ECONOMIC RESEARCH
FEDERAL RESERVE BANK OF ST. LOUIS

WORKING PAPER SERIES

Prior Knowledge, Module Design, and Student Dropout in Online K-12 Education

Authors	Manu Garcia, and Diego Mendez-Carbajo
Working Paper Number	2026-008A
Creation Date	April 2026
Citable Link	https://doi.org/10.20955/wp.2026.008
Suggested Citation	Garcia, M., Mendez-Carbajo, D., 2026; Prior Knowledge, Module Design, and Student Dropout in Online K-12 Education, Federal Reserve Bank of St. Louis Working Paper 2026-008. URL https://doi.org/10.20955/wp.2026.008

Federal Reserve Bank of St. Louis, Research Division, P.O. Box 442, St. Louis, MO 63166

The views expressed in this paper are those of the author(s) and do not necessarily reflect the views of the Federal Reserve System, the Board of Governors, or the regional Federal Reserve Banks. Federal Reserve Bank of St. Louis Working Papers are preliminary materials circulated to stimulate discussion and critical comment.

Prior Knowledge, Module Design, and Student Dropout in Online K-12 Education*

Manu García† Diego Méndez-Carbajo‡

April 30, 2026

Abstract

We examine student dropout in online K-12 education coursework using administrative data for 442,000 students, 64 economics and personal finance modules, and 2.1 million module assignments between 2014 and 2025. We find that module length, prior knowledge, embedded formative assessments, and school district demographics independently predict whether students complete assigned modules. Each additional page is associated with a 0.24-percentage-point decrease in completion probability, but this relationship is 30 percent weaker for students with above-median prior knowledge. Embedded knowledge checks amplify the negative effect of module length: the page effect more than doubles in modules containing these assessments. Dropout is elevated 33 percent above expected on pages immediately before knowledge checks. Districts with higher minority enrollment exhibit lower completion even after accounting for per-pupil expenditure and staffing. Survival analysis reveals that dropout risk is highest in the first 10 percent of module progress and generally declines thereafter, suggesting that early engagement is critical. Apparent differences between personal finance and economics modules disappear within schools, indicating institutional sorting rather than subject difficulty. These findings provide actionable guidance for instructional designers developing online educational content.

JEL Codes: I21, I24, A20, A21

Keywords: Online education, Student dropout, Prior knowledge, Formative assessment, K-12, Digital divide, Instructional design, Learning analytics

*We thank [acknowledgments]. The views expressed are those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of St. Louis or the Federal Reserve System.

†Federal Reserve Bank of St. Louis. Email: manu.garcia@stls.frb.org

‡Federal Reserve Bank of St. Louis. Email: diego.mendez-carbajo@stls.frb.org

Highlights

- Analysis of 2.1 million module assignments from 442,000 K-12 students over 11 years.
- Each additional page is associated with 0.24 percentage points lower completion.
- Prior knowledge buffers the negative relationship between module length and completion.
- Dropout spikes on pages immediately before embedded knowledge checks.
- Hazard of dropout is highest in first 10% of module progress, then declines.

1 Introduction

The use of online learning resources in primary and secondary schooling (K-12) in the United States is widely documented and points to the substantial role that these resources play in supporting the delivery of mandatory instruction. According to the U.S. Department of Education (National Center of Education Statistics (2022)), in the spring of 2020, 77 percent of public schools and 73 percent of private schools reported moving classes to self-paced or real-time online distance-learning formats.

Large-scale empirical studies of online learning practices frequently focus on settings where observational data is easy to collect, such as higher education (Poquet et al., 2022) and massive open online courses (MOOCs) (Formanek, Wenger, Buxner, Impey, & Sonam, 2017). In K-12 education, substantial attention has been directed to the use of virtual schooling as an alternative to in-person schooling (Barbour & Reeves, 2009). However, comparatively little attention has been given to the course-level use of online instruction resources across heterogeneous classrooms and across states. The difficulty of gathering those data is a major handicap to that line of research.

In this paper, we study the determinants and dynamics of student dropout in online K-12 economics and personal finance education. We use administrative data from Econ Lowdown, a free platform designed and operated by the Federal Reserve Bank of St. Louis, comprising 3.5 million modules assigned to 750,000 students across 64 standardized modules over an 11-year period (2014–2025).[1] Three features make these data unusual for the field. First, the scale: prior K-12 online education research typically draws on dozens or hundreds of students from a single course or school district (Barbour, 2017; Pane, Steiner, Baird, Hamilton, & Pane, 2017). Our data cover more than 5,000 schools in all 50 states, providing statistical power to detect effects that smaller studies cannot. Second, the granularity: the platform records page-level progress, so we observe not only whether students complete modules but

[1]The platform was decommissioned in mid-2025 and replaced with https://www.federalreserveeducation.org/.

exactly *where* they disengage. Third, the research design: teachers assign specific modules to students, eliminating the self-selection concerns that plague research on MOOCs (Reich & Ruipérez-Valiente, 2019). The majority of students complete multiple modules, enabling within-student comparisons that further control for unobserved individual differences.

We address four research questions:

RQ1. How do module length and prior knowledge impact completion in online K-12 modules?

RQ2. Does prior knowledge moderate the effect of module length on completion?

RQ3. How do embedded formative assessments (knowledge checks) relate to module completion and within-module dropout patterns?

RQ4. Do district demographics help explain completion after accounting for school resources?

The rest of this section reviews the related literature and positions our contribution within it.

1.1 K-12 Online Education

The use of online learning resources in K-12 education is expanding rapidly (Gemin, Pape, Vashaw, & Watson, 2015; Heppen et al., 2016). The features and format of these courses make them appealing to instructors, who can use technology (e.g., videos, animations, simulations) to engage students and provide immediate feedback on activities and assessments at scale. A systematic review of the literature on online teaching in that space identifies seven pillars of effective online instructional practice (Johnson, Walton, Strickler, & Elliott, 2023). Those pillars are evidence-based course design, connected learners, accessibility, supportive learning, environment, individualization and differentiation, active learning, and real-time assessment.

Teacher experience and familiarity with online instructional resources are key for their adoption (Liu, Ritzhaupt, Dawson, & Barron, 2016). So is teacher training (Woo, Archam-

bault, Borup, Buss, & Henriksen, 2025) and student access to dedicated technology such as laptop computers (Warschauer, 2004).

In that regard, the presence of a digital divide related to access and ability remains a major concern for the use and impact of online education in K-12 classrooms and beyond (Dolan, 2016; Hamlin, Adigun, & Adams, 2023; Hohlfeld, Ritzhaupt, Dawson, & Wilson, 2017; Kennedy, Rhoads, & Leu, 2016; Rauh, 2011; Xu & Jaggars, 2014). Moreover, neglecting the pillars of effective online instructional practice described above likely has negative impacts on educational opportunity gaps (Darling-Aduana, 2021) and test scores (Backes & Cowan, 2019).

As described by Zheng, Lin, and Kwon (2020), empirical studies generally examine online learning in K-12 settings in terms of student-level factors (e.g., individual behavioral data), instructor-level factors (e.g., teaching practices), and course-level factors (e.g., cohort effects and grading approaches). However, they reference Barbour (2017) and identify data sample sizes and the scarcity of details about instructional design as practical limitations to this research. For example, the small scale of a study on the online delivery of personalized learning prevents its authors from drawing robust conclusions about its positive effect on achievement (Pane et al., 2017). Also, concerns about selection into a particular type of learning modality (e.g., distance learning) can impact the interpretation of observed learning outcomes (Ahn & McEachin, 2017; Fitzpatrick, Berends, Ferrare, & Waddington, 2020; Schwerdt & Chingos, 2015).

We contribute to this literature by studying a setting that combines the rigor of teacher-assigned content with the scale and duration (over a decade) rarely available in K-12 online education research. Our data capture student-level behavior across heterogeneous classrooms and states, addressing the sample size and design-detail limitations identified in prior work.

1.2 Economics and Personal Finance Education

K-12 economics education and personal finance education maintain a complementary relationship. Developing personal finance skills is associated with higher financial well-being (Clark, Lusardi, & Mitchell, 2024; Lusardi & Mitchell, 2014), and economics is a core subject in social studies that shows the wide-ranging implications of personal spending and saving habits (Suiter, Wolla, & Levin, 2022). International evidence suggests that early financial education is important in shaping future financial behaviors, placing the locus of intervention in mandatory primary and secondary education (Kaiser, Lusardi, Menkhoff, & Urban, 2022; Oberrauch, Kaiser, & Lusardi, 2024).

At the same time, research shows that students learn economics better in a dedicated course (Buckles & Freeman, 1984; Walstad & Watts, 2015). This creates tension for the allocation of instructional time between the two subjects. In 2024, the latest data available at the time of this writing, 29 U.S. states required a course in economics to graduate and 36 states required a course in personal finance (Council for Economic Education, 2026). Several states require both, highlighting the perceived divide between the two domains. Urban, Schmeiser, Collins, and Brown (2020) argue that the effectiveness of state mandates depends critically on implementation quality, and Luedtke and Urban (2023) document heterogeneity in the format of mandated courses across states.

Research on online economic education focuses largely on undergraduate college settings (Bettinger, Fox, Loeb, & Taylor, 2017; Expósito, Sánchez-Rivas, Gómez-Calero, & Pablo-Romero, 2020; Hill & LoPalo, 2024; Ipinnaiye & Risquez, 2024) and comparatively little on K-12 (VanFossen & Herman-Ellison, 2006; Walstad & Watts, 2015). Research on online K-12 personal finance education is scarcer still, with early efforts centered on stock market simulations (Harter & Harter, 2021; Hinojosa et al., 2010) and experiential micro-economy programs (Batty, Collins, O'Rourke, & Odders-White, 2020). A review of delivery methods for youth financial literacy notes the value of online-accessible resources but identifies limited evidence on their effectiveness (Totenhagen et al., 2015).

Our data include 36 modules on economics (e.g., supply and demand, market equilibrium, unemployment, inflation) and 28 modules on personal finance (e.g., paychecks, decision-making, saving, credit). We contribute to this literature by providing the first large-scale evidence on completion dynamics in online K-12 economics and personal finance education, a setting where content is standardized, teacher assignment eliminates self-selection, and the panel structure enables within-student identification.

1.3 Dropout Behavior in Online Learning

Students who don't complete an online learning activity assigned to them are often regarded as unsuccessful, or at least partially successful, learners. We draw from two strands of literature on the topic of course non-completions and dropout behavior to inform our research.

One strand of the literature studies the reasons behind dropout decisions in K-12 online courses (Corry, Dardick, & Stella, 2016; de la Varre, Irvin, Jordan, Hannum, & Farmer, 2014). This line of research builds on the theoretical frameworks of Tinto (1975) and Bean and Metzner (1985) and emphasizes the interaction between individual student factors, course features, and school settings to determine the overall persistence of students in mandatory elementary and secondary online education.

However, it is important to draw a distinction between participation and withdrawal in virtual schooling, as an alternative to in-person schooling, and non-completing or dropping out of an individual online course. Our research is circumscribed to the behavior of individual students who, as part of a course or classroom cohort, are assigned one or several online modules by their teacher. We do not analyze the potential correlation between completing an online module and the subsequent academic performance in the overall course or eventual graduation (Hart, Friedmann, & Hill, 2018).

Another strand of the literature employs data collected from massive open online courses (MOOCs) to identify the triggers for not completing them. Although the purpose and character of MOOCs are not directly comparable to those of online K-12 education (Reich &

Ruipérez-Valiente, 2019), the asynchronous nature of their delivery has relevant implications for our own research.

Extensive reviews of MOOC dropout research identify three broad determinants: learner profiles, resource design, and instructional settings (Chen et al., 2020; Douglas, Merzdorf, Hicks, Sarfraz, & Bermel, 2020; Zhang, Gao, & Zhang, 2021). These categories map onto the student-level, course-level, and environmental factors identified in K-12 reviews (Hart, 2012; Lee & Choi, 2011; Zheng et al., 2020). With a narrower focus on resource design, the literature points to specific features captured in our data: the use of pre-tests and feedback on performance (Janelli & Lipnevich, 2021; Lipnevich, Janelli, Park, Calistro, & Esser, 2024), transitions between content units (Chen et al., 2020), and duration and use of post-tests (Xing, Liu, Kim, & Song, 2025).

The MOOC literature documents completion rates between 5 and 15 percent (Jordan, 2015; Reich & Ruipérez-Valiente, 2019), but these settings differ fundamentally from ours. MOOC students self-select into courses, face no external accountability, and may audit rather than complete. Our setting, where teachers assign specific modules to students in formal classroom settings, generates completion rates above 92 percent and allows us to focus on the factors behind the remaining 8 percent of assignments that are not completed.

1.4 Learning Analytics and Educational Data Mining

Our research connects to the growing field of learning analytics, which uses data from digital learning environments to understand and improve educational outcomes. Siemens (2013) and Joksimović et al. (2018) articulate a framework to understand fruitful engagement in open online settings built on contextual, student, and learning factors.

A central concern in this literature is the early identification of at-risk students. Jayaprakash, Moody, Lauría, Regan, and Baron (2014) show that predictive models using LMS data can identify struggling students within the first weeks of a course. Our hazard analysis contributes to this line of work by documenting the temporal pattern of disengagement. We

pinpoint when dropout risk is at its highest, identifying the timing of more effective interventions.

Educational data mining has also examined the role of formative assessment in digital environments. Pardos, Baker, San Pedro, Gowda, and Gowda (2014) find that student affect during problem-solving predicts persistence and learning. Baker, D'Mello, Rodrigo, and Graesser (2010) document that "gaming the system" (attempting to succeed through systematic guessing rather than learning) is associated with worse outcomes. We examine whether students disengage on pages containing knowledge checks or immediately before encountering those knowledge checks. The presence of assessment avoidance behavior may reflect low self-efficacy or negative affect toward evaluation.

Recent work has called for learning analytics research that moves beyond prediction to explanation and intervention (Joksimović et al., 2018; Wise, Zhao, & Hausknecht, 2019). We respond to this call by examining not just whether students complete modules but *where* they disengage and by linking dropout patterns to specific design features (module length, knowledge check placement) that instructional designers can modify.

1.5 Theoretical Framework and Hypotheses

Two theoretical traditions ground our research questions. We derive explicit hypotheses from each and summarize the conceptual model in Figure 1.

Cognitive load theory (Sweller, 1988; Sweller, Ayres, & Kalyuga, 2011) holds that learning is constrained by the limited capacity of working memory. When cognitive demands exceed this capacity, students may disengage. The theory distinguishes intrinsic load (determined by the complexity of the material and the learner's prior knowledge) from extraneous load (determined by instructional design). Three hypotheses follow directly:

> **H1 (Module length):** Longer modules are associated with lower completion rates, as cumulative cognitive demands increase the probability of disengagement.

H2 (Prior knowledge): Students with higher prior knowledge exhibit higher completion rates, as existing schemas reduce intrinsic cognitive load.

H3 (Interaction): The negative relationship between module length and completion is weaker for students with higher prior knowledge, as schemas buffer against cumulative cognitive demands.

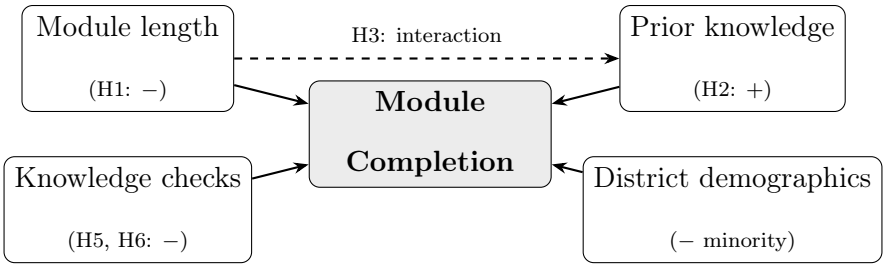
Cognitive load theory also helps explain the temporal dynamics of dropout. Because early pages establish foundational schemas that reduce the processing cost of later pages, the risk of dropout should be highest at the beginning and decline as students progress (**H4**).

Formative assessment theory provides a complementary perspective. Black and Wiliam (1998) document that well-designed formative assessments improve learning outcomes. Roediger and Butler (2011) and McDaniel, Anderson, Derbish, and Morrisette (2007) show that retrieval practice enhances long-term retention. For sure, formative assessment is generally beneficial for learning. However, we hypothesize that embedded knowledge checks may also trigger disengagement:

H5 (Knowledge checks): Modules with more embedded knowledge checks exhibit lower completion rates, as some students perceive assessments as evaluative rather than formative.

H6 (Anticipation): Dropout is elevated on pages immediately preceding knowledge checks, reflecting anticipatory disengagement among students who expect difficulty.

This hypothesis sets up a tension between the learning benefits and the persistence costs of embedded assessment, a tension our study can address.



H4: Hazard of dropout declines with module progress

Figure 1: Conceptual Model Linking Module and Student Characteristics to Completion

Notes: Arrows indicate hypothesized relationships. Signs in parentheses indicate predicted direction of association with completion. Dashed line indicates moderation effect (H3).

The paper proceeds as follows. Section 2 describes the institutional setting, data, and analytic approach. Section 3 presents results organized by research question. Section 4 discusses the implications for early engagement and instructional design. Section 5 concludes. Appendix B presents additional analyses and robustness checks.

2 Methods

2.1 Institutional Background and Resource Description

Econ Lowdown is a free online learning platform built and operated by the Federal Reserve Bank of St. Louis. It was designed to support economics and personal finance education in K-12 schools (Méndez-Carbajo & Malakar, 2020). Launched in 2010, the platform follows a teacher-mediated model: teachers register, create virtual classrooms, and assign specific learning resources to students. Students complete assigned resources asynchronously and receive immediate feedback on assessments. The platform's design is informed by principles of effective online instructional practice, including evidence-based course design, active learning, and real-time assessment (Johnson et al., 2023).

The platform offers 64 interactive modules covering economics (36 modules) and personal finance (28 modules). Each module follows a standardized structure: students complete a pre-test assessing baseline knowledge, progress through content pages combining text, graph-

ics, and multimedia, and finish with a post-test. Module length varies from 2 to 67 pages. Estimated completion times range from 15 to 120 minutes. According to the taxonomy of educational digital resources produced by Xie, Di Tosto, Chen, and Vongkulluksn (2018), the Econ Lowdown modules can be considered "individualized instruction" resources, suitable for self-paced, independent learning that can also support social learning activities.

Fifty of the 64 modules (78 percent) contain embedded knowledge checks (KCs), formative questions interspersed throughout content pages. Students must answer to proceed, but responses are ungraded and invisible to teachers. These assessments are intended as formative tools to promote engagement and retrieval practice (Roediger & Butler, 2011). The page-level structure of modules allows us to observe exactly where students disengage.

2.2 Data

Our data comprise administrative records from Econ Lowdown covering 2014 through early 2025. Although the platform launched in 2010, module-level completion reports with pre-test timestamps are available beginning in 2014. The platform hosts various resource types; we focus on 64 interactive modules for which we observe page-level completion.

The raw dataset contains 3,519,520 student-module observations from 749,554 unique students. Our main analysis focuses on public school students, defined as K-12 students with valid NCES identifiers, yielding 2,094,074 observations from 442,246 students. An additional 24,559 observations from private schools (identified via NCES private school identifiers) are analyzed separately. The remaining observations serve as a comparison sample for robustness.

The outcome measured is module completion, coded as 0/100 so that regression coefficients represent percentage-point effects. Overall completion is 92.5 percent. The platform records a module as complete when the student submits the post-test; students need not visit every single content page before answering the post-test. Only 0.9 percent of students do so.

For each module, we observe total pages and subject classification (economics or personal finance). Module length varies from 2 to 67 pages (mean 15; median 9). We construct variables from the module metadata: knowledge check indicators (presence, count, density, and position of embedded formative assessments within each module) from the page-level curriculum data.

Students complete a pre-test before each module, scored on a 0-100 scale (mean 55; standard deviation 29). We also observe pages completed for each student-module pair, which allows us to calculate the progress fraction at which non-completers disengage.

We link public school observations to NCES Common Core of Data at the district level, matching 85 percent to district records for minority enrollment share, per-pupil expenditure, and staffing ratios.

Table 1 presents descriptive statistics for the public school sample.

Table 1: Summary Statistics: Public School Sample

Variable	N	Mean	SD	Min	Max
Completed	2,094,074	93.00	26.00	0	100.00
Module Pages	2,094,074	14.86	15.14	2	67.00
Pre-test Score	2,089,865	54.50	29.37	0	100.00
Post-test Score	1,935,493	79.29	25.22	0	100.00
Personal Finance Module	2,094,074	0.58	0.49	0	1.00
Knowledge Checks in Module	2,094,074	3.79	4.93	0	21.00
KC Density (KCs/Page)	2,094,074	0.19	0.13	0	0.46

Notes: Public school sample includes K-12 students with valid NCES identifiers. Module length varies from 2 to 67 pages across 64 modules. Knowledge check variables are measured at the module level.

Table 2 describes the 1,938 public school districts in our sample. Observations come from districts with an average minority enrollment share of 45 percent, per-pupil expenditure of

approximately $15,000, and enrollment of 27,000 students. There is substantial variation in each dimension, providing power to detect relationships between school characteristics and completion.

Table 2: District Characteristics: Public School Sample (NCES)

Variable	N	Mean	SD	Min	Max
Minority Share	1,782,555	0.45	0.27	0	1.00
Expenditure per Pupil ($)	1,782,555	14113.28	4495.68	5933	106579.00
Teacher-Student Ratio	1,782,555	0.06	0.02	0	0.53

Notes: Number of student-module observations per school district. Variables from NCES Common Core of Data, matched at the district level. 85 percent of public school observations match to NCES records.

Figure 2 shows platform usage over time. Annual module assignments peaked at 420,000 in 2014, declined to approximately 230,000 in 2015–2017, then grew steadily to over 300,000 by 2023. Completion rates generally rose over the sample period, from 88 percent in 2014 to 95 percent in 2025. The COVID-19 pandemic (2020) produced no visible disruption in either usage or completion, consistent with the platform's role as a supplement to classroom instruction rather than a replacement for it. In early 2025, the platform managers announced its replacement with another portal, shutting down enrollments at the end of the academic year.

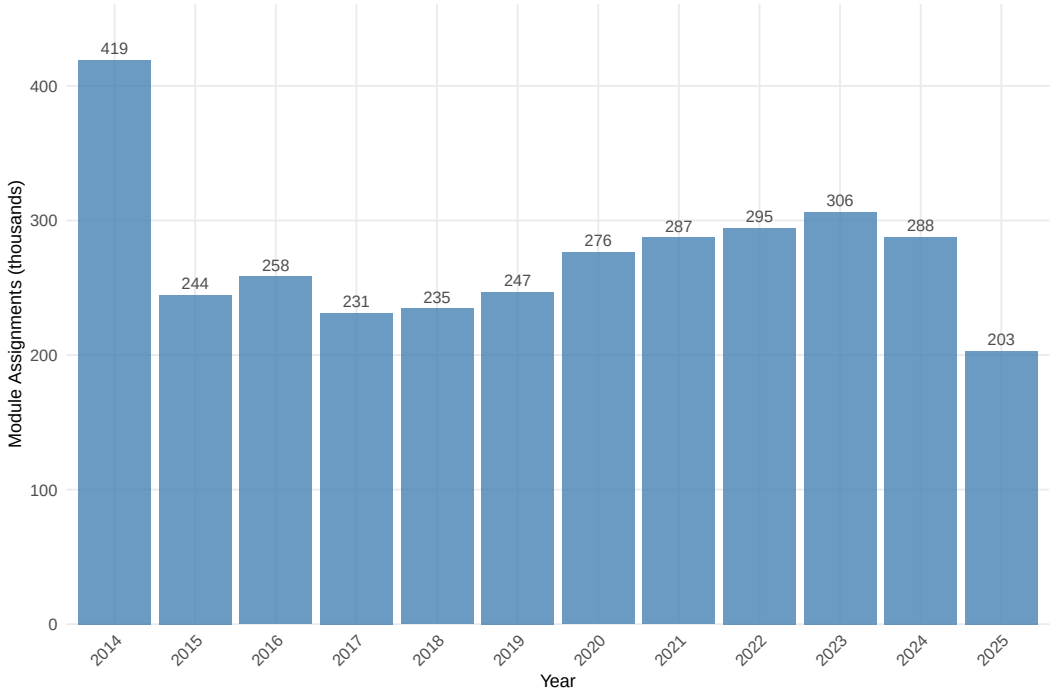


Figure 2: Platform Usage Over Time

Notes: Annual module assignments based on pre-test date. Full sample. The elevated count in 2014 reflects a higher share of non-K-12 users in early platform years; the public school sample used in our main analysis is less affected. Year 2025 is partial (through March). The drop in 2025 is due to the announcement of its replacement with https://www.federalreserveeducation.org/.

2.3 Data Analysis

The dependent variable is module completion, coded as 0/100 so that regression coefficients can be read directly as percentage-point effects. A module is recorded as complete when the student submits the post-test.

The independent variables fall into four groups. *Module characteristics* include total pages (a continuous measure of module length) and a binary indicator for personal finance versus economics. *Prior knowledge* is measured by the pre-test score (0–100) that each student completes before starting a module. For the interaction analysis, we also create a binary indicator for above-median pre-test performance. *Knowledge check variables* include the number of embedded formative assessments, their density (checks per page), and the

position of the first check within the module. *District characteristics*, available for the 85 percent of public school observations matched to NCES records, include minority enrollment share, per-pupil expenditure, and the teacher-student ratio.

We use linear regression models to predict module completion (implemented in R via the fixest package; Bergé, 2018). Although the nature of the dependent variable is binary (complete/incomplete), linear models offer straightforward interpretation: coefficients represent percentage-point changes in the probability of completion. Because students within the same school may share unobserved characteristics that affect completion, we compute robust standard errors that account for this clustering (analogous to adjusting for nested data in multilevel models).

We organize the analysis in four stages, progressively adding controls to assess the robustness of observed relationships. First, we estimate the baseline relationship between module characteristics, prior knowledge, and completion. We then add controls for unobserved heterogeneity at different levels:

- *State-level controls* account for regional differences in educational systems and student populations (e.g., curriculum standards, demographic composition).
- *School-level controls* account for all stable differences between schools (e.g., resources, teacher quality), so that comparisons occur among students within the same school.
- *Student-level controls*, feasible because 71 percent of students complete multiple modules, account for all stable student characteristics (e.g., ability, motivation, home environment). These models identify relationships from variation within the same student across different module assignments, providing the strongest test of whether module characteristics predict completion independent of student selection.

Second, we test whether prior knowledge moderates the relationship between module length and completion by including an interaction term (H3). A positive interaction coefficient would indicate that high-knowledge students are less sensitive to longer modules.

Third, we examine the role of embedded formative assessments by adding knowledge check variables (H5, H6). We estimate these models on the 50 modules (76 percent of observations) that contain knowledge checks.

Fourth, we incorporate district-level characteristics from NCES to examine whether school demographics help explain completion independent of school resources.

The main threat to internal validity in observational studies of online learning is self-selection: students who choose longer or harder modules may differ systematically from those who do not. Our setting largely eliminates this concern because teachers, not students, assign modules. The progressive fixed effects strategy addresses remaining confounding variables. School fixed effects control for all stable differences between schools (resources, student composition), so estimates reflect within-school variation. Student fixed effects, feasible because 71 percent of students complete multiple modules, go further: they control for all stable student characteristics (ability, motivation, home environment), so the page effect is identified from the same student's completion across different modules.

3 Results

3.1 Module Length and Prior Knowledge (RQ1–RQ2)

Table 3 presents the main results in five columns that progressively tighten the research design. Columns (1)-(3) estimate the baseline relationship between module characteristics, prior knowledge, and completion. Column (1) includes state fixed effects; Column (2) replaces these with school fixed effects, so comparisons occur within schools; Column (3) uses student fixed effects, identifying effects from the same student's completion across different modules. Columns (4)-(5) introduce the interaction between module length and prior knowledge to address RQ2.

Module length (H1). Each additional page is associated with a 0.24-percentage-point decrease in completion probability when controlling for state-level differences (Column 1).

This relationship is stable across all specifications, including within-student comparisons (Column 3), suggesting it is not driven by weaker students being assigned to longer modules. In practical terms, a 10-page increase in module length corresponds to a 2.4-percentage-point decrease in completion probability. To contextualize this effect size: moving from a short module (5 pages) to a long module (50 pages) is associated with approximately 11 percentage points lower completion, a substantial difference given the baseline completion rate of 92.5 percent. Figure 3 illustrates this relationship at the module level: longer modules exhibit systematically lower completion rates. These findings support H1.

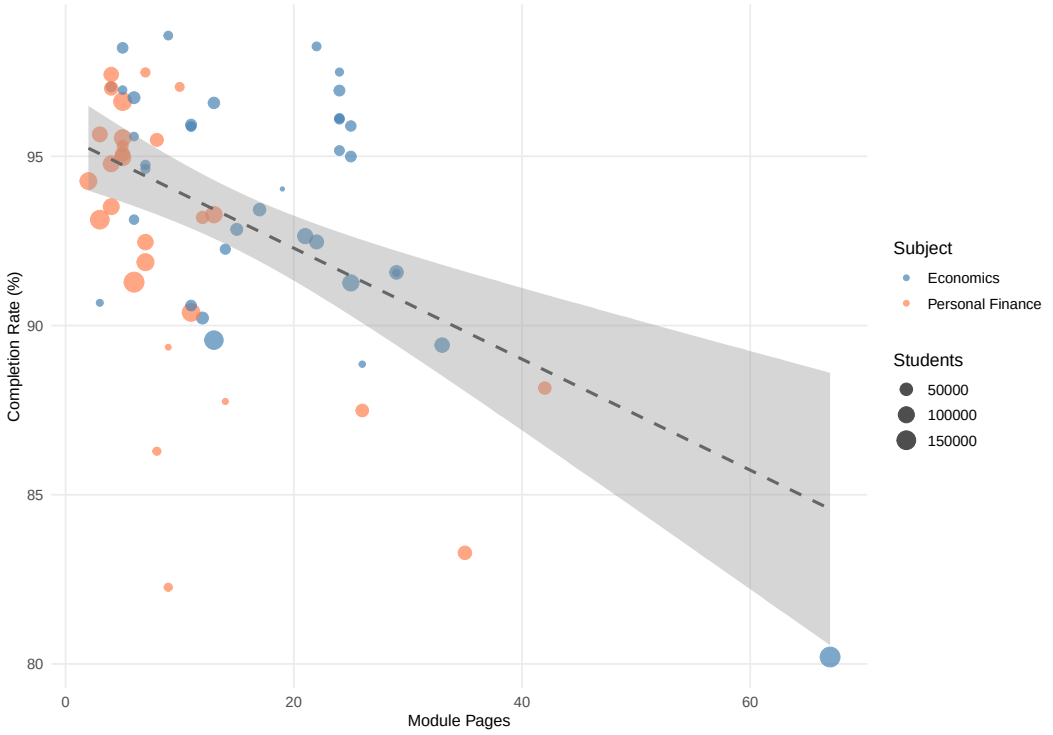
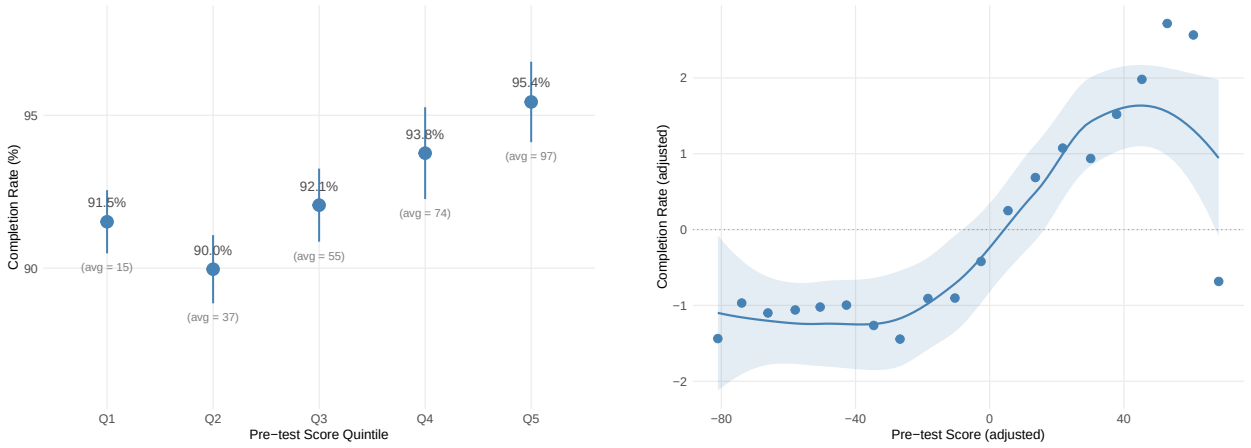


Figure 3: Module Length and Completion Rate

Notes: Each point represents one module. Point size reflects number of students. Line shows linear fit. Public school sample.

Prior knowledge (H2). Pre-test scores positively predict completion across all specifications, supporting H2. Figure 4a displays completion rates by pre-test quintile. Students in the top quintile complete 95.4 percent of modules versus 90.0 percent in the second quintile, a gap of 5.4 percentage points. This difference is practically meaningful: it represents a 54 percent reduction in the non-completion rate (from 10.0 to 4.6 percent). The relationship is

not strictly monotonic: the bottom quintile (91.6 percent) outperforms the second quintile, possibly because teachers could provide additional support to the least-prepared students or assign them shorter modules. Figure 4b confirms that the positive relationship holds after statistically accounting for module length and school differences.



(a) Completion by Pre-test Quintile (b) Adjusted for Module Length and School

Figure 4: Prior Knowledge and Completion

Notes: Panel (a): mean completion rate by pre-test quintile. Error bars show 95% confidence intervals clustered at the school level; labels show completion rate and average pre-test score per quintile. Panel (b): to isolate the relationship between prior knowledge and completion from differences in module design and school context, we statistically removed the effects of module length and school identity from both variables. Each point represents one-twentieth of the adjusted pre-test distribution. The curve is a local polynomial fit with 95% confidence band. A positive slope indicates that students with higher prior knowledge are more likely to complete, even after accounting for the module they were assigned and the school they attend. Public school sample.

Subject. The personal finance indicator is negative and significant in Columns (1)-(2) but attenuates to -0.2 and loses significance with student fixed effects (Column 3). The apparent subject difference reflects which schools adopt each subject type, not intrinsic subject difficulty.

Interaction: Prior knowledge moderates the length–completion relationship (H3). Columns (4)-(5) add an interaction between module pages and prior knowledge (operationalized as above-median pre-test performance). The interaction coefficient is positive and statistically significant, indicating that high-knowledge students are less sensitive to longer modules and

supporting H3. In Column (4), the estimated relationship between pages and completion is −0.28 percentage points per page for low-knowledge students but only −0.20 for high-knowledge students (calculated by summing the coefficients for module pages and pages × high knowledge), a 30 percent attenuation. In practical terms, for a 50-page module, this difference corresponds to 4 percentage points higher completion for high-knowledge students relative to low-knowledge students, beyond the main effect of prior knowledge. Column (5) confirms that this interaction persists when comparing students within the same school.

Figure 5 illustrates the interaction visually. Short modules show similar completion rates regardless of prior knowledge. As modules lengthen, a gap emerges: low-knowledge students' completion declines more steeply. For the longest modules, the gap exceeds 5 percentage points.

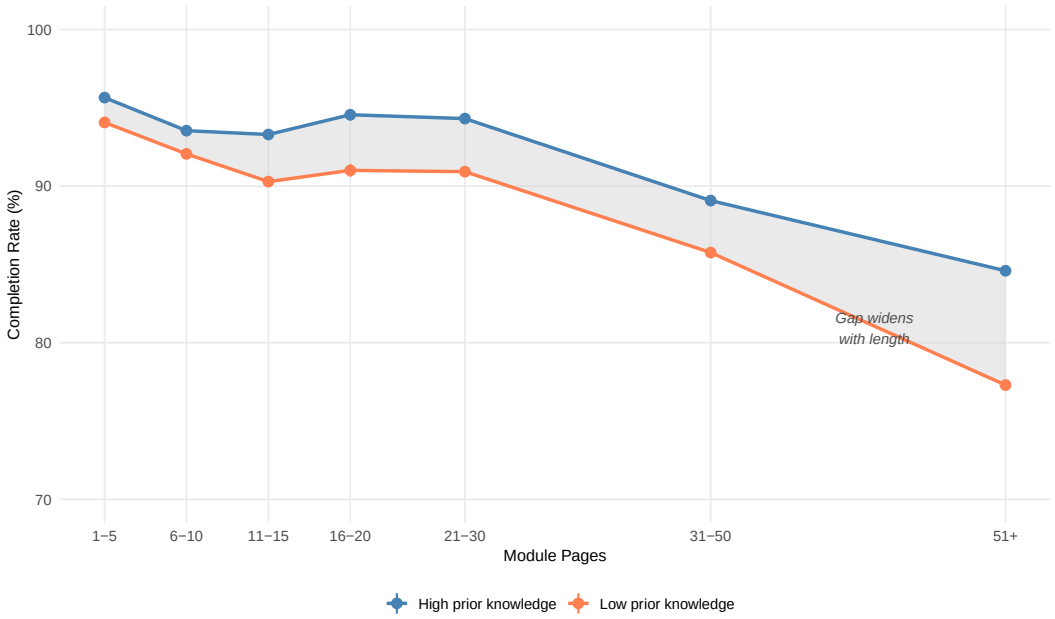


Figure 5: Completion by Module Length and Prior Knowledge

Notes: High/low prior knowledge defined as above/below median pre-test score. Error bars show 95% confidence intervals.

Table 3: Determinants of Module Completion

Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Personal Finance	-1.719***	-0.8381**	-0.2047	-1.695***	-0.8395**
	(0.4395)	(0.3437)	(0.2705)	(0.4376)	(0.3426)
Module Pages	-0.2352***	-0.2192***	-0.1812***	-0.2771***	-0.2470***
	(0.0148)	(0.0139)	(0.0153)	(0.0177)	(0.0156)
Pre-test Score	0.0446***	0.0382***	0.0274***		
	(0.0043)	(0.0033)	(0.0040)		
Private School	4.148*			4.179*	
	(2.247)			(2.266)	
High Knowledge				1.181***	1.293***
				(0.2598)	(0.1997)
Pages × High Knowledge				0.0833***	0.0544***
				(0.0115)	(0.0084)
Fixed-effects					
Student FE			Yes		
School FE		Yes			Yes
State FE	Yes			Yes	
Fit statistics					
Observations	2,114,397	2,114,397	1,986,547	2,114,397	2,114,397
R^2	0.04491	0.14298	0.44777	0.04512	0.14301
Within R^2	0.01869	0.01502	0.01532	0.01891	0.01505

Clustered (nces_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

3.2 Module Design: Knowledge Checks and Dropout (RQ3)

We now examine how embedded formative assessments relate to completion. Table 4 presents our analysis of knowledge check effects. Column (1) shows the baseline specification with module pages as the key predictor. Column (2) adds a binary indicator for whether the module contains any knowledge checks; this coefficient is small and not statistically significant, suggesting that simply having KCs does not directly affect completion.

Table 4: Knowledge Check Effects on Completion

Model:	Baseline (1)	Has KC (2)	Pages x KC (3)	School FE (4)	Student FE (5)
Variables					
Personal Finance	-1.719***	-1.678***	-1.508***	-0.7067**	-0.1705
	(0.4395)	(0.4587)	(0.4658)	(0.3429)	(0.2613)
Module Pages	-0.2352***	-0.2358***	-0.1054***	-0.1022***	-0.1169***
	(0.0148)	(0.0147)	(0.0278)	(0.0208)	(0.0217)
Pre-test Score	0.0446***	0.0447***	0.0447***	0.0382***	0.0274***
	(0.0043)	(0.0042)	(0.0042)	(0.0032)	(0.0039)
Private School	4.148*	4.153*	4.111*		
	(2.247)	(2.247)	(2.261)		
Has Knowledge Checks		0.1549	1.146***	0.7664***	0.2736
		(0.3130)	(0.4409)	(0.2574)	(0.2459)
Pages × Has KC			-0.1319***	-0.1176***	-0.0645***
			(0.0263)	(0.0219)	(0.0231)
Fixed-effects					
Student FE					Yes
School FE				Yes	
State FE	Yes	Yes	Yes		
Fit statistics					
Observations	2,114,397	2,114,397	2,114,397	2,114,397	1,986,547
R^2	0.04491	0.04492	0.04507	0.14310	0.44782
Within R^2	0.01869	0.01870	0.01886	0.01515	0.01540

Clustered (nces_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Pages × Has KC shows how the page effect differs for modules with vs without knowledge checks.

The key finding appears in Column (3), which interacts module pages with the presence of knowledge checks. The interaction reveals that knowledge checks amplify the negative

effect of module length. In modules without KCs, each additional page is associated with a 0.11-percentage-point decrease in completion. In modules with KCs, the effect more than doubles to 0.24 percentage points per page (calculated by summing the coefficients for module pages and pages x has knowledge check). The difference of 0.13 percentage points is highly significant ($t = -5.0$). Columns (4) and (5) show that this interaction persists with school and student fixed effects, indicating that the relationship is not driven by selection.

This finding deserves careful interpretation. Knowledge checks are formative by design: responses are ungraded and invisible to teachers. Yet students may not perceive them as formative. If a student encounters a question and cannot answer it, the experience may signal that the material is too difficult, prompting a decision to quit. The anticipation pattern we document below supports this interpretation: dropout is elevated on pages immediately before knowledge checks, suggesting that students disengage as they approach an assessment rather than at the assessment itself.

Figure 6 examines where dropouts occur relative to knowledge check pages, providing a direct test of H6. We classify each page in the curriculum as a KC page (containing a knowledge check), a pre-KC page (the page immediately before a knowledge check), or a regular page. If dropouts were spread evenly across all page types, each type would record dropout in proportion to its share of total pages. Instead, pre-KC pages receive 34 percent more dropout than expected, while regular pages receive 10 percent less. This pattern supports H6 and is consistent with an anticipation mechanism: some students disengage as they approach an assessment rather than at the assessment itself.

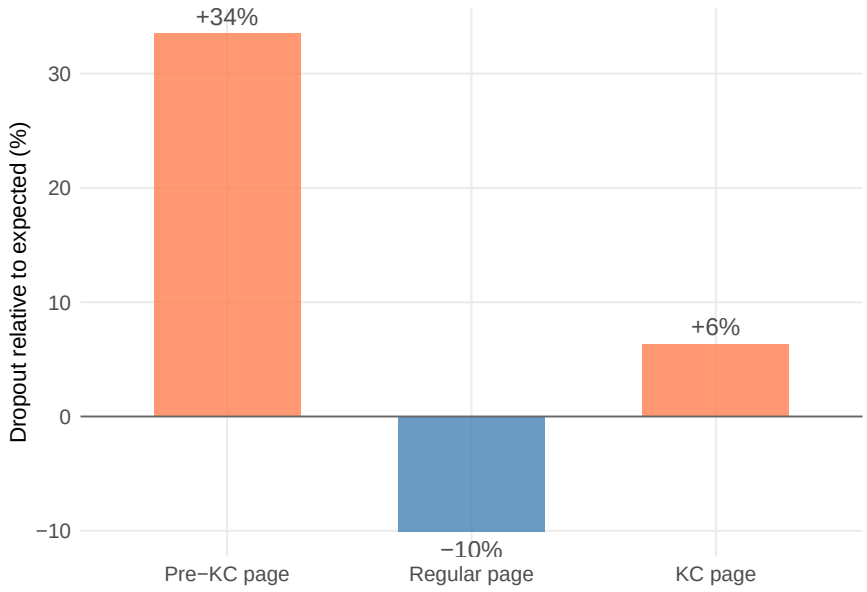


Figure 6: Dropout Relative to Expected by Page Type

Notes: Each bar shows how much more (or less) dropout occurs on that page type relative to what we would expect if dropout were spread evenly across all pages. For example, +16% means 16% more dropouts land on pre-KC pages than their share of total pages would predict. Pre-KC page = the page immediately before a knowledge check. Public school sample, modules with knowledge checks only.

3.3 District Characteristics and the Digital Divide (RQ4)

Table 5 examines how district characteristics help explain completion. Minority enrollment share is negatively associated with completion, and this relationship persists after controlling for per-pupil expenditure and staffing ratios.

Figure 7 shows completion rates by minority share quintile. The raw pattern is non-monotonic, reflecting geographic composition: the second quintile has the highest raw completion (95.2 percent) while the lowest minority quintile has the lowest (89.5 percent). This pattern reverses in the regression, which controls for state fixed effects: *within* states, higher minority enrollment is associated with lower completion.

Table 5: School Characteristics and Completion

Model:	(1)	(2)	(3)
Variables			
Personal Finance	-1.610***	-1.603***	-1.677***
	(0.4439)	(0.4440)	(0.4535)
Module Pages	-0.2378***	-0.2374***	-0.2372***
	(0.0154)	(0.0154)	(0.0154)
Pre-test Score	0.0462***	0.0464***	0.0467***
	(0.0048)	(0.0048)	(0.0049)
Minority Share	-4.873***	-4.506***	-4.187***
	(1.265)	(1.314)	(1.381)
Expenditure ($1000s)		-0.1232*	-0.2006*
		(0.0667)	(0.1024)
Student-Teacher Ratio			38.07
			(27.78)
Fixed-effects			
state	Yes	Yes	Yes
Fit statistics			
Observations	1,778,696	1,778,696	1,778,696
R^2	0.05475	0.05496	0.05515
Within R^2	0.02051	0.02073	0.02092

Clustered (nces_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

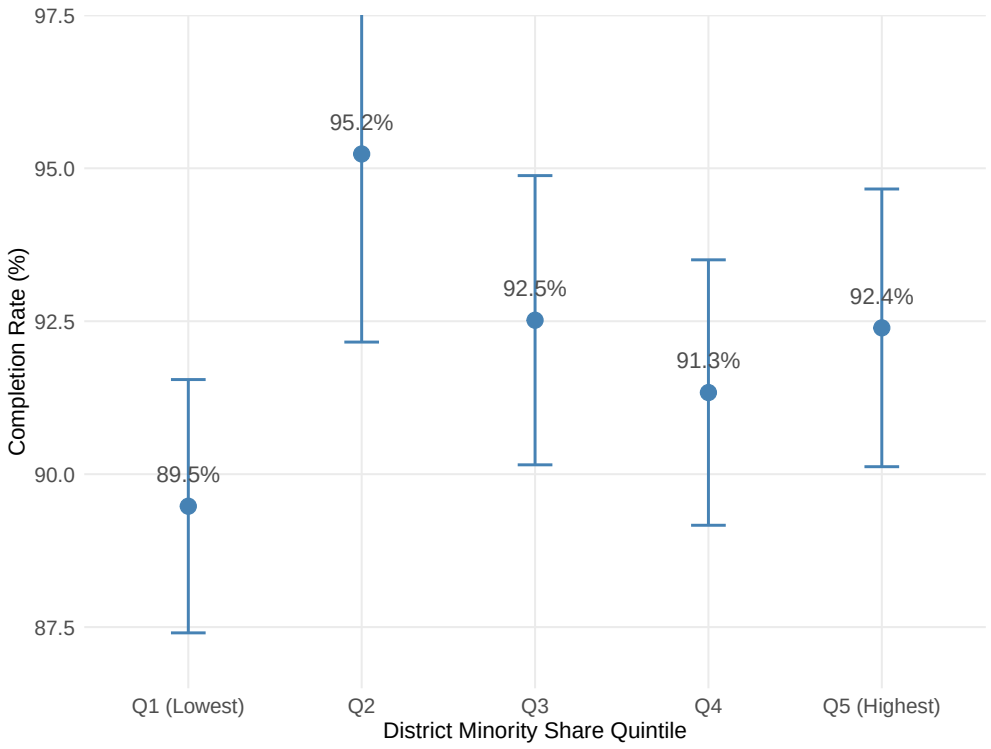


Figure 7: Completion Rate by District Minority Share

Notes: Districts grouped by minority share quintile. Error bars show 95% confidence intervals with standard errors clustered at the school level. Public school sample.

That this gap persists after controlling for expenditure and staffing is consistent with the broader literature on the digital divide in K-12 education (Dolan, 2016; Vigdor, Ladd, & Martinez, 2014; Warschauer, 2004). We find that resource-based policies alone may be insufficient to close the gap. Potentially relevant additional explanatory mechanisms, unexplored in our study, include differential technology access at home, content relevance, or competing demands on student time (among others).

3.4 Generalizability Across Samples

Table 6 tests whether the main findings generalize beyond our primary sample of public school students. We estimate the same specification across four subsamples: public schools matched to their NCES codes (our main sample), other organizations, all K-12 students enrolled in public and private schools, and college students.

Table 6: Comparison Across Samples

Model:	Public Schools (1)	Other (2)	All K-12 (3)	College (4)
Variables				
Personal Finance	-1.756***	-2.212***	-1.925***	-0.8544
	(0.4426)	(0.3463)	(0.3768)	(0.5698)
Module Pages	-0.2381***	-0.2427***	-0.2394***	-0.1035***
	(0.0149)	(0.0180)	(0.0136)	(0.0396)
Pre-test Score	0.0450***	0.0449***	0.0454***	0.0147***
	(0.0043)	(0.0043)	(0.0043)	(0.0051)
Fixed-effects				
state	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,089,865	1,387,701	2,240,767	265,905
R^2	0.04565	0.03276	0.04322	0.04075
Within R^2	0.01875	0.01926	0.01869	0.00954

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The page effect is remarkably stable across K-12 samples: −0.24 percentage points per page in public schools, other organizations, and the pooled K–12 school population. The pre-test effect is similarly consistent: 0.045 percentage points. College students show a smaller page effect (−0.10 percentage points) and weaker pre-test relationship, possibly reflecting greater self-regulation or different course structures in higher education. The stability of our main coefficients across 2.2 million K-12 observations and diverse institutional contexts strengthens confidence that the findings reflect fundamental features of online learning rather than idiosyncrasies of our primary sample.

Table 7 directly compares the public and private schools we were able to match to their NCES codes. Private school students exhibit higher completion rates (95.0 versus 92.5 percent), and the private school indicator is positive in the pooled regression (+3 percentage points, marginally significant). Moreover, the page and pre-test score effects are weaker and not significant for private schools, suggesting substantial differences in instructional practices or student characteristics between each type of institution. However, module length matters

regardless of institutional setting. Figure 8 illustrates this point: both school types show declining completion as modules lengthen, but private schools maintain higher completion throughout.

Table 7: Public vs Private School Comparison

Model:	Public (1)	Private (2)	Pooled (3)
Variables			
Constant	94.57***	96.63***	94.54***
	(0.4502)	(1.412)	(0.4510)
Personal Finance	-2.380**	-0.6306	-2.354**
	(0.9556)	(1.550)	(0.9455)
Module Pages	-0.2328***	-0.0677	-0.2299***
	(0.0144)	(0.0487)	(0.0143)
Pre-test Score	0.0513***	-0.0007	0.0508***
	(0.0057)	(0.0152)	(0.0057)
Private School			2.988*
			(1.526)
Fit statistics			
Observations	2,089,865	24,532	2,114,397
R^2	0.01775	0.00266	0.01758

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

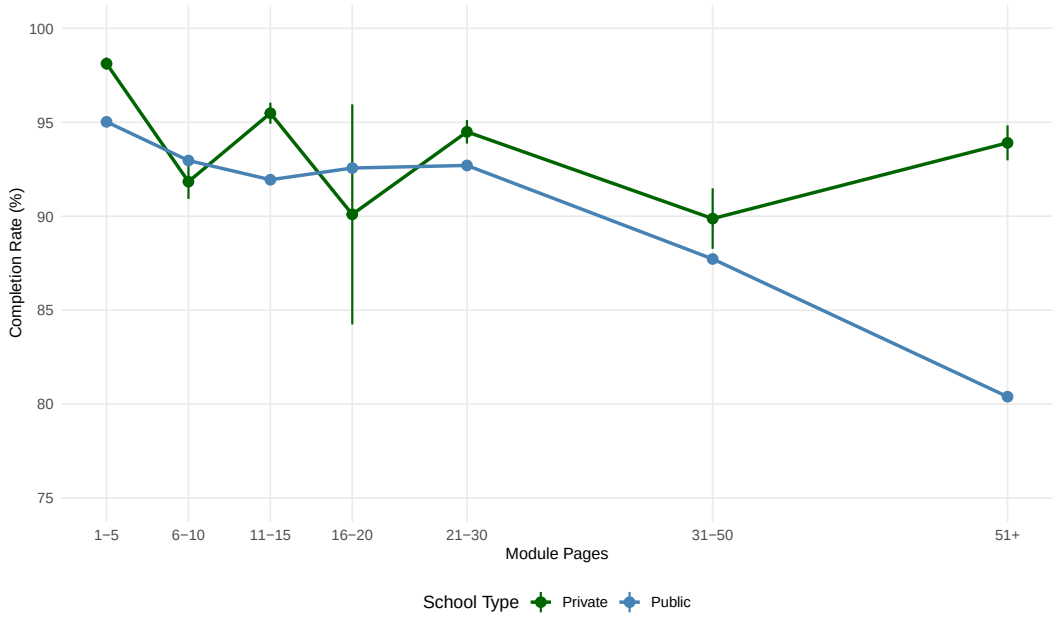


Figure 8: Completion by Module Length: Public vs Private Schools

Notes: Mean completion rates with 95% confidence intervals by module length bin.

4 Discussion

4.1 Early Engagement

Figure 9 shows where non-completing students disengage. The distribution has two concentrations: dropout is highest in the first 20 percent of module progress, reflecting students who disengage quickly, and spikes again near the end of the module. The late concentration reflects students who navigate through most content pages but never submit the post-test, consistent with the platform's definition of completion as post-test submission rather than page visitation.

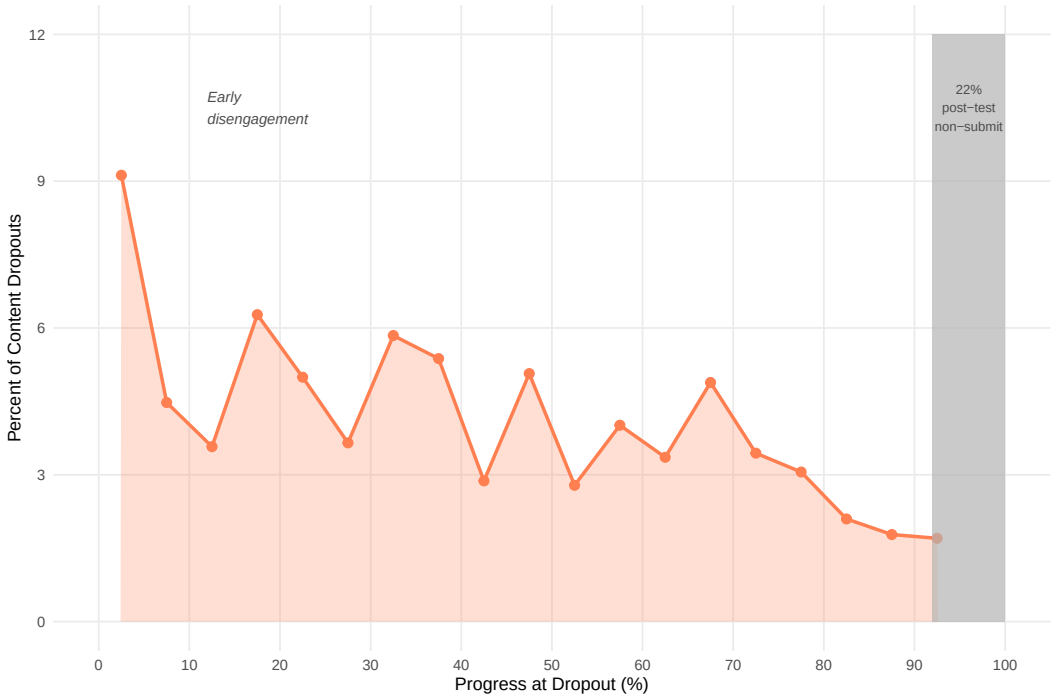


Figure 9: Distribution of Progress at Dropout

Notes: Among students who started but did not complete modules (N = 156,960). Progress measured as pages completed / total pages. Bins of 5 percentage points. Shaded area highlights the density of dropout.

Figure 10 presents the survival curve, showing the fraction of students whose page-level progress reaches each decile. Starting from 100 percent, survival drops to 98.9 percent by the first decile and continues declining through subsequent deciles. Because the completion indicator and the page-progress measure do not perfectly agree for all students (0.9 percent are marked complete without visiting every page), the survival curve should be interpreted as characterizing the shape of attrition rather than as an exact decomposition of the completion rate.

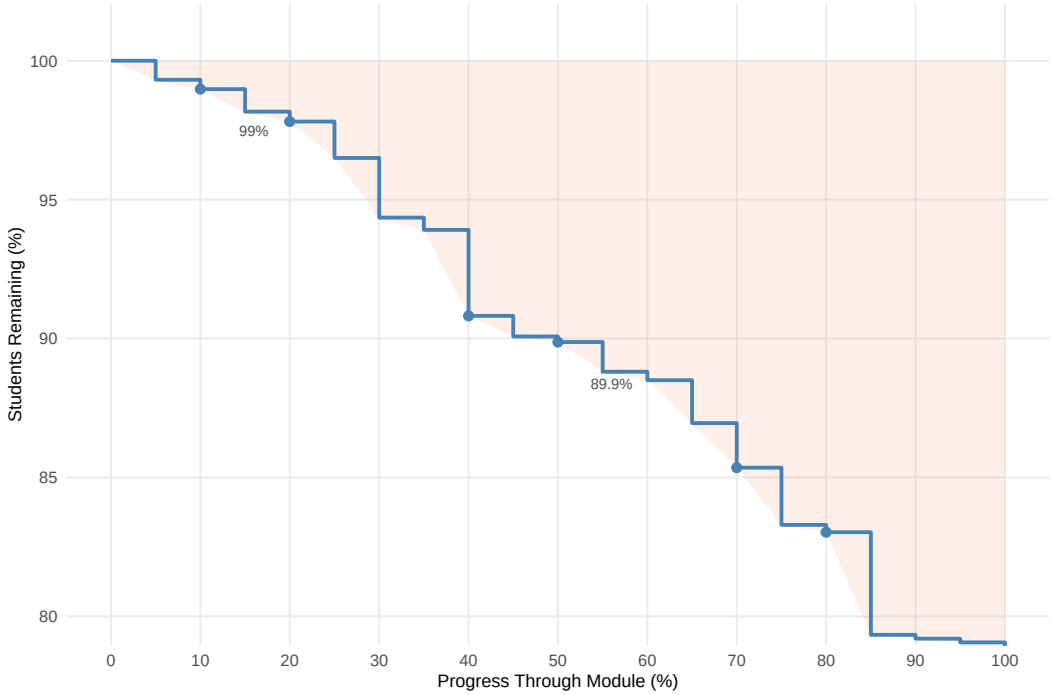


Figure 10: Survival Curve: Student Progression Through Modules

Notes: Step function shows the proportion of students reaching each progress threshold (5% increments). Shaded area represents cumulative attrition. Y-axis truncated at 80% for readability. Public school sample.

Figure 11 provides a direct test of H4. The conditional probability of dropout is highest in the first decile (1.0 percent) and falls to 0.3 percent in the final decile, a threefold decrease that supports the hypothesis, though the decline is not strictly monotonic: the hazard rises slightly in the middle deciles, possibly reflecting knowledge check placement. The overall pattern is clear: students who survive the opening pages are increasingly likely to finish.

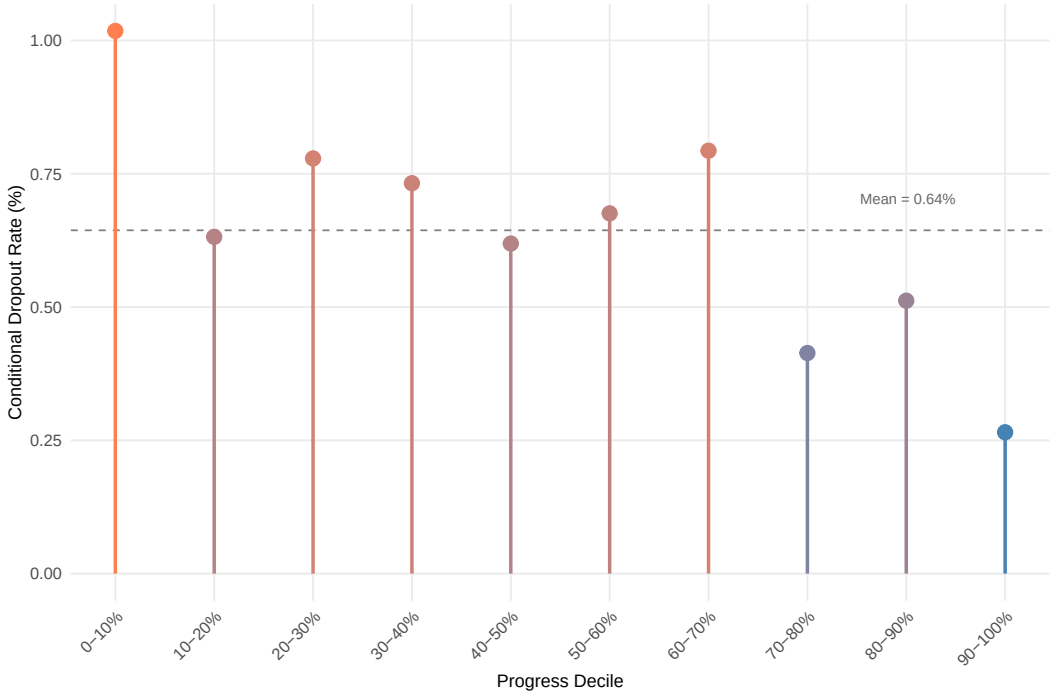


Figure 11: Conditional Dropout Hazard by Progress Decile

Notes: Hazard = Pr(dropout in decile d | reached decile d). Based on all student-module observations in the public school sample.

This pattern is consistent with cognitive load theory: early pages establish schemas that reduce the marginal cost of processing subsequent content (Sweller et al., 2011). Once a student invests in understanding foundational concepts, continuing becomes easier. The practical implication is clear: interventions targeting the opening pages of modules, such as engaging introductions, clear learning objectives, and early opportunities for success that nudge students toward persistence (Damgaard & Nielsen, 2018), may yield outsized returns.

Figure 12 stratifies survival curves by module length and helps understand the potentially mechanical effect of page count on dropout behavior. Longer modules offer more exit points than shorter modules before reaching the post-test, so their overall student retention rate is lower. However, long modules exhibit steeper early decline, suggesting students foresee, and dread, larger time commitments and heavier cognitive loads. By mid-module, short modules retain 97 percent of students versus 65 percent for long modules.

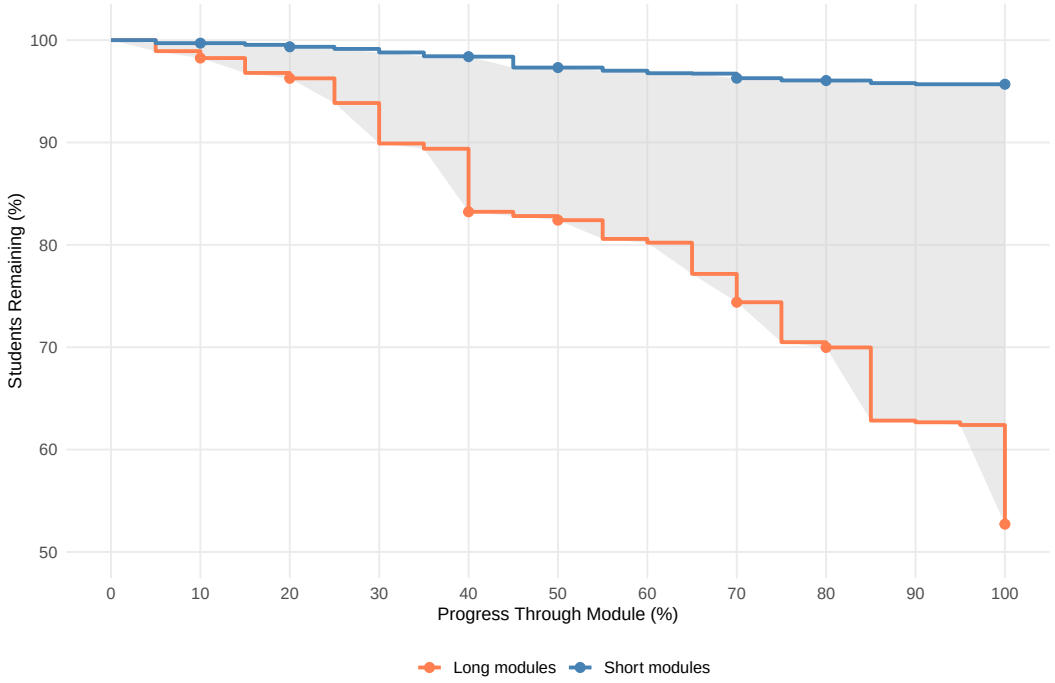


Figure 12: Survival Curves by Module Length

Notes: Short = ≤ median pages; Long = > median pages.

4.2 Implications for Instructional Design

Our findings suggest four design principles for online educational content. We frame these as actionable recommendations for curriculum developers, instructional designers, and teachers.

Reduce the evaluative signal of formative assessments. Formative assessment improves learning when students perceive it as supportive rather than evaluative (Black & Wiliam, 1998). Our data suggest that embedded questions trigger disengagement in some students, likely because they interpret the questions as tests. The anticipation effect (H6) is particularly noteworthy: students appear to disengage *before* reaching the assessment, suggesting that the mere presence of upcoming questions creates anxiety or avoidance.

Instructional designers can implement several specific changes to address this issue:

- *Reframe the language*: Use "Check your understanding" or "Practice question" rather than "Quiz" or "Test." Avoid countdown timers or other features that signal high-stakes evaluation.

- *Remove visible scores*: If the platform displays numerical feedback (e.g., "3/5 correct"), consider replacing it with qualitative feedback ("You're making progress!") or removing scores entirely for formative checks.

- *Provide immediate explanatory feedback*: When students answer incorrectly, offer explanations that treat the error as a learning opportunity rather than a failure. Phrases like "Not quite—here's why..." are preferable to "Incorrect."

- *Allow revision*: Let students attempt questions multiple times without penalty. This signals that the purpose is learning, not evaluation.

- *Delay placement*: Our data suggest that modules where the first knowledge check appears later have higher completion. Consider placing the first assessment after students have built enough engagement and self-efficacy to persist through it.

These changes could preserve the pedagogical benefits of retrieval practice (Roediger & Butler, 2011) while reducing the evaluative signal that prompts some students to quit.

Scaffold instruction for students with weaker preparation. The interaction between module length and prior knowledge (H3) implies that longer modules disproportionately disadvantage students who are already struggling. The 30 percent difference in sensitivity to module length between high- and low-knowledge students is practically meaningful: for a 50-page module, this translates to a 4-percentage-point completion gap beyond the direct effect of prior knowledge.

Instructional designers should consider implementing adaptive pathways:

- *Use pre-test scores for routing*: The pre-test, already built into many platforms, provides a natural signal for adaptive instruction. Students scoring below a threshold could receive a condensed version of the module, supplementary material before the main content, or a recommendation to complete a prerequisite module first.

- *Offer "express" and "comprehensive" tracks*: For essential content, create two versions—a shorter version covering core concepts and a longer version with additional depth. Let students (or teachers) choose based on prior knowledge.

- *Front-load accessible content*: When a single module must serve all students, place the most accessible content at the beginning. This helps low-knowledge students build foundational schemas before encountering more challenging material.

- *Provide just-in-time support*: Embed optional review content (e.g., "Need a refresher on X? Click here") that low-knowledge students can access without requiring all students to complete it.

These approaches align with the broader principle of differentiated instruction (Tomlinson, 2014) and could help close the completion gap between high- and low-knowledge students.

Invest in the opening pages. The declining hazard rate implies that the first few pages of each module are disproportionately important for retention. Providing engaging openings that establish relevance ("why does this matter?"), a roadmap ("in this module you will learn..."), and early opportunities for success (Damgaard & Nielsen, 2018) can help students survive the high-hazard initial segment. Our hazard data suggest that if a student reaches the second decile of a module, the probability of completion increases substantially.

Keep modules short, or segment long content. The page effect is one of the most robust findings in our data: it survives within-student comparisons and holds across samples. A 10-page increase reduces completion by 2.4 percentage points. For a 50-page module, this translates to more than 10 percentage points of lost completion relative to a 5-page module. Where content requires extended treatment, breaking it into shorter, self-contained segments with clear stopping points may preserve learning goals while reducing dropout. Prior work on the Econ Lowdown platform has found evidence consistent with this approach (Méndez-Carbajo & Wolla, 2019).

Table 8: Summary of Practical Implications

What We Found	What Instructional Designers Should Do
1. Module length matters Each page reduces completion by 0.24 pp	Keep modules to 15–25 pages; segment longer content into sequential units
2. Prior knowledge buffers length effects Page effect 30% weaker for prepared students	Use pre-test routing; offer express vs. comprehensive tracks
3. Knowledge checks amplify dropout Page effect doubles in KC modules; dropout spikes before assessments	Reframe as "practice"; place first KC after engagement established
4. Early pages are critical Dropout hazard highest in first 10% of module	Hook students early: establish relevance, provide roadmap, offer quick wins
5. Equity gaps persist Higher minority enrollment predicts lower completion	Provide scaffolding, shorter modules, and accessible content for underserved populations

4.3 Completion as an Outcome

Our measured outcome is module completion, not learning. This distinction matters. A student who finishes a module may not retain the material; a student who drops out may have learned something valuable before disengaging. Our data cannot speak to knowledge acquisition, skill development, or long-term retention. The post-test scores we observe are crude proxies at best: they measure performance immediately after exposure, not durable understanding.

We focus on completion for three reasons. First, completion is a necessary condition for the full learning experience. A student who drops out at page 5 of a 25-page module misses 80 percent of the content, including the cumulative assessments designed to consolidate learning. Second, completion is observable and unambiguous. Unlike learning, which requires validated assessments administered at multiple time points, completion is recorded automatically and consistently across all students and modules. Third, completion is actionable. Instructional designers can modify module features that affect completion; they cannot directly modify what students remember six months later.

To be sure, optimizing for completion could be counterproductive if it came at the expense of learning. A module could be made trivially easy, removing all challenge and all knowledge checks, and completion would likely rise. Learning would not. The design implications we offer should therefore be understood as conditional: shorter modules, scaffolded content, and reduced evaluative signaling are valuable *if* they increase completion without sacrificing the pedagogical features that produce learning. The existing evidence on retrieval practice (Roediger & Butler, 2011), spaced repetition (Cepeda, Pashler, Vul, Wixted, & Rohrer, 2006), and desirable difficulties (Bjork & Bjork, 2011) suggests that some friction in the learning process is beneficial. Our findings do not contradict this evidence. They suggest instead that the *timing* and *framing* of that friction matters: a knowledge check on page 3 may trigger dropout before the student has built enough investment to persist, while the same check on page 15 may function as intended.

Until such evidence accumulates, our results should be interpreted as informing the design of engaging online content, not as a blueprint for maximizing educational value.

4.4 Limitations and Future Research

Several limitations should be considered when interpreting our findings. At the same time, they sketch avenues for future research.

Lack of individual student characteristics. We observe students through anonymized identifiers and lack individual demographics (race, gender, socioeconomic status) and session timing. Linking module records to individual characteristics and observing when students work (school hours versus evenings, weekdays versus weekends) would sharpen our understanding of dropout mechanisms. The district-level demographics we do observe provide suggestive evidence of demographic disparities, but individual-level data would enable more precise targeting of interventions.

Single platform and subject area. Our data come from one platform (Econ Lowdown) focused on economics and personal finance. The modules share a common design philosophy, pedagogical approach, and student population. Whether these patterns generalize to other platforms, subject areas (STEM, humanities, language learning), or instructional designs (video-based, simulation-based, collaborative) remains an open empirical question.

Observational identification. Although our within-student design controls for stable student characteristics and teacher assignment provides exogenous variation in which modules students encounter, we cannot rule out all sources of confounding. Students may differentially engage with modules based on unobserved factors (interest in specific topics, time of semester) that correlate with module characteristics. Our estimates should be interpreted as associations rather than causal effects.

COVID-19 period. Our data span 2014-2025, including the COVID-19 pandemic. Completion rates generally rose through 2020 with no visible disruption, likely because the platform functions as a classroom supplement rather than a substitute for in-person instruction.

This limits our ability to speak to the effects of emergency remote learning or to generalize to contexts where online education is the primary mode of instruction.

Interventions not tested. Our analysis identifies factors associated with dropout but does not test interventions. The design recommendations we offer (shorter modules, reduced evaluative signaling, engaging openings) are informed by theory and prior literature, but their effectiveness has not been experimentally validated on this platform. The potential benefits of instructor interventions (Borrella, Caballero-Caballero, & Ponce-Cueto, 2022) or orientation programs (Zweig, Hanita, Stafford, & Khanani, 2021) remain open questions that future experimental work could address.

5 Conclusion

This paper uses administrative data from 442,000 students completing 2.1 million module assignments over 11 years to study dropout in online K-12 economics and personal finance education. We document that module length, prior knowledge, embedded formative assessments, and district demographics independently shape whether students complete assigned modules.

Three findings carry direct implications for instructional design. Module length matters, but prior knowledge moderates its effect: longer modules disproportionately disadvantage students with weaker preparation. Differentiated instruction, through shorter modules or additional scaffolding for low-knowledge students, could reduce this gap. Embedded knowledge checks trigger disengagement in some students despite being designed to assist learning. Reducing the evaluative signal of these assessments could preserve their pedagogical benefits while improving persistence. The hazard of dropout is highest in the first pages of each module and declines rapidly thereafter. Investing in engaging openings may yield outsized returns.

We also document that districts with higher minority enrollment show lower completion

even after controlling for school resources, a finding that connects to the broader literature on the digital divide in K-12 education. This gap suggests that improving online education for underserved students requires more than increased funding.

Despite these limitations (see Section 4.4), the scale of our data, the precision of page-level tracking, and the research design afforded by teacher assignment provide credible evidence on questions of growing importance for educational practice. The findings are directly relevant to instructional designers building online K-12 content, to teachers selecting and assigning digital resources, and to policymakers evaluating the equity implications of online education. As online learning becomes a permanent feature of K-12 instruction, understanding what leads students to disengage, and when, is essential for ensuring that digital content serves all students effectively.

Declaration of generative AI use. During the preparation of this work the authors used Claude to assist with data processing scripts, organize empirical output, and draft portions of the paper. The authors reviewed and edited all content and take full responsibility for the published article.

References

Ahn, J., & McEachin, A. (2017). Student enrollment patterns and achievement in ohio's online charter schools. *Educational Researcher*, *46*(1), 44–57.

Backes, B., & Cowan, J. (2019, 02). Is the pen mightier than the keyboard? the effect of online testing on measured student achievement. *Economics of Education Review*, *68*, 89-103.

Baker, R. S., D'Mello, S. K., Rodrigo, M. M. T., & Graesser, A. C. (2010). Better to be frustrated than bored: The incidence, persistence, and impact of learners' cognitive-affective states during interactions with three different computer-based learning environments. *International Journal of Human-Computer Studies*, *68*(4), 223–241.

Barbour, M. K. (2017). K-12 online learning and school choice: Growth and expansion in the absence of evidence. In R. A. Fox & N. K. Buchanan (Eds.), *The wiley handbook of school choice* (pp. 421–440). Hoboken, NJ: Wiley Blackwell.

Barbour, M. K., & Reeves, T. C. (2009). The reality of virtual schools: A review of the literature. *Computers & Education*, *52*(2), 402–416.

Batty, M., Collins, J. M., O'Rourke, C., & Odders-White, E. (2020). Experiential financial education: A field study of My Classroom Economy in elementary schools. *Economics of Education Review*, *78*, 102014.

Bean, J. P., & Metzner, B. S. (1985). A conceptual model of nontraditional undergraduate student attrition. *Review of Higher Education*, *12*(2), 485–540.

Bergé, L. (2018). Efficient estimation of maximum likelihood models with multiple fixed-effects: The R package FENmlm. *CREA Discussion Papers*(13). (R package fixest, https://CRAN.R-project.org/package=fixest)

Bettinger, E. P., Fox, L., Loeb, S., & Taylor, E. S. (2017). Virtual classrooms: How online college courses affect student success. *American Economic Review*, *107*(9), 2855–2875.

Bjork, E. L., & Bjork, R. A. (2011). *Making things hard on yourself, but in a good way: Creating desirable difficulties to enhance learning.* Worth Publishers.

Black, P., & Wiliam, D. (1998). Inside the black box: Raising standards through classroom assessment. *Phi Delta Kappan*, *80*(2), 139–148.

Borrella, I., Caballero-Caballero, S., & Ponce-Cueto, E. (2022). Taking action to reduce dropout in MOOCs: Tested interventions. *Computers & Education*, *179*, 104412.

Buckles, S., & Freeman, V. (1984). A longitudinal analysis of a developmental economics education program. *The Journal of Economic Education*, *15*(1), 5–10.

Cepeda, N. J., Pashler, H., Vul, E., Wixted, J. T., & Rohrer, D. (2006). Distributed practice in verbal recall tasks: A review and quantitative synthesis. *Psychological Bulletin*, *132*(3), 354–380.

Chen, C., Sonnert, G., Sadler, P. M., Sasselov, D. D., Fredericks, C., & Malan, D. J. (2020). Going over the cliff: MOOC dropout behavior at chapter transition. *Distance Education*, *41*(1), 6–25.

Clark, R. L., Lusardi, A., & Mitchell, O. S. (2024). Does being financially resilient lead to better economic outcomes? *Journal of Financial Literacy and Wellbeing*, *2*(3), 191–208.

Corry, M., Dardick, W., & Stella, J. (2016). An examination of dropout rates for hispanic or latino students enrolled in online k-12 schools. *Education and Information Technologies*, *22*(5), 2001–2012.

Council for Economic Education. (2026). *Survey of the states.* https://www.councilforeconed.org/policy-advocacy/survey-of-the-states/.

Damgaard, M. T., & Nielsen, H. S. (2018). Nudging in education. *Economics of Education Review*, *64*, 313–342.

Darling-Aduana, J. (2021). Authenticity, engagement, and performance in online high school courses: Insights from micro-interactional data. *Computers & Education*, *167*, 104175.

de la Varre, C., Irvin, M. J., Jordan, A. W., Hannum, W. H., & Farmer, T. W. (2014). Reasons for student dropout in an online course in a rural K–12 setting. *Distance Education*, *35*(3), 324–344.

Dolan, J. E. (2016). Splicing the divide: A review of research on the evolving digital divide among K-12 students. *Journal of Research on Technology in Education*, *48*(1), 16–37.

Douglas, K. A., Merzdorf, H. E., Hicks, N. M., Sarfraz, M. I., & Bermel, P. (2020). Challenges to assessing motivation in MOOC learners: An application of an argument-based approach. *Computers & Education*, *150*, 103829.

Expósito, A., Sánchez-Rivas, J., Gómez-Calero, M. P., & Pablo-Romero, M. P. (2020, 01). Examining the use of instructional video clips for teaching macroeconomics. *Computers & Education*, *144*, 103709.

Fitzpatrick, B. R., Berends, M., Ferrare, J. J., & Waddington, R. J. (2020). Virtual illusion: Comparing student achievement and teacher and classroom characteristics in online and brick-and-mortar charter schools. *Educational Researcher*, *49*(3), 161–175.

Formanek, M., Wenger, M. C., Buxner, S. R., Impey, C. D., & Sonam, T. (2017). Insights about large-scale online peer assessment from an analysis of an astronomy mooc. *Computers & Education*, *113*, 243-262.

Gemin, B., Pape, L., Vashaw, L., & Watson, J. (2015). *Keeping pace with K-12 digital learning: An annual review of policy and practice* (Tech. Rep.). Durango, CO: Evergreen Education Group.

Hamlin, D., Adigun, O., & Adams, C. (2023). Do fully virtual schools deliver in rural areas? a longitudinal analysis of academic outcomes. *Computers & Education*, 104789.

Hart, C. (2012). Factors associated with student persistence in an online program of study: A review of the literature. *Journal of Interactive Online Learning*, *11*(1), 19–42.

Hart, C., Friedmann, E., & Hill, M. (2018). Online course-taking and student outcomes in california community colleges. *Education Finance and Policy*, *13*, 42–71.

Harter, C., & Harter, J. (2021). Is financial literacy improved by participating in a stock market game? *Journal for Economic Educators*, *10*, 21-32. Retrieved from https://libjournals.mtsu.edu/index.php/jfee/article/view/1459

Heppen, J. B., Sorensen, N., Allensworth, E., Walters, K., Rickles, J., Taylor, S. S., &

Michelman, V. (2016). The struggle to pass algebra: Online vs. face-to-face credit recovery for at-risk urban students. *Journal of Research on Educational Effectiveness*, *10*(2), 272–296.

Hill, A., & LoPalo, M. (2024, 02). The effects of online vs in-class testing in moderate-stakes college environments. *Economics of Education Review*, *98*, 102505-102505.

Hinojosa, T., Miller, S., Swanlund, A., Hallberg, K., Brown, M., & O brien, B. (2010). *Title: The impact of the stock market game on financial literacy and mathematics achievement: Results from a national randomized controlled trial.* Retrieved from https://files.eric.ed.gov/fulltext/ED513109.pdf

Hohlfeld, T. N., Ritzhaupt, A. D., Dawson, K., & Wilson, M. L. (2017). An examination of seven years of technology integration in florida schools: Through the lens of the levels of digital divide in schools. *Computers & Education*, *113*, 135–161.

Ipinnaiye, O., & Risquez, A. (2024, 07). Exploring adaptive learning, learner-content interaction and student performance in undergraduate economics classes. *Computers & Education*, *215*, 105047-105047.

Janelli, M., & Lipnevich, A. A. (2021). Effects of pre-tests and feedback on performance outcomes and persistence in massive open online courses. *Computers & Education*, *161*, 104076.

Jayaprakash, S. M., Moody, E. W., Lauría, E. J., Regan, J. R., & Baron, J. D. (2014). Early alert of academically at-risk students: An open source analytics initiative. *Journal of Learning Analytics*, *1*(1), 6–47.

Johnson, C. C., Walton, J. B., Strickler, L., & Elliott, J. B. (2023). Online teaching in k-12 education in the united states: A systematic review. *Review of Educational Research*, *93*(3), 353–411.

Joksimović, S., Poquet, O., Kovanović, V., Dowell, N., Mills, C., Gašević, D., ... Brooks, C. (2018). How do we model learning at scale? a systematic review of research on MOOCs. *Review of Educational Research*, *88*(1), 43–86.

Jordan, K. (2015). Massive open online course completion rates revisited: Assessment, length and attrition. *International Review of Research in Open and Distributed Learning*, *16*(3), 341–358.

Kaiser, T., Lusardi, A., Menkhoff, L., & Urban, C. (2022). Financial education affects financial knowledge and downstream behaviors. *Journal of Financial Economics*, *145*(2), 255–272.

Kennedy, C., Rhoads, C., & Leu, D. J. (2016). Online research and learning in science: A one-to-one laptop comparison in two states using performance based assessments. *Computers & Education*, *100*, 141–161.

Lee, Y., & Choi, J. (2011). A review of online course dropout research: Implications for practice and future research. *Educational Technology Research and Development*, *59*(5), 593–618.

Lipnevich, A. A., Janelli, M., Park, M. J., Calistro, T., & Esser, F. J. (2024). The effects of practice testing and feedback on learners' performance and persistence in a massive open online course. *Journal of Research on Technology in Education*, 1–20.

Liu, F., Ritzhaupt, A. D., Dawson, K., & Barron, A. E. (2016). Explaining technology integration in k-12 classrooms: A multilevel path analysis model. *Educational Technology Research and Development*, *65*(4), 795–813.

Luedtke, A. O., & Urban, C. (2023). High school financial education courses in the united states: What is the importance of setting state policies? *Journal of Financial Literacy and Wellbeing*, *1*(3), 431–449.

Lusardi, A., & Mitchell, O. S. (2014). The economic importance of financial literacy: Theory and evidence. *Journal of Economic Literature*, *52*(1), 5–44.

McDaniel, M. A., Anderson, J. L., Derbish, M. H., & Morrisette, N. (2007). Testing the testing effect in the classroom. *European Journal of Cognitive Psychology*, *19*(4–5), 494–513.

Méndez-Carbajo, D., & Malakar, L. C. (2020). Flipping the classroom with econlowdown.org.

The Journal of Economic Education, *51*(1), 95–102.

Méndez-Carbajo, D., & Wolla, S. A. (2019). Segmenting educational content: Long-form vs. short-form online learning modules. *American Journal of Distance Education*, *33*(2), 108–119.

National Center of Education Statistics, U. D. o. E. (2022). *A publication of the national center for education statistics.* Retrieved from https://nces.ed.gov/pubs2022/2022019.pdf

Oberrauch, L., Kaiser, T., & Lusardi, A. (2024). Assessing financial literacy among the young. *Journal of Financial Literacy and Wellbeing*, *2*(1), 63–78.

Pane, J. F., Steiner, E. D., Baird, M. D., Hamilton, L. S., & Pane, J. D. (2017). *Informing progress: Insights on personalized learning implementation and effects* (Tech. Rep.). RAND Corporation.

Pardos, Z. A., Baker, R. S., San Pedro, M. O., Gowda, S. M., & Gowda, S. M. (2014). Affective states and state tests: Investigating how affect and engagement during the school year predict end-of-year learning outcomes. *Journal of Learning Analytics*, *1*(1), 107–128.

Poquet, O., Nguyen, Q., Kovanovic, V., Brooks, C., Dawson, S., & Biotteau, A. (2022). Grade-based similarity prevails in online course forums at scale. *Computers & Education*, *178*, 104401.

Rauh, J. (2011). Online education as a toll good: An examination of the south carolina virtual school program. *Computers & Education*, *57*(2), 1583–1594.

Reich, J., & Ruipérez-Valiente, J. A. (2019). MOOC pivot. *Science*, *363*(6423), 130–131.

Roediger, H. L., & Butler, A. C. (2011). The critical role of retrieval practice in long-term retention. *Trends in Cognitive Sciences*, *15*(1), 20–27.

Schwerdt, G., & Chingos, M. M. (2015). Virtual schooling and student learning: Evidence from the florida virtual school. *Working Paper*.

Siemens, G. (2013). Learning analytics: The emergence of a discipline. In *American*

behavioral scientist (Vol. 57, pp. 1380–1400). SAGE.

Suiter, M. C., Wolla, S. A., & Levin, L. (2022, 10). *Economics and personal finance courses: Complements not substitutes.* Retrieved 2026-04-15, from https://fraser.stlouisfed.org/title/9398/item/688200

Sweller, J. (1988). Cognitive load during problem solving: Effects on learning. *Cognitive Science*, *12*(2), 257–285.

Sweller, J., Ayres, P., & Kalyuga, S. (2011). *Cognitive load theory.* New York: Springer.

Tinto, V. (1975). Dropout from higher education: A theoretical synthesis of recent research. *Review of Educational Research*, *45*(1), 89–125.

Tomlinson, C. A. (2014). *The differentiated classroom: Responding to the needs of all learners* (2nd ed.). Alexandria, VA: Association for Supervision and Curriculum Development.

Totenhagen, C. J., Casper, D. M., Faber, K. M., Bosch, L. A., Wiggs, C. B., & Borden, L. M. (2015). Youth financial literacy: A review of key considerations and promising delivery methods. *Journal of Family and Economic Issues*, *36*(2), 167–191.

Urban, C., Schmeiser, M., Collins, J. M., & Brown, A. (2020). State financial education mandates: It's all in the implementation. *Journal of Financial Economics*, *137*(3), 620–643.

VanFossen, P. J., & Herman-Ellison, L. C. (2006). Internet-based economic education: The case of EconEdLink. *Social Education*, *70*(3), 151–156.

Vigdor, J. L., Ladd, H. F., & Martinez, E. (2014). Scaling the digital divide: Home computer technology and student achievement. *Economic Inquiry*, *52*(3), 1103–1119.

Walstad, W. B., & Watts, M. (2015). Perspectives on economics in the school curriculum: Coursework, content, and research. *The Journal of Economic Education*, *46*(3), 285–301.

Warschauer, M. (2004). *Technology and social inclusion: Rethinking the digital divide.* Cambridge, MA: MIT Press.

Wise, A. F., Zhao, Y., & Hausknecht, S. N. (2019). Learning analytics implementation

design. *Handbook of Learning Analytics*, 151–160.

Woo, L. J., Archambault, L., Borup, J., Buss, R. R., & Henriksen, D. (2025). A systematic reexamination of field experiences in pk-12 online environments in the united states. *Computers & Education*, *239*, 105447.

Xie, K., Di Tosto, G., Chen, S.-B., & Vongkulluksn, V. W. (2018). A systematic review of design and technology components of educational digital resources. *Computers & Education*, *127*, 90-106.

Xing, W., Liu, Z., Kim, T., & Song, Y. (2025). Why do students leave instructional videos? understanding students' in-video dropout behavior in a large online math learning platform. *Distance Education*, 1–24.

Xu, D., & Jaggars, S. S. (2014). Performance gaps between online and face-to-face courses: Differences across types of students and academic subject areas. *Journal of Higher Education*, *85*(5), 633–659.

Zhang, J., Gao, M., & Zhang, J. (2021). The learning behaviours of dropouts in MOOCs: A collective attention network perspective. *Computers & Education*, *167*, 104189.

Zheng, B., Lin, C.-H., & Kwon, J. B. (2020). The impact of learner-, instructor-, and course-level factors on online learning. *Computers & Education*, *150*, 103851.

Zweig, J., Hanita, M., Stafford, E., & Khanani, N. (2021). Impact of an orientation on online students' course outcomes. *Journal of Research on Technology in Education*, 1–24.

A Extended Hazard Analysis

This appendix provides additional detail on the within-module dropout dynamics discussed in Section 4.1.

Figure 13 ranks modules by completion rate. Variation is substantial, ranging from approximately 70 percent for the longest modules to over 98 percent for the shortest. Much of this variation is mechanically related to module length.

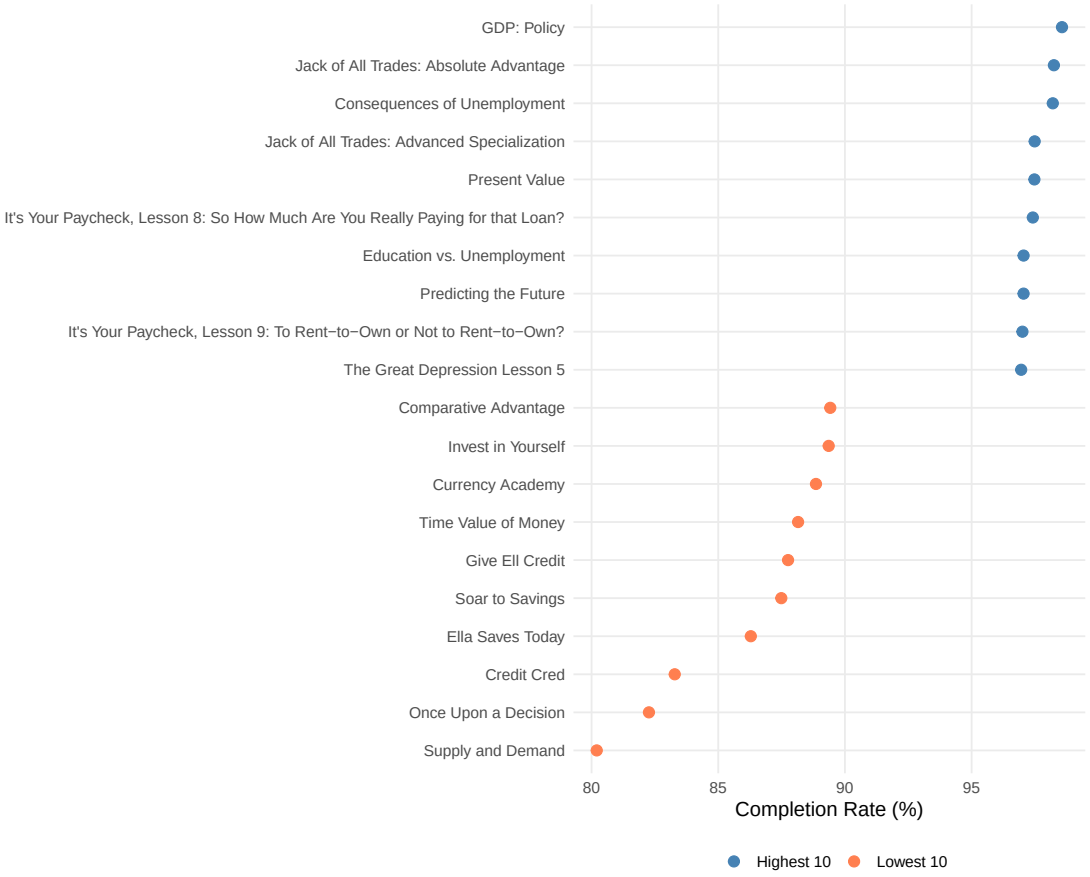


Figure 13: Module Completion Rates: Highest and Lowest 10

Notes: Top and bottom 10 modules by completion rate. Public school sample.

B Additional Analyses

This appendix presents robustness checks and heterogeneity analyses.

Table 9 tests the sensitivity of the main results. Column (1) is the baseline. Column

(2) adds a quadratic term for pages, revealing that the negative effect of length levels off for already-long modules. Columns (3)-(4) vary the level at which standard errors are computed (student, state); results are stable.

Table 9: Robustness Checks

Model:	Baseline (1)	Quadratic (2)	Student Cluster (3)	State Cluster (4)
Variables				
Personal Finance	-1.719***	-1.593***	-1.719***	-1.719**
	(0.4395)	(0.5080)	(0.0495)	(0.6484)
Module Pages	-0.2352***	-0.2080***	-0.2352***	-0.2352***
	(0.0148)	(0.0368)	(0.0019)	(0.0171)
Pre-test Score	0.0446***	0.0447***	0.0446***	0.0446***
	(0.0043)	(0.0042)	(0.0007)	(0.0048)
Private School	4.148*	4.158*	4.148***	4.148
	(2.247)	(2.249)	(0.2528)	(2.856)
$Pages^2$		-0.0004		
		(0.0005)		
Fixed-effects				
state	Yes	Yes	Yes	Yes
Fit statistics				
Observations	2,114,397	2,114,397	2,114,397	2,114,397
R^2	0.04491	0.04494	0.04491	0.04491
Within R^2	0.01869	0.01872	0.01869	0.01869

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 10 addresses a data quality concern. Four modules in our sample were segmented from a single parent module ("Supply and Demand"), and their original page counts reflected the parent rather than the segment. After correcting these page counts, the page effect is unchanged, confirming that our main results are not driven by measurement error in these modules.

Additional heterogeneity results appear in Figure 14 (completion by topic), Figure 15 (completion by assignment order), Figure 16 (completion by organization type), and Figure 17 (completion by state).

Table 10: Robustness: Excluding Segmented Modules

Model:	Full Sample (1)	Excl. Segmented (State FE) (2)	Excl. Segmented (School FE) (3)
Variables			
Personal Finance	-1.756***	-1.587***	-0.8825***
	(0.4426)	(0.4607)	(0.2784)
Module Pages	-0.2381***	-0.2069***	-0.2193***
	(0.0149)	(0.0210)	(0.0181)
Pre-test Score	0.0450***	0.0396***	0.0356***
	(0.0043)	(0.0044)	(0.0034)
Fixed-effects			
School FE			Yes
State FE	Yes	Yes	
Fit statistics			
Observations	2,089,865	1,799,909	1,799,909
R^2	0.04565	0.03909	0.13914
Within R^2	0.01875	0.00695	0.00713

Clustered (nces_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

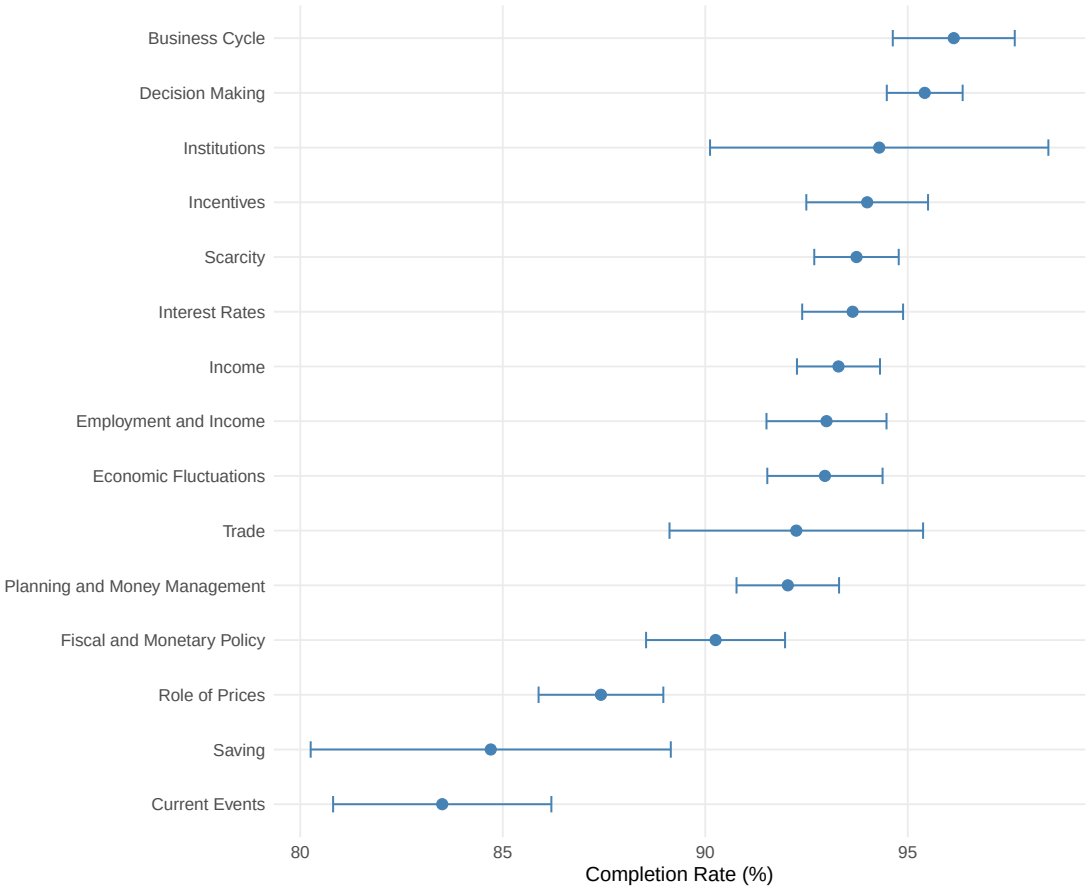


Figure 14: Completion Rate by Topic

Notes: Topics with $\geq$ 100 schools shown. Error bars show 95% confidence intervals with standard errors clustered at the school level. Public school sample.

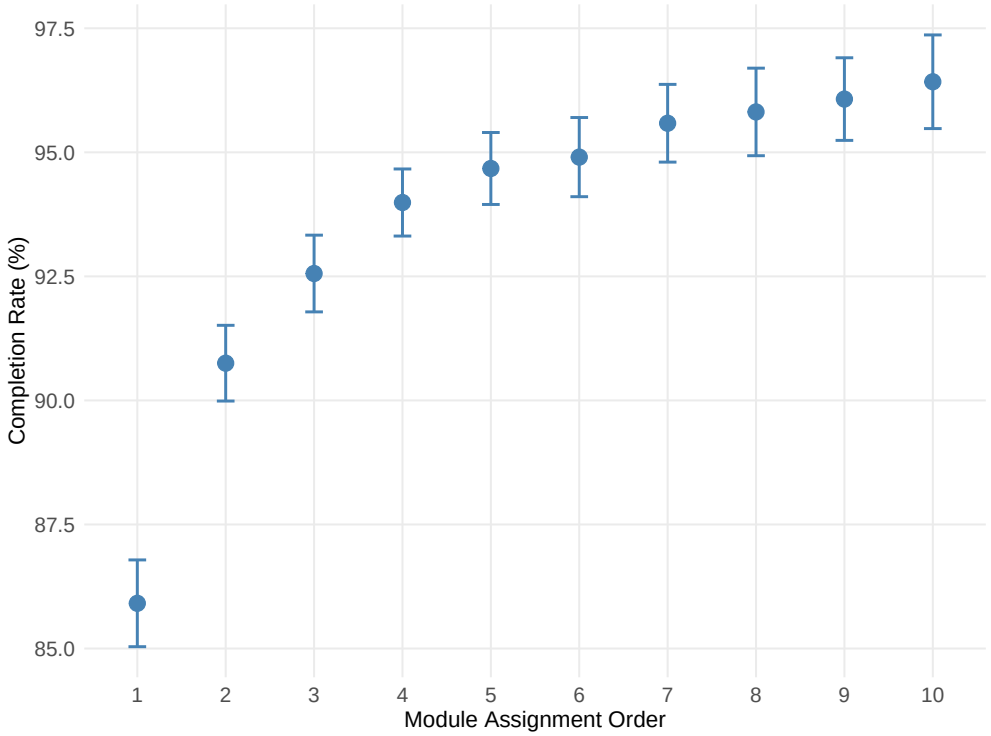


Figure 15: Completion by Assignment Sequence

Notes: Mean completion by ordinal position in student's sequence of module assignments. Error bars show 95% confidence intervals with standard errors clustered at the school level. Public school sample.

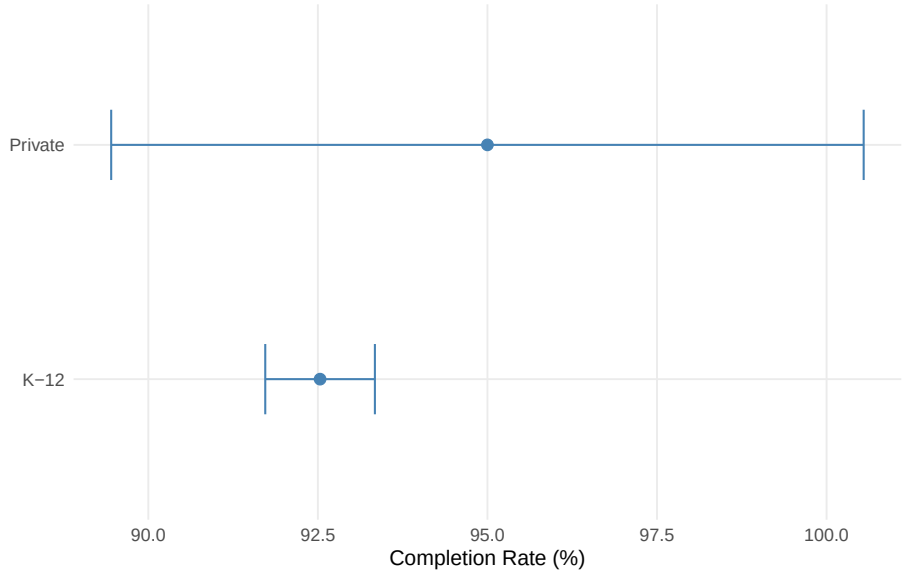


Figure 16: Completion Rate by Organization Type

Notes: Mean completion rates with 95% confidence intervals. Standard errors clustered at the school level. Full sample.

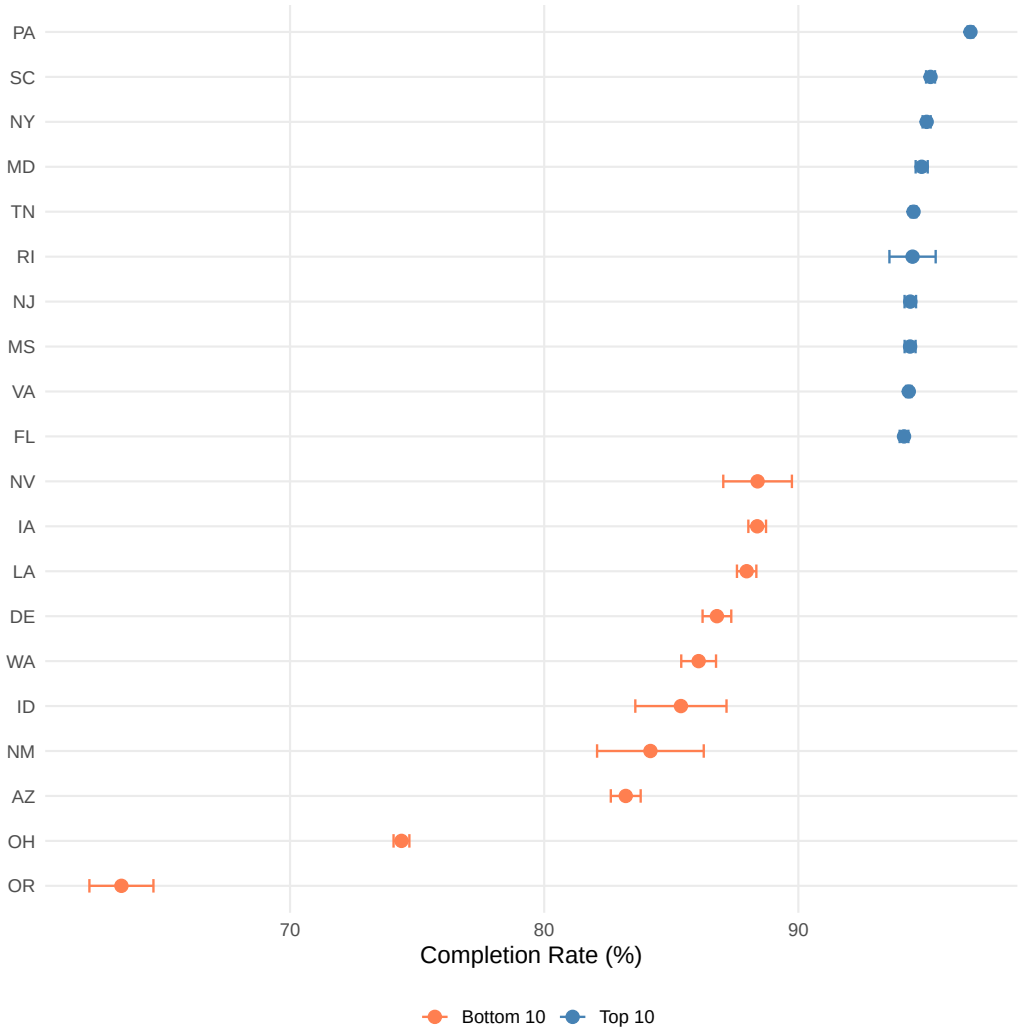


Figure 17: Completion Rate by State (Top and Bottom 10)

Notes: States with $\geq$ 1,000 observations. Public school sample.