Annenberg Institute
BROWN UNIVERSITY

EdWorkingPaper No. 26-1565

Expanded High-Speed Broadband Internet Access at Home and Student Achievement: Evidence from a Federal Subsidy Program

Nicolas Pardo
University of Southern California

Jared N. Schachner
University of Southern California

Hernan Galperin
University of Southern California

François Bar
University of Southern California

Policymakers have argued that expanding home broadband would improve educational outcomes. However, prior studies report mixed, null, or negative effects on achievement, with limited post-pandemic evidence. Using district-level achievement and enrollment data from a national broadband subsidy program serving 23 million low-income households, we examine whether increased uptake is associated with reading/language arts (RLA) and math achievement among economically disadvantaged (ECD) students across 7,825 districts from 2021–22 to 2023–24. Two-way fixed-effects estimates indicate that doubling subsidy uptake is associated with a 0.02 SD decline in math achievement and a 0.05 SD decline in RLA achievement, equivalent to 3.3% and 8.3% of the ECD/non-ECD gap. A placebo test using non-ECD students and an instrumental variables specification support these findings.

VERSION: September 2026

Suggested citation: Pardo, Nicolas, Jared N. Schachner, Hernan Galperin, and François Bar. (2026). Expanded High-Speed Broadband Internet Access at Home and Student Achievement: Evidence from a Federal Subsidy Program. (EdWorkingPaper: 26-1565). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/3wm9-2n46

Expanded High-Speed Broadband Internet Access at Home and Student Achievement: Evidence from a Federal Subsidy Program

Nicolas Pardo, Jared N. Schachner, Hernan Galperin, and François Bar

University of Southern California

Keywords: Broadband, home internet, achievement

Abstract

Policymakers have argued that expanding home broadband would improve educational outcomes. However, prior studies report mixed, null, or negative effects on achievement, with limited post-pandemic evidence. Using district-level achievement and enrollment data from a national broadband subsidy program serving 23 million low-income households, we examine whether increased uptake is associated with reading/language arts (RLA) and math achievement among economically disadvantaged (ECD) students across 7,825 districts from 2021–22 to 2023–24. Two-way fixed-effects estimates indicate that doubling subsidy uptake is associated with a 0.02 SD decline in math achievement and a 0.05 SD decline in RLA achievement, equivalent to 3.3% and 8.3% of the ECD/non-ECD gap. A placebo test using non-ECD students and an instrumental variables specification support these findings.

Since the rapid expansion of Internet adoption in the mid-1990s, policymakers and researchers have raised concerns about the educational implications of limited home connectivity among low-income K-12 students, commonly referred to as the homework gap (Anderson & Perrin, 2018). These concerns became especially salient during the COVID-19 pandemic, when the shift to remote learning in many U.S. school districts rendered a reliable residential broadband connection essential for students' engagement in school (Klein, 2021; Teräs et al., 2020). However, evidence on the educational benefits of residential broadband from both the pre-pandemic era and the acute pandemic phase remains decidedly mixed, and evidence on pandemic-era expansions in broadband access on post-pandemic educational outcomes is vanishingly rare.

This study examines the impact of a federal broadband subsidy program on post-pandemic learning recovery. Although efforts to expand access to high-quality internet connection date back several decades, the scale of the subsidy programs implemented in response to the COVID-19 pandemic was unprecedented. Underlying these pandemic-era direct-to-consumer broadband subsidy programs was the assumption that reducing income-based disparities in home connectivity is essential for narrowing educational achievement gaps, particularly during a time when educational resources were increasingly provided online (Prothero, 2022). At its peak in early 2024, the Affordable Connectivity Program (ACP), which provided a $30/month subsidy to eligible households, served more than 23M households. The ACP program coincided with a period of steep learning losses for K-12 students in the wake of the COVID-19 pandemic and the widening of stubbornly large student achievement gaps (Betthäuser et al., 2023; Engzell et al., 2021). Although slumping levels of and growing gaps in student achievement predate the pandemic, evidence suggests that school closures and other pandemic-related disruptions sharply accelerated learning losses and contributed to the widening of pre-existing gaps (Wyckoff, 2025). However, as the pandemic receded from 2022 onward, student achievement, especially in reading, showed limited recovery relative to 2019 benchmarks (Lewis & Kuhfeld, 2024). Although many factors could contribute to these concerning patterns, expanded access to residential broadband through programs such as ACP could have played a role, generating unintended consequences on some educational outcomes in the post-pandemic period.

We probe these possibilities, by drawing on district-level student achievement data matched with program enrollment records across 7,825 school districts for the 2022-24 period, to evaluate whether increases in district families' broadband subsidy uptake rate are associated with district-level declines in reading and math achievement among economically disadvantaged (ECD) students. Specifically, we address the following research questions:

RQ1: Are increases in program uptake associated with declining reading and math achievement scores among students in grades 3-8?

RQ2: Does the relationship between program uptake and student achievement vary by subject and across grade levels?

The results of our full-sample model specification, which includes district-grade fixed effects and several time-varying controls, suggest a small but statistically significant negative association between district-level estimates of program uptake and student test scores, that is larger for reading than for math. More specifically, a doubling of program uptake in a district is associated with a 0.02 SD decrease in math and a 0.05 SD decrease in reading/language arts (RLA) achievement among disadvantaged students. This is equivalent to approximately 3.3% and 8.3% of the average achievement gap between ECD and non-ECD students (0.60 SD), respectively. We also find evidence of impact heterogeneity across grade levels, with the effects increasing from elementary to middle school, particularly for math achievement.

Although the magnitude of the observed effects is relatively modest, the findings suggest that expanding home connectivity alone may not support, and may in fact hinder, post-pandemic learning recovery among disadvantaged students. More broadly, the results raise important questions about the effective use of technologies to support learning outside the school setting. We conclude by discussing several potential mechanisms that could underlie the results and point to evidence from other sources – including large-scale time-use surveys among teens – that are congruent with increased access to high-quality broadband Internet coinciding with less time spent on educationally-enriching activities. Although we cannot directly test these mechanisms, we outline promising avenues for future research aimed at identifying the conditions under which expanded access to high-quality Internet connections at home propels or impedes educational outcomes.

Background

Although residential Internet access has expanded considerably since the mid 1990s, significant income-based gaps among K-12 students persisted in the years leading up to the pandemic. According to estimates reported by NCES (2020), 14% of children below the poverty line had no home connectivity in 2019, and another 16% depended solely on a smartphone, suggesting that nearly one-third of low-income children lacked access to a reliable internet connection at home. Following the onset of the COVID-19 pandemic and the widespread closure of schools in early 2020, ensuring home connectivity for low-income students became a policy priority. Early interventions, led primarily by state and local governments in partnership with school districts, focused on facilitating access to remote learning platforms through the distribution of laptops and tablets alongside mobile hotspots and other temporary connectivity solutions (Burke, 2020; Klein, 2021). Ensuring uniform access to remote learning was a key priority under school closures.

Impacts of Home Connectivity on Students

Prior research suggests that internet access may affect K-12 student achievement through multiple channels. Home broadband could increase the efficiency of students' completion of homework or studying, including through access to study tools and educational resources like Khan Academy (Campbell, 2024; Vigdor & Ladd, 2010). Students may also benefit academically through their parents' use of online resources, including school websites and parent portals which facilitate communication with teachers and schools (Caldarulo et al., 2023). At the same time, home broadband may depress academic achievement through time displacement. By facilitating access to activities like gaming, streaming, and social media, broadband may reduce the time spent on homework and other educational activities (Cambini et al., 2021; Malamud et al., 2018). Parental supervision may play a critical role in limiting these distractions by influencing both the amount of time students spend online and the types of activities they engage in (Henriksen et al., 2022; Vigdor & Ladd, 2010). Another mechanism through which home broadband may negatively affect academic achievement is by making it easier for students to cheat and plagiarize (Campbell, 2024). This concern may become increasingly important as generative AI enables students to outsource a growing range of academic tasks using an expanding arsenal of tools (Goldstein, 2026).

Some studies examine the relationship between broadband and academic achievement at the extensive margin (whether a student has access) while others focus on the effects of faster internet or more reliable internet service (the intensive margin). As noted above, policy interventions to expand broadband access have primarily sought to reduce income-based disparities in home connectivity and, during the pandemic, facilitate remote learning. Intuitively, improved access should improve educational outcomes. However, the empirical evidence suggests a more complex relationship between broadband access, internet speed, and student achievement. Evidence from the pre-pandemic period is decidedly mixed, while relatively few studies examine outcomes during the pandemic. To date, we are not aware of any research examining the effects of broadband expansion in the post-pandemic era. We review the pre-pandemic literature distinguishing between studies covering the early internet era, before the widespread adoption of high-speed broadband (through 2010) and those examining the period after 2010.

The Early Broadband Period

Evidence from quasi-experimental studies in this period is decidedly mixed, with some studies finding no effect (Faber et al, 2015), others documenting positive effects (Sanchis-Guarner et al, 2025; Dettling et al., 2018), and still others reporting negative effects of home connectivity on educational outcomes (Vigdor & Ladd, 2010). In an early study of high-speed Internet expansion, Vigdor and Ladd (2010) examine zip-code-level variation in broadband availability in North Carolina between 2000 and 2005 and report small negative effects on math and reading scores among middle-school students, with larger negative effects for low-SES students. Using a similar identification strategy based on provider expansion into new geographic markets, Dettling et al. (2018) find positive effects of residential broadband expansion on PSAT and SAT performance. Their national study covered the 1999-2007 time period and focuses on high school students, finding larger positive impacts on higher income students and those with more educated families.

While the former studies center on availability, several studies in the UK context focus on broadband speed. Faber et al. (2015) examine the 2002–2008 period for students aged 7, 10, 14, and 16, while Sanchis-Guarner et al. (2025) study 14- and 16-year-old students between 2005 and 2008. Both exploit variation in broadband speeds in small (~15 buildings) geographic areas generated by idiosyncrasies internet service deployment boundaries. Faber et al. (2015) find precisely estimated null effects of faster broadband on national exam scores across ages and socioeconomic groups. In contrast, Sanchis-Guarner et al. (2025) report positive impacts of higher broadband speeds at age 14 that persist through age 16, although these gains are concentrated among higher-achieving and higher-SES students.

Post 2010s period

Studies covering more recent years, when social media, video streaming, and online gaming has become widespread, similarly report mixed findings. Henriksen et al (2022) examine the expansion of fiber broadband in Brazil from 2008 to 2013 and find that faster internet reduced student proficiency rates, while increasing drop out and grade retention. The negative effects are more pronounced among students in grades 1 through 5 than among those in grades 6 through 9. In a rigorous randomized experiment that provided laptops and home connectivity to low-income students in Peru, Malamud et al. (2018) find no measurable effects on reading or math achievement. Notably, time diaries and activity logs reveal extensive use of the internet for entertainment and communication alongside educational activities.

In contrast, other studies report educational benefits from the expansion of broadband access. Campbell (2024) examines the rollout of fiber broadband in North Carolina between 2011 and 2018. Linking fiber deployment data, which increased internet speeds, to administrative student records for students in grades 3 through 8, the study finds modest but positive effects on student achievement. The positive impacts on student achievement were driven primarily by positive income effects resulting from fiber (Campbell, 2024). Using county-level achievement data and a novel measure of broadband adoption, Caldarulo et al. (2023) find that higher rates of residential adoption are associated with improved math and reading achievement among students in grades 3 through 8, with the largest gains among Black, Hispanic, and low-income students.

Two studies explore the impact of staggered rollout of fiber in Italy. Although fiber primarily improves internet quality and speed, both Cambini et al. (2021) and Boeri (2026) evaluate the effects of fiber availability rather than speed itself, since traditional broadband services remained available in most areas. Cambini et al. (2021) follow students in grades 2, 5 and 8 between 2012 and 2017, finding negative effects on math and Italian achievement among eighth-grade students but no significant effects for second- or fifth-grade students. These negative effects are concentrated among students from less educated families. Examining the later period from 2016 to 2022, Boeri (2026) finds largely null effects for students in grades 5 and 8, with positive effects limited to economically advantaged students. While the analysis period in Boeri (2026) extends into the pandemic, the results are not disaggregated by pre- and post- pandemic, making it impossible to assess whether the effects of fiber availability differed during remote learning.

Pandemic era

Only one study of which we are aware examines the educational effects of expanded home connectivity beyond the period of pandemic-induced remote instruction. Schachner et al. (2025) examines the educational effects of Chicago Connected, a large-scale broadband expansion initiative launched by Chicago Public Schools during the pandemic to provide free high-speed internet access to low-income students. Using quasi-experimental variation in program eligibility and participation among students in grades 5-8, the authors report substantial heterogeneity driven by prior achievement: reduced engagement and achievement among lower-performing students, a pattern the authors characterize as skill-technology complementarity. Notably, these heterogeneous effects persisted beyond the period of remote instruction.

Our Contribution

Taken together, the existing literature points to uncertain and potentially heterogeneous effects of residential broadband on academic achievement. Importantly, however, all prior studies of which we are aware examine the expansion of home connectivity either before the pandemic or during periods of pandemic-induced remote learning. The scarcity of evidence from the post-pandemic period motivates a closer examination of the educational effects of home connectivity, particularly in light of the pandemic-induced shifts in online habits among teens and the expanded role of digital technologies in teaching and learning (Pew Research, 2025). Moreover, prior studies, regardless of the period during which they were conducted, tend to rely on single-state (e.g., Campbell, 2024; Vigdor and Ladd, 2010) or single-district cases (e.g., Schachner et al., 2025), limiting the studies' external validity. Other studies are conducted outside of the United States in countries with markedly different education systems (e.g., Henriksen et al., 2022; Malamud et al., 2018), while national studies from the UK focus on internet speed rather than adoption (e.g, Sanchis-Guarner et al., 2025; Faber et al., 2015). The study most closely related to ours is Caldarulo et al. (2023). However, we employ a substantially more granular unit of analysis, examine a different time period, and focus specifically on the effects of expanded home connectivity among economically disadvantaged students.

As described below, our analysis focuses on student achievement beginning in the 2021-22 school year, by which time 99% of U.S. school districts had resumed in-person instruction (American Enterprise Institute, 2021). There is scant evidence on the post-pandemic impacts of broadband connectivity. Further, our examination of a national subsidy program strengthens its external validity. Finally, the disaggregation of outcomes by subject and grade level allows us to identify heterogeneous effects that may be obscured in studies relying on aggregate achievement measures such as GPA or SAT scores. Grade disaggregation is particularly important given theories of time displacement that imply an age gradient. As students age, declining parental supervision and greater autonomy over internet use may increase the likelihood that expanded broadband access displaces time spent on academically productive activities. It follows that students in higher grades may exhibit larger negative educational effects of expanded broadband access than students in lower grades.

Data

Predictor variables: ACP uptake

Early district-led initiatives to expand broadband access folded with the launch of the federal Emergency Broadband Benefit (EBB) in early 2021, which was replaced by the larger Affordable Connectivity Program (ACP) in January 2022. Both programs offered a monthly broadband subsidy and a one-time connected-device subsidy, with eligibility based on household income thresholds or participation in qualifying safety net programs such as SNAP, Medicaid and the National School Lunch Program (NSLP). Whereas the EBB offered a monthly broadband subsidy of up to $50/month to households with incomes at or below 135% of the federal poverty line, the ACP broadened eligibility to households with incomes up to 200% of the poverty line, albeit with a reduced subsidy of $30/month.

An important feature of both the EBB and ACP is that eligibility was not contingent on lacking an existing home internet connection. According to an FCC survey conducted in late 2023, approximately 22% of beneficiaries had no internet access at home prior to receiving the subsidy, while an additional 25% relied exclusively on a mobile data plan (FCC, 2024). Thus, for nearly half of beneficiaries, the programs facilitated the transition from either no home internet access or mobile-only access to a home broadband connection. For the other half, who already had home broadband, the subsidy could instead support faster, more reliable, or higher-capacity service. ACP uptake therefore captures both the extensive margin of home broadband adoption and the intensive margin of broadband quality, although our data do not allow us to separately identify effects operating through these two margins. Our main predictor is a district level estimate of uptake in these programs (henceforth ACP for convenience). Program uptake is the monthly average number of households enrolled in either program divided by the number of eligible households in the district. We compute uptake across three time periods, with each period corresponding to the 12 months before May of a given year. For 2022 and 2023, this spans from May to April. We choose to average across this time span based on the overlap with the academic year immediately before the typical May school testing window (in the case of 2024, we average until February 2024 when ACP enrollment ended, even though enrolled households receive the subsidy through May). Table 1 displays descriptive statistics for ACP uptake over the study period. Adoption grew steadily, with average uptake nearly tripling between 2022 and 2024.

ACP household enrollment counts are reported by the Universal Service Administrative Company (which administered both subsidy programs) at the zip code/month level. We estimate the number of ACP-eligible households in each school district using ACS 5-year estimates.

Specifically, for each ZCTA, we divide the number of individuals living in households with incomes below 200% of the federal poverty line by the average household size, and then aggregate these estimates to the district level.

Zip code estimates of ACP enrollment are assigned to a single ZCTA using a geographic crosswalk provided by the Health Resources and Services Administration (Health Resources and Services Administration, 2026). We then conducted a spatial merge to generate district-level estimates of program enrollees. This spatial merge entails assigning each ZCTA to a school district's boundaries. In cases where ZCTAs are split across multiple district boundaries, we apply population-based weights (based on the 2020 U.S. Census) to estimate what portion of the ZCTA-based enrollment estimate should be assigned to each district (note this approach assumes a uniform geographical distribution of program enrollees across each ZCTA). The geographic crosswalks between ZCTA and school district boundaries, and the corresponding population weights, are drawn from the Missouri Census Data Center Geographic correspondence tool (Missouri Census Data Center, 2022).

Ultimately, the uptake rate is calculated as the number of enrolled households over the number of eligible households in each district, which is given by:

$$\mathrm{UptakeRate}_d = \frac{\sum_z \mathrm{Enrolled}_z \cdot \mathrm{weight}_{zd}}{\sum_z \mathrm{EligibleHH}_z \cdot \mathrm{weight}_{zd}}, \qquad \text{where} \quad \mathrm{EligibleHH}_z = \frac{\mathrm{EligibleIndiv}_z}{\mathrm{HHSize}_z} \tag{1}$$

where z indexes ZCTAs overlapping district d, weight_{zd} is the population-based spatial weight, $\mathrm{EligibleIndiv}_z$ is the number of individuals under 200% of the federal poverty line, and HHSize_z is the average household size. In a small number of districts (94 districts, approximately 1% of the analytic sample), uptake rate estimates exceed 1 (see Appendix A). Districts between 1 and 1.1 are top-censored to 1, while thse over 1.1 are dropped from the analytic sample affecting 94 district-years in total. We log-transform ACP uptake in the regression models because uptake rates are right-skewed across districts, with many concentrated at low levels and a long upper tail, making a log transformation appropriate for satisfying the linearity assumptions of two-way fixed effects models. In addition, the effect of expanded home connectivity is more plausibly proportional than additive (in other words, moving from 5% to 10% ACP uptake represents the same relative gain in home connectivity as moving from 20% to 40%, and the log scale captures this elasticity).

[Figure 1 here]

These programs operated from spring 2021 through spring 2024, with national enrollment rising steadily to a peak of more than 23M households in early 2024. Figure 1 and Table 1 document uptake trends across our analytic sample. Average district-level uptake nearly doubled from 16% in 2022 to 33% in 2023, then rose further to 47% in 2024. Although the pace of absolute growth slowed between the two periods (14 pp vs. 17 pp), uptake continued to increase through the end of our analytic period. Cross-sectional variation was substantial throughout: in 2023, districts at the 25th percentile averaged 21% uptake compared to 42% at the 75th percentile (a twofold difference) and this spread widened further by 2024 (31% vs. 60%), as Figure 1 illustrates. Together, these patterns reflect a program that expanded rapidly but unevenly across districts, driven by differences in demographic, economic, and policy conditions (for a discussion see Horrigan et al., 2024), and these are the variations we exploit for identification in our regression models.

Outcome variables: SEDA Achievement Data

Student achievement data is drawn from the 2025.1 release of the Stanford Educational Data Project (SEDA), which provides district-level estimates of math and RLA achievement by grade level (3rd through 8th) based on the performance of students during three post-pandemic semesters: spring of 2022, 2023, and 2024. District estimates are based on the counts of students who perform in each level of proficiency on state assessments. However, since state assessments and their proficiency categories are not directly comparable, data are normed and linked using the National Assessment of Educational Progress. Our main outcome of interest is: cohort standardized achievement, where each unit is equivalent to 1 standard deviation (SD) referenced to a common, average cohort. This common cohort is the average of the cohort of students in 4th grade in 2009, 2011, 2013, and 2015. The overall mean of the cohort-standardized achievement scale is near 0, and scores can be interpreted as effect sizes relative to this cohort. That is, an estimate of 1 indicates a district scored 1 SD above the average national reference. We refer the interested reader to additional documentation on how this achievement measure, the CS scale, is constructed (Fahle et al., 2026).

We restrict our analytic panel to districts with at least two points of achievement data for a given grade (either RLA or math) for three consecutive school years (2021-22, 2022-23, and 2023-24). We link all district-years to covariate data in the SEDA dataset, which includes district characteristics and demographic composition. Our final panel includes 104,775 district-grade-year observations across 7,825 districts over three school years. Sample characteristics are displayed in Table 2 which also compares districts included in the analytic sample with those excluded due to missing data. Notably, districts in the sample are larger on average, which is a likely result of minimum sample size requirements in the creation of the SEDA achievement data for the economically disadvantaged and non-disadvantaged subgroups.

Our main outcome of interest is the RLA and math achievement of economically disadvantaged (ECD) students. Although states differ somewhat in their definitions of economically disadvantaged (ECD) status, eligibility for free or reduced-price lunch remains one of the most common indicators used to identify ECD status. Since participation in the National School Lunch Program also qualified households for ACP benefits, ECD students represent a subset of ACP-eligible households. Further, because eligibility for free or reduced-price lunch is generally limited to households with incomes below 185% of the federal poverty line, and ACP eligibility extended to households with incomes up to 200% of the poverty line, ECD students would have been eligible for ACP benefits on an income basis alone.

To help contextualize our results, Figure 2 presents long-run achievement trends for ECD and non-ECD students from 2009 to 2024. Two distinct trends stand out. First, a large and persistent gap between ECD and NEC students throughout the period in line with prior research (Reardon, 2011) with non-ECD students averaging roughly 0.6-0.7 SD higher achievement. Second, achievement among both groups peaked around 2015-2017 and had been declining for several years before the pandemic. There are however noticeable drops between 2019 and the next available data in 2022 across both subjects and economic groups, consistent with well-documented pandemic learning loss (Betthäuser et al., 2023). The recovery that started in 2023 has since plateaued, while math has recovered more quickly than RLA regardless of economic status.

[Figure 2 here]

Methods

Our main models estimate how expanded broadband affects student outcomes by examining the relationship between within-district changes in the average ACP uptake rate and reading and math test scores among ECD students. As noted above, the outcome variables are annual measures of district-grade level reading language arts (RLA) and math scores estimates drawn from the SEDA dataset (Y_{igt}), and our key predictor is the proportion of eligible households enrolled in ACP in each district during each of the 12 months preceding the spring testing period (X_{it}). We also include a vector of standardized controls (C_{it}) with time-varying district characteristics that could confound the effect of ACP enrollment on student outcomes, including the total district student enrollment (logged) and district-level rates of the following demographic groups: free or reduced-price lunch (FRL) students, Black, Hispanic, Asian, and Native American. These time-varying controls correspond to the district demographics during the school year of the test scores outcomes. We also include year, grade and district fixed effects (α_t, α_g, and α_i, respectively). This two-way fixed effects model specification adjusts for time-invariant differences between school districts in our analytic sample.

The model can be written as follows:

$$Y_{igt} = \beta_0 + \beta_1 X_{it} + \gamma C_{it} + \alpha_i + \alpha_g + \alpha_t + \epsilon_{it} \qquad (2)$$

where β_1 is the main parameter of interest, capturing the estimated effect of a one-log unit increase in ACP enrollment during the school year preceding a given district's reading/math test scores among ECD students. All regressions are weighted by the inverse of the variance of the SEDA test scores estimate as recommended by the data source.

As a falsification test, we re-estimate the models using reading and math achievement among non-ECD students as the outcome of interest. This group serves as a comparison population that is largely ineligible for ACP benefits, although some contamination is possible. Specifically, a subset of non-ECD students may reside in households with incomes between 185% and 200% of the federal poverty line, making them ineligible for the National School Lunch Program but eligible for the ACP. In addition, some non-ECD students may qualify for ACP benefits through another household member's participation in a qualifying assistance program. Despite these potential sources of contamination, non-ECD students provide a reasonable approximation of a population largely unaffected by the program and therefore offer a useful placebo test of our identification strategy.

Results

We first examine results for RLA and math among ECD students, the subpopulation of students eligible for ACP benefits (RQ1). To facilitate interpretation, we evaluate the effect of doubling the ACP uptake rate for households within a given school district. Although a doubling would be an unusually large shift for a mature federal program such as SNAP or Medicaid, it is characteristic for a newly launched program and consistent with the uptake trends documented in Figure 1. In our analytic sample, 57% of districts experienced a doubling in uptake year-to-year at least once.

Tables 3 and 4 present results for three models: a naive model without fixed effects or controls, a model with district, year and grade fixed effects but no controls (M2), and our preferred full model that includes both fixed effects and demographic controls (M3). The results suggest a negative association between ACP uptake and student achievement in both math and RLA, which persists after including district, year and grade fixed effects and time-varying covariates. In our preferred model, a doubling of program uptake in a district is associated with a 0.02 SD decrease in math and a 0.05 SD decrease in reading/language arts (RLA) achievement among ECD students.

To contextualize these effects, we estimate the mean achievement gap between economically disadvantaged and non-economically disadvantaged students in our analytic sample, which is about 0.60 SD in both RLA and math. In this context, the observed effect is equivalent to approximately 3.3% and 8.3% of the average achievement gap between ECD and non-ECD students, respectively. If evaluated in interquartile shifts, we find that moving from the

25th to the 75th percentile of program uptake (from 17% to 45% of eligible households enrolled) is associated with a 0.03 SD decrease in math achievement and a 0.07 SD decrease in reading achievement among ECD students, equivalent to about 5.0% and 11.7% of the average achievement gap between ECD and non-ECD students, respectively.

The larger effects observed for reading than for math are consistent with a substantial body of research suggesting that reading literacy is more strongly influenced by out-of-school experiences, including the home literacy environment, parental engagement, and other family factors, whereas math literacy is more closely tied to classroom instruction and in-school learning (Borman and Dowling, 2006; Waldfogel, 2012). Because expanded home connectivity through ACP participation affects students' experiences outside of school, its educational impact would be expected to manifest more readily in reading than in math achievement.

Placebo Test

As a placebo test, we re-estimate the models described above, using reading and math achievement among non-ECD students as the dependent variables. Because these students are drawn from households that are largely ineligible for ACP benefits, the program-induced expansion in home connectivity should have little or no effect on their academic performance. The results in tables 5 and 6 confirm this expectation. Although the naive model and the model without covariates suggest a small negative effect, the saturated model with fixed effects and covariates indicates the coefficients are indistinguishable from zero. This finding supports our interpretation of the main models for ECD students.

Heterogeneity by Grade Level

Next, we examine heterogeneity in the effects of expanded home connectivity across grade levels (RQ2). Consistent with our hypothesis that time displacement and declining parental supervision are important mechanisms underlying the observed results, we expect the effects to become more pronounced in higher grades, reflecting both increased technology use and greater autonomy in students' online activities. To test this hypothesis, we add an interaction term between ACP uptake and grade to equation 2. This captures any grade-specific effect, relative to the baseline effect for grade 3.

As shown in Table 7, the negative association between program uptake and math achievement increases monotonically with grade level, providing support for our hypothesis about underlying mechanisms. The results for RLA are qualitatively similar (Table 8), although the grade-level gradient is less pronounced. This suggests that while the overall effects of expanded home connectivity are significantly larger for reading than for math, there is less cross-grade variation in impact magnitude. Figure 3 illustrates this by plotting the estimated total effects for math and RLA across grades. The figure also reports grade-specific estimates for non-ECD students, which are largely null and thus provide additional support for the falsification test results discussed above.

[Figure 3 here]

Robustness Test: Instrumental Variable Estimation

Our main specification is a two-way fixed effects (TWFE) model which absorbs unobserved variation across school districts (district-level fixed effects) and over time (year fixed effects). However, a concern remains that unobserved factors may simultaneously drive ACP uptake and student outcomes, biasing TWFE estimates. To address this endogeneity threat, we estimate an instrumental variable (IV) specification on a subset of states for which data are available. The specification uses SNAP uptake rates for households with children and households without children. First-stage results in Table 11 confirm that both are strong predictors of ACP uptake (satisfying the relevance condition). SNAP uptake should increase ACP uptake as one of the primary paths to verifying ACP eligibility and through local capacity to support enrollment. A potential threat to the exclusion restriction is the direct link between food assistance and student outcomes documented in prior research (Gassman-Pines & Bellows, 2018). This concern is relevant for the baseline instrument: students in counties where families with school-age children access SNAP at higher rates can be expected to experience less food insecurity, which as shown in prior research affects student achievement directly. Our robustness instrument addresses this concern: among childless households, variations in SNAP uptake have no plausible pathway to student test scores through food security or any other direct household-to-school channel. For further details on the IV specification see Appendix B.

The 2SLS baseline estimates imply that a 1 pp increase in ACP uptake is associated with a -0.0073 SD decrease in reading achievement and a -0.0062 SD decrease in math. The robustness instrument (SNAP uptake among families without children) yields qualitatively similar estimates though somewhat attenuated in magnitude (-0.0047 SD for RLA and -0.0048 SD for math). To put these results in context, the average ACP uptake rate for districts in the IV sample is approximately 41%, and the interquartile range spans about 29pp. Using the 2SLS coefficients from Table 14 (robustness instruments), a shift of that magnitude would be associated with a decrease of approximately 0.14 SD in each subject, which corresponds to almost one-fourth of the achievement gap between economically disadvantaged and non-disadvantaged students in the IV sample. Taken together, the IV estimates suggest that the ACP-induced expansion in home connectivity, at the margin captured by SNAP uptake variations, may have modestly reduced short-run academic achievement among ECD students in the 2023–24 school year.

Discussion

Our results challenge long-held beliefs about the potential educational benefits of closing gaps in access to high-quality broadband internet at home; they raise concerns that expanded access may actually hinder learning recovery when not complemented with initiatives that promote education-oriented use and limit time displacement. Although further research is needed to understand the mechanisms underlying these findings, we point to evidence suggesting a number of relevant mechanisms.

The first and most plausible mechanism is the displacement of homework and other educational activities by non-educational uses of digital technologies, including video gaming, streaming, and social media. A growing body of evidence suggests that the pandemic-induced increase in youth's digital media use has persisted well beyond the return to in-person schooling. Using nationally representative surveys of U.S. teenagers, Pew Research Center reports that the share of teens who say they are online almost constantly has roughly doubled from 24% in 2015 to 46% in 2022, a figure that remained virtually unchanged in the 2024 study update (Faverio & Sidoti, 2024). This increase may be driven, in part, by expanded access to high-quality broadband internet connections at home via ACP and other programs.

To explore this in more detail, we use microdata from the American Time Use Survey (ATUS), a nationally representative survey administered by the U.S. Bureau of Labor Statistics, to examine changes in time allocated to leisure computer use and homework among teens before and after the pandemic, disaggregated by household poverty status. Prior to 2020, teens from households below 185% of the federal poverty line spent approximately 0.4 fewer hours per week on leisure computer activities than their higher-income peers, plausibly driven in part by more limited access to technology in lower-income households. By 2022-2023, however, low-income teens spent almost 0.7 more hours per week on leisure computer use than their higher-income peers. Concurrently, homework time declined for both groups but more sharply among low-income teens. Although these are descriptive patterns, they are consistent with a time-displacement mechanism in which expanded home connectivity increases engagement in non-educational online activities at the expense of homework and other learning-related uses of time. This interpretation is particularly plausible for older students, who generally experience greater autonomy in their use of digital technologies and lower levels of parental supervision. Parents approval of digital activities including smartphone ownership, video games, and social media increase with age (Auxier et al., 2020) as their constraints on their time spent online and supervision of their online activities decline (Anderson, 2016).

Given the end of ACP and remote instruction, universal internet for students has dropped on the list of national policy priorities. Districts and states will be responsible for determining to what extent they will want to fill the gap in internet access and how. The negative association between achievement and internet expansion alongside other research showing negative effects suggests that districts may want to be strategic in providing supports to encourage academically enhancing internet use. In doing so, they may close pathways for negative impacts while maintaining open educationally improving technology. When districts are the internet provider, access to harmful or unproductive areas can be limited, such as restricting access to potentially distracting leisure activities such as streaming or gaming. Districts should rigorously examine how and when students are using digital resources they provide as well as partner with parents to encourage safe and productive internet use.

Limitations & Potential Extension

While we made efforts to construct ACP uptake as a sound predictor that captures greater district-level prevalence of subsidized home broadband, it is not without limitations. The spatial merge from zip code to district as well as the approximation of the number of eligible households are both subject to error from population change over time and measurement error. As we have alluded to previously, our falsification test is limited by the presence of ACP-eligible students among non-ECD students. Our main analyses are also impacted by some misalignment between eligibility as not all ECD students were eligible for the EBB program which spanned half of the first year in our analytic sample. Additionally, while our study aims to rigorously examine the association between ACP uptake and student achievement, we cannot rule out the possibility of unobserved time-varying confounders that may bias our estimates. Finally, the broadband subsidy captures both the extensive (i.e., new residential broadband) and intensive (i.e., higher-speed broadband) margin, but data limitations prevent us from disentangling the two.

Future research, potentially leveraging research partnerships, could examine usage logs or reported usage to more closely examine time displacement and parental supervision mechanisms. Connecting with parents through surveys could also provide greater insights into the degree of parental regulation of internet use at home. Additionally, research with more granular data on household-level ACP enrollment and/or student-level achievement could shed light on the specific pathways through which increased home connectivity may have affected student achievement without the limitations of aggregate data. Specifically, being able to identify individual treated households over time alongside a students' achievement would allow for within-student identification of the impacts of home broadband. Further research with more granular data would also allow greater insights into potentially heterogenous effects along dimensions previously captured in the literature such as prior achievement (Schachner et al., 2025).

References

Anderson, M., & Perrin, A. (2018). Nearly one-in-five teens can't always finish their homework because of the digital divide. https://www.pewresearch.org/short-reads/2018/10/26/nearly-one-in-five-teens-cant-always-finish-their-homework-because-of-the-digital-divide/

Anderson, M. (2016, January 7). *How parents monitor their teen's digital behavior*. In *Parents, teens and digital monitoring*. Pew Research Center. https://www.pewresearch.org/internet/2016/01/07/how-parents-monitor-their-teens-digital-behavior/

American Enterprise Institute (2021). *2020–21 instructional status.* Return to Learn Tracker. https://www.returntolearntracker.net/instructional_status/ [Accessed: 2026-03-11].

Auxier, B., Anderson, M., Perrin, A., & Turner, E. (2020, July 28). *Parenting children in the age of screens*. Pew Research Center. https://www.pewresearch.org/internet/wp-content/uploads/sites/9/2020/07/PI_2020.07.28_kids-and-screens_FINAL.pdf

Betthäuser, B. A., Bach-Mortensen, A. M., & Engzell, P. (2023). A systematic review and meta-analysis of the evidence on learning during the COVID-19 pandemic. *Nature Human Behaviour, 7*, 375–385. https://doi.org/10.1038/s41562-022-01506-4

Boeri, F. (2026). High-speed broadband and educational achievements. *Journal of Regional Science*, *66*(2), 445-483.

Borman, G. D., & Dowling, N. M. (2006). Longitudinal achievement effects of multiyear summer school: Evidence from the Teach Baltimore randomized field trial. *Educational Evaluation and Policy Analysis*, *28*(1), 25-48.

Burke, M. (2020, March). Thousands of Los Angeles high school students are not accessing online learning during school closures. https://edsource.org/2020/thousands-of-los-angeles-high-school-students-are-not-accessing-online-learning-during-school-closures/627448

Caldarulo, M., Mossberger, K., & Howell, A. (2023). Community-wide broadband adoption and student academic achievement. *Telecommunications Policy, 47* (1), 102445.

Cambini, C., Sabatino, L., & Zaccagni, S. (2021). The faster the better? The effect of ultra-fast broadband on students' performance. https://doi.org/http://dx.doi.org/10.2139/ssrn.3925004

Campbell, R. C. (2024). Need for speed: Fiber and student achievement. *Telecommunications Policy, 48* (6), 102767. https://doi.org/10.1016/j.telpol.2024.102767

Dettling, L. J., Goodman, S., & Smith, J. (2018). Every little bit counts: The impact of high-speed internet on the transition to college. *Review of Economics and Statistics, 100* (2), 260–273.

Engzell, P., Frey, A., & Verhagen, M. D. (2021). Learning loss due to school closures during the covid-19 pandemic. *Proceedings of the National Academy of Sciences, 118* (17), e2022376118. https://doi.org/10.1073/pnas.2022376118

Faber, B., Sanchis-Guarner, R., & Weinhardt, F. (2015). ICT and education: Evidence from student home addresses (No. w21306). National Bureau of Economic Research.

Fahle, E. M., Saliba, J., Reardon, S. F., Shear, B. R., Kalogrides, D., & Ho, A. D. (2026). *Stanford Education Data Archive: Technical Documentation (Version 6.0)* (tech. rep.). https://purl.stanford.edu/xh833nn4025

Faverio, M., & Sidoti, O. (2024, December 12). *Teens, social media and technology 2024*. Pew

Research Center. https://www.pewresearch.org/internet/2024/12/12/teens-social-media-and-technology-2024/

Federal Communications Commission. (2024, February). *ACP consumer survey*. https://www.fcc.gov/acp-survey

Gassman-Pines, A., & Bellows, L. (2018). Food instability and academic achievement: A quasi-experiment using SNAP benefit timing. American Educational Research Journal, 55(5), 897-927. DOI: 10.3102/0002831218761337

Goldstein, D. (2026, June 18). *Student cheating is becoming impossible to detect in an A.I. era - The New York Times*. New York Times. https://www.nytimes.com/2026/06/18/us/ai-apps-students-cheat.html

Health Resources and Services Administration. (2026). Zip code to zcta crosswalk [Accessed: 2026-03-12; downloadable dataset available on HRSA GeoCare Navigator Resources page (ZIP Code to ZCTA Crosswalk).].

Henriksen, A. L., Zoghbi, A. C., Tannuri-Pianto, M., & Terra, R. (2022). Education outcomes of broadband expansion in Brazilian municipalities. *Information Economics and Policy*, *60*, 100983.

Horrigan, J. B., Whitacre, B. E., & Galperin, H. (2024). Understanding uptake in demand-side broadband subsidy programs: The affordable connectivity program case. *Telecommunications Policy, 48* (8), 102812.

Imbens, G. W., & Angrist, J. D. (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, *62*(2), 467–475. https://doi.org/10.2307/2951620

Kleibergen, F., & Paap, R. (2006). Generalized reduced rank tests using the singular value decomposition. *Journal of Econometrics*, *133*(1), 97–126.

Klein, A. (2021, April). During COVID-19, Schools Have Made a Mad Dash to 1-to-1 Computing. What Happens Next? https://www.edweek.org/technology/during-covid-19-schools-have-made-a-mad-dash-to-1-to-1-computing-what-happens-next/2021/04

Lewis, K., & Kuhfeld, M. (2024). Recovery still elusive: 2023-24 student achievement highlights persistent achievement gaps and a long road ahead. brief. *NWEA*.

Malamud, O., Cueto, S., Cristia, J. P., & Beuermann, D. (2018). Do children benefit from internet access? Experimental evidence from a developing country. https://doi.org/10.18235/0001392

Missouri Census Data Center. (2022). Geocorr 2022: Geographic correspondence engine [Accessed: 2026-03-12; Version 1.8; the Geocorr application generates custom geographic correlation lists based on 2020 Census geography.].

National Center for Education Statistics (2020, November). *Table 702.12.* Digest of Education Statistics. https://nces.ed.gov/programs/digest/d21/tables/dt21_702.12.asp

National Center for Education Statistics. (2024). *Public school district file, 2023–24* [Data set]. U.S. Department of Education. https://nces.ed.gov/programs/edge/Geographic/SchoolLocations

Pew Research Center. (2025). Teens and internet, device access fact sheet [Published July 10, 2025; Accessed: 2026-03-12].

Prothero, A. (2022, September). Fewer Districts Are Providing Home Internet Access, But Students Still Need It. https://www.edweek.org/technology/fewer-districts-are-providing-home-internet-access-but-students-still-need-it/2022/09

Reardon, S. F. (2011). The widening academic achievement gap between the rich and the poor: New evidence and possible explanations. In G. J. Duncan & R. J. Murnane (Eds.), *Whither Opportunity? Rising Inequality, Schools, and Children's Life Chances* (pp. 91–116). Russell Sage Foundation.

Sanchis-Guarner, R., Montalbán, J., & Weinhardt, F. (2025). Home broadband and human capital formation. *Economics of Education Review, 108*, 102679. https://doi.org/https://doi.org/10.1016/j.econedurev.2025.102679

Schachner, J. N., Marwell, N. P., de la Torre, M., Gwynne, J. A., & Allensworth, E. (2025). Heterogeneous effects of closing the digital divide during covid-19 on student engagement and achievement. edworkingpaper no. 25-1153. Annenberg Institute for School Reform at Brown University.

Stock, J. H., & Yogo, M. (2002). Testing for Weak Instruments in Linear IV Regression. NBER Technical Working Paper No. 284 October 2002.

Teräs, M., Teräs, H., Arinto, P., Brunton, J., Daryono, D., & Subramaniam, T. (2020). Covid-19 and the push to online learning: Reflections from 5 countries.

Universal Service Administrative Company. (2024, February 8). *Additional ACP data*. https://www.usac.org/about/affordable-connectivity-program/acp-enrollment-and-claims-tracker/additional-acp-data/

Vigdor, J. L., & Ladd, H. F. (2010). Scaling the digital divide: Home computer technology and student achievement (tech. rep. No. Working Paper 48). National Center for Analysis of Longitudinal Data in Education Research.

Waldfogel, J. (2012). The role of out-of-school factors in the literacy problem. *Future Child, 22* (2), 39–54. https://doi.org/10.1353/foc.2012.0016

Wyckoff, J. (2025). Puzzling over declining academic achievement. edworkingpaper: 25-1197. [Retrieved from Annenberg Institute for School Reform at Brown

University]. https://doi.org/10.26300/kjaa-ec47

Tables

Table 1. ACP Uptake Rate by Year

	Mean	SD	Min	P25	Med	P75	Max
2022							
Uptake Rate	16.1	8.8	0.7	9.8	14.4	20.6	100.0
Uptake Rate (log)	2.6	0.6	-0.4	2.3	2.7	3.0	4.6
2023							
Uptake Rate	32.7	16.1	1.6	20.5	29.9	42.1	100.0
Uptake Rate (log)	3.4	0.5	0.4	3.0	3.4	3.7	4.6
2024							
Uptake Rate	47.0	20.9	1.8	31.0	44.0	60.4	100.0
Uptake Rate (log)	3.7	0.5	0.6	3.4	3.8	4.1	4.6

Table 2. Sample Characteristics: Analytic Sample vs. Excluded Observations

	Excluded			Included		
	Mean	SD	N	Mean	SD	N
%FRL	0.427	0.243	7853	0.526	0.200	22483
%Black	0.077	0.196	7853	0.086	0.158	22483
%Hispanic	0.123	0.168	7853	0.200	0.235	22483
%Asian	0.018	0.054	7853	0.026	0.062	22483
%Native American	0.021	0.094	7853	0.020	0.082	22483
RLA score (ECD)	-0.588	0.527	490	-0.413	0.340	19161
Math score (ECD)	-0.656	0.533	425	-0.410	0.388	18265
Total Enrollment	561.689	1823.748	7854	2291.478	6241.430	22483

Table 3. Math: Economically Disadvantaged

	Naive	M1	M2
ACP uptake (logged)	-0.072***	-0.040***	-0.031***
	(0.005)	(0.008)	(0.008)
Constant	-0.206***	-0.315***	-0.080
	(0.017)	(0.026)	(0.046)
N	97236.000	97236.000	97233.000
adj R2	0.018	0.748	0.748
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Grade FE	No	Yes	Yes
Demographics	No	No	Yes

Inverse variance weights. SE clustered at district. Demographics standardized.
Source: SEDA 2025.1.
* p<0.05, ** p<0.01, *** p<0.001

Table 4. RLA: Economically Disadvantaged

	Naive	M1	M2
ACP uptake (logged)	-0.068***	-0.075***	-0.070***
	(0.005)	(0.008)	(0.008)
Constant	-0.222***	-0.197***	-0.077
	(0.016)	(0.026)	(0.046)
N	104778	104778	104775
adj R2	0.022	0.742	0.742
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Grade FE	No	Yes	Yes
Demographics	No	No	Yes

Inverse variance weights. SE clustered at district. Demographics standardized.
Source: SEDA 2025.1.
* p<0.05, ** p<0.01, *** p<0.001

Table 5. Math: non-Economically Disadvantaged

	Naive	M1	M2
ACP uptake (logged)	-0.039***	-0.018*	0.002
	(0.005)	(0.008)	(0.008)
Constant	0.403***	0.335***	0.475***
	(0.017)	(0.027)	(0.051)
N	91867	91867	91867
adj R2	0.005	0.801	0.803
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Grade FE	No	Yes	Yes
Demographics	No	No	Yes

Inverse variance weights. SE clustered at district. Demographics standardized.
Source: SEDA 2025.1.
* p<0.05, ** p<0.01, *** p<0.001

Table 6. RLA: non-Economically Disadvantaged

	Naive	M1	M2
ACP uptake (logged)	-0.046***	-0.031***	-0.016
	(0.005)	(0.009)	(0.009)
Constant	0.399***	0.351***	0.427***
	(0.016)	(0.030)	(0.050)
N	98871	98871	98871
adj R2	0.009	0.796	0.798
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Grade FE	No	Yes	Yes
Demographics	No	No	Yes

Inverse variance weights. SE clustered at district. Demographics standardized.
Source: SEDA 2025.1.
* p<0.05, ** p<0.01, *** p<0.001

Table 7. Math: ECD - Grade interactions

	Naive	M1	M2
ACP uptake (logged)	-0.040***	-0.008	0.002
	(0.006)	(0.008)	(0.008)
Grade 4 # ACP uptake (logged)	-0.015***	-0.013***	-0.013***
	(0.003)	(0.003)	(0.003)
Grade 5 # ACP uptake (logged)	-0.020***	-0.024***	-0.024***
	(0.004)	(0.003)	(0.003)
Grade 6 # ACP uptake (logged)	-0.042***	-0.047***	-0.047***
	(0.005)	(0.004)	(0.004)
Grade 7 # ACP uptake (logged)	-0.053***	-0.061***	-0.061***
	(0.006)	(0.004)	(0.004)
Grade 8 # ACP uptake (logged)	-0.069***	-0.064***	-0.065***
	(0.007)	(0.005)	(0.005)
Grade 4	-0.011	-0.007	-0.007
	(0.010)	(0.009)	(0.009)
Grade 5	-0.022	-0.007	-0.007
	(0.013)	(0.012)	(0.012)
Grade 6	0.011	0.026	0.027
	(0.016)	(0.015)	(0.015)
Grade 7	0.005	0.071***	0.072***
	(0.019)	(0.015)	(0.015)
Grade 8	0.081***	0.089***	0.091***
	(0.023)	(0.017)	(0.017)
Constant	-0.213***	-0.334***	-0.088
	(0.020)	(0.027)	(0.046)
N	97236	97236	97233
adj R2	0.047	0.749	0.750
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Demographics	No	No	Yes

Reference: Grade 3. Interactions = deviation from Grade 3.
SE clustered at district. Source: SEDA 2025.1.
* p<0.05, ** p<0.01, *** p<0.001

Table 8. RLA: ECD - Grade interactions

	Naive	M1	M2
ACP uptake (logged)	-0.062***	-0.054***	-0.049***
	(0.006)	(0.008)	(0.008)
Grade 4 # ACP uptake (logged)	-0.008***	-0.010***	-0.010***
	(0.002)	(0.002)	(0.002)
Grade 5 # ACP uptake (logged)	0.000	-0.013***	-0.013***
	(0.004)	(0.003)	(0.003)
Grade 6 # ACP uptake (logged)	-0.011**	-0.026***	-0.026***
	(0.004)	(0.004)	(0.004)
Grade 7 # ACP uptake (logged)	-0.008	-0.029***	-0.029***
	(0.005)	(0.004)	(0.004)
Grade 8 # ACP uptake (logged)	-0.011*	-0.029***	-0.029***
	(0.005)	(0.005)	(0.005)
Grade 4	-0.016	-0.004	-0.004
	(0.009)	(0.008)	(0.008)
Grade 5	-0.089***	-0.013	-0.013
	(0.013)	(0.010)	(0.010)
Grade 6	-0.076***	0.001	0.001
	(0.015)	(0.013)	(0.013)
Grade 7	-0.104***	0.000	0.000
	(0.017)	(0.014)	(0.014)
Grade 8	-0.089***	0.004	0.004
	(0.018)	(0.016)	(0.016)
Constant	-0.150***	-0.202***	-0.082
	(0.020)	(0.028)	(0.047)
N	104778	104778	104775
adj R2	0.044	0.742	0.743
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Demographics	No	No	Yes

Reference: Grade 3. Interactions = deviation from Grade 3.
SE clustered at district. Source: SEDA 2025.1.
* p<0.05, ** p<0.01, *** p<0.001

Table 9. Math: NEC - Grade interactions

	Naive	M1	M2
ACP uptake (logged)	-0.023***	0.000	0.020*
	(0.006)	(0.009)	(0.008)
Grade 4 # ACP uptake (logged)	-0.009**	-0.008**	-0.008**
	(0.003)	(0.003)	(0.003)
Grade 5 # ACP uptake (logged)	-0.014***	-0.016***	-0.016***
	(0.004)	(0.003)	(0.003)
Grade 6 # ACP uptake (logged)	-0.020***	-0.023***	-0.023***
	(0.005)	(0.004)	(0.004)
Grade 7 # ACP uptake (logged)	-0.026***	-0.027***	-0.027***
	(0.005)	(0.004)	(0.004)
Grade 8 # ACP uptake (logged)	-0.033***	-0.028***	-0.029***
	(0.006)	(0.005)	(0.005)
Grade 4	-0.010	-0.019*	-0.019*
	(0.009)	(0.009)	(0.009)
Grade 5	-0.031*	-0.039***	-0.039***
	(0.012)	(0.011)	(0.011)
Grade 6	-0.044**	-0.055***	-0.055***
	(0.015)	(0.014)	(0.014)
Grade 7	-0.074***	-0.072***	-0.070***
	(0.017)	(0.013)	(0.013)
Grade 8	-0.060**	-0.094***	-0.091***
	(0.020)	(0.015)	(0.015)
Constant	0.442***	0.379***	0.523***
	(0.018)	(0.028)	(0.051)
N	91867	91867	91867
adj R2	0.026	0.801	0.803
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Demographics	No	No	Yes

Reference: Grade 3. Interactions = deviation from Grade 3.
SE clustered at district. Source: SEDA 2025.1.
* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Table 10. RLA: NEC - Grade interactions

	Naive	M1	M2
ACP uptake (logged)	-0.050***	-0.030**	-0.014
	(0.005)	(0.010)	(0.009)
Grade 4 # ACP uptake (logged)	-0.003	-0.002	-0.002
	(0.003)	(0.002)	(0.002)
Grade 5 # ACP uptake (logged)	0.003	-0.000	-0.000
	(0.003)	(0.003)	(0.003)
Grade 6 # ACP uptake (logged)	0.001	-0.004	-0.004
	(0.004)	(0.003)	(0.003)
Grade 7 # ACP uptake (logged)	0.003	-0.004	-0.004
	(0.005)	(0.004)	(0.004)
Grade 8 # ACP uptake (logged)	0.010*	0.001	0.001
	(0.005)	(0.004)	(0.004)
Grade 4	-0.003	-0.006	-0.006
	(0.008)	(0.008)	(0.008)
Grade 5	-0.048***	-0.034***	-0.033***
	(0.011)	(0.009)	(0.009)
Grade 6	-0.062***	-0.050***	-0.049***
	(0.013)	(0.011)	(0.011)
Grade 7	-0.102***	-0.068***	-0.066***
	(0.015)	(0.013)	(0.013)
Grade 8	-0.154***	-0.105***	-0.102***
	(0.016)	(0.014)	(0.014)
Constant	0.467***	0.398***	0.472***
	(0.018)	(0.032)	(0.051)
N	98871.000	98871.000	98871.000
adj R2	0.023	0.796	0.798
District FE	No	Yes	Yes
Year FE	No	Yes	Yes
Demographics	No	No	Yes

Reference: Grade 3. Interactions = deviation from Grade 3.
SE clustered at district. Source: SEDA 2025.1.
* p<0.05, ** p<0.01, *** p<0.001

Table 11. First-Stage Regression Results

	RLA Sample		Math Sample	
	(1)	(2)	(3)	(4)
	Baseline IV	Robustness IV	Baseline IV	Robustness IV
SNAP access (school-age HHs)	0.335***		0.325***	
	(0.069)		(0.068)	
SNAP access (no-children HHs)		0.460***		0.473***
		(0.082)		(0.080)
Observations	7,649	10,148	7,356	9,763
R-squared	0.285	0.315	0.289	0.327
KP-F statistic	23.74	31.44	22.68	34.81
Partial R2 (instrument)	0.048	0.066	0.046	0.070
Covariates	Yes	Yes	Yes	Yes
Grade FE	Yes	Yes	Yes	Yes

Dependent var.: ACP uptake rate (percent). KP-F = Kleibergen-Paap F.
Stock-Yogo 10% critical value = 16.38.
County-clustered SEs; grades 3-8 with grade FE, year 2024.
Source: SEDA 2025.1. * p<0.05, ** p<0.01, *** p<0.001

Table 12, Panel A. RLA Achievement

	Baseline IV		Robustness IV	
	(1) OLS	(2) RF	(3) 2SLS	(4) RF	(5) 2SLS
ACP uptake	0.002***		-0.007**		-0.005*
	(0.000)		(0.003)		(0.002)
SNAP access (school-age HHs)		-0.002**			
		(0.001)			
SNAP access (no-children HHs)				-0.002*	
				(0.001)	
Observations	31,097	7,649	7,649	10,148	10,148
KP-F statistic			23.74		31.44
Partial R2 (instrument)			0.0481		0.0661
Bootstrap p (ACP uptake)			0.005		0.019
Covariates	Yes	Yes	Yes	Yes	Yes
Grade FE	Yes	Yes	Yes	Yes	Yes

Table 12, Panel B. Math Achievement

		Baseline IV		Robustness IV	
	(1) OLS	(2) RF	(3) 2SLS	(4) RF	(5) 2SLS
ACP uptake	0.001*		-0.006*		-0.005*
	(0.000)		(0.002)		(0.002)
SNAP access (school-age HHs)		-0.002**			
		(0.001)			
SNAP access (no-children HHs)				-0.002**	
				(0.001)	
Observations	29,107	7,356	7,356	9,763	9,763
KP-F statistic			22.68		34.81
Partial R2 (instrument)			0.0461		0.0698
Bootstrap p (ACP uptake)			0.000		0.012
Covariates	Yes	Yes	Yes	Yes	Yes
Grade FE	Yes	Yes	Yes	Yes	Yes

RF = reduced form; 2SLS instruments the ACP uptake rate (percent) with county SNAP access rates.
County-clustered SEs in parentheses; 2SLS SEs from wild cluster bootstrap (999 reps).
Covariates: log enrollment, %FRL, race shares; grades 3-8, year 2024.
Source: SEDA 2025.1. * p<0.05, ** p<0.01, *** p<0.001

Figures

Figure 1

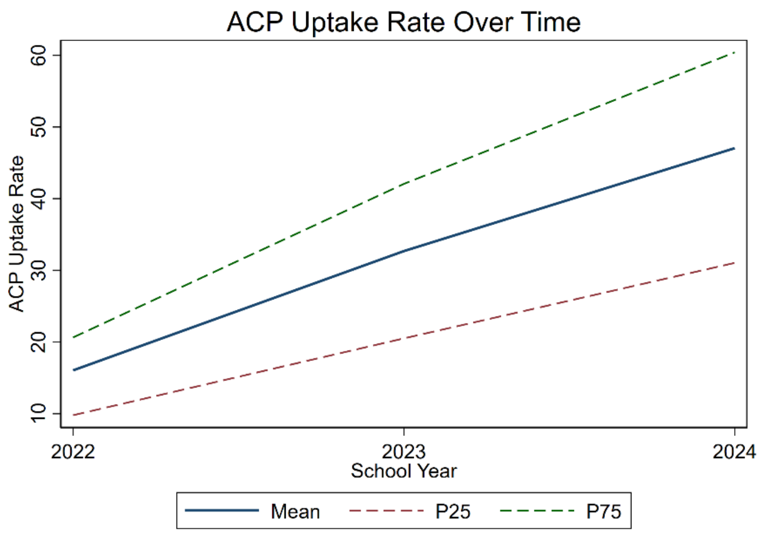

Figure 2

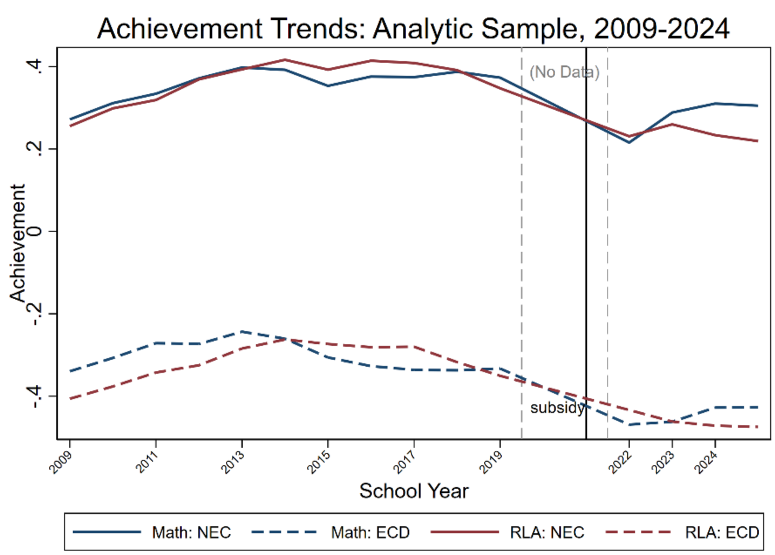

Figure 3

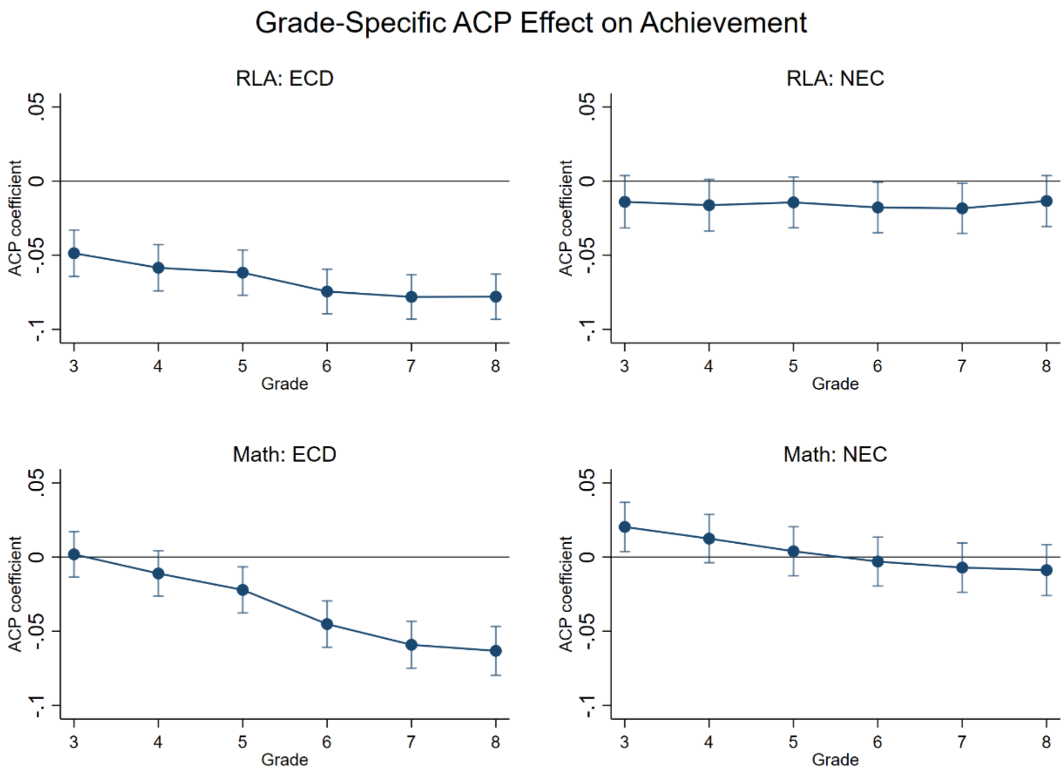
Grade-Specific ACP Effect on Achievement
RLA: ECD
RLA: NEC
Math: ECD
Math: NEC
ACP coefficient
Grade
.05
0
-.05
-.1
3
4
5
6
7
8

Figure Captions

Figure 1: District-level ACP uptake 2022-2024

Figure 2: Achievement trends collapse across grades using inverse-variance weights. Source: SEDA 2025.1

Figure 3: Grade-specific ACP coefficients by Economic Status from columns M2. Top panel shows reading/language arts and the bottom panel shows math. Right column shows placebo sample.

Appendix A

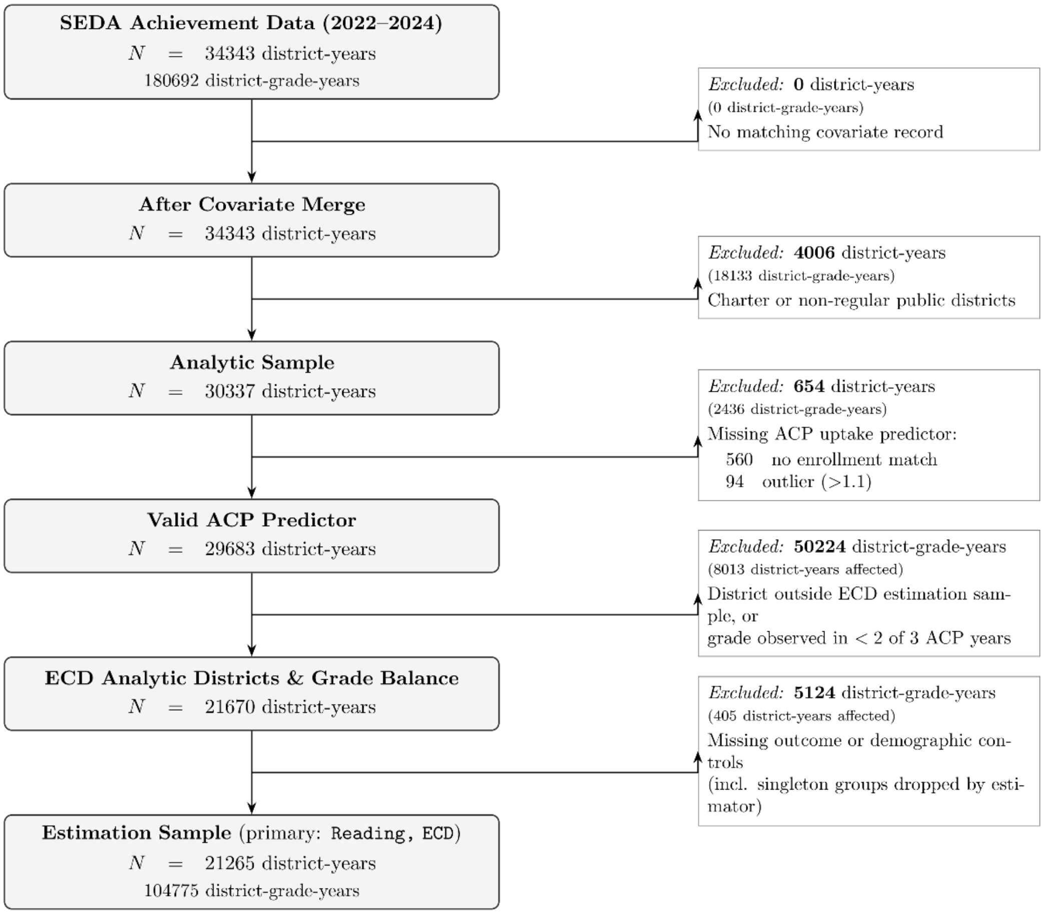

Figure A1: Sample construction for the regressions in model M2 for RLA outcome for economically disadvantaged students.

Appendix B

Our main specification is a two-way fixed effects (TWFE) model which absorbs unobserved variation across school districts (district-level fixed effects) and over time (year fixed effects). However, a concern remains that unobserved factors may simultaneously drive ACP uptake and student outcomes, biasing TWFE estimates. To address this endogeneity threat, we estimate an instrumental variable (IV) specification on a subset of states for which data are available. The specification uses SNAP uptake rates for two subpopulations as instruments. First-stage results confirm that both are strong predictors of ACP uptake (satisfying the relevance condition), while the mechanisms described below support the exclusion restriction (which requires that instrument variation affects student achievement only through its effect on ACP uptake).

We use county-level SNAP uptake rate (defined as the share of eligible households participating in SNAP) for two distinct subpopulations as our instruments. Two related mechanisms connect SNAP participation to ACP uptake. First, SNAP enrollment was one of the primary pathways to verify ACP eligibility, accounting for approximately 40% of all applications processed through USAC's National Verifier (USAC, 2024). Households already enrolled in SNAP therefore faced a simpler administrative path to ACP enrollment. Second, the local infrastructure built to support SNAP enrollment (including county offices, staff, and outreach programs) likely facilitates enrollment in other assistance programs, including the newly launched ACP subsidy. Both mechanisms imply that school districts in counties with higher SNAP uptake rates should also have higher ACP uptake, independent of the district's underlying level of economic disadvantage.

The two instruments are: (1) the county SNAP uptake rate among families with school-age children (baseline instrument) and (2) the county SNAP uptake rate among childless households (robustness instrument). Both instruments capture variation in local SNAP administrative capacity rather than the absolute scale of disadvantage, since they measure the share of eligible households participating rather than raw participant counts. These instruments and ACP uptake enter our models in levels (rather than logged as in the TWFE specification) because the relationship between SNAP and ACP uptake is additive such that absolute differences are of greatest interest. District-level demographic controls further absorb any residual correlation between SNAP uptake and underlying poverty or food insecurity.

A potential threat to the exclusion restriction is the direct link between food assistance and student outcomes documented in prior research (Gassman-Pines & Bellows, 2018). This concern is relevant for the baseline instrument: students in counties where families with school-age children access SNAP at higher rates can be expected to experience less food insecurity, which as shown in prior research affects student achievement directly. Our robustness instrument addresses this concern: among childless households, variations in SNAP uptake have no plausible pathway to student test scores through food security or any other direct household-to-school channel. In other words, any effect of this instrument on student achievement must operate through ACP uptake.

The availability of SNAP uptake estimates at the county level is noticeably limited (most estimates are at the state level, which offers limited variation for reliable inference). We take advantage of a special Census Bureau program in partnership with USDA (which administers SNAP) and state SNAP offices that uses American Community Survey (ACS) data linked to state administrative records to estimate SNAP uptake rates at the county level (for participating states only).[1] Since very few states have data for more than one year, we create a cross-sectional dataset at the county level using the most recently available data for each county. To match county-level SNAP uptake with student outcomes at the district level, we crosswalk using the NCES EDGE Geocode Local Education Agency (LEA) file for 2023–24, which provides the administrative county of record for every public school district in the U.S. Because coverage is limited to a subset of states which participate in the Census Bureau program, this sample is significantly smaller than the main sample in TWFE estimates. All models are weighted by the inverse of the squared standard error of each district's achievement estimate, and standard errors are clustered at the county level.

The first stage estimates the relationship between the instrument (*Z*) and ACP uptake (*X*), conditional on district covariates (*C*). This can be written as:

$$X_d = \pi_0 + \pi_1 Z_d + \pi_2 C_d + \epsilon_d \tag{3}$$

where X_d is the ACP uptake rate in district *d*, Z_d indexes the SNAP uptake rate in district *d* (following the county to school district crosswalk), C_d is vector of district-level covariates, and ϵ_d are the first-stage error terms.

Table 11 reports first-stage results for both instruments in the RLA and math samples.[2] The regression coefficients are significant at $p<0.01$, confirming that both instruments are strong predictors of ACP uptake. Kleibergen-Paap cluster-robust F-statistics range from 22.7 to 23.7 for the baseline instrument and from 31.4 to 34.8 for the robustness instrument, all well above the Stock-Yogo 10% maximal IV size critical value of 16.38 (Stock and Yogo, 2002; Kleibergen and

[1] The data and estimation methodology are available at www.census.gov/library/visualizations/interactive/snap-eligibility-access.html. States with pre-pandemic SNAP uptake data only are excluded from the analysis. The final sample includes counties in the following states: Arizona, Idaho, Indiana, Iowa, Kentucky, Maryland, Massachusetts, North Carolina, North Dakota, Nevada, New Jersey, South Carolina, South Dakota, Tennessee, Wisconsin, and Wyoming.

[2] Sample sizes between Math and RLA differ slightly because of data availability in achievement data

Paap, 2006). The bootstrap Wu-Hausman test rejects exogeneity of ACP uptake in all four specifications, confirming OLS is inconsistent and IV estimation is warranted.

Next, the 2SLS (two-stage least squares) equation replaces the endogenous regressor ACP uptake rate with its fitted values from the first stage. This can be written as:

$$Y_d = \beta_0 + \beta_1 \hat{X}_d + \beta_2 C_d + \epsilon_d \tag{4}$$

where Y_d is the student outcome of interest in district d, $\widehat{X_d}$ is the predicted ACP uptake rate in district d from first stage, C_d is vector of district-level covariates, and ϵ_d is the error term. The coefficient of interest is β_1 which approximates the Local Average Treatment Effect (LATE) of ACP uptake on student outcomes (Imbens and Angrist, 1994). Table 12 presents the results of the 2SLS equation, alongside the naïve OLS (for reference purposes only) and the reduced form (RF) model, which regresses student outcomes on the SNAP uptake instruments directly (with similar district-level covariates).

The 2SLS baseline estimates imply that a 1 pp increase in ACP uptake is associated with a -0.0073 SD decrease in reading achievement and a -0.0062 SD decrease in math. The robustness instrument (SNAP uptake among families without children) yields qualitatively similar estimates though somewhat attenuated in magnitude (-0.0047 SD for RLA and -0.0048 SD for math). To put these results in context, the average ACP uptake rate for districts in the IV sample is approximately 41%, and the interquartile range spans about 29pp. Using the 2SLS coefficients from Tables 12 and 13 (robustness instruments), a shift of that magnitude would be associated with a decrease of approximately 0.14 SD in each subject, which corresponds to almost one-fourth of the achievement gap between economically disadvantaged and non-disadvantaged students in the IV sample.

The stability of 2SLS estimates across instruments provides evidence in support of the exclusion restriction. The baseline instrument operates through SNAP-enrolled households with school-age children, while the robustness instrument relies on childless households, a population with no direct path to school district outcomes. The reduced-form coefficients are consistent in sign and similar in magnitude across instruments, implying that the negative achievement effect is not an artifact of the particular instrument used or of food-security spillovers from SNAP enrollment. Taken together, the IV estimates suggest that the ACP-induced expansion in home connectivity, at the margin captured by SNAP uptake variations, may have modestly reduced short-run academic achievement among ECD students in the 2023-24 school year.