	0.368	0.947	-0.010	0.126	0.937	0.990	-0.054	0.060	0.370	0.947
Cohort 2012	-0.062	0.061	0.307	0.940	-0.161	0.127	0.206	0.851	-0.061	0.061	0.313	0.941
Cohort 2013	0.103	0.063	0.104	1.108	0.054	0.131	0.682	1.055	0.109	0.062	0.080	1.116
Cohort 2014	0.172	0.065	0.008	1.187	0.103	0.134	0.439	1.109	0.170	0.064	0.008	1.185
Cohort 2015	0.434	0.068	<.001	1.544	0.161	0.146	0.271	1.175	0.418	0.068	<.001	1.519
Cohort 2016	0.579	0.072	<.001	1.784	0.311	0.155	0.044	1.365	0.571	0.072	<.001	1.770
Cohort 2017	0.745	0.076	<.001	2.105	0.498	0.163	0.002	1.645	0.744	0.077	<.001	2.103
Male	-0.192	0.032	<.001	0.825	-0.225	0.069	0.001	0.798	-0.194	0.032	<.001	0.824
Black	-0.777	0.059	<.001	0.460	-0.560	0.123	<.001	0.571	-0.770	0.060	<.001	0.463
Asian	0.694	0.078	<.001	2.002	0.883	0.132	<.001	2.417	0.695	0.078	<.001	2.005
Hispanic	-0.755	0.061	<.001	0.470	-0.703	0.121	<.001	0.495	-0.760	0.061	<.001	0.468
Multi-Racial/Other	-0.590	0.161	<.001	0.554	-0.319	0.290	0.271	0.727	-0.588	0.157	<.001	0.556
< 25% outside of GE ^b	-0.521	0.075	<.001	0.594	-0.411	0.127	0.001	0.663	-0.538	0.076	<.001	0.584
25-60% outside of GE ^b	-0.910	0.068	<.001	0.403	-0.753	0.114	<.001	0.471	-0.915	0.069	<.001	0.401
Free lunch	-1.205	0.070	<.001	0.300	-1.346	0.181	<.001	0.260	-1.199	0.071	<.001	0.301
Reduced lunch	-0.609	0.111	<.001	0.544	-0.513	0.254	0.043	0.598	-0.610	0.109	<.001	0.543
EL	-0.429	0.036	<.001	0.651	-0.099	0.079	0.214	0.906	-0.416	0.037	<.001	0.660
Goodness-of-fit												
Log-likelihood	-11675.240				-21388.560 ^c				-11721.180 ^c			
Tjur's <i>d</i>	.116				.106 ^c				.098 ^c			

^a Robust standard errors; ^b General education classroom; ^c Based on approximation of the linear probability model due to link function and weights

SUPPLEMENTAL MATERIALS

Table A3

Parameter estimates, standard errors, *p*-values, and odds ratios for the effect of [Intervention] on postsecondary completion, controlling for gender, race, special education status, free and reduced-price lunch, and EL status (*n* =12,733). Estimates are separate for an unweighted model, and propensity score weighted models with unstabilized and stabilized weights.

	Unweighted				Unstabilized weighted				Stabilized weighted			
	Log-odds	S.E.	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio
Parameter estimates												
[Intervention]	0.357	0.083	< .001	1.429	0.398	0.094	<.001	1.489	0.396	0.094	<.001	1.486
Intercept	1.543	0.075	< .001	4.679	1.649	0.159	<.001	5.202	1.558	0.073	<.001	4.750
Cohort 2010	-0.012	0.058	0.834	0.988	-0.045	0.122	0.715	0.956	-0.015	0.058	0.794	0.985
Cohort 2011	-0.110	0.059	0.062	0.896	-0.137	0.125	0.274	0.872	-0.115	0.059	0.052	0.892
Cohort 2012	-0.300	0.059	< .001	0.741	-0.267	0.121	0.028	0.766	-0.300	0.059	<.001	0.741
Cohort 2013	-0.852	0.063	< .001	0.427	-0.982	0.131	<.001	0.375	-0.867	0.063	<.001	0.420
Male	-0.573	0.039	< .001	0.564	-0.615	0.082	<.001	0.540	-0.581	0.039	<.001	0.559
Black	-0.716	0.063	< .001	0.489	-0.694	0.141	<.001	0.500	-0.715	0.063	<.001	0.489
Asian	0.507	0.077	< .001	1.661	0.605	0.137	<.001	1.832	0.478	0.077	<.001	1.613
Hispanic	-0.718	0.066	< .001	0.488	-0.610	0.137	<.001	0.544	-0.714	0.066	<.001	0.490
Multi-Racial/Other	-0.795	0.190	< .001	0.451	-0.667	0.360	0.064	0.513	-0.797	0.190	<.001	0.451
< 25% outside of GE ^b	-0.433	0.106	< .001	0.648	-0.553	0.173	0.001	0.575	-0.451	0.110	<.001	0.637
25-60% outside of GE ^b	-0.816	0.101	< .001	0.442	-1.006	0.166	<.001	0.366	-0.812	0.102	<.001	0.444
Free lunch	-0.892	0.066	< .001	0.410	-1.100	0.158	<.001	0.333	-0.908	0.066	<.001	0.403
Reduced lunch	-0.398	0.110	< .001	0.672	-0.650	0.266	0.015	0.522	-0.429	0.112	<.001	0.651
EL	-0.053	0.045	0.246	0.949	0.209	0.096	0.030	1.232	-0.020	0.046	0.663	0.980
Goodness-of-fit												
Log-likelihood		-7963.050				-12852.630 ^c				-7968.813 ^c		
Tjur's <i>d</i>		.122				.128 ^c				.113 ^c		

^a Robust standard errors; ^b General education classroom; ^c Based on approximation of the linear probability model due to link function and weights

SUPPLEMENTAL MATERIALS

Table A4

Parameter estimates, standard errors, *p*-values, and odds ratios for the effect of [Intervention] on postsecondary graduation institution type, controlling for gender, race, special education status, free and reduced-price lunch, and EL status (*n* = 5,824). Estimates are separate for an unweighted model, and propensity score weighted models with unstabilized and stabilized weights.

	Unweighted				Unstabilized weighted				Stabilized weighted			
	Log-odds	S.E.	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio	Log-odds	S.E. ^a	<i>p</i>	Odds ratio
Parameter estimates												
[Intervention]	0.434	0.199	0.029	1.544	0.398	0.217	0.066	1.489	0.453	0.225	0.044	1.574
Intercept	3.197	0.191	< .001	24.461	2.835	0.542	<.001	17.035	3.131	0.187	<.001	22.894
Cohort 2010	0.007	0.117	0.953	1.007	0.334	0.246	0.175	1.396	0.051	0.115	0.657	1.052
Cohort 2011	0.142	0.121	0.240	1.153	0.345	0.272	0.206	1.412	0.212	0.120	0.078	1.236
Cohort 2012	0.410	0.131	0.002	1.507	0.375	0.278	0.177	1.455	0.472	0.129	<.001	1.603
Cohort 2013	0.525	0.158	0.001	1.690	0.426	0.343	0.214	1.531	0.571	0.155	<.001	1.770
Male	0.434	0.093	< .001	1.544	0.458	0.191	0.016	1.580	0.447	0.092	<.001	1.564
Black	-0.987	0.169	< .001	0.373	-0.888	0.311	0.004	0.411	-0.966	0.170	<.001	0.381
Asian	0.261	0.207	0.208	1.298	0.186	0.337	0.581	1.204	0.319	0.206	0.121	1.376
Hispanic	-1.108	0.172	< .001	0.330	-1.141	0.332	0.001	0.319	-1.098	0.173	<.001	0.333
Multi-Racial/Other	-0.620	0.463	0.180	0.538	-0.210	0.536	0.695	0.810	-0.581	0.455	0.201	0.559
< 25% outside of GE ^b	-0.659	0.217	0.002	0.517	-0.391	0.327	0.232	0.676	-0.609	0.225	0.007	0.544
25-60% outside of GE ^b	-1.011	0.196	< .001	0.364	-0.505	0.245	0.039	0.604	-0.966	0.195	<.001	0.381
Free lunch	-0.752	0.176	< .001	0.471	-0.603	0.347	0.082	0.547	-0.764	0.179	<.001	0.466
Reduced lunch	-0.582	0.257	0.024	0.559	-0.045	0.406	0.912	0.956	-0.615	0.260	0.018	0.541
EL	-0.441	0.096	< .001	0.644	-0.367	0.205	0.073	0.693	-0.462	0.096	<.001	0.630
Goodness-of-fit												
Log-likelihood	-1972.899				-3254.542 ^c				-1980.972 ^c			
Tjur's <i>d</i>	.064				.057 ^c				.044 ^c			

^a Robust standard errors; ^b General education classroom; ^c Based on approximation of the linear probability model due to link function and weights