

The Effects of Game-Based Financial Education on Adolescents' Financial Literacy, Risk Preferences, and Economic Decision-Making: A Narrative Review

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Abstract

Adolescents increasingly encounter digital payment systems, E-brokerages, and investment content before they take on meaningful financial responsibility, and schools have responded with new instruction that incorporates games and simulations. This narrative review deals with the impact of game-based financial education on adolescents. Drawing on randomized and quasi-experimental studies of escape rooms, board games, and online games, along with meta-analytic work on financial education more broadly, three outcomes have consistently been shown: performance on knowledge assessments, choices within the tasks, and daily financial coordination. The evidence for the first is consistent, for the second suggestive, and for the third uncertain. The key variable that determines a program's success is its design, not the format - the accuracy of its content, the realism of its constraints, the responsiveness of its feedback, and the rewards. The review argues that game-based education is effective enough to stand alongside direct instruction rather than replace it. It identifies longitudinal measurement against real financial conduct as the field's central unmet need.

I. Introduction

The order in which young people acquire

financial exposure and responsibility has reversed. A fifteen-year-old adolescent can now

move funds between accounts or open a cryptocurrency position from a phone, long before they earn significant income. This does not mean adolescents make mistakes in every financial decision, but that they make decisions without the experience and guidance that once preceded them.

Financial education is the standard institutional response, and its justification relies on empirical association. Across nations, the positive correlation between financial knowledge measures and welfare was omnipresent (Lusardi & Mitchell, 2014). Despite greater wealth accumulation, the association cannot establish the effects of instruction. Knowledge and conduct may follow from something else - numeracy, parental modeling, income stability - and a program that raises the first may overshadow the second. Hasting et al. (2013) caution on this point, arguing that the effectiveness of financial education programs is separate from the empirical question of the correlation between literacy and success, and that the evaluation has not fully resolved it.

Both claims are testable for the game-based instruction. One claim is that decisions practiced in games transfer more readily than in traditional education. The counter-claim is equally coherent, arguing that a scoring rule is a designer's choice, not a feature of financial reality. To address these two claims, this study uses a narrative review approach. The authors selected relevant theoretical and empirical studies, with particular attention to adolescent participants, game-based

or simulation-based education, and measurable educational or behavioral outcomes. The review prioritized randomized and quasi-experimental studies when assessing intervention effects, while using broader reviews and theoretical studies to define key concepts. Since the studies differed in their participants, instructional formats, outcome measures, and follow-up periods, the evidence was synthesized thematically rather than statistically.

II. Conceptual Framework

1. Financial Literacy, Capability, and Behavior

Three finance-related concepts constantly appear in public discussion. Financial literacy is defined as what a person fully understands: the relationship between interest and inflation, the relationship between risk and expected return, and the instruments that allow money to flow in the market. Financial capability augments the means and opportunity to act - an account to save into, income beyond subsistence, and an institution willing to transact. Financial behavior records what people save, borrow, and hold.

An adolescent may answer each literacy item correctly without income to allocate, making capability a binding limit rather than expressive knowledge. Another could choose to save because a parent established the habit, without justifiable reasoning. A third may understand the arithmetic of credit and carry a balance because cash flow, not comprehension, is the constraint.

Most financial education evaluations assess literacy because it is low-cost and yields immediate results. Behavior requires either self-report, which

is subject to bias, or administrative records, which cannot capture all data sets and require longer follow-up periods. As a result, the literature speaks confidently about the first construct and inferentially about the third. This distinction in view is the simplest protection against reading more into a positive result than it contains.

2. Game-Based Financial Education

Game-based learning manipulates the primary learning environment, whereas gamification adds selected game elements to an otherwise conventional activity. Serious games concentrate on education, while simulations reproduce selected features to create real financial encounters.

This distinction shows different learning mechanisms rather than different labels. In games, rules carry instructional content, and students acquire knowledge by identifying system rewards (Plass et al. 2015; Michael & Chen 2006). When game elements are layered onto conventional instruction, the content remains unchanged, and the elements sustain attention (Deterding et al. 2011). A point system may enhance participation level without changing what the lesson conveys. In contrast, a simulation conveys through its structure - either an accurate model of a financial constraint or a distorted one, depending on its construction. Boyle et al. (2016) reviewed empirical work on digital and serious games across educational domains. It reported engagement benefits and learning while emphasizing that outcomes varied considerably

with design and evaluation degree. Thus, a finding that “games work” is too coarse to guide practice; the question is which features of which games produced which effects. Evidence from game-based university lectures outside finance, or from business simulations for adult learners, may illuminate general mechanisms of engagement but does not explain financial behavior enough.

3. Behavioral Biases and Financial Decision-Making

Choices under financial uncertainty differ from consistent value-maximization patterns, and that matters because they predict students’ responses to a game’s structure. Prospect Theory (Kahneman & Tversky, 1979; Tversky & Kahneman, 1992) describes several key features. Outcomes are evaluated as gains or losses relative to the reference point rather than as final wealth - reference dependence. Losses are weighted more heavily than gains of equal magnitude - loss aversion - though the magnitude of that asymmetry varies substantially across samples and approaches (Walasek et al. 2024). Low probabilities are treated as greater than they are, while moderate and high probabilities are treated as smaller - probability weighting. Present bias, in turn, causes immediate rewards to dominate delayed ones, which reverses when both are moved into the future.

Applied to game design, each of these has concrete implications. A simulation that displays each student's standing relative to a class leader installs a reference point, and any student below

it operates in the loss domain. In this region, risk-seeking is most reliably observed. A game with rare, large payoffs engages probability weighting and will make concentrated positions feel more attractive than their expected value warrants. A game whose consequences all resolve within a single class period cannot engage present bias at all, and therefore cannot teach anything about it, whatever the accompanying lesson plan states.

Prospect theory is used here only in this auxiliary capacity. The argument is not that adolescents are irrational, nor that the theory needs defending. It is that a game's reward structure determines which behavioral tendency the game exercises, and that this is a design decision rather than an incidental feature.

III. Effects of Game-Based Financial Education

1. Financial Knowledge and Engagement

Experimental and quasi-experimental studies generally report positive short-term effects on financial knowledge and engagement. However, the magnitude of improvement varies by the duration of instruction, the game content, and the outcome measure used.

The most informative feature of the Stock Market Game evaluation by Hinojosa et al. (2010) is not that it found an effect but that the effect behaved differently across two outcome domains. Financial knowledge rose consistently for participating students throughout the Grades 4–10 range covered by the trial. Mathematics performance did not: gains concentrated among younger participants and became unreliable in

the upper grades. If the simulation worked purely by making abstract content concrete, one would expect the two to track each other. Their divergence suggests the mechanism is domain-specific—the game taught the content it modeled, and transfer to an adjacent skill was weaker and age-dependent. The trial's randomized design and national scope make this a credible result about what the game conveyed, and a silent one about what participants did with it afterward.

Cannistrà et al. (2024) provide the strongest causal claim in this cluster. Their online Financial Escape Room reached 2,220 secondary students across several countries, with assignment occurring at the school level, which removes the contamination problem that arises when classrooms within a school are split. Treatment-group performance exceeded control on content covering interest calculation, account management, and saving. The design supports the inference that the activity caused the score difference. It does not support any inference about duration because measurement occurred close to the intervention, and this is a structural limitation rather than an oversight—the study was designed to establish causation, and it did.

Reisdorfer-da-Silva et al. (2025) shift the setting. Working in Brazilian public schools with a quasi-experimental design, they trained teachers before introducing financially themed board games and then measured knowledge, attitudes, and self-reported behavior. Participating students scored higher across the measured dimensions. The absence of randomization weakens the causal

claim relative to the two studies above. Still, the study contributes something they lack: it tests a low-technology format in a resource-constrained public system, where the digital infrastructure assumed by the escape room may not exist.

Read together, these studies converge on short-term knowledge and diverge on almost everything else – format, duration, setting, and the breadth of what was measured. That convergence is meaningful, but also narrow.

Taken together, Hinojosa et al. (2010) and Cannistrà et al. (2024) support a short-term knowledge effect across two substantially different formats and settings. That convergence is meaningful, but it remains narrow: neither study establishes that the gains persist or transfer to everyday financial behavior.

A separate caution deals with engagement, which is often reported alongside knowledge and treated as interchangeable with it. Engagement is a precondition for learning. A student absorbed in a simulation may focus on the scoring rule - which moves improve the standing - while attending not at all to the financial principle the scoring rule was built to represent. These are indistinguishable from the front of a classroom. Studies that divide motivation and knowledge are the ones capable of telling them apart.

2. Risk Preferences and Investment Decisions

Increased willingness to accept financial risk is not necessarily negative. The educational question is whether students learn to distinguish

informed, diversified risk-taking from concentrated or poorly understood risk.

Increased willingness to accept financial risk is not necessarily negative. The educational question is whether students learn to distinguish informed, diversified risk-taking from concentrated or poorly understood risk.

The most useful finding in this area is a null one. Razen et al. (2021) measured financial literacy, elicited economic preferences experimentally, and observed field behavior among 627 Austrian adolescents, and the variables did not reduce to a single underlying dimension. Literacy did not predict caution; patience and risk attitude did not align into one profile. This result forecloses the simplest interpretation of any post-intervention change in risk tolerance. If knowledge and caution were two expressions of the same trait, a shift toward risk after instruction would be straightforwardly bad news. They are not, so it is not.

Bhattacharya et al. (2020) point in the same direction from a different angle. In their comparison of money-management and investment curricula against economics and history controls, both financial conditions raised students' expressed willingness to bear financial risk. In contrast, the money-management condition was additionally associated with thrift and delayed gratification. A single intervention thus moved risk tolerance and future orientation upward at once – a combination that is difficult to classify as either a success or a failure without

knowing more about the reasoning behind the choices.

What I find genuinely hard is that a game can raise risk tolerance through two entirely different routes. In the first, a student understands the relationship between variance and expected return, recognizes that a long horizon tolerates volatility, and adjusts accordingly. In the second, a student takes a concentrated position, wins, and concludes that the approach was sound. Angerer et al. (2024) documented the second route with precision: in repeated lottery-choice tasks with children and adolescents, early success arriving by chance predicted greater subsequent risk-taking. The relevance to classroom simulations is immediate. Any market game running over a few weeks will give some fraction of participants early, undeserved success, and the game itself has no way to mark it as undeserved.

This dynamic is amplified, not dampened, by the engagement benefits documented elsewhere. Platz and Jüttler (2022) found that game-based instruction increased situational interest and engagement, with the effect pronounced among students who valued financial topics but had limited prior access. Those students are precisely the ones for whom an early simulated windfall is most likely to feel like evidence about their own aptitude.

The implication for assessment is that the quantity of risk a student accepts after instruction is close to uninformative on its own. The difference is whether the student can state the

probability and magnitude of a loss, describe the position's diversification, and defend it against a specified alternative. A simulation that never elicits that account has not merely failed to teach the distinction — it has left the researcher studying it unable to observe which outcome occurred.

3. Time Preference and Economic Decision-Making

Saving, budgeting, and borrowing are variants of a single structural problem: a benefit available now weighed against a larger benefit available later. This makes intertemporal choice the natural target for instruction, and two randomized trials have addressed it directly.

Luhrmann et al. (2018) assigned a financial-education program to German secondary students and measured outcomes through an incentivized experimental task rather than a questionnaire. Participants made more time-consistent choices and showed lower overall present bias. Pitthan and De Witte (2025) took a different route to a related outcome, randomizing 814 Belgian secondary students between conventional financial instruction and instruction that additionally addressed myopic bias by name. The behavioral condition produced gains in both bias awareness and financial literacy, suggesting that teaching students about the mechanism of a bias is not redundant with teaching them the content the bias distorts.

Both are credible results. They also address different things, and the central analytic requirement of this section is to keep three

outcome levels apart. The following results are credible and show different things, and the central analytic requirement of this section is to separate three outcome levels. Improvement for knowledge scores is well established, short-term, and described in Section 3.1. It is the easiest level to move and the least informative about conduct—choices within experimental tasks. Luhrmann et al. (2018) demonstrated change at this level. An incentivized choice is a substantial step beyond a test item — real money is at stake, and the decision is made rather than described. It remains a decision taken under controlled conditions, within a short window, with the full choice set displayed—not real long-term financial conduct. Here the literature divides. Kaiser et al. (2022), synthesizing randomized evaluations, found positive average causal effects on downstream behaviors including saving and budgeting. Fernandes et al. (2014), working from a broader and earlier body of evidence, found weak effects that attenuated as the interval between instruction and measurement lengthened, and concluded that education delivered close to the moment of decision is more useful than education delivered years ahead of it.

The three levels are frequently reported as though movement at one implies movement at the next, and nothing in the evidence licenses that. Most evaluations of game-based financial education operate exclusively at the first level. A further consideration constrains what the third level could show even in principle: actual saving depends on income, household circumstance,

access to an account, and the arrival of emergencies. These are the constraints a simulation removes to be playable, which means a game cannot be credited with teaching students to overcome them.

Table 1. Selected studies on game-based and behavioral financial education

Study	Participants and method	Key finding
Hinojosa et al. (2010)	Grades 4–10, national RCT, Stock Market Game	Saving and investing knowledge rose across grade bands; mathematics effects held only in lower grades
Cannistrà et al. (2024)	2,220 students in four European countries; multi-country RCT	Online game-based course improved financial literacy by 0.313 standard deviations
Platz & Jüttler (2022)	Upper-secondary economics students; game-based learning study	Game-based instruction affected situational interest in financial topics
Razen et al. (2021)	627 Austrian adolescents; tests,	Literacy was generally unrelated to

	incentivized tasks, and field behavior	experimentally elicited risk and time preferences		randomized experiments involving more than 160,000 participants	produced positive average effects on knowledge and downstream behavior
Bhattacharya et al. (2016, 2020)	Middle-school financial camp; high-school course with eight instructional periods	Modest knowledge gains; money-management instruction linked to thrift and delayed gratification	Rodriguez-Raga et al. (2022)	1,337 Colombian students aged 6–18; game-, text-, and web-based conditions	Outcomes varied by delivery format, marking both the reach and the limits of game-based delivery
Angerer et al. (2024)	Children and adolescents, repeated lottery-choice experiment	Early success arriving by chance predicted higher subsequent risk-taking	Senol et al. (2023)	Trading behavior in simulated investment environments	Biases differed between simulated and real markets, restricting generalization from game results
Lührmann et al. (2018)	German secondary students; randomized program and incentivized choice task	Financial education increased consistency in intertemporal choices	Suleiman (2024)	Stock-market simulation followed by guided discussion	Structured reflection after play improved the quality of subsequent decisions
Pitthan & De Witte (2025)	814 Belgian secondary students, RCT	Adding myopic-bias content outperformed conventional instruction on literacy and bias awareness	Park (2017)	Korean participants, investment simulation	Participation in simulation shifted risk attitudes
Kaiser et al. (2022)	Meta-analysis of 76	Financial education	Han (2019)	Mobile stock-market	Educational value

	simulation	accompanied by elevated overconfidence and trading-related psychological effects
Lee (2025)	Adolescent financial knowledge and overconfidence	Calibration of confidence as consequential for outcomes as objective knowledge

IV. Design Conditions and Limitations

1. Feedback and Structured Reflection

The defensible case for game-based instruction is that it lets students make financial decisions and observe consequences without bearing real losses. Hinojosa et al. (2010) and Cannistrà et al. (2024) both indicate that this can outperform conventional delivery on short-term knowledge. The case has a limit that is easy to miss: what a student learns from a round of play is not the outcome itself but the explanation they attach to it, and unguided students attach flattering rather than accurate explanations.

The specific failure this produces is conflating decision quality with outcome quality. These come apart constantly in financial contexts. A well-reasoned, diversified position can lose money over any short interval. A reckless concentrated bet can win. A simulation that ranks students by result has, by construction, told them that the second student did better. Absent explicit

correction, that is the lesson delivered — and it is the inverse of the intended one.

Pitthan and De Witte's (2025) result supports the corrective. Instruction that made the mechanism of bias explicit outperformed instruction that conveyed only content, which suggests students benefit from examining how a decision was reached rather than only what it returned. Reflection operationalizes this at the level of individual gameplay.

A brief structured debrief after each round is sufficient:

- What decision did the student make?
- What information influenced the decision?
- Did sound reasoning or luck cause the result?
- What would the student change in a similar situation?

The third question carries the most weight, and it is the one a game cannot answer on its own. Determining whether an outcome followed from reasoning or chance requires knowing what the student expected before the result arrived, which is why the questions work in sequence rather than individually.

The teacher's role in this is substantive rather than supervisory. Every simulation contains features that do not correspond to the world — compressed time horizons, absent transaction costs, unrealistic liquidity, income that arrives on schedule. Only an instructor can mark which elements of the model are load-bearing and which are artifacts of making the model playable.

Without that marking, students have no way to know which of the game's lessons to carry out of the room.

2. Reward Structures and Excessive Risk-Taking

A game that rewards only the highest final portfolio value may teach students to maximize returns rather than evaluate whether the underlying risk is justified.

The mechanism is arithmetic. Over a few weeks, the strategy most likely to produce the highest return in a class is the concentrated, high-variance one—not because it is sound, but because it has the widest outcome distribution. The same property makes it likely to produce the worst result as well, but a leaderboard displays the top and buries the bottom.

Competition compounds this through the reference-dependence channel described in Section 2.3. A visible ranking places every student not in the lead into the loss domain relative to the leader, and loss framing is the condition under which risk-seeking is most consistently observed. The design intended to motivate therefore systematically pushes the majority of the class toward the behavior the curriculum aims to discourage.

None of this requires abandoning competition or eliminating risk from the simulation, which would make it both dull and inaccurate. It requires that the scoring rule stop treating terminal value as the sole criterion. Composite

scoring can weight diversification, the quality of stated reasoning, responsiveness to new information, and consistency with a goal the student declared in advance — that last element being particularly useful, since it makes strategy drift visible to the student rather than only to the instructor.

Assessment should follow the same logic. A grader needs enough from each student to determine whether a position was understood, which means the submitted work has to make the reasoning inspectable: why each holding is present, what a single adverse event would do to the portfolio as a whole, and how the allocation corresponds to the horizon the student is playing toward. A comparison against a low-risk benchmark serves this well because it forces an explicit argument about whether the additional return compensated for the additional variance, rather than letting the return figure speak for itself.

Budgeting games face a parallel design requirement. Their instructional value lies in making opportunity cost visible—allocating more to one category necessarily leaves less for another—and in showing why a budget that balances in a typical month fails when a repair bill or medical expense arrives. That value depends on realistic constraints. A game in which income is stable and adequate, and in which shortfalls follow only from allocation errors, teaches that financial difficulty results from choice. For students whose households experience unstable hours, housing costs consuming most of the

income, or caregiving obligations, this is both inaccurate and alienating.

3. Real-World Transfer and Research Limitations

Consider what a school considering adoption could legitimately conclude from this literature. The honest answer is: less than the volume of positive findings suggests, because the studies are not measuring the same thing.

The heterogeneity runs along every dimension. Participants range from primary pupils to final-year secondary students, whose numeracy and prior exposure differ enormously. Interventions range from a single session to a full term. Formats include board games, escape rooms, and market simulations, which share a label and little else. Outcome instruments range from multiple-choice tests and incentivized choice tasks to self-reported behavior—three different constructs, as Section 3.3 argued. Because these vary jointly rather than one at a time, a difference in results between two studies cannot be attributed to any single factor, and the literature offers no basis for treating a brief intervention as a smaller dose of a long one. They are different treatments.

Two further problems affect how we should read individual findings. Statistical significance and practical significance diverge: a study with several thousand participants can detect a score improvement too small to matter educationally. In contrast, a study of one classroom may miss an effect that would matter a great deal. And publication patterns favor positive findings,

which means the visible literature likely overstates the average effect by an unknown amount.

The apparent disagreement between Fernandes et al. (2014) and Kaiser et al. (2022) is best understood.

Read through this lens rather than as a dispute about the facts. The two syntheses were answering different questions. Fernandes et al. asked what the accumulated evidence showed about financial education in general, admitted a wide range of study designs accordingly, and tracked how effects behaved as the interval between instruction and measurement grew. Kaiser et al. asked what could be established causally, restricted inclusion to randomized designs, and drew on a body of trials substantially larger and more recent than was available a decade earlier. Both conclusions can hold simultaneously. An average effect can be positive at the point of measurement while decaying thereafter, and the later synthesis's stronger causal warrant does not extend to duration, because most of the trials it includes did not follow participants long enough to measure it. Transfer remains the unanswered question beneath all of this. A student can master the rules of a specific game without extracting the principle the game was built to encode, and success within the simulation cannot distinguish the two. The evidence here is not merely absent but mildly discouraging: Senol et al. (2023) observed that behavioral biases manifest differently in simulated and real markets, which implies that conduct inside a game is an imperfect proxy for conduct outside it even when the financial

structure is held constant.

Follow-up measurement at intervals long enough to test persistence, against conduct rather than recall, would settle these questions. Such studies are largely absent, and until they exist, the strongest defensible claim remains the short-term one.

Conclusion

Game-based financial education shows a consistent short-term advantage on the outcomes most often measured. Across a national randomized simulation trial, a multi-country online escape room, and teacher-led board games in public schools, students who played outperformed comparison groups on financial-knowledge assessments, and engagement with the material rose alongside. Studies that measured decisions rather than recall extend this modestly: instruction, particularly when it explicitly names behavioral biases, is associated with more time-consistent choices and greater awareness of the reasoning behind a choice. These are genuine results, and they justify the approach's place in the curriculum.

The evidence does not support crediting the game format itself. Outcomes appear to turn on the accuracy of the financial content, the realism of the constraints, the informativeness of the feedback, the structure of the reward, and whether the activity builds in reflection rather than appending it. A scoring rule that ranks students by terminal portfolio value will teach return maximization irrespective of the stated

objective, and early luck can be misread as skill by students and instructors alike. The evidence base carries its own limits: samples, durations, formats, and outcome instruments vary jointly across studies, which makes direct comparison unwarranted more often than it is attempted, and the divergence between Fernandes et al. (2014) and Kaiser et al. (2022) reflects differences in inclusion criteria and in the question each was answering rather than a contradiction in the underlying data. Whether short-term gains survive into adult financial conduct is not currently known, because the studies capable of showing it have not been conducted.

The defensible position is neither dismissal nor substitution. Games supply repeated decisions, visible consequences, and motivation, which direct instruction supplies poorly. Direct instruction supplies accurate content, correction of misconceptions, and conceptual structure, which games supply poorly and sometimes distort. The two are complementary, and a program combining them is easier to justify from the evidence than either alone. For research, the priority is a change in what gets measured: follow-up at intervals long enough to test persistence, outcomes drawn from actual saving, borrowing, and investment behavior rather than test performance, and comparisons isolating specific design features, so that the field can begin to distinguish the elements of a game that produce learning from those that produce only play.

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