Cognitive Anchors and Market Inertia? Perceived Understanding, Stated Intention, and Current Investment Participation

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Abstract

Why do some people who say they understand investing participate while others do not? We examine this question in a cross-sectional, non-probability online survey of 151 response records from a youth-focused sample, separating perceived investment understanding, stated twelve-month intention, perceived investing know-how, savings confidence, and current self-reported participation. Under the strict Yes-versus-No participation definition, participation rose across perceived-understanding categories from 3.7% to 13.5% to 53.2%, yet 37 of 79 records in the "Yes" understanding category reported nonparticipation. A fixed-margin Fisher–Freeman–Halton test indicated association in the 3×2 table ($p \approx 4.07\times10^{-8}$). In the expanded-instrument Firth model ($N = 103$), twelve-month intention was positively associated with current participation (odds ratio 2.068; 95% profile interval 1.338–3.495). The intention association remained positive under both prespecified alternative participation codings; perceived investing know-how had positive point estimates under all three codings, but its interval interpretation changed with those classifications. These results describe the extensive margin of current participation; they do not identify causal mechanisms, measure objective financial knowledge, or track subsequent behavior. By examining these distinctly worded self-assessments separately, we document how a pronounced participation gradient can coexist with substantial nonparticipation among respondents who report understanding investing basics.

Keywords: investment participation; subjective financial knowledge; stated intention; perceived investing know-how; household finance; youth-focused survey

The knowledge–action question

Consider two answers a person might give on the same day: "Yes, I understand the basics of investing" and "No, I am not currently investing." The answers can both be sincere. Understanding a concept, feeling able to begin, deciding to begin, and already participating are different aspects of a financial decision. They become especially consequential when returns are uncertain, resources are limited, and institutional opportunities differ. The puzzle is not why every informed person has failed to invest; it is how self-assessed understanding relates to the participation margin without determining it.

In the strict-outcome table, participation rises from 3.7% among respondents answering "No" to perceived understanding, to 13.5% answering "Sort of," and 53.2% answering "Yes."

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At the same time, 37 of the 79 "Yes" respondents—46.8%—reported not currently investing. The gradient and the within-category nonparticipation are the paper's joint finding, not a contradiction. We call this coexistence a *knowledge–action gap* as study-specific descriptive shorthand for perceived understanding alongside current self-reported nonparticipation, not as a score or an observed transition from tested knowledge to later behavior.

Our economic focus is current self-reported participation: how participation differs across respondents' assessments of their understanding, intentions, and ability to begin.

Why subjective assessments belong in a household-finance account

Household-finance research asks not only what people know but how they navigate products and constraints. Campbell (2006) locates participation and portfolio choice in a costly, information-intensive choice environment. Van Rooij, Lusardi, and Alessie (2011) relate tested financial literacy to stock ownership in an adult setting. Closer to young adults, Xiao et al. (2011) apply and extend the theory of planned behavior to risky credit behavior among first-year college students, examining subjective and objective financial knowledge and behavioral intention. These studies motivate a more specific question at the investment-participation margin: how do perceived understanding of investing concepts and confidence about knowing how to begin relate to current participation when intention and savings confidence are considered alongside them? We address this question by examining these distinctly worded self-assessments within a youth-focused investing survey.

Subjective assessment is worth examining on its own terms. Allgood and Walstad (2016) distinguish perceived from tested financial literacy in adult survey data; Hadar, Sood, and Fox (2013) show experimentally in consumer-choice settings that perceived knowledge can matter even when objective information is held fixed. These sources motivate treating a self-rating as information about the respondent's decision perspective, while our single "understanding the basics" answer remains a judgment rather than a test of competence.

Research questions and scope

We ask two questions. First, how does current investment participation vary across perceived-understanding categories? Second, how are perceived understanding, stated intention, perceived investing know-how, and savings confidence each associated with participation when considered together? H1 addresses the first question through the common-core comparison; H2 addresses the second through the expanded-instrument model. *Cognitive anchors* and *market inertia* remain theoretical possibilities, not mechanisms identified in these data. A persistent reference belief might shape how a person sees investing; a participation state might persist when circumstances change. Neither is measured by a one-time answer about understanding or current investing. Later research could test these possibilities by observing beliefs, opportunities, and behavior over time.

H1 and H2 address different questions in different analytic samples; H2 does not explain away H1.

An earlier Yandra–Bala organizational report developed a related financial-action-gap motivation for a November 2025–January 2026 survey period (Yandra and Bala 2026). The final export used here is cumulative and contains historical records, but the extent of overlap between that report's analyzed records and this paper's analytic samples cannot be established from the report. We therefore treat it as conceptual lineage, not an independent replication.

1 Literature review and theoretical framework

1.1 Financial literacy, subjective understanding, and participation

Financial knowledge may help someone recognize a product, compare risk and return, understand a fee, or judge suitability. The financial-literacy literature has developed objective questions for such abilities and connected performance on them to financial choices (Lusardi and Mitchell 2014). In the Dutch Household Survey, van Rooij, Lusardi, and Alessie (2011) used dedicated literacy modules when studying stock ownership. The measurement in our survey is different: "Do you understand the basics of Investing? (stocks, crypto, ETFs)" asks respondents for a categorical judgment of their own understanding. Its mention of stocks, crypto, and ETFs does not make the answer a test about any of these assets.

Subjective and objective knowledge can diverge. A respondent may know a few facts yet feel unable to evaluate a product, or feel familiar with one while missing important risks. The phrase "I understand the basics" could mean being able to define a stock, discuss it with peers, or make an informed decision. Allgood and Walstad (2016) give reason to consider perceived and tested literacy separately; Hadar, Sood, and Fox (2013) show in consumer-choice experiments that changing perceived knowledge can matter even when objective information is held fixed.

A self-assessment can carry information about the participation margin without specifying what someone will do. Neither participation nor nonparticipation is, by itself, a benchmark for a good financial decision.

1.2 Conceptual understanding and procedural know-how

Knowing what an investment is and knowing how one would begin are related but not identical questions. The first may concern conceptual categories and basic vocabulary. The second can involve a sequence of practical judgments: where to obtain information, what steps an account requires, what permissions or resources are needed, how to compare available choices, and when to stop if a product is unsuitable. The expanded survey asks about this second domain with the exact statement, "I feel confident I know HOW to start investing right now." We call the response *perceived investing know-how*. The capitalized "HOW" is part of the administered wording and directs attention toward beginning a process; it does not demonstrate that the respondent can complete that process.

This distinction matters economically because changing a decision environment is different from changing someone's opinion of their own ability. Beshears, Choi, Laibson, and Madrian (2013) study a simplified enrollment route in an adult retirement-savings context. Their intervention changed the available task by reducing a multidimensional enrollment choice to a simpler one; participation rose in that setting. We did not alter the survey respondents' choices or observe their enrollment steps. The example instead clarifies a possible future contrast: a person might report low know-how because the task is genuinely difficult, because it seems difficult, or because a legal or financial constraint makes the task unavailable. A self-rating alone cannot adjudicate among these possibilities.

We use *perceived readiness* as an umbrella for judgments relevant to beginning, not as a measured factor. The survey's wording separates a judgment about basic concepts, confidence about how to begin now, intention over twelve months, and confidence in saving. These are con-

ceptually different questions, although the single items do not establish empirical discriminant validity.

1.3 Intention, current participation, and TPB

The theory of planned behavior relates behavior to intention and perceived behavioral control, with intentions shaped by attitudes, subjective norms, and control beliefs (Ajzen 1991). Ajzen (2002) further distinguishes self-efficacy-like judgments from perceived controllability within the discussion of behavioral control. This distinction helps interpret our question about *knowing how* to begin: it asks about a respondent's confidence in a practical ability, not whether account access, money, permissions, or other conditions actually permit action. Our single know-how item is not a validated perceived-behavioral-control scale, and the survey does not measure the full TPB system. TPB motivates attention to intention and control-like judgments without making ability a necessary condition for any one observed response.

The intention item—"I intend to start investing within the next 12 months"—is future-oriented, whereas participation asks about the present. Both were answered at one survey occasion. A current investor might take "start" to mean a new product or greater involvement; a noninvestor might intend to begin but lack an opportunity. Sheeran and Webb (2016) distinguish intentions from later behavior. Here, the valuable comparison is contemporaneous: whether a stronger stated plan accompanies current reported participation.

Savings confidence is yet another assessment. Confidence in being able to save could accompany investing because both reflect financial experience; current investing could also change how someone evaluates their saving ability. Resources or family support could affect both. The regression contains no timing or identification strategy to choose among these directions.

1.4 Fuzzy-trace theory, cognitive anchors, and market inertia

FTT proposes that people can represent information in both detailed, verbatim form and in a simpler gist that captures its bottom-line meaning (Reyna 2004). In a financial decision, one might remember a precise fee or rule, a general impression of risk, or both. The theory raises a prospective question: does the representation of a financial choice affect whether an intention is carried through? The survey does not elicit gist or verbatim representations, test recall, or ask respondents to solve a task.

We propose *cognitive anchors* as persistent reference beliefs, judgments, or frames that may orient financial choices. This author-defined idea is not identical to the anchoring-and-adjustment bias associated with salient numerical reference values (Tversky and Kahneman 1974), and a confident survey response is not itself an anchor. A future study could elicit an initial judgment about the complexity of opening an account, observe whether it persists despite new information, and compare later decisions under altered opportunities.

Market inertia is also a proposition about persistence, not a synonym for answering "No" once. Research on automatic enrollment in adult retirement plans documents how participation and default allocations can depend on a changed choice architecture, with persistence after enrollment (Madrian and Shea 2001). That is a reason to investigate inertia in financial decisions, not evidence that the nonparticipants in this survey are procrastinating or following a default. To identify persistence, we would need observations across time and information about available options and changes in circumstances.

Our proposed bridge between TPB and FTT is a research agenda. TPB asks how intention

and perceived control relate to behavior; FTT suggests that the *representation* of a task could shape what seems possible or worth doing. A future study could measure representations, intentions, opportunities, and later choices in sequence, or experimentally change the form in which a choice is presented.

1.5 Resources, access, and educational choices

Financial participation has material as well as psychological conditions. An individual may lack spare funds, face eligibility or account-opening requirements, depend on an adult's participation, prefer liquidity, or reasonably judge market risk too high for a current objective. Campbell (2006) discusses how household choices and participation costs depend on circumstances and how difficult it is to infer welfare from an observed portfolio. These considerations are especially important in a youth-focused survey with recorded age bands extending into an open-ended 26+ category: respondents need not have the same legal opportunities, income, support, or decision autonomy. We did not measure those conditions sufficiently to control for them. A know-how response should not be read as a proxy that absorbs them.

Financial education provides a practical reason to distinguish conceptual, procedural, and intentional assessments. Fernandes, Lynch, and Netemeyer (2014) reported small downstream behavioral effects for financial-literacy interventions in the studies they reviewed. A later synthesis of 76 randomized experiments by Kaiser, Lusardi, Menkhoff, and Urban (2022) found average positive effects of financial education on knowledge and downstream behavior and disputed a simple rapid-decay conclusion, while not establishing long-run durability. Brown et al. (2016) examine young-adult debt behavior following exposure to school financial or mathematics education; debt outcomes are not interchangeable with investment participation. This literature motivates attention to what an educational design teaches and what its evaluation measures, without establishing one superior instructional style for this survey's respondents.

We propose a *defensive* versus *opportunity-oriented* educational distinction as a future design question, not an observed treatment contrast. Budgeting, liquidity management, and caution can protect welfare; understanding available investment choices can be useful when participation is suitable. A comparative study could ask how instruction and task design affect comprehension, calibrated confidence, and the ability both to act and to recognize when not to invest.

1.6 What the present framework permits us to ask

The conceptual sequence is longer than the estimated model. Perceived understanding, perceived know-how, intention, and current behavior occupy different places in a possible decision process, while resources and institutions shape available choices. TPB raises questions about motivation and control; FTT raises questions about representation; household finance directs attention to the participation margin and its constraints. Together, H1 and H2 motivate distinguishing conceptual judgment, perceived procedure, and intention in the next study.

2 Data and methods

2.1 Design and analysis population

We analyze a cross-sectional online survey from an independent youth-led research effort administered through the Youth Economics Initiative (YEI). Recruitment used non-probability convenience, purposive digital, and peer-network circulation, with recruitment support from

Democrashe, Inspire Youth NJ, and The Center for Teen Empowerment. The study had no known probability sampling frame. Our unit is a submitted response record. The available export and historical linkage checks do not independently establish that each record represents a distinct person, but neither do they establish that any retained record is a duplicate. We therefore state counts as response records rather than verified unique respondents.

The final cumulative export, collected after the September 25, 2026 survey closure, contains 151 submitted records, all retained for model-specific eligibility screening. Because the export is cumulative, the separate historical Wave 1 file was not appended. Administration records classify 42 responses as legacy-instrument records and 109 as expanded-instrument records, the latter including intention, perceived investing know-how, and the primary-barrier item. This is an instrument change, not a longitudinal follow-up. One legacy response has unresolved historical provenance; no affirmative evidence of duplication or invalidity was found, so it remains in the retained records. It is eligible for H1 under the same rules as other responses and structurally unavailable for H2.

Recorded age categories are "13-15," "16-18," "19-21," "22-25," and an open-ended "26+" band. Fourteen records selected "26+"; they were retained if otherwise eligible. We neither convert that category to a midpoint nor infer an age ceiling. Thus the analysis population is the set of respondents selecting these survey bands, not a verified sample restricted to ages 13–26. Age is not an H1 or H2 predictor. The survey record does not establish a final country count or a recruitment-chapter count for this analysis.

2.2 Measures and outcome coding

Two questions appeared in both instrument versions. Perceived investment understanding was elicited by "Do you understand the basics of Investing? (stocks, crypto, ETFs)" with responses "No," "Sort of," and "Yes." We represent those answers as a categorical factor with "No" as the reference, not as numerical scores of one through three. Current participation was elicited by "Are you currently investing?" with substantive answers "Yes," "No," "Not sure," and "Sort of." The strict primary outcome codes "Yes" as 1 and "No" as 0 and leaves the two ambiguous responses unavailable for this binary comparison. It does not silently equate uncertainty or partial engagement with nonparticipation.

The expanded instrument asked "I intend to start investing within the next 12 months" and "I feel confident I know HOW to start investing right now." Each used 1 = Strongly Disagree and 5 = Strongly Agree, with intermediate categories 2–4 not given individual verbal labels. We call the latter item *perceived investing know-how* in the paper while retaining its original wording and underlying model coding. The question "How confident are you in your savings ability?" used 1 = Not confident and 5 = Very confident, again with unlabeled intermediate categories. Savings confidence is not an alternative label for investing know-how. The expanded instrument's "What is your biggest barrier to investing right now?" asks for one primary reported category. Its descriptive denominator includes investors, including the "I have no barrier — I already invest" answer.

The two prespecified sensitivity definitions retain the response categories but assign them different binary outcomes. Sensitivity A codes "Yes" as 1 and "No," "Not sure," and "Sort of" as 0. Sensitivity B codes "Yes" and "Sort of" as 1 and "No" and "Not sure" as 0. Their purpose is to show how ambiguous participation wording affects the comparison. An exact case-only

variant of a participation label was standardized in a derived field; the original raw response remains unchanged. Appendix Table A1 displays the standardized substantive categories.

2.3 Analysis and sample accounting

H1 uses the strict 3 × 2 table of understanding by participation. We report within-understanding-category percentages and a two-sided Fisher–Freeman–Halton exact conditional test. Under fixed observed row and column margins, the implementation enumerates all 928 admissible tables and includes those with conditional probability no greater than that of the observed table. Exact combinatorial ordering determines tail membership before numerical conversion for the reported p value. Bivariate Firth penalized-logistic category contrasts supplement the table in the presence of a sparse "No" reference cell (Firth 1993; Heinze and Schemper 2002).

H1 addresses the bivariate relationship between perceived understanding and current participation using items available across both instrument versions. The expanded instrument permits a different question: whether perceived understanding, stated intention, perceived investing know-how, and savings confidence each contain conditional information about current participation when considered jointly. H2 therefore includes the four survey dimensions specified before final inference, rather than selecting them according to their observed associations. Sparse response combinations can make ordinary logistic estimates unstable under separation. We use Firth penalized logistic regression and report profile penalized-likelihood intervals. This six-parameter model uses only expanded-instrument records. With "No" understanding as reference, its linear predictor is

$$\begin{aligned}\textbf{H2:}\quad \operatorname{logit}\Pr(Y_i=1) &= \beta_0 + \beta_1 \mathbf{1}(K_i = \text{Sort of}) \\ &\quad + \beta_2 \mathbf{1}(K_i = \text{Yes}) + \beta_3 I_i + \beta_4 H_i + \beta_5 S_i,\end{aligned} \tag{1}$$

where Y_i is the stated participation outcome under the named definition, K_i is perceived understanding, I_i is the 1–5 intention response, H_i is the 1–5 investing-know-how response, and S_i is the 1–5 savings-confidence response. Each 1–5 term is a one-parameter trend: its odds ratio is for a one-category increase in the **coded answer**, assuming a constant log-odds increment. This does not claim equally spaced psychological states. H2 contains no age, country, gender, income, education, barrier, interaction, or mediation term. We report exponentiated slopes with 95% profile penalized-likelihood intervals, not Wald intervals. The exponentiated intercepts, which are reference odds rather than contrasts, are retained in Appendix Table A3. For H2, the predictor-zero reference profile lies outside the administered 1–5 scales and should not be read as a typical respondent's odds.

The primary H1 sample has 143 strict-outcome records: 48 events ("Yes") and 95 non-events ("No"). Eight participation answers—three "Not sure" and five "Sort of"—are unavailable for H1 but remain in the retained dataset. H2's primary complete-case sample consists of 103 of the 109 expanded-instrument records (33 events, 70 non-events). The other 42 records were never administered the expanded items, and six expanded-instrument records have an ambiguous primary outcome (one "Not sure," five "Sort of"). There is no additional ordinary missingness in the required H2 predictors among those 109 expanded records. Both sensitivity models include all 109, with 33 events/76 non-events under A and 38 events/71 non-events under B. Relative to primary H2, sensitivities change both outcome definition and sample membership; A

and B themselves use the same 109 records. Selection is model-specific complete-case selection, not pairwise deletion, imputation, or a change to the retained survey population (Table 1).

Table 1: Response records and analytic membership.

Population or model	N	Events	Non-events	Model-specific unavailable/excluded
Final submitted and retained	151	—	—	0 records removed
Legacy instrument	42	—	—	Expanded items not administered
Expanded instrument	109	—	—	H2 administration base
H1 strict primary	143	48	95	8 ambiguous participation answers from 151
H2 primary	103	33	70	6 ambiguous participation answers from 109 expanded; 42 legacy structurally outside base
H2 Sensitivity A	109	33	76	0 from expanded base
H2 Sensitivity B	109	38	71	0 from expanded base

Note: Counts describe records, not verified distinct people. Model-specific unavailability does not mean removal from the retained survey. H2 items were not administered to the 42 legacy-instrument records.

2.4 Survey procedure and data availability

The work was conducted as independent youth-led academic research; formal institutional IRB review was not sought or obtained. The survey included a required consent item and a notice for participants under 18 advising them to understand how their information would be used and to consult a parent or guardian if they had questions. Independent parental permission was not verified.

Raw responses were accessible to the two lead researchers. The originals were stored in the Google Forms/Sheets environment used for administration; local analytical copies were excluded from version control. The stated retention plan is up to five years following formal publication for audit and replication verification, followed by deletion of respondent-level raw files from active and archived storage. Aggregate outputs, code, and non-identifying reproducibility artifacts may be retained. Respondent-level raw records are not offered for public deposit or third-party sharing. A missing historical provisional cumulative snapshot limits exact accounting of additions from that snapshot to the final export; the preserved Wave 1 source and the final cumulative export were authenticated separately.

3 Results

3.1 Perceived understanding and strict participation

Table 2 shows a substantial gradient across the three perceived-understanding categories. Only 1 of 27 strict-outcome records in "No" reported current participation (3.7%), compared with 5 of 37 in "Sort of" (13.5%) and 42 of 79 in "Yes" (53.2%). The complementary count in the final category—37 of 79, or 46.8%—is central to the paper's motivating question (Figure 1).

The Fisher–Freeman–Halton exact test rejects the null hypothesis of independence between perceived understanding and current participation in these strict-response records ($p \approx 4.07 \times 10^{-8}$). Complementary Firth estimates quantify the category contrasts, with profile penalized-likelihood intervals showing their uncertainty. The bivariate Firth contrast for "Sort of" versus

Table 2: Perceived understanding and strict current participation.

Perceived understanding	Not participating: No	Currently participating: Yes	Total	Yes %	No %
No	26	1	27	3.7	96.3
Sort of	32	5	37	13.5	86.5
Yes	37	42	79	53.2	46.8
Total	**95**	**48**	**143**	**33.6**	**66.4**

Note: The denominator of each percentage is the row total; $N = 143$. The two-sided Fisher–Freeman–Halton exact conditional test, ordered by table probability with fixed observed margins, gives $p \approx 4.07 \times 10^{-8}$ (928 admissible tables).

"No" is 2.990 (95% profile interval 0.549–30.476); for "Yes" versus "No" it is 20.022 (4.836–185.025). Only one respondent in the "No" understanding group reported participating, so the large second odds ratio should be read beside its very wide interval rather than as a precise magnitude (Table 3).

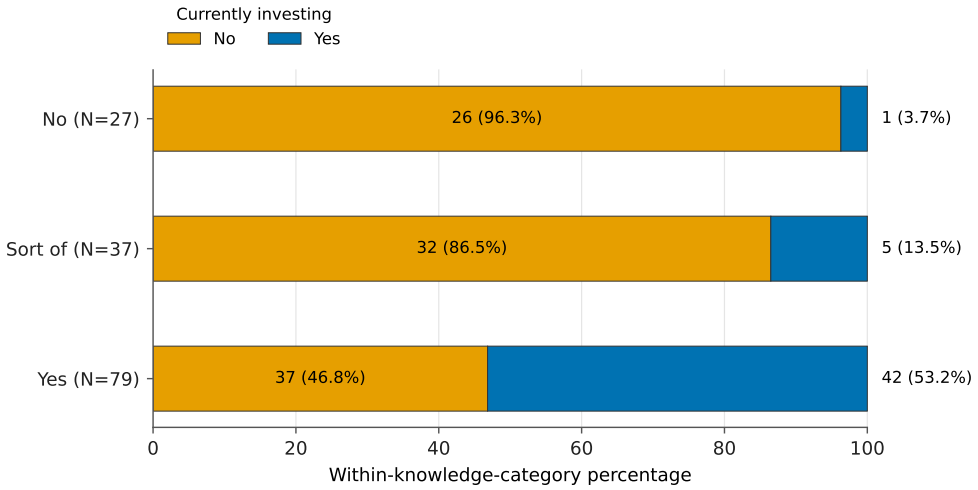


Figure 1: Perceived understanding and current participation under the strict outcome.
Note: Each stacked bar uses the number of respondents in that understanding category with an unambiguous Yes or No participation response: No 27, Sort of 37, Yes 79 ($N = 143$). Percentages are within category; ambiguous participation answers do not enter this figure.

3.2 The expanded-instrument model

Among 103 primary H2 complete cases, a one-category higher intention response was associated with approximately twice the odds of current participation, holding perceived understanding, investing know-how, and savings confidence constant (OR = 2.068; 95% profile interval 1.338–3.495). The corresponding investing-know-how estimate is 1.867 (0.987–3.871). Savings confidence is estimated at 1.028 (0.571–1.876). Relative to "No" perceived understanding, the "Sort of" contrast is 0.452 (0.045–6.032) and the "Yes" contrast is 0.921 (0.088–13.258) (Table 3; Figure 2). The wide intervals around the understanding contrasts and savings confidence

do not establish that these associations are absent. The raw level 1 of the know-how response has zero events among 11 primary-model records, a sparseness flagged in diagnostics; all fitted Firth models converged with full-rank designs and verified profile intervals.

The strong H1 gradient and the different H2 understanding estimates are not competing estimates of the same effect. H1 is a bivariate comparison in 143 strict common-core records. H2 is a conditional comparison in 103 expanded-instrument records, including three additional self-reports. Their numerical difference cannot be decomposed into mediation, attenuation, or the independent importance of one item. The intention estimate describes association with *current* participation as coded, not prospective prediction of what respondents will do during the next year.

Table 3: Primary Firth slope estimates.

Model	Contrast or coded-category trend	OR	95% profile interval
H1	Understanding: Sort of vs No	2.990	0.549–30.476
H1	Understanding: Yes vs No	20.022	4.836–185.025
H2 primary	Understanding: Sort of vs No	0.452	0.045–6.032
H2 primary	Understanding: Yes vs No	0.921	0.088–13.258
H2 primary	12-month intention, per category	2.068	1.338–3.495
H2 primary	Investing know-how, per category	1.867	0.987–3.871
H2 primary	Savings confidence, per category	1.028	0.571–1.876

Note: Odds ratios (ORs) and 95% profile penalized-likelihood intervals. H1 is bivariate, $N = 143$ (48 events); H2 is conditional on the other displayed items in the expanded-instrument sample, $N = 103$ (33 events). The exponentiated intercepts are reference odds, not comparative ORs, and appear in Appendix Table A3.

3.3 Outcome-definition sensitivities and reported barriers

The intention odds ratio remains above one across the examined definitions: 1.959 (1.278–3.271) under A and 2.238 (1.457–3.743) under B, alongside 2.068 (1.338–3.495) in primary H2. Investing know-how has positive point estimates under all three: 1.867 (0.987–3.871), 1.945 (1.060–3.921), and 1.547 (0.889–2.838). Its interval relation to one changes with classification of the ambiguous responses; this is a coding-sensitive interpretation, not an effect that switches on or off. The understanding and savings-confidence intervals remain broad (Table 4). These within-survey sensitivity models do not provide independent replications, and coefficient comparisons are not a test of differences between them.

Among the 109 expanded-instrument records, "I don't know where to start" was selected by 24, "I don't have enough money" by 22, and "Fear of losing money" by 20. Twenty-one selected "I have no barrier — I already invest." Table A2 and Figure 3 show every category in the administered order. Because current investors are included and each record supplies only one selected response, this distribution should not be interpreted as identified causes of nonparticipation or as a complete ranking of all difficulties respondents faced.

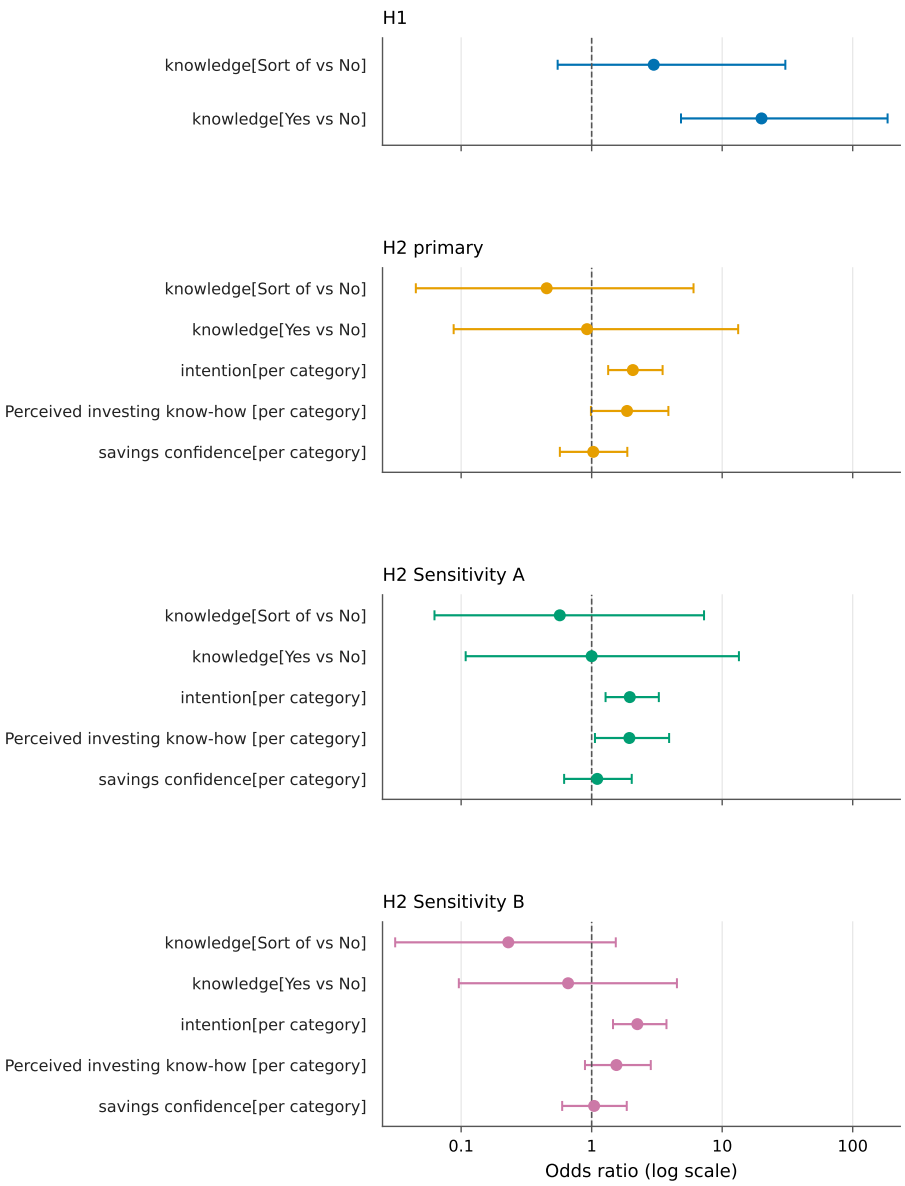


Figure 2: Firth estimates for understanding category contrasts and ordered-item trends.

Note: Points are odds ratios (ORs); horizontal lines are 95% profile penalized-likelihood intervals on a logarithmic OR axis, and the vertical line marks OR = 1. Understanding estimates compare Sort of or Yes with No; intention, investing know-how, and savings-confidence estimates are per coded response category. Panels show bivariate H1 under strict Yes-versus-No participation ($N = 143$), conditional H2 under the same strict coding ($N = 103$), H2 Sensitivity A with Yes = 1 and No/Not sure/Sort of = 0 ($N = 109$), and H2 Sensitivity B with Yes/Sort of = 1 and No/Not sure = 0 ($N = 109$). Differences between H1 and H2 panels are not a decomposition of mechanisms because their samples and conditioning sets differ.

Table 4: H2 outcome-coding sensitivities.

Contrast or coded-category trend	A OR	A interval	B OR	B interval
Understanding: Sort of vs No	0.570	0.062–7.260	0.230	0.031–1.530
Understanding: Yes vs No	0.9997	0.108–13.436	0.660	0.096–4.501
12-month intention, per category	1.959	1.278–3.271	2.238	1.457–3.743
Investing know-how, per category	1.945	1.060–3.921	1.547	0.889–2.838
Savings confidence, per category	1.103	0.615–2.028	1.044	0.594–1.857

Note: Sensitivity A codes Yes participation as 1 and No, Not sure, and Sort of as 0 (33 events; $N = 109$). Sensitivity B codes Yes and Sort of as 1 and No and Not sure as 0 (38 events; $N = 109$). Both use the same expanded-instrument records and H2 predictors. Entries are odds ratios (ORs) with 95% profile penalized-likelihood intervals; knowledge entries are category contrasts against No, and the other entries are per-coded-category trends. The near-one estimate is shown to four decimals rather than rounded to 1.000.

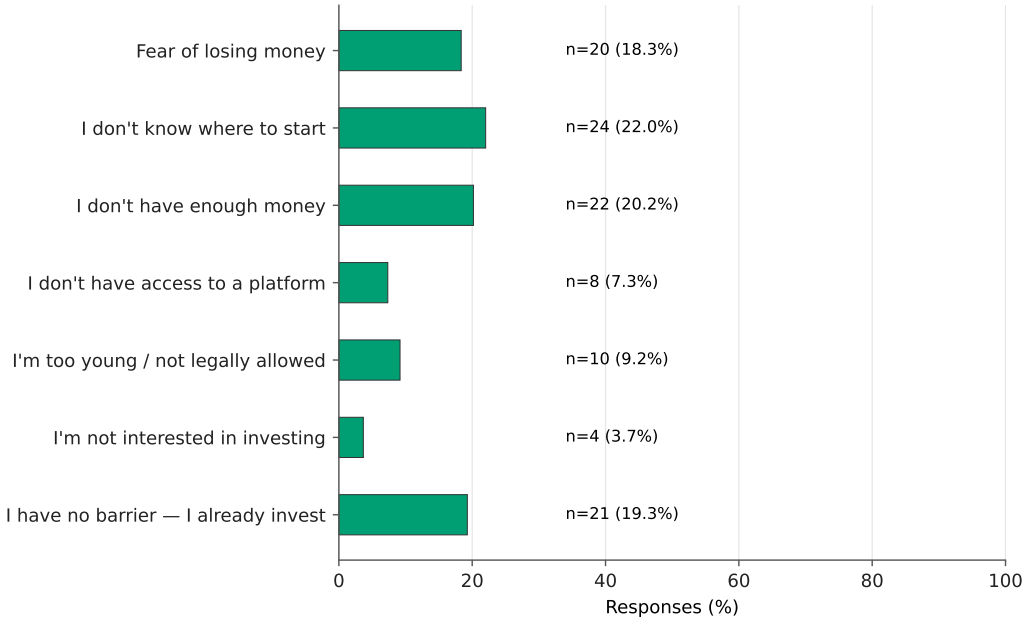


Figure 3: One reported primary investing barrier.

Note: Bar lengths show the percentage of all 109 expanded-instrument responses selecting each category, and annotations show the corresponding count and percentage. Current investors are included; one category is "I have no barrier — I already invest." These are reported primary barriers, not identified causal constraints.

4 Discussion

The H1 gradient and the 37 of 79 high self-raters who reported nonparticipation form a descriptive puzzle: a self-assessment carries information about participation without determining it. This study-defined knowledge–action gap is not a judgment about nonparticipants, whose choices, opportunities, and goals may differ.

Perceived conceptual familiarity and confidence about starting warrant separate attention. The H2 know-how point estimates are positive in the primary and both sensitivity codings, with greater interval uncertainty than the intention estimates. That pattern makes perceived procedure worth measuring separately, while the one-item survey design leaves open how the

two self-assessments relate to demonstrated knowledge or actual task ability. An objective concept task and a procedural exercise would clarify whether confidence is calibrated, misplaced, or responsive to a genuine obstacle.

Stated twelve-month intention displays a positive conditional association with current participation across the three H2 outcome codings. The present-tense outcome and future-oriented intention were collected together, so the direction and timing of any process remain open. A later-wave observation could show whether intention precedes first participation, follows existing participation, or changes as circumstances change.

The reported primary barriers put material and perceived obstacles in the same frame. Their shares cover all expanded-instrument responses, not noninvestors alone. Their value is to distinguish questions for later work: Is a procedural difficulty about information, the task itself, or perceived ability? Is insufficient money a binding resource constraint or a choice to preserve liquidity? Does fear reflect distorted risk beliefs or an appropriate preference given the person's circumstances? A participation study that asks only about understanding would miss these alternatives.

The theoretical vocabulary helps organize those questions. TPB motivates measurement of intention and control-like beliefs alongside actual opportunities. FTT asks whether a person's bottom-line representation of a choice differs from the details they can recall. A proposed cognitive anchor would be a persistent initial belief or frame that later evidence might dislodge; market inertia would require observing a state over time as opportunities change. Their payoff is a research design that jointly observes representations, intentions, feasible choices, and subsequent action, rather than assigning a mechanism to a cross-sectional coefficient.

For financial education, these findings motivate evaluating conceptual learning, perceived ability to begin, and stated intention as separate outcomes. Fernandes, Lynch, and Netemeyer (2014) and Kaiser, Lusardi, Menkhoff, and Urban (2022) differ in their assessment of the behavioral effects of financial education, underscoring the importance of design and outcome choice. Our proposed defensive and opportunity-oriented framing could guide a comparison of information, practical rehearsal, and simplified decision tasks, with comprehension and suitability assessed alongside participation. More entry is not automatically better.

5 Limitations and continuation

The study is an observational, non-probability survey of submitted response records, not a probability sample or an independently verified register of unique people. Its findings describe these records and do not estimate a population participation rate. The exact test and profile intervals treat submitted records as independent observations; the available linkage cannot verify unique respondents, so unrecognized repeated submissions could affect inferential calibration. All responses were collected cross-sectionally; a future-oriented intention answer cannot be placed before or after an individual's investment decision. The open-ended "26+" age category prevents a verified 13–26 description of the full retained sample. Institutional opportunities may differ substantially across the recorded age bands and were not comprehensively measured.

The substantive measures are self-reports. The survey does not test objective investment knowledge, procedural performance, account ownership, investment amounts, portfolio quality, or welfare. Single questions about understanding, know-how, and intention cannot establish a

validated multi-item construct or how respondents interpreted "basics," "HOW," and "start." The intention and know-how items use agreement-style wording; later measurement could compare those statements with direct, task-specific questions. Intermediate 2–4 response positions on the ordered items lacked individual verbal labels, which may affect interpretation (Krosnick 1999). The ambiguous participation responses warrant the displayed prespecified sensitivity codings. The smaller H2 sample, sparse reference cells, and broad profile intervals limit precision; Firth estimation handles aspects of sparse-data fitting but not selection, confounding, or measurement error. The unavailable historical provisional snapshot limits exact accounting of additions between that snapshot and the final export, although the final cumulative export and preserved Wave 1 source were authenticated separately.

The next measurement design should pair each self-assessment with an appropriate task. Objective questions could test diversification, fees, and risk, while a procedural exercise could examine account-opening steps and recognition of reasons not to invest. Cognitive interviews or multiple items could test whether respondents understand the subjective prompts as intended. Follow-up observations could distinguish first entry, continued participation, and non-entry after a stated plan. Measures of available funds, liquidity needs, product access, and age-related permissions would help separate perceived ability from actual opportunity. An intervention study could then compare information, rehearsal, and simplified decision tasks against an appropriate comparison condition, assessing comprehension and suitability as well as participation. These additions would turn the proposed cognitive-anchor, inertia, and TPB–FTT ideas into testable questions while allowing that nonparticipation may be a well-informed choice.

6 Conclusion

Reported investing rose from 3.7% of respondents saying they did not understand the basics, to 13.5% saying "Sort of," to 53.2% saying "Yes." Yet 37 of the 79 respondents in the "Yes" category—46.8%—were not currently investing. In the expanded-instrument analysis, a one-category higher stated twelve-month intention had an odds-ratio estimate of about 2.07 for current participation, with a positive conditional association across all three prespecified outcome definitions. Perceived investing know-how also had a positive primary estimate of about 1.87, though its interval interpretation was more sensitive to classification of ambiguous participation answers. This coexistence of a strong gradient and substantial nonparticipation is a descriptive knowledge–action gap, not a test of objective knowledge.

These findings suggest a more useful architecture for studying financial decisions. A self-rating of understanding cannot, on its own, answer whether a respondent knows how to begin, intends to begin, has resources or access, or judges participation suitable. The reported barriers—starting knowledge, money, and fear of loss—reinforce the value of keeping procedural, material, and risk-related questions distinct. For household finance, the participation margin is a decision under opportunities and constraints, not a simple literacy test. For financial education, the implication is not that facts have failed; evaluation could also examine task navigation, calibrated confidence, and decision quality. The cross-sectional evidence establishes associations among answers, not the sequence or cause of action.

The next study should move from answers to decisions. Objective concept questions and practical tasks could be paired with self-assessments to test calibration; repeated observations

could show whether plans precede first entry, continued participation, or a reasoned choice not to invest. Measures of funds, permissions, product access, and risk capacity could separate perceived difficulty from a binding constraint. A comparison of information, procedural rehearsal, and simplified choices could ask not merely whether more people participate, but whether they make suitable choices. Cognitive anchors, market inertia, and the proposed bridge between motivation and representation belong in that future test. Understanding a financial choice and being positioned to act on it are different economic questions; explaining the distance between them is the next task for behavioral household finance.

Data and Code Availability

Respondent-level survey records are not publicly released or offered for third-party sharing under the project's participant-use and data-governance procedures. The manuscript and appendices report the survey items used in the analysis, participation-coding rules, model specifications, and aggregate results. Analysis code has not been publicly released with this version.

A Additional descriptive tables and figures

Table A1: Perceived understanding by participation response before binary coding.

Perceived understanding	No	Yes	Not sure	Sort of	Total
No	26	1	0	2	29
Sort of	32	5	3	0	40
Yes	37	42	0	3	82
Total	**95**	**48**	**3**	**5**	**151**

Note: The display uses substantive response categories after standardizing one exact case-only raw variant ("yes") to "Yes" in a derived field; the raw export was unchanged. All 151 retained records are shown.

Table A2: One selected primary investing barrier.

Selected response	N	%
Fear of losing money	20	18.3
I don't know where to start	24	22.0
I don't have enough money	22	20.2
I don't have access to a platform	8	7.3
I'm too young / not legally allowed	10	9.2
I'm not interested in investing	4	3.7
I have no barrier — I already invest	21	19.3
Total	**109**	**100.0**

Note: The item was administered to the 109 expanded-instrument records, including current investors. Counts sum to 109. Percentages are shares of all 109 responses to this item; they are not noninvestor-only rates. These are reported barriers, not estimated causes.

Table A3: Exponentiated intercepts: reference odds, not comparative odds ratios.

Model	Reference odds, exp(intercept)	95% profile interval
H1	0.05660	0.00633–0.21571
H2 primary	0.003730	0.000131–0.044729
H2 Sensitivity A	0.002478	0.000084–0.032607
H2 Sensitivity B	0.009105	0.000667–0.075439

Note: These values are retained for complete reporting of the specified fits. The H1 baseline is the No-understanding category. The H2 zero-valued item profile is outside the administered 1–5 scales and is not a typical respondent profile. Intervals are 95% profile penalized-likelihood intervals.

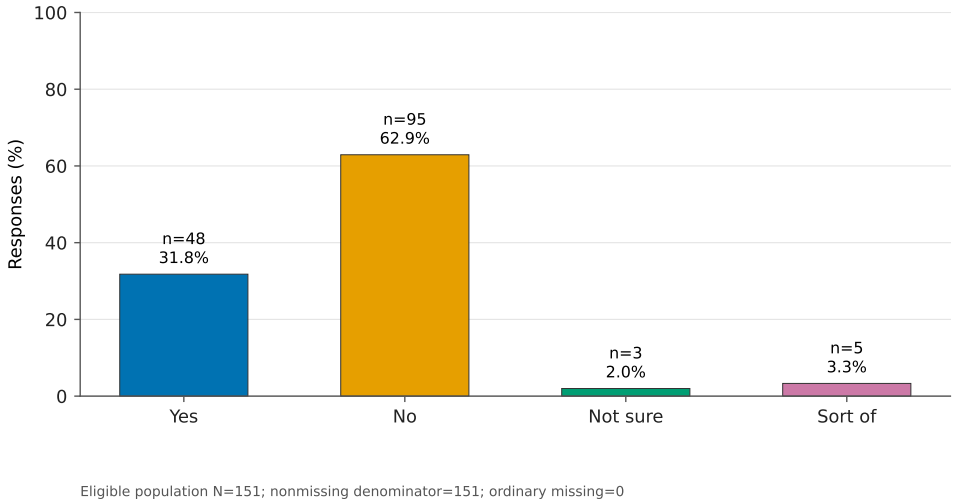


Figure A1: Participation responses before binary coding.

Note: Counts for Yes, No, Not sure, and Sort of in all 151 retained response records, after exact case-only standardization in the derived field.

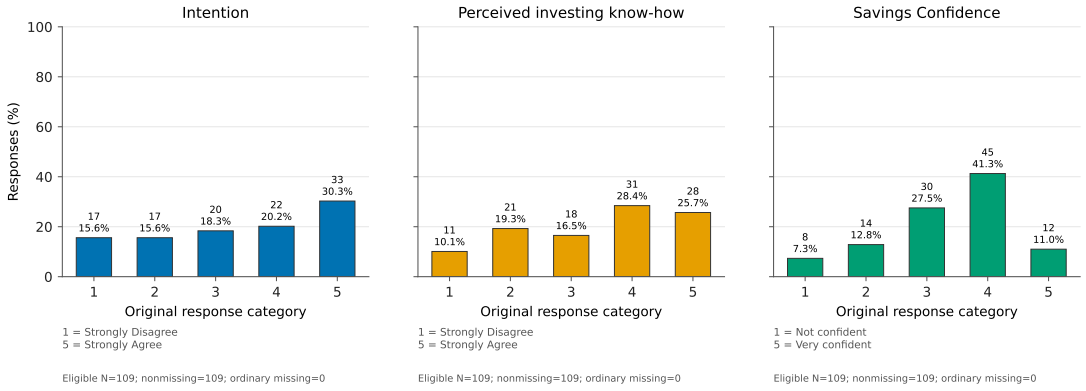


Figure A2: Expanded-instrument behavioral responses.

Note: Separate distributions of stated 12-month intention, perceived investing know-how, and savings confidence among 109 expanded-instrument records. Endpoint anchors are named as administered; response categories 2–4 have no individual verbal labels.

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