



**Financial Education as Human Capital:
Secondary School Curriculum Design and Macroeconomic Outcomes Across
OECD Nations**

Taisei Amemiya and Jameson Augustin
Tenaflly High School; The University of Georgia
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Abstract

This paper examines how secondary school financial literacy scores and curriculum design relate to macroeconomic outcomes. The sample is an unbalanced panel of ten selected OECD nations, comprising 24 country–wave observations from the 2012, 2015, 2018, and 2022 PISA cycles. We distinguish integrated curricula, which cover investment mechanics such as equity markets and pension structures, from thrift-based curricula centered on saving and debt avoidance. Three outcomes are considered: domestic private credit as a percentage of GDP, the Gini index, and annual real GDP growth. Models use ordinary least squares with wave-year fixed effects and HC3 heteroskedasticity-robust standard errors. Neither financial literacy scores nor curriculum type is significant under the Bonferroni-adjusted confirmatory threshold ($\alpha^* = 0.0083$ for six tests). One coefficient stands out: secondary school enrollment is HC3-significant ($p = 0.004$). Yet its bootstrap 95% confidence interval includes zero. The evidence is therefore suggestive, not causal; curriculum assignment lacks exogenous variation.

Introduction

Across high-income economies, a curious macroeconomic pattern persists. Households may save heavily while adult financial literacy remains low, leaving capital in low-yield deposits rather than productive equity markets. Japan offers a sharp example: Sticha and Sekita (2023) find that only 36 percent of adults correctly answer the “Big Three” questions on compound interest, inflation, and risk diversification. Related patterns appear in Southern Europe, particularly Italy and Portugal, where market participation and private credit depth seem modest relative to household saving. The puzzle, then, is not a shortage of saving. It is the difficulty of turning savings into efficiently allocated capital—a difficulty policymakers sometimes link to the design of secondary school financial education.

The research question is straightforward: how are integrated and thrift-based secondary school financial education frameworks associated with income inequality and GDP growth across selected OECD countries? East Asia supplies much of the motivation, but not the empirical sample. PISA Financial Literacy participation is too sparse for a four-cycle East Asian panel. China (four provinces—Beijing, Shanghai, Jiangsu, and Guangdong) participated in 2012 and 2015, while no East Asian economy participated in the 2018 or 2022 assessments; Japan, South Korea, Hong Kong, and Singapore did not appear in all four cycles (OECD, 2014, 2017, 2020, 2024). Comparable national strategy documents were also not consistently available. Consequently, the analysis covers ten European and Middle Eastern OECD nations. That boundary matters: the conclusions do not extend to East Asia or to high-saving economies in general. Within the chosen sample, however, financial literacy scores and curriculum design vary enough to support a focused comparison.

The literature has documented links between financial literacy and household choices, including retirement planning and debt management. Its country-level implications are less settled. Prior models treat financial literacy as specialized human capital that may improve financial-sector allocation, but they also warn that instructional time can crowd out general education. Content may therefore matter as much as the existence of a course.

The paper makes three contributions. First, it shifts attention from household behavior to national curriculum design. Second, it separates the cultivation of thrift from instruction in investment mechanics and asks whether that distinction appears in inequality outcomes. Third, it combines OECD and World Bank data to see whether the hypothesized macroeconomic relationships are detectable in a small panel. The scale is modest, but the question is consequential.

The rest of the paper follows a conventional path. [section 2](#) reviews the literature, and [section 3](#) explains the data and variable construction. [section 4](#) presents the estimates before [section 5](#) de-

tails the econometric strategy and tests. [section 6](#) interprets the evidence and policy implications. [section 7](#) closes with directions for future research.

Literature Review

Scholarship on financial literacy began largely with household behavior and has gradually moved toward macroeconomic questions. The review is organized around three themes: individual outcomes, financial literacy as specialized human capital, and aggregate links to inequality and growth.

A broad point of agreement is that financial literacy shapes individual economic well-being. Mitchell and Lusardi ([2015](#)) link it to borrowing, saving, and spending, with the largest knowledge gaps among women and less-educated groups. Japan again offers a revealing case. Only 36 percent of adults correctly answer the “Big Three” questions on compounding, inflation, and diversification (Sticha & Sekita, [2023](#)). Limited knowledge may help explain why savings remain in cash deposits rather than moving into productive equity markets, although the evidence here is consistent with that mechanism rather than proof of it.

Knowledge is not the whole story. Asaad ([2015](#)) distinguishes financial confidence from demonstrated knowledge and finds that overconfidence is associated with risky, costly behavior. A useful curriculum must therefore do more than repeat rules about thrift. It should also cultivate an accurate sense of what students understand—and what they do not.

Recent models move beyond behavior and treat financial literacy as specialized human capital. Bucci et al. ([2025](#)) argue that it can improve the allocation of savings. There is a catch: expanding financial instruction may crowd out general education. Curriculum design, in other words, cannot be reduced to adding more classroom hours.

Their calibration identifies a threshold. Above it, financial education raises long-run output growth relative to an equilibrium without deliberate financial-literacy accumulation. We return to this framework in the Discussion, where it is more relevant to GDP growth than to inequality.

Lusardi ([2019](#)) surveys evidence from the “Big Three” questions and documents especially pronounced gaps in risk-diversification knowledge: across countries, respondents perform worst on risk concepts, and roughly 30% report that they do not know the answer to the risk-diversification question. She argues that these patterns underscore the importance of familiarizing learners with risk and diversification.

Macroeconomic evidence is newer. Using panel quantile regression for 61 countries, Boikos et al. ([2025](#)) find a positive association between financial literacy and GDP per capita growth. They also estimate that literacy mitigates the diminishing growth returns to financial development by 7.41% on average. The estimated growth effect is larger at lower quantiles of the conditional growth distribution. Importantly, those quantiles describe growth outcomes, not countries’ level of financial development.

Calcagno and Marsiglio ([2025](#)) extend this line of inquiry by analyzing financial literacy’s role in reducing aggregate shocks to saving productivity. They posit that underinvestment in financial education can produce poverty traps in which economies become locked into low-growth cycles due to inefficient capital allocation. This is corroborated by Nogueira et al. ([2025](#)), who examine twenty OECD countries and find that human development, GDP per capita, and tertiary education attainment are positively associated with financial literacy, while inequality in access to health and education and household debt are negatively associated with it.

Financial education affects later behavior (Kaiser & Menkhoff, [2020](#)), but most studies still treat it as a single intervention. Curriculum content is rarely separated. Lusardi et al. ([2017](#)) establish the theoretical importance of financial knowledge for wealth inequality, and Lusardi and Mitchell ([2014](#))

review the broader evidence. Neither directly compares thrift-based curricula, common in traditional savings cultures, with integrated curricula that teach investment mechanics. That is the gap taken up here.

Using World Bank and OECD data, we ask whether literacy–growth associations or curriculum moderation can be detected in a comparable OECD panel. The analysis builds on the macro-level evidence assembled by Boikos et al. (2025), but it turns attention toward curriculum content.

Data

The unit of observation is a country–PISA wave pair (i, t) . The sample consists of ten OECD member nations that participated in at least one round of the OECD PISA Financial Literacy module: the Czech Republic, Spain, Israel, Italy, the Netherlands, Norway, Poland, Portugal, the Slovak Republic, and Slovenia. Together these countries contribute 24 country–wave observations across four assessment cycles (2012, 2015, 2018, and 2022), forming an unbalanced panel because not every country participated in every PISA wave (Table 2). The sample is restricted to countries that administered the optional PISA financial literacy component and for which World Bank World Development Indicators (WDI) data were available for the outcome variables of interest (OECD, 2014, 2017, 2020, 2024; World Bank, 2026).

PISA Financial Literacy Data

The primary independent variable—country-level mean financial literacy (FL) score—is drawn from the OECD PISA Financial Literacy volumes for 2012, 2015, 2018, and 2022. PISA assesses a nationally representative sample of 15-year-old students using an Item Response Theory (IRT) scale with an OECD average of approximately 500 points and a standard deviation of approximately 100 points. Scores are comparable across waves through common-item equating (OECD, 2014, 2017, 2020, 2024). In this sample, FL scores range from 445 (Slovak Republic, 2015) to 520 (Poland, 2018), with a pooled mean of 489.7 points and a standard deviation of 18.4 points (Table 3).

Curriculum Type

The binary treatment variable, *curriculum type*, distinguishes between two frameworks for secondary school financial education. A country is coded *integrated* (= 1) if its national financial education strategy explicitly includes investment mechanics—such as equity markets, bonds, pension fund structure, or capital market instruments—as part of the curriculum. A country is coded *thrift-based* (= 0) if the national strategy emphasizes savings, budgeting, and debt avoidance without teaching the mechanics of market investment. Coding is based on OECD/INFE national financial education strategy documents (OECD, 2015). Readers should note that country-level classification relies on the availability and interpretation of national strategy documents; in a 10-country sample, a miscoding of even one country could materially affect the results, and a country-by-country source audit would be needed to fully reproduce this variable. The variable is treated as time-invariant because no country in the sample underwent a major documented curriculum reform between waves. Integrated countries: Czech Republic, Spain, Israel, the Netherlands, and Norway. Thrift-based countries: Italy, Poland, Portugal, the Slovak Republic, and Slovenia. Table 1 documents the primary source evidence for each country’s classification.

Table 1
Country-Level Curriculum Classification: Integrated vs. Thrift-Based

Country	Code	Classification	Primary Source / Coding Evidence
Czech Republic	CZE	Integrated	National Strategy for Financial Literacy (2010, rev. 2017): explicitly includes investment vehicles (equities, bonds, pension funds) across subjects.
Spain	ESP	Integrated	Financial Education Plan (CNMV/Banco de España, 2008+): covers basic investment instruments including bonds and equity funds.
Israel	ISR	Integrated	Ministry of Finance national financial education program: includes equity markets and capital market instruments.
Italy	ITA	Thrift-based	Committee for Financial Education (Comitato Edufin, 2017): emphasizes saving, budgeting, and responsible credit; investment mechanics not in core scope.
Netherlands	NLD	Integrated	Wijzer in geldzaken (Money Wise, 2007+): includes investment basics and pension fund structure.
Norway	NOR	Integrated	Consumer Education curriculum (upper secondary): includes investment and pension topics.
Poland	POL	Thrift-based	National Financial Education Strategy: emphasizes savings and responsible borrowing; investment mechanics secondary.
Portugal	PRT	Thrift-based	National Plan for Financial Literacy (PNFF, 2011 +): focuses on savings, budgeting, and over-indebtedness avoidance.
Slovak Republic	SVK	Thrift-based	National Standard for Financial Literacy: personal finance management, saving, and responsible credit.
Slovenia	SVN	Thrift-based	Slovenian financial education strategy: savings, budgeting, and consumer protection emphasis.

Note. Classification is based on OECD/INFE national strategy documents (OECD, 2015). A country is coded *integrated* if its documented national strategy explicitly includes investment mechanics (equities, bonds, pensions, capital market instruments); it is coded *thrift-based* if the strategy emphasizes savings, budgeting, and debt avoidance without investment instruction. The documentary basis for the Israel and Norway classifications is less definitive than for other countries in the sample; see [subsection 6.6](#) for discussion.

World Bank Macroeconomic Indicators

Three outcome variables and four control variables were sourced from the World Bank WDI, matched to each country in the PISA wave year (World Bank, 2026).

Dependent variables:

- *Private Credit (% GDP)* — Domestic credit to the private sector by banks as a percentage of GDP (WB code: FD.AST.PRVT.GD.ZS), the primary measure of credit depth (SRQ1). Note that high credit-to-GDP ratios may reflect overleverage as well as financial deepening; this outcome is therefore labelled “credit depth” rather than “capital allocation efficiency” throughout.
- *Gini Index* — The Gini coefficient of income inequality, ranging from 0 (perfect equality) to 100 (maximum inequality), derived from household surveys (WB code: SI.POV.GINI) (SRQ2).
- *GDP Growth (%)* — Annual percentage growth rate of real GDP in constant local currency units (WB code: NY.GDP.MKTP.KD.ZG) (SRQ3).

Control variables:

- *Gross Savings (% GDP)* — Gross national savings as a share of GDP (NY.GNS.ICTR.ZS), included to control for baseline thrift culture.
- *Government Education Expenditure (% GDP)* — General government expenditure on education as a percentage of GDP (SE.XPD.TOTL.GD.ZS), controlling for overall educational investment.
- *Secondary Enrollment Rate* — Gross secondary school enrollment ratio (SE.SEC.ENRR), controlling for educational access.
- *Private Credit (% GDP)* — Also included as a control in the Gini and GDP growth specifications when it is not the dependent variable.

Wave-year fixed effects (dummy variables for 2015, 2018, and 2022; baseline year = 2012) are included in all regression specifications to absorb common macroeconomic shocks, including the euro-area sovereign debt crisis in 2012 and the post-COVID recovery in 2022.

Missing Values

All World Bank indicators are recorded as continuous numerical values (percentages or index scores). The Gini Index is survey-based and not observed annually; only country–wave observations where a survey value falls within one calendar year of the PISA wave year are included in the inequality regression, yielding $N = 23$ for that specification. Private credit and GDP growth are fully observed across all 24 country–wave pairs. Table 3 reports complete descriptive statistics for all regression-relevant variables.

Results

All models use ordinary least squares (OLS), wave-year fixed effects, and HC3 heteroskedasticity-robust standard errors. The estimates describe associations, not causal effects. With only 24 country–wave observations, power is limited and uncertainty is substantial.

Descriptive Statistics

Table 2 reports country-level means for GDP growth, inequality, private credit depth, and government education expenditure over 2000–2023 (World Bank, 2026). Pooled statistics at the PISA-wave-year level appear in Table 3. The mean FL score was 489.7 points (SD = 18.4), with values from 445 to 520. GDP growth averaged 2.65% (SD = 2.89). Its range was wide: -3.13% in Italy in 2012 and 6.99% in Portugal in 2022. The mean Gini index was 31.5 (SD = 4.6), while private credit averaged 77.5% of GDP (SD = 28.0). Spain was the clear high-credit case, reaching 157.4% in 2012.

The figures offer a visual first pass. Figure 1 plots FL scores against GDP growth; labels identify the country and wave, while colour marks curriculum type. Figure 2 repeats the comparison for inequality. Finally, Figure 3 traces private credit from 2000 to 2023, shading PISA years and using solid lines for integrated-curriculum countries.

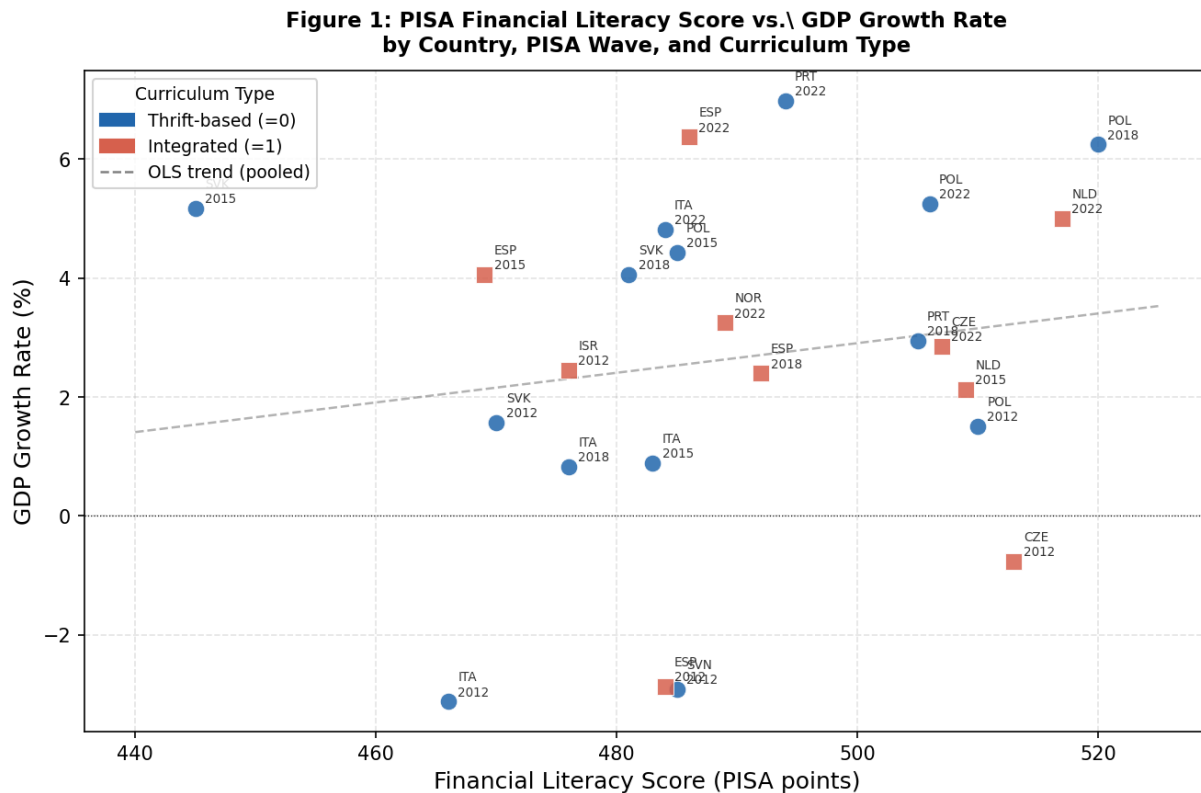


Figure 1

PISA Financial Literacy Score vs. GDP Growth Rate by Country, PISA Wave, and Curriculum Type. Blue circles = thrift-based curriculum; red squares = integrated curriculum. The dashed grey line is a pooled OLS trend.

Table 2
Country Summary Statistics (Annual Means, 2000–2023)

Country	Curriculum Type	PISA Waves	Mean GDP Growth (%)	Mean Gini	Mean Private Credit (% GDP)	Mean Gov't Educ. Exp. (% GDP)
Czech Republic	Integrated	2	2.48	26.1	43.1	4.21
Spain	Integrated	4	1.73	34.5	125.2	4.37
Israel	Integrated	1	3.80	40.1	68.6	5.81
Italy	Thrift-based	4	0.56	34.6	77.9	4.23
Netherlands	Integrated	2	1.63	28.3	107.9	5.15
Norway	Integrated	1	1.64	27.4	106.1	7.11
Poland	Thrift-based	4	3.67	32.3	40.7	4.93
Portugal	Thrift-based	2	1.05	35.8	122.6	5.02
Slovak Republic	Thrift-based	3	3.37	25.7	52.3	4.07
Slovenia	Thrift-based	1	2.45	25.2	57.5	5.38

Note. GDP growth is annual real GDP growth rate. Gini index measures income inequality (0=perfect equality, 100=maximum inequality). Private credit and education expenditure are expressed as percentages of GDP. Annual means computed over 2000–2023 from World Bank WDI. PISA waves refer to years in which the country participated in the OECD PISA Financial Literacy module.

Table 3
Descriptive Statistics: Pooled PISA-Wave Observations

Variable	<i>N</i>	Mean	SD	Min	Median	Max
Financial Literacy Score (PISA pts)	24	489.67	18.43	445.00	485.50	520.00
GDP Growth Rate (%)	24	2.65	2.89	-3.13	2.90	6.99
Gini Index	23	31.51	4.56	25.00	33.00	41.30
Private Credit (% GDP)	24	77.52	27.98	39.31	77.71	157.42
Gross Savings (% GDP)	24	22.89	6.36	16.68	21.88	48.63
Gov't Educ. Expenditure (% GDP)	24	4.56	0.53	3.85	4.51	5.67
Secondary Enrollment Rate (%)	24	108.98	12.45	91.78	105.71	138.77

Note. Unit of observation is a country–PISA wave pair. Sample includes 10 OECD countries observed across up to four PISA financial literacy waves (2012, 2015, 2018, 2022), yielding 24 country–wave observations. Gini Index has $N = 23$ (Netherlands 2022 excluded). All World Bank indicators are matched to the PISA wave year.

Capital Allocation: Private Credit

Table 4 reports Specification 1a, with domestic private credit as the outcome. The FL score coefficient was negative and marginally significant ($\hat{\beta}_1 = -0.593$, $p = 0.089$), the opposite of the predicted sign. Curriculum type was clearly non-significant ($\hat{\beta}_2 = 7.248$, $p = 0.635$).

One result stands out. Secondary enrollment has a positive HC3 association with private credit ($\hat{\beta} = 1.953$, $p = 0.004$).¹ Under this model, a one-percentage-point rise in gross secondary enrollment corresponds to a 1.953 percentage-point rise in private credit as a share of GDP, holding other variables constant. Because enrollment is a control, it is assessed at $\alpha = 0.05$, not within the Bonferroni family reserved for FLScore and Integrated. The model has $R^2 = 0.690$ and Adjusted $R^2 = 0.524$.

Income Inequality: Gini Index

Specification 2 uses 23 observations because the Netherlands lacked a 2022 Gini value within one year of the PISA wave. Neither FL score ($\hat{\beta}_1 = -0.105$, $p = 0.389$) nor curriculum type ($\hat{\beta}_2 = 2.078$, $p = 0.798$) was significant. Their supplementary joint F-test was also non-significant. The model's R^2 was 0.487, but its Adjusted R^2 fell to 0.132—a reminder of how many parameters are being estimated from so little data. None of the wave fixed effects reached conventional significance, which fits the relative stability of Gini values in this sample.

GDP Growth Specifications

Table 6 presents three growth models. In the linear baseline, both FL score ($\hat{\beta}_1 = -0.021$, $p = 0.466$) and curriculum type ($\hat{\beta}_2 = 1.105$, $p = 0.465$) were non-significant. Private credit was negative ($\hat{\beta} = -0.040$, $p = 0.033$), a pattern consistent with oversaturation at high credit-to-GDP ratios. Still, that coefficient misses the Bonferroni threshold of $\alpha^* = 0.0083$. It should not carry too much interpretive

¹The country-level bootstrap 95% confidence interval is $[-0.044, 3.280]$ and therefore includes zero. The HC3 p-value and bootstrap interval conflict, so the estimate should be treated as a fragile signal rather than firm evidence of a non-zero effect.

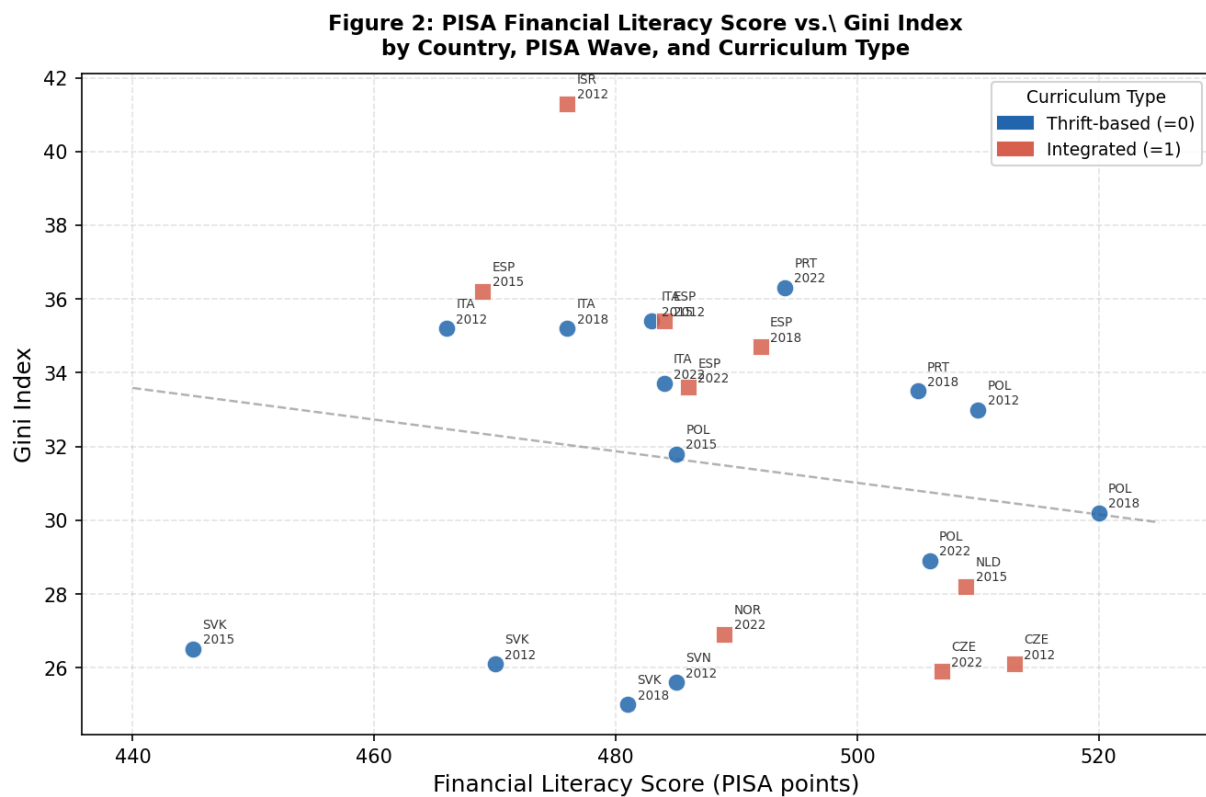


Figure 2

PISA Financial Literacy Score vs. Gini Index by Country, PISA Wave, and Curriculum Type. Blue circles = thrift-based curriculum; red squares = integrated curriculum.

Table 4*Specification 1: Financial Literacy, Curriculum Type, and Private Credit (% GDP)*

<i>Dep. var.:</i>	(1) <i>Full Model (1a)</i> <i>Private Credit (% GDP)</i>
FL Score (PISA pts)	-0.593† (0.349)
<i>95% CI:</i>	[-1.308, 0.870]
Curriculum Type (1=Integrated)	7.248 (15.282)
<i>95% CI:</i>	[-42.549, 64.932]
Gross Savings (% GDP)	-0.152 (1.940)
<i>95% CI:</i>	[-8.044, 3.346]
Gov't Educ. Exp. (% GDP)	0.749 (13.531)
<i>95% CI:</i>	[-37.087, 35.888]
Secondary Enrollment (%)	1.953** (0.671)
<i>95% CI:</i>	[-0.044, 3.280]
<i>Observations</i>	24
<i>R²</i>	0.690
<i>Adj. R²</i>	0.524
Wave FE	Yes
Std. errors	HC3

Note. HC3 heteroskedasticity-robust standard errors in parentheses. Bootstrap 95% confidence intervals (999 replications, country-level resampling) in brackets. Wave fixed effects (2015, 2018, 2022 dummies; baseline = 2012) included in all specifications. † $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Bonferroni-adjusted significance threshold for key regressors: $\alpha^* = 0.0083$.

Table 5*Specification 2: Financial Literacy, Curriculum Type, and Income Inequality (Gini Index)*

<i>Dep. var.:</i>	(1) <i>Full Model (2)</i> <i>Gini Index</i>
FL Score (PISA pts)	-0.105 (0.122)
95% CI:	[-0.444, 0.149]
Curriculum Type (1=Integrated)	2.078 (8.105)
95% CI:	[-12.613, 18.827]
Gross Savings (% GDP)	-0.487 (1.211)
95% CI:	[-2.935, 0.341]
Gov't Educ. Exp. (% GDP)	2.115 (5.796)
95% CI:	[-6.089, 12.677]
Secondary Enrollment (%)	0.132 (0.323)
95% CI:	[-0.507, 1.571]
Private Credit (% GDP)	0.014 (0.085)
95% CI:	[-0.553, 0.193]
<i>Observations</i>	23
<i>R</i> ²	0.487
<i>Adj. R</i> ²	0.132
Wave FE	Yes
Std. errors	HC3

Note. HC3 heteroskedasticity-robust standard errors in parentheses. Bootstrap 95% confidence intervals (999 replications, country-level resampling) in brackets. Wave fixed effects (2015, 2018, 2022 dummies; baseline = 2012) included in all specifications. † $p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. Bonferroni-adjusted significance threshold for key regressors: $\alpha^* = 0.0083$.

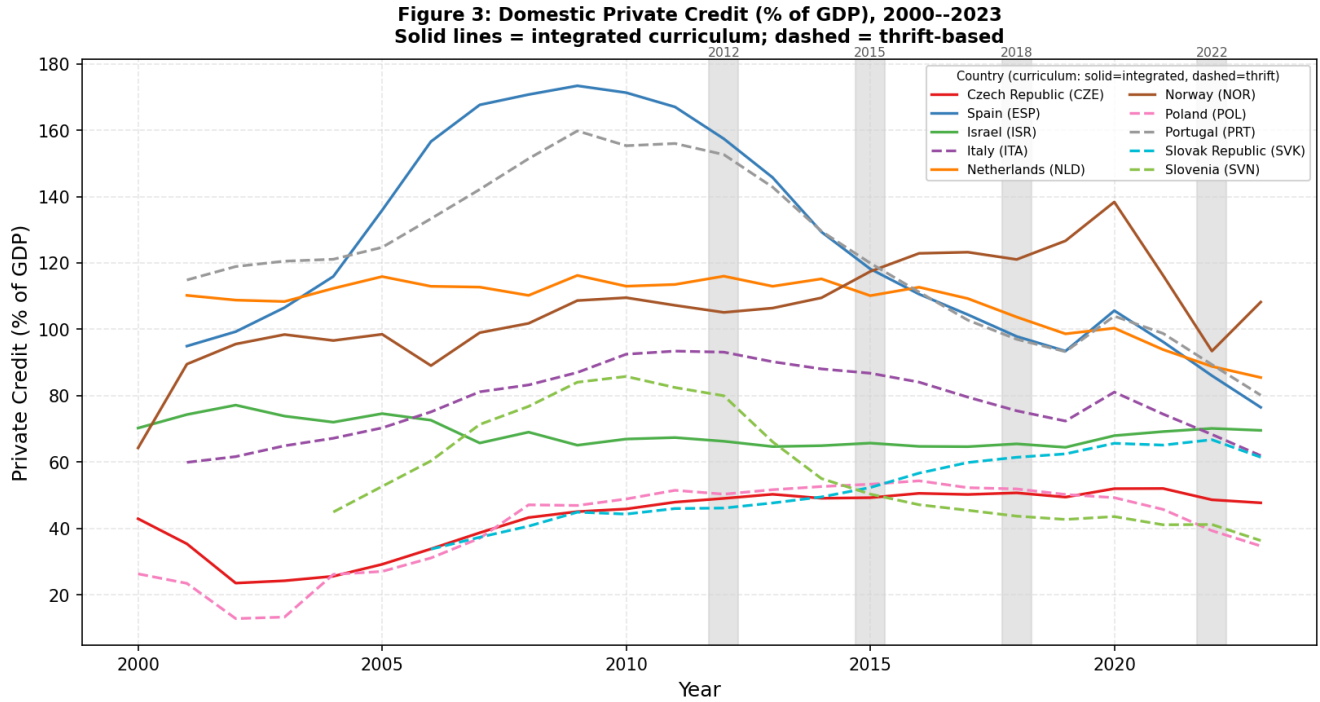


Figure 3

Domestic Private Credit (% of GDP), 2000–2023, for all ten sample countries. Solid lines indicate integrated-curriculum countries; dashed lines indicate thrift-based countries. Shaded bands mark PISA financial literacy assessment years (2012, 2015, 2018, 2022).

weight. By contrast, the 2015, 2018, and 2022 wave effects were all positive and significant relative to 2012 ($p < 0.003$), pointing to much stronger common time shocks.

Model 3b adds a squared FL term. It was positive and non-significant ($\hat{\beta}_2 = 0.001$, $p = 0.481$). Thus, the data do not support an inverted-U relationship, and no growth-maximising threshold can be identified.

In the interaction specification (Model 3c), the $FL \times Curriculum$ interaction was negative and not statistically significant ($\hat{\beta}_4 = -0.114$, $p = 0.125$), providing no evidence that an integrated curriculum amplifies or strengthens the relationship between financial literacy and GDP growth. Model 3c achieved the highest explanatory power of the three growth specifications ($R^2 = 0.818$; Adjusted $R^2 = 0.678$). It is important to note that the slope-moderation Model 3c—which includes both the quadratic FL^2 term and the $FL \times Curriculum$ linear interaction simultaneously—produces an apparently significant raw FL coefficient ($\hat{\beta}_1 = -1.606^*$, $p = 0.047$). This should not be over-interpreted: in this model the linear and quadratic FL components represent the conditional relationship *among thrift-based countries only* (Integrated = 0), evaluated at $FL = 0$ (outside the data range); the change in significance is a collinearity artefact of the saturated specification in a small sample. Evaluated at the sample mean ($\bar{FL} = 489.7$), the implied marginal FL effect is +0.35 pp for thrift-based and +0.24 pp for integrated countries—neither significant and both substantively small.

The broad pattern is simple. Wave-year effects dominate, while all six confirmatory FLscore and Integrated coefficients fail the Bonferroni threshold ($\alpha^* = 0.0083$). The exploratory models tell a similar story: mean-evaluated marginal effects are small and imprecise. Wide standard errors are unsurprising in a panel of 24 observations. The null estimates deserve caution because this design can easily miss effects of plausible magnitude.

Dep. var.:	GDP Growth (%)			
FL Score (PISA pts)	-0.021 (0.029)	-0.899 (1.247)	-1.606* (0.807)	
95% CI:	[-0.103, 0.113]	[-8.613, 2.304]	[-9.890, 2.225]	
FL Score ²	—	0.001 (0.001)	0.002* (0.001)	
95% CI:		[-0.002, 0.009]	[-0.002, 0.011]	
Curriculum Type (1=Integrated)	1.105 (1.512)	1.048 (1.728)	57.061 (35.661)	
95% CI:	[-6.419, 6.911]	[-6.711, 7.332]	[-137.975, 363.644]	
FL Score × Curriculum	—	—	-0.114 (0.075)	
95% CI:			[-0.752, 0.291]	
Gross Savings (% GDP)	-0.128 (0.117)	-0.132 (0.182)	-0.069 (0.311)	
95% CI:	[-1.059, 0.395]	[-1.121, 0.515]	[-1.995, 0.716]	
Private Credit (% GDP)	-0.040* (0.019)	-0.035 (0.022)	-0.036† (0.020)	
95% CI:	[-0.122, 0.046]	[-0.128, 0.111]	[-0.145, 0.187]	
Gov't Educ. Exp. (% GDP)	1.089 (1.555)	1.070 (1.643)	0.486 (1.377)	
95% CI:	[-3.173, 5.487]	[-4.200, 5.523]	[-4.799, 6.742]	
Observations	24	24	24	
R ²	0.725	0.739	0.818	
Adj. R ²	0.578	0.572	0.678	
Wave FE	Yes	Yes	Yes	
Std. errors	HC3	HC3	HC3	

Note. HC3 heteroskedasticity-robust standard errors in parentheses. Bootstrap 95% confidence intervals (999 replications, country-level block resampling) in brackets. Wave fixed effects are included in all models; their coefficient estimates are omitted for brevity because these control parameters are not part of the confirmatory hypothesis tests. Model (3b) adds FL² to probe for nonlinearity. Model (3c) is a *linear slope-moderation* specification: FL×Curriculum shifts the FL–growth slope for integrated countries but a curriculum-specific curvature term (FL²×Curriculum) is not estimated given $N = 24$; FL and FL² terms capture the thrift-based-country relationship (Integrated = 0). Both Models 3b and 3c are *exploratory* (EQ1) and are not subject to Bonferroni correction. **Mean-evaluated marginal effects** (at FL = 489.7 pts, all non-significant): Model 3b: +0.08 pp/pt; Model 3c thrift-based: +0.35 pp/pt; Model 3c integrated: +0.24 pp/pt; curriculum effect at mean FL: $\approx +1.2$ pp. The raw *Integrated* coefficient in Model 3c (57.061) evaluates at FLScore = 0, far outside the data

Methodology

Research Hypotheses

This study tests three formal hypotheses derived from the theoretical framework linking financial literacy and curriculum design to macroeconomic outcomes.

H1: Higher country-level PISA financial literacy scores are positively associated with greater private credit depth, measured as domestic credit to the private sector as a percentage of GDP. The theoretical basis is that populations with stronger financial literacy are better equipped to engage with formal credit markets, improving the efficiency with which savings are intermediated into productive investment. Accordingly, we expect a positive and statistically significant coefficient on the FL score in the private credit specification.

H2: Countries whose secondary school financial education curricula integrate investment mechanics—including equity markets, bonds, pension fund structures, and capital market instruments—exhibit lower income inequality, as measured by the Gini index, and stronger GDP growth relative to countries with thrift-based curricula that emphasize savings and debt avoidance alone. Controls include gross savings rates, government education expenditure, and secondary school enrollment for the private credit and Gini specifications; the GDP growth specifications (Models 3a–3c) exclude secondary enrollment due to degrees-of-freedom constraints and instead include private credit as an additional control. We expect the binary *Integrated* indicator to carry a negative coefficient in the inequality specification and a positive coefficient in the GDP growth specification.

Exploratory Extension (EQ1): Building on the quantile-varying pattern documented by Boikos et al. (2025), two exploratory specifications test whether nonlinearity (Model 3b) or curriculum-slope moderation (Model 3c) of the FL–GDP-growth relationship is detectable in this sample. Because these extensions involve post-specification model choices rather than pre-registered directional predictions about the sign or magnitude of nonlinear effects, they are treated as *exploratory* and are not included in the confirmatory Bonferroni family; their coefficients are assessed against the conventional $\alpha = 0.05$ threshold.

Baseline Specification

The primary empirical model is an OLS panel regression estimated at the country–PISA-wave level:

$$Y_{it} = \beta_0 + \beta_1 \text{FLScore}_{it} + \beta_2 \text{Integrated}_i + \gamma \mathbf{X}_{it} + \delta_t + \varepsilon_{it} \quad (1)$$

where Y_{it} denotes the outcome variable for country i in PISA wave year t ; the three outcome variables estimated in separate specifications are domestic private credit as a percentage of GDP (SRQ1), the Gini index of income inequality (SRQ2), and the annual real GDP growth rate (SRQ3). FLScore_{it} is the country-level mean PISA financial literacy score, an IRT-scaled continuous measure centered at approximately 500 points. Integrated_i is a time-invariant binary variable equal to 1 if country i employs an integrated investment-focused financial education curriculum and 0 if it employs a thrift-based curriculum. $\mathbf{X}_{it}$ is a vector of time-varying control variables including gross national savings as a share of GDP, general government expenditure on education as a share of GDP, and the gross secondary school enrollment ratio; private credit is additionally included as a control in the inequality and GDP growth specifications where it is not the dependent variable. In the GDP growth specifications (Models 3a–3c), the secondary school enrollment ratio is excluded from $\mathbf{X}_{it}$ due to degrees-of-freedom constraints in this small sample; this deviation from the baseline control set is noted in the table note for Table 6. δ_t denotes a set of wave-year fixed effects (indicator

variables for 2015, 2018, and 2022, with 2012 as the baseline) that absorb common macroeconomic shocks shared across all countries in a given assessment cycle. ε_{it} is the idiosyncratic error term.

GDP Growth Extensions

Two exploratory extensions of the baseline GDP growth model probe whether nonlinearity (Model 3b) or curriculum-slope moderation (Model 3c) is detectable in this sample; see Exploratory Extension EQ1 above.

The *quadratic specification* augments the baseline model with a squared FL score term to test for a nonlinear, diminishing-returns relationship between financial literacy and growth:

$$\text{GDPGrowth}_{it} = \beta_0 + \beta_1 \text{FLScore}_{it} + \beta_2 \text{FLScore}_{it}^2 + \beta_3 \text{Integrated}_i + \gamma \mathbf{X}_{it} + \delta_t + \varepsilon_{it} \quad (2)$$

A negative and significant coefficient β_2 would indicate an inverted-U relationship and support the existence of a growth-maximizing threshold level of financial literacy. A positive coefficient would instead indicate accelerating returns, inconsistent with the diminishing-returns hypothesis.

Interpreting polynomial and interaction coefficients at the sample mean: Because $\text{FLScore} = 0$ is far outside the data range (445–520 points), the raw coefficients on the quadratic and interaction terms are not directly interpretable. The meaningful quantity is the marginal effect of FL on GDP growth evaluated at the pooled sample mean, $\bar{\text{FL}} = 489.7$ points. In Model 3b, this marginal effect is $\hat{\beta}_1 + 2\hat{\beta}_2\bar{\text{FL}} = -0.899 + 2(0.001)(489.7) \approx +0.08$ percentage points per PISA point (not significant). In Model 3c, the mean-evaluated marginal effects are approximately +0.35 pp per PISA point for thrift-based countries and +0.24 pp for integrated countries (neither significant). The curriculum effect in Model 3c evaluated at mean FL is $\hat{\beta}_3 + \hat{\beta}_4\bar{\text{FL}} = 57.061 + (-0.114)(489.7) \approx +1.2$ pp (not significant; wide bootstrap CI). These mean-evaluated quantities are the appropriate basis for substantive interpretation and are summarized in the table note for [Table 6](#).

The *linear slope-moderation specification* (Model 3c) extends Model 3b by adding a multiplicative interaction between FL score and curriculum type to test whether the curriculum modifies the *slope* of the literacy–growth association. A curriculum-specific curvature term ($\text{FLScore}^2 \times \text{Integrated}$) would allow each curriculum type its own inverted-U shape but is not estimated given the sample-size constraints of $N = 24$:

$$\text{GDPGrowth}_{it} = \beta_0 + \beta_1 \text{FLScore}_{it} + \beta_2 \text{FLScore}_{it}^2 + \beta_3 \text{Integrated}_i + \beta_4 (\text{Integrated}_i \times \text{FLScore}_{it}) + \gamma \mathbf{X}_{it} + \delta_t + \varepsilon_{it} \quad (3)$$

A positive and significant interaction coefficient β_4 would indicate that integrated curricula amplify the linear growth returns to financial literacy relative to thrift-based curricula. In this model, β_1 and β_2 capture the FL–growth relationship (linear and quadratic) for thrift-based countries ($\text{Integrated} = 0$), β_4 captures the additional slope difference for integrated countries, and the shared β_2 imposes the same curvature for both curriculum types.

Estimation and Inference

All models are estimated by Ordinary Least Squares (OLS). **Primary inference rule:** HC3 heteroskedasticity-robust standard errors are the primary basis for all significance stars, p-values, and significance claims throughout this paper. HC3 applies a stronger finite-sample correction than HC1 and is

specifically recommended for small-sample settings. Bootstrap 95% confidence intervals (999 replications, resampling at the country level to preserve within-country correlation) are reported in all tables as supplementary diagnostic information; where HC3 and bootstrap inferences conflict, both results are noted explicitly in the text.

Within-country dependence: Because the panel contains up to four PISA-wave observations per country, observations from the same country are not independent. HC3 standard errors address cross-observation heteroskedasticity but do not model within-country error correlation. Cluster-robust standard errors would address this correlation but require enough clusters to be reliable—conventional guidance recommends at least 30 to 50 clusters, far more than the 10 countries available here. Country-level block bootstrap resampling (used for supplementary confidence intervals) preserves within-country structure and partially addresses this correlation, but the coverage properties of a block bootstrap with only 10 blocks are also approximate. All reported p-values and confidence intervals should therefore be treated as descriptive approximations; the study’s fundamental limitation is power and sample size, not the specific standard-error formula.

Multiple-testing correction: A Bonferroni adjustment is applied only to the *confirmatory* hypothesis tests corresponding to H1 and H2. The confirmatory family consists of six tests: the coefficients on FLScore and Integrated, each tested across the three primary outcome specifications (private credit, Gini, and GDP growth linear, Model 3a). This yields $\alpha^* = 0.05/6 \approx 0.0083$. The exploratory extension EQ1—comprising the quadratic FL term (Model 3b) and the FL $\times$ Curriculum interaction (Model 3c)—and all *control variables* (gross savings, government education expenditure, secondary enrollment, private credit) are assessed against the conventional $\alpha = 0.05$ threshold and are not subject to Bonferroni correction. Secondary school enrollment is a control variable and is therefore not part of the confirmatory Bonferroni family; its HC3 significance ($p = 0.004$) is assessed at standard $\alpha = 0.05$ and noted as a notable control-variable finding.

Because the sample consists of only 24 country–wave observations drawn from 10 countries across four PISA cycles, statistical power is inherently limited. Wide confidence intervals and non-significant coefficient estimates may reflect insufficient power to detect effects of plausible economic magnitude rather than a true absence of association. All reported estimates should therefore be interpreted as descriptive associations between financial education characteristics and macroeconomic outcomes; given the absence of an exogenous source of variation in curriculum design or financial literacy scores, no causal interpretation is warranted.

Discussion

Interpretation of Main Findings

The central result is a pattern of null estimates. Across all three outcomes, neither country-level PISA financial literacy nor curriculum type survives Bonferroni correction. That pattern needs context, especially given the small sample and the predictions of earlier work.

For H1, the private-credit coefficient on FL score was negative and only marginally significant at 10% ($\hat{\beta}_1 = -0.593$, $p = 0.089$). The sign runs against the hypothesis. That is not a failed result; it is an awkward result, and awkward results are often the ones worth examining. Two explanations are plausible.

First, diminishing returns may matter. All ten countries are high-income OECD economies with mature financial systems, so an additional point of literacy may have little visible effect on aggregate credit depth. Boikos et al. (2025) find larger literacy–growth associations at lower quantiles of the conditional growth distribution, though not specifically in financially developing economies. In an

already-saturated system, more knowledge need not produce more credit.

Second, there may be a “prudent borrower” effect. Financially literate households might avoid high-cost debt, limit leverage, and favor planned saving or diversified investment. Prior research links literacy to borrowing, portfolio choice, and costly debt experiences (Lusardi & Mitchell, 2014; Lusardi & Tufano, 2015; Mitchell & Lusardi, 2015). On this account, literacy could reduce demand for low-value credit while improving the quality of capital allocation. The present outcome cannot separate productive business lending from expensive consumer debt, so this mechanism remains conjectural. Even so, the distinction is worth pursuing: diminishing returns and prudent borrowing imply very different policies.

For H2, neither FL score nor curriculum type was significant in the Gini or growth models. The missing integrated-curriculum premium contrasts with Bucci et al. (2025), whose model allows financial education to improve allocational efficiency and long-run growth. Their framework does not compare curriculum types or directly model inequality, so the closest comparison is our growth specification. Measurement offers one reconciliation. A national document may describe a curriculum as integrated even when classroom instruction remains mostly thrift-oriented. If so, the binary indicator captures policy language better than actual pedagogy.

The Secondary Enrollment Finding

The enrollment coefficient deserves a closer look, even though its bootstrap interval crosses zero. It is best treated as a fragile descriptive signal. Still, its positive direction makes sense. Gross enrollment may capture literacy, numeracy, familiarity with institutions, and the ability to understand contracts—capacities that a cohort-level PISA mean cannot fully represent. This reading is consistent with the education-related patterns reported by Nogueira et al. (2025). Timing also matters. Private credit is a stock shaped by years of decisions from adults, firms, and lenders, whereas PISA measures one cohort of 15-year-olds. Enrollment may therefore proxy for accumulated educational infrastructure. Put simply, broad access to schooling may be the platform on which specialized financial knowledge becomes useful.

Wave Effects and Macroeconomic Context

Wave-year effects were large in every growth model ($p < 0.003$ relative to 2012). The contrast between the eurozone crisis baseline and the later recovery years overwhelms the plausible contribution of curriculum differences. This is a basic identification problem: the macroeconomic tide is much larger than the educational signal. A larger sample or exogenous curriculum reforms would be needed to separate the two.

The marginally significant negative association between private credit and GDP growth in the baseline specification (Model 3a: $\hat{\beta} = -0.040$, $p = 0.033$) is consistent with the finance–growth literature’s observation that very high credit-to-GDP ratios can be associated with financial fragility rather than growth. This pattern is especially visible in the Spanish data, where private credit exceeded 150% of GDP in 2012, coinciding with the height of the country’s sovereign debt crisis.

Connection to Prior Literature

At the program level, Kaiser and Menkhoff (2020) find statistically significant average effects of school financial education on knowledge and behavior. Yet effect sizes vary. Design, treatment intensity, timing, class size, and grade level explain only part of that variation: their Table 3 reports R^2 values

of about 20–42% for knowledge and 6–12% for behavior. Their study does not examine macroeconomic outcomes. It does, however, reinforce a concern here: national PISA means and a binary curriculum label may conceal the classroom details that actually drive learning.

Lusardi and Mitchell (2014) review the theory and evidence linking financial literacy to financial decision-making and economic outcomes. Building on that review and the broader literature, we interpret financial literacy as potentially necessary but not sufficient for financial well-being because institutional conditions, including the depth and accessibility of financial markets, may shape whether knowledge translates into productive capital allocation; this institutional-moderation claim is our interpretive synthesis rather than a proposition directly tested in their article. In economies where financial markets are already deep and accessible, as in the OECD sample examined here, the marginal contribution of additional financial education may simply be smaller. The quantile-varying pattern documented by Boikos et al. (2025) is related but distinct: they find larger financial-literacy growth effects at lower quantiles of the conditional GDP growth distribution, not specifically in less financially-developed economies.

Policy Implications

Policy implications should remain modest. Secondary enrollment is the only control with conventional HC3 significance ($p = 0.004$), but its bootstrap interval crosses zero. Expanding educational access may complement financial-literacy reform; it should not be presented as a proven route to deeper capital markets. Targeting enrollment gaps among lower-income and rural students is plausible policy, but the financial-market benefit remains a hypothesis.

The curriculum distinction also remains policy-relevant. A non-significant coefficient from 24 observations is not evidence that content has no effect. The practical lesson is narrower: future evaluations need larger samples and richer information about what teachers actually teach.

Third, the dominance of wave-year fixed effects in explaining GDP growth variation—capturing common shocks such as the post-crisis recovery—implies that macroeconomic stabilization policy may exert a larger short-run influence on growth outcomes than educational policy reforms. Financial literacy curricula, whatever their ultimate effects, operate on a timescale of decades; policymakers should calibrate expectations accordingly and avoid seeking near-term macroeconomic payoffs from curriculum redesign alone.

Limitations

Several limitations narrow the conclusions. Most important is power. With 24 observations from 10 countries, the minimum detectable OLS effect is larger than the effect sizes that prior research would usually regard as economically plausible.

Beyond power, the curriculum type variable is a time-invariant binary classification derived from national strategy documents that may not accurately reflect actual pedagogical practice at the classroom level. Significant heterogeneity in instruction quality, teacher training, and assessment culture within each country is collapsed into a single binary indicator, introducing measurement error that would attenuate coefficient estimates toward zero.

The documentary basis for classifying Israel and Norway is less definitive than for the other countries and should be verified against current ministry or curriculum documents; because one miscoding could materially affect a 10-country sample, future robustness checks should drop or recode these two cases.

Finally, the absence of an exogenous source of variation in curriculum design means that selection bias cannot be ruled out. Countries that adopt integrated curricula may differ from thrift-based countries in institutional, cultural, or economic dimensions not fully captured by the observed controls, potentially confounding the estimated curriculum effects. Future research employing a differences-in-differences design around documented curriculum reform events would provide substantially stronger identification.

Conclusion

This study examined financial literacy, curriculum design, and three macroeconomic outcomes: private credit depth, inequality, and real GDP growth. The panel covers ten OECD nations and four PISA cycles from 2012 to 2022. A broader Asian savings puzzle motivated the question, but East Asia is not in the sample because participation and comparable curriculum documents were insufficient. The scope of inference is therefore narrow by design: selected high-income OECD economies, not high-saving economies as a whole.

The main finding is a set of non-significant associations. Neither PISA scores nor the integrated-versus-thrift-based distinction is significant for any outcome after Bonferroni correction. Gross secondary enrollment is the exception under HC3 inference ($\hat{\beta} = 1.953$, $p = 0.004$), but its bootstrap interval includes zero. That conflict rules out a firm claim. At most, enrollment offers a tentative signal that broad educational access may accompany deeper credit markets.

Power constraints shape every result. A 24-observation panel cannot reliably detect many effects of plausible economic size. Curriculum may matter even though this design cannot see it. The estimates are best read as a descriptive baseline, not a verdict.

Even so, null evidence is useful. After controlling for wave effects and broad educational access, this panel shows no statistically reliable macroeconomic association for financial literacy scores or curriculum type. The result narrows what can be claimed from these data. It does not establish zero effects. Policymakers should resist that leap and wait for larger studies with better measures of classroom practice.

Future work can improve on the design in three ways. A broader sample, including additional OECD participants and emerging economies, would raise power and institutional variation. It would also show whether the quantile pattern in Boikos et al. (2025) and the curriculum mechanisms in Bucci et al. (2025) travel beyond the present sample. Documented curriculum reforms could support a differences-in-differences design. Classroom evidence—teacher training, lesson-plan audits, and student financial behavior—would address the most stubborn measurement problem: what is written in a national strategy is not always what happens in a classroom.

The research agenda remains open. Governments continue to expand financial education mandates, often with high expectations and limited cross-national evidence. This paper offers a quieter lesson: careful null results help set those expectations. Sometimes the honest answer is that the available data are not yet strong enough.

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