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On the Complementarity of K-12 Personal Finance and Economic Education: Evidence from Online Modules*

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September 23, 2026

Abstract

We compile data from 2.4 million economics and personal finance module assignments completed by 513,000 K-12 students on an educational online platform. We exploit variation in the order in which these students complete the modules –economics or personal finance first– to estimate directional spillovers. The spillover runs one way. Controlling for school demographics, students who take economics first score 3.42 points higher on personal finance post-tests; taking personal finance first raises economics post-tests by 0.15 points on the same specification. A placebo using exposure that occurs after the outcome puts the return at 0.8 to 1.4 points per pre-treatment module. The advantage does not shrink as the gap between subjects grows, and it is largest in raw points for lower-performing students, 5.1 compared with 1.6, though mostly because higher-performing students sit at the test ceiling. Economics appears to supply foundational concepts that personal finance does not. When both subjects are taught, the order matters: economics first is the better order, and one or two economics modules capture most of the available gain.

JEL Codes: G53 A21, I21

Keywords: Economic education, Financial literacy, K-12 education, Online learning, Curriculum design

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Conflict of Interest Statement. The authors declare no conflicts of interest.

Data Availability Statement. The data that support the findings of this study are available from the corresponding author upon reasonable request. The data are administrative records from a platform operated by the Federal Reserve Bank of St. Louis and are subject to institutional data governance procedures.

Ethics Statement. This study uses de-identified administrative records collected in the ordinary course of platform operation. No human subjects research protocols were required. The data contain no personally identifiable information.

AI Use Declaration. During the preparation of this work the authors used Claude (Anthropic) to assist with data processing scripts, organize empirical output, and draft and revise portions of the manuscript. The authors reviewed and edited all content and take full responsibility for the accuracy and integrity of the published article.

1 Introduction

The number of U.S. states requiring personal finance education for high school graduation has more than doubled over the past decade, rising from 17 states in 2016 to 41 states in 2026. Over the same period, economics requirements grew more modestly, from 20 to 25 states. This rapid expansion of personal finance mandates has raised concerns about crowding out: if instructional time is fixed, will requiring personal finance reduce exposure to economics? Or do these subjects reinforce each other, such that students who learn one are better prepared to learn the other?

In this paper, we study the potential complementarity between K-12 economics and personal finance education. We exploit a unique dataset from Econ Lowdown, a free online learning platform operated by the Federal Reserve Bank of St. Louis from 2014 to 2025. The platform offers 64 interactive modules (36 in economics, 28 in personal finance) that teachers assign to students. Each module includes a pre-test, instructional content, and a post-test, allowing us to measure both baseline knowledge and learning. Our data comprise 2.4 million module assignments completed by 513,000 K-12 students from 2014 to 2025. The setting offers three advantages for studying cross-subject relationships. First, content is standardized across classrooms, eliminating variation in curriculum quality. Second, teachers rather than students choose which modules are assigned, which limits student self-selection into content. Third, the panel structure allows us to observe the same student completing modules in both subjects over time.

We develop our argument in five steps. First, we characterize the overlap between economics and personal finance enrollment. Of the 513,000 K-12 students in our data, 36.7% completed only economics modules, 31.6% completed only personal finance modules, and 31.7% completed modules in both subjects. Teachers are heavily sorted by subject: 78% assign at least 80% of their modules in a single subject.

Second, we examine sequencing among the 162,612 students who took both subjects. Among the 147,876 whose sequencing status can be determined, personal finance comes first

for 53%, economics comes first for 36%, and 11% started both on the same day. The median time gap between a student's first module in each subject is 25 days, suggesting that teachers who assign both subjects tend to integrate them within a single course or unit.

Third, we estimate cross-subject performance relationships. The correlation between economics and personal finance post-test scores is 0.47. In regressions controlling for pre-test scores in both subjects, a 1-point increase in economics post-test scores is associated with a 0.37-point increase in personal finance post-test scores. The relationship is bidirectional, though somewhat stronger in the reverse direction: a 1-point increase in personal finance post-test scores predicts a 0.47-point increase in economics post-test scores. Unconditional on pre-test scores, learning gains are also positively correlated across subjects (0.32 and 0.34 in the two directions). These patterns indicate that students who excel in one subject tend to excel in the other, and students who learn more in one subject also learn more in the other, beyond what shared baseline ability would predict.

Fourth, we exploit sequencing variation to estimate directional spillovers. Among students who take both subjects, some encounter economics first while others encounter personal finance first. This variation allows us to test whether prior economics exposure improves personal finance outcomes. We find a large asymmetry. Holding the specification fixed, economics-first raises personal finance post-test scores by 3.59 points while personal-finance-first raises economics post-test scores by 0.26 points, a ratio of nearly 14 to 1. Adding school demographic controls leaves the forward effect at 3.42 points and reduces the reverse effect to 0.15 points, no longer distinguishable from zero ($p = 0.22$), so the asymmetry becomes complete rather than merely large.

Fifth, we examine dose-response relationships and timing. Counting only economics modules completed before the student's first personal finance module, each additional pre-treatment economics module is associated with a 1.4-point improvement in personal finance post-test scores, with sharply diminishing returns after the first few modules. A placebo using exposure that occurs after the outcome bounds the share of this association attributable to

selection, leaving a causal range of 0.8 to 1.4 points per module. Surprisingly, we find no evidence that spillovers decay over time. Students who complete economics months before personal finance show larger benefits than those who complete both in quick succession, suggesting that economic concepts become ingrained and become more useful over time.

Our findings have direct implications for the ongoing policy debate. As states consider adding personal finance requirements, some have worried about displacing economics from an already crowded curriculum. Our evidence suggests that economics and personal finance are complements: students who learn one are better prepared to learn the other. The complementarity we document implies that requiring both subjects may be more effective than requiring either alone.

Related literature. This paper contributes to three strands of research. First, we contribute to the literature on K-12 economics education. Buckles and Freeman (1984) and Martin and Bender (1985) provide early evidence that students learn economics better in dedicated courses. Walstad (2015) reviews the evolution of economics in the school curriculum. Research on online economics education has focused primarily on undergraduate settings (Bettinger, Fox, Loeb, & Taylor, 2017; Expósito, Sánchez-Rivas, Gómez-Calero, & Pablo-Romero, 2020; Hill & LoPalo, 2024; Ipinnaiye & Risquez, 2024), with limited attention to K-12 (VanFossen & Herman-Ellison, 2006).

Second, we contribute to the growing literature on financial literacy education. Lusardi and Mitchell (2011) document that financial illiteracy is widespread in both well-developed and rapidly changing markets, that it is most pronounced among women and among the young and the old, and that the financially literate are more likely to plan for retirement. Kaiser, Lusardi, Menkhoff, and Urban (2022) synthesize the experimental evidence and find that financial education has economically meaningful effects on both financial knowledge and downstream financial behavior; we return to their findings in Section 4 to calibrate the size of our own estimates. Totenhagen et al. (2015) reviews delivery methods for youth financial literacy. Batty, Collins, O'Rourke, and Odders-White (2020) evaluates an elementary school

financial education program. Urban, Schmeiser, Collins, and Brown (2020) studies the effects of state personal finance mandates on adult financial behavior. Oberrauch, Kaiser, and Lusardi (2024) documents the importance of early financial education in shaping long-run outcomes.

Third, we speak to the policy debate on curriculum mandates. Luedtke and Urban (2023) documents heterogeneity in how states implement personal finance requirements. Suiter, Wolla, and Levin (2022) argues that economics and personal finance are complements rather than substitutes. We provide the first large-scale empirical evidence on this question.

The paper proceeds as follows. Section 2 describes the institutional setting and data. Section 3 presents the empirical framework and evidence. Section 4 discusses implications and limitations. Section 5 concludes.

2 Data and Setting

2.1 The Econ Lowdown Platform

Econ Lowdown is a free online learning platform designed and operated by the Federal Reserve Bank of St. Louis (Méndez-Carbajo & Malakar, 2020). It operated from 2010 until late 2025, when it was replaced with a successor portal. The platform provides interactive modules covering economics and personal finance for a broad range of users: traditional K-12 classrooms, colleges and universities, homeschool families, non-U.S. institutions, and other organizations. The platform follows a teacher-mediated model: Teachers register, create virtual classrooms, and assign specific modules to students. Students complete assigned modules asynchronously and receive immediate feedback.

Each module follows a standardized structure. Students begin with a pre-test assessing baseline knowledge, typically 5 to 10 multiple-choice questions. They then progress through content pages combining text, graphics, and embedded videos. Some modules include knowledge checks (formative assessments) within the content. Students conclude with a post-test

measuring learning outcomes.

The platform offers 64 modules. The 36 economics modules cover topics including supply and demand, market equilibrium, unemployment, inflation, monetary policy, trade, and economic systems. The 28 personal finance modules address paychecks, budgeting, saving, credit, insurance, and financial decision-making. Module length varies from 2 to 67 pages, with a median of 11 pages. Economics modules are substantially longer than personal finance modules (median 16 versus 7 pages).

2.2 Sample and Key Variables

Our analysis uses the universe of K-12 module assignments from November 2014 through November 2025, the platform's final month of operation. The data comprise 2,388,572 student-module observations from 512,631 unique students. Table 1 presents summary statistics by subject.

Table 1: Summary Statistics by Subject

	Observations	Students	Modules	Completion (%)	Avg Pre-test	Avg Post-test	Avg Gain
Economics	1,013,613	350,890	36	91.5	50.6	78.5	27.3
Personal Finance	1,374,959	324,353	28	93.3	57.6	79.9	21.9
Total	2,388,572	512,631	64	92.6	54.6	79.3	24.2

Notes: Learning gain is defined as post-test score minus pre-test score. Scores are on a 0–100 scale. The total unique students (512,631) is less than the sum of economics and personal finance students because 162,612 students completed modules in both subjects.

Personal finance modules account for 58% of total assignments, while economics modules account for 42%. The overall completion rate is 93%, with slightly higher completion in personal finance (93.3%) than economics (91.5%). Average pre-test scores are higher in personal finance (57.6) than economics (50.6), suggesting students enter with more baseline knowledge of personal finance topics. Average learning gains are larger in economics (27.3 points) than personal finance (21.9 points), indicating greater scope for learning when baseline knowledge is lower.

A key feature of our data is the panel structure: We observe the same student complet-

ing multiple modules over time. This allows us to study cross-subject relationships within students. Figure 1 shows the distribution of students across subject categories, and the sequencing among those who took both.

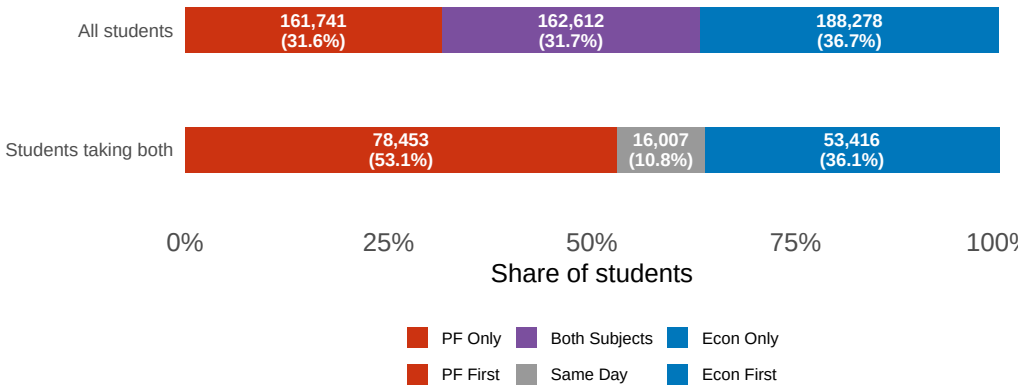


Figure 1: Student Enrollment and Sequencing

Notes: Top bar: distribution of 512,631 K-12 students by subject group. Bottom bar: sequencing among the 147,876 students whose sequencing status can be determined (see Table 2). "Same day" indicates students whose first economics and first personal finance module occurred on the same calendar date.

Of the 512,631 students, 188,278 (36.7%) completed only economics modules, 161,741 (31.6%) completed only personal finance modules, and 162,612 (31.7%) completed modules in both subjects. The 32% who took both subjects form the primary sample for our cross-subject analyses.

We further refine this sample to address questions related to student performance across economics and personal finance subjects and to the sequence in which the modules are completed. Table 2 traces the construction of the two analysis samples we use throughout the paper, in the order in which the paper references them.

Panel A builds the *cross-subject performance sample*. Of the 162,612 K-12 students who completed modules in both subjects, 147,354 have recorded and valid pre- and post-test scores in both. This sample imposes a score requirement but not an ordering requirement,

since a correlation between contemporaneous scores does not depend on which subject came first. It is the sample for the correlations in Section 3.

Panel B builds the *sequencing sample*, the subset of the performance sample for which an order can be assigned. Of the 162,612 students who took both subjects, 147,876 have a determinable sequencing status, meaning we can tell whether economics came first, personal finance came first, or both began on the same day; the remaining 14,736 lack a usable pre-test date in at least one subject. Excluding the 16,007 who started both on the same day leaves 131,869 with a clear order, and requiring valid pre- and post-test scores in both subjects leaves 119,790. This is the sample for every sequencing, dose-response, and timing estimate. Equivalently, of the 147,354 students in Panel A, 119,790 have a clear order, 14,275 started both subjects on the same day, and 13,289 cannot be ordered.

Table 2: Sample Construction

	Students
Panel A: Cross-subject performance sample	
K-12 students	512,631
Completed modules in both subjects	162,612
with valid pre- and post-tests in both subjects	147,354
Panel B: Sequencing sample	
Completed modules in both subjects	162,612
with a determinable sequencing status	147,876
excluding same-day starts	131,869
with valid pre- and post-tests in both subjects	119,790

Notes: Recorded and valid pre-test and post-test scores in both economics and personal finance. Panel B is a subset of Panel A: Of the 147,354 students in the performance sample, 119,790 have a clear order, 14,275 started both subjects on the same day, and 13,289 cannot be ordered. "Same-day starts" are students whose first economics and first personal finance module share a calendar date, for whom no order can be assigned; there are 16,007 in total, of whom 14,275 have valid scores. Panel B applies the ordering restriction before the score restriction, which is why its intermediate counts of 147,876 and 131,869 do not appear in Panel A.

Where available for a subset of K-12 schools, we merge district-level demographic characteristics from the NCES Common Core of Data, including minority student share, per-pupil

expenditure, enrollment, and student-teacher ratios. Of the 119,790 students in the sequencing sample, 100,896 are matched to NCES demographic data. Because the Common Core of Data covers public school districts, this matched subsample contains no private schools. We use it to assess the robustness of our sequencing estimates to the inclusion of demographic controls and school fixed effects (Section 4).

2.3 School and Teacher Patterns

The 32% student overlap rate masks substantial heterogeneity across schools and teachers. Our data include 3,260 schools with NCES identifiers. Of these, 1,303 (40.0%) assigned modules in both subjects, 1,157 (35.5%) assigned only economics, and 800 (24.5%) assigned only personal finance. Schools offering both subjects engage more intensively with the platform: They average 318 students completing modules and 1,459 module assignments, compared with 79 students and 124 assignments for economics-only schools.

Teacher sorting by subject is even more pronounced. We observe 7,973 unique teachers. Figure 2 shows the distribution of teachers by the share of their assignments in economics.

The distribution is bimodal, with peaks at the extremes. We classify teachers based on assignment shares: 3,813 (47.8%) assign primarily personal finance modules (at least 80% of their assignments), 2,422 (30.4%) assign primarily economics modules, and only 1,738 (21.8%) assign a mixed portfolio. This sorting likely reflects within-school division of labor, where economics and personal finance are taught by different instructors under different curricula.

2.4 Sequencing Patterns

Among the 147,876 students whose sequencing status can be determined (Table 2), personal finance comes first for 78,453 (53%), economics comes first for 53,416 (36%), and both subjects were started on the same day for 16,007 (11%). The predominance of personal finance as the entry point likely reflects curricular decisions: Personal finance topics such as

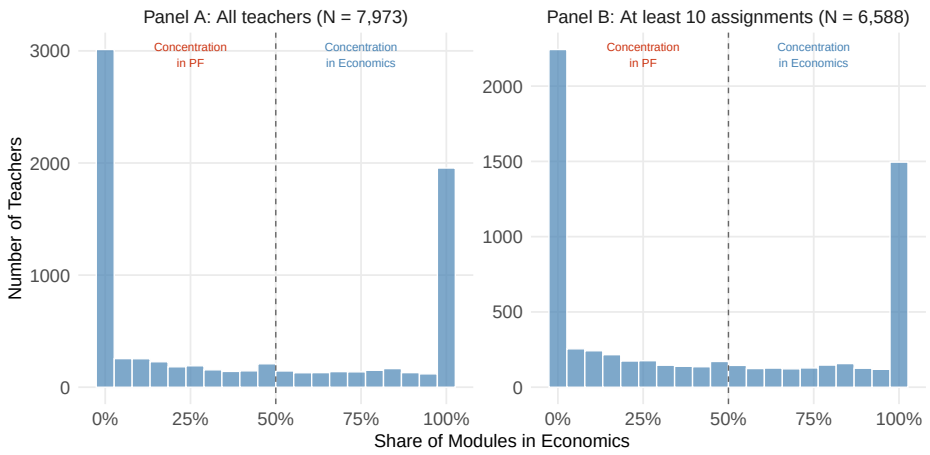


Figure 2: Teacher Sorting by Subject

Notes: Distribution of teachers by share of module assignments in economics. Panel A includes all 7,973 teachers; Panel B restricts to 6,588 teachers with at least 10 module assignments. The bimodal pattern persists after excluding low-volume teachers, indicating genuine sorting rather than small-sample artifacts. The dashed line at 50% indicates equal assignment of both subjects.

paychecks and budgeting are concrete and immediately relevant to students' lives, making them natural starting points before more abstract economics concepts.

Figure 3 shows the distribution of time gaps. Among students who did not start both subjects on the same day, the median gap is 25 days and the mean is 36 days. The short median gap suggests that when teachers assign both subjects, they integrate them within the same course or semester rather than treating them as separate curricula. The right tail reflects cases where students encounter the second subject in a subsequent school year.

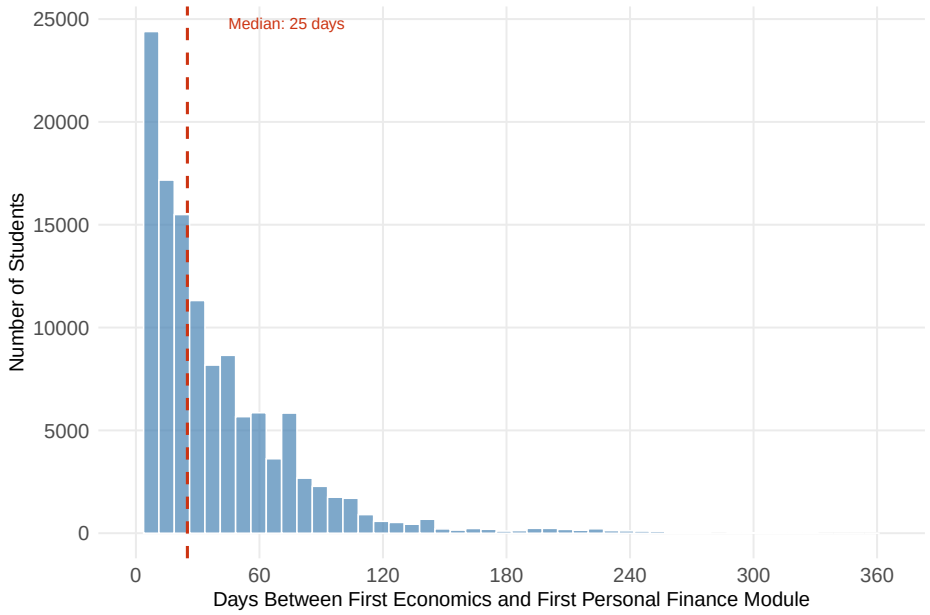


Figure 3: Time Gap Between First Economics and First Personal Finance Module

Notes: Distribution of days between first economics module and first personal finance module for students who completed both. Excludes students who started both on the same day. Histogram truncated at 365 days; 4 observations exceed this threshold. Median = 25 days.

3 Empirical Evidence

Our empirical strategy proceeds in two stages. We first document cross-subject correlations in performance and completion, establishing the basic patterns of complementarity. We then exploit sequencing variation to identify spillovers between subjects.

3.1 Cross-Subject Performance

We now turn to the central question: Does performance in one subject predict performance in the other? We use the cross-subject performance sample of 147,354 K-12 students (Table 2, Panel A), which requires valid pre- and post-test scores in both subjects but does not require an identifiable order, since a correlation between contemporaneous scores does not depend on sequencing. Our baseline specification is

$$Y_{i,PF} = \alpha + \beta Y_{i,Econ} + \gamma X_{i,PF} + \delta X_{i,Econ} + \varepsilon_i \tag{1}$$

where $Y_{i,PF}$ is student i's average post-test score in personal finance, $Y_{i,Econ}$ is the corresponding economics score, and $X_{i,PF}$ and $X_{i,Econ}$ are pre-test scores measuring baseline knowledge. The coefficient β captures the association between economics and personal finance performance, conditional on prior knowledge in both subjects. We estimate symmetric specifications with the economics post-test as the outcome and also examine learning gains (post-test minus pre-test) to focus on knowledge acquired during the module rather than initial ability.

Figure 4 presents a residualized binned scatter plot: Both economics and personal finance post-test scores are first regressed on pre-test scores in both subjects, and the figure plots the relationship between the residuals. This partials-out shared baseline ability, isolating the conditional association between performance in the two subjects.

The partial correlation is 0.42. Controlling for baseline knowledge in both subjects, a 1-point improvement in economics performance is associated with a 0.37-point improvement

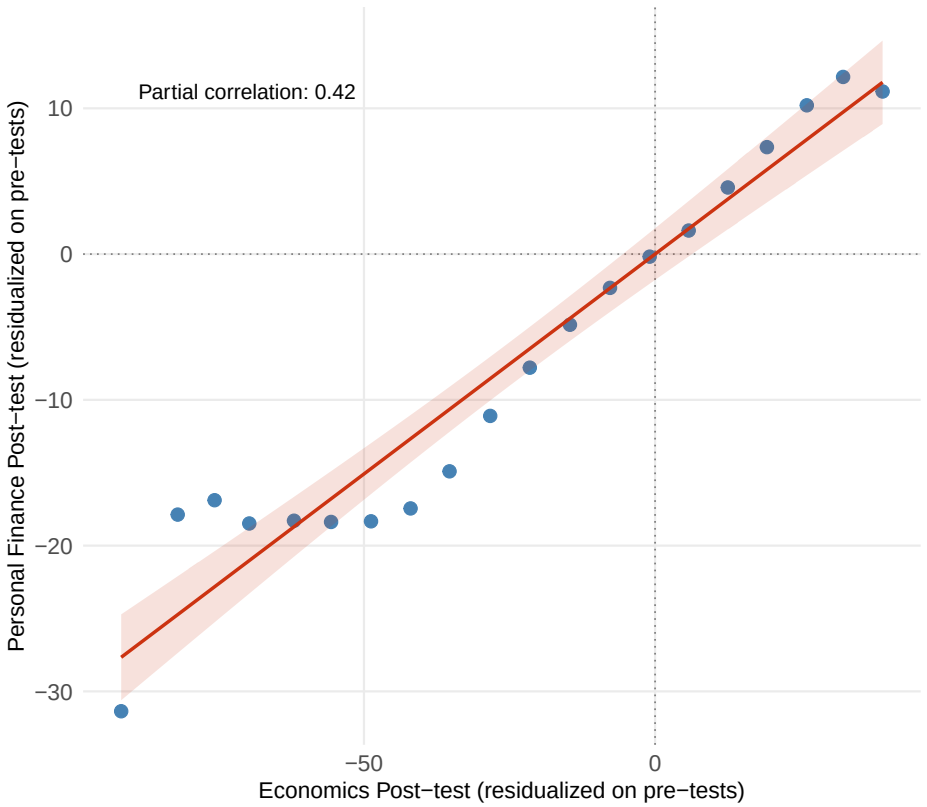


Figure 4: Cross-Subject Performance: Post-test Scores (Conditional on Pre-tests)

Notes: Binned scatter plot of residualized post-test scores for 147,354 K-12 students who completed modules in both subjects. Both economics and personal finance post-tests are regressed on pre-test scores in both subjects; the plot shows residuals. Each point represents the mean of a ventile. Line shows linear fit. Partial correlation = 0.42.

in personal finance performance. The symmetric coefficient is 0.47: Personal finance performance is a somewhat stronger predictor of economics performance than vice versa, possibly reflecting the sequencing patterns documented above. The magnitudes are economically meaningful: Two students with identical pre-test scores who differ by 10 points on the economics post-test (about half a standard deviation) are predicted to differ by 3.7 points on the personal finance post-test. Wherever we express an effect in standard-deviation units, the reference is the student-level distribution of average post-test scores in the sequencing sample, whose standard deviation is 18.9 points for personal finance and 20.3 points for economics.

3.2 Learning Gains

The post-test correlations establish that students who excel in one subject tend to excel in the other. High-ability students may perform well everywhere, so a natural next step asks whether students who *learn more* in one subject also learn more in the other, defining learning gain as post-test minus pre-test score.

Here one must be careful, because the obvious specification is not a new test. Conditional on both pre-test scores, regressing the personal finance gain on the economics gain is algebraically the same regression as equation (1). The gain differs from the post-test by the pre-test, which is already a control, so the two designs span the same space and the slope is identical. The estimate is 0.370 against 0.371 for post-test levels, and residualizing on both pre-tests maps the gains onto the post-tests exactly, which is why the binned scatter would reproduce the same partial correlation of 0.42. Differencing adds no information once both pre-tests are conditioned on, and we therefore report the conditional relationship once rather than twice.

What is genuinely distinct is the gain-gain relationship *without* pre-test controls. A 1-point increase in the economics learning gain is associated with a 0.32-point increase in the personal finance learning gain, and the reverse coefficient is 0.34. These are unadjusted for

baseline ability, so we treat them as descriptive. They confirm that the co-movement in gains is not an artifact of the conditioning we impose elsewhere, but they cannot separate knowledge transfer from shared ability.

The substantive conclusion is unchanged from Section 3: Students who learn more in one subject also learn more in the other. The point is that this is one result rather than two and that the sequencing analysis below, not the differencing of levels, is what separates transfer from shared ability.

3.3 Cross-Subject Dropout

Complementarity may extend beyond performance to engagement itself. A student who struggles to complete economics modules may face the same challenges in personal finance, whether due to time constraints, disengagement, or topic difficulty. We test this by computing completion rates separately for each subject among students who took both.

The correlation between economics and personal finance completion rates is 0.29. Table 3 shows the joint distribution. Among students who completed all their economics modules, only 9.2% dropped out of at least one personal finance module. Among students who dropped out of at least one economics module, 31.1% also dropped out of personal finance, more than three times the rate. A chi-squared test decisively rejects independence ($p < 0.001$). In linear probability models, dropping out of economics is associated with a 22-percentage-point increase in the probability of dropping out of personal finance; the symmetric estimate is 28 percentage points. The factors driving non-completion operate similarly across both subjects.

3.4 Sequencing: Does Order Matter?

The correlations documented above are consistent with complementarity but cannot establish causality. We now exploit sequencing variation to test whether completing economics first is associated with better personal finance outcomes. Under the assumption that sequencing

Table 3: Cross-Subject Dropout

	Personal Finance	
Economics	Completed All	Dropped Out
Completed All	122,499 (75.3%)	12,372 (7.6%)
Dropped Out	19,129 (11.8%)	8,612 (5.3%)

Notes: Cross-tabulation for 162,612 K-12 students who completed modules in both subjects. "Dropped Out" indicates failure to complete at least one assigned module. Chi-squared test of independence: $p < 0.001$.

is quasi-random conditional on observables, comparing outcomes for economics-first versus personal-finance-first students identifies the effect of prior subject exposure. Our specification is

$$Y_{i,PF} = \alpha + \beta \cdot \mathbf{1}[\text{Econ First}]_i + \gamma X_{i,PF} + \delta X_{i,Econ} + \varepsilon_i \tag{2}$$

where $\mathbf{1}[\text{Econ First}]_i$ equals 1 if student i completed an economics module before any personal finance module, and $X_{i,PF}$ and $X_{i,Econ}$ are pre-test scores in each subject. The coefficient β identifies the effect of having completed economics before taking personal finance, controlling for baseline ability in both subjects. We deliberately exclude module counts from the baseline specification, since the number of modules a student completes is partly an outcome of the sequence rather than a pre-determined characteristic. Appendix A reports the specification with module counts added, which is the natural response to the imbalance documented in the balance table, and shows that it raises the estimate.

This identification strategy has limitations. Sequencing may not be random: Motivated students might start with personal finance (more immediately relevant) or economics (more foundational), depending on their goals. However, in our setting, sequencing is largely a classroom-level curricular decision rather than a student-level choice and this is testable. Grouping the sequencing sample into teacher-year cells with at least two students, every

student follows the same order in 59% of cells, covering 43% of students. Where ordering varies within a teacher-year it is a minority phenomenon; where it does not, the order is a property of the class, not of the student.

In the sequencing sample of 119,790 students (Table 2, Panel B), 47,703 (40%) completed economics first and 72,087 (60%) completed personal finance first. Table 4 presents estimates of the sequencing effect across an identification ladder, adding controls progressively to assess robustness.

Table 4: Sequencing Effects: Does Order Matter?

	PF Post			PF Post (FE)	Econ Post			Econ Post (FE)
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Constant	80.59***	59.65***	25.55***		77.74***	57.93***	39.44***	
	(0.0694)	(0.1907)	(2.090)		(0.0906)	(0.2067)	(2.164)	
Economics First	3.372***	3.591***	3.417***	2.565***				
	(0.1116)	(0.1033)	(0.1136)	(0.4387)				
PF Pre-test		0.2705***	0.2676***	0.2496***		0.0663***	0.0619***	0.0536***
		(0.0027)	(0.0030)	(0.0096)		(0.0027)	(0.0030)	(0.0059)
Econ Pre-test		0.0903***	0.0887***	0.0789***		0.3089***	0.3130***	0.2689***
		(0.0027)	(0.0030)	(0.0063)		(0.0030)	(0.0033)	(0.0100)
Private School		0.5041**				0.9393***		
		(0.1840)				(0.2034)		
Minority Share			1.448***				4.201***	
			(0.2100)				(0.2322)	
Expenditure per Pupil (log)			3.533***				1.721***	
			(0.2183)				(0.2248)	
PF First					-0.1755	0.2636*	0.1491	1.138*
					(0.1188)	(0.1109)	(0.1221)	(0.5596)
Fixed-effects								
nces_id				Yes				Yes
Fit statistics								
Observations	119,790	119,790	100,896	111,775	119,790	119,790	100,896	111,775
R^2	0.00764	0.15685	0.15619	0.24543	1.79×10^{-5}	0.12470	0.12704	0.25393

Notes: OLS estimates. Sample includes K-12 students who completed modules in both subjects with clear sequencing (economics first or personal finance first, excluding same-day starts). Pre-test controls include economics and personal finance pre-test scores. Private indicator equals one for schools with non-numeric NCES identifiers. School controls include minority student share and log per-pupil expenditure, available for 100,896 students matched to NCES demographic data. Column (4) and (8) include school fixed effects with standard errors clustered at the school level. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The results reveal a striking asymmetry. Students who complete economics first score

3.59 points higher on personal finance post-tests, controlling for pre-test scores and a private school indicator (column 2). Adding school demographic controls (minority share and log per-pupil expenditure) leaves the estimate essentially unchanged at 3.42 points (column 3). With school fixed effects, which absorb all time-invariant school characteristics, the estimate is 2.57 points (column 4). The estimate declines by 28% across this ladder and remains large and significant throughout. We do not describe it as stable: The appendix uses precisely this movement as the benchmark for observable selection when computing Oster bounds. In contrast, students who complete personal finance first score only 0.26 points higher on economics post-tests in the same specification (column 6), a ratio of nearly 14 to 1. Adding school demographic controls leaves the reverse effect at 0.15 points, statistically indistinguishable from zero (column 7, $p = 0.22$); so, under our preferred specification, the reverse spillover cannot be distinguished from zero at all. With school fixed effects, the reverse estimate grows to 1.14 (column 8), suggesting the raw estimate reflects negative confounding by school characteristics.

This asymmetry suggests that economics provides foundational concepts that facilitate personal finance learning, but personal finance provides fewer building blocks for economics. Understanding opportunity cost, compound interest, and market mechanisms from economics may help students grasp budgeting, saving, and investment decisions in personal finance. The reverse transfer appears weaker: Knowing how to budget or manage credit cards provides less scaffolding for understanding supply and demand or macroeconomic policy. We use column (3) as our preferred specification throughout, as it controls for observable student and school characteristics. Because the demographic controls come from the NCES Common Core of Data, which covers public school districts, this specification is estimated on the 100,896 students in public schools matched to those records rather than on the full sample of 119,790.

3.5 Dose-Response: How Many Modules Matter?

If economics knowledge helps personal finance learning, does more exposure produce larger benefits? The sequencing results establish that completing economics first raises personal finance outcomes, but they are silent on intensity: A student who completes one economics module and a student who completes ten both count as "economics first." We now ask whether the margin matters.

Identifying a dose-response requires care. Simply regressing personal finance post-test scores on the total number of economics modules completed, pooling both sequencing groups, would conflate the dose with reverse causation: Among students who took personal finance first, every economics module was completed *after* the personal finance post-test and cannot have caused it. We therefore estimate the dose-response separately for each direction, restricting to first-movers. For the economics-to-personal-finance direction, we estimate

$$Y_{i,PF} = \alpha + \beta \tilde{N}_{i,Econ} + \gamma \tilde{N}^2_{i,Econ} + \delta X_{i,PF} + \delta' X_{i,Econ} + \theta N_{i,PF} + \varepsilon_i \tag{3}$$

on the 50,460 students who completed economics before personal finance, where $\tilde{N}_{i,Econ}$ denotes economics modules completed strictly before the student's first personal finance pre-test date. This count exceeds the 47,703 economics-first students in the sequencing sample because the dose-response outcome is the personal finance post-test alone, so a valid economics post-test is not required. The timing analysis in Section 3 uses the same wider sample for the same reason. The quadratic term allows for diminishing returns. The symmetric specification uses $\tilde{N}_{i,PF}$ as the treatment and $Y_{i,Econ}$ as the outcome, estimated on the 73,574 students who completed personal finance first. Adding school demographic controls restricts the sample to 43,576 (econ-first) and 60,981 (PF-first) students matched to NCES records. Table 5 presents estimates for both directions.

The dose-response is front-loaded. Column (2) shows an average return of 1.41 points per pre-treatment economics module controlling for pre-test scores and school demographics.

Table 5: Dose-Response: More Modules, Better Outcomes?

	PF Post				Econ Post			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Constant	80.61***	25.70***	25.90***	25.95***	77.41***	48.11***	37.15***	36.31***
	(0.1326)	(3.093)	(3.116)	(3.111)	(0.0949)	(3.125)	(3.122)	(3.122)
N Econ Modules (pre-PF)	1.309***	1.407***	1.409***	2.586***				
	(0.0419)	(0.0436)	(0.0437)	(0.1003)				
PF Pre-test		0.2251***	0.2248***	0.2256***		0.0945***	0.0959***	0.0958***
		(0.0040)	(0.0041)	(0.0041)		(0.0044)	(0.0044)	(0.0044)
Econ Pre-test		0.1056***	0.1056***	0.1034***		0.2988***	0.2929***	0.2940***
		(0.0049)	(0.0049)	(0.0049)		(0.0042)	(0.0042)	(0.0042)
Minority Share		1.865***	1.856***	1.680***		5.581***	5.724***	5.634***
		(0.3477)	(0.3476)	(0.3474)		(0.3008)	(0.2953)	(0.2958)
Expenditure per Pupil (log)		3.661***	3.648***	3.504***		0.5325	1.320***	1.349***
		(0.3185)	(0.3196)	(0.3193)		(0.3269)	(0.3255)	(0.3254)
N PF Modules (total)			-0.0198	-0.0073				
			(0.0229)	(0.0230)				
N Econ Modules (pre-PF)^2				-0.1338***				
				(0.0097)				
N PF Modules (pre-Econ)					-0.0046	0.3096***	0.1326***	0.4654***
					(0.0245)	(0.0247)	(0.0257)	(0.0617)
N Econ Modules (total)							0.9548***	0.9670***
							(0.0219)	(0.0218)
N PF Modules (pre-Econ)^2								-0.0181***
								(0.0030)
Fit statistics								
Observations	50,460	43,576	43,576	43,576	73,574	60,981	60,981	60,981
R^2	0.01691	0.15470	0.15471	0.15807	4.61×10^{-7}	0.12624	0.14965	0.15022

Heteroskedasticity-robust standard-errors in parentheses

Notes: OLS estimates. Columns (1)–(4): students who completed economics before personal finance ($N = 50{,}460$ in col. 1; $N = 43{,}576$ in cols. 2–4 due to NCES matching). Columns (5)–(8): students who completed personal finance before economics ($N = 73{,}574$ in col. 5; $N = 60{,}981$ in cols. 6–8). The dosage variables count only modules completed strictly before the student's first module in the other subject, verified using pretest dates. Pre-test controls include economics and personal finance pre-test scores. School controls include minority student share and log per-pupil expenditure. Heteroskedasticity-robust standard errors in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Adding a control for total PF modules (column 3) leaves the estimate unchanged at 1.41 points. The quadratic specification (column 4) reveals that the marginal benefit declines steeply with each successive module: The first economics module raises personal finance post-test scores by 2.5 points, the second by 2.2 points, the third by 1.9 points, and the fifth by 1.4 points. Returns approach zero beyond the tenth module. Cumulatively, a student who completes five economics modules before starting personal finance scores 9.6 points higher than an otherwise identical student who completed none. To calibrate the magnitude: 9.6 points is approximately 0.5 standard deviations of the personal finance post-test distribution, which moves a student at the median up to the 69th percentile. Two or three modules capture the bulk of this gain: The first two modules alone account for 4.6 of the 9.6 points.

This front-loading has a direct curriculum design implication. Even minimal economics exposure (a single module covering opportunity cost, compound interest, or market mechanisms) generates substantial personal finance benefits. Schools need not require a full-year economics course before personal finance instruction to realize most of the spillover. The point of near-zero marginal returns (around the tenth module) is well beyond what most teachers assign, so the entire practical range of the dose-response is in the region of positive and meaningful effects.

These estimates are larger than those from specifications that count all economics modules regardless of timing, because modules completed after the personal finance post-test cannot have caused it and including them attenuates the estimate toward zero. Restricting to pre-treatment exposure removes that mechanical attenuation, but it does not by itself remove selection: Students who complete more economics modules may simply be more engaged. Appendix A bounds this. A placebo using exposure that occurs strictly after the outcome attributes 44% of the association to selection, so the causal effect lies between roughly 0.8 and 1.41 points per module. We use 1.41 in the calculations above and flag them as upper bounds. As a further validation, measuring dosage in pages of economics content read before the first personal finance module yields a consistent estimate: Each 10 pages

of pre-treatment economics content is associated with 0.50 points on the personal finance post-test, equivalent to approximately 0.9 points per module at the mean economics module length of 18 pages.

The symmetric direction shows a weaker and less stable dose-response. The raw coefficient on pre-treatment personal finance modules for PF-first students is essentially zero (-0.005, column 5) and turns positive once we control for pre-test scores (0.31, column 6). Adding a control for total economics modules (column 7) reduces it to 0.13, and the quadratic specification (column 8) yields a positive linear term that is offset by a negative quadratic. The instability across specifications suggests that the PF-to-economics dose-response largely reflects selection rather than a causal effect, consistent with the asymmetry documented throughout this section.

The dose-response is uniform across economics topics. As we show in Section 4 (Figure 7), each of the four economics topics with sufficient exposure to estimate produces a similar per-module effect (1.0–1.6 points), and no topic share is significant once total pre-treatment modules and teacher fixed effects are included. That joint test is imprecise, so we treat total exposure as the dimension the data identify robustly rather than claiming that composition is irrelevant.

3.6 Timing: Do Spillovers Decay?

A natural question is whether the benefit of economics-first persists over time. If economics knowledge helps personal finance learning through immediate priming, the benefit should decay as the gap between subjects increases. If instead economics provides durable conceptual foundations, the benefit should persist or even grow. We estimate

$$Y_{i,PF} = \alpha + \beta \cdot \mathbf{1}[\text{Econ First}]_i + \lambda \cdot \text{Gap}_i + \phi \cdot \mathbf{1}[\text{Econ First}]_i \times \text{Gap}_i + \gamma X_i + \varepsilon_i \quad (4)$$

where Gap_i is the number of days between the student's first economics module and first personal finance module. A negative interaction coefficient ϕ would indicate that the benefit of economics-first decays as the gap increases.

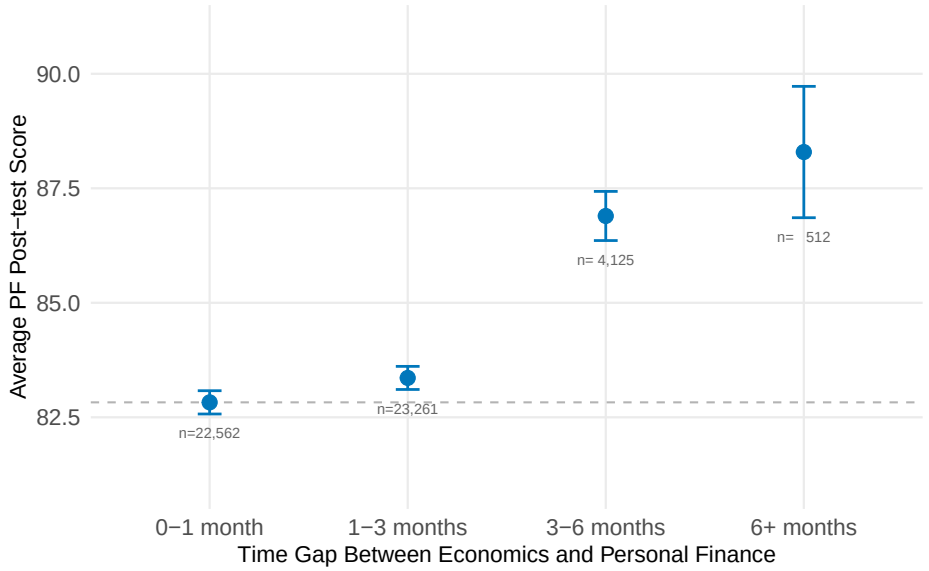


Figure 5: Spillover Persistence: Does the Gap Between Subjects Matter?

Notes: Average personal finance post-test score by time gap between first economics and first personal finance module, for K-12 students who completed economics first. Categories: 0–1 month (n = 22,562), 1–3 months (n = 23,261), 3–6 months (n = 4,125), 6+ months (n = 512). Error bars show 95% confidence intervals.

Figure 5 plots personal finance post-test scores by the time gap for students who completed economics first. Surprisingly, performance increases with the gap. Students who take personal finance within one month of completing at least one economics module average 82.8 on the post-test. Those who wait 1–3 months average 83.4, those who wait 3–6 months average 86.9, and those who wait more than six months average 88.3.

Table 6 presents regression estimates, with column (1) showing the main effects and column (2) adding the interaction between economics-first and the time gap.

The interaction between economics-first and days between subjects is positive (0.028, $p < 0.001$). This indicates that the benefit of completing economics first grows, rather than

Table 6: Timing Effects: Do Spillovers Decay?

Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Constant	79.99***	27.98***	28.91***	66.96***	65.18***
	(0.0815)	(2.086)	(2.078)	(0.2709)	(0.3687)
Economics First	3.299***	2.459***	-1.614***		
	(0.1121)	(0.1587)	(0.3579)		
Days Between Subjects	0.0039**	-0.0070***		0.0246***	
	(0.0014)	(0.0018)		(0.0020)	
PF Pre-test		0.3125***	0.2775***	0.2606***	0.2617***
		(0.0026)	(0.0029)	(0.0034)	(0.0034)
Minority Share		1.432***	1.170***		
		(0.2056)	(0.2049)		
Expenditure per Pupil (log)		3.425***	3.301***		
		(0.2176)	(0.2156)		
Economics First × Days Between Subjects		0.0282***			
		(0.0028)			
Log(Days Between Subjects)			-0.7958***		0.8289***
			(0.0636)		(0.0774)
Econ Pre-test			0.0950***		
			(0.0030)		
Economics First × Log(Days Between Subjects)			1.584***		
			(0.1052)		
Fit statistics					
Observations	126,795	107,272	107,272	50,460	50,460
R^2	0.00734	0.15400	0.16347	0.12492	0.12464

Heteroskedasticity-robust standard-errors in parentheses

Notes: OLS estimates. The timing sample of 126,795 students is wider than the sequencing sample of 119,790 because it requires a clear order, a positive time gap, and valid personal finance pre- and post-test scores, but not valid economics scores, the personal finance post-test being the only outcome used here. Column (1) reports the main effects; column (2) adds the interaction between economics-first and the time gap; column (3) replaces the gap and its interaction with logarithms. Columns (4)–(5) restrict to the 50,460 students who completed economics first and relate the personal finance post-test to the gap alone, in levels and in logs. Pre-test controls include economics and personal finance pre-test scores. School controls include minority student share and log per-pupil expenditure; the smaller samples in columns (2)–(3) reflect NCES matching. Heteroskedasticity-robust standard errors in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

decays, over time. Each additional day of gap increases the economics-first advantage by 0.028 points. Over a 90-day (three-month) gap, the accumulated effect is 2.5 additional points.

This pattern admits three interpretations, and we cannot separate them. Economics knowledge may consolidate, becoming more accessible when students later encounter personal finance. Alternatively, the growing advantage cuts against a priming story: If the effect were driven by recency, it would be strongest when the subjects are taught close together. But the third possibility is selection, and it is the one this variation is least able to exclude. Gap length is not assigned: It is chosen jointly with the order by the school calendar and course structure. Students with long gaps often cross a school year and differ in grade progression, and the longest category contains 512 students, four-tenths of one percent of the sample. We therefore read the timing results as evidence against a purely transient mechanism and not as evidence for durable knowledge transfer specifically.

4 Discussion

Our findings provide strong evidence for complementarity between K-12 economics and personal finance education. The evidence spans multiple levels of analysis: correlations in performance and learning gains, sequencing effects, and dose-response relationships. Figure 6 summarizes the key estimates.

To judge whether these magnitudes matter, it helps to compare them with what financial education itself achieves. Kaiser et al. (2022) synthesize 76 randomized experiments covering more than 160,000 individuals and estimate the average effect of financial education on financial knowledge at 0.20 standard deviations, falling to 0.15 once publication bias is accounted for; the average effect on financial behavior is smaller, between 0.06 and 0.10. Our sequencing estimates correspond to 0.12 to 0.18 standard deviations of the personal finance post-test, taking the within-teacher estimate and our preferred specification as the range.

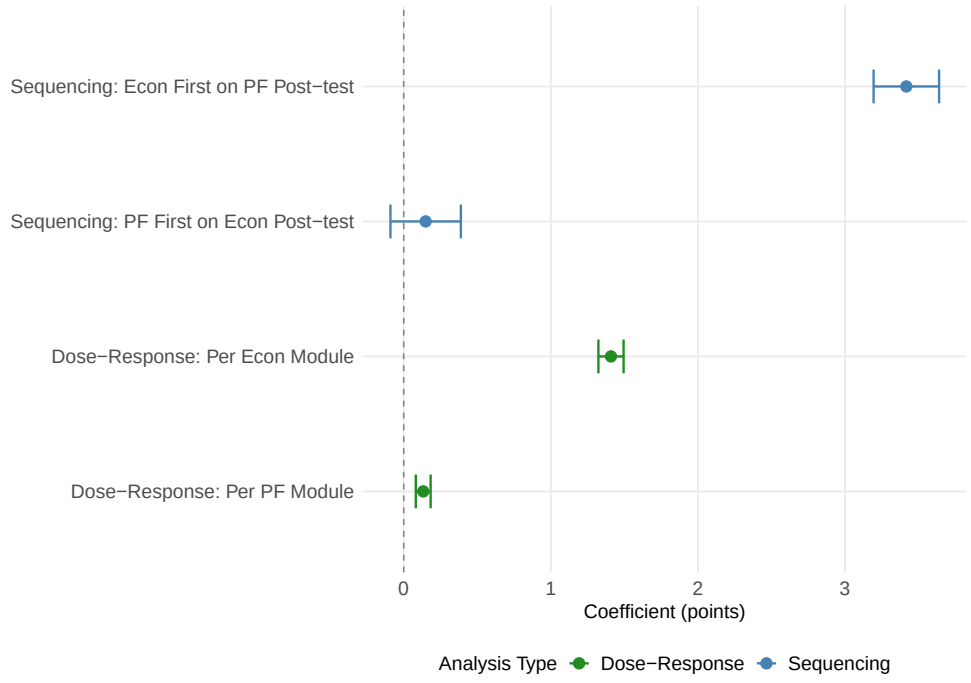


Figure 6: Summary of Sequencing and Spillover Estimates

Notes: Coefficient estimates from specifications controlling for pre-test scores and school demographics. Sequencing: effect of completing economics first (or personal finance first) on the post-test in the other subject. Dose-response: effect per additional pre-treatment module. The figure deliberately omits the cross-subject gain correlations, which are algebraically the post-test correlations of Section 3 and would appear here as a second result rather than the same one. Error bars show 95% confidence intervals.

They are therefore of the same order as the average effect of providing financial education at all. On the scheme Kraft (2020) proposes for education interventions, where effects above 0.20 standard deviations count as large and those between 0.05 and 0.20 as medium, our estimates are medium-sized and theirs sits at the boundary. What distinguishes the two is cost rather than size: Our effect is obtained by reordering instruction that schools already deliver, at no additional instructional time. The comparison is a calibration and not a like-for-like contest, since their counterfactual is the absence of financial education and ours is the same instruction in the opposite order.

4.1 Mechanisms

What explains the complementarity we document? The sequencing results, which condition on pre-test scores in both subjects and survive teacher fixed effects, allow us to move beyond correlations driven by common ability. The striking asymmetry illuminates the underlying mechanism.

Topic-level evidence. To test whether specific economics content drives the spillover, we decompose pre-treatment economics exposure by topic. The platform assigns each module to one of seven economics topics, four of which have at least 2,000 exposed students in our sample. In individual regressions, each of these four produces a significant positive effect on personal finance post-test scores, ranging from 1.0 to 1.6 points per module (Figure 7). This uniformity is itself informative: No single topic dominates. The three remaining topics (Institutions, the Federal Reserve System, and Trade) have too few exposed students to estimate separately, so we cannot rule out that they behave differently. The result holds within our K-12 sample: All 47,703 econ-first students underlying this analysis are restricted to the K-12 population defined in Section 2, so the topic-level pattern is not an artifact of pooling K-12 students with other platform users.

The critical test is whether topic *composition* matters conditional on total dosage. We construct topic share variables (the fraction of pre-treatment economics modules in each

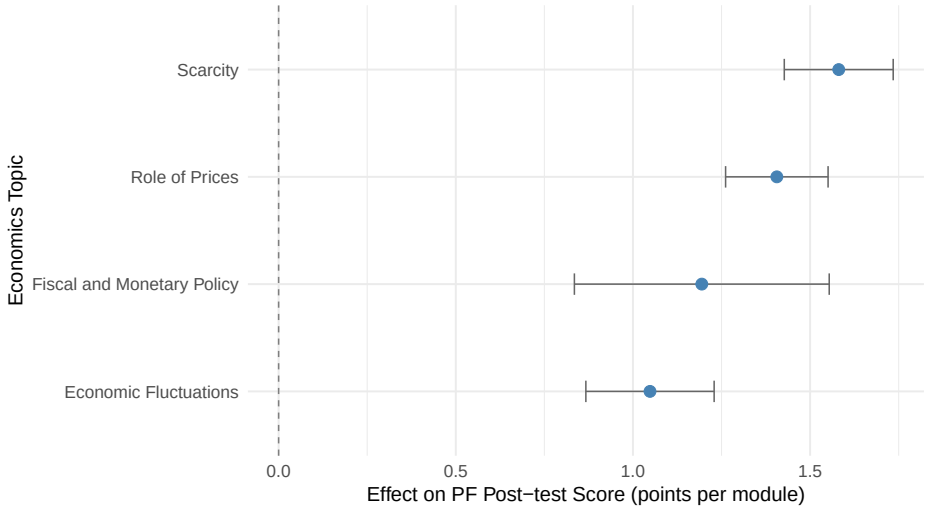


Figure 7: Effect of Economics Modules on Personal Finance Post-test, by Topic

Notes: Each point shows the effect of one additional economics module in that topic on the personal finance post-test score, estimated separately for each of the four topics with at least 2,000 exposed students. All regressions control for pre-test scores. Error bars show 95% confidence intervals. Sample restricted to econ-first students with identifiable pre-treatment topic composition ($N = 47{,}703$). The similarity of the four estimates is what motivates treating total exposure, rather than topic mix, as the dimension the data identify robustly; see the text for the composition tests, which disagree across specifications.

topic) and estimate them first one at a time and then jointly with teacher fixed effects. The two specifications disagree, and we report both.

Entered individually, three of the four shares are statistically significant: A higher share of Scarcity modules raises the personal finance post-test (1.07 points, $p < 0.001$), while higher shares of Economic Fluctuations (-0.77, $p = 0.002$) and Fiscal and Monetary Policy modules (-1.59, $p = 0.0001$) lower it. Entered jointly with teacher fixed effects, no share is significant (smallest $p = 0.16$), while the coefficient on total modules remains large and precise (0.78 points per module, $p < 0.001$).

We do not read the joint specification as establishing that composition is irrelevant. Its standard errors are roughly eight times those of the individual specifications, because the four shares are collinear with each other and with total exposure. So, it is an imprecise null rather than a precise zero. What the data support is weaker and still useful: The per-module effect is similar across topics (1.0 to 1.6 points), total pre-treatment exposure is the one dimension identified robustly, and no composition effect survives the inclusion of teacher fixed effects. Whether the individual share results reflect content or the kinds of teachers who assign each topic, we cannot say.

The contemporaneous correlations add nuance. While sequencing effects are strongly asymmetric, the cross-subject association is more symmetric (0.37 in one direction and 0.47 in the other): Students who do better in either subject also do better in the other. That symmetry likely reflects shared cognitive skills such as quantitative reasoning and reading comprehension, which develop through any content and transfer in both directions, whereas the asymmetry in the sequencing estimates points to economics-specific concepts that personal finance does not supply in return.

The timing results provide additional evidence against alternative mechanisms. If common teacher effects or temporary priming drove the sequencing results, we would expect the economics-first advantage to decay over time. Instead, it grows. This pattern is consistent with durable knowledge transfer rather than transient effects.

4.2 Heterogeneity and Additional Evidence

The causal interpretation of our sequencing results rests on the assumption that, conditional on observables, whether a student takes economics or personal finance first is quasi-random. Appendix A presents balance tests and teacher and school fixed-effects specifications supporting this assumption; the sequencing effect survives all of these checks, including a 2.2-point effect identified from within-teacher variation alone.

Heterogeneity by ability. We examine whether sequencing effects differ by student baseline ability. Figure 8 plots the effect of economics-first separately for students in each tercile of baseline ability (measured by average pre-test scores).

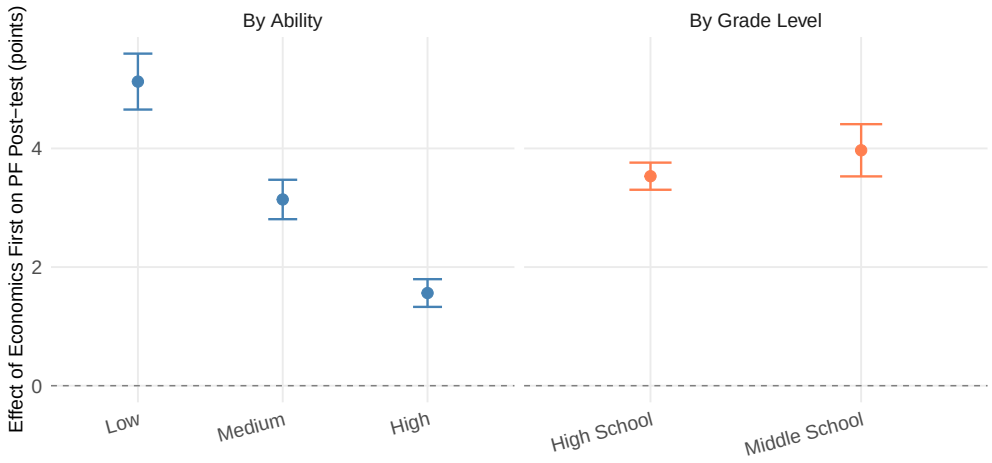


Figure 8: Heterogeneity in Sequencing Effects by Ability and Grade Level

Notes: Effect of completing economics first on personal finance post-test, estimated separately by subgroup. Left panel: ability terciles based on average of economics and personal finance pre-test scores. Right panel: middle school versus high school, assigned by modal grade of completed modules. All regressions control for pre-test scores. Error bars show 95% confidence intervals.

Low-ability students show the largest benefit from economics-first (5.1 points), while high-ability students show the smallest (1.6 points). Two explanations compete, and unlike most such pairs they can be partly adjudicated.

The first is the *scaffolding hypothesis*: Low-ability students lack the foundational con-

cepts (opportunity cost, compound interest) that economics provides. In the language of the transfer literature, this is near transfer between two closely related domains, which Barnett and Ceci (2002) identify as the condition under which transfer is most likely to occur. When they encounter personal finance without this foundation, they struggle to connect budgeting and saving decisions to underlying economic principles. Economics-first provides the scaffolding they need. High-ability students, in contrast, may arrive with these concepts already in place, whether from prior coursework, family environment, or general cognitive ability.

The second is the *ceiling effect hypothesis*: High-ability students score near the ceiling on personal finance post-tests regardless of sequencing, so measured effects are mechanically smaller where there is less room to move. This is testable, and the test favors it. Among high-ability students the mean personal finance post-test is 90.1 and 39.8% score a perfect 100, leaving 9.9 points of headroom on average, against 26.1 points for the low-ability tercile. Expressing each estimate as a share of the headroom available to that group, the effects are 19.6% for low-ability students and 15.8% for high-ability students. The gradient that looks like a factor of 3.3 in raw points is a factor of 1.24 once the measurement constraint is accounted for.

We therefore reverse the reading a large raw gap invites. Most of the ability gradient reflects differential room on the test rather than differential returns to scaffolding. A modest gradient does survive the adjustment, so scaffolding is not ruled out, but it is a residual rather than the main story. Dividing by headroom is a crude normalization and not a decomposition, since it assumes effects scale with available room; it is enough to refute the strong reading and not enough to establish the ceiling as the whole explanation.

The policy implication survives the mechanism question, and separating the two is what makes it defensible. When schools allocate curriculum, they face the raw effects: 5.1 points for a lower-performing student against 1.6 for a higher-performing one. Whether that difference arises from scaffolding or from the ceiling, prioritizing economics-first sequencing for lower-performing students raises measured personal finance outcomes where they are lowest.

Heterogeneity by grade level. We assign each student a grade level based on the modal grade of their completed modules and estimate the sequencing effect separately for middle school and high school students (N = 118,656). Middle school students show a somewhat larger effect (3.97 points, SE = 0.22) than high school students (3.53 points, SE = 0.12). In an interaction model, the middle school × economics-first coefficient is 0.41 points (SE = 0.25, $p = 0.11$). The sequencing effect operates at both levels, with suggestive evidence that younger students benefit slightly more from the scaffolding that economics provides.

Retakes as a behavioral outcome. If economics-first improves personal finance comprehension, it should reduce the probability that students need to retake personal finance modules. We define retakes as instances where a student attempts the same module more than once. Econ-first students are 5.5 percentage points less likely to retake any personal finance module (a 46% reduction relative to the PF-first base rate of 11.9%), controlling for pre-test scores ($p < 0.001$). As a placebo, we examine economics retakes: Econ-first students show a 0.6-percentage-point *increase* in economics retakes, consistent with mechanical exposure (they attempted more economics modules). The economics retake rate is essentially identical across sequencing groups (12.3% for economics-first against 11.9% for personal-finance-first), so the 9-to-1 ratio between the personal finance reduction and the economics increase is consistent with improved comprehension rather than a general change in test-taking behavior. We treat this as suggestive rather than confirmatory. One reason is that economics-first students are assigned fewer personal finance modules, so they have fewer opportunities to retake one. The same mechanical-exposure logic we apply to the economics side applies here, and we do not condition on the number of opportunities.

4.3 Implications for Curriculum Design

The complementarity we document supports integrated approaches to curriculum design. Rather than treating economics and personal finance as competing for limited instructional

time, educators might design curricula that leverage connections between the subjects. For example, a unit on markets could lead naturally into a unit on financial markets and investing. A unit on budgeting could introduce opportunity cost and trade-offs.

The sequencing patterns in our data reveal that nearly half of students who take both subjects encounter personal finance first, likely because personal finance topics (paychecks, budgeting, saving) are concrete and immediately relevant. However, our results indicate that economics-first sequencing produces substantially larger benefits for personal finance learning, suggesting that the current default ordering in many classrooms may not be optimal.

4.4 Implications for State Policy

The rapid growth in personal finance mandates (from 17 to 41 states since 2016) has raised concerns about crowding out economics. The complementarity we document speaks directly to this debate. If economics provides foundational concepts that improve personal finance learning, then displacing economics instruction to make room for personal finance may be counterproductive. States considering new mandates might therefore consider not only whether to require each subject, but how to structure requirements to encourage integrated instruction. Our sequencing results suggest that if schools must choose an order, economics-first produces larger benefits for personal finance learning than the reverse.

Our main analysis is restricted to K-12 students and controls for a private school indicator. A natural concern is whether results are driven by systematic differences across school districts in resources or demographics, which might generate both sorting into sequencing and differences in outcomes. Appendix A shows that the sequencing effect is stable across control sets incorporating district demographics and school and teacher fixed effects, and Oster (2019) bounds on selection on unobservables.

4.5 Limitations

Several limitations warrant discussion.

External validity. Our data come from a single platform, Econ Lowdown. While the platform was widely used (2.4 million K-12 module assignments over 11 years), the schools and teachers who adopted online modules may not be representative of K-12 education broadly. Three selection concerns arise. First, teachers who use online modules may be more technologically sophisticated or innovative than average. Second, schools using the platform skew toward states with economics and personal finance requirements. Third, students who complete assigned modules may be more engaged than students who do not. Our findings apply most directly to the population of students, teachers, and schools that use online educational resources. The complementarity we document may be larger or smaller in traditional classroom settings.

That said, several factors support broader applicability. The platform is free and requires minimal technical infrastructure, lowering barriers to adoption. The content is developed by economics educators and aligned with national standards. The teacher-mediated model (teachers assign modules, not students) reduces student self-selection. And the large sample size and geographic diversity (all 50 states plus DC) provide some reassurance. We interpret our estimates as informative about the underlying relationship between economics and personal finance learning, while acknowledging that magnitudes may differ in other settings.

Causal identification. Our causal identification relies on sequencing being quasi-random conditional on observables. We provide evidence supporting this assumption: Pre-test scores are balanced across sequencing groups, and the effect survives teacher and school fixed effects. However, we cannot definitively rule out selection on unobservables that correlate with both sequencing and outcomes. The fact that future enrollment predicts (weakly) past performance suggests some selection exists, though the effect survives our strongest identification checks.

Measurement ceiling. Our outcome is censored from above, and by more than a rounding error: 25.9% of students in the sequencing sample score a perfect 100 on the personal finance post-test. Because the testing score ceiling is 100, every estimate we report

is attenuated and should be read as a lower bound on the effect on latent knowledge. This runs in the direction of understating our results rather than overstating them, and it explains part of why the effect appears small for high-ability students, two-fifths of whom sit at the ceiling. A censored-regression specification would recover a larger effect at the cost of a distributional assumption; we report the linear estimates and flag the attenuation rather than trade transparency for magnitude.

Outcome horizon. Our outcomes measure immediate learning (post-test scores). The ultimate test of complementarity would examine long-run financial behavior. Whether students exposed to both subjects make better financial decisions as adults remains an open question. The knowledge transfer we document may or may not persist into adulthood.

Generalizability of content. The asymmetric sequencing effects may reflect the specific content of our modules rather than a general property of the subjects. Different curricula might produce different patterns of knowledge transfer. Our topic-level analysis finds similar per-module effects across the four topics we can estimate, and no composition effect that survives teacher fixed effects, but that joint test is imprecise and the uniformity might not hold for curricula with substantially different pedagogical approaches or content depth.

5 Conclusion

This paper provides the first large-scale evidence on the relationship between K-12 economics and personal finance education. Using 2.4 million module assignments from the Econ Lowdown platform, we document strong complementarity between the two subjects and identify directional spillovers using variation in sequencing.

Our main findings can be summarized as follows. First, students who complete economics before personal finance score 3.42 points higher on personal finance post-tests, controlling for pre-test scores and school demographics. With teacher fixed effects (comparing students assigned by the same teacher), the effect remains at 2.2 points, demonstrating that this is not

driven by teacher quality differences. The reverse effect is 0.26 points estimated on the same specification, a ratio of nearly 14 to 1, and 0.15 points and indistinguishable from zero once school demographics are added. The raw effect is largest for low-ability students (5.1 points against 1.6 for high-ability students), though most of that gradient reflects a measurement ceiling rather than differential returns to scaffolding.

Second, cross-subject performance is strongly and symmetrically associated. Conditional on pre-test scores in both subjects, a 1-point increase in the economics post-test predicts a 0.37-point increase in the personal finance post-test and 0.47 points in the reverse direction. Because a learning gain differs from a post-test only by the pre-test we already condition on, the same coefficients describe gains. This symmetry contrasts with the asymmetric sequencing results and suggests shared cognitive skills that develop in either subject.

Third, dose-response relationships are positive but exhibit sharply diminishing returns. Each additional economics module completed before personal finance improves personal finance post-test scores by between 0.8 and 1.4 points, with most of the effect concentrated in the first one or two modules and the range reflecting how much of the association a placebo test attributes to selection. No symmetric dose-response emerges for personal finance modules on economics outcomes, consistent with the asymmetry in the sequencing results.

Fourth, spillovers persist over time. Students who complete economics months before personal finance show larger benefits than those who complete both in quick succession, suggesting that economic concepts consolidate and become more useful rather than decay.

These findings bear on policy, though it is worth being precise about how. Economics and personal finance are complements in learning: A student who studies one is better prepared to study the other. That is a statement about ordering among students who take both subjects, which is what our variation identifies. It is not a statement about the crowding-out debate, which concerns the extensive margin, whether requiring personal finance displaces economics from the curriculum. We observe neither instructional time nor what schools would teach in the absence of a requirement, so we cannot and do not speak to that question. What our

results do support is narrower and still useful for curriculum design: When both subjects are taught, economics should come first, and even one or two economics modules capture most of the available gain.

Several questions remain open for future research. Does the complementarity we document extend to long-run outcomes such as financial behavior and economic decision-making? Does the spillover operate through general reasoning or through topic-specific knowledge, a question our joint composition test is too imprecise to settle? How do online and in-person instruction compare in generating cross-subject spillovers?

One promising avenue concerns optimal sequencing based on prior knowledge. Our findings suggest that economics-first sequencing benefits most students, but this average effect may mask heterogeneity. Students with strong baseline financial literacy might benefit less from economics-first sequencing, while those with weak foundations might benefit more. The "Big Three" financial literacy questions developed by Lusardi and Mitchell (2014) (assessing understanding of compound interest, inflation, and risk diversification) provide a validated instrument for measuring baseline financial knowledge. Future research could use this instrument as a diagnostic tool to sort students into optimal curricular sequences: Students who answer incorrectly on the compound interest question, for example, might be directed to economics modules on interest rates before encountering personal finance content on saving and investing. Such adaptive sequencing could personalize the complementarity we document, directing students to fill conceptual gaps before building applied knowledge.

Answers to these questions would further inform curriculum design and state policy.

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A Robustness Checks

This appendix presents robustness checks supporting the causal interpretation of our sequencing results.

A.1 Robustness of the Sequencing Effect

The causal interpretation of our sequencing results rests on the assumption that, conditional on observables, whether a student takes economics or personal finance first is quasi-random. We conduct several tests to evaluate this assumption.

Balance on observables. Table 9 compares students who took economics first with those who took personal finance first, separating covariates that are fixed before the ordering decision from those the ordering decision affects. Following Imbens and Rubin (2015), normalized differences below 0.25 suggest adequate balance. Every variable in Panel A clears that threshold: economics pre-test (0.085), personal finance pre-test (−0.058), starting in the fall semester (0.029), middle school (0.058), and first year observed (−0.011). This matters more than balance on pre-tests alone. A teacher who teaches economics in the fall and personal finance in the spring chooses the order and the calendar together, so semester of entry is the covariate most likely to carry the confound, and it is balanced to three decimal places. Grade level and cohort year are balanced, too, so economics-first students are not drawn from different years, different grades, or different points in the school calendar.

Panel B reports the three variables that fail that threshold, none of them pre-determined. Economics-first students complete fewer personal finance modules (3.19 versus 4.59, normalized difference −0.32) and fewer modules in total (−0.32), and they wait longer between subjects (43.8 versus 30.6 days, 0.35). Module counts are outcomes of the sequence. The gap is better described as jointly determined with the order by the same curricular decision than as caused by it, which is why we do not treat conditioning on it as a correction either. We report these rows because a balance table should not be silent about them and because

they raise the question of which way the imbalance pushes the estimate. Table 7 answers that.

Adding module counts raises the economics-first estimate from 3.59 to 4.04 points, and to 3.90 with district demographics or with the time gap included. The direction is intuitive: Completing more personal finance modules mechanically raises a student's average personal finance post-test, and economics-first students complete fewer of them, so omitting dosage understates the economics-first advantage. In the reverse direction, the imbalance works the other way. The personal-finance-first estimate falls from 0.26 points to -0.12 and remains insignificant ($p = 0.29$). So, once dosage is held fixed, there is no evidence of any spillover from personal finance to economics. The imbalances flagged by the balance table therefore work against our finding rather than for it. We are careful about what this does and does not show. Conditioning on module counts is not a bias correction, because the counts are themselves affected by the sequence, so the resulting coefficient answers a different question: the effect of ordering while holding total exposure fixed. What it establishes is that the imbalance cannot explain our result. Holding dosage fixed, the economics-first advantage is larger and the reverse effect is zero. We retain the specification without dosage controls as the baseline and report both.

A placebo test and what it implies for the dose-response. A natural concern is that students who complete more economics modules are simply more engaged and that engagement rather than economics content raises their personal finance scores. We test this directly. Among personal-finance-first students, economics modules completed after their first personal finance module cannot have caused that module's post-test, so if the dose-response were purely causal, post-treatment exposure should be uninformative.

Designing this test requires care about the outcome. A student's *average* personal finance post-test will not do because, for a personal-finance-first student, that average includes modules completed after the intervening economics modules. So, the coefficient captures genuine spillover as well as selection. We therefore use the post-test of the student's *first*

Table 7: Robustness to Dosage Controls

	PF Post				Econ Post	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Constant	59.65***	56.62***	13.93***	56.26***	57.93***	54.73***
	(0.1907)	(0.2154)	(2.112)	(0.2201)	(0.2067)	(0.2247)
Economics First	3.591***	4.040***	3.900***	3.890***		
	(0.1033)	(0.1047)	(0.1149)	(0.1067)		
PF Pre-test	0.2705***	0.2751***	0.2725***	0.2750***	0.0663***	0.0692***
	(0.0027)	(0.0027)	(0.0030)	(0.0027)	(0.0027)	(0.0027)
Econ Pre-test	0.0903***	0.0899***	0.0873***	0.0904***	0.3089***	0.3070***
	(0.0027)	(0.0027)	(0.0030)	(0.0027)	(0.0030)	(0.0029)
Private School	0.5041**	0.8196***		0.8518***	0.9393***	1.344***
	(0.1840)	(0.1843)		(0.1843)	(0.2034)	(0.2033)
N Econ Modules		0.5033***	0.5703***	0.5152***		0.8717***
		(0.0153)	(0.0167)	(0.0154)		(0.0163)
N PF Modules		0.1267***	0.1483***	0.1175***		-0.0569***
		(0.0102)	(0.0109)	(0.0103)		(0.0122)
Minority Share			1.554***			
			(0.2088)			
Expenditure per Pupil (log)			4.396***			
			(0.2190)			
Days Between Subjects				0.0108***		
				(0.0013)		
PF First					0.2636*	-0.1190
					(0.1109)	(0.1117)
Fit statistics						
Observations	119,790	119,790	100,896	119,790	119,790	119,790
R^2	0.15685	0.16652	0.16858	0.16696	0.12470	0.14367

Heteroskedasticity-robust standard-errors in parentheses

Notes: Sequencing sample. Columns (1)–(4) have the personal finance post-test as the dependent variable; columns (5)–(6) the economics post-test. Column (1) reproduces the baseline specification; columns (2)–(4) add the variables that fail the balance test.
Heteroskedasticity-robust standard errors in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

personal finance module and count economics modules whose pre-test falls strictly after that module's post-test date. Those modules cannot have affected that score under any story. The distinction matters: On the averaged outcome, the placebo is 0.83 and on the first-module outcome it is 0.54. So, the averaged version overstates selection by roughly half. Holding the outcome fixed and varying only the timing rule moves the estimate from 0.527 to 0.536, which is to say almost nothing. The contamination lives in the outcome, not the timing.

Selection is nonetheless real. Table 8 reports the clean pair on the same outcome: 1.23 points per pre-treatment module among economics-first students compared to 0.54 points per post-outcome module among personal-finance-first students. The placebo accounts for 44% of the association, leaving 56% attributable to pre-treatment exposure. Applying that share to our headline estimate of 1.41 points implies a causal effect of roughly 0.8 points per module. This step assumes the selection share is similar for the averaged outcome and the economics-first population, which we cannot verify; we therefore treat 0.8 as a calibration rather than a formal bound. We therefore report the dose-response as a range of 0.8 to 1.4 points rather than a point estimate. The range excludes zero, and the calculations earlier in the paper that use 1.41 should be read as upper bounds.

Two further considerations limit the concern. Our specifications control for pre-test scores in both subjects, absorbing fixed differences in ability and engagement. And our teacher fixed effects estimates compare students assigned different sequences by the *same teacher*, so any systematic selection by teachers, such as assigning more-engaged students to economics-first, is absorbed. The sequencing effect of 2.2 points with teacher fixed effects represents within-teacher variation and cannot be explained by differential teacher assignment of students to sequences. We note that the placebo bears on the dose-response, which measures intensity, rather than on the sequencing estimate itself, which compares students at the same level of exposure.

Teacher fixed effects. A key concern is that teachers who assign economics first may

Table 8: Dose-Response and Placebo: Pre- versus Post-Treatment Exposure

Model:	Dose (pre-treatment) (1)	Placebo (post-outcome) (2)
Variables		
Constant	62.96***	54.29***
	(0.3315)	(0.2951)
N Econ Modules (before first PF)	1.230***	
	(0.0441)	
PF Pre-test	0.2167***	0.3314***
	(0.0040)	(0.0041)
Econ Pre-test	0.1053***	0.0624***
	(0.0049)	(0.0040)
Private School	1.741***	1.222***
	(0.3291)	(0.2683)
N Econ Modules (after first PF)		0.5362***
		(0.0252)
Fit statistics		
Observations	49,612	74,793
R^2	0.12047	0.12842

Heteroskedasticity-robust standard-errors in parentheses

Notes: The dependent variable in both columns is the post-test of the student's first personal finance module, so the two estimates are directly comparable. Column (1) counts economics modules whose pre-test precedes that module's pre-test, among economics-first students. Column (2) counts economics modules whose pre-test falls strictly after that module's post-test, among personal-finance-first students; these cannot have affected the outcome. Heteroskedasticity-robust standard errors in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 9: Balance Table: Economics-First vs. Personal-Finance-First Students

	Econ first	PF first	Difference	Norm. diff.
Panel A: Determined before or independently of the ordering				
Economics pre-test	51.16	49.41	1.75	0.085
Personal finance pre-test	59.39	60.76	-1.37	-0.058
Started in fall semester	0.496	0.482	0.014	0.029
Middle school	0.195	0.172	0.022	0.058
First year observed	2020.73	2020.76	-0.03	-0.011
Panel B: Determined jointly with or after the ordering				
Economics modules	3.78	4.29	-0.52	-0.161
Personal finance modules	3.19	4.59	-1.41	-0.320
Total modules	6.96	8.89	-1.92	-0.323
Days between subjects	43.8	30.6	13.2	0.354

Notes: Sequencing sample, 119,790 students (Table 2, Panel B). Panel A of this table contains covariates fixed before or independently of the ordering decision; Panel B contains quantities the ordering decision affects. "Started in fall semester" and "Middle school" are indicators; "First year observed" is a calendar year. Normalized difference calculated as $(\bar{x}_1 - \bar{x}_0)/\sqrt{(s_1^2 + s_0^2)/2}$; values below 0.25 indicate adequate balance. We omit p-values because with 119,790 observations every difference is statistically significant and none of them is therefore informative about balance.

be systematically different from those who assign personal finance first. To address this, we restrict to teachers who have students in both sequencing groups (691 teachers in the K-12 sample), allowing us to estimate effects from within-teacher variation. Table 10 presents the results.

Table 10: Robustness: Teacher and School Fixed Effects

Model:	No FE (1)	Teacher FE (2)	School FE (3)	School + Teacher FE (4)
Variables				
Economics First	3.294***	2.207***	2.558***	2.098***
	(0.5342)	(0.3681)	(0.4384)	(0.3888)
Fixed-effects				
teacher		Yes		Yes
NCES School			Yes	Yes
Fit statistics				
Observations	100,447	100,447	99,192	99,192

Notes: Sample restricted to students whose teachers have students in both sequencing groups (columns 1–2) or whose schools have students in both groups (columns 3–4). Standard errors clustered at teacher or school level in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The baseline estimate on the restricted sample is 3.29 points (column 1). Adding teacher fixed effects reduces the estimate to 2.21 points (column 2), a 33% reduction but still highly significant. This attenuation suggests that some of the raw difference reflects teacher effects, but a substantial effect remains. With school fixed effects, the estimate is 2.56 points (column 3); adding both school and teacher fixed effects yields 2.10 points (column 4). The effect is robust to controlling for unobserved teacher and school heterogeneity. Three school fixed-effects estimates appear in the paper and differ only by sample: 2.57 on the full sequencing sample (Table 4), 2.56 on schools with students in both sequencing groups (this table), and 2.44 on the NCES-matched subsample (Table 11). The two teacher fixed-effects estimates are 2.21 here and 1.95 with demographic controls and school fixed effects added. We quote 2.2 in the abstract as the headline within-teacher estimate; 1.95 is the most demanding

specification we report and the more conservative figure.

A.2 Robustness to School Demographics

Our main analysis restricts to K-12 students and controls for a private school indicator. A natural concern is whether results are driven by systematic differences across school districts in resources or demographics, which might generate both sorting into sequencing and differences in outcomes.

Table 11 presents results across control sets and identification strategies. Column (1) shows our baseline K-12 specification with pre-test controls and a private school indicator ($N = 119{,}790$). Column (2) adds district demographic controls (minority student share and log per-pupil expenditure) from the NCES Common Core of Data; this restricts the sample to 100,896 students matched to NCES demographic records. The coefficient is stable at 3.42 points, indicating that district characteristics do not confound the sequencing effect. Column (3) uses expenditure and minority quintile indicators for a more flexible functional form; results are nearly identical at 3.46 points.

Columns (4)–(5) employ fixed effects to control for unobserved heterogeneity. School fixed effects (column 4) absorb all time-invariant school characteristics, including demographics, resources, and school culture. The effect remains significant at 2.44 points. Adding teacher fixed effects (column 5), which isolate within-teacher variation in sequencing, yields an effect of 1.95 points. The stability of the coefficient across these demanding specifications provides reassurance that our findings reflect genuine complementarity in learning rather than selection on school characteristics.

District characteristics show positive associations with performance: Controlling for pre-test scores, a 10 percentage point higher minority share is associated with approximately 0.15 points higher personal finance post-test scores, and a 10% increase in per-pupil expenditure is associated with approximately 0.35 points higher scores. These associations likely reflect selection into platform usage rather than causal effects of school resources.

Table 11: Robustness to School Demographics

Model:	Baseline (1)	+ Demographics (2)	+ Quintiles (3)	School FE (4)	School+Teacher FE (5)
Variables					
Economics First	3.591***	3.417***	3.461***	2.443***	1.952***
	(0.1033)	(0.1136)	(0.1137)	(0.4663)	(0.4150)
Minority Share		1.448***	0.3203		
		(0.2100)	(0.2195)		
Fixed-effects					
NCES School				Yes	Yes
teacher					Yes
Fit statistics					
Observations	119,790	100,896	100,896	100,896	100,896
R^2	0.15685	0.15619	0.15839	0.24093	0.25716

Notes: Dependent variable is personal finance post-test score. All specifications control for economics and personal finance pre-test scores. Column (1) includes a private school indicator and all K-12 students. Columns (2)–(5) restrict to schools matched to NCES demographic data. Demographic controls in column (2): minority student share and log per-pupil expenditure. Column (3) uses quintile indicators for each. Standard errors clustered at school level in columns (4)–(5). *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

A.3 Oster Bounds

We apply the methodology of Oster (2019) to assess how much selection on unobservables would be needed to explain away the sequencing effect. The controlled OLS estimate is 3.59 points ($R^2 = 0.157$; Table 4, column 2). Adding school fixed effects reduces the estimate to 2.57 points while increasing R^2 to 0.245 (column 4). Under the assumption of proportional selection ($R_{\max} = 1.3 \times \tilde{R}^2$), the implied $\delta = 3.01$: Unobservable selection would need to be three times as large as the observable selection (measured by the OLS-to-FE movement) to drive the estimate to zero. The identified set under $\delta = 1$ is $[1.72, 2.57]$. Since $\delta > 1$ is the standard threshold for robustness (Oster, 2019), and our estimate exceeds it by a wide margin, unobservable selection is unlikely to explain the sequencing effect.