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Heterogeneous Effects of Closing the Digital Divide During COVID-19 on Student Engagement and Achievement

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ABSTRACT

Equitably expanding technology access among K-12 students has long been viewed as critical for equalizing educational opportunities. But these interventions may influence students' academic outcomes in unexpected ways. Prior research suggests key technological resources, like broadband Internet, are a double-edged sword, conferring both educational benefits and distractions for children. Clarifying the academic effects of technology-oriented investments is particularly important given that the amount of funding devoted to them has spiked over the last five years, owing to the COVID-19 pandemic, when remote learning rendered high-speed Internet access indispensable to instruction. In this study, we leverage Chicago Public Schools' pandemic-era broadband expansion initiative to assess whether overall levels of, and equity in, educational engagement and achievement improved with increased technology access. Analyses reveal a *skill-technology complementarity*: broadband program participation boosted remote learning engagement and achievement for previously high-performing students and reduced engagement and achievement for low-performing pupils. We conclude that increased technology access may come with greater costs for low-achieving students and benefits for high-achieving ones— contributing to widening pandemic-era educational inequities. Continued investments in expanding technology access without complementary supports for vulnerable students may further fuel these inequities; counterbalancing the negative effects of technology for low-achieving students is thus imperative.

INTRODUCTION

Stubbornly sharp disparities in both children's academic achievement and digital technology access have led many researchers and policymakers to believe that gaps in the latter may partially explain gaps in the former (Attewell, 2001; Bulman & Fairlie, 2016; Escueta et al., 2020; Fairlie, 2004). The perceived link between digital access and academic achievement disparities has strengthened as K-12 education has become increasingly intertwined with technology. Indeed, this view underpinned major investments in equitably expanding access to broadband Internet and other technological resources– especially amidst the COVID-19 pandemic when remote learning rendered a high-speed Internet connection indispensable to instruction in many districts (Klein, 2021; Teräs et al., 2020).

Yet, prior research suggests increasing technological access for digitally disconnected students has ambiguous implications for overall levels of, and inequities in, educational engagement and achievement. Technology may be a double-edged sword in the educational realm, conferring a combination of benefits, like access to supplementary tutoring and research tools (Escueta et al., 2020), and distractions, like increased time spent on YouTube, social media, and gaming (Orben et al., 2022). Whether the educational benefits of increased access to technological resources in general, and broadband Internet in particular, outweigh the costs for the overall student population– and to varying extents across student subgroups, with important implications for educational equity– remains unresolved (Fairlie & Loyalka, 2020).

Probing the educational effects of a broadband expansion program implemented amidst the COVID-19 remote learning period provides a rare opportunity to clarify these dynamics. The exogenous shocks of the pandemic and of a coinciding broadband expansion may partially mitigate the selection bias that has afflicted many prior estimates of technology access effects on educational outcomes. Moreover, the vast scale of the expansion in a diverse urban school district permits the examination of increased technology access' heterogeneous effects across student subgroups. Tracking these access effects during a highwater mark in technology's centrality to K-12 education may be particularly informative as technological penetration of education is likely to persist.

Theoretical Expectations of Technology Access' Effects on Educational Outcomes and Equity

Since the dawn of the Internet age, optimists have argued that the increased penetration of K-12 education with technological tools could boost rates of student learning and reduce stubbornly large educational inequities. Computers, tablets, smartphones, and high-speed internet access might facilitate a more personalized educational experience whereby students engage in more enjoyable educational experiences that better fit their specific preferences, more readily access information and individualized support from myriad resources, more easily collaborate and connect with other students in other places, and receive more frequent feedback on their progress (Escueta et al., 2020).

But in the decades since, a less sanguine set of possibilities has emerged. Many have worried that technology can become a major distraction for K-12 students, especially as applications with arguably limited educational value like social media, videogames, streaming services and YouTube videos cannibalize a growing share of children's time– particularly amidst the COVID-19 pandemic. In a high-profile report, the U.S. Surgeon General argued that key Internet-based technologies like social media may be highly addictive and even dangerous to children's mental health (U.S. Surgeon General, 2023), with negative educational impacts (Jackman et al., 2021; Mizani et al., 2022). Reflecting these concerns, California recently passed a law forcing public schools to limit or prohibit the use of smartphones in children's classrooms (Governor of California, 2024); Australia just passed a law banning social media access for children under 16 (Kaye & Menon, 2024).

Rapidly shifting perceptions of technology's impacts on K-12 educational outcomes reflects in part a lack of scholarly consensus regarding the effects of expanded access to various technological tools on the K-12 population as a whole and for key student subgroups. Prior research on the educational effects of technology access in the U.S., conducted before the COVID-19 pandemic, showed mixed results (Bulman & Fairlie, 2016; Fairlie & Loyalka, 2020), with some studies finding increased technology access yields null effects on academic achievement (Beuermann et al., 2015; Fairlie & Robinson, 2013; Malamud et al., 2019; Starkey & Zhong, 2019), others showing associations with higher achievement (Fairlie et al., 2009; Jackson et al., 2006), and others still with lower achievement (Carter et al., 2017; Malamud & Pop-Eleches, 2011; Vigdor et al., 2014). Complicating matters further, patterns of heterogeneity by child age, socioeconomic class, and academic skill level have been inconsistent.

Probing educational outcomes amidst the COVID-19 pandemic may provide some key hints regarding the effects of increased penetration of, and access to, technological tools within the K-12 educational setting. During the pandemic, millions of American school children were rapidly shifted from in-person instruction to remote learning. Because remote learning required a reliable, high-speed Internet connection for full engagement, many school districts invested in expanding computer and broadband Internet access rapidly to households that lacked computers or Internet access altogether or that relied on weak, unreliable connections (e.g., via smartphone) or on public institutions that provided free Wi-Fi (e.g., public libraries) but were shuttered due to public health concerns (Klein, 2021; Teräs et al., 2020).

This more technologically-oriented educational paradigm did not appear to confer better outcomes for the student population as a whole or for particularly vulnerable student groups. Many pandemic-era studies have shown that from 2020-21 onward, student cohorts exhibited worse educational engagement and achievement outcomes compared to earlier cohorts of a similar age (Betthäuser et al., 2023; Engzell et al., 2021). This underperformance largely persisted even as the public health crisis and its associated social dislocations ebbed, while technology use in the educational setting remained elevated relative to the pre-pandemic baseline (Fahle et al., 2024; Kuhfeld et al., 2022), Concerningly, socioeconomically disadvantaged children fared far worse during the COVID-19 pandemic and its aftermath than their more advantaged peers vis-à-vis educational engagement and achievement (Goudeau et al., 2021; Haelermans et al., 2022; Reimer et al., 2021).

Yet recent research suggests the divergence in pandemic-era academic achievement trajectories appears to have been even sharper by children's pre-pandemic academic skills than by class background (Callen et al., 2024; NAEP, 2022), which hints at another, rarely considered possibility; intra-cohort variation in technology access' effects may be stratified by baseline skills, reflecting what we call a *skill-technology complementarity*. Much in the way that economists have shown how high-skill employees more effectively leverage job-based technology to drive higher productivity and wage growth than do their lower-skill counterparts (Acemoglu, 2002; Autor et al., 1998), we hypothesize that in a technology-dominated educational setting, academically-skilled students may have more effectively leveraged increased access to technological tools, like broadband Internet access, toward academic ends than did less-skilled pupils—both during the initial remote learning period, and in the subsequent return to in-person learning.

There is some evidence to support this hypothesis from pre-pandemic research (Bergdahl et al., 2020; Shapley et al., 2009; Wakefield & Frawley, 2020), though studies explicitly scrutinizing baseline skills as a moderator of technology access' effects are scarce and, much like the pre-pandemic literature on increased technology access' overall effects, the findings are mixed

(Beuermann et al., 2015; Fairlie & Robinson, 2013; Jackson et al., 2011; Malamud et al., 2019). These mixed findings may reflect, in part, the preponderance of small-n studies that are potentially underpowered to detect heterogeneous effects and the reliance on observational analyses lacking robust strategies for addressing selection.

The Present Study: An Exogenous Shock to Technology Access in a Large and Diverse Urban School District

This study overcomes these limitations, clarifying whether increased technology access coincides with increased skill-based stratification in academic engagement and achievement, by exploiting an exogenous shock to thousands of K-12 students' technology access amidst the COVID-19 pandemic. In summer 2020, Chicago Public Schools (CPS) responded to the pandemic by partnering with the City of Chicago, Kids First Chicago, as well as other philanthropic and community-based organizations, to launch Chicago Connected (CC)– a program intended to connect 100,000 students in 60,000 households to free broadband internet service for up to four years, starting in the summer of 2020. In a prior initiative, CPS had already ensured that all students had access to their own Internet-ready device.

The CC program offered four years of free internet service to eligible households, through wired broadband, hotspot connections, or both– based on the eligible household's preference. Program eligibility was initially based on a hardship index score calculated in summer 2020 for every CPS student's household. The index score, which ranged from 1 to 9, was higher for households that exhibited more indicators of structural disadvantage (e.g., free or reduced-price lunch eligibility; Medicaid enrollment; English Learner); households that exhibited none of these disadvantage indicators were not given a hardship score at all. In summer 2020, CC program eligibility was granted to CPS households that received one of the highest hardship index scores (6-9). CC program resources permitted two expansions of program eligibility to less severely disadvantaged households. In August 2020, CPS households with hardship index scores of 4 or 5 were added to the eligible pool of households. Finally, in November/December 2020, all students who qualified for free and reduced-price lunch or Medicaid became eligible. (For more details on the CC program, see De La Torre, 2023; Kids First Chicago, 2021).

The CC program's use of hardship index scores to determine eligibility in place of randomization, the continuous expansion of eligibility to lower scores, and non-random sorting into program participation among those who became eligible present important challenges to identifying the causal effects of the broadband expansion program on student academic achievement and engagement in remote learning. These challenges are deepened by a lack of detailed insight into CPS households' pre-program broadband access, participants' rationales for program (non)participation, and how CPS households' home lives shifted amidst the pandemic– shifts that may have diverged by children's pre-pandemic academic achievement levels.

Despite these constraints, we go beyond most prior observational analyses in bolstering the internal validity of our program effect estimates, by leveraging longitudinal, student-level administrative data with extensive pre-treatment covariates for nearly 80,000 CPS students who were in 5^{th}-8^{th} grades at the outbreak of COVID-19. These models statistically adjust for an unusually wide range of student characteristics that could bias our estimated effects of both CC-eligibility and of CC program participation among CC-eligible students on school engagement and achievement during the two semesters following the initial COVID-19 outbreak (i.e., fall 2020 and spring 2021). Our models thus plausibly isolate the causal effect of eligibility for, and participation in, a program that facilitated access to a reliable, higher-speed broadband connection, compared to otherwise similar non-participants who did not experience program-induced changes to broadband access and quality.

Overall, the study provides a rare opportunity to rigorously examine how bridging digital divides may impact levels of, and skill-based inequalities in, achievement and engagement when technology is a necessity for, rather than an optional supplement to, engaging in K-12 education. These conditions are likely to become increasingly common in the U.S., as technology's penetration of education persists post-pandemic and as an array of major disruptions wrought by climate change, zoonotic diseases and natural disasters threaten to reinstitute remote learning in the future.

DATA & METHODS

For our analyses, we identify 77,056 non-charter CPS students who were continuously enrolled in the district from fall 2017 through fall 2020; who were in 5th-8th grade in fall 2020; and who have complete data on all outcomes and covariates listed below. 70% of this analytic sample were eventually deemed eligible (in either summer or fall 2020) for the CC program (n=53,605); the remaining 30% were deemed too advantaged to qualify (n=23,451). Using this analytic sample, we estimate both CC program eligibility and participation effects.

Generating CC Eligibility Effect Estimates

As we alluded above, CC program eligibility effect estimates cannot be recovered by leveraging a sharp regression discontinuity design that employs hardship index scores of six or higher, or four or higher, because even students with lower scores eventually became eligible in November/December

2020– before the end of the fall 2020 semester, when our key outcomes of interest (described below) were measured. For CC eligibility estimates, we thus opt to compare achievement and engagement outcomes among a subset of CPS students who were *ever* eligible for the CC program to those who were *never* eligible, recognizing that some of the students in the former category were only CC-eligible for 1-2 months of the fall 2020 semester. The validity of our eligibility effect estimates generated through this approach is bolstered by: the inclusion of myriad control variables; the execution of robustness checks comparing engagement outcomes among students who became CC-eligible earlier versus later; and the specification of a narrower analytic sample "bandwidth" across the ever-eligible versus never-eligible discontinuity.

With regard to the latter point, regression discontinuity analyses require specifying a "bandwidth" (Imbens & Lemieux, 2008) (what we subsequently refer to as a "sample range") to determine which specific subset of students across the program eligibility threshold should be included, ensuring balance on covariates can be achieved. Without this narrowed sample range, the two comparison groups (i.e., all CC-eligible versus all CC-ineligible) may diverge too sharply on observed/unobserved confounding variables for valid program eligibility effect estimates to be generated. Our program eligibility analyses thus include all CC-ineligible students but only CC-eligible students whose hardship score suggested they were in households experiencing only modest, rather than severe, levels of disadvantage (i.e., CC-eligible students who scored 1-5 out of the 9-point hardship index).

By pooling all CC-ineligible students with this subset of CC-eligible students (n=48,305) and including a binary variable indicating CC eligibility, the coefficient on the binary indicator functions as a program eligibility estimate–i.e., the effect of *ever* becoming CC eligible (regardless of specific timing of eligibility or of treatment status) on our outcomes, conditional on myriad control variables that account for the considerable heterogeneity in sociodemographic advantage and pre-pandemic engagement/achievement across the ever-eligible and never-eligible comparison groups. Our specific outcomes and controls are described below, as is additional justification for our program eligibility analysis sample range decision that limits our CC-eligible group to only students with modest hardship index scores (1-5; see "Pre-Treatment Analyses to Assess Causal Interpretation").

Generating CC Participation Effect Estimates

Our CC program participation effect analyses are based on the same group of moderately-disadvantaged CC-eligible students included in our program eligibility analyses. However, CC-ineligible students are excluded from the program participation analyses, as they could not take up CC. For this smaller analytic subsample of 24,854 moderately-disadvantaged CC-eligible students, we construct binary variables capturing CC take-up ("treatment") effects on our pandemic-era academic/engagement outcomes.

Just as the program design vis-à-vis eligibility was somewhat complicated, so too was program treatment. Two versions of treatment were available to CC-eligible students. Some students received a (a) CC-provided Wired Connection (n=4,607; 19% of 24,854 students in CC-eligible moderately-disadvantaged subsample); others received (b) CC-provided Hotspot Only (n=986; 4% of this subsample). Note that some students received both a CC-provided Wired Connection and a Hotspot. They are marked as (a) CC-Wired Connection recipients. The remainder of CC-eligible students in our moderately-disadvantaged analytic sample group (c) did not participate in CC, despite being eligible (reference/control group; n=19,261; 77% of subsample).

Data limitations preclude us from clarifying eligible households' reasons for (non)participation. We believe the modest take-up rate likely reflects that many moderately-disadvantaged CC-eligible households had access to some Internet connection (including through a smartphone data plan) and did not believe they needed a CC-sponsored connection. It is therefore possible that the non-treated control group used for our CC program participation analyses diverges from the treated group in ways that could threaten our estimates' validity, with the former group having greater resources and motivation to access Internet before the pandemic required it for school-based learning. We thus incorporate an extensive set of control variables that adjust for potential differences across groups when predicting our focal outcomes. We also incorporate the controls into models that predict *pre*-pandemic outcomes to see if significant differences between CC-eligible treated and non-treated groups emerge (see "Pre-Treatment Analyses to Assess Causal Interpretation"). These models' results increase confidence that our CC program participation analyses plausibly isolate the causal effect of CC participants' accessing a more reliable, higher-speed broadband connection, compared to otherwise similar non-participants who did not experience program-induced changes to broadband access and quality.

Note that in supplementary analyses, we relax our moderate disadvantage-based sample range constraint, estimating program participation effects among all 53,605 CC-eligible students in our analytic sample, regardless of hardship index score. By including CC-eligible students of all disadvantage levels in these supplementary analyses, we expand the range of students to which our program participation findings apply. For a visual overview of CC eligibility and participation pathways, see Figure 1. The right-hand side of the figure provides information on the pre-COVID achievement patterns for each analytic sample subgroup.

Figure 1 about here

Assessing Balance Across Program Eligibility and Participation Comparison Groups

We assess pre-treatment balance in key variables across comparison groups to gauge potential threats to the internal validity of our CC program eligibility and participation effect estimates. Starting with the program eligibility comparison, moderately-disadvantaged CC-eligible students exhibit lower values on pre-pandemic and pandemic-era outcomes (i.e., lower spring 2020 course pass rates and Fall 2019 GPA) when measured against CC-ineligible students (see Figure 1). This is unsurprising since CC eligibility was determined based on severity of socioeconomic disadvantage– a well-documented predictor of academic achievement and engagement (Ready, 2010). Given this pre-treatment imbalance, our core program eligibility analyses control for extensive pre-treatment achievement and engagement outcomes and sociodemographic factors, detailed below. These models generate valid CC program eligibility effect estimates if eligibility is independent of our key outcomes, conditional on our full slate of covariates, when pooling the CC-ineligible and moderately-disadvantaged CC-eligible groups. Analyses below are congruent with this possibility (see "Pre-Treatment Analyses to Assess Causal Interpretation").

When shifting to assessing balance across the CC program participation analyses' comparison groups, students who took up a CC-Wired Connection and those who did not (i.e., were not "treated") are closely aligned on spring 2020 pass rates and pre-pandemic (fall 2019) GPA. The observed balance across these two comparison groups within this subsample partially assuages concerns about biased CC program participation effect estimates. Our program participation analyses still control for an extensive control variables to help ensure balance across groups.

Outcomes of Interest

We estimate CC program eligibility and participation effects on (a) engagement, i.e., log-ins to CPS's remote learning platform, Google Classroom; and (b) achievement, i.e., fall 2020 GPA and spring

2021 GPA. Although much prior research on pandemic-era educational outcomes examines standardized test scores, rather than engagement or grades, our focus on the latter helps mitigate important threats to internal validity posed by non-random selection into testing during pandemic-impacted school years. Standardized tests were not mandatory for CPS students during 2020-21 and relatively few students took them. Our GPA measure is uncontaminated by this source of bias. Despite legitimate concerns about grade inflation and measurement error, several studies of CPS students suggest GPA is more strongly predictive of longer-term educational attainment outcomes than are standardized test scores (e.g., Allensworth & Clark, 2020). We also address grade inflation concerns in robustness checks that include school-level fixed effects.

The remote learning and GPA measures are based on data from fall 2020, when nearly all CPS 5th-8th graders attended school remotely. By spring 2021, a large portion returned to in-person instruction. Fall 2020 thus constitutes a highwater mark in broadband connectivity's importance to academic outcomes, though we also assess spring 2021 GPA as a secondary academic achievement outcome to assess the durability of the CC program's achievement effects.

Outcome (b) captures GPA based on final grades for all enrolled courses during the fall 2020, and then spring 2021, semester. Outcome (a) is a three-component index drawn from Google Classroom data tracked for each student in our analytic sample: mean number of minutes logged in per school day with a staff member present; mean number of log-ins per day; and percent of school days in which the student logged in at least once. Each of the measures is standardized (mean=0, SD=1); once standardized, all three are averaged and re-standardized. Multivariate models based on an engagement index derived from a Principal Components Analysis of the same three component variables generate nearly identical results to those reported below (available upon request).

Our three-component engagement index exhibits a similar correlation to GPA during the remote learning period as attendance has to GPA in pre-pandemic years (~0.3), suggesting these engagement measures may capture participation in learning in a similar way. Moreover, supplementary analyses, available upon request, suggest that our remote learning engagement index is a key predictor of fall 2020 GPA, when adjusting for an extensive set of control variables.

Capturing Skill-Based Heterogeneity in Technology's Effects

Our key analytic objective is to examine variation in technology's engagement and achievement effects by baseline skill levels. We thus create categorical variables capturing students' pre-pandemic academic achievement level and then interact them with the CC program eligibility indicator variable and program participation indicator variables for analyses estimating program eligibility effects and program participation effects, respectively.

To generate these pre-pandemic achievement measures, we first calculate each student's average GPA encompassing all four semesters from fall 2017 through spring 2019. We then standardize the measure relative to the full analytic sample encompassing all CC-ineligible and CC-eligible students (i.e., including students across all nine hardship index categories). Finally, we use these standardized values to assign each student to one of four groups: *low-achievers* (pre-pandemic GPA >1 SD below analytic sample mean); *mid/low achievers* (between 1 SD below analytic sample mean and mean); *mid/high achievers* (just above mean and up to one SD above it); *high achievers* (> 1 SD above analytic sample mean). Low-achievers serve as the omitted reference group, and binary variables indicate whether each student is a mid/low, mid/high, or high achiever, revealing nonlinearities in pre-pandemic achievement's moderation of CC's pandemic-era engagement and achievement effects. Supplementary analyses operationalize pre-pandemic achievement in a linear rather than categorical manner and generate similar results (available upon request).

Control Variables

We include a rich set of student demographic and pre-treatment engagement and achievement variables to help ensure balance across program eligibility and program participation comparison groups. Beyond the categorical pre-pandemic achievement variables already described, we control for pre-pandemic student engagement by calculating the percent of school days CPS students attended (averaged across 2017-18/2018-19 school years). We also control for students' pre-pandemic standardized test scores, using their average grade-standardized performance across all NWEA Math and English tests for which they have valid scores in spring 2018/2019.

Although these controls essentially function as lagged outcome measures and thus help mitigate omitted variable bias concerns, the dramatic shift in context wrought by the pandemic may have introduced new constraints to students' educational engagement. Therefore, we also include two variables gauging student course engagement and performance in spring 2020—when teaching and learning became remote, *but before CC was introduced*: (a) total number of enrolled courses and (b) percent of all courses passed. During spring 2020, CPS implemented a binary grading policy: students either received a pass or an incomplete for every course. Many students, across achievement levels, received at least one Incomplete that term and therefore passed fewer than 100% of their courses (Gwynne et al., 2023). Additional controls (described in more detail in the Online Supplement–Methodological Appendix) capture student sociodemographics, measured in fall 2020, including: the student's grade level, gender, race/ethnicity, housing instability, household socioeconomic status, disability status, English proficiency, household size, and the degree of structural disadvantage within the student's home neighborhood. Empirical analyses confirm that several of these sociodemographic factors predict CC participation (De La Torre, 2023), and prior research suggests they shape engagement and achievement, too (Lloyd & Schachner, 2021). Descriptive statistics for our outcomes and a subset of predictors are displayed in Table 1.

Table 1 about here

Pre-Treatment Analyses to Assess Causal Interpretation

We assess balance across the CC program eligibility and participation comparison groups described above by using multivariate models that gauge whether there are significant *pre*-treatment differences in engagement- and achievement-related outcomes between (1) CC-ineligible and moderately-disadvantaged CC-eligible students and by (2) treatment/participation status– i.e., Wired Connection, Hotspot Only, no take-up– among moderately-disadvantaged CC-eligible students in our analytic samples. If, net of our control variables, significant differences in pre-treatment outcomes are detected, then our program eligibility and participation effect estimates may not be internally valid.

We thus use Equation (1) below but predict *the proportion of students' enrolled courses that they passed in spring 2020*, after the initial shift to remote instruction but before CC became available. Recall that we use course passage rates instead of GPA because of the spring 2020 grading policy that replaced traditional letter grades with a "Pass" or "Incomplete" for each class. If CC eligibility or treatment status are significant predictors of this pre-program outcome, then concerns of biased program eligibility and participation estimates become more acute.

Starting with our assessment of potential program eligibility effect estimate bias, Table S1, Model 1 in the Online Supplement reveals that the binary CC eligibility indicator does *not* significantly predict spring 2020 pass rate within our specified sample range (i.e., pooling moderately-disadvantaged CC-eligible with hardship index scores 1-5 and all CC-ineligible students). In supplementary analyses, we expanded the sample range to encompass more disadvantaged students (i.e., with hardship index scores 1-6, 1-7, 1-8, 1-9) but doing so yields a significant and negative coefficient on CC eligibility when predicting this pre-CC achievement outcome of spring 2020 pass rate. Thus, all subsequent program eligibility analyses return to the sample range encompassing CC-eligible students with hardship scores of only 1-5, pooled with all CC-ineligible students.

Table S1, Model 2 includes interaction terms to assess heterogeneity by pre-spring 2020 academic achievement level; no interaction terms' coefficients are significant here, either. Models 3 and 4 further show nonsignificant differences by CC eligibility status when predicting Fall 2019 GPA and nonsignificant interactions between CC eligibility status and baseline academic achievement. Models 5 and 6 show nonsignificant differences by CC eligibility status when predicting pre-pandemic standardized test scores. The lack of significant pre-CC differences in engagement- and achievement-related outcomes by CC eligibility status increases our confidence in the internal validity of our program eligibility estimates based on our specified sample range.

Shifting to CC program participation analyses, Table S2 in the Online Supplement assesses whether CC take-up status significantly predicts pre-CC outcomes, net of controls, among only moderately-disadvantaged CC-eligible students. When predicting spring 2020 course pass rates (Model 1), the coefficient on CC Wired Connection (reference group: CC non-takeup) does not approach significance. However, the coefficient on CC Hotspot Only is significant ($p<0.01$), negative, and substantively large. All else equal, students who would later become CC Hotspot Only recipients achieved a spring 2020 course passage rate five percentage points lower than otherwise-similar CC-eligible non-participants. Although descriptive statistics in Figure 1 already hinted at Hotspot Only recipients' distinct disadvantage vis-a-vis housing insecurity, disadvantaged community exposure, and pre-pandemic achievement, Table S2, Model 1 suggests they were also likely disadvantaged during the pandemic in ways that could not be fully captured by the rich set of controls we have available (e.g., due to loss of a parent) and that could threaten the validity of CC program participation effect estimates generated from analyses that include this group of students. For this reason, our models estimating CC participation effects exclude the CC Hotspot Only group.

We believe that once this subgroup is removed, our estimate of CC Wired Connection effects warrant a plausibly causal interpretation, especially given Table S2, Models 3-6 results suggesting that moderately-disadvantaged CC Wired Connection students do not exhibit significant differences in pre-pandemic GPA and standardized test scores when compared to otherwise-similar CC-eligible students who did not take up CC. We account for additional threats to the internal validity of our program eligibility and participation effect estimates via robustness and falsification checks below.

Analytic Strategy

In estimating effects of CC eligibility and of CC Wired Connection take-up among our analytic sample students, we use ordinary least squares (OLS) regressions that predict each outcome (Y) as a function of student sociodemographics, pre-treatment achievement and engagement measures, and the focal CC program eligibility/participation described above. Concretely, the CC program eligibility effect is estimated in the following manner:

(Equation 1)

$Y_i = \beta_0 + \beta_1(\text{CC Eligible})_i + \beta_2(\text{Mean NWEA scores '18-'19})_i + \beta_3(\text{Attendance Rate '17-18,'18-19})_i + \beta_4(\text{\% Spring 2020 courses passed})_i + \beta_5(\text{Mid/low pre-pandemic achievement})_i + \beta_6(\text{Mid/high pre-pandemic achievement})_i + \beta_7(\text{High pre-pandemic achievement})_i + \mathbf{X}_i\beta_x + \mathbf{Z}_i\beta_y + e_i$

Here, the achievement outcome is the GPA of CPS student *i* in fall 2020, when virtually all students in the district received remote instruction. We cluster standard errors by CPS school attended for the majority of the 2020-21 school year, given that the error terms of students within the same school may be correlated.

The key parameter of interest in this model is the coefficient on the CC eligibility indicator variable (β_1), capturing how much higher (/lower) fall 2020 GPAs were, on average, for CC-eligible students compared to GPAs of CC-ineligible students who were in the same grade level (captured by a vector of grade-level fixed effects, represented by vector $\mathbf{X}_i$) with similar student-level characteristics (e.g., race/ethnicity, disability, represented by vector $\mathbf{Z}_i$) and comparable pre-pandemic grades, test scores, and attendance. To achieve our core analytic objective and gauge heterogeneity in CC effects by baseline skill levels, we slightly modify Equation 1 above:

(Equation 2)

$Y_i = \beta_0 + \beta_1(\text{CC Eligible})_i + \beta_2(\text{Mean NWEA scores '18-'19})_i + \beta_3(\text{Attendance Rate '17-18,'18-19})_i + \beta_4(\text{\% Spring 2020 courses passed})_i + \beta_5(\text{Mid/low pre-pandemic achievement})_i + \beta_6(\text{Mid/high pre-pandemic achievement})_i + \beta_7(\text{High pre-pandemic achievement})_i + \beta_8(\text{Mid/low pre-pandemic achievement})_i(\text{CC Eligible})_i + \beta_9(\text{Mid/high pre-pandemic achievement})_i(\text{CC Eligible})_i + \beta_{10}(\text{High pre-pandemic achievement})_i(\text{CC Eligible})_i + \mathbf{X}_i\beta_x + \mathbf{Z}_i\beta_y + e_i$

In this example, which allows the effect of CC-eligibility to vary by pre-pandemic achievement level, the focal parameters are β_8, β_9, and β_{10}. These coefficients reveal whether CC program eligibility effects vary for students with higher pre-pandemic GPAs compared to those with lower pre-pandemic GPAs.

Estimating CC program participation effects entails replicating Equations 1 and 2, but with two key differences. First, the analytic sample is limited only to moderately-disadvantaged CC-eligible students who either received a Wired Connection or did not participate at all. Second, the binary CC-eligibility indicator variable is replaced with a binary CC-Wired Connection take-up indicator variable. The CC-Wired Connection indicator variable is then interacted with the three categorical variables capturing students' pre-pandemic academic achievement to assess skill-based heterogeneity in program participation effects.

RESULTS

The Effects of CC Eligibility

We first estimate CC program eligibility effects on the remote learning engagement index. Table 2, Model 1 suggests the main effect on the CC-eligibility indicator variable is significant and negative, though very small in magnitude. All else equal, moderately disadvantaged CC-eligible students rated 0.03 SDs lower on the three-component remote learning index than did otherwise-similar CC-ineligible students, suggesting eligibility for increased broadband access may yield a very slight detriment to remote learning engagement for the sample as a whole.

Table 2 about here

To assess whether this overall pattern obscures skill-based heterogeneity, we interact the CC eligibility indicator with the pre-pandemic achievement indicator variables and generate the predicted skill-technology complementarity pattern (Model 2). For students with the lowest baseline skills, CC eligibility exerts a nontrivial negative effect of 0.10 SD on the remote learning engagement index. But for students with the highest baseline skills, CC eligibility predicts a 0.04 *boost* to remote learning engagement. Students who were mid/high-achievers pre-pandemic see a less negative effect of eligibility compared to the lowest achievers. There is no significant difference in CC eligibility effects between the latter group and mid/low achievers.

Models 3-6 use the same analytic framework as Models 1 and 2 but generate program eligibility effect estimates for GPA rather than remote learning engagement outcomes. The results again show a negative main effect of CC eligibility: within this analytic subsample, program eligibility predicts a 0.05 point reduction in fall 2020 GPA. However, unlike the remote learning engagement outcome, this estimated effect does not appear to vary by pre-pandemic achievement level. Similar patterns apply when specifying the same model on student GPA in spring 2021, when most students returned to in-person instruction.

Figure 2A summarizes these results, by presenting predicted values of each outcome for CC-eligible and CC-ineligible students within our analytic subsample, stratified by pre-pandemic academic achievement, using model specifications from Table 2 that hold all other covariates at their means. The figure reveals that inequality in the predicted values of remote learning engagement by pre-pandemic achievement is considerably larger for CC-eligible versus CC-ineligible students.

Figure 2 about here

The Effects of a CC Wired Connection

Next, we estimate the main effects of taking up a CC-provided Wired Connection (versus not participating in the program) among moderately-disadvantaged CC eligible students. Table 3's models suggest that at least among this subsample, the CC Wired Connection "treatment" does not confer a significant direct effect on the remote learning engagement index or GPA.

Table 3 about here

However, a technology-skill complementarity pattern emerges across outcomes when the binary CC Wired Connection take-up variable is interacted with the pre-pandemic achievement indicator variables. Model 2, which includes these interaction terms and predicts the remote learning outcome, suggests that for moderately-disadvantaged CC-eligible students with low pre-pandemic achievement, take-up of a CC Wired Connection predicts a 0.11 SD reduction in remote learning engagement. But for otherwise similar students with higher pre-pandemic achievement levels, the CC Wired Connection boosts remote learning engagement. For mid/low, mid/high achievers, and high achievers, CC Wired Connection take-up predicts a 0.03, 0.04, and 0.06 SD boost to remote learning engagement, respectively.

Similar skill-technology heterogeneity patterns emerge when the outcome shifts to GPA. Model 4 suggests that for moderately-disadvantaged CC-eligible students with the lowest pre-pandemic achievement level, a Wired Connection predicts a fall 2020 GPA that is 0.05 points lower than it otherwise would be. The magnitude of this negative effect is attenuated for students with mid/low pre-pandemic achievement and then reverses in direction for mid/high and high achievers. These groups see substantively modest but statistically significant boosts of 0.02 and 0.06 GPA points associated with CC Wired Connection take-up.

Schools switched to hybrid instruction in the middle of the spring 2021 semester, with students attending school some days and engaging in remote learning other days. Even in this hybrid environment and at a time when the COVID-19 pandemic's toll in Chicago began to ebb, the same general pattern of skill-based heterogeneity in CC's achievement effects emerges. The lowest achievers see a small estimated GPA penalty of CC Wired Connection receipt (-0.05 points), while high achievers see a small estimated benefit (0.05 points).

Figure 2B summarizes these program participation analyses' results, presenting predicted values of each outcome for CC-Wired Connection and CC non-participants within our analytic subsample of moderately-disadvantaged CC-eligible students, stratified by pre-pandemic academic achievement, using model specifications from Table 3 that hold all other covariates at their means. The upshot is that both achievement and engagement inequality increased among CC Wired Connection recipients compared to otherwise-similar CC-eligible nonparticipants, reinforcing the skill-technology complementarity hypothesis.

ROBUSTNESS CHECKS

We run several robustness checks that further support our core finding of skill-based heterogeneity in the CC Wired Connection's engagement and achievement effects. First, we recalculate our CC program participation effect estimates by expanding the sample range of the CC-eligible analytic sample on which they were originally based. Specifically, we include all CC-eligible students, *regardless of disadvantage level/hardship score*, rather than just moderately-disadvantaged students. Table S3 in the Online Supplement presents these models (see Models 1, 3, 5). Across all three outcomes– remote learning engagement, fall 2020 GPA, and spring 2021 GPA– the same pattern of baseline skill-based heterogeneity in CC Wired Connection's estimated effects emerges for this larger group of CC-eligible students. The magnitude of these coefficients is similar to those reported above (i.e., for the smaller group of moderately-disadvantaged students).

Using this larger analytic sample, we add models that include school fixed effects (Table S3, Models 2, 4, 6), which simultaneously adjust for unobserved heterogeneity and school differences in grading practices. We excluded these fixed effects previously to ensure we had sufficient statistical power to capture small effects within a narrowed analytic sample (i.e., only moderately-disadvantaged students). But when we expand our analytic sample to include all CC-eligible students in our analytic sample and only compare those within the same school, our results closely mirror the program participation effect estimates presented above that were based on a narrower analytic sample, with models excluding school fixed effects; this consistency further assuages internal validity concerns. Specifically, in these school fixed effects models, students with the highest pre-pandemic GPAs who received a CC Wired Connection were more engaged in remote learning and achieved a higher fall 2020 GPA than otherwise would have been predicted. The reverse is true when comparing CC-eligible students within the same school who had the lowest pre-pandemic GPAs.

Next, we leverage the expansion of CC eligibility criteria in fall 2020 to compare estimated CC effects separately for: (a) those who were eligible for CC beginning in summer 2020 or earlier, before the program expanded eligibility to all students who were either eligible for free/reduced price lunch or Medicaid, and (b) those who had access to CC only part-way through the fall 2020 semester, after the eligibility expansion to all students who were either eligible for free/reduced price lunch or Medicaid. By stratifying the sample in this manner, we can run a falsification check whereby CC participation should only predict remote learning engagement in the early portion of the fall semester (i.e., September/October) among group (a), but not (b).

To execute this analysis, we re-estimate the remote learning engagement index based on log-in data from September and October 2020 only, and then use stratified models to estimate CC Wired Connection effects on this outcome among all of our analytic sample students who were CC-eligible in summer versus mid-fall 2020. As expected, the skill-technology heterogeneity pattern emerges among the former group of students. But for the latter group, the CC Wired Connection main effects and the interaction effects (i.e., with baseline achievement) do not approach significance (see Online Supplement Table S4).

As a final set of robustness checks, we assess other hypothesized sources of heterogeneity in technology's effects on engagement and achievement. Unlike the skill-based heterogeneity patterns discussed above, results (available upon request) reveal no evidence of grade level, race/ethnicity, or gender as moderators of CC program eligibility or participation effects on our engagement and achievement outcomes.

DISCUSSION

Reducing inequities in K-12 students' access to key technologies, like broadband Internet, has loomed large as a potential antidote to longstanding educational inequities. This intuition strengthened as the COVID-19 pandemic induced an abrupt shift to remote learning; a broadband connection suddenly became a key component of, rather than an optional supplement to, classroom instruction. However, the pandemic coincided with a striking divergence in educational outcomes by children's pre-pandemic academic skills. These patterns are suggestive of a potential skill-technology complementarity whereby students with higher achievement experience more of the benefits, and fewer of the detriments, associated with technology becoming increasingly accessible and increasingly important in the K-12 educational setting.

Our empirical analysis of the Chicago Connected broadband expansion program largely reinforces this skill-technology complementarity account. Program eligibility and participation (i.e., take-up of an offered wired broadband connection) predicted boosts to remote learning engagement in fall 2020 for 5th-8th grade students with high pre-pandemic GPAs but declines for students with low pre-pandemic GPAs. There were also heterogeneous effects of CC Wired Connection take-up by pre-pandemic GPA. The heterogeneous program participation effects detected when predicting spring 2021 GPA are suggestive of technology access' inequitable effects being internally valid and durable, beyond the remote learning context and beyond the pandemic's most acute phase.

It is important to note that our findings' internal validity depends on strong assumptions that are difficult to meet when conducting analyses on observational data, without randomization, and when program eligibility discontinuities (e.g., hardship index-based thresholds) changed multiple times throughout the timeframe of interest. We thus cannot fully rule out the possibility that unobserved factors account for engagement and achievement differences that we ascribed to the effects of Chicago Connected eligibility and participation. These factors could include difficult-to-observe pandemic-era conditions that differentially boosted students with high pre-pandemic GPAs and undermined students with low pre-pandemic GPAs, which might underlie our focal skill-technology complementarity to a degree.

These types of limitations are endemic to large-scale policy interventions implemented in volatile real-world conditions like pandemic-era Chicago. The pandemic upended myriad conditions that could affect children's educational engagement and achievement– and these shifts likely unfolded differently for different types of children. Thus, the internal and external validity of any educational research findings conducted during COVID-19 deserve close scrutiny. However, we went beyond most prior observational research on the technology access-educational equity link, by incorporating an unusually extensive set of control variables to ensure balance across comparison groups and by running myriad placebo tests and robustness checks, nearly all of which reinforced our core skill-technology complementary hypothesis.

Data limitations precluded us from clarifying what the counterfactual condition of ineligible and CC-eligible non-participants was. Did most have an alternative form of Internet connection, accessible by a computer instead of a smartphone? If most ineligible, non-participants did have some form of connection, was it as strong and reliable as the CC-sponsored connection, ensuring uninterrupted access to the Google Classroom platform? Lacking this clarity, it is difficult to fully assess what the estimated program eligibility and participation effects are capturing.

Data constraints also precluded us from testing potential explanations for our skill-based heterogeneity patterns. We speculate that academically thriving students may have higher executive function skills (Best et al., 2011; Effeney et al., 2013; Zelazo et al., 2017), increasing the likelihood they leveraged enhanced technology toward educational ends. Another possibility is that academic high-performers benefit from higher levels of parental supervision compared to students with similar demographic backgrounds and economic resources. Given parental supervision's apparent importance to productive Internet engagement among children, this difference may partially explain our results (Gallego et al., 2020; Islam et al., 2020).

Whatever the underlying explanations, the results point to potential complementarities between technology and skills that, if reinforced by studies with (quasi)experimental designs, have important implications for contemporary educational inequities– beyond the pandemic period. As seen in research on workplace technology, skill-based stratification in technology's benefits may be occurring in the K-12 educational setting as well. This skill-based complementarity may have been amplified in the absence of robust school-based technology supports that help ensure all students cultivate digital literacy skills and avoid the pitfalls of technological tools, particularly social media, including distraction and addiction (Orben et al., 2022). Given the pandemic-induced emergency shift to remote learning, it is little wonder that rapidly expanding technology access took precedence over developing extensive supports to ensure technology's use toward academic ends.

Future research should illuminate precisely how high-achieving students may be leveraging digital technologies to propel their education and clarify why low-achieving students may see declining academic engagement and performance with increased access to digital technologies. Qualitative studies would be particularly helpful in illuminating the mechanisms undergirding these dynamics. Retesting the skill-technology complementarity hypothesis in other geographic and temporal contexts would also be valuable: digital technology use in the educational setting plausibly looks very different in pandemic-era Chicago than in other environments.

Policymakers need not wait until these nuances are resolved before taking action. As technology's penetration of K-12 education continues, one key insight of this study is particularly important: bolstering educational equity via digital technologies requires more than simply providing a computer and Internet connection to students. Technology access may be a necessary but not sufficient condition for students to realize achievement gains in an educational landscape dominated by technology (Escueta et al., 2020). Leaders must develop strategies that counter technology's potential negative influences, especially on low-achieving students and other vulnerable subgroups.

To help guide these efforts, a key next step might entail examining whether and why certain teachers or schools vary in how they leverage technology (Rafalow, 2020). Have any specific practices effectively attenuated the skill-based gradient in technology's educational effects? These findings will foster more equitable intervention models, whereby inevitable increases in technological investments are linked with instructional supports aimed at ensuring all children, of all skill levels, evolve into highly engaged students and effective digital citizens.

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KEY STATEMENTS

Competing Interests

The authors declare no competing interests.

Data Availability

The data on which this study is based are restricted access and were obtained from Chicago Public Schools (CPS) under special contractual arrangements designed to protect sensitive student information. The analyses presented here were not preregistered.

Ethical Statements

The methodology for this study was approved by the University of Chicago's Institutional Review of Board (IRB) and Chicago Public Schools.

Informed Consent

Informed consent for participation in this study was not deemed necessary by University of Chicago's Institutional Review of Board (IRB) and Chicago Public Schools, given strong protections to student confidentiality and limited risk to participants.

Table 1. Descriptive Statistics for Analytic Sample of CPS Students in Grades 5-8 as of 2020-21 School Year

Analytic Subsample	CC-Ineligible: Nondisadvantaged	CC-Eligible: Moderately Disadvantaged (Hardship Index 1-5)			CC-Eligible: Severely Disadvantaged (Hardship Index 6-9)		
CC Participation Status, among CC-Eligible		CC Participant: Wired	CC Participant: Hotspot Only	CC No Takeup	CC Participant: Wired	CC Participant: Hotspot Only	CC No Takeup
Pandemic Engagement/Achievement							
Spring 2020 Course Pass Rate (Pre-CC Rollout)	0.90 (0.17)	0.86 (0.21)	0.78 (0.25)	0.86 (0.21)	0.82 (0.24)	0.77 (0.27)	0.81 (0.25)
Fall 2020 Remote Learning Engagement	0.13 (0.83)	0.06 (0.98)	-0.28 (1.24)	-0.00 (0.95)	0.03 (1.07)	-0.39 (1.30)	-0.12 (1.12)
Fall 2020 GPA	3.24 (0.84)	2.92 (0.95)	2.55 (1.03)	2.90 (0.93)	2.67 (0.98)	2.50 (0.98)	2.63 (0.96)
Spring 2021 GPA	3.19 (0.90)	2.86 (1.01)	2.50 (1.09)	2.85 (0.98)	2.66 (1.02)	2.56 (1.00)	2.64 (0.99)
Pre-Pandemic Engagement/Achievement (2017-18/2018-19)							
High achiever	0.33 (0.47)	0.17 (0.37)	0.09 (0.28)	0.16 (0.36)	0.10 (0.30)	0.06 (0.23)	0.08 (0.27)
Mid/high achiever	0.38 (0.49)	0.42 (0.49)	0.33 (0.47)	0.40 (0.49)	0.33 (0.47)	0.28 (0.45)	0.31 (0.46)
Mid/low achiever	0.20 (0.40)	0.28 (0.45)	0.33 (0.47)	0.29 (0.45)	0.36 (0.48)	0.35 (0.48)	0.36 (0.48)
Low achiever	0.09 (0.28)	0.14 (0.34)	0.25 (0.43)	0.15 (0.36)	0.22 (0.41)	0.31 (0.46)	0.25 (0.43)
Mean attendance rate	0.96 (0.03)	0.96 (0.04)	0.95 (0.05)	0.96 (0.04)	0.96 (0.04)	0.95 (0.05)	0.95 (0.04)
Mean NWEA score	0.42 (1.00)	0.11 (0.87)	-0.12 (0.86)	0.09 (0.86)	-0.40 (0.95)	-0.50 (0.96)	-0.41 (0.95)
Student Sociodemographics							
White	0.30 (0.46)	0.06 (0.24)	0.03 (0.16)	0.08 (0.27)	0.02 (0.12)	0.01 (0.10)	0.02 (0.15)
Black	0.20 (0.40)	0.30 (0.46)	0.54 (0.50)	0.38 (0.48)	0.23 (0.42)	0.54 (0.50)	0.37 (0.48)
Hispanic	0.40 (0.49)	0.57 (0.50)	0.39 (0.49)	0.47 (0.50)	0.73 (0.44)	0.42 (0.49)	0.57 (0.49)
Asian	0.07 (0.25)	0.06 (0.24)	0.03 (0.17)	0.06 (0.23)	0.02 (0.13)	0.03 (0.18)	0.03 (0.16)
Other	0.03 (0.17)	0.01 (0.08)	0.01 (0.09)	0.01 (0.12)	0.00 (0.06)	0.01 (0.07)	0.01 (0.08)
Free-lunch eligible	0.38 (0.48)	0.86 (0.35)	0.89 (0.32)	0.77 (0.42)	0.96 (0.19)	0.98 (0.15)	0.96 (0.20)
Reduced-lunch eligible	0.04 (0.20)	0.08 (0.27)	0.06 (0.24)	0.10 (0.31)	0.02 (0.15)	0.01 (0.12)	0.02 (0.15)
Precariously housed	0.00 (0.06)	0.02 (0.13)	0.12 (0.33)	0.01 (0.11)	0.03 (0.18)	0.24 (0.43)	0.03 (0.16)
Community hardship index	-0.48 (1.09)	-0.01 (0.87)	0.38 (0.88)	-0.04 (0.95)	0.32 (0.74)	0.63 (0.79)	0.41 (0.79)
Limited English	0.11 (0.32)	0.13 (0.34)	0.06 (0.24)	0.10 (0.30)	0.41 (0.49)	0.22 (0.42)	0.31 (0.46)
Any disability/disturbance	0.12 (0.32)	0.07 (0.25)	0.09 (0.28)	0.08 (0.28)	0.22 (0.42)	0.26 (0.44)	0.24 (0.42)
Student *N*	23,451	4,607	986	19,261	8,196	2,977	17,578

Notes [1] Sociodemographic measures are captured based on data from the 2020-21 school year. [2] Some students, particularly in households with multiple CPS children, were given access to both a CC-provided Wired Connection and a CC-provided Hotspot. These children are coded as being CC-Wired recipients. [3] The following variables are standardized relative to the full analytic sample (mean=0, SD=1): Fall 2020 Remote Learning Engagement, Mean NWEA score, Community hardship index. [4]High achiever defined as pre-pandemic average GPA (pooled across all semesters in 2017-18/2018-19 school years) over 1 SD above analytic sample mean; Mid/high achiever's average GPA during same timeframe was between 0 and 1 SD of analytic sample mean; Mid/low achiever's average GPA was between -1 SD and 0. Low achievers' average GPA was more than 1 SD below analytic sample mean. [5] Spring 2021 GPA was unavailable for a small portion (<1%) of the analytic sample.

Table 2. Select Coefficients from OLS Regressions Predicting Pandemic-Era Outcomes for CC-Ineligible & Moderately Disadvantaged CC-Eligible Students

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Outcomes	Remote Learning Engagement Index (Fall 2020)		Fall 2020 GPA		Spring 2021 GPA	
CC-Ineligible (ref)						
CC Eligible/Moderately Disadvantaged	-0.034*	-0.096**	-0.047**	-0.014	-0.044**	0.005
	(0.016)	(0.036)	(0.014)	(0.037)	(0.016)	(0.039)
CC Eligible/Moderately Disadvantaged X Mid/Low Achiever ('17-18/'18-19)		0.031		-0.064		-0.059
		(0.034)		(0.034)		(0.034)
X Mid/High Achiever		0.071*		-0.041		-0.064
		(0.033)		(0.034)		(0.036)
X High Achiever		0.131*		0.008		-0.038
		(0.051)		(0.035)		(0.038)
Low Achiever (ref)						
Mid/Low Achiever	0.158**	0.136**	0.371**	0.411**	0.306**	0.344**
	(0.024)	(0.033)	(0.020)	(0.032)	(0.022)	(0.032)
Mid/High Achiever	0.247**	0.199**	0.800**	0.825**	0.710**	0.750**
	(0.034)	(0.041)	(0.028)	(0.040)	(0.031)	(0.041)
High Achiever	0.282**	0.213**	1.162**	1.167**	1.129**	1.158**
	(0.052)	(0.067)	(0.035)	(0.047)	(0.039)	(0.049)
Student *N*	48,305	48,305	48,305	48,305	47,775	47,775

Notes

[1] Remote Learning Engagement Index is constructed as a three-component index drawn from Google Classroom data in Fall 2020: mean number of minutes logged in per school day with a staff member present; mean number of log-ins per day; and percent of school days in which the student logged in at least once. Each of the three measures is standardized relative to the full analytic sample of both CC-Ineligible and CC-Eligible students to have a mean of 0 and a standard deviation of 1 and then, once standardized, all three are averaged and re-standardized. [2] Moderately Disadvantaged CC-Eligible Students refers to CC-Eligible students within households scoring 1-5 on CPS' Hardship Index, which ranged from 1-9. [3] All models include the following controls: grade-level fixed effects; pre-pandemic average (i.e., 2017-18 and 2018-19) math/reading NWEA standardized test scores and attendance rates; the number of courses taken in Spring 2020 and Fall 2020; the number of courses taken in Spring 2021 (for Models 5 & 6, only); and sociodemographic controls (all measured in 2020-21), including indicators for: gender; race (i.e., Black, Hispanic, Asian, Other, White-ref); limited English proficiency; cognitive disability; learning disability; emotional or behavioral disturbance; or other disability; housing instability/homelessness; eligible for free lunch; eligible for reduced-price lunch. Continuous controls include an index capturing the degree of community hardship in the student's home census block group and the number of CPS students in the child's household. [4] Standard errors are clustered by school the student attended in Fall 2020 (for models 1-4) or in Spring 2021 (for models 5-6). [6] ** $p < 0.01$, * $p < 0.05$ (two-tailed test).

Table 3. Select Coefficients from OLS Regressions Predicting Pandemic-Era Outcomes for Moderately Disadvantaged CC-Eligible Students (*Non*-Hotspot Only)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Outcomes	Remote Learning Engagement Index (Fall 2020)		Fall 2020 GPA		Spring 2021 GPA	
CC-Eligible Non-Participant (ref)						
CC-Eligible Wired Connection	0.018	-0.110	0.004	-0.053	0.010	-0.049
	(0.016)	(0.059)	(0.014)	(0.032)	(0.015)	(0.038)
CC-Eligible Wired Connection X Mid/Low Achiever (‘17-18/’18-19)		0.136*		0.030		0.034
		(0.065)		(0.040)		(0.046)
X Mid/High Achiever		0.147*		0.075*		0.081
		(0.063)		(0.037)		(0.043)
X High Achiever		0.174**		0.111**		0.097*
		(0.065)		(0.038)		(0.046)
Low Achiever (ref)						
Mid/Low Achiever	0.149**	0.124**	0.338**	0.333**	0.278**	0.272**
	(0.028)	(0.029)	(0.022)	(0.024)	(0.025)	(0.027)
Mid/High Achiever	0.227**	0.201**	0.769**	0.755**	0.679**	0.664**
	(0.041)	(0.041)	(0.029)	(0.030)	(0.033)	(0.035)
High Achiever	0.278**	0.245**	1.152**	1.131**	1.112**	1.094**
	(0.050)	(0.049)	(0.037)	(0.037)	(0.042)	(0.043)
Student *N*	23,868	23,868	23,868	23,868	23,663	23,663

Notes

[1] Remote Learning Engagement Index is constructed as a three-component index drawn from Google Classroom data in Fall 2020: mean number of minutes logged in per school day with a staff member present; mean number of log-ins per day; and percent of school days in which the student logged in at least once. Each of the three measures is standardized relative to the full analytic sample of both CC-Ineligible and CC-Eligible students to have a mean of 0 and a standard deviation of 1 and then, once standardized, all three are averaged and re-standardized. [2] Moderately Disadvantaged CC-Eligible Students refers to CC-Eligible students within households scoring 1-5 on CPS’ Hardship Index, which ranged 1-9. [3] All models include the following controls: grade-level fixed effects; pre-pandemic average (i.e., 2017-18 and 2018-19) math/reading NWEA standardized test scores and attendance rates; the proportion of Spring 2020 courses passed; the number of courses taken in Spring 2020 and Fall 2020; the number of courses taken in Spring 2021 (for Models 5 & 6); and sociodemographic controls (all measured in 2020-21), including indicators for: gender; race (i.e., Black, Hispanic, Asian, Other, White-ref); limited English proficiency; cognitive disability; learning disability; emotional or behavioral disturbance; or other disability; housing instability/homelessness; eligible for free lunch; eligible for reduced-price lunch. Continuous controls include an index capturing the degree of community hardship in the student’s home census block group and the number of CPS students in the child’s household. [4] Some students, particularly in households with multiple CPS children, were given access to both a CC-provided Wired Connection and a CC-provided Hotspot. These children are coded as being CC-Wired Connection recipients. [5] Standard errors are clustered by school the student attended for the majority of the 2020-2021 school year. [6] ** $p < 0.01$, * $p < 0.05$ (two-tailed test).

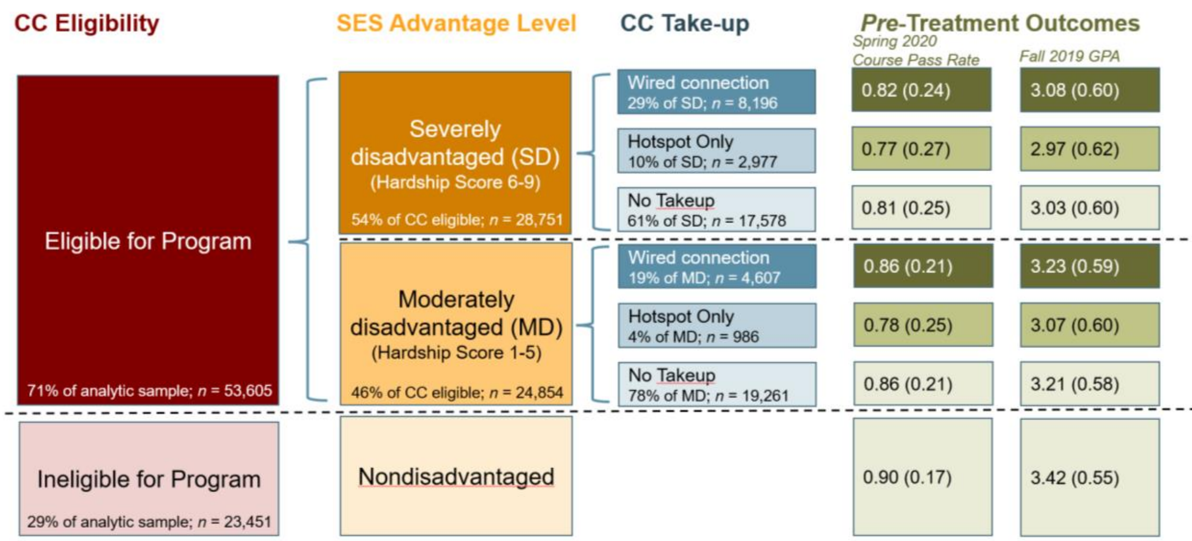

Figure 1. Chicago Connected program participation pathways, socioeconomic advantage levels, and pre-treatment achievement/engagement outcomes for CPS students in 5th-8th grade in fall 2020.

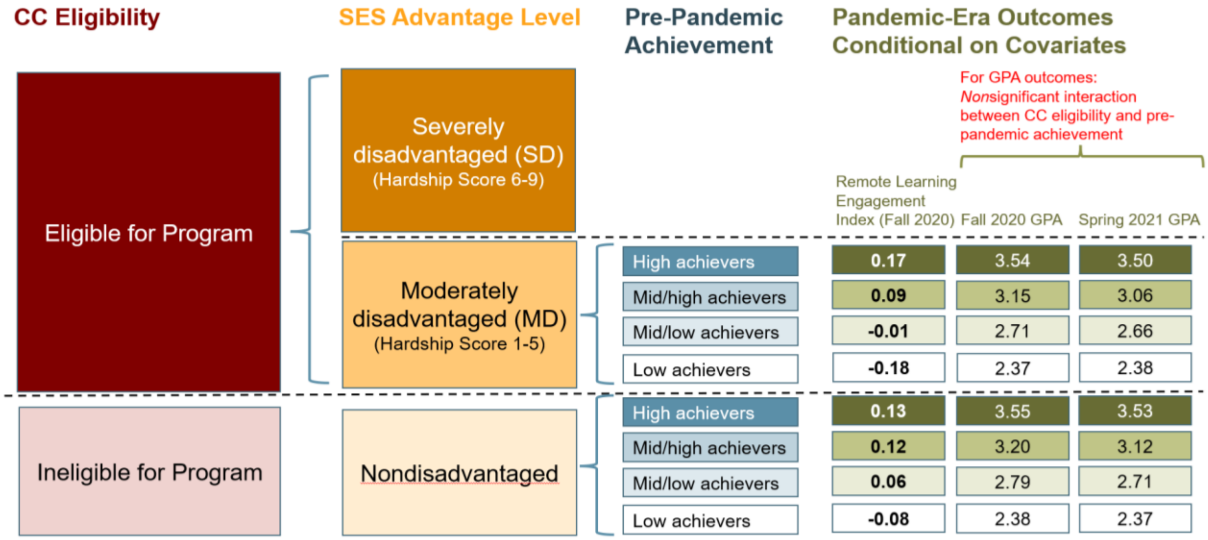

Figure 2A. Simulations of Chicago Connected program eligibility effects: predicted pandemic-era engagement and achievement outcomes, stratified by CC-eligibility and pre-pandemic achievement level, holding all other covariates at their means.

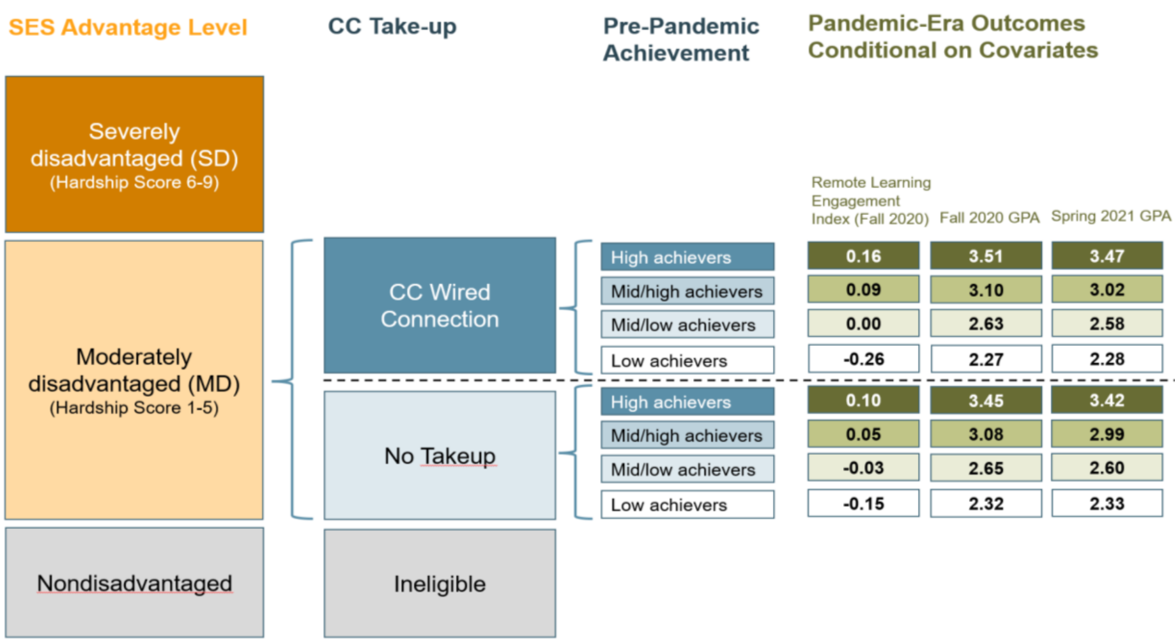

Figure 2B. Simulations of Chicago Connected program participation effects: predicted pandemic-era engagement and achievement outcomes among moderately disadvantaged CC-eligible students, stratified by CC participation status and pre-pandemic achievement level, holding all other covariates at their means.

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Table S1. Select Coefficients from OLS Placebo Tests Predicting *Pre*-Treatment Outcomes for CC-Ineligible & Moderately Disadvantaged CC-Eligible Students

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Outcome	Proportion of Spring 2020 Courses Passed		Fall 2019 GPA		NWEA 2019 Mean Reading/Math Score	
CC-Ineligible (ref)						
CC Eligible/Moderately Disadvantaged	-0.004 (0.005)	-0.016 (0.012)	-0.009 (0.005)	0.008 (0.018)	0.002 (0.006)	-0.018 (0.015)
CC Eligible/Moderately Disadvantaged X Mid/Low Achiever ('17-18/'18-19)		0.005 (0.010)		-0.023 (0.018)		0.023 (0.018)
X Mid/High Achiever		0.015 (0.011)		-0.026 (0.018)		0.032 (0.017)
X High Achiever		0.023 (0.012)		-0.004 (0.020)		0.000 (0.019)
Low Achiever (ref)						
Mid/Low Achiever	0.065** (0.007)	0.061** (0.010)	0.396** (0.014)	0.410** (0.017)	0.090** (0.012)	0.075** (0.016)
Mid/High Achiever	0.105** (0.010)	0.095** (0.012)	0.747** (0.017)	0.763** (0.020)	0.212** (0.015)	0.193** (0.019)
High Achiever	0.130** (0.012)	0.117** (0.015)	1.017** (0.020)	1.023** (0.024)	0.389** (0.018)	0.383** (0.022)
Student *N*	48,305	48,305	48,140	48,140	45,806	45,806

Notes

[1] Moderately Disadvantaged CC-Eligible Students refers to CC-Eligible students within households scoring 1-5 on CPS' Hardship Index, which ranged from 1-9.
[2] All models include the following controls: grade-level fixed effects; and sociodemographic controls (all measured in 2019-20), including indicators for: gender; race (i.e., Black, Hispanic, Asian, Other, White-ref); limited English proficiency; cognitive disability; learning disability; emotional or behavioral disturbance; or other disability; housing instability/homelessness; eligible for free lunch; eligible for reduced-price lunch. Continuous controls include: pre-pandemic average (i.e., 2017-18 and 2018-19) math/reading NWEA standardized test scores and attendance rates; an index capturing the number of courses taken in the semester in which the outcome was measured; the degree of community hardship in the student's home census block group; and the number of CPS students in the child's household. All of these continuous control variables are standardized relative to the full analytic sample to have mean of 0 and SD of 1. [3] Some students, particularly in households with multiple CPS children, were given access to both a CC-provided Wired Connection and a CC-provided Hotspot. These children are coded as being CC-Wired Connection recipients. [4] Sample sizes are slightly smaller for Models 3-6 due to adjustments in control variables included (i.e., number of Fall 2019 courses for Models 3-4; Mean Reading/Math Score for NWEA 2018). [4] Standard errors are clustered by school the student attended in Spring 2020 (Models 1 and 2), in Fall 2019 (Models 3 and 4), and in Spring 2019 (Models 5 and 6). [5] ** $p < 0.01$, * $p < 0.05$ (two-tailed test).

Table S2. Select Coefficients from OLS Placebo Tests Predicting *Pre*-Treatment Outcomes for Moderately Disadvantaged CC-Eligible Students

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Outcome	Proportion of Spring 2020 Courses Passed		Fall 2019 GPA		NWEA 2019 Mean Reading/Math Score	
CC-Eligible Non-Participant (ref)						
CC Participant: Wired	0.006	-0.009	-0.005	-0.023	0.015	0.031
	(0.004)	(0.013)	(0.008)	(0.028)	(0.008)	(0.025)
CC Participant: Hotspot Only	-0.048**	-0.027	0.022	0.056	-0.001	-0.010
	(0.014)	(0.021)	(0.024)	(0.047)	(0.017)	(0.040)
CC Participant: Wired						
X Mid/Low Achiever		0.011		0.006		-0.032
('17-18/'18-19)		(0.015)		(0.031)		(0.030)
X Mid/High Achiever		0.020		0.028		-0.011
		(0.014)		(0.028)		(0.027)
X High Achiever		0.024		0.035		-0.018
		(0.014)		(0.029)		(0.029)
CC Participant: Hotspot Only						
X Mid/Low Achiever		-0.061**		-0.032		0.034
		(0.022)		(0.045)		(0.050)
X Mid/High Achiever		-0.006		-0.034		-0.007
		(0.021)		(0.045)		(0.050)
X High Achiever		-0.004		-0.141*		-0.011
		(0.027)		(0.057)		(0.058)
Low Achiever (ref)						
Mid/Low Achiever	0.063**	0.065**	0.379**	0.380**	0.105**	0.105**
	(0.008)	(0.009)	(0.016)	(0.016)	(0.014)	(0.015)
Mid/High Achiever	0.105**	0.103**	0.720**	0.717**	0.242**	0.239**
	(0.011)	(0.011)	(0.020)	(0.020)	(0.017)	(0.017)
High Achiever	0.132**	0.129**	0.993**	0.991**	0.409**	0.409**
	(0.013)	(0.014)	(0.023)	(0.023)	(0.020)	(0.022)
Student *N*	24,854	24,854	24,782	24,782	23,766	23,766

Notes

[1] Moderately Disadvantaged CC-Eligible Students refers to CC-Eligible students within households scoring 1-5 on CPS' Hardship Index, which ranged 1-9.
[2] All models include the following controls: grade-level fixed effects; CPS Hardship Index fixed effects; pre-pandemic average (i.e., 2017-18 and 2018-19) math/reading NWEA standardized test scores and attendance rates; the number of courses taken in the semester when the outcome is measured; and sociodemographic controls (all measured in 2019-20), including indicators for: gender; race (i.e., Black, Hispanic, Asian, Other, White-ref); limited English proficiency; cognitive disability; learning disability; emotional or behavioral disturbance; or other disability; housing instability/homelessness; eligible for free lunch; eligible for reduced-price lunch; and whether the student's household included three or more CPS students. Continuous controls include an index capturing the degree of community hardship in the student's home census block group. [3] Some students, particularly in households with multiple CPS children, were given access to both a CC-provided Wired Connection and a CC-provided Hotspot. These children are coded as being CC-Wired Connection recipients. [4] Sample sizes are slightly smaller for Models 3-6 due to adjustments in control variables included (i.e., number of Fall 2019 courses for Models 3-4; Mean Reading/Math Score for NWEA 2018). [5] Standard errors are clustered by school the student attended in Spring 2020 (Models 1 and 2), in Fall 2019 (Models 3 and 4), and in Spring 2019 (Models 5 and 6). [5] ** $p < 0.01$, * $p < 0.05$ (two-tailed test).

Table S3. Select Coefficients from OLS Regressions Predicting Pandemic-Era Outcomes for All CC-Eligible Students, Except for Hotspot Only CC Participants

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Outcomes	Remote Learning Engagement Index (Fall 2020)		Fall 2020 GPA		Spring 2021 GPA	
CC-Eligible Non-Participant (ref)						
CC-Eligible Wired Connection	-0.002	-0.008	-0.051*	-0.037*	-0.071**	-0.047*
	(0.033)	(0.028)	(0.020)	(0.017)	(0.022)	(0.019)
CC-Eligible Wired Connection X Mid/Low Achiever (‘17-18/’18-19)	0.030	0.031	0.036	0.025	0.074**	0.056*
	(0.035)	(0.032)	(0.025)	(0.023)	(0.027)	(0.024)
X Mid/High Achiever	0.065	0.050	0.069**	0.056**	0.095**	0.083**
	(0.035)	(0.030)	(0.024)	(0.021)	(0.025)	(0.022)
X High Achiever	0.089*	0.069*	0.122**	0.098**	0.133**	0.102**
	(0.040)	(0.031)	(0.027)	(0.025)	(0.031)	(0.028)
Low Achiever (ref)						
Mid/Low Achiever	0.146**	0.119**	0.351**	0.316**	0.285**	0.257**
	(0.025)	(0.017)	(0.017)	(0.013)	(0.020)	(0.015)
Mid/High Achiever	0.229**	0.194**	0.785**	0.722**	0.682**	0.622**
	(0.034)	(0.019)	(0.025)	(0.021)	(0.030)	(0.023)
High Achiever	0.272**	0.214**	1.172**	1.095**	1.095**	1.021**
	(0.044)	(0.025)	(0.033)	(0.029)	(0.039)	(0.031)
School Fixed Effects		X		X		X
Student *N*	49,642	49,642	49,642	49,642	49,256	49,256

Notes [1] Remote Learning Engagement Index is constructed as a three-component index drawn from Google Classroom data in Fall 2020: mean number of minutes logged in per school day with a staff member present; mean number of log-ins per day; and percent of school days in which the student logged in at least once. Each measure is standardized relative to the full analytic sample of both CC-Ineligible and CC-Eligible students to have mean=0 and SD=1; once standardized, all three are averaged and re-standardized. [2] All CC-Eligible Students includes all analytic sample students within households that received any score (1-9) on CPS' Hardship Index.[3] All models include fixed effects capturing the hardship index score assigned to each child's household and the following controls: grade-level fixed effects; pre-pandemic average (i.e., 2017-18, '18-19) math/reading NWEA standardized test scores and attendance rates; the proportion of Spring 2020 courses passed; the number of courses taken in Spring 2020 and Fall 2020; the number of courses taken in Spring 2021 (for Models 5 & 6); and sociodemographic controls (all measured in 2020-21), including indicators for: gender; race (Black, Hispanic, Asian, Other, White-ref); limited English proficiency; cognitive disability; learning disability; emotional or behavioral disturbance; or other disability; housing instability/homelessness; eligible for free lunch; eligible for reduced-price lunch. Continuous controls include an index capturing community hardship in the student's home census block group and the number of CPS students in the child's household. [4] Some students were given access to both a CC-provided Wired Connection and a CC-provided Hotspot. These children are coded as being CC-Wired Connection recipients. [5] Standard errors are clustered by school the student attended for the majority of the 2020-2021 school year. [6] ** $p < 0.01$, * $p < 0.05$ (two-tailed test).

Table S4. Select Coefficients from OLS Regressions Predicting September/October 2020 Remote Learning Engagement Index (Hotspot Only Students Excluded)

	Model 1	Model 2
Analytic Sample	CC-Eligible: CC Offer Extended Early (Summer 2020 or earlier)	CC-Eligible: CC Offer Extended Late (Nov/Dec 2020)
CC-Eligible Non-Participant (ref)		
CC-Eligible Wired Connection	-0.030 (0.028)	-0.026 (0.019)
CC-Eligible Wired Connection X Mid/Low Achiever (‘17-18/’18-19)	0.033 (0.033)	0.017 (0.201)
X Mid/High Achiever	0.054 (0.031)	0.003 (0.196)
X High Achiever	0.064* (0.032)	0.075 (0.195)
Low Achiever (ref)		
Mid/Low Achiever	0.117** (0.019)	0.084 (0.062)
Mid/High Achiever	0.202** (0.022)	0.150** (0.059)
High Achiever	0.219** (0.028)	0.186** (0.062)
School Fixed Effects (Attended, ‘20-21)	X	X
Student *N*	45,309	4,333

Notes [1] September/October 2020 Remote Learning Engagement Index is constructed as a two-component index drawn from Google Classroom data collected during September and October 2020: mean number of minutes logged in per school day with a staff member present; and percent of school days in which the student logged in at least once. Each measure is standardized relative to the full analytic sample of both CC-Ineligible and CC-Eligible students to have mean=0 and SD=1; once standardized, both are averaged and re-standardized. [2] All models include the following controls: grade-level fixed effects; pre-pandemic average (i.e., 2017-18, ‘18-19) math/reading NWEA standardized test scores and attendance rates; the proportion of Spring 2020 courses passed; the number of courses taken in Spring 2020 and Fall 2020; and sociodemographic controls (all measured in 2020-21), including indicators for: gender; race (Black, Hispanic, Asian, Other, White-ref); limited English proficiency; cognitive disability; learning disability; emotional or behavioral disturbance; or other disability; housing instability/homelessness; eligible for free lunch; eligible for reduced-price lunch. Continuous controls include an index capturing community hardship in the student’s home census block group and the number of CPS students in the child’s household. [4] Some students were given access to both a CC-provided Wired Connection and a CC-provided Hotspot. These children are coded as being CC-Wired Connection recipients. [5] Standard errors are clustered by school the student attended for the majority of the 2020-2021 school year. [6] ** $p < 0.01$, * $p < 0.05$ (two-tailed test).

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Methodological Appendix

Full set of control variables

Lagged measures of student academic engagement/achievement (all continuous, unless noted)

- Course pass rate (spring 2020)
- Number of courses taken (spring 2020)
- Mean attendance rate (2018-2019 school years)
- Mean NWEA Math and English/Language Arts standardized scores (spring 2018/2019)
- Categorical measure of pre-pandemic mean GPA (averaging total GPA across every semester in 2017-18 and 2018-19 school years)
    - (1) *low-achievers*: pre-pandemic average GPA is more than one SD below the analytic sample mean (<2.73);
    - (2) *mid/low achievers*: pre-pandemic average GPA is between one SD below the analytic sample mean and the mean (2.73-3.25);
    - (3) *mid/high achievers*: pre-pandemic average GPA is just above the analytic sample mean and one SD above it (3.26-3.78);
    - (4) *high achievers*: pre-pandemic average GPA exceeds one SD above analytic sample mean (>3.78)

Sociodemographic controls (all binary indicators, unless noted)

- Grade level (ref: fifth; sixth, seventh, eighth)
- Gender (ref: male; female)
- Race/ethnicity (ref: White; Latino, Black, Asian, Other)

- Free or reduced price lunch eligibility
- Limited English proficiency
- Cognitive disability
- Learning disability
- Emotional or behavioral disturbance
- Other disability
- Housing instability/homelessness
- Whether the child's household included three or more CPS students
- Community hardship index: a continuous variable capturing poverty and unemployment rates, income levels, overcrowded housing, and public assistance receipt within students' residential Census block groups