

Assessing the Efficacy of Financial Literacy Programs among Teenagers

Mr. Arnav Ajit Pandey

Abstract

Thirty-nine US states required personal finance coursework for high school graduation in 2026, up from fewer than half the states four years earlier. The requirement spread while the research literature was in open disagreement about whether such coursework changes anything a policymaker would care about. This review assesses what the experimental and quasi-experimental record establishes about adolescent financial education: which outcomes move, by how much, for how long, and through what mechanism. The field's defining dispute, between Fernandes, Lynch and Netemeyer's 2014 finding that interventions explain 0.1 percent of variance in financial behaviour and Kaiser, Lusardi, Menkhoff and Urban's 2022 estimate of 0.100 standard deviations across 76 randomized experiments, traces to three choices about sample, statistical model and effect-size metric. Adolescents convert instruction into behaviour more readily than children or adults, at 0.120 standard deviations for 14- to 25-year-olds. Large trials in Brazil, Peru and Germany show knowledge gains at low per-pupil cost, improved repayment three years after graduation, and more time-consistent choices in incentivized tasks. The Brazilian program also increased expensive credit use and late repayment, demonstrating that a program can meet its knowledge and savings objectives while leaving participants worse off elsewhere. Turkish and German evidence suggests the operative mechanism is patience and decision quality rather than content mastery. Long-run consequences for adult financial welfare remain substantially unmeasured.

Keywords: financial literacy; adolescents; school-based financial education; meta-analysis; state mandates; intertemporal choice; PISA

1. A policy that moved faster than its evidence

Thirty-nine US states required high school students to complete personal finance coursework as a condition of graduation in 2026, according to the Council for Economic Education's biennial Survey of the States. Two years earlier the count was thirty-five. In 2022 it was fewer than half the states. California, Colorado, Delaware and Hawaii adopted semester-long requirements in the most recent cycle alone, affecting an estimated 2.3 million students. Definitions matter here: the thirty-nine figure combines twenty-six states with a standalone course and thirteen where personal finance content sits inside another required subject, usually economics. Next Generation Personal Finance, which counts only guaranteed standalone semesters, puts the figure at thirty.

Legislatures rarely expand a graduation requirement this quickly. The expansion happened while the research literature was in open disagreement about whether school-based financial education changes anything a policymaker would care about. Between 2014 and 2022 the same body of studies was read as showing near-zero effects and as showing effects comparable to mainstream educational interventions, and both readings appeared in leading journals.

The gap that the coursework is meant to close is real enough to measure. In the PISA 2022 financial

literacy assessment, covering twenty countries and economies, 18 percent of students across the fourteen participating OECD countries fell below baseline proficiency, meaning they could not apply financial knowledge to ordinary situations. Only 8 percent reached Level 5, the top band, where students handle non-routine problems and unfamiliar products. The Netherlands and the Flemish community of Belgium put more than 15 percent of their students in that band; several participating systems put under 1 percent there. Socio-economically advantaged students outscored disadvantaged students by 87 points on average, more than a full proficiency level, and family background alone accounted for roughly 12 percent of within-country variation.

Meanwhile the exposure is not hypothetical. Roughly 60 percent of 15-year-olds across the OECD sample held a bank account or a payment card, and more than 85 percent had bought something online in the preceding year. In the United States the share of students holding a payment or debit card rose from 14.3 percent in 2012 to 52.3 percent in 2022. Teenagers are transacting whether or not anyone teaches them to.

This review examines what the experimental and quasi-experimental record actually establishes about adolescent financial education: which outcomes move, by how much, for how long, and through what mechanism. The literature's central methodological quarrel gets extended treatment because the size of the reported effect turns out to depend heavily on choices that look technical and are not.

2. What "efficacy" is being claimed

Three distinct outcomes get bundled under the word. The first is measured financial knowledge, typically a test score on questions about interest, inflation and risk diversification. The second is self-reported or administratively observed behaviour: saving, budgeting, borrowing, insurance take-up, repayment. The third is financial welfare, which almost nobody measures directly because it requires either long panels or a defensible model of what optimal behaviour would have been.

Conflating the first with the second produced much of the confusion in this field. Knowledge tests are proximate to the intervention and often written by the same people who designed the curriculum, which inflates measured gains. Behaviour is distal, noisy, and frequently constrained by circumstances a course cannot alter. A teenager who learns how compound interest works but has no income cannot demonstrate the learning through savings behaviour for several years, by which point most study funding has expired.

Lusardi and Michaud's model with Mitchell (2017) sharpens the point in a way most evaluations ignore. If financial knowledge is a costly investment with returns that depend on the scale of one's assets, then optimal knowledge is low for people with few assets. A young person who declines to acquire or act on financial knowledge may be behaving efficiently, not myopically. Kaiser, Lusardi, Menkhoff and Urban (2022) concede the implication directly: determining which behaviours should optimally change requires a theoretical framework the literature often lacks. Without one, a null result on behaviour is uninterpretable. It may indicate a failed program, or a program that worked on students who had nothing to apply it to.

The measurement problem compounds when outcomes are self-reported. Students who have just completed a savings unit know what answer the survey wants. Only a handful of studies escape this by linking to administrative records, and those studies form the most credible part of the evidence base.

3. The meta-analytic dispute

Daniel Fernandes, John Lynch and Richard Netemeyer published the first meta-analysis of the field in *Management Science* in 2014, covering 168 papers and 201 studies. Their headline result became the most cited finding in the literature: interventions to improve financial literacy explained 0.1 percent of the variance in the financial behaviours studied. Effects were weaker in low-income samples, and interventions of any size showed negligible effects on behaviour measured twenty months or more after delivery. They proposed a narrowed role for "just-in-time" education attached to specific decisions, and a larger role for choice architecture and regulation.

The finding was widely read as a verdict. It was not obviously one. Fernandes and colleagues drew their strongest conclusion, that randomized evidence shows no behavioural effect, from thirteen randomized controlled trials yielding fifteen coded observations. Their weighted average effect across those RCTs was 0.018 standard deviations, with a confidence interval spanning zero.

Tim Kaiser and Lukas Menkhoff reopened the question with a series of meta-analyses using different inclusion rules and different statistical assumptions. Their 2017 paper in the *World Bank Economic Review* found positive average effects and identified moderators, particularly intervention intensity and the presence of a teachable moment. Their 2020 paper in *Economics of Education Review*, restricted to the school setting and covering 37 quasi-experiments and experiments, reported an average effect on financial knowledge of

0.25 standard deviations and a smaller but still significant effect on behaviour of 0.05 standard deviations. The pattern held when the sample was cut to the 18 randomized studies. Metaregressions showed higher effects from more intensive treatments and smaller classes, with diminishing marginal returns.

The decisive contribution came in 2022, when Kaiser, Lusardi, Menkhoff and Urban published a meta-analysis of 76 randomized experiments with more than 160,000 participants in the *Journal of Financial Economics*. Their estimate of the average treatment effect on financial behaviours was 0.100 standard deviations, with a 95 percent interval from 0.071 to 0.129. On financial knowledge the estimate was 0.204 standard deviations, interval 0.152 to 0.255. The behavioural estimate is roughly 5.5 times the figure Fernandes and colleagues reported.

Three differences generate that gap, and none of them is a matter of one team being careless.

The first is sample. The field grew steeply after 2014. Thirteen RCTs became seventy-six. Fernandes and colleagues had, by their own account, selected the subset of the literature that showed the weakest effects and argued it was the most credible subset. That argument was defensible in 2014 and was overtaken by data.

The second is the statistical model. Fernandes and colleagues assumed a single true effect across studies and weighted by inverse variance accordingly. Kaiser and colleagues tested that assumption and rejected it: formal heterogeneity tests attributed 86.4 percent of observed between-study variance to genuine differences in true effects rather than sampling error. A Cochran's Q of 464.71 leaves little room for the common-effect assumption in a literature spanning brochures and year-long courses across 32 countries. Under a random-effects specification with robust variance estimation, the pooled estimate rises. Even holding the common-effect assumption fixed and simply updating the data, the estimate rose to 0.065 standard deviations, an interval that excludes zero.

The third is the effect-size metric, and this is where the disagreement stops being technical. Fernandes and colleagues reported variance explained, a squared correlation. Kaiser and colleagues argue the metric manufactures the appearance of triviality. Take the median effect of structured pedagogy

interventions in developing countries, about 0.13 standard deviations. Converted to a correlation, it explains roughly 0.36 percent of variance in learning outcomes, which sounds like nothing. Evans and Yuan (2019) estimate the same effect as equivalent to between 0.6 and 0.9 years of ordinary schooling, with a net present value in Kenya of around \$1,338 against average annual income of \$1,079. Squaring a small correlation makes any education intervention look futile, including ones nobody disputes.

Kaiser and colleagues also attempted to replicate the original RCT estimate and could not, documenting four coding errors in the classification of effect sizes. They report this in an appendix without drawing conclusions from it, which is the right handling; the substantive disagreement does not rest on the errors. Against Kraft's (2019) benchmarks for education research, where effects above 0.20 standard deviations count as large and 0.05 to 0.20 as medium, the knowledge effect sits at the top of the range and the behavioural effect in the middle. The costs are modest: among the twenty studies reporting them, mean per-participant cost was \$60.40 and the median \$22.90 in 2019 dollars.

4. Where the age evidence points

Disaggregating by participant age matters for a review focused on teenagers, and the pattern is not the intuitive one.

Kaiser and colleagues estimate the behavioural effect for youth aged 14 to 25 at 0.120 standard deviations, above the adult estimate of 0.107 and well above the estimate for children under 14, which is 0.064. Knowledge effects run the other way: children show the largest gain at 0.276 standard deviations, though based on only 15 estimates across seven studies with an interval running from 0.008 to 0.545. Youth show 0.186, adults 0.200.

Adolescents therefore occupy a useful position. They learn nearly as much as younger children and convert learning into behaviour more readily than any other age group in the sample. The most plausible explanation is proximity of application. A 15-year-old with a debit card and a part-time job faces decisions within months of instruction; a 10-year-old does not; an adult may have already committed to a mortgage or a pension default that the course cannot undo.

Kaiser and Menkhoff's school-focused meta-analysis complicates this slightly. Within schools specifically, knowledge effects exceed the all-ages average while behaviour effects fall below it. They attribute the split to learning capacity being high in young people while the motivation to convert knowledge into action depends on immediacy of financial decisions, which schooling does not supply. Both readings can be true: adolescence beats childhood and later adulthood on conversion, and classroom delivery still converts worse than education delivered at a decision point.

5. Field experiments in schools

Three large randomized trials carry most of the weight for adolescents specifically, and their results are more instructive read together than separately.

Miriam Bruhn and colleagues evaluated a comprehensive high school program across 892 schools in six Brazilian states, reaching roughly 25,000 students, and published the results in *American Economic Journal: Applied Economics* in 2016. The program raised financial proficiency by a quarter of a standard deviation and raised grade-level passing rates, so it did not crowd out other learning. Savings and budgeting improved. Effects spilled over to parents, whose financial behaviour also shifted.

It also increased students' use of expensive credit to finance consumer purchases, and raised the likelihood of falling behind on repayments. The program taught students what credit products were and how they worked, and some students responded by using them. Bruhn and colleagues raise willpower depletion as one candidate explanation: students spending self-control on savings and budgeting decisions may have had less available for consumption restraint. Whatever the mechanism, the result is the most important single finding in the adolescent literature, because it demonstrates that a program can succeed on its knowledge objective, succeed on two of its behavioural objectives, and still leave participants worse off on a third.

Verónica Frisanchó's evaluation of *Finanzas en mi Colegio* in Peru, published in *The Economic Journal* in 2023, is the strongest study on persistence. Nearly 20,000 students in public high schools received mandatory personal finance lessons in the final three grades. Exit exams five months in showed knowledge gains of 0.16 standard deviations, with no cost to performance in other subjects. Frisanchó then matched students to Peruvian public credit bureau records three years later, one to two years after graduation. Treated students were less likely to carry negative records from unpaid or delinquent bills and credit card statements. The treatment did not change whether students borrowed at all; among the minority who held outstanding loans, repayment behaviour improved.

Two features of the Peru study deserve emphasis over its headline result. Per-pupil cost was \$4.80, and a dollar of additional spending corresponded to roughly 3.3 points on the PISA financial literacy scale. And teachers, who delivered the curriculum after training, recorded knowledge gains roughly double those of their students, became more likely to save through formal channels, borrowed more from banks and reduced their own delinquency rates. Frisanchó's separate work on parental spillovers found limited average effects but sizeable ones within disadvantaged households. The programme's returns, in other words, accrued substantially to adults who were not its target.

Melanie Lührmann, Marta Serra-Garcia and Joachim Winter took a different approach in Germany, randomizing a short financial education program among high school students and measuring outcomes through an incentivized choice experiment rather than a survey. Their results, published in *American Economic Journal: Economic Policy* in 2018, show treated students making more time-consistent intertemporal choices, allocating payments to a single date rather than splitting across dates, and displaying greater consistency with the law of demand. Their earlier work in the *Journal of Banking and Finance* had established that the same short course moved knowledge and interest in financial matters.

The German design answers a question the Brazilian and Peruvian designs cannot. Incentivized experimental choices are not self-reports and not subject to demand effects in the same way. What moved was decision quality, specifically a reduction in narrow bracketing, the tendency to evaluate financial choices in isolation rather than as part of a portfolio of decisions.

6. What the classroom competes with

Financial education for adolescents does not arrive on blank ground. It arrives after roughly fifteen years of household financial socialization and alongside a peer environment that shapes spending in real time. PISA 2022 measured both for the first time at scale, and the findings constrain what any semester course can plausibly achieve.

Across the fourteen OECD countries assessed, 83 percent of 15-year-olds reported deciding independently how to spend their own money, and those students scored about 30 points higher on the financial literacy assessment than students who did not. The direction of causation is unclear and

probably runs both ways; parents who judge a child competent grant more autonomy, and autonomy generates practice. Ninety-three percent reported saving money at least once in the preceding twelve months, which means the behaviour most curricula are designed to instil is already near-universal in some form. What varies is not whether teenagers save but how well they manage the tension between saving and the spending their friends encourage, which PISA identified as a live influence on adolescent purchasing.

The OECD's assessment of school exposure is more damaging to the mandate case than the mandate counts suggest. Only about two in three students reported ever encountering tasks in school that explored financial issues, and those who had performed better. Requirements on paper and instruction in classrooms are different quantities, separated by teacher availability, timetabling and the choices individual schools make about how to satisfy a standard. The gap between the thirty-nine states counted by the Council for Economic Education and the thirty counted by Next Generation Personal Finance is a small instance of a general problem: a state can satisfy a mandate by folding a unit into an economics course taught by someone with no preparation in personal finance, and the evaluation literature will record that student as treated.

Socio-economic background compounds this. The 87-point advantage PISA recorded for advantaged students exceeds a full proficiency level, and 12 percent of within-country variation in financial literacy traces to family background. Universal mandates are usually defended on equity grounds, and the equity argument is the strongest one available, since students whose parents cannot teach them have nowhere else to learn. But the same background factors that depress baseline scores also predict weaker school resources and less-prepared teachers, so a universal requirement can widen the gap it was enacted to close.

Cross-country variation in program effects fits this pattern. Kaiser, Lusardi, Menkhoff and Urban found treatment effects on financial knowledge roughly 46 percent smaller in developing economies than in high-income ones, a statistically significant difference. Effects on behaviour differed by only about 10 percent and not significantly. Knowledge transmission appears to depend on the surrounding education system in a way that behaviour change does not, which suggests behaviour change is running through some channel other than content mastery.

7. The behavioural channel

The German finding fits a body of work suggesting that what changes in successful adolescent programs is not primarily knowledge.

Sule Alan and Seda Ertac ran a randomized educational intervention in Turkish schools designed to improve children's ability to imagine future selves and encourage forward-looking behaviour, delivered by the children's own trained teachers. Published in the *Journal of Political Economy* in 2018, the study found treated students making more patient intertemporal choices in incentivized tasks, with effects persisting almost three years, replicating in a separate sample, and robust across elicitation methods. The effects extended beyond experimental tasks: treated students were significantly less likely to receive a low behaviour grade at school. Alan and Ertac trace the conduct improvement to improved ability to defer consumption rather than to any content the curriculum conveyed.

The curriculum contained no compound interest formulas. It targeted a preference parameter that most economic models treat as fixed.

Shawn Cole and colleagues reached a compatible conclusion from a different direction. Carpena, Cole,

Shapiro and Zia, working in India and publishing in *Management Science*, separated financial education's effects on numeracy from its effects on attitudes and awareness, and found the latter far more tractable than the former. Drexler, Fischer and Schoar's rule-of-thumb training, published in *American Economic Journal: Applied Economics* in 2014, made the same case from the design side: simplified heuristics outperformed standard accounting instruction among microentrepreneurs with limited schooling.

Read together, these studies suggest that programs teaching adolescents *how to think about* money outperform programs teaching them *facts about* money, and that the effective ingredient may be closer to self-regulation training than to financial instruction. That reframing is not yet reflected in most state standards, which enumerate content domains.

It also cuts against a common defence of financial education. If the mechanism is preference change rather than information transfer, then arguments about the marketplace changing faster than any curriculum lose some force, but so do arguments that more content hours will help.

8. Long-run outcomes from US mandates

Randomized trials rarely follow participants long enough to observe adult financial outcomes. The US state mandates supply a quasi-experimental substitute, comparing cohorts graduating just before and just after a requirement took effect, within and across states.

This literature contains an instructive early contradiction. Bernheim, Garrett and Maki (2001) examined state mandates enacted between 1957 and 1982 and found improved adult savings behaviour. Cole, Paulson and Shastry, in the *Journal of Human Resources* in 2016, studied the same mandates with state fixed effects and found no effects on investment or credit management. The mandates of that era frequently amounted to a visiting bank representative delivering a single lecture rather than a course, which makes the null unsurprising and makes both papers weak evidence about contemporary requirements.

The modern mandates have been studied with administrative credit data. Brown, Grigsby, van der Klaauw, Wen and Zafar, writing in the *Review of Financial Studies* in 2016, found that financial education requirements reduced non-student debt and improved credit outcomes among young adults. Urban, Schmeiser, Collins and Brown, in *Economics of Education Review* in 2020, examined credit report outcomes for 18- to 21-year-olds and found fewer defaults and higher credit scores. Their qualification is as important as their result: state-level heterogeneity was substantial, and they concluded that well-funded teacher preparation may be what distinguishes states where the mandate worked from states where it did not.

Stoddard and Urban, in the *Journal of Money, Credit and Banking* in 2020, examined college financing and found mandated students more likely to complete the FAFSA, more likely to borrow from federal rather than private sources, more likely to receive grants and scholarships, and less likely to carry credit card balances. Effects on federal borrowing and credit card use were larger for low-income students, who were also less likely to work while enrolled. The mechanism is legible: students shifted between financing instruments with different prices, which is exactly what a course covering financial aid would be expected to produce.

Daniel Mangrum, in the *Journal of Financial Economics* in 2022, sharpened the outcome measure. Federal student loan borrowers in difficulty can enter income-driven repayment or forbearance and thereby avoid delinquency, which means default rates understate variation in repayment progress. Using

College Scorecard repayment rates instead, Mangrum found no effect of mandates on default, consistent with prior work, but a positive effect on repayment progress. Melody Harvey, in the *Journal of Consumer Affairs* in 2019, found mandates reduced young adults' use of alternative financial services, payday lending among them.

Carly Urban's 2023 paper in *Economics of Education Review* addressed the most common objection to graduation requirements, that adding a course displaces something else and pushes marginal students out. She found no evidence that state-mandated financial education reduced high school graduation rates.

The pattern across these papers is consistent and narrow. Mandates shift behaviours that are proximate to course content and mediated by an identifiable choice: which loan to take, which repayment plan to enter, whether to use a payday lender. They do not reliably shift outcomes determined mainly by income and shocks, which is where default sits.

9. The case against

Lauren Willis has pressed the strongest sustained critique, in the *Iowa Law Review* in 2008, the *San Diego Law Review* in 2009, and *American Economic Review: Papers and Proceedings* in 2011. Her arguments are structural rather than empirical, and the newer meta-analytic evidence does not dispose of them.

Willis argues that the financial marketplace changes faster than any curriculum can track, so content taught at seventeen is partly obsolete at thirty. She argues that financial decisions engage cognitive biases that information does not correct, and that firms have both the incentive and the capability to exploit those biases faster than education can inoculate against them. She argues that consumers are not expected to serve as their own physicians or attorneys and should not be expected to serve as their own financial advisers. And she raises a possibility that the effectiveness debate tends to skip: education that raises confidence more than competence makes consumers worse off, because overconfident consumers stop seeking advice.

That last point interacts uncomfortably with the Brazilian result. Students who learned about credit products used more of them and fell behind on repayment more often. Whether that reflects genuine improvement in access or the confidence effect Willis describes is not resolved by the data.

Willis also frames the comparison correctly. The relevant question is not whether financial education has a positive effect but whether it outperforms alternatives on the same budget: product regulation, default settings, disclosure redesign, or restricting the products themselves. Kaiser and colleagues' cost figures make financial education look cheap in absolute terms. They do not establish that it is the best available use of the money, and no study in this literature attempts that comparison directly.

Two considerations push back. The first is externalities. Haliassos, Jansson and Karabulut, writing in the *Review of Financial Studies*, document that financial literacy transmits between households, so the social return to educating one person exceeds the private return. The Brazilian and Peruvian trials found the same thing inside families and schools: parents and teachers changed their behaviour because students were taught. Frisanco's Peruvian teachers gained twice what their students did and reduced their own delinquency rates, which means a cost-effectiveness calculation restricted to student outcomes understates the return. Willis's comparison to physicians and attorneys also assumes that competent, affordable financial advice is available to households of ordinary means, which for the teenagers this literature studies it generally is not.

The second concerns obsolescence. The claim that curricula date faster than markets change is strongest against content-heavy instruction and weakest against the interventions that have performed best in randomized trials. Patience, planning horizon and resistance to narrow bracketing do not become obsolete when a new product launches. If Alan and Ertac's Turkish results and the German intertemporal-choice findings identify the operative mechanism, then Willis's obsolescence argument indicts one design of financial education rather than the category.

Neither response rescues the version of the policy that most state standards encode, which is a content survey delivered once in eleventh or twelfth grade by a teacher without specialist preparation. Willis's critique lands squarely on that version, and the empirical literature does not defend it.

10. What predicts an effective program

The moderator evidence is thinner than the average-effect evidence, and should be read as suggestive.

Intensity matters, with diminishing returns. Kaiser and Menkhoff found more intensive treatments and smaller classes produced larger effects, but the marginal return falls off. The median intervention in the Kaiser, Lusardi, Menkhoff and Urban sample involved seven hours of instruction; the mean was 11.7, inflated by a tail of long programs. Most of what has been evaluated is far shorter than a semester course, which means the mandate literature and the RCT literature are not measuring the same treatment. Teacher preparation appears repeatedly. Urban and colleagues attribute cross-state variation in mandate effects to it. Frisanco's Peruvian teachers gained twice what their students did, which suggests teachers began well short of mastery. A curriculum delivered by an under-prepared instructor is a different intervention from the same curriculum delivered by a trained one, and the evaluation literature rarely distinguishes them.

Timing relative to a decision matters, which is the one point on which Fernandes and colleagues and their critics broadly agree. Kaiser and Menkhoff estimate that teachable-moment delivery adds something in the range of 0.05 to 0.07 standard deviations. Fernandes and colleagues built their entire policy recommendation around it. For adolescents this argues for placing coursework adjacent to real decisions, financial aid applications and first credit accounts among them, rather than in whichever grade has curricular room.

Active learning methods, including simulation and experiential formats, associate with larger effects in Kaiser and Menkhoff's work and in Amagir and colleagues' review of programs for children and adolescents. The evidence here is correlational within the meta-analytic sample rather than experimental, since almost no study randomizes pedagogy while holding content fixed.

11. What remains unsettled

The persistence question is genuinely open, and the honest summary is that the literature cannot answer it. Kaiser and colleagues found some decay between short-run and six-month measurement, with behavioural effects falling from 0.099 to 0.071 standard deviations, and then no further significant decline: 0.088 at twelve months or more, 0.065 at eighteen, 0.057 at twenty-four. Those later estimates rest on seven studies. The confidence intervals cannot rule out sustained effects and cannot rule out effects near zero. On financial knowledge, no study in their sample measured outcomes beyond eighteen months at all. Fernandes and colleagues' claim of rapid decay was drawn from a meta-regression with four covariates on 29 observations, which does not support the weight it has carried.

Frisanco's three-year credit bureau follow-up in Peru and the US mandate studies partially fill this gap,

and they point in a modestly positive direction. Both rely on administrative records rather than self-report, which is the right direction of travel for the field. Neither observes outcomes deep enough into adulthood to speak to retirement saving, home purchase or wealth accumulation, which is where the policy rationale ultimately rests.

The welfare question is barely addressed. Almost every study codes more saving and less borrowing as improvements. For a liquidity-constrained 19-year-old financing education, more borrowing may be optimal and less borrowing a cost. Stoddard and Urban's finding that mandated students shifted from private to federal loans is one of the few results in the literature with an unambiguous welfare interpretation, because the price difference between the instruments is known.

Publication and specification choices deserve continued scrutiny. Kaiser and colleagues tested for publication selection bias and found their conclusions robust to correction, and found no significant difference between studies published in top economics journals and elsewhere. That is reassuring about the average. It says nothing about whether the outcomes researchers choose to report are the ones that policy should track.

For adolescents specifically, the record supports a bounded claim. School-based financial education reliably raises measured financial knowledge by an amount comparable to what good interventions achieve in mathematics and reading. It produces smaller but detectable changes in financial behaviour, larger for 14- to 25-year-olds than for any other age group. It shifts choices between financial instruments more reliably than it shifts outcomes driven by income. It can produce harm alongside benefit when it makes unfamiliar credit products salient without building the restraint to use them well. Its per-participant cost is low relative to its measured effect. And its long-run consequences for adult financial welfare remain, after two decades of research and forty state legislatures acting on the premise, substantially unmeasured.

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