# The Demand for Robo-Advising: Adoption and Delegation Intensity

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**Abstract**

Technology has made financial advice cheaper and easier to reach than it was, yet the households who would gain most from it still use it least. We ask what stands in the way, using monthly proprietary data on 100,000 Chinese mutual fund investors and a robo-advisor that executes the portfolios it recommends, so that adoption and the share of fund wealth delegated are observed separately. Underdiversification and total turnover lower adoption but raise the share adopters delegate; confirmation bias raises adoption and lowers that share. Experienced fund investors are the slowest to adopt, and those who do adopt delegate less than others. Three explanations fit. Investors correct the mistakes delegation makes visible, fastest among those whose portfolios were the most concentrated or most heavily traded to begin with and who follow the platform most closely, which we call selective awareness. They surrender fund selection reluctantly even where they accept trading automation. And experienced adopters reclaim control as delegation tenure accumulates.

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## 1. Introduction

Households make costly and persistent investment mistakes, particularly those least equipped to avoid them (Calvet et al., 2007; Campbell, 2006). Underdiversification and excessive trading alone cost retail investors a material share of their investment returns (Barber & Odean, 2000; Goetzmann & Kumar, 2008). Yet traditional human financial advice has not adequately reached or served these households. Advisory fees and account minimums exclude many households with small accounts, precisely where these mistakes are concentrated. Moreover, advisers have been found to recommend investments that reflect their own mistaken beliefs or incentives rather than their clients' needs (Chalmers

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& Reuter, 2020; Egan et al., 2019; Foerster et al., 2017; Hackethal et al., 2012; Linnainmaa et al., 2021). Robo-advisors have emerged as a prominent response to these limitations. They construct and rebalance diversified, rule-based portfolios for fees of a few tenths of a percent, serve account sizes that human advisers have rarely reached (Reher & Sokolinski, 2024), and, among the investors studied to date, improve diversification and reduce trading mistakes (Bianchi & Brière, 2026; D'Acunto et al., 2019; Hao et al., 2022; Reher & Sokolinski, 2024; Rossi & Utkus, 2024). For broader assessments of the technology's promise and performance, see D'Acunto and Rossi (2023); for its implications for financial inclusion, see Philippon (2019).

Price barriers and conflicted incentives are unlikely to be the whole story, however. Particularly sharp evidence comes from Bhattacharya et al. (2012), whose field experiment addressed both frictions simultaneously by offering free, unbiased advice directly to roughly 8,000 retail clients of a German brokerage. Only about 5% obtained the advice. Moreover, those who stood to gain the most, according to the authors' measures of need, were the least likely to obtain it, while the few who did obtain it rarely followed its recommendations. Price and incentive frictions therefore cannot fully explain the limited use of financial advice. What else prevents households from seeking and following such advice remains unclear. Resolving this question matters because the welfare gains documented in the literature are conditional on take-up and adherence.

Robo-advice provides a useful setting in which to investigate these remaining barriers to demand because it substantially reduces two prominent frictions: price and access. Accordingly, this paper asks what governs investors' demand for robo-advice. Addressing this question requires treating use not as a single adoption decision but as a sequence of decisions. An investor first decides whether to adopt the service. Conditional on adoption, the investor decides how much of their fund wealth to delegate to the algorithm and whether to maintain that allocation over time. We refer to these post-adoption decisions as commitment. We therefore study demand along two distinct margins: entry, measured by initial adoption, and commitment, measured by the delegated share and the evolution of that share over the investor's tenure. The delegated share is the fraction of fund wealth allocated to the algorithm rather than to self-directed funds.

The two margins reflect different economic frictions. Entry requires investors to discover the product, evaluate it, and develop sufficient trust to try it. Commitment, by contrast, requires investors to delegate control over fund selection and trading. Investor characteristics may therefore relate differently to the two margins. Investors with concentrated portfolios may be slow to adopt because they do not perceive concentration as costly, yet delegate a substantial share after observing a diversified algorithmic portfolio perform well. Conversely, experienced fund investors may evaluate the product quickly but delegate only a small share because they value retaining control over fund selection, the very decision that robo-advice automates. A binary adoption indicator conflates these cases, which may help explain why the literature reaches differing conclusions about who adopts robo-advice (Baulkaran & Jain, 2023; Belanche et al., 2019; Brenner & Meyll, 2020; D'Acunto et al., 2019; D'Hondt et al., 2020; Piehlmaier, 2022; Scherer & Lehner, 2025).

Testing this distinction requires evidence that is rarely available within a single setting, a constraint recognized in the robo-advising literature. D'Acunto et al. (2019) report that the biases they study "were not fully eliminated, because investors could place additional trades on top of what the

robo-adviser suggested." This observation highlights why the commitment margin matters: the extent to which adoption can alter portfolio choices is bounded by the share investors delegate, which their data do not observe. Surveys measure willingness to use advice but not realized delegated shares. Data from robo-advising providers record product use but often omit the self-directed holdings and trades needed to measure delegation. Portfolio datasets observe holdings but rarely permit a comparison between delegated and self-directed investments within the same account. The closest existing analogue is the retirement-plan setting, in which participants who hold a target-date fund alongside other funds reveal a delegated share (Mitchell & Utkus, 2022). However, the institutional features of these plans prevent that share from carrying the demand interpretation required here.[1]

We study BangNiTou, a fully delegated mutual fund robo-advisor founded by Ant Group and Vanguard and embedded in Alipay, with asset-based fees ranging from 0.20 to 0.50 percent. Within the same account, we observe self-directed fund holdings, delegated robo assets, automatic investment plans, cash-management balances, platform engagement, transactions, and realized returns. The setting is particularly useful because it records three ways of holding funds that differ in the extent of algorithmic delegation. Investors can select and trade funds themselves, which we label *DIRECT*. They can instead establish an automatic investment plan that purchases their chosen funds on a schedule, which we label *AUTO*. This option automates the timing of purchases while leaving fund selection to the investor. Finally, investors can delegate both fund selection and trading to the algorithm through the robo-advising service we study, which we label *RA*. Because all investors in our sample invest through mutual funds, they already delegate security selection within each fund to professional fund managers. This feature holds constant their exposure to delegated security selection through mutual funds. The decision rights that vary across the three products are therefore which funds to hold and when to trade them.

We observe 100,000 mutual fund investors monthly from September 2020 through September 2024, yielding 4.9 million investor-months. We also construct ten behavioral measures from each investor's own fund trades before the decisions we model. These measures summarize how investors traded when making their own fund decisions, and each measure is constructed using only trades that precede the outcome it is used to explain. Of the investors in our sample, 70,034 use RA at least once, whereas 29,966 hold mutual funds but never use RA. The sample is deliberately stratified by whether an investor ever adopts RA. Consequently, the observed adoption rate should not be interpreted as the platform-wide penetration rate.[2]

Because the delegated share is observed only for adopters, we estimate Heckman selection models that jointly estimate whether an investor adopts RA and, conditional on adoption, how much the investor delegates. We use platform tenure as an exclusion restriction: it enters the adoption equation as

[1] In a retirement plan, the investment menu is set by the employer, entry is frequently a default rather than an active choice, and the non-delegated sleeve is drawn from the same restricted menu rather than constructed independently by the household. Mitchell and Utkus (2022) report that mixed target-date investors, defined as those directing part of their contributions to a target-date fund, constitute roughly one-third of target-date fund holders. Thus, although the delegated-share margin is present in the data, its interpretation as demand does not carry over to the present setting.

[2] See Section 4.2 for further discussion of the sampling design. Stratification improves power for studying rare events but prevents the design from recovering their unconditional prevalence (Manski & Lerman, 1977). Our estimand is the cross-sectional slope within the sampled strata, and we report the unweighted estimates because they provide greater precision. Weighted estimates yield qualitatively similar behavioral relationships, although the selection term is sensitive to reweighting; Section 8 and Online Appendix Table A.3 provide details.

a measure of opportunities to discover the product but is excluded from the delegated-share equation. We report unweighted estimates because our object of interest is a slope within the sampled population rather than population prevalence. Re-estimating the models with sampling weights produces the same qualitative conclusions about how investor characteristics relate to adoption and the delegated share. We estimate the models separately by month and report Fama–MacBeth averages with Newey–West standard errors (Fama & MacBeth, 1973; Newey & West, 1987). This approach is appropriate because the characteristics of interest change slowly, if at all, and investor fixed effects would absorb the cross-sectional variation on which our analysis relies. The key identifying assumption is that, conditional on the included controls, platform tenure affects the delegated share only through adoption. Section 8 examines the sensitivity of our findings to relaxing this assumption.

Our estimates show that the characteristics associated with adoption are not necessarily those associated with post-adoption delegation. Several characteristics are associated with the two margins in opposite directions. Wealth primarily predicts adoption rather than commitment. A one-standard-deviation increase in log total financial assets is associated with a 14.2-percentage-point increase in adoption, equivalent to about 44 percent of the mean participation rate. By contrast, its negative association with the delegated share becomes close to zero after behavioral controls are included. Fund experience is positively associated with RA participation in the pooled sample but negatively associated with the delegated share conditional on adoption; it also predicts a lower probability of first adoption. Risk preference is positively associated with both margins and therefore does not distinguish between adoption and commitment.

The behavioral measures reveal the paper's central pattern. A one-standard-deviation increase in portfolio concentration is associated with a 6.5-percentage-point decrease in adoption, equivalent to about 17 percent of the 37.2 percent mean participation rate, and a 2.0-percentage-point increase in the delegated share among adopters, equivalent to about 13 percent of its 15.1 percent conditional mean. Total turnover exhibits the same cross-margin pattern: higher turnover is associated with a 3.0-percentage-point decrease in adoption and a 4.5-percentage-point increase in the delegated share, equivalent to 8 percent of the mean participation rate and 30 percent of the mean delegated share, respectively. Confirmation bias exhibits the opposite pattern, with a 3.0-percentage-point increase in adoption and a 2.2-percentage-point decrease in the delegated share. Limited attention, gambling preference, and herding are negatively associated with both margins. Thus, for concentration and turnover, investors who exhibit more of the behaviors that robo-advice is designed to address are less likely to adopt but delegate larger shares conditional on adoption. This pattern is not confined to the pool of ongoing users. When we focus on investors' initial adoption and delegation decisions, concentration is associated with a lower probability of first adoption and a 4.5-percentage-point increase in the initial delegated share. The divergence is therefore present at entry rather than arising only among investors who remain users.

These opposite associations show why a binary use indicator is too coarse for a delegated product: it collapses entry and commitment into a single outcome even though the two margins can move in opposite directions. The estimates do not, however, identify why the margins diverge. Several mechanisms can generate the same sign pattern, and the baseline cross-section cannot distinguish among them. We therefore test each of the three mechanisms developed below using evidence beyond

the baseline cross-sectional relationships. We also evaluate a fourth explanation, namely that recent returns account for the entire pattern.

The first mechanism is selective awareness. RA is not only a substitute for self-directed portfolio management; it also provides a salient benchmark against which investors can assess their own portfolio choices. Delegation therefore need not respond only to differences in ability that investors already understand. Exposure to a diversified, rule-based portfolio may reveal the consequences of concentration and frequent trading and update investors' beliefs about the quality of their own portfolio management. This interpretation predicts that these behaviors should decline after adoption, especially among investors with high pre-adoption levels and those who engage more frequently with the platform. Behaviors for which RA provides no comparable benchmark should not respond similarly.

The findings follow this pattern. Concentration and turnover decline with RA tenure, with substantially larger declines among investors in the top decile of the respective pre-adoption measure. Over one year, the additional declines are 0.13 for concentration and 0.19 for turnover, equivalent to 0.4 and 0.5 of their pooled standard deviations, respectively. The declines also become steeper with login frequency, whereas the comparison biases do not exhibit the same negative engagement gradient. Because login frequency is neither randomly assigned nor specific to RA, these patterns do not directly identify belief updating. They are nevertheless consistent with a selective learning process: the behaviors that RA displays and corrects most visibly respond more strongly to exposure than behaviors for which it provides no equally salient benchmark. This selectivity also helps explain why attention, gambling, and herding depress both margins. The benchmark interpretation has a rational foundation. Investors who optimally learn about the assets they already hold may rationally choose concentration, so its cost need never become salient to them (Van Nieuwerburgh & Veldkamp, 2010). Concentrated holdings may also reflect rational preferences rather than error (Polkovnichenko, 2005), and portfolio attention may be selective rather than continuous (Sicherman et al., 2016).

The second mechanism is decision authority. A robo-advisor normally combines two functions: it automates purchases and takes over the choice of which funds to hold. An investor who does not use RA may therefore be avoiding either automation or the loss of authority over fund selection. AUTO separates these two dimensions by automating the timing of purchases while allowing the investor to continue choosing the funds. We stack each investor-month into DIRECT, AUTO, and RA rows and include investor-month fixed effects, which absorb wealth, market conditions, and other factors common to the same investor in the same month. The resulting coefficients capture two incremental contrasts: the step from self-direction to automation and the further step from automation to full delegation. The characteristics associated with the first step differ from those associated with the second, and several have opposite associations with the two steps. Concentration raises the automation step by 5.8 percentage points but lowers the delegation step by 3.3 percentage points. Turnover produces the same pattern, raising the first step by 3.5 percentage points and lowering the second by 3.2 percentage points. Engagement and attention bias produce the opposite pattern: each lowers the automation step by 4.2 percentage points, while they raise the delegation step by 1.6 and 2.6 percentage points, respectively. A general preference for automation cannot readily explain these reversals because it should move the two steps in the same direction or produce no incremental difference between AUTO and

RA. For each of these four characteristics, as well as anchoring, the second-step coefficient is significantly different from zero and opposite in sign to the first-step coefficient. This interpretation builds on experimental evidence that decision rights have intrinsic value beyond their instrumental payoff (Bartling et al., 2014) and that people retain authority even when doing so is costly (Fehr et al., 2013). We bring this mechanism to a portfolio setting in which the value of decision authority is revealed through investors' choices rather than their stated preferences. The design also separates dimensions that algorithm-aversion experiments bundle. Dietvorst et al. (2018) restore algorithm use by allowing subjects to modify its output; AUTO is the field counterpart of this retained control.

The third mechanism is reclaiming control. It concerns how commitment evolves over time rather than at a single point and accounts for a baseline result that the first two mechanisms do not: fund experience is positively associated with pooled participation but negatively associated with both the conditional delegated share and first adoption. If experience increases the value investors place on selecting funds themselves, adoption need not imply sustained commitment. Among active adopters, longer RA tenure is associated with lower delegated shares, and this gradient is steeper for investors with more pre-adoption fund experience, with interaction coefficients ranging from $-0.023$ to $-0.043$. Self-directed portfolios exhibit the same pattern: concentration falls sharply at adoption but returns toward its pre-adoption level over roughly two years. Delegation is therefore not an absorbing state and, particularly for experienced investors, may be partial or transitional.

Recent performance is the leading alternative explanation for all three mechanisms, but the evidence qualifies rather than displaces the mechanism account. If households simply chase returns, the characteristic gradients may proxy for performance feedback. Performance is associated with demand, but in a structured way: the magnitude of investors' own returns matters at first adoption, when both gains and losses deter entry, whereas their sign matters at exit, when favorable self-directed returns predict departure from RA. Including performance does not attenuate any of the characteristic gradients. An additional residual channel also remains. In the unweighted estimates, selection on unobservables is positive and does not weaken as controls are added. The coefficient on the selection correction increases from 0.158 to 0.212 after skill and the behavioral measures are included, indicating that the marginal adopter selected by unobserved factors delegates more, not less. This is the only coefficient in the paper that does not survive population reweighting, and we interpret it accordingly. Two features of the estimate inform the nature of the unobservable. Its positive sign is inconsistent with tentative entry or adverse selection into the product, either of which would predict below-average delegation and a negative coefficient. Its stability suggests that measured skill and the ten behavioral biases do not proxy for it. A disposition that increases both entry and commitment is consistent with money-doctor theories of trust in delegated management (Gennaioli et al., 2015; Guiso et al., 2008). We therefore offer trust as an interpretation of the residual rather than as a finding, since the design identifies it only through the covariance between the two equations, not as a measured belief. The evidence throughout is observational: the mechanism labels rest on sign patterns, timing, and dose-response relationships, not on randomized variation in beliefs or in the authority investors surrender, and we do not estimate causal effects of adoption.

One implication follows, which we offer as a hypothesis for platforms to test rather than as a result established by our analysis. If selective awareness constrains entry, a platform seeking to reach

underdiversified or high-turnover investors could make their existing portfolio choices salient before they decide whether to adopt. A natural test would compare this intervention with advertising based on the algorithm's past performance.

The paper proceeds as follows. Section 2 places the paper in the literature and states what it contributes. Section 3 sets out the two-margin framework and shows why opposite signs are a generic outcome when entry costs and benefit loadings differ. Section 4 describes the setting, the data, and the designs. Section 5 reports the baseline Heckman evidence, the first-adoption evidence, and why the baseline requires mechanism tests. Section 6 tests the three mechanisms and the performance alternative. Sections 7 and 8 reports post-adoption consequences and robustness. Section 9 concludes.

## 2. Related Literature and Contribution

We make four contributions. First, separating adoption from conditional delegation helps reconcile contrasting findings on who uses robo-advice (Baulkaran & Jain, 2023; Belanche et al., 2019; Brenner & Meyll, 2020; D'Acunto et al., 2019; D'Hondt et al., 2020; Piehlmaier, 2022; Scherer & Lehner, 2025). A binary use indicator collapses entry and commitment into a single measure. When the same characteristic has opposite associations with the two margins, differences in sample composition can produce different combinations of these associations. Prior studies can therefore reach different conclusions without implying inconsistent underlying relationships.

This reconciliation is clearest in relation to Piehlmaier (2022), who finds that overconfident investors are more likely to adopt. The comparison depends on which form of overconfidence is measured. His measure is survey-based and captures self-assessed confidence relative to tested knowledge. Our measures are revealed through trading behavior. Among the proxies used in the literature, we jointly include the two most relevant to delegation: total turnover (Barber & Odean, 2000, 2001) and speculative turnover (Barber & Odean, 2002; Han & Kumar, 2013). Total turnover is negatively associated with adoption but positively associated with the delegated share. A binary use indicator can therefore capture different combinations of these two associations across samples, making the two sets of findings compatible once entry is separated from commitment. Speculative turnover, conditional on total turnover, is negatively associated with both margins, so separating the margins does not reconcile that result. Speculative turnover captures the belief that an investor knows when to exit and re-enter a position, a view that a rule-based portfolio cannot accommodate. These investors therefore neither enter nor commit.[3]

Separating entry from commitment also extends a distinction already established in household finance. This literature distinguishes between whether a household participates in a market and how well it invests conditional on participation (Calvet et al., 2007; Campbell, 2006). For a product that the household does not manage itself, adoption and portfolio quality are not directly linked. After adopting a robo-advisor, investors still choose the fraction of their fund wealth to delegate and continue managing the remainder. Their overall portfolios combine an algorithm-managed sleeve with a self-directed sleeve, with the delegated share determining the relative weight of each. Adoption and the quality of the resulting portfolio can therefore move separately. A policy that increases adop-

[3] Stated confidence and realized overtrading need not move together, nor must the two revealed measures. The choice of proxy may therefore contribute to differences across studies.

tion without changing delegated shares would alter portfolios much less than the adoption rate alone suggests.

Second, we treat behavioral investment patterns as determinants of demand for a corrective technology rather than as nuisance controls. Behavioral finance documents costly household investment mistakes (Barber & Odean, 2000, 2001, 2008; Goetzmann & Kumar, 2008; Kumar, 2009), while the effects literature shows that algorithms can reduce these mistakes conditional on use (Bianchi & Brière, 2026; D'Acunto et al., 2019; Reher & Sokolinski, 2024; Rossi & Utkus, 2024). Together, these literatures leave selection into use largely unaddressed. We supply this intermediate step, which matters because the aggregate gains from a corrective technology depend on whether the investors who stand to gain most expose themselves to it. This selection problem is dynamic because perceived need need not be fixed before adoption. Exposure to a rule-based portfolio can provide information about the quality of the investor's own portfolio management, making use itself part of the process through which demand evolves. Rossi and Utkus (2024) come closest, finding that the investors who benefit most from a hybrid robo-advisor are also the most likely to adopt and remain enrolled. Our finding runs in the opposite direction, but the difference reflects the object being measured. Their measure is a gain already realized among enrolled investors, so their selection result is conditional on adoption and speaks primarily to who remains enrolled. We construct potential need from investors' own trades before they decide whether to adopt, so our measure speaks to who enters in the first place. Both findings can hold simultaneously: investors with the most to gain may be slow to enter, while those who benefit most after entering may be more likely to remain.[4]

Third, we bring the economics of decision rights to household portfolio choice and measure the cost of relinquishing authority. Theoretical and experimental work shows that decision rights can have value beyond their instrumental payoffs, that people may retain authority even when doing so is costly, and that holding a right and exercising it are distinct (Aghion & Tirole, 1997; Bartling et al., 2014; Fehr et al., 2013). This literature typically infers the value of authority from willingness to pay in laboratory settings, where the right is abstract and the price of retaining it is set by the experimenter. Household finance has made limited use of this idea, partly because portfolio data rarely reveal a moment at which an investor visibly transfers a decision right. Our setting contains such a choice, and the right is concrete: authority over which funds to hold and the trades used to implement that choice.

Our measurement follows from the product menu rather than an elicited valuation. In the same month, an investor can manage a portfolio directly, use an automatic plan that schedules purchases while preserving fund-selection authority, or delegate fund selection to the robo-advisor. Comparing AUTO with RA within the same investor-month holds fixed the platform, interface, and mechanical execution of trades, leaving fund-selection authority as the distinguishing feature. The two incremental steps do not move together. Portfolio concentration raises the automation step by 5.8 percentage points but lowers the delegation step by 3.3 percentage points, and total turnover moves the two steps in the same directions. Investors with active views about which funds to own tilt toward a product that automates trading while preserving fund choice and away from one that transfers that choice. This pattern is consistent with a cost of relinquishing authority but not with a general preference for au-

[4]The settings also differ in ways that could produce the same contrast. Their setting is a hybrid advisor that retains a human adviser and serves previously self-directed clients with much larger accounts. Ours is a mass-market platform with small accounts and effectively no minimum.

tomation. Because the coefficients are incremental, the estimates describe a relative tilt: concentrated investors still favor RA over DIRECT by a net 2.5 percentage points, but they favor the fund-choice-preserving product by more. Retaining fund-selection authority therefore has a measurable cost in this market, reflected in delegated wealth rather than stated willingness to pay.

This interpretation also speaks to the algorithm-aversion literature. Subjects may abandon an algorithm after observing it err (Dietvorst et al., 2015), return to it when allowed to modify its output (Dietvorst et al., 2018), or prefer algorithmic to human judgment (Logg et al., 2019). These designs generally combine acceptance of an algorithm with surrender of decision authority because subjects cannot accept one without the other. When an experiment separates the two, its findings point in the same direction as ours. Holzmeister et al. (2023) find that subjects prefer delegating to an algorithm over delegating to a compensated professional, suggesting that resistance is not directed solely at the machine. They also find that subjects who rate their own decision-making more highly delegate less, a pattern consistent with an authority cost and mirrored by our fund-experience result. Our product menu provides a field setting in which automation and authority can be separated: investors can accept automation through AUTO while retaining control over fund selection. The distinction extends beyond robo-advice to managed accounts, target-date funds, and advisor-sold products, which automate different tasks and transfer different decision rights.

Fourth, we add a commitment margin to the literature on demand for financial advice. The relevant question is not only whether a household accepts advice but also how much of its portfolio it delegates after doing so. Existing work asks whether households obtain advice when it is available and affordable. It finds that many do not, that those who stand to gain most are often the least likely to obtain it, and that the advice received may be miscalibrated or conflicted (Bhattacharya et al., 2012; Foerster et al., 2017; Hackethal et al., 2012; Linnainmaa et al., 2021; von Gaudecker, 2015). Studies that follow households after they accept advice have focused on what advisers do rather than on how much authority households retain. For example, Greig et al. (2025) study a hybrid robo-advisor in which portfolio management is automated and human advisers provide other services, and they relate retention to adviser behavior. In our setting, the adviser is fixed because advice is provided by an algorithm, while investors choose how much of their portfolios to delegate.

The closest paper is Bhattacharya et al. (2012), whose experiment motivates our analysis but addresses a different question. They test whether improving the supply of advice is sufficient by offering clients free, unbiased advice. The treatment is the offer, the outcomes are whether clients obtain and follow the advice, and the finding is that most do neither. We take this result as our premise and ask what determines demand once price and access are no longer the principal constraints. Their study evaluates whether a supply-side intervention is sufficient; ours studies the residual demand for advice. The two papers nevertheless share a subject and a puzzle. Both study retail investors who could benefit from correcting their portfolios, find that those with the most to gain are the least likely to obtain help, and treat this gap as the object to be explained rather than as noise.

Three features of our setting allow us to take this demand question further. First, their intervention provides a recommendation that clients must still implement, whereas our product is delegated and executes portfolio decisions automatically. This distinction separates reluctance to delegate from failure to implement a recommendation. Second, their main outcome is an accept-or-reject decision, whereas

our panel distinguishes first use, the share of wealth delegated, continuation, exit, and re-entry. We can therefore measure commitment directly. Third, they measure need using survey and portfolio information collected at one point in time, whereas we construct each investor's behavioral measures from their own trades in the months preceding adoption. This allows us to ask which behaviors predict each margin. These differences are not criticisms of their design, which was constructed to answer a different question and does so cleanly.

Finally, we follow adopters month by month after entry, showing that demand for advice is not settled at adoption. Adoption studies often treat use as an absorbing state, so retaining assets may be interpreted as evidence that a product has succeeded. Our tenure evidence instead reveals a partly reversible adjustment. The tenure slope of the delegated share is close to zero for the least experienced adopters but reaches $-0.051$ at one standard deviation above mean pre-adoption experience, while self-directed portfolios return toward their pre-adoption concentration over roughly two years. Viewed alongside selective awareness, these results suggest that successful correction may reduce demand for continued delegation. Exposure to the algorithm is followed by declines in the behaviors it corrects, while delegated shares subsequently fall fastest among adopters best equipped to manage their own portfolios. Both patterns connect delegation to learning through trading in household finance (Linnainmaa, 2011; Nicolosi et al., 2009; Seru et al., 2010), and both imply that assets retained alone are an incomplete measure of whether a corrective product has succeeded.

## 3. A Framework for the Two Margins of Delegation

This section develops a deliberately minimal framework. Its purpose is not to explain why a particular investor values delegation, but to establish two baseline results that discipline the empirical analysis. First, the same investor characteristic can have opposite effects on adoption and the delegated share. Second, characteristics can enter first adoption, the conditional share, continuation, and the evolution of the share over tenure through different combinations of primitives. No single use indicator therefore summarizes the demand for delegation. The framework treats these loadings as reduced-form objects. The mechanism sections interpret their economic origins; the model establishes which patterns can arise and how they map into the empirical objects.

### 3.1. Setup

Investor $i$ has fund wealth normalized to one and a vector of characteristics $X_i$, including the demographics, preferences, skills, and behavioral measures used in the empirical analysis. The investor may place a share $s \in [0, 1]$ of fund wealth under delegated management. The per-period payoff from delegating share $s$ is

$$v_i(s) \;=\; a_i s \;-\; \tfrac{b}{2} s^2, \qquad b > 0, \tag{1}$$

where $a_i$ is the perceived net marginal benefit of the first unit of delegation. It combines the expected improvement of the delegated sleeve over self-management, net of fees and any cost the investor attaches to ceding fund selection. We do not decompose these components here. The perceived benefit

has observed and unobserved components:

$$a_i \;=\; \alpha' X_i + \theta_i, \tag{2}$$

where $\theta_i$ is a latent disposition toward delegated management that is not captured by the observed covariates.

Let $k_i$ denote the per-period equivalent of the one-time cost of entry:

$$k_i \;=\; \kappa' X_i + u_i, \tag{3}$$

where $u_i$ is an unobserved entry-cost component. We assume that the support of (3) satisfies $k_i \geq 0$ and, for the selection result below, that $u_i$ is independent of $\theta_i$.[5] The entry cost encompasses discovery, evaluation, and setup, the frictions that precede an investor's first delegation decision. The central premise is that nothing requires $\alpha$ and $\kappa$ to be aligned. A characteristic can reduce the cost of discovering and evaluating the product while lowering its perceived marginal benefit, or increase the entry cost while raising that benefit. The data, rather than the framework, determine these loadings.

Because the investor can always choose $s = 0$, non-use is optimal when $a_i \leq 0$. We therefore focus on the region $0 < a_i < b$, which ensures an interior delegated share. Define the per-period value of access as

$$V(a_i) = \max_{s \in [0,1]} v_i(s).$$

On the maintained interior region,

$$V(a_i) = \frac{a_i^2}{2b}.$$

Conditional on entry, the investor chooses

$$s_i^* \;=\; \frac{a_i}{b}, \tag{4}$$

and enters when the value of access covers the entry cost:

$$V(a_i) \;=\; \frac{a_i^2}{2b} \;\geq\; k_i. \tag{5}$$

Restricting attention to $a_i > 0$ is substantive rather than notational. On this region, the entry index is increasing in the latent disposition $\theta_i$, a monotonicity condition used in the selection result below.

### 3.2. Opposite Signs Across the Two Margins

Consider a characteristic $x$ with loadings $(\alpha_x, \kappa_x)$. The conditional delegated share depends only on the benefit loading:

$$\frac{\partial s_i^*}{\partial x} \;=\; \frac{\alpha_x}{b}. \tag{6}$$

[5] A forward-looking investor would compare the discounted stream of access values, net of maintenance costs, with the one-time entry cost. If perceived benefits drift, waiting would also have option value. These refinements do not change the central separation between the loadings that govern entry and those that govern the delegated share.

By contrast, the net-value index governing entry changes with $x$ according to

$$\frac{\partial}{\partial x}\left(\frac{a_i^2}{2b} - k_i\right) \;=\; s_i^*\alpha_x - \kappa_x. \tag{7}$$

The benefit loading therefore enters the entry margin scaled by the share the investor would delegate. The economic intuition is straightforward. An increase in the perceived marginal benefit of delegation has little value at entry when the investor would delegate only a small fraction of fund wealth. The entry cost, by contrast, is incurred in full regardless of that share. The empirical counterpart of $s_i^*$ is the mean conditional delegated share of 0.151. At this share, a benefit loading must be approximately $1/0.151$, or 6.6 times, as large as an opposing entry-cost loading to offset it. Opposite signs across the two margins can therefore arise readily at delegated shares of the magnitude observed in the data.

**Prediction 1.** *The conditional-share gradient of a characteristic $x$ has the sign of $\alpha_x$. The entry-index gradient has the sign of $s_i^*\alpha_x - \kappa_x$, as does the adoption-probability gradient under a monotone threshold-crossing specification. If $\alpha_x > 0$ and $\kappa_x > s_i^*\alpha_x$, the characteristic lowers adoption while raising the conditional share. If $\alpha_x < 0$ and $\kappa_x < s_i^*\alpha_x$, it raises adoption while lowering the conditional share. Because $s_i^*$ is small, relatively modest entry-cost loadings can generate either pair of opposite signs.*

The latent disposition $\theta_i$ provides the economic source of the selection structure used in the empirical analysis. On the region $a_i > 0$, a higher $\theta_i$ increases both the entry index in (5) and the delegated share in (4). Under the maintained independence between $\theta_i$ and $u_i$, the unobserved components of selection and conditional delegation are therefore positively related. This is the sample-selection problem formalized by Heckman (1979); the empirical Heckman specification adds the corresponding distributional assumptions.

*Remark* 1. A latent disposition that raises the perceived benefit of delegation generates positive dependence between the unobserved determinants of entry and the delegated share. Under the empirical Heckman specification, this dependence implies a positive coefficient on the selection correction in the share equation.

The remark concerns selection into the stock of active users, which is the margin addressed by the pooled Heckman model. Selection into first use depends on the latent disposition as it stands at entry. The dynamic extension below shows why the first-adoption and pooled selection corrections need not coincide.

### 3.3. Margins Over Time

The second baseline result is that investor characteristics can affect more than the initial decision to try the product. Let the perceived benefit evolve with exposure:

$$a_{i\tau} \;=\; a_i + (\psi' X_i + \eta_i)\,\tau, \tag{8}$$

where $\tau$ denotes months of active use, $\psi$ is an unrestricted drift loading, and $\eta_i$ is a mean-zero latent component of the drift that is not captured by the observed covariates. The drift summarizes any

effect of exposure on the perceived benefit, including updating, habituation, or changes in tastes, without selecting among these interpretations. We maintain $a_{i\tau} < b$ over the relevant range so that the delegated share of an active user remains interior:

$$s^*_{i\tau} = \frac{a_{i\tau}}{b}.$$

Keeping the account active entails a per-period cost $m \geq 0$. An active user continues while $V(a_{i\tau}) \geq m$, or equivalently while

$$a_{i\tau} \geq \sqrt{2bm},$$

and exits when the perceived benefit falls below this threshold. We take $m$ to be small relative to the entry costs of relevant entrants, so they begin above the maintenance threshold and only a sufficiently negative drift induces exit. The magnitude of $m$ determines how closely continuation tracks the delegated share. When maintenance costs are close to zero, as for an account that is costless to leave open, the exit threshold is also close to zero. Continuation can then remain high even as delegated shares decline.

This configuration matches the coexistence of a 97.35 percent monthly continuation rate with declining delegated shares among the most experienced adopters. It also allows the reclaiming-control gradient in Section 6.3 to coexist with near-universal continuation. A model in which a high threshold caused the same index to govern both the delegated share and survival could not accommodate these two patterns simultaneously.

The empirical margins now reflect different combinations of loadings. First adoption depends on $(\alpha_x, \kappa_x)$ through (7), evaluated at zero tenure. The delegated share at entry depends on $\alpha_x$, while its subsequent path depends on $\psi_x$. The tenure slope of the delegated share and its interaction with investor characteristics therefore trace the drift among active users. Because exit is rare, survivorship is less likely to dominate this slope, although rare exit does not eliminate selection concerns. Continuation and exit depend on both $\psi_x$ and $m$, because only a sufficiently negative drift causes the perceived benefit to cross the maintenance threshold. Nothing requires the sign of the drift loading $\psi_x$ to match the static share loading $\alpha_x$.

**Prediction 2.** *First adoption, the delegated share at entry, the tenure slope of the share, and continuation depend on $(\kappa_x, \alpha_x, \psi_x)$ through different combinations. Without proportionality restrictions on these loadings, the same characteristic can deter first adoption, raise the initial delegated share, and predict a declining share over tenure, followed by exit if the maintenance threshold eventually binds.*

The latent drift plays for the dynamic margins a role analogous to that of the latent disposition in the static margins. At first use, the dependence between the selection and share equations arises from $\theta_i$, while the entry-cost shock $u_i$ affects selection alone. Among active users, the delegated share also contains the accumulated component $\eta_i\tau$, and survival depends on the evolution of $a_{i\tau}$. The pooled selection correction can therefore reflect both the initial disposition and accumulated drift heterogeneity, so it can differ from, and may exceed, the first-adoption correction. The small and statistically insignificant first-adoption Mills estimate is consistent with, but does not establish, weakly positive selection at entry. A genuinely negative first-adoption correction, such as the cautious-experimenter interpretation considered in the lifecycle analysis, lies outside the single-disposition benchmark. It

would require an additional latent entry motive associated with a lower initial delegated share. Latent drift can cause the pooled correction to diverge after entry, but it cannot by itself reverse the entry-stage sign at $\tau = 0$.

A composition result follows for pooled participation, the outcome summarized by a single use indicator. The stock of active users at a given date combines the flow of new entrants with accumulated survival. Two channels can therefore produce a pooled coefficient whose sign differs from that of the first-adoption coefficient. The first is differential survival. A characteristic satisfying $\kappa_x > s_i^* \alpha_x$ but $\psi_x > 0$ can be underrepresented among new adopters while becoming overrepresented among longer-tenure survivors.

The second channel allows the characteristic itself to evolve with use, rather than remaining fixed as in the baseline setup. For example, cumulative fund experience may increase mechanically over time. Writing this extension as $x_{i\tau} = x_{i0} + q_i \tau$, active-user observations can contain systematically higher values of $x$ than the entry observations of the same investors. The pooled cross-section then reflects both the entry relationship and the accumulation of the characteristic. Either channel allows pooled participation and first adoption to have coefficients of opposite signs without inconsistency. This is the rationale for interpreting the pooled and flow estimates jointly in Section 5.3.

### 3.4. What the Framework Delivers and What It Does Not

The framework provides the economic counterparts of the objects estimated in the empirical analysis. The entry condition in (5) corresponds to the selection equation, and the share rule in (4) corresponds to the outcome equation. The latent disposition generates the correlated unobservables summarized by the inverse Mills ratio. The excluded platform-tenure variable shifts $k_i$ without entering $a_i$, while the drift is the object traced by the tenure and trajectory specifications. The framework also establishes the two baseline results. Opposite signs can arise naturally when benefit and entry-cost loadings differ, and characteristics affect continued use as well as initial adoption whenever their drift loadings are nonzero.

Two boundaries are important. First, because fund wealth is normalized to one, the framework describes delegated shares but is silent about asset levels. An extension incorporating wealth levels would be required to interpret delegated assets, so the share-based and level-based selection corrections reported in the robustness analysis need not coincide. Second, the framework deliberately does not explain why any loading has a particular sign. A negative entry gradient combined with a positive share gradient is consistent with multiple accounts of how a characteristic affects perceived benefits and entry costs. Similarly, the drift can represent learning, salience, habituation, or changes in tastes. The framework therefore cannot substitute for the mechanism evidence in Section 6, which applies a separate design to each candidate interpretation. Its contribution is to map the primitive loadings into the empirical margins and to show that opposite signs do not require knife-edge restrictions.

# 4. Setting, Data, and Empirical Design

## 4.1. Institutional Setting

BangNiTou is a fully delegated mutual fund robo-advisor founded jointly by Ant Group and Vanguard and embedded in Alipay, China's dominant mobile-payments and wealth-management platform. The product launched in April 2020 as part of China's third-party fund-advisory pilot and reached one million users within a year (Bloomberg News, 2021). Alipay is also the largest third-party mutual fund distributor in China, and the economics of fund distribution through such platforms differ from those of bank and brokerage channels (Hong et al., 2025). After collecting suitability information, BangNiTou constructs and periodically rebalances a diversified mutual fund portfolio on the investor's behalf. Annual advisory fees range from 0.20 to 0.50 percent of delegated assets, placing the service in the same broad price range as Betterment, Wealthfront, and other retail robo-advisors.[6]

The setting is useful because the same Alipay account provides three ways to invest in mutual funds. The first is direct investing, which we label *DIRECT*: investors select and trade funds themselves. The second is an automatic investment plan, which we label *AUTO*: investors select the funds, while the platform purchases them on a fixed schedule; selling and rebalancing remain under the investor's control. The third is BangNiTou, *RA*: investors delegate both fund selection and trading to the algorithm, which selects, purchases, sells, and rebalances funds on their behalf.

These three modes differ in the extent of delegation and can be viewed as rungs on a ladder. Under DIRECT, investors retain both fund-selection and trading authority. Under AUTO, they delegate the timing and execution of scheduled purchases but continue to determine which funds to own and when to sell them. Under RA, they delegate both fund selection and the trades that implement the resulting portfolio. The central comparison in this paper is between AUTO and RA. Both automate purchase execution, but only RA transfers authority over fund selection and subsequent trading. Comparing them therefore helps distinguish investors' willingness to relinquish decision authority from their willingness to automate the mechanical execution of trades. The products may also differ in branding and framing, as discussed in Section 6.2. This comparison forms the basis of the within-investor-month design developed in that section.

Delegation in this market is layered. Every investor in our sample already delegates security selection within each mutual fund to a professional fund manager. Thus, exposure to professionally delegated asset management is common to all investors in the sample. The marginal decision rights divided among the three investment modes concern which funds to hold and how to trade them. Throughout the paper, we use fund selection as shorthand for this bundle: the choice of funds and the trades used to implement that choice, both of which RA assumes and AUTO preserves. Similarly, self-directed investing refers to investor-directed fund selection rather than stock picking, and taking up advice refers to transferring these marginal decision rights.

Because AUTO and RA operate on the same platform, they share a common account environment, liquidity conditions, and execution infrastructure. Their central economic difference is who selects

[6]The advisory fee is charged on delegated assets and does not depend on which funds the algorithm selects. Self-directed purchases and automatic investment plans incur no advisory fee, although fund-level management fees apply across all three modes.

the funds, although product-specific branding and framing may also matter. The comparison therefore sharpens, but does not fully isolate, the distinction between demand for automation and demand for delegation. The two products have comparable usage rates and substantial overlap. Across the 4.9 million investor-months, 13.95 percent record both RA and an automatic investment plan, 18.12 percent record RA only, 18.99 percent record an automatic investment plan only, and 48.94 percent record neither.

The study was conducted remotely through the Ant Open Research Laboratory.[7] The data were sampled and de-identified before being made available in the Ant Group research environment. The laboratory operates as a secure sandbox in which the authors could conduct empirical analyses remotely but could not inspect individual records.

### 4.2. Sample Construction and Estimand

The sample contains 100,000 investors observed monthly for 49 months, from September 2020 through September 2024, yielding 4.9 million investor-months. To enter the sample, an investor had to be registered on Alipay before the sample period, fall within the eligible age range at sampling, and have at least one consumption record, mutual fund holding, fund purchase, and fund redemption during the observation window. These activity requirements are substantive. The behavioral measures are constructed from investors' own fund trades, so investors without trades would have no measurable trading behavior. The paper also focuses on active investment choices.

The resulting sample is younger than typical Western brokerage samples. Mean age is 32.5, median age is 30, and investors range from 19 to 64 years old. Women constitute 42.5 percent of the sample. Mean on-platform total financial assets are CNY 95,245, with a median of CNY 30,220. These characteristics bound the interpretation of our findings: the sample represents active mutual fund investors on a mobile platform rather than the broader household population.

The sample is stratified by whether an investor ever adopts RA during the observation window. Of the 100,000 investors, 70,034 use RA at least once, whereas 29,966 hold mutual funds but never use RA. Conditional on ever adopting, investors hold RA for an average of 22.4 months. This oversampling of adopters is deliberate. In the underlying platform population, first adoption is a rare monthly event, and the intensive-margin outcome is observed only in months with positive RA assets. A uniform sample of the same size would therefore contain relatively few first-adoption and RA-active observations, limiting the precision of the lifecycle and conditional-share estimates.

The stratification determines what the sample can identify. It cannot identify the platform-wide prevalence of RA use because oversampling mechanically increases the proportion of adopters. It can identify how investor characteristics relate to the different margins of RA use within the sampled population of active mutual fund investors. These within-sample cross-sectional slopes are our estimands. This distinction is standard in choice-based sampling for rare outcomes (Manski & Lerman, 1977): without additional information, the sample does not identify the population intercept or unconditional prevalence, but the relationships within the sampled strata remain estimable.

We report unweighted estimates in the main text and weighted estimates as a robustness exercise. Population weights rescale the ever-adopter and never-adopter strata toward their platform shares,

[7] www.deor.org.cn/labstore/laboratory

which is appropriate when the target is a population quantity. Our primary estimands are instead defined within the sampled population: the relationships between investor characteristics and the margins of RA use among the 70,034 ever-adopters and 29,966 never-adopters we observe. We do not claim that the unweighted slopes recover platform-population slopes without further assumptions. The familiar case-control result for logit slopes does not apply directly because sampling is based on adoption at any point during the full window rather than on the monthly outcome, the selection equation is estimated by probit, and ever-adopters contribute pre-adoption, active, and post-exit observations.

The weighted estimates provide a comparison under an alternative estimand. Online Appendix Table A.1 shows that reweighting preserves the cross-margin sign patterns underlying the paper's behavioral results. The selection-correction coefficient is the only focal coefficient that does not remain robust to reweighting, and Section 8 discusses the interpretation of this sensitivity. We report the unweighted estimates as the main results because they estimate the same qualitative relationships with greater precision. Neither the weighted nor the unweighted estimates are used to infer unstratified platform penetration.

Two additional features of the empirical design follow from the stratification. First, sampling is based on ever adopting rather than on adoption in a particular month. Each ever-adopter therefore contributes their complete monthly history, including inactive months. The monthly Fama–MacBeth estimations use the cross-sectional variation within each month while preserving each investor's lifecycle path. Second, never-adopters enter the selection equation but not the outcome equation because the conditional RA share is undefined when RA assets equal zero. We therefore report extensive- and intensive-margin moments separately and use the conditional mean RA share when translating outcome-stage coefficients into economic magnitudes.

### 4.3. Outcomes and Covariates

Two broad margins organize the analysis: entry and commitment. Entry includes whether and when an investor first adopts RA. Commitment includes both the share of fund wealth delegated after adoption and the subsequent lifecycle of that delegation, including continuation, exit, and re-entry. The main extensive-margin outcome, denoted $d_{it}$, is an indicator equal to one when investor $i$ holds positive RA assets in month $t$. Conditional on $d_{it} = 1$, the main intensive-margin outcome is

$$s_{it} = \frac{\text{RA Value}_{it}}{\text{RA Value}_{it} + \text{Mutual Fund Value}_{it}}, \qquad d_{it} = 1. \tag{9}$$

The denominator combines the RA sleeve with the non-RA mutual fund sleeve, whose fund selection remains under the investor's control. The resulting share measures the proportion of fund wealth delegated to the algorithm. Across all investor-months for which the denominator is defined, its unconditional mean is 0.053. Among the 1,571,475 RA-active investor-months, its conditional mean is 0.151. We use the conditional mean when interpreting outcome-stage coefficients. Appendix specifications consider two alternative intensive- margin outcomes: log RA assets and RA assets divided by total financial assets.

Three lifecycle outcomes distinguish states that a single binary RA-use measure would combine. First-adoption events constitute 1.43 percent of all investor-months in the panel. This statistic is an

incidence per observed month rather than a transition rate because its denominator includes post-adoption months in which investors are no longer at risk of first adoption. The first-adoption hazard among not-yet-adopters is therefore higher; Section 5.3 reports the corresponding 3.2 percent rate. The monthly continuation rate, defined as holding RA conditional on having held it in the preceding month, is 97.35 percent across 1,532,289 at-risk months. The re-entry rate among lapsed adopters is 1.30 percent across 752,774 at-risk months.

These outcomes describe economically distinct decisions. First adoption is rare and reflects discovery and initial evaluation. Continuation concerns persistence in a product the investor already knows, while re-entry represents a return following an earlier exit or interruption. A single use indicator would combine these states, so the lifecycle analysis treats them separately.

The covariates include demographics, platform assets, risk preference, prior exposure to other robo-products, platform tenure, fund experience, selection ability, timing ability, platform engagement, and ten behavioral measures. The demographic and account controls include log total financial assets, age, gender, a survey-based six-level measure of risk preference, cash and money-market balances, and an indicator for using other robo-products on the platform. Cash and money-market balances comprise Yu'e Bao and similar cash-management holdings. We interpret them jointly as a proxy for liquidity preference, precautionary saving, and payment-account intensity rather than as risky wealth.

Fund experience is the cumulative number of months in which the investor has held at least one mutual fund; it averages 23.2 months in the panel. It measures accumulated experience rather than recent performance. This distinction is important for our later interpretation, which concerns familiarity with selecting and monitoring funds rather than realized returns.

Selection ability and timing ability decompose realized fund returns into the returns associated with fund choice and those associated with trading timing. Selection ability is the value-weighted excess return of the investor's funds relative to their Morningstar categories. It measures whether the investor holds funds that outperform their category peers. Timing ability is the profit or loss attributable to within-month trading, net of the passive return on beginning-of-month holdings. It measures whether the investor purchases and sells funds at advantageous times. Because these two dimensions can diverge, a single performance measure would conflate them. We therefore separate them following Grinblatt and Keloharju (2009).

The ten behavioral measures are central to the analysis and draw on distinct strands of behavioral finance. We first describe the behavior each measure is intended to capture and then explain its construction. Underdiversification is the tendency to hold too few or excessively similar assets, exposing the portfolio to risk that could be reduced without lowering expected returns. Total turnover measures how rapidly investors trade their portfolios. The revealed-measure literature commonly uses total turnover as a proxy for overconfidence (Barber & Odean, 2000, 2001), based on the premise that investors who overestimate the precision of their information trade more than is warranted.

Speculative turnover is a second trading-based proxy for overconfidence but is conceptually distinct from total turnover. It records round trips in which an investor sells a position after a gain and quickly repurchases the same or a similar fund. It therefore captures deliberate position-level speculation, whereas total turnover measures overall trading intensity. The two measures are constructed

separately because an investor can trade intensively without completing such a round trip, and both enter the empirical specifications simultaneously.

The remaining seven measures capture behaviors for which delegation has no direct analogue. Limited attention is the tendency to trade salient funds, such as those with extreme recent returns or recent launches, rather than funds identified through a broader search. The disposition effect is the tendency to sell winners too early and hold losers too long. Extrapolation captures the tendency to purchase funds following high recent returns. Gambling preference is a preference for lottery-like payoffs from low-priced funds offering a small probability of a large gain. Anchoring is the tendency to base trading decisions on salient reference prices, such as a fund's twelve-month high or low. Confirmation bias is the tendency to retain positions whose past performance validates the investor's earlier judgment. Herding is the tendency to purchase or sell funds that other investors are purchasing or selling. Each measure is designed to distinguish the relevant behavioral pattern from its rational counterpart.

Online Appendix A.4 provides the detailed construction of each measure. Underdiversification is measured using portfolio HHI, the sum of squared portfolio weights, which has a panel mean of 0.423. The effective number of funds is computed as $1/\text{HHI}_{it}$ for each investor-month and then averaged; its mean is 4.9. These measures connect to evidence that retail investors hold concentrated portfolios and may fail to recognize the cost of underdiversification (Blume & Friend, 1975; Goetzmann & Kumar, 2008; Huberman, 2001). Total turnover, the most common revealed proxy for overconfidence, is motivated by evidence that high-turnover retail investors underperform on average (Barber & Odean, 2000, 2001). The limited-attention measure captures trading in salient funds, following Barber and Odean (2008). The disposition-effect measure follows the PGR-minus-PLR logic of Odean (1998). Extrapolation captures lagged return chasing (Barberis et al., 2015; Greenwood & Shleifer, 2014); gambling preference captures trading in lottery-like funds (Kumar, 2009); anchoring captures trading near prior price extremes; confirmation bias captures the retention of lagged winner positions; and herding captures trading in funds with high lagged aggregate flows, following Lakonishok et al. (1992).

Three construction principles help distinguish the behavioral measures from their rational counterparts. First, the measures are lagged when used to model month-$t$ decisions. Second, directional components are separated where possible. Third, aggregate-flow variables exclude the focal investor's own flows. These choices allow Section 6.1 to distinguish correctable active-opinion biases, attachment biases, and cognitive-constraint biases. A single omnibus behavioral index would not support this classification.

Continuous regressors are winsorized at the first and ninety-ninth percentiles and standardized within each month. Winsorization limits the influence of extreme platform records, while within-month standardization expresses each coefficient in units of a within-month standard deviation and makes monthly estimates comparable before Fama–MacBeth averaging. Two conventions follow. First, a one-standard-deviation change in a regressor refers to one within-month standard deviation, which may differ from the pooled standard deviation reported in Table 1 because the latter also incorporates variation across months. Second, outcomes remain in their natural units unless the table notes state otherwise. A coefficient on the delegated share therefore represents a change in that share,

whereas the dose-response outcomes in Section 7.2 are standardized within month and interpreted in standard deviations of the outcome.

Binary variables are not standardized. All right-hand-side variables except age, gender, and risk preference are lagged by one month. After an investor's first observed trade, behavioral variables are carried forward through non-trading months on the premise that the underlying tendency persists until new trading evidence becomes available. Returns are never carried forward.

### 4.4. Heckman Specification

The baseline design estimates a Heckman two-step model separately in each monthly cross-section. The first stage is a probit model of RA participation:

$$\Pr(d_{it} = 1 \mid Z_{it-1}) = \Phi\big(Z'_{it-1}\gamma\big)\,, \tag{10}$$

where $Z_{it-1}$ contains $X_{it-1}$ and the excluded platform-tenure variable. The second stage estimates the RA share among RA-active investor-months:

$$s_{it} = X'_{it-1}\beta + \delta\widehat{\lambda}_{it} + \varepsilon_{it}, \qquad d_{it} = 1, \tag{11}$$

where $\widehat{\lambda}_{it}$ is the inverse Mills ratio constructed from the first-stage probit. The inverse Mills ratio summarizes the selection implied by the first stage among observations for which the delegated share is observed.

We estimate these relationships separately within each calendar month and average the monthly coefficients following Fama and MacBeth (1973). We compute Newey–West standard errors with three lags, allowing the monthly coefficient estimates to be serially correlated within a quarter (Newey & West, 1987). These standard errors do not incorporate uncertainty from estimating the first-stage probit because the second stage treats $\widehat{\lambda}_{it}$ as an observed regressor. Given the large monthly cross-sections, the first-stage index is estimated precisely, but a Murphy–Topel adjustment or bootstrap would provide the formal generated- regressor correction. Province indicators enter both stages to absorb regional differences in financial development, platform penetration, and product diffusion. The baseline, skills, and behavioral-measure specifications retain 48, 47, and 36 monthly cross-sections, respectively. The behavioral panel is shorter because constructing its measures requires rolling windows that are unavailable in the earliest months.

The Heckman correction reflects the economic structure of the two-margin problem rather than serving only as a statistical adjustment. The delegated share is observed only after an investor has adopted RA, so the two outcomes may depend on common unobserved factors. For example, latent trust in algorithmic management or comfort with relinquishing control may increase both the probability of participation and the share delegated conditional on participation. If so, the RA-active observations are selected on an unobserved dimension that also affects the outcome. An OLS regression estimated only among RA-active observations may then confound the relationships between observed characteristics and the delegated share with this selection.

Under the standard Heckman normalization, the coefficient on the inverse Mills ratio satisfies

$$\delta = \rho\sigma_\varepsilon,$$

where $\rho$ is the correlation between the unobserved components of the selection and outcome equations and $\sigma_\varepsilon$ is the standard deviation of the outcome disturbance. The sign of $\delta$ therefore indicates whether unobserved factors associated with selection into RA are also associated with higher or lower delegated shares. This interpretation requires a variable that shifts participation but is excluded from the share equation, an economic account of the common unobserved factor, and evidence that the selection correction is not merely proxying for omitted observables (Heckman, 1979; Li & Prabhala, 2007; Vella, 1998). Section 5.2 examines these conditions in turn.

The two-step estimator does not constrain the implied correlation

$$\widehat{\rho} = \frac{\widehat{\delta}}{\widehat{\sigma}_\varepsilon}$$

to lie within the admissible interval $[-1, 1]$. In the behavioral-measure specification, the average implied $\widehat{\rho}$ is 1.381. This value is incompatible with a literal bivariate-normal correlation and therefore limits the structural interpretation of the selection correction in that specification. We report $\widehat{\delta}$, the estimated coefficient on the inverse Mills ratio, rather than treating $\widehat{\rho}$ as a valid correlation parameter. We interpret the signs and relative magnitudes of the observable slopes but do not interpret the selection coefficient as a calibrated correlation.

Estimating the model month by month rather than as a pooled panel is substantive. Several covariates change slowly or not at all, including age, gender, baseline wealth, risk preference, and accumulated experience. Investor fixed effects would absorb much of the cross-sectional variation on which the main question depends. Monthly cross-sectional estimation preserves this variation, permits the coefficients to vary across market environments, and produces a time-series average of the cross-sectional relationships over the 49-month window. This approach cannot remove time-invariant investor heterogeneity. As an alternative, Online Appendix Table A.6 reports a panel fixed-effects control-function specification. The three focal intensive-margin behavioral signs remain unchanged, while the demographic gradients are absorbed by construction. Anchoring and gambling preference, two smaller behavioral coefficients, change sign under this estimator. Because neither coefficient supports a central argument in the paper, we interpret them as less precisely determined rather than as stable findings.

Platform tenure is the excluded variable, and its proposed exclusion operates through product discovery. Longer platform tenure provides more opportunities to encounter and evaluate RA. The relevance condition is strong: months since registration has estimated marginal effects of 0.011, 0.008, and 0.014 in the baseline, skills, and behavioral panels, with $t$-statistics of 10.54, 7.80, and 10.98, respectively. Two features reduce, but do not eliminate, concerns about the exclusion restriction. Province indicators absorb regional differences in platform penetration that might otherwise make tenure a proxy for local product diffusion. In addition, the characteristics central to the paper are constructed from pre-adoption trading histories rather than from investors' tenure on the platform.

The principal concern is that platform tenure may proxy for attachment to the provider. Provider

tenure can influence intensive-margin attachment through switching costs (Klemperer, 1995); under this interpretation, platform tenure belongs in the outcome equation and the exclusion restriction fails. Online Appendix Table A.2 considers specifications that include tenure in both equations and exclude it from both. Section 8 explains what these alternatives do and do not establish. As in other Heckman designs, the validity of the exclusion restriction is not directly testable. It is the principal identifying assumption behind the selection correction, which motivates the analysis in Section 4.5 of which findings do not depend on it.

Selection-stage coefficients in the non-interacted Heckman tables are reported as approximate marginal effects evaluated at the RA-active rate in the corresponding panel. Online Appendix A.2 provides the scaling. Outcome-stage coefficients are reported in raw RA-share units. Thus, a coefficient of 0.020 represents a two-percentage-point increase in the conditional RA share, equivalent to approximately 13 percent of its 0.151 conditional mean.

### 4.5. What the Two-Margin Result Does Not Rest On

The exclusion restriction is the principal untestable assumption behind the selection correction. We therefore distinguish the descriptive allocation relationships observed among adopters from the selection-adjusted outcome coefficients. Several components of the paper's central pattern do not depend on the exclusion restriction.

A direct benchmark removes the selection correction altogether. Online Appendix Table A.3 re-estimates the delegated-share equation by OLS on exactly the same RA-active investor-month samples used in the corresponding Heckman outcome equations. The regressions omit both the inverse Mills ratio and platform tenure. The three focal behavioral signs remain: the coefficients on underdiversification and total turnover are $+0.045$ and $+0.057$, respectively, while the coefficient on confirmation bias is $-0.034$. Thus, the principal conditional-share signs are visible in the observed allocations of adopters and are not introduced by the selection correction. These regressions do not correct for nonrandom adoption and therefore do not validate the exclusion restriction or replace the selection-adjusted estimates.

The adoption result does not rely on excluding platform tenure from the share equation. In the selection equation, tenure enters as an ordinary regressor, and removing it has almost no effect on the focal coefficients. Across the specifications in Online Appendix Table A.2, the concentration coefficient is $-0.172$ with tenure included and $-0.170$ without it; the corresponding turnover coefficients are $-0.083$ and $-0.084$, and the confirmation-bias coefficients are $+0.080$ and $+0.081$. The finding that investors exhibiting the most correctable behavioral patterns are less likely to adopt is therefore insensitive to the placement of tenure.

The authority design reproduces the intensive-margin sign pattern without using the exclusion restriction. The design in Section 6.2 stacks DIRECT, AUTO, and RA observations within each investor-month and identifies the automation and delegation contrasts from within-investor-month variation. It has no selection stage or excluded variable. The estimates produce the same split: concentration is positively associated with the automation step but negatively associated with the additional delegation step.

Two other specifications retain the exclusion restriction but use different sources of variation. The

first-adoption design in Section 5.3 measures entry using pre-adoption histories and recovers the negative adoption and positive delegated-share associations at the investor's first delegation decision. The pattern therefore does not depend on the stock of active users. The panel control-function specification in Online Appendix Table A.6 replaces the Fama–MacBeth approach with investor fixed effects and two-way clustered standard errors. It produces conditional-share coefficients of $+0.045$ for concentration, $+0.040$ for turnover, and $-0.015$ for confirmation bias. The focal pattern therefore does not depend on cross-sectional averaging.

The positive intensive-margin coefficients are not specific to the denominator of the delegated-share measure. Because that denominator includes non-RA mutual fund assets, it may be correlated with portfolio concentration and turnover. Two alternative outcomes address this concern. Dividing RA assets by total financial assets produces coefficients of $+0.017$ for concentration, $+0.017$ for turnover, and $-0.006$ for confirmation bias. Using log RA assets, which contains no share denominator, produces coefficients of $+0.204$, $+0.069$, and $-0.048$, respectively. These estimates are reported in Online Appendix Table A.5.

The exclusion restriction remains important for the magnitude of the selection-correction coefficient and, consequently, for quantitative claims about how much of the delegated share is associated with unobserved selection. Section 8 reports this sensitivity and discusses its interpretation.

## 5. Who Uses Robo-Advising?

Throughout this section, we interpret each magnitude relative to the relevant base rate. Extensive-margin coefficients are percentage-point changes in the probability of RA use, evaluated against the participation mean reported in the corresponding table. This mean ranges from 32.6 percent in the baseline panel to 37.2 percent in the biases panel and equals 32.3 percent in the return specifications of Section 6.4. Intensive-margin coefficients are percentage-point changes in the conditional RA share, evaluated against the mean delegated share of 0.151 among RA-active investor-months. This distinction matters because a given coefficient has a different economic magnitude relative to a participation rate of 32 to 37 percent than relative to a conditional delegated share of 15.1 percent. Because the sample is stratified on ever adopting RA, these benchmarks describe the estimation sample rather than platform-wide prevalence.

### 5.1. Baseline Heckman Estimates

Table 2 reports three nested Heckman specifications. The baseline panel includes demographic and account characteristics. The skills panel adds fund experience, selection ability, and timing ability. The biases panel adds the ten behavioral measures, with total and speculative turnover entering jointly. The three panels retain 48, 47, and 36 monthly cross-sections, respectively.

Among the demographic characteristics, wealth operates primarily through the entry margin. A one-standard-deviation increase in log total financial assets is associated with increases in RA participation of 14.2 percentage points in the baseline panel, 13.4 percentage points in the skills panel, and 7.9 percentage points in the biases panel. Relative to the corresponding RA participation rates, these estimates amount to approximately 44, 39, and 21 percent of the respective means. The intensive-

margin pattern is different. The conditional-share coefficient is negative in the baseline and skills panels, at $-0.079$ and $-0.049$, or approximately 52 and 32 percent of the 0.151 conditional mean. Once the behavioral controls are included, however, the coefficient falls to $-0.010$, approximately 7 percent of the conditional mean, and is no longer statistically significant. Wealth is therefore strongly associated with entry, whereas its baseline relationship with the delegated share is largely absorbed by behavioral composition. This pattern helps reconcile conflicting findings in the prior literature about whether investors with larger portfolios substitute away from robo-advising. Investors with larger portfolios are more likely to enter, but their conditional commitment is not significantly lower once behavior is held fixed.

Risk preference, by contrast, is positively associated with both margins. A one-standard-deviation increase corresponds to a 1.9- to 2.9-percentage-point increase in participation, equivalent to 5 to 9 percent of the corresponding participation mean, and a 0.8- to 2.0-percentage-point increase in the conditional share, equivalent to 5 to 13 percent of its mean. Risk preference therefore provides a useful benchmark: the two-stage specification does not mechanically generate opposite signs across the two margins.

Female investors are less likely to use RA and delegate less conditional on doing so. Across the three panels, the corresponding differences are four to five percentage points in participation and 1.3 to 2.0 percentage points in the conditional share, approximately 12 to 14 percent and 9 to 13 percent of the two means. Prior use of the platform's other robo products is the strongest observed predictor of RA participation, with a coefficient of approximately 22 percentage points, equivalent to 59 to 69 percent of the participation mean. Its conditional-share coefficient rises to 6.7 percentage points in the biases panel. This variable appears to capture persistent heterogeneity in investors' willingness to use delegated products that is not exhausted by wealth, risk preference, or the behavioral measures.

Age illustrates why distinguishing the two margins matters when interpreting demographic evidence. In the baseline specification, age has little association with participation and a small negative conditional-share coefficient of $-0.005$. After the behavioral controls are included, age becomes positively associated with participation, at 1.6 percentage points per within-month standard deviation, while its conditional-share coefficient becomes economically small and statistically insignificant. This change is consistent with age combining several offsetting behavioral channels. Online Appendix Table A.18 shows that older investors trade less, hold less concentrated portfolios as measured by HHI, and exhibit stronger confirmation bias. Conditional on these behaviors, age is associated mainly with the extensive margin. Although modest relative to the wealth and prior-robo coefficients, the post-bias coefficient amounts to approximately 4 percent of the participation mean. The result may help explain why studies based on binary adoption outcomes reach different conclusions about age: the estimated relationship depends partly on which correlated behaviors the researcher observes.

Fund experience generates the principal cross-margin pattern among the skill variables. In the skills panel, a one-standard-deviation increase in fund experience is associated with a 3.5-percentage-point increase in RA participation and a 2.8-percentage-point decrease in the conditional RA share. These estimates amount to approximately 10 percent of the skills-panel participation mean and 19 percent of the conditional-share mean. The positive participation coefficient is consistent with experience reducing the cost of evaluating a fund-of-funds robo product. The negative conditional-share

coefficient admits several interpretations. Experience may increase the value an investor places on retaining fund-selection authority, it may proxy for behavioral characteristics that account for the relationship, or the pooled coefficient may reflect the composition of the continuing-user pool rather than initial commitment. We do not distinguish among these accounts at this stage. Section 5.4 sets out the alternatives, and Sections 6.2 and 6.3 bring separate evidence to bear on them. In the joint specification reported in Online Appendix Table A.4, the participation coefficient remains positive at 2.4 percentage points, whereas the conditional-share coefficient falls to $-0.005$ and becomes statistically insignificant. Fund experience therefore retains an independent extensive-margin association, while part of its intensive-margin relationship appears to operate through correlated behavioral characteristics. Table 3 provides an additional lifecycle qualification: pre-adoption fund experience is negatively associated with first RA adoption. Experience can therefore deter the initial move into delegation even though experienced investors are more prevalent in the pool of active users.

The behavioral variables generate the paper's central taxonomy. Underdiversification and total turnover exhibit the sign pattern that motivates our selective-awareness account. A one-standard-deviation increase in underdiversification is associated with a 6.5-percentage-point reduction in RA participation and a 2.0-percentage-point increase in the conditional RA share. The entry association amounts to approximately 17 percent of the biases-panel participation mean, and the share association to approximately 13 percent of the conditional mean. The interpretation rests on the pair of signs rather than on either coefficient in isolation. Negative coefficients on both margins would indicate a general reluctance to use the product, whereas positive coefficients on both would indicate greater adoption accompanied by greater commitment. A positive participation coefficient combined with a negative share coefficient would instead resemble the attachment pattern documented below for confirmation bias. The observed negative-positive pattern is the one that motivates the selective-awareness interpretation, although Section 5.4 considers alternative explanations. Investors with more concentrated portfolios are less likely to adopt, but those who adopt delegate a larger share, and their concentration subsequently declines with tenure. Together, these findings are consistent with exposure to a diversified algorithmic portfolio making the costs of concentration more salient.

Total turnover exhibits the same negative-positive sign pattern, although the relative magnitudes differ. A one-standard-deviation increase in total turnover is associated with a 3.0-percentage-point reduction in participation, approximately 8 percent of the participation mean, and a 4.5-percentage-point increase in the conditional share, approximately 30 percent of its mean. Thus, compared with underdiversification, the turnover pattern is weighted more heavily toward commitment than toward entry. One possible interpretation is that high-turnover investors are already active market participants and are therefore less deterred from trying the product, while delegation more directly substitutes for the discretionary trading captured by total turnover. Both sign pairs remain stable across the checks that retain the exclusion restriction, including the joint specification, population reweighting, and a panel control-function estimator with investor fixed effects. Section 8 summarizes these checks, and the Online Appendix reports the corresponding tables. The pattern is consistent with demand-side selection into a corrective technology: concentrated and high-turnover investors exhibit behaviors that diversified, rule-based RA is designed to discipline (Barber & Odean, 2000, 2001; Goetzmann & Kumar, 2008), yet these investors may be less likely to recognize the associated costs before adopting.

Speculative turnover differs from total turnover on the intensive margin. This distinction warrants attention because the two measures are often treated as interchangeable proxies for overconfidence, whereas here they have opposite relationships with the delegated share. Holding total turnover fixed, a one-standard-deviation increase in speculative turnover is associated with a 0.2-percentage-point reduction in participation ($t = -3.37$) and a 1.9-percentage-point reduction in the conditional share ($t = -8.35$). The latter amounts to approximately 13 percent of the conditional mean and contrasts with the positive 4.5-percentage-point coefficient on total turnover.

One interpretation of this distinction concerns whether RA can substitute for the underlying behavior. RA trades infrequently because it holds diversified portfolios and rebalances according to a rule. Delegating a larger share can therefore displace much of the investor's discretionary trading. Total turnover captures broad trading activity, including the costly trading documented by Barber and Odean (2000), and the positive share coefficient is consistent with investors transferring more wealth to a product that limits such activity. Total turnover also declines with RA tenure, as shown in Section 6.1.

Speculative turnover represents a more directed behavior: selling a recent winner and repurchasing it shortly afterward. Such trading is consistent with confidence in the precision of one's own signals (Gervais & Odean, 2001; Statman et al., 2006) and is expressed through position-level decisions rather than trading volume alone. A rule-based product does not accommodate this type of active view. Investors who wish to retain the ability to act on such views may therefore keep more wealth outside RA, producing a lower delegated share. Table 4, introduced in Section 6.2, provides complementary evidence: speculative turnover is negatively associated with both the automatic-plan and robo-advising steps, at $-0.031$ and $-0.013$, respectively. This pattern is consistent with these investors being reluctant to transfer the relevant decisions to either product. The distinction remains under population reweighting and the other robustness checks reported in the Online Appendix.

The contrast within turnover illustrates a broader distinction among the behavioral measures. Delegation can substitute for activities that the algorithm itself performs, but it need not change the beliefs, preferences, or constraints that investors bring to the product. Trading activity and portfolio construction belong more naturally to the first category, which helps explain why underdiversification and total turnover are positively associated with the delegated share and why both may decline following exposure. Position-level conviction belongs to the second category, as do limited attention, a preference for lottery-like payoffs, and herding. These characteristics are negatively associated with both margins, consistent with delegation changing neither what investors attend to nor what they prefer to hold. Attachment to previously validated positions lies between the two: such investors may be willing to try RA alongside their existing positions but unwilling to transfer a large share. Section 6.1 develops these patterns using post-adoption trajectories. The turnover results show that the distinction between behavior that RA can displace and behavior that it cannot may arise even within a single broad behavioral category.

Confirmation bias exhibits the opposite cross-margin pattern. A one-standard-deviation increase is associated with a 3.0-percentage-point increase in participation and a 2.2-percentage-point reduction in the conditional RA share, approximately 8 percent of the participation mean and 15 percent of the conditional-share mean. The sign pair survives the joint specification reported in the Online

Appendix. This positive-negative pattern is consistent with an attachment interpretation and is the reverse of the pattern observed for underdiversification and total turnover. An investor whose previous choices have recently been validated can add an RA sleeve without unwinding those positions, making entry relatively easy. Allocating a large share to RA, however, may require reducing the positions to which the investor is attached, thereby limiting commitment. The opposing patterns also show why aggregating the behavioral measures into a single index could obscure their relationships with demand. The disposition-effect literature documents attachment to self-selected positions (Odean, 1998; Shefrin & Statman, 1985), including among mutual fund investors (Bailey et al., 2011). Delegation may change this relationship: the disposition effect observed for self-selected assets reverses for delegated assets, for which responsibility can be attributed to the manager (Chang et al., 2016). Our interpretation extends this logic from the realization of losses to the allocation of capital.

Attention, gambling, and herding biases are negatively associated with both margins. This pattern is consistent with the framework in Section 3, in which a characteristic increases the effective cost of entry without increasing the perceived benefit of delegation. The magnitudes are smaller than those of the principal active-opinion measures, ranging from 0.6 to 0.9 percentage points on entry and from 0.3 to 0.5 percentage points on the conditional share. Nevertheless, the common direction across the two margins is informative. These characteristics may reflect constraints or preferences that remain with investors after delegation rather than behaviors that exposure to RA directly addresses. The contrast between this same-signed group and the opposite-signed selective-awareness group provides a discriminating comparison: an account in which all behavioral biases relate uniformly to RA demand would not predict these different sign patterns. The results are consistent with cognitive constraints or preference-like behaviors that RA does not necessarily relax (Barber & Odean, 2008; Kumar, 2009; Lakonishok et al., 1992). The remaining measures, anchoring, extrapolation, and the disposition effect, do not fit either pattern, and our interpretation does not rely on them. Taken together, the results show that behavioral characteristics have heterogeneous relationships with RA demand, separating into active-opinion behaviors that RA can potentially correct, attachment-related behaviors, and constraints or preferences that delegation does not directly alter.

These baseline findings speak to the conflicting literature on RA adoption. Studies that characterize adopters as younger, male, or holding smaller portfolios estimate a combination of entry and commitment in which demographic and wealth variables may absorb unobserved behavioral composition. Studies finding weaker demographic relationships may examine settings in which these behavioral channels are observed or less important. The results here show how these findings can coexist within a single platform. Wealth is strongly associated with entry but not with conditional delegation once behavior is controlled. Age is associated primarily with entry conditional on behavior and has little relationship with commitment. Behavioral characteristics themselves have heterogeneous signs across the two margins. The reconciliation is therefore not that one set of prior findings is necessarily incorrect, but that a binary adoption outcome can be too coarse for a delegated product.

### 5.2. Selection on Unobservables

The selection-correction coefficient is positive in all main specifications. The coefficient on the inverse Mills ratio is 0.158 in the baseline panel, 0.203 in the skills panel, and 0.212 in the biases panel;

the joint specification produces a coefficient of 0.277. These coefficients are large relative to a share outcome whose conditional mean is 0.151, although they should not be interpreted one-for-one as percentage-point effects because the inverse Mills ratio is a generated nonlinear index. Their persistence across specifications nevertheless indicates that residual selection is economically relevant relative to the observed relationships reported in the same table.

Under the maintained selection model and exclusion restriction, the positive coefficient indicates that the unobserved determinants of adoption and the unobserved determinants of the delegated share are positively related. Importantly, it does not imply that a threshold-level "marginal adopter" delegates more than the average adopter. Economically, the sign is less consistent with an omitted disposition that encourages adoption while reducing commitment, as a cautious-experimenter account might imply. It is instead consistent with an unobserved characteristic that increases both investors' propensity to adopt and their willingness to delegate conditional on adoption. Comfort with delegated algorithmic management is one possible candidate.

The coefficient's stability across specifications provides additional information. Adding fund experience, selection ability, timing ability, and the behavioral measures does not reduce the selection-correction coefficient; it instead rises from 0.158 to 0.212. This stability suggests that the included skills and behavioral measures do not account for the residual relationship between entry and commitment. In particular, the residual channel is not readily explained by the measured components of underdiversification, turnover, confirmation bias, or investment skill.

Interpreting this residual requires restraint. The coefficient on the inverse Mills ratio identifies a relationship between the unobserved components of the two equations, not a particular investor characteristic. Giving it economic content requires an excluded variable that shifts entry, an explicit account of the potential unobservable, and evidence that the proposed channel is not merely an omitted observable (Li & Prabhala, 2007; Vella, 1998). The stability across the expanded specifications bears on the third consideration. Two additional results in the Online Appendix further constrain the interpretation.

First, the evidence weighs against a simple wealth or scale interpretation. The selection-correction coefficient is positive for the share outcomes but negative for the level outcome, at $-1.411$ for the log of RA assets compared with $+0.212$ for the delegated share (Online Appendix Table A.5). Although coefficients across differently scaled outcomes are not directly comparable, their opposing signs are difficult to reconcile with an omitted characteristic that simply makes adopters hold more assets. They are more consistent with an unobserved disposition related to the fraction of fund wealth an investor is willing to place under algorithmic management.

The design does not identify that disposition directly. The trust literature provides one possible interpretation. Trust predicts financial-market participation beyond wealth, education, and measured sophistication (Guiso et al., 2008), and delegated management may provide reassurance in addition to investment performance (Gennaioli et al., 2015). Trust has also been proposed as a determinant of robo-advice demand (Belanche et al., 2019; Scherer & Lehner, 2023). In this setting, long-standing familiarity with Alipay as a financial interface may make an investor both more willing to try RA and more willing to allocate a larger share to it. We therefore view trust or comfort with delegated management as an interpretation of the residual, not as a measured or identified mechanism.

Selective awareness could generate a similar residual relationship. If investors who become sufficiently aware of their portfolio problems are both more likely to adopt and more willing to delegate, an unmeasured component of awareness could produce a positive selection-correction coefficient. Trust and selective awareness are nevertheless distinct. Selective awareness concerns an investor's beliefs about her own portfolio and may therefore change with exposure to the algorithmic portfolio. Consistent with this implication, Section 6.1 shows that the relevant behaviors decline following adoption. Trust instead concerns the product or the institution providing it. The measured components of selective awareness, underdiversification and turnover, are already included in the specification and do not absorb the residual relationship. The design cannot, however, determine how much of the remaining association reflects unmeasured awareness, trust, platform attachment, or a broader affinity for delegated services.

This residual relationship complements the observed cross-margin results. The measured characteristics can have different associations with adoption and the delegated share, while observably similar investors also differ along an unobserved dimension that is positively related to both outcomes. A binary adoption specification would combine these channels in a single reduced-form use measure. The two-stage design makes the observed cross-margin relationships and the residual selection relationship separately visible.

### 5.3. First Adoption

The pooled estimates cannot by themselves rule out a survivorship interpretation. If concentrated investors who remain in the RA-active pool are also those who delegate heavily, the positive share coefficient could reflect the composition of continuing users rather than initial commitment. Addressing this possibility requires examining the flow into RA rather than only the stock of active users. The pooled Heckman specification measures whether an investor holds RA in a given month. The resulting stock includes first adopters, continuing users, re-entrants, and long-tenure users. A characteristic that is positively associated with being observed among active users could nevertheless be negatively associated with crossing the adoption threshold for the first time. Table 3 therefore examines first adoption of RA and AUTO using a pre-window construction in which the regressors summarize the investor's pre-adoption history rather than her contemporaneous active-user state.

Distinguishing the stock of active users from the flow of first adopters clarifies apparent sign differences. Pooled participation identifies who is observed holding RA in a given month, whereas first adoption identifies who crosses the entry threshold among investors who have not previously adopted. Differences between the pooled and first-adoption coefficients therefore need not be contradictory; they can indicate that a characteristic operates differently across stages of the product lifecycle.

Wealth is positively associated with first adoption of both products and negatively associated with the initial conditional share. For RA, the first-stage marginal effect of pre-window log total financial assets is 0.029, and the initial-share coefficient is $-0.041$. The corresponding AUTO coefficients are 0.042 and $-0.080$. These entry associations are economically meaningful relative to first-adoption rates of 7.7 percent for RA and 8.4 percent for AUTO. The wealth coefficient amounts to approximately 38 percent of the RA first-adoption rate and 50 percent of the AUTO first-adoption rate. The 7.7 percent RA rate exceeds the 1.43 percent of panel investor-months in which a first adoption occurs

(Table 1) because the two rates use different denominators. The pre-window design collapses each never-adopter's history into a single at-risk observation, so its denominator consists of pre-adoption and never-adopter records rather than all investor-months. The initial-share coefficients are also economically meaningful: the RA coefficient amounts to approximately 27 percent of the conditional RA-share mean, and the AUTO coefficient is larger relative to the AUTO purchase-share outcome. The pooled Heckman estimates exhibit the same broad pattern. Wealth is associated with a greater probability of entry, but larger accounts do not allocate a larger initial share conditional on entering.

Risk preference is associated with increases in first adoption of 0.7 percentage points for RA and 0.8 percentage points for AUTO, but it has no statistically reliable relationship with the initial share. Female investors display a smaller cross-margin pattern. They are less likely to adopt RA for the first time but allocate more conditional on first adoption. The female coefficients are $-0.007$ in the RA first stage and $+0.026$ in the RA outcome stage, implying a modest entry difference and an initial-share difference equal to approximately 17 percent of the conditional RA-share mean. Thus, the lower probability of first adoption does not translate into a smaller initial allocation among women who adopt. This result also differs from the pooled conditional-share coefficients for women, which range from $-0.013$ to $-0.020$. Anchoring and extrapolation likewise change signs between the pooled and first-adoption designs. We report these differences without assigning them a mechanism; only the concentration and confirmation patterns are supported by the subsequent mechanism tests.

Pre-window fund experience is negatively associated with first adoption of both products. The coefficient is $-0.190$ for RA and $-0.137$ for AUTO, and both are highly statistically significant. These are the largest first-stage magnitudes in the table relative to the corresponding first-adoption rates. Fund experience therefore generates the clearest distinction between the stock and flow estimates: it is positively associated with pooled RA participation but negatively associated with first adoption. A lifecycle interpretation is that experienced investors face a stronger self-directed outside option and therefore cross the initial delegation threshold more slowly. Those who eventually adopt may nevertheless be disproportionately represented among continuing users, generating a positive association with the observed stock of RA-active investor-months. Section 6.3 examines this lifecycle interpretation directly.

Measured selection ability points in the same direction at entry, reducing first RA adoption by 1.3 percentage points. This result is also consistent with a more valuable self-directed outside option.

The selection-correction coefficient also differs between the pooled and first-adoption specifications. In the first-adoption models, the coefficient on the inverse Mills ratio is $-0.081$ for RA and $-0.090$ for AUTO, compared with the positive coefficients in the pooled Heckman specifications. Neither first-adoption estimate is statistically significant, with $t$-statistics of $-0.989$ and $-0.827$, respectively, so the evidence does not establish negative selection at entry. Taken only as suggestive point estimates, the negative signs would indicate that the unobserved determinants of first adoption and initial commitment are negatively related. Their contrast with the positive pooled coefficients would be consistent with investors initially entering cautiously and subsequently deepening delegation. Because the first-adoption coefficients are statistically indistinguishable from zero, we do not rely on this interpretation. We note only that the estimates do not contradict the dynamic-delegation evidence in Section 6.3.

The first-adoption estimates also address alternative interpretations of the behavioral results. For underdiversification, the positive conditional-share coefficient in the pooled model could reflect post-adoption survivorship if concentrated investors who remain active delegate relatively more. The first-adoption estimates are not subject to this particular post-entry composition mechanism. Underdiversification is negatively associated with first RA adoption and positively associated with the initial RA share, with coefficients of $-0.005$ and $+0.045$, respectively. The initial-share coefficient amounts to approximately 30 percent of the conditional RA-share mean and is measured at the investor's first delegation decision. The negative-positive sign pattern is therefore present at entry rather than arising solely among continuing users. Confirmation bias exhibits the reverse pattern at the same stage, with coefficients of $+0.030$ for first adoption and $-0.040$ for the initial share. These magnitudes amount to approximately 39 percent of the RA first-adoption rate and 26 percent of the conditional RA-share mean.

Turnover matches the pooled pattern only partly. It is negatively associated with first RA adoption, although only at the 10 percent significance level, and has no statistically significant relationship with the initial RA share. The entry association is therefore present at first adoption, whereas the positive share association emerges only in the pooled active-user sample. This difference points to timing rather than necessarily contradicting the pooled result. The concentration pattern is already present at the first delegation decision, while the positive relationship between turnover and commitment appears among active users and is accompanied by the tenure-related decline documented in Section 6.1. The findings are therefore consistent with underdiversification becoming salient at or before entry and the turnover pattern emerging more gradually after adoption. Among the remaining measures, attention and herding are negatively associated with first adoption, consistent with the cognitive-constraint interpretation. Anchoring is negatively associated with both first adoption and the initial RA share in the pre-window design, reinforcing our decision not to impose a common mechanism on all behavioral measures. The strongest entry patterns occur among the same variables that matter in the pooled Heckman: underdiversification and confirmation generate opposite cross-margin signs, while attention-related constraints deter entry.

The comparison with first AUTO adoption addresses whether investors who remain outside RA are avoiding algorithmic execution or the surrender of fund-selection authority. Several predictors have the same first-stage sign for RA and AUTO, consistent with shared discovery or platform-access channels. Wealth, risk preference, fund experience, anchoring, turnover, attention, underdiversification, confirmation, and herding all have similar first-stage signs across the two products. The outcome-stage coefficients reveal sharper differences. Turnover reduces initial AUTO intensity by 4.4 percentage points but has no statistically reliable relationship with initial RA intensity. Confirmation bias is positively associated with first AUTO adoption and has a positive but statistically insignificant AUTO-share coefficient, whereas it is negatively associated with the initial RA share. Female investors are less likely to start RA but are not less likely to start an automatic investment plan. Execution automation and full delegation therefore differ at the point of entry as well as among continuing users.

### 5.4. Competing Accounts of the Baseline Pattern

The baseline results establish a pattern that requires explanation. Investors with greater levels of the fund-management behaviors that RA is designed to address are less likely to adopt, yet those who do adopt delegate a larger share of their fund wealth. The same characteristic is negatively associated with entry and positively associated with commitment. The pattern is also selective: underdiversification and total turnover exhibit the negative-positive pattern, confirmation bias exhibits the reverse pattern, and attention, gambling, and herding are negatively associated with both margins. The positive selection-correction coefficient does not eliminate this pattern; under the maintained selection model, it indicates an additional positive relationship between the unobserved determinants of adoption and commitment.

Two broad classes of explanations can generate these findings. The first treats the positive share coefficient as reflecting something other than a substantive commitment decision. We consider five such explanations using the evidence already presented.

The first is post-adoption survivorship. Concentrated investors who remain in the RA-active sample may delegate more, causing the pooled coefficient to reflect the composition of continuing users. The first-adoption estimates address this explanation directly: concentration is negatively associated with first RA adoption and positively associated with the initial delegated share by 4.5 percentage points, approximately 30 percent of the conditional mean (Section 5.3). Because the share is measured at the first delegation decision, this estimate cannot be generated solely by post-entry survivorship.

The second explanation is mechanical. Because the delegated share divides RA assets by the sum of RA and self-directed mutual fund assets, concentrated investors could appear to have a high delegated share simply because they hold few assets in the denominator. The relationship remains when the denominator is changed to total financial assets, with a concentration coefficient of $+0.017$; when the denominator is removed and the outcome is the log level of RA assets, with a coefficient of $+0.204$; and when low-risk RA strategies and money market funds are excluded from both the numerator and denominator, with a coefficient of $+0.052$ (Section 4.5).

The third explanation is product risk. RA may be a relatively conservative product, in which case investors with stronger risk preferences could remain outside it and those with weaker risk preferences could delegate more, while concentration merely proxies for risk appetite. Risk preference is observed and included directly in the specification. In the biases panel, it is positively associated with both participation and the conditional share, by 1.9 and 1.2 percentage points, respectively. This same-signed pattern differs from the negative-positive relationship observed for concentration.

The fourth explanation is a monetary barrier to entry, with concentration proxying for the resources needed to overcome it. Wealth is measured directly and behaves differently from concentration. In the biases panel, a one-standard-deviation increase in log total financial assets is associated with a 7.9-percentage-point increase in participation and a statistically insignificant conditional-share coefficient of $-0.010$. Moreover, the product has no minimum investment requirement. These facts weigh against a minimum-investment constraint as the source of the concentration pattern, although they do not eliminate other entry frictions such as discovery, attention, or evaluation costs.

The fifth explanation is that the behavioral variables proxy for one another, so the intensive-margin coefficients reflect correlated characteristics rather than the named measures. The joint specification

in Online Appendix Table A.4 includes demographics, skills, and all ten behavioral measures simultaneously. The negative-positive sign pairs for underdiversification and turnover, and the positive-negative pair for confirmation bias, remain.

The second class of explanations treats the cross-margin pattern as economically meaningful. We consider three mechanisms. The first is selective awareness. Investors with the greatest potential need for RA may not recognize the relevant problems before adoption, while exposure to a diversified, rule-based portfolio makes those problems more salient. The second is decision authority. Investors may avoid RA because they value retaining the choice of which funds to hold, rather than because they oppose automation itself. Characteristics that predict willingness to automate execution may therefore differ from those that predict willingness to surrender fund-selection authority. The third is reclaiming control. Delegation may be partial or transitional, particularly for experienced investors who subsequently rebuild their self-directed positions. A fourth possibility competes with all three: recent returns may be the omitted state variable underlying the observed relationships.

Each account implies evidence not contained in the baseline estimates, and Section 6 examines them in turn. Selective awareness implies that the behaviors made salient by RA should decline following adoption, with larger declines among investors displaying the highest pre-adoption levels and steeper declines among investors who engage more frequently with the platform. Behaviors that RA does not make salient should not exhibit the same engagement-related pattern. A general preference for automation implies similar demand for AUTO and RA, whereas the within-investor-month product ladder can separate automation from the additional transfer of fund-selection authority. A static view of delegation implies no systematic relationship between RA tenure and the delegated share. Finally, a performance-based explanation implies that controlling for recent returns should absorb the characteristic gradients. These tests allow the subsequent analysis to distinguish among interpretations of the baseline cross-margin pattern.

## 6. Three Mechanisms

The baseline establishes that adoption and the delegated share have different determinants in this sample, but it does not explain why. We organize the additional evidence around three mechanisms that describe different stages of delegated decision-making. Selective awareness concerns what investors may learn about their own portfolio management once RA provides a benchmark. Decision authority concerns whether investors respond by transferring fund selection to the algorithm rather than merely automating execution. Reclaiming control concerns whether that transfer persists as experience with the product accumulates. The mechanisms are distinct but need not be mutually exclusive: use may change investors' perceived need for delegation, while preferences for authority determine how much they delegate and whether that delegation persists. Each mechanism requires evidence that the baseline cross-section cannot provide. Recent performance is an alternative explanation that could potentially account for all three patterns, and the section concludes by examining it.

### 6.1. Selective Awareness

Selective awareness has an ex ante and an ex post component. Before adoption, demand depends on whether investors recognize the costs of their own portfolio choices. Investors with more severe concentration or trading problems may therefore enter less frequently if they do not perceive those behaviors as costly. After adoption, RA places a diversified, rule-based portfolio alongside the investor's self-directed portfolio and thereby provides a benchmark for evaluating the latter. Exposure to this benchmark may update beliefs about the quality of self-management. Delegation is therefore not only a response to an ability difference known before adoption; using the product may itself change the investor's perceived need for delegation.

We do not observe investors' beliefs about their own ability directly. Accordingly, we evaluate selective awareness through its behavioral implications. If RA makes concentration and frequent trading more salient, these behaviors should decline after adoption, decline more among investors with higher pre-adoption levels, and decline more rapidly among investors who engage more frequently with the platform. Behavioral characteristics for which RA provides no comparably visible corrective benchmark should not exhibit the same engagement-related pattern.

We evaluate this prediction by tracking the behavioral measures over RA tenure. Because monthly bias measures are noisy, the primary outcomes $y_{it}$ are rolling six-month trailing means of underdiversification, turnover, and the comparison biases. The specifications include investor and month fixed effects, with standard errors clustered by investor and month. Constructing the behavioral measures requires sufficiently long fund-trading histories, so this analysis uses the subsample for which these measures are available: 1,321,291 adopter-months in the within-adopter panel and 1,847,939 investor-months after matched never-adopters are included, compared with 4.9 million investor-months in the full panel. The matched difference-in-differences specification assigns pseudo-adoption dates to never-adopters so that treated and control investors are aligned in calendar time. The never-adopter comparison enters through the interaction

$$y_{it} = \rho\,(\text{adopter}_i \cdot \tau_{it}) + \text{controls}_{it} + \mu_i + \lambda_t + \varepsilon_{it}, \tag{12}$$

where $\tau_{it}$ is months since first RA use, capped at 24, and $\mu_i$ and $\lambda_t$ denote investor and month fixed effects. The adopter indicator and the calendar-time component common to treated and control investors are absorbed by the fixed effects, leaving the interaction as the identified coefficient. The design is descriptive: it asks whether the behaviors underlying the selective-awareness interpretation decline following exposure to RA.

The negative-positive sign pattern for underdiversification and turnover in the baseline cross-section is consistent with selective awareness, but a static specification cannot reveal the post-adoption dynamics on which that interpretation depends. The event-time analysis supplies this missing evidence.

Selective awareness is one of the paper's three mechanisms, but the behavioral evidence within this mechanism produces three distinct signatures. The first is the selective-awareness signature. Underdiversification and turnover are behaviors that RA can plausibly discipline, yet investors exhibiting them may not perceive their costs before adoption. Such investors enter less frequently, while those

who cross the entry threshold delegate more and subsequently reduce these behaviors. This interpretation has a rational benchmark. Investors who optimally acquire information about assets they already hold may rationally choose concentrated portfolios, in which case the perceived cost of concentration need not become salient (Van Nieuwerburgh & Veldkamp, 2010). Investors may also misperceive their own past performance (Glaser & Weber, 2007).[8] The second signature is attachment. Confirmation bias may facilitate trial when RA is added as a separate sleeve but limit commitment when investors remain attached to their validated positions. The third is cognitive constraint. Attention, gambling, and herding biases need not be relaxed by RA and may therefore reduce both adoption and commitment. The differences among these signatures distinguish the behavioral evidence from an account in which all biases relate uniformly to RA demand.

Table 5 examines the post-adoption predictions. Panel A uses rolling six-month trailing means, and Panel B uses raw monthly behavioral measures. The table distinguishes the two focal active-opinion measures, underdiversification and turnover, from comparison measures that the framework does not predict will exhibit the same engagement-related declines. This classification concerns post-adoption trajectories rather than baseline cross-margin signs. Confirmation bias, for example, belongs to the comparison set because the attachment interpretation does not predict that engagement with RA will reduce it, even though confirmation bias has a distinct positive-negative sign pattern in the baseline Heckman model. The cognitive-constraint measures, attention, gambling, and herding, are reported in Online Appendix Table A.12 and are likewise not expected to exhibit the same engagement-related declines.

The results go beyond a negative average tenure slope. In the smoothed specification, underdiversification and turnover decline with RA tenure at monthly rates of $-0.0013$ and $-0.0029$, respectively. These declines are substantially larger among investors with high pre-adoption levels of the corresponding behavior. Over a twelve-month tenure difference, the high-underdiversification interaction implies an additional decline of approximately 0.13 in the concentration measure, while the high-turnover interaction implies an additional decline of approximately 0.19 in turnover. An indicator for high pre-adoption underdiversification or turnover also predicts larger declines in both focal measures. Relative to pooled standard deviations of 0.318 for concentration and 0.390 for turnover, the additional declines amount to approximately 0.4 and 0.5 standard deviations for the high-bias groups.

For turnover, the tenure pattern also complements the first-adoption evidence. Turnover is negatively associated with first adoption but does not significantly predict the initial delegated share (Section 5.3). Its positive association with the delegated share appears among active users, and turnover then declines with tenure. Thus, the relationship emerges more gradually than that for underdiversification, whose negative-positive sign pattern is already present at the first delegation decision.

The matched difference-in-differences estimates yield a similar pattern. The adopter-by-tenure coefficient is negative for both underdiversification and turnover, at $-0.0008$ and $-0.0018$, respectively, and the interactions with high pre-adoption bias and the active-opinion indicator remain negative and statistically significant. These estimates show that the focal behaviors decline more strongly among

[8] The broader pattern in which those with weaker performance are less aware of their deficits is documented in psychology (Kruger & Dunning, 1999).

adopters than along the corresponding pseudo-tenure trajectories of matched never-adopters.

Platform engagement provides the most discriminating evidence for the selective-awareness interpretation. A negative tenure slope alone could reflect mean reversion, portfolio maturation, or other changes correlated with tenure. These alternatives do not, by themselves, predict that the decline should be steeper among investors who view the platform more frequently. Selective awareness does make this prediction because the proposed mechanism operates through exposure to the algorithmic benchmark. Login frequency is also consistent with prior evidence that portfolio attention is selective (Sicherman et al., 2016). In the smoothed panel, the adopter-by-tenure-by-engagement interaction is negative and significant for both focal measures: $-0.0002$ ($t = -3.07$) for underdiversification and $-0.0006$ ($t = -6.68$) for turnover. In the raw monthly panel, the corresponding coefficients are $-0.0003$ ($t = -4.90$) and $-0.0007$ ($t = -8.33$).

The comparison measures do not exhibit the same engagement pattern. The engagement interactions for anchoring and extrapolation are statistically indistinguishable from zero in both panels. Confirmation bias is an exception within the comparison set, with a small positive coefficient of $+0.0003$ in the smoothed panel. The distinction is therefore comparative rather than absolute: the focal active-opinion behaviors exhibit negative and more consistent engagement-related trajectories, whereas the comparison measures are either flat or move in the opposite direction. Online Appendix Table A.12 reinforces this contrast. Attention, the disposition effect, gambling, and herding all have positive engagement interactions, so the negative engagement gradient appears for the focal pair and not for the other behavioral measures.

The comparison measures are not uniformly flat with respect to tenure itself, and our interpretation does not require them to be. Anchoring increases with tenure, at $+0.0009$ within adopters and $+0.0015$ in the matched specification. Extrapolation increases in the matched specification, while confirmation bias declines, at $-0.0005$ in the smoothed panel and $-0.0022$ in the raw panel. Their average tenure trajectories therefore do not share a common direction. For this reason, the discriminating comparison rests primarily on the engagement interaction rather than on the average tenure coefficient. Mean reversion, portfolio maturation, and the mechanical transfer of wealth into a sleeve that the investor does not trade can generate average tenure trends, but they do not readily explain why the focal behaviors decline more among investors who engage more frequently.

One qualification concerns smoothing. In the raw monthly panel, the simple tenure coefficient and the adopter-by-tenure coefficient for underdiversification are no longer statistically significant. The average decline in underdiversification is therefore clearer in the smoothed series than in the raw monthly observations. The heterogeneity and engagement interactions, which contain the main mechanism evidence, remain present for both focal measures when the smoothing window is removed. Figure 2 displays this contrast by plotting the negative engagement interactions for underdiversification and turnover against the flatter or oppositely signed responses of the comparison measures.

These trajectories connect the demand analysis to the literature on the consequences of robo-advising. Prior research finds that robo-advising can improve diversification and reduce investment mistakes conditional on use (Bianchi & Brière, 2026; Hao et al., 2022; Reher & Sokolinski, 2024; Rossi & Utkus, 2024). Our results show that the same behavioral characteristics are related to both voluntary demand and post-adoption behavior. Investors with more severe correctable behaviors are

not the most likely to enter; in the baseline Heckman specification, they are less likely to adopt. Conditional on adoption, however, their subsequent trajectories are consistent with learning through exposure to diversified, rule-based management.

This evidence shifts attention from whether RA can improve investor behavior conditional on use to which investors expose themselves to that correction. Any aggregate benefit depends on whether investors exhibiting the relevant behaviors adopt voluntarily and how much wealth they delegate. The negative adoption coefficients for underdiversification and turnover identify the demand-side bottleneck. Their positive conditional-share coefficients and post-adoption declines identify the margin on which behavioral improvement appears among those who cross that bottleneck. The combined evidence is consistent with selective awareness: investors exhibiting behaviors that RA can address enter less frequently, while adopters delegate more and subsequently reduce those behaviors.

The mechanism remains interpretive. The post-adoption declines are consistent with learning from exposure, but they do not establish that investors consciously recognize the costs of concentration or turnover. Mean reversion, concurrent attention shocks, and changes in portfolio needs may also contribute to the observed trajectories. Login frequency is neither randomly assigned nor specific to the robo-advisor and may capture sophistication or general attentiveness. The evidence is therefore comparative. The focal behaviors decline more among investors with higher pre-adoption levels and exhibit a more consistently negative engagement gradient than the comparison measures. This pattern is less consistent with a uniform mechanical post-adoption trend and supports the dynamic implication of selective awareness.

### 6.2. Decision Authority versus Automation

Decision authority is the right to choose which funds to hold and to implement those choices through trading. RA transfers both fund selection and trading to the algorithm. AUTO automates the timing of purchases but leaves fund selection, selling, and rebalancing with the investor. Comparing the two products therefore helps separate a preference for automated execution from a willingness to delegate fund-selection authority. The products share the platform, account interface, and execution infrastructure, making this comparison possible.

The authority ladder compares DIRECT, AUTO, and RA within each investor-month. It asks whether investors who avoid RA resist automation itself or the transfer of fund-selection authority. The design stacks the three product rows within each investor-month and encodes the ladder using two cumulative indicators: $c_T = 1$ for AUTO and RA, and $c_S = 1$ for RA only. For product $k \in \{\text{DIRECT}, \text{AUTO}, \text{RA}\}$ in investor-month $it$, the estimating equation is

$$\text{use}_{i,k,t} = \alpha_{it} + \gamma_T \left(X_i \cdot c_k^T\right) + \gamma_S \left(X_i \cdot c_k^S\right) + (\text{baseline} \times c_k^T, c_k^S) + \varepsilon_{i,k,t}, \tag{13}$$

where $c_k^T = \mathbf{1}\{k \in \{\text{AUTO}, \text{RA}\}\}$, $c_k^S = \mathbf{1}\{k = \text{RA}\}$, and $\alpha_{it}$ denotes investor-month fixed effects. These fixed effects absorb wealth, market conditions, and other investor-month characteristics common to the three product rows. The coefficient on $X \times c_T$ captures the differential associated with automated execution relative to direct investing. The coefficient on $X \times c_S$ captures the additional differential associated with moving from AUTO to RA, which transfers fund-selection authority. A

Wald test of $\gamma_T = \gamma_S$ evaluates whether a characteristic is associated similarly with the two increments. These coefficients are within-investor-month product-use differentials rather than sequential transition hazards.

Section 5.4 leaves open whether the negative relationship between experience and the conditional RA share reflects the cost of surrendering fund selection. A simpler alternative is that RA users have a general preference for financial automation. Under that explanation, the same characteristics should be associated with AUTO and RA in similar directions, and the additional transfer of fund-selection authority should not generate systematic sign reversals. The baseline cannot distinguish these explanations because RA combines automated execution with delegated fund selection.

Table 4 separates these components using 672,228 stacked observations. Because the comparison is made within investor-month, it describes how the same investor uses the three products in the same month rather than how her behavior changes over time. The steps should therefore be interpreted as contrasts along an authority ladder, not as sequential transitions. Separate AUTO estimates already show that the two products have different determinants. Underdiversification increases AUTO intensity by 8.8 percentage points compared with 2.0 percentage points for RA, while turnover is negatively associated with both AUTO margins but positively associated with the conditional RA share (Online Appendix Table A.7).

The central finding is that the characteristics do not move the two increments in parallel. For several variables, the two coefficients have opposite signs. Pre-use platform engagement is negatively associated with the automation increment by 4.2 percentage points and positively associated with the additional full-delegation increment by 1.6 percentage points. Equality of the two coefficients is strongly rejected ($\chi^2 = 29.38$, $p < 0.001$). Total turnover produces the reverse split, with coefficients of $+3.5$ percentage points for the automation increment and $-3.2$ percentage points for the full-delegation increment ($\chi^2 = 31.80$, $p < 0.001$). Underdiversification produces the largest split, at $+5.8$ and $-3.3$ percentage points ($\chi^2 = 69.60$). The corresponding coefficients are $-3.8$ and $+4.8$ percentage points for anchoring ($\chi^2 = 57.79$) and $-4.2$ and $+2.6$ percentage points for attention bias ($\chi^2 = 29.63$).

Figure 1 plots these coefficients, their confidence intervals, and the tests of equality. The magnitudes are economically meaningful relative to the frequencies with which the products are held: AUTO appears in 32.9 percent of investor-months and RA in 32.1 percent (Section 4.1). For example, the underdiversification coefficients of $+5.8$ and $-3.3$ percentage points each amount to roughly one-tenth of the corresponding product-use rate. These opposite-signed differentials are difficult to reconcile with a single, monotonic preference for automation. A general preference for automation could make both AUTO and RA more attractive, but it does not naturally predict that a characteristic should be positively associated with the move toward AUTO and negatively associated with the additional move toward RA, or vice versa.

For the variables with opposite signs, the direction of the split is consistent with an authority-cost interpretation. Total turnover and portfolio concentration measure the intensity of investors' active views about portfolio management. Both are positively associated with the automation increment and negatively associated with the full-delegation increment. The composite authority-bias index produces the same negative full-delegation coefficient, at $-0.023$ ($t = -3.90$), while its automation coefficient

is close to zero. Equality is rejected at the 5 percent level ($\chi^2 = 4.10$). Investors with stronger active views may value scheduled execution while retaining the right to select funds, whereas full delegation transfers precisely that decision.

Engagement, attention bias, and anchoring exhibit the reverse split. For these investors, automating purchase timing alone may not address the relevant constraint, whereas delegating fund selection removes more of the underlying decision burden. These interpretations remain suggestive because the characteristics are not randomly assigned and may capture other differences across investors. Nevertheless, their opposing relationships with the two increments show that execution automation and full delegation are empirically distinct.

The authority interpretation does not describe every behavioral measure. Speculative turnover is negatively associated with both increments, at $-0.031$ and $-0.013$, and retains both signs at each horizon reported in the Online Appendix. These investors are therefore less likely to use either form of automation. Confirmation, gambling, herding, and the disposition effect are associated with only one increment or with neither. We therefore interpret the authority pattern as applying to the opposite-signed coefficients rather than as a general law governing all characteristics. Five variables, total turnover, portfolio concentration, anchoring, attention, and engagement, have individually significant coefficients of opposite sign, with equality rejected at $p < 0.001$ in every case. This pattern is inconsistent with explaining all product-use differences through a single preference for automation.

Measured skill provides a more qualified result. The skill composite is unrelated to the automation increment ($\gamma_T = 0.004$, $t = 0.75$) and positively but imprecisely related to the full-delegation increment ($\gamma_S = 0.012$, $t = 1.79$). The Wald test does not reject equality ($\chi^2 = 0.54$). Selection ability produces a somewhat sharper split, with coefficients of $-0.006$ for automation and $+0.014$ for full delegation ($\chi^2 = 3.65$, $p = 0.056$). At longer pre-use windows, the full-delegation coefficient for the skill composite reaches $+0.016$ and is significant at the 5 percent level, while the automation coefficient becomes negative (Online Appendix Table A.10). These results suggest that investors with greater measured selection ability are not especially attracted to scheduled execution and may be more willing to use full delegation. The evidence is not conclusive, however. Equality between the two increments is never rejected at the 5 percent level for the skill composite, and timing ability exhibits the reverse tendency, loading weakly on automation ($+0.009$, $t = 1.83$) but not on full delegation. We therefore draw the narrower conclusion that the authority-cost pattern is more closely related to the intensity of investors' active views than to measured ability itself.

The composite indices were defined before estimating these relationships. The skill index averages selection and timing ability. The authority-bias index averages confirmation, both turnover measures, herding, and the disposition effect. The placebo index averages underdiversification, gambling, anchoring, attention, and extrapolation. The placebo label reflects the variables on which the authority mechanism was not initially expected to load; it does not imply that every component is individually unrelated to the two increments. Indeed, underdiversification and attention have large coefficients of opposite sign. Once aggregated, the placebo index is statistically indistinguishable from zero at both increments, with coefficients of $-0.007$ and $+0.003$, and the cross-increment difference is not significant ($\chi^2 = 0.74$). The aggregation therefore illustrates why a composite measure can conceal heterogeneous relationships that remain visible in the disaggregated estimates.

One apparent tension arises between the baseline Heckman model and the authority ladder. Underdiversification is positively associated with the conditional RA share by 2.0 percentage points in the pooled Heckman specification but is positively associated with the automation increment and negatively associated with the additional full-delegation increment in the ladder. The two designs measure different objects. The pooled Heckman model asks how much fund wealth an RA adopter delegates. The ladder asks how a characteristic is associated with the relative use of DIRECT, AUTO, and RA within an investor-month. The AUTO Heckman estimates help reconcile these results: underdiversification increases AUTO intensity by 8.8 percentage points but RA intensity by 2.0 percentage points. Concentrated investors can therefore delegate a meaningful share to RA conditional on adopting while still displaying a stronger relative preference for the product that preserves fund-selection authority.

The estimated patterns describe two broad types of investors. Investors with strong views about which funds to hold may value automated execution because it reduces the effort required to implement their choices while preserving their authority over fund selection. Investors constrained by attention may instead derive relatively little benefit from scheduled execution alone but value a product that also assumes responsibility for portfolio allocation. The results therefore distinguish between automating a task and delegating a decision, two features that conventional measures of fintech adoption often combine.

The authority ladder does not establish that fund-selection authority is the only difference between AUTO and RA. The products share a platform, account interface, liquidity environment, and execution infrastructure, and the investor-month fixed effects absorb factors common to the three product rows. Nevertheless, AUTO and RA may also differ in branding, perceived sophistication, default framing, or formal advisory status. Fund-selection authority is the economically relevant distinction that the design is intended to isolate, but these other product attributes remain bundled with it. We therefore interpret the within-investor-month differences as evidence consistent with an authority channel rather than as causal estimates of the value of decision rights alone.

### 6.3. Reclaiming Control Over Tenure

Reclaiming control refers to a reduction in the authority previously transferred to RA. An adopter who lowers her delegated share as tenure accumulates reallocates part of the fund-selection decision back toward self-direction. This mechanism is the dynamic counterpart of the authority cost and treats delegation as a continuous choice rather than an absorbing state. Whereas the authority ladder describes how an investor uses three products within a given month, this subsection introduces the time dimension by examining how the delegated share varies with RA tenure.

If experience increases the value of retaining fund-selection authority, adoption need not mark the end of the decision. Experienced investors who adopt may subsequently reduce their delegated shares as they return part of the portfolio to self-direction. A pooled cross-section cannot reveal this relationship because it combines investors at different tenures. The relevant test therefore follows the delegated share over RA tenure and asks whether its tenure gradient is more negative among investors with greater pre-adoption fund experience.

The specification estimates the RA share among RA-active investor-months as a function of pre-

RA fund experience and its interaction with RA tenure:

$$\text{rsh}_{it} = \beta \, \text{exp}_i + \delta_1 \left(\text{exp}_i \cdot \widetilde{\tau}_{it}\right) + \delta_2 \left(\text{exp}_i \cdot \widetilde{\tau}_{it}^2\right) + \phi_1 \widetilde{\tau}_{it} + \phi_2 \widetilde{\tau}_{it}^2 + \text{controls}_{it} + \varepsilon_{it}. \tag{14}$$

Pre-RA experience, $\text{exp}_i$, is measured over the six months preceding first RA use and standardized across eligible adopters. This window provides sufficient history to measure prior experience while limiting overlap with behavior close to adoption. Online Appendix Table A.10 varies the analogous pre-use window in the authority analysis. Tenure, $\widetilde{\tau}_{it}$, is the cumulative number of RA-active months, demeaned within calendar month. The coefficient $\delta_1$ indicates whether the tenure-share gradient differs with pre-adoption fund experience. Equation (14) is estimated using monthly Fama–MacBeth regressions with Newey–West standard errors. A fractional-logit specification provides a functional-form check that accounts for the bounded dependent variable. Because eligible adopters require six months of pre-RA history, investors already using RA at the beginning of the sample window are excluded. The resulting analysis includes 1,097,096 of the 1,571,475 RA-active investor-months across 44 monthly cross-sections (Table 8).

The estimates are consistent with reclaiming control, subject to an important interpretive qualification. The Fama–MacBeth estimates describe within-calendar-month cross-sectional tenure gradients among active adopters, not within-investor changes in the delegated share. Longer-tenure active adopter-months are associated with lower RA shares in both the OLS and fractional-logit specifications. The tenure coefficient is $-0.028$ in the linear OLS specification and $-0.048$ in the quadratic OLS specification. The fractional-logit coefficient is $-0.330$ on the logit scale. Pre-RA fund experience is also negatively associated with the delegated share, with a coefficient of approximately $-0.196$ in the OLS specifications.

The focal interaction between pre-RA experience and tenure is negative: $-0.023$ in the linear OLS specification, $-0.043$ in the quadratic OLS specification, and $-0.304$ in the fractional-logit specification. The quadratic interaction terms are negative but statistically insignificant, so the differential tenure gradient is adequately summarized by the linear interaction. Online Appendix Table A.11 reports similar signs and magnitudes under alternative adopter-sample restrictions.

Figure 3 translates the linear interaction into the estimated tenure gradient at different levels of pre-RA fund experience. The gradient is close to zero, at $-0.005$, for adopters one standard deviation below mean experience. It becomes progressively more negative as experience increases, reaching $-0.051$ one standard deviation above the mean. The estimated gradient crosses zero at approximately 1.2 standard deviations below mean experience. Thus, except among the least-experienced adopters, longer tenure is associated with a lower delegated share, and this negative association is stronger among investors with more pre-adoption fund experience.

The magnitudes are economically meaningful relative to the conditional RA-share mean of 0.151 in the main Heckman specification. Even after tenure is demeaned within calendar month, differences in relative tenure and pre-adoption experience correspond to a substantial fraction of the average delegated share. The interaction is therefore economically relevant rather than only directionally informative. Its statistical precision is more limited. The interaction is significant at the 10 percent level in the linear OLS specification and at the 5 percent level in the quadratic OLS and fractional-logit specifications. It is significant at either the 5 or 10 percent level across the three Online Appendix variants. The

negative interaction is consequently stable in sign but not estimated with uniformly high precision. By contrast, the tenure gradient at and above mean experience is statistically significant at conventional levels.

These results complement the authority ladder. The ladder describes differences in the transfer of decision authority at a point in time, while the tenure analysis shows that longer-tenure investors with greater pre-adoption fund experience hold smaller delegated shares. The evidence is inconsistent with treating delegation as an absorbing state or assuming that commitment deepens monotonically after adoption. Instead, RA may be a partial or transitional form of delegation, particularly for experienced investors who retain an active self-directed sleeve.

The event-study evidence in Section 7.1 provides a related view from the self-directed side. Portfolio concentration falls sharply at adoption and subsequently moves back toward its pre-adoption level. Online Appendix Figure A.2 shows that the within-adopter concentration gap moves from $-0.027$ in month five to $+0.023$ by month 36, while the RA share returns to its adoption-month level. These patterns suggest a reallocation back toward self-direction over longer tenures, although they do not imply that the delegated share declines monotonically in every month.

Tenure also helps interpret the relationship between fund experience and RA use. Fund experience may reduce discovery and evaluation frictions, but it can also increase the value investors place on retaining fund-selection authority. Learning-by-doing models provide one possible foundation: experience may improve investors' ability to manage funds and increase what they perceive themselves to forgo when delegating (Arrow, 1962; Ben-Porath, 1967). The results do not establish that experience causally improves skill, but they are consistent with experienced investors continuing to value authority over their self-directed portfolios.

Combined with the behavioral trajectories in Section 6.1, the tenure evidence suggests a broader interpretation of corrective advice. The behaviors that RA makes salient decline most among investors with high initial levels and frequent platform engagement. At the same time, investors with greater pre-adoption experience hold smaller delegated shares at longer tenures and rebuild their self-directed portfolios. This pattern is consistent with learning through trading (Linnainmaa, 2011; Nicolosi et al., 2009; Seru et al., 2010), but the design does not directly identify learning as the channel.

The evidence also qualifies adjacent accounts of delegation. In rational lifecycle models, delegation may absorb the increasing time costs of self-management and deepen over time (Kim et al., 2016), while retirement-plan participants rarely adjust their allocations directly (Agnew et al., 2003; Sialm et al., 2015). In this open-platform setting, voluntary delegation instead becomes weaker with relative tenure among investors with greater self-directed experience. The direction also contrasts with concerns that reliance on automation erodes users' skills (Parasuraman & Riley, 1997). We do not observe skill formation directly, but the reallocation toward self-direction suggests that product success need not be measured solely by assets retained. Some investors may use RA partially or transitionally rather than as a permanent replacement for self-management.

### 6.4. Alternative Explanations and Concerns

Recent performance is the leading alternative to an interpretation based on stable investor characteristics. If investors chase recent returns or withdraw from underperforming sleeves, the demographic,

skill, and behavioral coefficients may proxy for performance feedback. This subsection therefore treats performance as an alternative explanation rather than as a fourth mechanism. The question is whether controlling for recent returns accounts for the cross-margin characteristic patterns. The results show that returns are related to demand but do not absorb those patterns.

The analysis includes return measures in the RA-share equation and in the first-adoption and life-cycle specifications. The Online Appendix reports specifications containing single level measures of fund and RA performance. The main return table decomposes the returns into positive and negative components, $\beta^{+}r_{it}^{+} + \beta^{-}r_{it}^{-}$, and separately examines indicators for above-median returns. The lifecycle table distinguishes first adoption, exit, and continuation. When entered as single level measures, fund and RA returns have economically small coefficients in the RA-share equation, no larger than 0.003 across nine specifications (Online Appendix Tables A.8 and A.9). This result does not imply that returns are irrelevant; rather, it suggests that a single linear return measure conceals asymmetric or magnitude-dependent relationships.

Table 7 reveals the pattern obscured by the single-measure specifications. Once fund returns are separated into positive and negative components and included in both stages, both components are negatively associated with RA adoption. In the raw-return decomposition, a one-standard-deviation increase in the positive-return component corresponds to a 2.6- to 3.1-percentage-point reduction in adoption, while the corresponding increase in the magnitude of the negative-return component corresponds to a 2.4- to 2.7-percentage-point reduction. Each estimate is significant at the 1 percent level and amounts to approximately 7 to 10 percent of the participation mean. Both large gains and large losses in the investor's self-directed portfolio are therefore associated with a lower probability of RA adoption. The above-median indicator is statistically insignificant in the selection equation at every horizon, reinforcing the conclusion that the magnitude of recent performance is more informative for entry than a simple good-versus-bad return distinction.

Among adopters, both components are positively associated with the delegated share. The coefficient on fund return$^{+}$ ranges from 0.006 to 0.009 across the three horizons, while the coefficient on the magnitude of fund return$^{-}$ ranges from 0.006 to 0.008. These estimates amount to approximately 4 to 6 percent of the 0.151 conditional-share mean per within-month standard deviation and are similar in magnitude. The sign symmetry is inconsistent with a simple account in which only gains produce greater comfort with delegation or only losses induce investors to move toward the algorithm. It is instead consistent with the magnitude or salience of recent self-directed outcomes being associated with greater conditional delegation.

The two components of RA returns exhibit a less symmetric pattern. RA return$^{+}$ is positively associated with the delegated share at all three horizons, with significance at the 10 percent level after one month and at the 1 percent level after three and six months. The coefficients range from 0.006 to 0.009, similar to those on the fund-return components. RA return$^{-}$ is also positive throughout but becomes statistically significant only at the six-month horizon. In the gain region, recent performance in both the self-directed and delegated sleeves is therefore positively associated with commitment. In the loss region, the relationship is clearer for the investor's self-directed portfolio.

The excess-return decomposition sharpens this distinction. The self-directed fund-return components remain positive and statistically significant, with coefficients ranging from 0.008 to 0.012,

whereas the robo excess-return components are small or negative and the above-median indicators are weak. These results weigh against a simple relative-performance contest between the two sleeves and instead point to the salience of outcomes in the investor's own self-directed portfolio.

Recent performance is therefore related to both entry and conditional delegation, but its role depends more on the magnitude of returns than on a simple distinction between gains and losses. The associations are also modest relative to those of wealth, experience, and the focal behavioral measures. They amount to several percentage points for participation and at most approximately one percentage point for the conditional share. Including the return measures leaves the principal cross-margin characteristic coefficients largely unchanged. The coefficient on the inverse Mills ratio also remains positive, between 0.091 and 0.144, and statistically significant across these specifications. We interpret this only as evidence that adding observed returns does not absorb the estimated selection correction, not as identification of a particular latent characteristic. Recent performance therefore contributes to variation in RA demand but does not explain the full pattern separating entry from conditional commitment.

These findings do not contradict the experimental evidence on algorithm aversion. The experiments in Dietvorst et al. (2015) and Dietvorst et al. (2018) isolate responses to observable algorithmic errors in settings where performance signals are clear and can be attributed directly to the algorithm. The monthly sleeve-level returns in our field setting combine market exposure, fund composition, rebalancing, investor cash flows, and product mix. A weak or sign-symmetric response to these returns therefore does not reject algorithm aversion. It instead indicates that monthly realized returns are too noisy and bundled to account for the stable cross-margin structure of RA demand in this sample.

## 7. Consequences for the Self-Directed Portfolio

The mechanism tests provide evidence on why entry and commitment have different determinants. This section asks whether the two margins are also associated differently with the portfolio that investors continue to manage themselves. Two designs address this question. The event-time analysis describes changes around adoption by tracing coefficients $\pi_j$ on relative-time indicators $\mathbf{1}\{\tau_{it} = j\}$, measured relative to the month before adoption. The dose-response analysis relates lagged RA-share bins to outcomes in the self-directed sleeve. Together, these designs examine whether entry and commitment leave empirically distinct patterns in the self-directed portfolio, as the three mechanisms imply.

One caveat applies throughout the section. Because adoption and the delegated share are chosen by investors, the estimates describe demand-related portfolio patterns rather than causal treatment effects. Adoption is also staggered, and event-study coefficients under staggered adoption need not recover a common treatment path (Roth et al., 2023). We therefore interpret the event-time and dose-response estimates descriptively. The relevant comparison is whether entry and the delegated share are associated differently with the self-directed portfolio, not whether either has an identified causal effect on that portfolio.

### 7.1. Event-Time Evidence Around Adoption

The event-time figures describe the extensive margin by showing how the self-directed portfolio changes around RA adoption, irrespective of the amount delegated. Around adoption spells, self-directed portfolio concentration falls sharply. The spell-level event study in Online Appendix Figure A.1 shows that the Herfindahl index falls by approximately 0.038 at the beginning of an adoption spell and reaches a trough 0.057 below the reference level four months later. The delegated share rises by 17.6 percentage points at adoption and subsequently declines, reaching 8.9 percentage points above the reference level by month 12. Turnover increases by 0.024 in the adoption month, consistent with portfolio reallocation at entry, and then falls below its pre-adoption reference level, reaching approximately $-0.12$ by month 12. The longer-horizon tenure analysis shows that part of the initial decline in concentration subsequently unwinds.

Figure 4 displays this timing more directly. In the first-adoption series, HHI equals 0.403 in the month before adoption. It falls to 0.365 in the adoption month and reaches approximately 0.342 four to five months later, a decline of roughly 15 percent relative to the pre-adoption level. This change is economically meaningful in light of the diversification losses documented for concentrated household portfolios by Goetzmann and Kumar (2008) and Calvet et al. (2007). The decline is not permanent, however. HHI rises to 0.358 by month 12, 0.381 by month 18, and 0.403 by month 24, at which point it is indistinguishable from the pre-adoption reference. The any-adoption series, which includes re-entry spells, exhibits a similar pattern over its shorter horizon. The event-time evidence therefore describes a substantial diversification movement around entry that dissipates over approximately two years, rather than a permanent transformation of the self-directed sleeve. This gradual reversion is consistent with evidence that households adjust portfolio weights only partially and with long delays (Calvet et al., 2009).

Adopters who concurrently use the platform's other advisory products hold broader and less actively traded self-directed portfolios in those months. They hold 8.93 more funds and have concentration lower by 0.065 (Online Appendix Table A.19). Thus, using delegated products does not necessarily eliminate self-directed investing. Taken together, the estimates show that entry into RA is associated with economically meaningful changes in the self-directed portfolio even before variation in the delegated share is considered.

### 7.2. Dose Response in the Delegated Share

The event study abstracts from the amount delegated; the dose-response analysis reintroduces this intensive margin. Table 9 relates lagged RA-share bins to outcomes in the self-directed sleeve using a panel specification with investor and month fixed effects. Investor-months with no lagged RA share form the omitted category. If a larger delegated share merely intensified the portfolio pattern associated with entry, the self-directed outcomes would change monotonically across the bins. The estimates instead display a non-monotonic relationship.

Low and moderate delegated shares are associated with less concentrated, broader, and less actively traded self-directed portfolios. Bin 1, which includes lagged RA shares in $(0, 0.20]$ and accounts for 26.8 percent of the panel observations, is associated with HHI lower by 0.216 standard deviations,

the number of funds higher by 0.196 standard deviations, and turnover lower by 0.105 standard deviations. Bin 2 has the same signs but smaller magnitudes. The middle bin marks a transition: HHI remains slightly lower, but the number of funds declines and turnover increases.

Figure 5 plots the binned coefficients. The non-monotonicity is the central result: low delegated shares and near-complete delegation are associated with different residual self-directed portfolios. In Bin 4, HHI is higher by 0.117 standard deviations and turnover by 0.299 standard deviations, while the number of funds is lower. In Bin 5, where the lagged RA share exceeds 0.80, the probability of holding any self-directed mutual fund is lower by 33.2 percentage points. Among observations with self-directed mutual fund holdings, concentration is higher by 0.396 standard deviations, the number of funds is lower by 0.122 standard deviations, and turnover is higher by 0.805 standard deviations. At very high delegated shares, the remaining self-directed sleeve is therefore smaller, more concentrated, and more actively traded.

A larger delegated share does not simply magnify the extensive-margin pattern observed around adoption. Moderate delegation is associated with broader and less actively traded self-directed portfolios, consistent with RA absorbing some marginal or lower-conviction positions. Near-complete delegation instead leaves a smaller, more concentrated, and more actively traded residual sleeve. This pattern is consistent with investors retaining outside RA the positions over which they have stronger views, as an authority-cost interpretation would predict. The evidence remains associational, however, and does not establish that investors deliberately retain these particular positions.

A mechanical or naive-allocation account (Benartzi & Thaler, 2001) could explain part of the contraction in the self-directed sleeve, but it less readily explains why the residual becomes both more concentrated and substantially more actively traded as the delegated share rises. Online Appendix Table A.16 provides a complementary comparison. The RA-share bins are unrelated to gambling bias and are associated with the disposition effect in only one of the five bins. The dose pattern therefore appears more closely related to the composition of the residual portfolio than to uniform changes in the investor's measured behavioral characteristics.

### 7.3. Performance Across the Lifecycle Margins

If recent performance were the principal state variable governing RA use, its relationship with demand should be particularly visible at first entry. Online Appendix Tables A.13 and A.14 add prior self-directed fund returns to the first-adoption specifications. The coefficients are generally small or unstable across horizons and return definitions, and their signs differ between the pre-window and lag-one hazard specifications. Before first adoption, investors do not have individual RA return histories, so their self-directed fund portfolios provide the directly observed investor-specific performance signal. The evidence does not support a simple account in which investors enter RA primarily following poor self-directed returns or mechanically chase a particular recent fund-return state.

Table 10 examines return associations across the RA lifecycle. Because fund returns enter the selection equation in each specification, the table shows how recent self-directed performance is related to first adoption, exit, and continuation. Section 6.4 shows that these relationships are generally modest. The lifecycle analysis adds that they differ across margins. The magnitude of recent returns is associated with first entry, whereas their sign is more relevant to exit and continuation. Performance

therefore enters the lifecycle in a structured way rather than through a single return-chasing channel.

Panel A examines first adoption. In the selection equation, both components of the lagged fund return are negatively associated with the probability of first adoption. A one-standard-deviation increase in the positive-return component is associated with a reduction of approximately 0.2 to 0.3 percentage points, while a corresponding increase in the magnitude of the negative-return component is associated with a reduction of approximately 0.6 percentage points. All estimates are significant at the 1 percent level. Relative to a first-adoption rate of approximately 3.2 percent, these magnitudes are economically meaningful, with the loss component approaching one-fifth of the base rate.

The 3.2 percent rate differs from both the 1.43 percent panel rate and the 7.7 percent pre-window rate because the three designs use different denominators. This specification conditions on at-risk months for investors who have not yet adopted and excludes their post-adoption observations. Its coefficients must therefore be interpreted relative to its own at-risk base rate. The decomposition also helps explain why specifications using a single signed return produce weaker results. Both gains and losses are negatively associated with first adoption when their magnitudes are entered separately. The pattern is therefore inconsistent with simple return chasing because larger gains deter entry as well as larger losses. Conditional on first adoption, the initial-share coefficients are small at short horizons and become positive and statistically significant only at six months, when the coefficients on fund return$^{+}$ and fund return$^{-}$ reach 0.024 and 0.042, respectively.

Panel B examines exit using a probit specification. Positive recent self-directed fund performance is associated with an increase of approximately 0.1 to 0.3 percentage points in the probability of exiting RA, whereas losses are associated with a reduction of approximately 0.1 to 0.2 percentage points. RA performance matters primarily through a positive lag-one coefficient on RA return$^{-}$, indicating that poor recent RA performance is associated with a greater probability of exit.

Panel C examines continuation and produces the corresponding pattern. Positive self-directed fund returns are associated with a lower probability of continuation, whereas losses are associated with a higher probability. Thus, gains increase exit and reduce continuation, while losses reduce exit and increase continuation. Because the continuation rate is close to one, the associated changes in probability are small in levels, and their direction is more informative than their magnitude.

This exit-continuation pattern is consistent with reinforcement learning (Choi et al., 2009). Strong self-directed performance rewards the activity of managing funds independently and is therefore associated with movement away from delegation. The relationship between performance and exit also differs from evidence in retirement plans, where allocations are frequently shaped by defaults and inertia and respond only weakly to returns (Agnew et al., 2003; Samuelson & Zeckhauser, 1988).

Within the pool of active investors, return associations are strongest on the intensive margin. Both self-directed fund-return components are positively associated with the conditional RA share in Panel C, with coefficients ranging from approximately 0.012 to 0.014. These estimates amount to roughly 8 to 9 percent of the conditional RA-share mean. RA performance enters through its positive component at every horizon and through its negative component at six months. Recent returns therefore have different relationships with the lifecycle margins. Their magnitude is associated with first entry and the amount delegated, while their sign is more closely related to exit and continuation. These patterns do not absorb the characteristic-based distinction between adoption and conditional commitment.

The lifecycle evidence further clarifies who uses RA. First entry is associated with pre-adoption wealth, experience, risk preference, gender, and behavioral characteristics. Conditional allocation among active users is associated with many of the same characteristics, as well as with return salience and accumulated product experience. Exit and continuation respond differently to gains and losses and are not well summarized by a one-directional return-chasing account. Treating RA use as a single binary outcome would combine these lifecycle states and conceal their distinct economic relationships.

### 7.4. Interpretation and Limits

The consequence evidence supports a narrow, margin-specific conclusion. The event-time figures show that outcomes in the self-directed sleeve change around RA entry. The dose-response estimates show that these outcomes vary non-monotonically across lagged RA-share bins. The adopter-panel estimates further show that use of the platform's other advisory products can coexist with substantial self-directed investing rather than mechanically replacing it. Together, these findings establish that entry and the delegated share are empirically distinct margins and that their relationships with the self-directed sleeve are not captured by a single monotonic pattern.

The results do not identify the total causal effect of RA or decompose such an effect into entry and commitment components. Quantifying the welfare consequences of delegation would require both a causal research design and a mapping from portfolio changes into investor utility, as in the diversification-welfare literature (Calvet et al., 2007; Rossi & Utkus, 2024). We do not attempt that exercise.

## 8. Robustness

The Online Appendix organizes the robustness analyses around the claims they evaluate. Most checks preserve the paper's principal sign patterns and economic interpretations. The important exception is the exclusion restriction, discussed below, for which the estimates display meaningful sensitivity.

The cross-margin sign patterns are not specific to the baseline specification or estimator. Online Appendix Table A.4 jointly includes demographics, skills, and all behavioral measures and preserves the sign pairs for underdiversification, turnover, and confirmation bias. Online Appendix Table A.6 replaces the Fama–MacBeth estimator with a panel control-function specification containing investor fixed effects and two-way clustered standard errors. It recovers the same intensive-margin signs for both the main delegated-share outcome and the risky-share outcome. The main attenuation concerns fund experience in the joint specification, whose conditional-share coefficient becomes small and statistically insignificant. This attenuation is already incorporated into the interpretation in the main text.

The principal behavioral signs also remain under alternative outcome definitions, product comparisons, samples, and measurement windows. Online Appendix Table A.5 preserves the behavioral relationships across alternative intensive-margin outcomes. The selection-correction coefficient is positive for the share outcomes and negative for the level outcome. This pattern weighs against interpreting the residual selection channel as a simple wealth or scale proxy, although it does not by itself identify willingness to cede control. The stand-alone AUTO Heckman estimates in Online Appendix

Table A.7 confirm that AUTO and RA have different determinants, as required for the authority-ladder comparison.

Online Appendix Table A.10 varies the pre-use horizon and relaxes the sample restriction. The five cross-step equality rejections emphasized in the main text remain at every reported horizon, while the results for extrapolation and the composite indices, on which the interpretation does not rely, are less stable. Online Appendix Table A.11 re-estimates the reclaiming-control interaction under three restrictions on the adopter sample. The interaction remains negative and similar in magnitude across the three specifications. The return analyses in Online Appendix Tables A.8, A.9, and A.15 show that including recent performance does not absorb the characteristic gradients reported in the main text. Online Appendix Table A.17 and Figure A.3 examine stability across sample windows and adoption cohorts. Online Appendix Tables A.18 and A.19 document the age-bias correlations and the self-directed portfolio relationships used in the interpretation.

Population reweighting produces a similar conclusion for the observed behavioral coefficients. The calibrated-weight estimates in Online Appendix Table A.1 preserve the principal cross-margin sign pairs. Underdiversification and turnover have conditional-share coefficients of $+0.055$ and $+0.062$, respectively, while the coefficient on confirmation bias is $-0.039$. Reporting the unweighted estimates as the main results therefore improves precision without determining the behavioral sign patterns.

The selection-correction coefficient is an exception. Under population weighting, the coefficient on the inverse Mills ratio becomes negative and statistically significant, ranging from $-0.098$ to $-0.197$, compared with positive coefficients of $+0.158$ to $+0.212$ in the unweighted specifications. Reweighting changes the composition of the estimation sample, giving greater influence to observations representative of the platform's larger non-adopter population. Because the selection-correction coefficient summarizes residual dependence between the entry and outcome equations, it is particularly sensitive to this compositional change. The proposed trust interpretation in Section 5.2 is therefore specific to the unweighted estimates and is not robust to population reweighting. None of the paper's mechanism evidence relies on this interpretation.

The adopter-only OLS estimates provide a useful benchmark for interpreting the exclusion sensitivity. They preserve the focal intensive-margin signs without using either the inverse Mills ratio or platform tenure, but they condition on observed adoption and therefore do not address selection into the outcome sample. The exclusion analysis below concerns the stronger, selection-adjusted interpretation of the conditional-share relationships.

The most consequential sensitivity concerns the exclusion restriction. Online Appendix Table A.2 compares three specifications. In the benchmark Arm E, platform tenure enters the selection equation but is excluded from the delegated-share equation. Arm B includes tenure in both equations, while Arm C excludes it from both. All three arms use the same block of nine behavioral measures, excluding speculative turnover. The benchmark coefficients therefore differ slightly from those in Table 2. For example, the outcome coefficient on underdiversification is $+0.018$ rather than $+0.020$. The relevant comparison is across arms within Online Appendix Table A.2.

The outcome-stage coefficients are not stable across these specifications. In the biases specification, the underdiversification coefficient changes from $+0.018$ in the benchmark to $-0.042$ in Arm B. The confirmation-bias coefficient changes from $-0.023$ to a statistically insignificant $+0.006$. The

turnover coefficient remains positive but falls from $+0.039$ to $+0.009$ and becomes statistically insignificant. The selection-corrected intensive-margin estimates are therefore sensitive to whether platform tenure is permitted to enter the outcome equation.

The coefficient movements exhibit a common structure rather than fifteen independent changes. Across all fifteen outcome coefficients, the difference between Arm B and the benchmark is approximately 0.30 times the corresponding selection-equation coefficient. The correlation is 0.997, and a regression through the origin produces an $R^2$ of 0.99. Thus, the Arm B coefficient vector can be reproduced almost entirely by applying a common multiple to the selection coefficients.

This pattern reflects the limited independent variation in the selection correction when there is no excluded variable. Without a variable excluded from the share equation, the inverse Mills ratio is a nonlinear transformation of a probit index constructed from the same regressors that appear in the outcome equation. Over the relevant range, this transformation is approximately linear in the index. Including the correction term then approaches including a linear combination of the outcome regressors a second time, causing the coefficients to move jointly. Whether a particular coefficient changes sign depends on the size of its benchmark outcome coefficient relative to the common shift implied by its selection coefficient. For underdiversification, the relevant threshold is $0.30 \times 0.172 = 0.052$, which exceeds the benchmark coefficient of $0.018$, so the sign changes. For turnover, the corresponding threshold is $0.30 \times 0.084 = 0.025$, below the benchmark coefficient of $0.039$, so the sign remains positive.

The diagnostics reinforce this interpretation. The $R^2$ from regressing the inverse Mills ratio on the outcome regressors is 0.995 in Arms B and C, compared with 0.988 in the benchmark. The condition number of the outcome design matrix increases from 96 in the benchmark to 138 in the alternative arms. The implied $\hat{\rho}$ ranges from 3.5 to 4.2 in those arms, outside the admissible range for a correlation coefficient. The benchmark also exhibits strain: its implied $\hat{\rho}$ is 1.381 in the biases specification, and the joint specification in Online Appendix Table A.4 likewise produces a value greater than one. For this reason, we report the estimated coefficient on the inverse Mills ratio, $\hat{\lambda}$, rather than interpreting the implied $\hat{\rho}$ as a correlation.

These diagnostics imply that the benchmark is not free of functional-form and collinearity concerns. The exclusion supplies additional variation, but the inverse Mills ratio remains highly predictable from the outcome regressors even in the benchmark specification. The more extreme diagnostics in Arms B and C show that identification deteriorates further when the exclusion is removed. The exercise therefore establishes that platform tenure performs substantial identifying work in the outcome equation. It does not establish that the selection-corrected outcome coefficients are robust to relaxing the exclusion, and we do not claim that they are. These coefficients should be interpreted as conditional on both the validity of the exclusion restriction and the maintained selection-model functional form.

Section 4.5 identifies the results that do not depend on this exclusion. The adoption-side estimates are obtained from the selection equation and do not rely on recovering outcome-stage coefficients from the selection correction. The authority ladder has no selection stage and requires no excluded variable. By contrast, the first-adoption and panel control-function analyses retain the tenure exclusion while changing the sample or source of variation. They therefore provide evidence across alternative

designs but do not independently validate the exclusion restriction.

## 9. Conclusion

Robo-advising delegates investment decisions rather than merely digitizing access to financial markets. This distinction changes how demand for the product should be measured. We use Alipay panel data from September 2020 through September 2024 in a setting where the algorithm implements its own recommendations. The setting therefore separates three decisions: whether an investor adopts, how much fund wealth she delegates, and whether she maintains that allocation over time. The latter two decisions capture the extent of her commitment. These margins are associated with different investor characteristics, and several characteristics have opposite relationships across them. A binary measure of use consequently combines decisions that a delegated product keeps distinct.

A small set of estimates summarizes the central pattern. A one-standard-deviation increase in portfolio concentration is associated with a 6.5-percentage-point reduction in adoption and a 2.0-percentage-point increase in the conditional delegated share. The latter amounts to 13 percent of the mean delegated share of 0.151. Turnover exhibits the same sign pattern, with a conditional-share coefficient equal to 30 percent of the mean. Confirmation bias exhibits the reverse pattern, being positively associated with adoption and negatively associated with the delegated share. Wealth operates primarily through entry: a one-standard-deviation increase is associated with a 14.2-percentage-point increase in adoption. Pre-adoption fund experience is the strongest observed deterrent to first adoption, with a first-stage coefficient of $-0.190$ relative to a first-adoption rate of 7.7 percent.

The selection-correction coefficient is positive in the main unweighted specifications but becomes negative when the sample is reweighted to platform proportions. It is also sensitive to the exclusion restriction and should not be given a structural interpretation. Trust remains one possible interpretation of the positive unweighted coefficient, but it is not an identified finding and does not form part of the mechanism evidence. More generally, the results help explain why prior studies reach different conclusions about who uses robo-advice (Baulkaran & Jain, 2023; Brenner & Meyll, 2020; Piehlmaier, 2022). Different samples and outcome definitions may combine entry, commitment, and lifecycle stages differently within a single adoption indicator.

Three mechanisms are consistent with the separation between entry and commitment, each evaluated using evidence unavailable in the baseline cross-section. First, selective awareness can account for the principal behavioral pattern. Underdiversification and turnover decline after adoption, with larger declines among investors exhibiting higher pre-adoption levels and more negative engagement gradients among investors who use the platform more frequently. Behavioral characteristics that RA does not make comparably salient do not exhibit the same engagement-related decline. Second, the within-investor-month authority ladder distinguishes full delegation from automated execution. High-turnover and concentrated investors are relatively more likely to use AUTO and less likely to take the additional step toward RA, whereas engagement and attention constraints produce the opposite pattern. Third, commitment varies with tenure. The association between tenure and the delegated share is more negative among investors with greater pre-adoption fund experience, while the self-directed portfolio gradually rebuilds. Recent performance is also related to lifecycle choices, but including it

does not absorb the principal characteristic gradients.

Taken together, the evidence is consistent with delegated advice being diagnostic as well as substitutive. Using a rule-based portfolio may change what investors perceive about their own portfolio management, so demand need not be a fixed response to ability differences understood before adoption.

The policy implication follows from these mechanisms rather than directly from the coefficient magnitudes and should be treated as a hypothesis for future testing. If selective awareness constrains entry, a platform seeking to reach underdiversified or high-turnover investors could make investors' own portfolio patterns salient before adoption and compare this intervention with advertising the algorithm's past performance. If delegation is partly reversible, assets retained may also be an incomplete measure of whether a corrective product has provided value.

Two limitations bound these conclusions. First, the evidence comes from one platform in one country. Second, the mechanism analyses are observational. They produce patterns consistent with selective awareness, decision authority, and reclaiming control but do not uniquely identify these mechanisms. The interpretation of residual selection as trust is more tentative still, given its sensitivity to weighting and the exclusion restriction. Experiments that vary the salience of investors' own portfolio choices or the decision rights transferred to the algorithm could identify these channels more directly.

The margin decomposition may nevertheless extend beyond this setting. Whenever participation requires delegation, entry and commitment can respond to different characteristics. Target-date funds, managed accounts, and advisor-sold products share this structure and can be examined using the same distinction. The evidence here indicates that demand for delegated financial advice is not well summarized by adoption alone.

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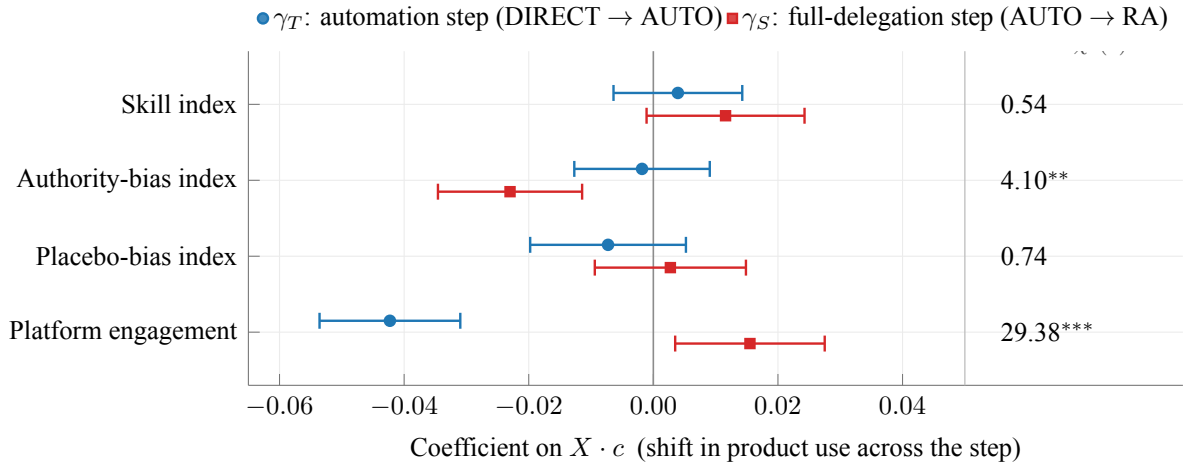
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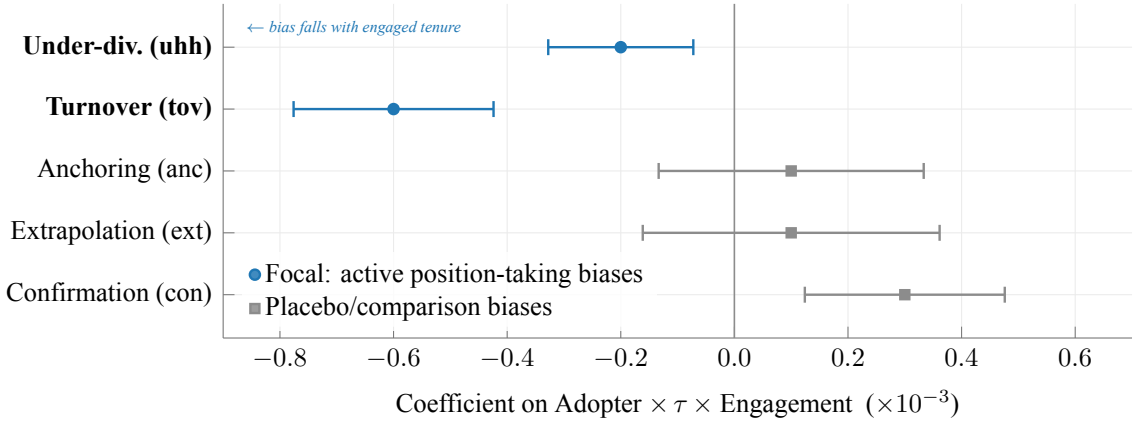
# Figures

Figure 1: Decision-Authority Mechanism: What Moves Investors Across Each Delegation Step



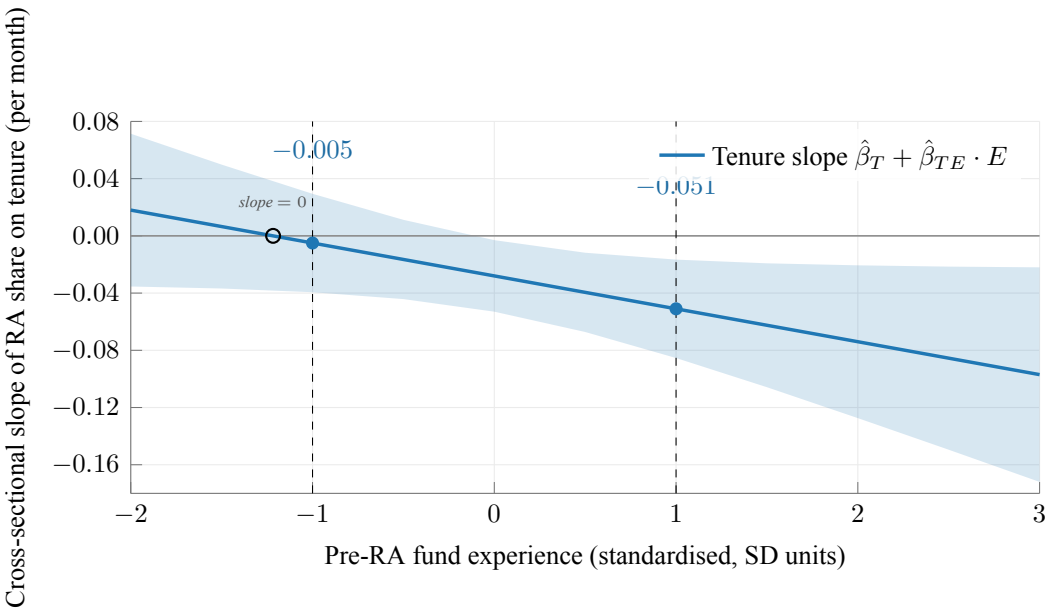

*Notes.* Each row reports two coefficients from the headline stacked linear-probability specification using the six-month pre-use window and the identifying-only sample. The model is estimated on the investor-month × product panel with investor-month choice-set fixed effects. $\gamma_T$ (blue circles) is the coefficient on $X \cdot c_T$ and measures the difference in the association of $X$ with AUTO versus DIRECT use. $\gamma_S$ (red squares) is the coefficient on $X \cdot c_S$ and measures the incremental difference for RA versus AUTO use. Horizontal bars are 95% confidence intervals computed from the reported two-way clustered standard errors. The right-hand column reports the Wald $\chi^2(1)$ test of $\gamma_T = \gamma_S$ for each index (** $p < 0.05$, *** $p < 0.01$). Overlap of the two confidence intervals is not the equality test, because the Wald test uses the joint covariance of the two coefficients.
*Reading the figure.* The skill index is not associated with the automation step ($\gamma_T = 0.004$, $t = 0.75$), while its coefficient for the full-delegation step is positive and marginally significant ($\gamma_S = 0.012$, $t = 1.79$); the two coefficients are not statistically different (Wald $\chi^2 = 0.54$, $p = 0.461$). The authority-bias index is approximately zero for the automation step but negative for the full-delegation step ($\gamma_S = -0.023$, $t = -3.90$), and the two step coefficients differ at the 5% level (Wald $\chi^2 = 4.10$, $p = 0.043$). The placebo-bias index is statistically indistinguishable from zero at both steps and shows no cross-step difference. Platform engagement exhibits the sharpest split: it is strongly negative for the automation step ($\gamma_T = -0.042$, $t = -7.34$) and positive for the full-delegation step ($\gamma_S = 0.016$, $t = 2.53$), with a highly significant equality rejection (Wald $\chi^2 = 29.38$, $p < 0.001$). These patterns are descriptive associations consistent with the decision-authority mechanism, not causal effects.

Figure 2: Selective Awareness: Engagement-Dosed Bias Response to RA Tenure



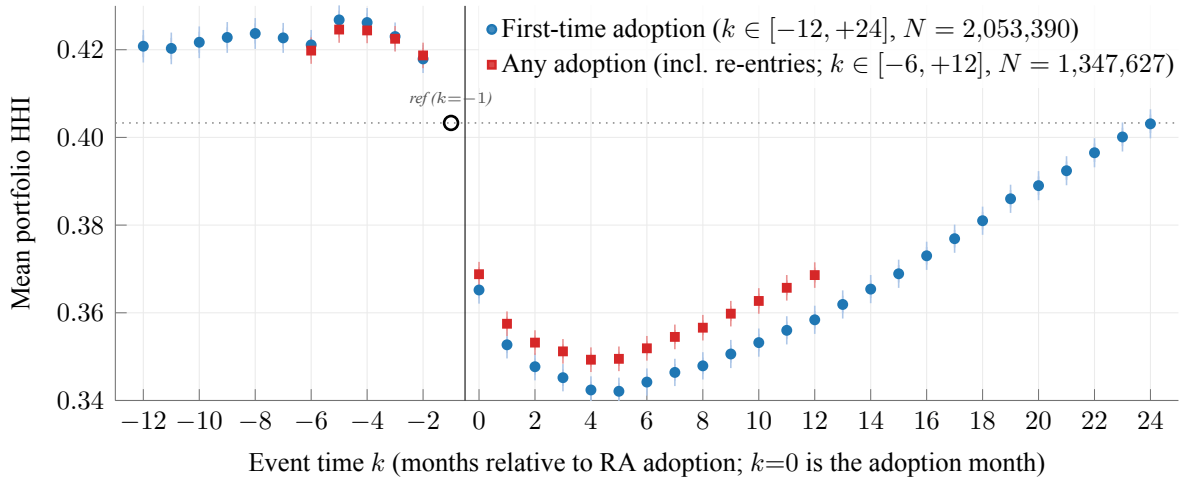

*Notes.* Each point is the triple-interaction coefficient Adopter $\times \tau \times$ Engagement (multiplied by $10^3$) from the difference-in-differences specification with matched never-adopters in Panel A of Table 5; $\tau$ is months since first robo-advisor adoption (capped at 24) and Engagement is the standardised lagged 12-month login count. The coefficient measures how the bias trajectory over RA tenure steepens with platform engagement. Horizontal bars are 95% confidence intervals; standard errors are recovered as $|\text{coef}/t|$ from $t$-statistics two-way clustered by investor and calendar month. *Reading the figure.* For engaged adopters, the two focal biases that require active position-taking—under-diversification ($-0.0002$, $t = -3.07$) and turnover ($-0.0006$, $t = -6.68$)—decline significantly faster with RA tenure. Among the comparison biases, anchoring and extrapolation are statistically indistinguishable from zero, consistent with the engagement dose not producing a blanket bias improvement. Confirmation bias is the one exception: it carries a small, significantly *positive* coefficient ($+0.0003$, $t = 3.34$), i.e. it does not decline. The contrast—focal biases falling, comparison biases flat or rising—is consistent with selective awareness for the biases tied to active position-formation rather than across-the-board improvement.

Figure 3: Reclaiming Control: Tenure Slope of RA Share by Pre-RA Experience



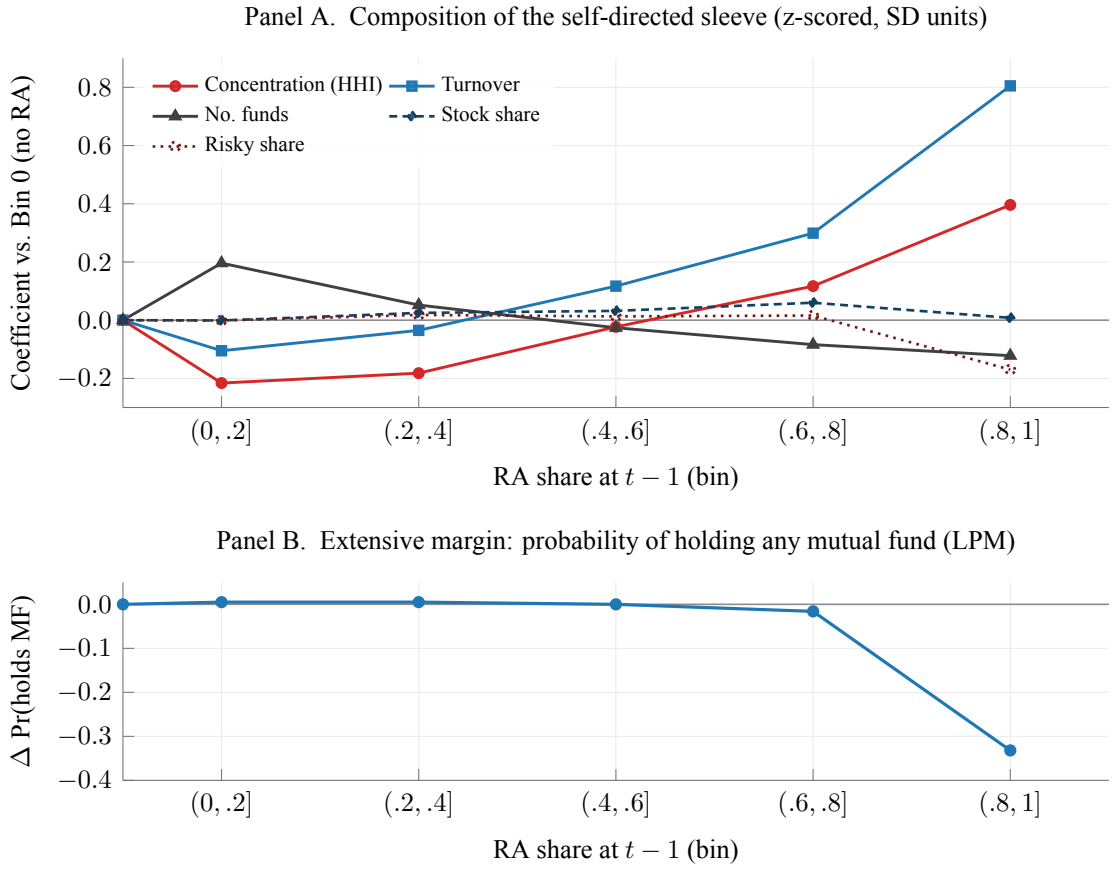

*Notes.* The figure plots the cross-sectional slope of RA share (RA assets / (RA + MF), among RA-active adopter-months) on robo-advisor tenure, as a function of standardised pre-RA fund experience $E$. Because estimation is Fama–MacBeth (monthly cross-sectional regressions), this is the average within-month gradient of RA share with respect to tenure—the RA-share difference associated with a one-month difference in tenure across adopters—rather than a within-investor time derivative, and it is a local slope that should not be extrapolated over many months. Coefficients are from column (1) of Table 8 (linear interaction, Fama–MacBeth OLS): $\hat{\beta}_T = -0.028$ (Tenure main effect, $t = -2.19$) and $\hat{\beta}_{TE} = -0.023$ (Pre-RA experience × Tenure, $t = -1.91$); the plotted quantity is $\hat{\beta}_T + \hat{\beta}_{TE} \cdot E$. The shaded band is a 95% confidence interval from the delta method, treating $\hat{\beta}_T$ and $\hat{\beta}_{TE}$ as approximately independent and recovering standard errors as $|\hat{\beta}/t|$. *Reading the figure.* The tenure slope is essentially zero for the least-experienced adopters ($E = -1$ SD: $-0.005$, within the confidence band of zero) and turns increasingly negative as pre-RA experience rises ($E = +1$ SD: $-0.051$). The slope crosses zero at $E \approx -1.2$ SD, so for all but the least-experienced adopters longer RA tenure is associated with a lower RA share of wallet, and the gradient is most negative for the most fund-experienced investors. This is the reclaiming-control pattern: investors with more pre-RA fund-investing experience exhibit lower delegated shares at longer RA tenures. The experience×tenure interaction is itself significant only at the 10% level ($\hat{\beta}_{TE} = -0.023$, $t = -1.91$), so the *steepening* of the decline with experience is suggestive; the tenure decline at and above mean experience is significant at the 5–1% level (e.g. $t = -2.9$ at $E = +1$). We plot the slope rather than fitted RA-share levels because the linear specification produces predicted shares outside $[0, 1]$ at extreme combinations of tenure and experience; the slope plot is invariant to that boundary issue.

Figure 4: What Changes When Investors Adopt RA?




*Notes.* Mean portfolio Herfindahl index (HHI) by event time relative to RA adoption. Each level is computed as $\bar{y}_{k=-1} + \hat{\beta}_k$, where $\hat{\beta}_k$ is the event-study coefficient from a within-investor fixed-effects regression of HHI on event-time dummies and $\bar{y}_{k=-1}$ is the cross-sectional mean of HHI in the month before adoption (the omitted reference category in the regression: $0.4033$ for first-time adoption, $0.4066$ for any adoption). Vertical bars are 95% confidence intervals, computed as $1.96 \times \text{se}(\hat{\beta}_k)$ and treating the baseline as a known constant. The vertical dashed line marks the adoption transition between $k = -1$ and $k = 0$; the horizontal dotted line marks the first-time-adoption baseline of $0.4033$. The open marker at $k = -1$ is the omitted reference category. *Two series.* The blue circles plot the *first-time adoption* specification, anchoring each investor at her first-ever RA adoption ($k \in [-12, +24]$, $N = 2{,}053{,}390$). The red squares plot the *any-adoption* specification that stacks every adoption event including re-entries ($k \in [-6, +12]$, $N = 1{,}347{,}627$). The two series have different sample windows because the first-time specification can look further back from $k = 0$ (more pre-adoption history is available per investor), while the any-adoption specification has shorter usable pre/post windows once re-entry events are stacked. *Reading.* Mean HHI rises modestly in the year before first adoption, peaking around $0.427$ at $k = -5$, then drops sharply to $0.365$ in the adoption month, troughs at $0.342$ four to five months after adoption, and drifts back toward the pre-adoption baseline thereafter, reaching $0.403$ at $k = +24$ ($\hat{\beta}_{24} = -0.0002$, $|t| < 1$, indistinguishable from the reference). The any-adoption series tracks the first-time series closely over its shorter window, suggesting re-entries reproduce the same diversification pattern. Companion outcomes (RA share and risky RA share) are reported in the main event-study table.

Figure 5: Robo-Advisor Dose-Response on the Self-Directed Mutual-Fund Sleeve




*Notes.* Each marker is the within-investor coefficient on a lag-1 RA-share bin from the two-way fixed-effects regressions in Table 9, relative to the omitted no-RA reference (Bin 0, $rsh_{t-1} = 0$). Points are plotted at the bin midpoint, and each series is anchored at the origin, where Bin 0 (the omitted no-RA reference) has a coefficient of zero by construction. Connecting lines join the five estimated bin coefficients and serve only as visual guides; the bins are categorical, not a continuous dose. *Panel A* shows the composition of the self-directed mutual-fund sleeve: outcomes are winsorised at 1/99 and z-scored within month, so coefficients are in within-month standard-deviation units. As RA intensity rises, the residual self-directed sleeve becomes more concentrated (HHI), higher-turnover, and held in fewer funds; stock share rises modestly and risky share falls only at the highest dose. *Panel B* shows the extensive margin: the linear-probability coefficient for holding any mutual fund is flat across low and middle doses and collapses at the top bin ($-0.332$), i.e. investors with near-total RA share largely exit self-directed mutual funds altogether. Among columns (2)–(6), every bin coefficient is significant at the 1% level except: Stock share Bins 1 and 5 (insignificant); Risky share Bins 1, 3, and 4 (insignificant) and Bin 2 (significant at the 10% level only). In column (1), Bin 3 is insignificant. Standard errors are two-way clustered on investor and month. These are within-investor co-movements across the RA-share dose, not causal effects of RA usage.

# Tables

Table 1: Summary Statistics for Variables Used in the Main Heckman and Robustness Analyses

| Variable | N | Mean | SD | Min | p25 | Median | p75 | Max |
|---|---|---|---|---|---|---|---|---|
| **Panel A. Demographics (Investor-Level)** | | | | | | | | |
| Age (years) | 100,000 | 32.499 | 9.607 | 19.000 | 25.000 | 30.000 | 37.000 | 64.000 |
| Risk Preference (0-5) | 99,992 | 2.781 | 1.069 | 0.000 | 2.000 | 3.000 | 4.000 | 5.000 |
| Female dummy | 100,000 | 0.425 | — | 0.000 | — | — | — | 1.000 |
| **Panel B. Assets and Allocation (Investor-Month)** | | | | | | | | |
| Total Financial Assets (CNY) | 4,900,000 | 95,245 | 174,510 | 0.000 | 6,534.730 | 30,220 | 99,531 | 1.37M |
| Robo-Advisor Assets (CNY) | 4,900,000 | 2,194.960 | 8,526.560 | 0.000 | 0.000 | 0.000 | 199.460 | 102,820 |
| Total Mutual-Fund Assets (CNY) | 4,900,000 | 66,238 | 134,884 | 0.000 | 1,833.940 | 15,482 | 63,181 | 1.11M |
| Baby/Money-Market Assets (CNY) | 4,900,000 | 20,630 | 48,786 | 0.000 | 167.667 | 2,656.380 | 16,043 | 393,365 |
| Log(Total Financial Assets) | 4,900,000 | 9.824 | 2.571 | 0.000 | 8.785 | 10.316 | 11.508 | 14.132 |
| RA Share of TFA | 4,837,721 | 0.030 | 0.087 | 0.000 | 0.000 | 0.000 | 0.007 | 0.712 |
| RA Share of (RA + MF), unconditional | 4,492,147 | 0.053 | 0.148 | 0.000 | 0.000 | 0.000 | 0.017 | 1.000 |
| *RA Share of (RA + MF), \| RA active* | 1,571,475 | **0.151** | 0.219 | 0.000 | 0.013 | 0.057 | 0.185 | 1.000 |
| Risky RA Share, unconditional | 4,452,494 | 0.079 | 0.189 | 0.000 | 0.000 | 0.000 | 0.038 | 1.000 |
| *Risky RA Share, \| risky RA active* | — | 0.225 | 0.262 | — | 0.028 | 0.115 | 0.331 | — |
| Login Count (last 12 months) | 4,899,785 | 1,758 | 1,402 | 37.000 | 739.000 | 1,382 | 2,378 | 7,977 |
| Months Since Registration | 4,900,000 | 112.691 | 42.284 | 11.000 | 82.000 | 109.000 | 141.000 | 229.000 |
| **Panel C. Behavioral Biases, Diversification, and Trading Skills** | | | | | | | | |
| *Behavioral biases (winsorised 1%/99% within month; natural units, not standardized)* | | | | | | | | |
| Anchoring Bias | 4,670,815 | 0.491 | 0.428 | 0.000 | 0.000 | 0.500 | 1.000 | 1.000 |
| Total Turnover | 4,402,068 | 0.291 | 0.390 | 3.20e-05 | 0.021 | 0.096 | 0.420 | 4.610 |
| Attention Bias | 4,670,815 | 0.416 | 0.412 | 0.000 | 0.000 | 0.323 | 0.913 | 1.000 |
| Disposition Effect | 4,367,178 | 0.022 | 0.376 | -1.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| Underdiversification (HHI) | 4,717,162 | 0.423 | 0.318 | 0.032 | 0.162 | 0.319 | 0.604 | 1.000 |
| Extrapolation Bias | 4,670,815 | 0.187 | 0.325 | 0.000 | 0.000 | 0.000 | 0.247 | 1.000 |
| Gambling Bias | 4,670,815 | 0.125 | 0.267 | 0.000 | 0.000 | 0.000 | 0.076 | 1.000 |
| Confirmation Bias | 3,619,997 | 0.821 | 0.340 | 0.000 | 0.900 | 0.990 | 1.000 | 1.000 |
| Herding Bias | 4,670,815 | 0.871 | 0.284 | 0.000 | 0.950 | 1.000 | 1.000 | 1.000 |
| *Portfolio diversification* | | | | | | | | |
| Number of Fund Holdings | 4,717,162 | 10.493 | 13.982 | 1.000 | 3.000 | 6.000 | 12.000 | 94.000 |
| Effective N ($1/\sum w^2$) | 4,717,162 | 4.929 | 5.157 | 1.000 | 1.655 | 3.139 | 6.163 | 31.742 |
| Weighted Portfolio Correlation | 3,568,973 | 0.367 | 0.273 | -0.329 | 0.158 | 0.394 | 0.572 | 0.942 |
| *Skills and experience* | | | | | | | | |
| Fund Experience (months) | 4,717,162 | 23.215 | 13.420 | 1.000 | 12.000 | 23.000 | 34.000 | 49.000 |
| Selection Skill | 4,685,235 | -0.002 | 0.024 | -0.233 | -0.011 | -0.001 | 0.007 | 0.138 |
| Timing Rate | 4,571,144 | 2.14e-04 | 0.015 | -0.288 | -0.002 | 1.75e-04 | 0.002 | 0.312 |
| RA Experience (months) | 2,377,855 | 14.426 | 10.932 | 0.000 | 6.000 | 11.000 | 21.000 | 49.000 |
| **Panel D. Returns (Winsorised at 1%/99% Within Month; Natural Units, Not Standardized)** | | | | | | | | |
| *Self-directed fund returns* | | | | | | | | |
| Fund Raw Return | 4,243,288 | -0.003 | 0.055 | -0.500 | -0.029 | -3.79e-04 | 0.019 | 0.305 |
| Fund Excess Return vs CSI300 | 4,233,456 | 4.46e-04 | 0.080 | -0.524 | -0.038 | 0.006 | 0.045 | 0.337 |
| Fund $\beta$-Adj Return vs CSI300 | 3,308,592 | -0.004 | 0.052 | -0.223 | -0.027 | -0.002 | 0.012 | 0.298 |
| Fund Category-Adjusted Return | 4,180,163 | -0.003 | 0.026 | -0.245 | -0.013 | -0.002 | 0.007 | 0.143 |
| *Robo-advisor returns* | | | | | | | | |
| RA Raw Return | 1,492,840 | -0.002 | 0.031 | -0.287 | -0.014 | 0.000 | 0.006 | 0.165 |
| RA Excess Return vs CSI300 | 1,492,840 | 0.002 | 0.065 | -0.311 | -0.025 | 0.007 | 0.039 | 0.197 |
| RA $\beta$-Adj Return vs CSI300 | 1,172,397 | -0.002 | 0.031 | -0.128 | -0.014 | -7.38e-04 | 0.007 | 0.160 |
| RA Strategy-Adjusted Return | 1,485,437 | -5.83e-04 | — | — | — | — | — | — |
| **Panel E. Auto-Invest Usage and Product Overlap** | | | | | | | | |
| Auto-Invest Usage (monthly) | 4,900,000 | 0.329 | — | 0.000 | — | — | — | 1.000 |
| Auto-Invest Share (if active) | 1,614,024 | 0.733 | 0.385 | 2.00e-06 | 0.353 | 1.000 | 1.000 | 1.000 |
| *RA × auto-invest cross-tabulation of the 4,900,000 investor-months:* | | | | | | | | |
| Both RA and auto-invest | 683,373 | 13.95% | — | — | — | — | — | — |
| RA only | 888,102 | 18.12% | — | — | — | — | — | — |
| Auto-invest only | 930,651 | 18.99% | — | — | — | — | — | — |

*Continued on next page*

*Table 1 (continued)*

| Variable | N | Mean | SD | Min | p25 | Median | p75 | Max |
|---|---|---|---|---|---|---|---|---|
| Neither | 2,397,874 | 48.94% | — | — | — | — | — | — |
| **Panel F. Adoption Lifecycle (Investor-Month Transitions)** | | | | | | | | |
| *Month-to-month state transitions across 4,900,000 investor-months:* | | | | | | | | |
| Stay out (non-RA → non-RA) | 3,287,884 | 67.10% | — | — | — | — | — | — |
| Continue (RA → RA) | 1,491,648 | 30.44% | — | — | — | — | — | — |
| Enter (non-RA → RA) | 79,827 | 1.63% | — | — | — | — | — | — |
| Exit (RA → non-RA) | 40,641 | 0.83% | — | — | — | — | — | — |
| *Conditional rates:* | | | | | | | | |
| First-adoption rate (per at-risk investor-month) | — | 1.43% | — | — | — | — | — | — |
| Continuation rate (\| RA at $t-1$) | 1,532,289 | 97.35% | — | — | — | — | — | — |
| Re-entry rate (\| RA at $\leq t-2$ only) | 752,774 | 1.30% | — | — | — | — | — | — |
| RA-Active Months per Adopter | 70,034 | 22.439 | 14.172 | 1.000 | 10.000 | 18.000 | 36.000 | 49.000 |

*Notes.* Summary moments for all variables used in the main Heckman regressions and robustness tests, computed on the investor–month panel covering 100,000 investors over 49 analysis months (4.9 million investor-months total); the data span September 2020 – September 2024, with the first month reserved for constructing one-month-lagged regressors. **All continuous variables are winsorised at the 1st and 99th percentiles within each calendar month before the moments are computed, but they are *not* standardized: all statistics are in natural units in the table.** The within-month $z$-scoring used in estimation is applied inside the regression pipeline only, after these moments. Because winsorisation caps vary month by month, pooled minima and maxima reflect the most extreme monthly caps and can lie outside the pooled 1st–99th percentile range. **Panel A** reports time-invariant investor-level demographics computed once per investor. **Panel B** reports investor–month asset balances in Chinese yuan (CNY) and allocation shares. The conditional row *RA Share of (RA + MF) | RA active* (mean 0.151, $N$ = 1,571,475) is the dependent variable in the outcome stage of the Heckman model and the RA Share regressions; the unconditional version above it (which includes all RA-inactive observations as zeros) has mean 0.053. The Risky RA Share, the dependent variable in the risky-allocation robustness specifications, excludes low-risk RA strategies from the numerator and money-market funds from the denominator; its conditional row restricts to months with positive risky RA holdings. Login Count (last 12 months) is reported here for the engagement specifications; the median investor logs in roughly ∼1,382 times per year and the inter-quartile range spans 739–2,378 logins. **Panel C** reports behavioral biases, portfolio-diversification measures, and trading skills; biases take natural values on their constructed ranges (most on $[0, 1]$, Disposition Effect on $[-1, 1]$); Effective $N$ and Weighted Portfolio Correlation are computed on investors with at least two fund holdings. **Panel D** reports fund and robo-advisor monthly returns: (i) raw, (ii) excess vs the CSI300 index, (iii) $\beta$-adjusted excess (using a 36-month rolling market $\beta$ with a 12-month minimum), and (iv) category-adjusted (self-directed funds) or strategy-peer-adjusted (RA). Only $N$ and the mean were reported for the RA Strategy-Adjusted Return in the source output. **Panel E** reports auto-invest usage and product overlap: the monthly investor-level participation rate (0.329) and, conditional on participation, the share of fund purchases executed via automated plans (0.733). The cross-tabulation partitions all 4,900,000 investor-months by contemporaneous RA and auto-invest status; the four cells (Both 683,373; RA only 888,102; Auto-invest only 930,651; Neither 2,397,874) are mutually exclusive and sum exactly to the panel. **Panel F** reports adoption-lifecycle transitions across consecutive investor-months. The four mutually exclusive month-to-month transitions sum to the full panel of 4,900,000 rows. Conditional rates convert these counts into hazard-style rates: the continuation rate conditions on the 1,532,289 at-risk months with RA holdings in $t-1$ (of which 1,491,648 continue and 40,641 exit); the re-entry rate conditions on the 752,774 at-risk months of prior adopters inactive in $t-1$; the first-adoption rate is per at-risk investor-month among never-yet-adopters. 70.0% of investors adopt RA at some point during the sample window (70,034 of 100,000), and conditional on ever adopting, investors hold RA for 22.4 months on average. Sample sizes differ across rows because (i) RA and auto-invest moments are conditional on the relevant product being active; (ii) return moments require non-missing NAV data; (iii) bias and skill moments require minimum fund-holding histories. $N$ is reported as the total number of non-missing observations at the relevant unit (investor or investor-month). All statistics are unweighted.

Table 2: Main Heckman Results

| | Panel 1: Baseline (T=48) | | Panel 2: +Skills (T=47) | | Panel 3: +Biases (T=36) | |
|---|---|---|---|---|---|---|
| Variable | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) |
| Constant | -0.197*** | 0.054 | -0.190*** | -0.036 | -0.152*** | -0.030 |
| | (-6.752) | (0.824) | (-7.663) | (-0.690) | (-12.755) | (-0.662) |
| Log(Total Financial Assets)$_{t-1}$ | 0.142*** | -0.079*** | 0.134*** | -0.049*** | 0.079*** | -0.010 |
| | (12.560) | (-4.230) | (11.754) | (-5.280) | (6.152) | (-1.555) |
| Risk Preference | 0.029*** | 0.008* | 0.022*** | 0.020*** | 0.019*** | 0.012*** |
| | (5.616) | (1.725) | (4.261) | (5.410) | (6.600) | (9.274) |
| Age | 0.004 | -0.005** | 0.003 | -0.010* | 0.016*** | -0.002 |
| | (0.770) | (-2.010) | (0.612) | (-1.791) | (19.588) | (-0.868) |
| Female | -0.042*** | -0.013*** | -0.043*** | -0.020*** | -0.052*** | -0.015*** |
| | (-17.139) | (-2.851) | (-19.983) | (-4.461) | (-19.614) | (-3.624) |
| Combined RA Dummy$_{t-1}$ | 0.224*** | 0.036** | 0.222*** | 0.051*** | 0.218*** | 0.067*** |
| | (24.914) | (2.248) | (29.158) | (3.577) | (90.186) | (4.187) |
| Baby Assets$_{t-1}$ | -0.037*** | 0.014*** | -0.034*** | 0.009*** | -0.021*** | 0.001 |
| | (-18.856) | (3.497) | (-16.496) | (2.926) | (-9.788) | (0.309) |
| Fund Experience (Months)$_{t-1}$ | | | 0.035*** | -0.028*** | | |
| | | | (9.546) | (-6.063) | | |
| Selection Ability$_{t-1}$ | | | 0.004* | 0.000 | | |
| | | | (1.886) | (-0.073) | | |
| Timing Ability$_{t-1}$ | | | 0.001 | -0.001 | | |
| | | | (0.968) | (-1.376) | | |
| Anchoring Bias$_{t-1}$ | | | | | 0.021*** | 0.002 |
| | | | | | (12.172) | (1.057) |
| Total Turnover$_{t-1}$ | | | | | -0.030*** | 0.045*** |
| | | | | | (-14.832) | (5.886) |
| Speculative Turnover$_{t-1}$ | | | | | -0.002*** | -0.019*** |
| | | | | | (-3.365) | (-8.348) |
| Attention Bias$_{t-1}$ | | | | | -0.009*** | -0.005*** |
| | | | | | (-7.133) | (-4.544) |
| Disposition Effect$_{t-1}$ | | | | | 0.003*** | -0.002*** |
| | | | | | (5.211) | (-3.971) |
| Underdiversification HHI$_{t-1}$ | | | | | -0.065*** | 0.020*** |
| | | | | | (-99.824) | (3.811) |
| Extrapolation Bias$_{t-1}$ | | | | | -0.005*** | -0.001 |
| | | | | | (-2.878) | (-1.356) |
| Gambling Bias$_{t-1}$ | | | | | -0.009*** | -0.003*** |
| | | | | | (-5.833) | (-2.977) |
| Confirmation Bias$_{t-1}$ | | | | | 0.030*** | -0.022*** |
| | | | | | (26.037) | (-5.566) |
| Herding Bias$_{t-1}$ | | | | | -0.006*** | -0.003*** |
| | | | | | (-7.422) | (-2.762) |
| Months Since Registration$_{t-1}$ | 0.011*** | | 0.008*** | | 0.014*** | |
| | (10.538) | | (7.802) | | (10.981) | |
| Inverse Mills Ratio ($\lambda$) | | 0.158*** | | 0.203*** | | 0.212*** |
| | | (3.480) | | (4.494) | | (4.324) |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ / $R^2$ | 0.082 | 0.133 | 0.078 | 0.146 | 0.090 | 0.229 |
| N (total) | 4,799,616 | 1,564,155 | 4,471,044 | 1,539,119 | 3,306,408 | 1,230,489 |

*Notes.* Estimates are from unweighted Heckman two-stage (Tobit-2) selection models estimated monthly and averaged following Fama and MacBeth (1973). Reported selection-stage values are *approximate marginal effects* on the probability of RA adoption, computed as $\text{ME}_k = \phi(\Phi^{-1}(\bar{P}_s)) \cdot \hat{\beta}_k$, where $\hat{\beta}_k$ is the Fama–MacBeth average Probit coefficient and $\bar{P}_s$ is the RA-active rate in each specification's estimation sample (RA-adopter months ÷ selection-equation investor-months): $\bar{P}_s = 0.326$ (Baseline), $0.344$ (+Skills), $0.372$ (+Biases), with scaling factors $\phi(\Phi^{-1}(\bar{P}_s)) = 0.360$, $0.368$, and $0.378$ respectively. Marginal effects are computed from the unrounded coefficients and scaling factors and then rounded for display. All estimates are reported to three decimals; Newey–West HAC $t$-statistics (max lag = 3), computed from the time series of monthly coefficients, are in parentheses; $t$-statistics, stars, and significance are invariant to the scaling. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Panel 1 reports the baseline specification. Panel 2 adds investor skills (fund experience, selection ability, timing ability). Panel 3 adds behavioral biases. The selection (Probit) dependent variable is $\text{RA}_{it} = 1$ if the investor holds robo-advisor assets; the outcome dependent variable is $\text{RA}_{share}$ = RA assets / (RA + mutual fund assets), observed only for RA-adopter months. As a robustness check, Table A.1 reports the same specifications estimated with calibrated inverse-probability weights. Table A.2 reports the sensitivity of these estimates to alternative placements of the exclusion restriction.

Table 3: First Adoption of Robo-Advisor and Auto-Invest: Pre-Window Construction

| | Robo-Advisor | | Auto-Invest | |
|---|---|---|---|---|
| Variable | Probit ME | Outcome ($\text{RA}_{share}$) | Probit ME | Outcome ($\text{AUTO}_{share}$) |
| *Panel A: Demographics* | | | | |
| log(TFA), pre-window avg | 0.029*** | -0.041*** | 0.042*** | -0.080*** |
| | (3.709) | (-6.146) | (5.860) | (-2.680) |
| Risk preference | 0.007*** | -0.005 | 0.008*** | -0.005 |
| | (6.824) | (-1.258) | (7.096) | (-0.450) |
| Age at adoption | -0.002 | -0.006* | -0.001 | 0.001 |
| | (-0.578) | (-1.825) | (-0.483) | (0.055) |
| Female | -0.007** | 0.026*** | 0.004 | 0.053*** |
| | (-2.065) | (3.686) | (0.837) | (2.956) |
| Combined-RA share | 0.007*** | -0.005 | 0.002 | 0.011 |
| | (3.249) | (-1.256) | (0.838) | (0.686) |
| Baby assets | -0.013* | 0.020*** | -0.017*** | 0.006 |
| | (-1.884) | (2.995) | (-5.280) | (0.401) |
| *Panel B: Skills* | | | | |
| Fund experience | -0.190*** | 0.027 | -0.137*** | 0.076 |
| | (-6.196) | (0.562) | (-6.707) | (0.821) |
| Selection score (TM) | -0.013*** | -0.013 | -0.010** | 0.044 |
| | (-3.453) | (-0.790) | (-2.446) | (1.326) |
| Timing score (TM) | 0.014** | -0.002 | 0.012 | 0.009 |
| | (2.028) | (-0.407) | (1.442) | (0.462) |
| *Panel C: Behavioural biases* | | | | |
| Anchoring | -0.030*** | -0.032*** | -0.031*** | -0.027 |
| | (-3.841) | (-2.620) | (-5.058) | (-1.112) |
| Turnover | -0.011* | 0.005 | -0.013*** | -0.044** |
| | (-1.928) | (0.593) | (-4.030) | (-2.592) |
| Attention | -0.018*** | 0.006 | -0.017*** | -0.004 |
| | (-3.575) | (1.109) | (-7.589) | (-0.212) |
| Disposition effect | 0.002 | -0.003 | 0.002 | 0.005 |
| | (1.211) | (-1.161) | (1.282) | (0.582) |
| Under-diversification (HHI) | -0.005*** | 0.045*** | -0.007*** | 0.051*** |
| | (-3.437) | (8.034) | (-3.680) | (4.435) |
| Extrapolation | 0.010 | -0.005 | -0.002 | 0.000 |
| | (1.070) | (-1.455) | (-0.731) | (0.024) |
| Gambling | 0.001 | 0.003 | 0.003 | 0.008 |
| | (0.935) | (0.768) | (0.873) | (0.479) |
| Confirmation | 0.030*** | -0.040*** | 0.025*** | 0.025 |
| | (4.801) | (-4.648) | (7.715) | (1.175) |
| Herding | -0.007* | -0.001 | -0.009*** | -0.013 |
| | (-1.949) | (-0.590) | (-8.701) | (-0.731) |
| Months since reg., $t-1$ | 0.004 | | 0.002 | |
| | (0.722) | | (1.500) | |
| Inverse Mills ratio | | -0.081 | | -0.090 |
| | | (-0.989) | | (-0.827) |
| Province Dummies | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ / $R^2$ | 0.195 | — | 0.196 | — |
| N | 254,744 | 19,368 | 82,592 | 6,902 |

*Notes.* The table reports unweighted Heckman two-step (Tobit-2) estimates of first adoption of the robo-advisor (RA) and auto-invest (AUTO) products, estimated monthly and aggregated by Fama–MacBeth. The RA columns set $D_{i,t} = 1$ in the investor's first robo-advisor month with Stage 2 outcome $rsh_{i,t} = RA_{i,t}/(RA_{i,t} + MF_{i,t})$; the AUTO columns set $D_{i,t} = 1$ in the first auto-invest month with Stage 2 outcome $ash_{i,t} = auto_buy_{i,t}/total_buy_{i,t}$. Stage 2 in each event is estimated on the $D = 1$ cross-section. Both events use the same pre-window construction. Female is binary. Stage-1 (selection) values are approximate marginal effects on the first-adoption probability, $\text{ME}_k = \phi(\Phi^{-1}(\bar{P}))\hat{\beta}_k$, scaled by each event's own first-adoption rate in the pre-window estimation sample, – $\bar{P}_{\text{RA}} = 0.077$ (factor $0.144$) and $\bar{P}_{\text{AUTO}} = 0.084$ (factor $0.154$). Stage-2 outcome coefficients are unscaled; $t$-statistics and significance are invariant to the scaling. The reported specification adds investor skills (Panel B) and behavioural biases (Panel C) to the demographics block (Panel A); panels are organised by variable group for readability. Baseline-only and baseline + skills specifications, the lag-1 hazard-style design, and the fund-return specifications are reported in the Online Appendix. Newey–West HAC $t$-statistics (lag = $\min(3, T-1)$) in parentheses below estimates. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Province dummies included, not reported.

Table 4: Decision-Authority Mechanism: Stacked Linear Probability Model with Investor-Month Fixed Effects

| Variable | Automation step DIRECT → AUTO ($\gamma_T$) | Full-delegation step AUTO → RA ($\gamma_S$) | Equality test $\chi^2(1)$ |
|---|---|---|---|
| *Specification (1). Composite indices and pre-use platform engagement* | | | |
| Skill composite index | 0.004 | 0.012* | 0.542 |
| | (0.747) | (1.792) | [0.461] |
| Authority-bias composite index | -0.002 | -0.023*** | 4.102** |
| | (-0.327) | (-3.897) | [0.043] |
| Placebo-bias composite index | -0.007 | 0.003 | 0.735 |
| | (-1.140) | (0.442) | [0.391] |
| Pre-use login frequency | -0.042*** | 0.016** | 29.379*** |
| | (-7.337) | (2.532) | [<0.001] |
| Within $R^2$ | 0.2672 | 0.2672 | 0.2672 |
| *Specification (2). Disaggregated authority-relevant skills and biases* | | | |
| Selection ability | -0.006 | 0.014** | 3.651* |
| | (-1.069) | (2.308) | [0.056] |
| Timing ability | 0.009* | 0.003 | 0.431 |
| | (1.829) | (0.557) | [0.511] |
| Confirmation bias | 0.036*** | 0.012 | 3.722* |
| | (5.449) | (1.640) | [0.054] |
| Total turnover | 0.035*** | -0.032*** | 31.797*** |
| | (5.069) | (-4.913) | [<0.001] |
| Speculative turnover | -0.031*** | -0.013** | 3.539* |
| | (-5.832) | (-2.121) | [0.060] |
| Herding bias | -0.015** | -0.001 | 1.563 |
| | (-2.392) | (-0.163) | [0.211] |
| Disposition effect | 0.005 | -0.009 | 1.662 |
| | (0.947) | (-1.394) | [0.197] |
| Within $R^2$ | 0.2700 | 0.2700 | 0.2700 |
| *Specification (3). Disaggregated placebo biases* | | | |
| Under-diversification (HHI) | 0.058*** | -0.033*** | 69.600*** |
| | (9.101) | (-6.067) | [<0.001] |
| Gambling bias | 0.000 | -0.013** | 1.114 |
| | (-0.009) | (-2.012) | [0.291] |
| Anchoring bias | -0.038*** | 0.048*** | 57.794*** |
| | (-5.979) | (8.165) | [<0.001] |
| Attention bias | -0.042*** | 0.026*** | 29.630*** |
| | (-6.199) | (3.993) | [<0.001] |
| Extrapolation bias | 0.010 | -0.020*** | 6.096** |
| | (1.569) | (-3.066) | [0.014] |
| Within $R^2$ | 0.2708 | 0.2708 | 0.2708 |
| Baseline-control interactions | Yes | Yes | Yes |
| Stacked N | 672,228 | 672,228 | 672,228 |

*Notes.* The three products, ordered by the amount of decision authority the investor delegates to the platform, are DIRECT (self-directed mutual fund investing), AUTO (automatic-investment plans, in which the investor selects funds and the platform automates the timing of purchases), and RA (the robo-advisor, in which the algorithm selects funds and manages the portfolio). The dependent variable $use_{i,t,k}$ equals one if investor $i$ holds product $k \in \{\text{DIRECT}, \text{AUTO}, \text{RA}\}$ in month $t$ and zero otherwise; every investor-month is stacked into three rows, one per product alternative. Products may be held simultaneously, so this is a stacked multivariate linear probability model rather than a mutually exclusive multinomial-choice model. Two cumulative-authority indicators encode the stacking: $c_T$ equals one for AUTO and RA and zero for DIRECT; $c_S$ equals one for RA and zero otherwise. With entity fixed effects on the investor-month absorbing each investor-month's mean use rate, the coefficient $\gamma_T$ on $X \cdot c_T$ identifies how covariate $X$ shifts the investor across the DIRECT-to-AUTO step, and $\gamma_S$ on $X \cdot c_S$ identifies the additional shift across the AUTO-to-RA step, where fund choice itself is surrendered). The Wald column tests $H_0 : \gamma_T = \gamma_S$ from the clustered covariance matrix. Type measures are per-investor averages over the six months immediately preceding the investor's first AUTO or RA use, requiring at least three non-missing pre-use months for every type variable. Baseline controls (Log $\text{TFA}_{t-1}$, risk preference, age, female indicator, baby-asset $\text{holdings}_{t-1}$), each interacted with $c_T$ and $c_S$, are included in every specification but suppressed for brevity; those estimates, the product-step intercepts, and the underlying standard errors are reported in Table A.10. Estimation is with entity fixed effects on the investor-month; standard errors are two-way clustered on investor and month. The identifying-only sample drops investor-months in which the investor is on the same product across all three stacked rows. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed).

Table 5: Bias Trajectories over Robo-Advisor Tenure: Selective Learning

*Panel A: Rolling 6-month trailing mean of bias (headline)*

| | Focal: predicted decline | | Placebo: predicted null | | |
|---|---|---|---|---|---|
| Regressor | Under-div. (uhh) | Turnover (tov) | Anchoring (anc) | Extrap. (ext) | Confirm. (con) |
| *Within-adopter sample* | | | | | |
| $\tau$ | $-0.0013^{***}$ | $-0.0029^{***}$ | $+0.0009^{***}$ | $+0.0002$ | $-0.0005^{**}$ |
| | $(-6.53)$ | $(-11.23)$ | $(+3.16)$ | $(+0.36)$ | $(-2.35)$ |
| $\tau\times$ High Under-div. | $-0.0109^{***}$ | $-0.0050^{***}$ | $+0.0014^{***}$ | $+0.0012^{***}$ | $-0.0001$ |
| | $(-20.22)$ | $(-7.48)$ | $(+2.87)$ | $(+3.15)$ | $(-0.24)$ |
| $\tau\times$ High Turnover | $-0.0029^{***}$ | $-0.0157^{***}$ | $+0.0010^{**}$ | $+0.0004$ | $+0.0017^{***}$ |
| | $(-7.77)$ | $(-15.89)$ | $(+2.10)$ | $(+1.26)$ | $(+3.22)$ |
| $\tau\times$ AOT | $-0.0056^{***}$ | $-0.0103^{***}$ | $+0.0008^{**}$ | $+0.0005^{*}$ | $+0.0010^{**}$ |
| | $(-16.66)$ | $(-12.74)$ | $(+2.27)$ | $(+1.81)$ | $(+2.41)$ |
| *Difference-in-differences with matched never-adopters* | | | | | |
| Adopter $\times\ \tau$ | $-0.0008^{***}$ | $-0.0018^{***}$ | $+0.0015^{***}$ | $+0.0011^{***}$ | $-0.0002$ |
| | $(-6.20)$ | $(-6.83)$ | $(+6.89)$ | $(+3.81)$ | $(-1.20)$ |
| Adopter $\times\ \tau\ \times$ Engagement | $-0.0002^{***}$ | $-0.0006^{***}$ | $+0.0001$ | $+0.0001$ | $+0.0003^{***}$ |
| | $(-3.07)$ | $(-6.68)$ | $(+0.84)$ | $(+0.75)$ | $(+3.34)$ |
| Adopter $\times\ \tau\ \times$ High Under-div. | $-0.0109^{***}$ | $-0.0052^{***}$ | $+0.0013^{***}$ | $+0.0011^{***}$ | $-0.0001$ |
| | $(-20.49)$ | $(-7.80)$ | $(+2.69)$ | $(+2.90)$ | $(-0.15)$ |
| Adopter $\times\ \tau\ \times$ High Turnover | $-0.0029^{***}$ | $-0.0157^{***}$ | $+0.0010^{**}$ | $+0.0004$ | $+0.0017^{***}$ |
| | $(-7.69)$ | $(-15.95)$ | $(+2.12)$ | $(+1.25)$ | $(+3.23)$ |
| Adopter $\times\ \tau\ \times$ AOT | $-0.0057^{***}$ | $-0.0104^{***}$ | $+0.0008^{**}$ | $+0.0005^{*}$ | $+0.0011^{**}$ |
| | $(-16.70)$ | $(-12.92)$ | $(+2.19)$ | $(+1.67)$ | $(+2.45)$ |
| Observations (adopter-only) | 1,321,291 | 1,321,291 | 1,321,291 | 1,321,291 | 1,150,704 |
| Observations (DiD sample) | 1,847,939 | 1,847,939 | 1,847,939 | 1,847,939 | 1,611,731 |

*Notes for Panel A.* The table reports two-way fixed-effects panel regressions of an investor-month behavioural bias measure on a tenure variable and its interactions. The dependent variable is the rolling six-month trailing mean of the bias measure ending at month $t-1$, so the month-$t$ realisation is constructed entirely from history. Every regression includes investor and calendar-month fixed effects; standard errors are two-way clustered by investor and calendar month. The two focal biases, under-diversification (uhh) and turnover (tov), proxy for active opinion-taking and are predicted to decline as robo-advisor experience accumulates. The three clean placebos, anchoring (anc), extrapolation (ext), and confirmation (con), capture biases that do not require active position-formation and are predicted to be null. Additional bias measures (attention, disposition, gambling, herding) are reported in the Online Appendix. *Variable definitions.* $\tau$ is months since first robo-advisor adoption, capped at 24. Adopter equals one for investors who ever hold the robo-advisor. High Under-div. and High Turnover are indicators equal to one for adopters whose six-month pre-adoption mean of uhh (respectively tov) falls in the top decile of the eligible-adopter distribution; AOT (active opinion-taker) equals one if either high-decile indicator is one. Engagement is the investor's lagged 12-month login count, standardised. Never-adopters receive a pseudo-adoption date drawn from the empirical distribution of actual adopter adoption dates so that the calendar distribution of $\tau$ is comparable across treated and control samples. All specifications include one-month-lagged log of one plus total financial assets, lagged cash-management asset holdings, and lagged standardised login count as level controls; coefficients on these controls are omitted from the table for brevity.

*Reading the falsification.* The triple Adopter $\times\ \tau\ \times$ Engagement is the cleanest discriminator. It is negative and significant for the focal pair ($t = -3.07$ for uhh and $t = -6.68$ for tov) and statistically indistinguishable from zero for all three placebos. The within-adopter and DiD heterogeneity rows are numerically near-identical by construction, because the high-decile indicators are zero for never-adopters and the DiD triple interaction therefore reduces to the corresponding double interaction estimated on the adopter sample; both rows are reported because they arise from different estimating samples. Significance levels are based on standard-normal critical values applied to cluster-robust $t$-statistics, reported in parentheses below coefficient estimates: *** $|t| \geq 2.576$ ($p < 0.01$); ** $|t| \geq 1.960$ ($p < 0.05$); * $|t| \geq 1.645$ ($p < 0.10$); two-sided.

*Panel B: Raw monthly bias (robustness)*

| | Focal: predicted decline | | Placebo: predicted null | | |
|---|---|---|---|---|---|
| Regressor | Under-div. (uhh) | Turnover (tov) | Anchoring (anc) | Extrap. (ext) | Confirm. (con) |
| *Within-adopter sample* | | | | | |
| $\tau$ | $+0.0001$ | $-0.0003$ | $+0.0007^{**}$ | $-0.0001$ | $-0.0022^{***}$ |
| | $(+0.70)$ | $(-1.15)$ | $(+2.03)$ | $(-0.14)$ | $(-2.75)$ |
| $\tau\times$ High Under-div. | $-0.0058^{***}$ | $-0.0011^{**}$ | $+0.0005$ | $+0.0006$ | $-0.0007$ |
| | $(-14.55)$ | $(-2.19)$ | $(+0.97)$ | $(+1.42)$ | $(-0.73)$ |
| $\tau\times$ High Turnover | $-0.0011^{***}$ | $-0.0062^{***}$ | $+0.0011^{*}$ | $-0.0002$ | $+0.0004$ |

*Continued on next page*

*Table 6 (continued) – Panel B*

| | Focal: predicted decline | | Placebo: predicted null | | |
|---|---|---|---|---|---|
| Regressor | Under-div. (uhh) | Turnover (tov) | Anchoring (anc) | Extrap. (ext) | Confirm. (con) |
| | (−3.49) | (−11.94) | (+1.96) | (−0.40) | (+0.53) |
| $\tau\times$ AOT | $-0.0028^{***}$ | $-0.0038^{***}$ | +0.0006 | +0.0001 | +0.0003 |
| | (−10.06) | (−8.94) | (+1.47) | (+0.24) | (+0.39) |
| *Difference-in-differences with matched never-adopters* | | | | | |
| Adopter $\times\ \tau$ | +0.0002 | $-0.0006^{**}$ | $+0.0010^{***}$ | $+0.0009^{*}$ | −0.0020 |
| | (+1.25) | (−2.55) | (+3.94) | (+1.85) | (−1.56) |
| Adopter $\times\ \tau\times$ Engagement | $-0.0003^{***}$ | $-0.0007^{***}$ | +0.0001 | +0.0001 | +0.0002 |
| | (−4.90) | (−8.33) | (+0.86) | (+1.12) | (+1.19) |
| Adopter $\times\ \tau\times$ High Under-div. | $-0.0059^{***}$ | $-0.0012^{**}$ | +0.0004 | +0.0005 | −0.0009 |
| | (−14.60) | (−2.39) | (+0.86) | (+1.23) | (−0.99) |
| Adopter $\times\ \tau\times$ High Turnover | $-0.0011^{***}$ | $-0.0062^{***}$ | $+0.0011^{**}$ | −0.0002 | +0.0004 |
| | (−3.39) | (−11.95) | (+1.98) | (−0.39) | (+0.49) |
| Adopter $\times\ \tau\times$ AOT | $-0.0028^{***}$ | $-0.0039^{***}$ | +0.0006 | +0.0001 | +0.0002 |
| | (−9.90) | (−8.97) | (+1.43) | (+0.17) | (+0.30) |
| Observations (adopter-only) | 1,274,140 | 1,274,140 | 1,274,140 | 1,274,140 | 1,231,806 |
| Observations (DiD sample) | 1,781,292 | 1,781,292 | 1,781,292 | 1,781,292 | 1,722,690 |

*Notes for Panel B.* The dependent variable is the raw monthly bias measure as observed at month $t$, replacing the rolling six-month trailing mean used in Panel A. The estimator, sample, matched-control construction, variable definitions, controls, and inference are identical to Panel A; see the Panel A notes for full detail. *Reading the smoothing comparison.* The engagement dose-response (Adopter $\times\ \tau\times$ Engagement) and the focal-bias heterogeneity interactions retain their sign and statistical significance for both focal biases when the smoothing window is removed; for the focal pair the engagement coefficient becomes sharper in Panel B ($t = -4.90$ for uhh; $t = -8.33$ for tov) than in Panel A. The simple time term $\tau$ and the DiD baseline term Adopter $\times\ \tau$ for uhh, by contrast, lose significance in the raw monthly construction. The pattern is consistent with learning that operates heterogeneously across investors and is detectable in the smoothed series for the average adopter but only in subsamples for raw monthly snapshots. The clean placebos remain null on the engagement triple in both panels Significance levels follow Panel A: *** $|t| \geq 2.576$ ($p < 0.01$); ** $|t| \geq 1.960$ ($p < 0.05$); * $|t| \geq 1.645$ ($p < 0.10$); two-sided. Cluster-robust $t$-statistics are reported in parentheses below coefficient estimates.

Table 7: Return Predictors of Robo-Advisor Allocation Share

| | Lag 1 month | | Cumulative 3 months | | Cumulative 6 months | |
|---|---|---|---|---|---|---|
| Variable | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) |
| *Panel A: Positive/negative decomposition* | | | | | | |
| *A.1: Raw returns* | | | | | | |
| Fund return$^{+}$ | $-0.026^{***}$ | $0.006^{***}$ | $-0.029^{***}$ | $0.007^{***}$ | $-0.031^{***}$ | $0.009^{***}$ |
| | $(-14.711)$ | (2.708) | $(-14.876)$ | (3.140) | $(-16.606)$ | (3.228) |
| Fund return$^{-}$ | $-0.024^{***}$ | $0.007^{***}$ | $-0.024^{***}$ | $0.006^{***}$ | $-0.027^{***}$ | $0.008^{***}$ |
| | $(-9.180)$ | (4.954) | $(-8.431)$ | (3.434) | $(-8.098)$ | (3.406) |
| RA return$^{+}$ | | $0.006^{*}$ | | $0.008^{***}$ | | $0.009^{***}$ |
| | | (1.941) | | (2.822) | | (3.121) |
| RA return$^{-}$ | | 0.001 | | 0.005 | | $0.006^{**}$ |
| | | (0.361) | | (1.625) | | (2.373) |
| Inverse Mills ratio | | $0.119^{***}$ | | $0.119^{***}$ | | $0.098^{***}$ |
| | | (2.631) | | (3.347) | | (4.241) |
| $\bar{N}$ (uncensored) | | 27,968 | | 27,234 | | 25,324 |
| $N$ (selection) | 86,647 | | 86,973 | | 86,378 | |
| Pseudo-$R^2$ / $R^2$ | 0.0589 | 0.1116 | 0.0596 | 0.1133 | 0.0596 | 0.1117 |
| *A.2: Excess returns over CSI 300* | | | | | | |
| Fund return$^{+}$ | $-0.022^{***}$ | $0.008^{***}$ | $-0.024^{***}$ | $0.008^{***}$ | $-0.026^{***}$ | $0.011^{***}$ |
| | $(-8.806)$ | (4.592) | $(-9.418)$ | (6.203) | $(-10.089)$ | (3.525) |
| Fund return$^{-}$ | $-0.024^{***}$ | $0.012^{***}$ | $-0.029^{***}$ | $0.011^{***}$ | $-0.029^{***}$ | $0.010^{***}$ |
| | $(-8.763)$ | (5.485) | $(-9.280)$ | (5.521) | $(-8.983)$ | (5.597) |
| RA return$^{+}$ | | $-0.001$ | | $-0.004$ | | $-0.005$ |
| | | $(-0.256)$ | | $(-1.535)$ | | $(-1.499)$ |
| RA return$^{-}$ | | $-0.002$ | | $-0.003$ | | $-0.002$ |
| | | $(-0.677)$ | | $(-1.467)$ | | $(-0.842)$ |
| Inverse Mills ratio | | $0.134^{***}$ | | $0.144^{***}$ | | $0.113^{***}$ |
| | | (2.924) | | (4.136) | | (4.921) |
| $\bar{N}$ (uncensored) | | 28,247 | | 26,914 | | 25,312 |
| $N$ (selection) | 86,691 | | 86,507 | | 86,166 | |
| Pseudo-$R^2$ / $R^2$ | 0.0583 | 0.1115 | 0.0601 | 0.1138 | 0.0594 | 0.1115 |
| *Panel B: Above-median indicator* | | | | | | |
| *B.1: Raw returns* | | | | | | |
| Fund: above median | 0.002 | $-0.001$ | 0.003 | 0.002 | 0.000 | 0.002 |
| | (0.566) | $(-0.176)$ | (0.542) | (0.532) | (0.044) | (0.411) |
| RA: above median | | 0.001 | | 0.001 | | $-0.003$ |
| | | (0.195) | | (0.375) | | $(-0.548)$ |
| Inverse Mills ratio | | $0.109^{**}$ | | $0.111^{***}$ | | $0.092^{***}$ |
| | | (2.424) | | (2.954) | | (3.741) |
| $\bar{N}$ (uncensored) | | 28,008 | | 27,234 | | 25,324 |
| $N$ (selection) | 86,719 | | 86,973 | | 86,378 | |
| Pseudo-$R^2$ / $R^2$ | 0.0560 | 0.1016 | 0.0566 | 0.1033 | 0.0563 | 0.1022 |
| *B.2: Excess returns over CSI 300* | | | | | | |
| Fund: above median | 0.002 | $-0.001$ | 0.003 | 0.002 | 0.002 | 0.001 |
| | (0.533) | $(-0.182)$ | (0.670) | (0.495) | (0.220) | (0.321) |
| RA: above median | | 0.001 | | 0.001 | | $-0.001$ |
| | | (0.220) | | (0.173) | | $(-0.276)$ |
| Inverse Mills ratio | | $0.110^{**}$ | | $0.110^{***}$ | | $0.091^{***}$ |
| | | (2.425) | | (2.978) | | (3.613) |
| N | 86,515 | 27,995 | 86,762 | 27,221 | 86,166 | 25,312 |
| Pseudo-$R^2$ / $R^2$ | 0.0560 | 0.1012 | 0.0566 | 0.1023 | 0.0563 | 0.1018 |
| Baseline controls | | | Yes | | | |
| Province dummies | | | Yes | | | |

*Notes.* Stage-1 coefficients are reported as approximate average marginal effects on the robo-advisor adoption probability, $\mathrm{ME}_k = \phi(\Phi^{-1}(\bar{P}))\hat{\beta}_k$ with $\bar{P} \approx 0.323$ (average monthly RA-active rate) and scaling factor $0.359$; $t$-statistics and significance are invariant to the scaling. Estimation uses Fama and MacBeth (1973) monthly cross-sectional Heckman two-step regressions; the reported coefficient is the time-series mean of the monthly estimates and the $t$-statistic is the ratio of that mean to its Newey–West HAC standard error with lag truncation $\min(3, T-1)$. The within $R^2$ is the plain $R^2$ averaged across monthly Stage 2 regressions; the pseudo-$R^2$ is the McFadden pseudo-$R^2$ averaged across monthly Stage 1 probits. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed).

Table 8: Pre-RA Experience and Robo-Advisor Allocation over Tenure

| Variable | Interaction: linear OLS | Interaction: quadratic OLS | Interaction: quadratic Fractional logit |
|---|---|---|---|
| *Pre-RA experience and tenure interactions* | | | |
| Pre-RA experience | -0.196*** | -0.196*** | -1.318*** |
| | (-2.622) | (-2.884) | (-3.530) |
| Pre-RA experience × tenure | -0.023* | -0.043** | -0.304** |
| | (-1.912) | (-1.972) | (-2.247) |
| Pre-RA experience × tenure$^2$ | — | -0.013 | -0.071 |
| | | (-1.232) | (-1.248) |
| *Tenure (main effects)* | | | |
| Tenure (within-month demeaned) | -0.028** | -0.048** | -0.330** |
| | (-2.194) | (-2.109) | (-2.394) |
| Tenure$^2$ | -0.000 | -0.013 | -0.073 |
| | (-0.390) | (-1.267) | (-1.268) |
| Province Dummies | Yes | Yes | Yes |
| N | 1,097,096 | 1,097,096 | 1,097,096 |
| $R^2$ | 0.162 | 0.163 | 0.190 |

*Notes.* The sample consists of investor-months in which the investor holds positive robo-advisor assets (the adopter-only subsample). The dependent variable $rsh_{i,t} = ra_assets_{i,t}/(ra_assets_{i,t} + mf_assets_{i,t})$ is the share of risky-asset wallet allocated to the robo-advisor and is bounded in $[0, 1]$. Because the analysis is conducted within the adopter sample, the Heckman selection correction used elsewhere in the paper is not applied here.

*Pre-RA experience* is the investor-level average of *exp_fund_months* over the $L_{\text{PRE}} = 6$ months immediately before the investor's first robo-advisor month, requiring at least three non-missing months; the quantity is z-scored once across eligible adopters and is therefore time-invariant for each investor. *Tenure* is the cumulative count of robo-advisor- active months for the investor (gap months do not accrue), demeaned within each calendar month to absorb time effects; *Tenure*$^2$ is the demeaned tenure squared. The interaction *Pre-RA experience* × *Tenure* is the focal regressor; the reclaiming-control hypothesis predicts a negative coefficient on this term, meaning that adopters with more pre-RA fund-investing experience reduce their robo-advisor share of wallet as robo-advisor tenure accumulates.

Estimation uses Fama and MacBeth (1973) monthly cross-sectional regressions. The reported coefficient is the time-series mean of the monthly cross-sectional estimates; the reported $t$-statistic is the ratio of that mean to its Newey-West HAC standard error with lag truncation $\min(3, T-1)$. All three columns include *Tenure* and *Tenure*$^2$ as main effects. The columns differ in the skill-tenure interaction specification. Column (1) includes only the linear interaction *Pre-RA experience* × *Tenure*; columns (2) and (3) include both the linear interaction and the quadratic interaction *Pre-RA experience* × *Tenure*$^2$. Columns (1) and (2) are estimated by ordinary least squares; column (3) replaces OLS with a fractional-logit generalised linear model (binomial family, logit link; Papke and Wooldridge, 1996) to accommodate the bounded support of the dependent variable. Coefficients in column (3) are on the logit scale and are not directly comparable in magnitude to the OLS coefficients in columns (1) and (2). The $R^2$ reported for columns (1) and (2) is the plain $R^2$ averaged over monthly cross-sectional regressions; for column (3) it is the McFadden pseudo-$R^2$ ($1 -$ deviance/null deviance) averaged across months and is not comparable to the OLS values. The total-observations row reports the sum of monthly cross-sectional sample sizes across the $T = 44$ months in which a specification clears the pre-flight checks (degrees of freedom, finite design matrix, full column rank, condition number $< 10^{12}$, and GLM convergence for column (3)).

Baseline controls (log total financial assets$_{t-1}$, stated risk preference, age, female indicator, combined-advisor dummy$_{t-1}$, cash-management assets$_{t-1}$, months-since-registration$_{t-1}$, and province dummies) are included in every regression but suppressed for brevity; full estimates are reported in the Online Appendix.

Newey-West HAC $t$-statistics in parentheses below coefficient estimates. Significance levels (against standard-normal critical values for the Fama-MacBeth time-series mean): *** $|t| \geq 2.576$ ($p < 0.01$); ** $|t| \geq 1.960$ ($p < 0.05$); * $|t| \geq 1.645$ ($p < 0.10$); two-sided.

Table 9: Robo-Advisor Dose-Response on the Self-Directed Mutual-Fund Sleeve

| | (1) Holds MF (LPM) | (2) Concentration (HHI) | (3) No. funds | (4) Stock share | (5) Risky share | (6) Turnover |
|---|---|---|---|---|---|---|
| *RA-share bins at $t-1$ (Bin 0: $rsh_{t-1}=0$ omitted as reference)* | | | | | | |
| Bin 1 (0–0.20] | 0.005*** | -0.216*** | 0.196*** | -0.001 | -0.001 | -0.105*** |
| | (7.188) | (-50.801) | (27.425) | (-0.342) | (-0.148) | (-18.416) |
| Bin 2 (0.20–0.40] | 0.005*** | -0.182*** | 0.052*** | 0.025*** | 0.018* | -0.035*** |
| | (7.496) | (-30.097) | (13.509) | (3.551) | (1.943) | (-6.684) |
| Bin 3 (0.40–0.60] | 0.000 | -0.023*** | -0.026*** | 0.032*** | 0.013 | 0.117*** |
| | (0.360) | (-2.943) | (-5.953) | (2.653) | (0.854) | (14.131) |
| Bin 4 (0.60–0.80] | -0.016*** | 0.117*** | -0.084*** | 0.060*** | 0.016 | 0.299*** |
| | (-9.199) | (11.166) | (-13.206) | (4.173) | (1.001) | (23.017) |
| Bin 5 (0.80–1.00] | -0.332*** | 0.396*** | -0.122*** | 0.008 | -0.170*** | 0.805*** |
| | (-19.380) | (26.236) | (-16.846) | (0.462) | (-6.842) | (36.967) |
| log(TFA), $t-1$ | 0.015*** | -0.454*** | 0.261*** | -0.071*** | 0.108*** | -0.549*** |
| | (18.403) | (-34.248) | (23.040) | (-10.161) | (10.708) | (-61.998) |
| Baby assets, $t-1$ | -0.002*** | 0.061*** | -0.035*** | -0.012*** | -0.039*** | 0.124*** |
| | (-11.430) | (30.271) | (-22.460) | (-4.065) | (-17.600) | (49.691) |
| Combined-advisor dummy, $t-1$ | -0.001 | -0.080*** | 0.187*** | -0.026*** | -0.009 | -0.035*** |
| | (-1.281) | (-12.568) | (19.274) | (-3.596) | (-1.258) | (-6.584) |
| Investor FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Full | MF active | MF active | MF active | MF active | MF active |
| $N$ | 4,492,147 | 4,325,040 | 4,325,040 | 4,339,137 | 4,339,137 | 4,075,169 |
| Investors | 100,000 | 100,000 | 100,000 | 100,000 | 100,000 | 98,067 |
| Within $R^2$ | 0.102 | 0.125 | 0.114 | 0.003 | 0.004 | 0.108 |

*Notes.* The table reports two-way fixed-effects panel regressions (`PanelOLS` with investor and month effects) of self-directed mutual-fund (MF) sleeve outcomes on lag-1 robo-advisor (RA) intensity. RA intensity is $rsh_{i,t-1} = RA_{i,t-1}/(RA_{i,t-1} + MF_{i,t-1})$, binned into six categories; the no-RA baseline (Bin 0, $rsh_{t-1}=0$, which includes never-adopters and pre-adoption months, 65.0% of rows) is the omitted reference. Each bin coefficient is the within-investor change in the period-$t$ outcome relative to Bin 0. The five non-zero bins hold 26.8%, 4.1%, 1.8%, 0.9%, and 1.4% of panel rows respectively. Outcomes: column (1) is a linear probability model for holding any mutual fund (*mf_active*); columns (2)–(6) are estimated on the MF-active subsample. Concentration is the within-MF Herfindahl index; No. funds is the count of distinct funds held; Stock share is (stock + index + mixed) over total MF; Risky share is one minus the money-market fraction; Turnover is the active-trading proxy. Outcomes (2)–(6) are winsorised at 1/99 and z-scored within month, so coefficients are in within-month standard-deviation units. Controls (log total financial assets$_{t-1}$, cash-management assets$_{t-1}$, both winsorised and z-scored within month, and a combined-advisor dummy$_{t-1}$) are reported. Investor demographics, skills, and province are absorbed by investor fixed effects. Standard errors are two-way clustered on investor and month; clustered $t$-statistics in parentheses. *Interpretation.* The treatment is lagged one month, which guards against contemporaneous reverse causation but not joint within-month determination of RA intensity and MF behaviour. The estimates describe within-investor co-movement across the RA-share dose, not a causal effect of RA usage. Secondary outcomes (auto-invest share, gambling bias, disposition effect) are reported in the Online Appendix. Stars: *** $p<0.01$, ** $p<0.05$, * $p<0.10$ (two-tailed).

Table 10: Return Predictors of Robo-Advisor Allocation across Adoption-Lifecycle Margins

| | Lag 1 month | | Cumulative 3 months | | Cumulative 6 months | |
|---|---|---|---|---|---|---|
| Variable | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) |
| *Panel A: First adoption* | | | | | | |
| Fund return$^+$ | -0.003*** | 0.004 | -0.003*** | 0.009 | -0.002*** | 0.024** |
| | (-7.014) | (0.525) | (-5.572) | (1.574) | (-5.725) | (2.058) |
| Fund return$^-$ | -0.006*** | -0.001 | -0.006*** | 0.014 | -0.006*** | 0.042** |
| | (-5.878) | (-0.046) | (-5.765) | (1.254) | (-5.864) | (2.354) |
| RA return$^+$ | | | | | | |
| RA return$^-$ | | | | | | |
| Inverse Mills ratio | | 0.096 | | -0.124 | | -0.432** |
| | | (0.464) | | (-1.029) | | (-2.433) |
| N | 1,704,054 | 54,568 | 1,717,153 | 48,672 | 1,715,065 | 46,920 |
| Pseudo-$R^2$ / $R^2$ | 0.033 | 0.184 | 0.030 | 0.188 | 0.030 | 0.193 |
| *Panel B: Exit (probit-only design; marginal effects)* | | | | | | |
| Fund return$^+$ | 0.001*** | | 0.002*** | | 0.003*** | |
| | (2.783) | | (4.703) | | (8.644) | |
| Fund return$^-$ | -0.002*** | | -0.002*** | | -0.001** | |
| | (-3.017) | | (-2.912) | | (-2.306) | |
| RA return$^+$ | 0.000 | | -0.000 | | 0.000 | |
| | (0.777) | | (-0.562) | | (0.262) | |
| RA return$^-$ | 0.002*** | | 0.001 | | 0.001 | |
| | (2.928) | | (0.688) | | (0.986) | |
| $N$ (selection) | 1,263,735 | | 1,177,032 | | 1,118,619 | |
| Pseudo-$R^2$ | 0.025 | | 0.026 | | 0.026 | |
| *Panel C: Continuation* | | | | | | |
| Fund return$^+$ | -0.003** | 0.012*** | -0.004*** | 0.012*** | -0.006*** | 0.014*** |
| | (-2.375) | (10.882) | (-3.291) | (8.562) | (-8.449) | (8.265) |
| Fund return$^-$ | 0.004*** | 0.012*** | 0.004*** | 0.012*** | 0.003** | 0.012*** |
| | (3.130) | (10.702) | (3.024) | (9.150) | (2.290) | (6.168) |
| RA return$^+$ | | 0.007** | | 0.009*** | | 0.009*** |
| | | (2.322) | | (2.931) | | (3.634) |
| RA return$^-$ | | 0.002 | | 0.005 | | 0.007** |
| | | (0.501) | | (1.629) | | (2.370) |
| Inverse Mills ratio | | 0.445*** | | 0.539*** | | 0.411** |
| | | (3.296) | | (3.984) | | (2.511) |
| N | 1,412,944 | 1,286,824 | 1,444,456 | 1,316,070 | 1,406,508 | 1,124,192 |
| Pseudo-$R^2$ / $R^2$ | 0.020 | 0.108 | 0.021 | 0.111 | 0.020 | 0.110 |
| Baseline controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Province dummies | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes.* Estimates are from unweighted Heckman two-step (Tobit-2) models estimated monthly and aggregated by Fama–MacBeth. The Stage 1 selection equation is a probit on the relevant participation margin; the Stage 2 outcome equation is an OLS of the robo-advisor allocation share $ra_share_{i,t} = RA_{i,t}/(RA_{i,t} + MF_{i,t})$ on the uncensored subsample with the inverse Mills ratio included. The selection margin differs by panel: in Panel A, $D_{i,t} = 1$ in the investor's first robo-advisor month and 0 in all prior months (post-adoption months excluded); in Panel B (probit-only), $D_{i,t} = 1$ if the investor held RA in $t-1$ and not in $t$; in Panel C, $D_{i,t} = 1$ if the investor held RA in both $t-1$ and $t$, 0 if the investor held in $t-1$ but exited in $t$. Each return $r$ is decomposed into its positive part $r^+ = \max(r, 0)$ and negative part $r^- = \max(-r, 0)$, both winsorised at 1/99 and z-scored within month, and entered jointly. The fund (Fund) and robo-advisor (RA) raw monthly returns enter the same regression. All Stage-1 coefficients are reported as approximate average marginal effects on the relevant participation probability, $\text{ME}_k = \phi(\Phi^{-1}(\bar{P}))\hat{\beta}_k$, where $\bar{P}$ is the average monthly selection rate in each design and $\hat{\beta}_k$ is the Fama–MacBeth average probit coefficient; $t$-statistics and significance are invariant to this scaling. The scaling factor is $0.072$ in Panel A (first adoption, $\bar{P} \approx 0.032$), $0.061$ in Panel B (exit, $\bar{P} \approx 0.027$), and $0.161$ in Panel C (continuation, $\bar{P} \approx 0.911$). Because continuation is a near-ceiling event ($\bar{P}$ close to one), its Panel C marginal effects describe the response of the marginal would-be exiter and should be read with that caveat; the underlying probit coefficients are correspondingly larger. Baseline controls (log total financial assets$_{t-1}$, stated risk preference, age, female indicator, combined-advisor dummy$_{t-1}$, cash-management assets$_{t-1}$, and province dummies) are included and suppressed in both stages. Months-since-registration ($t-1$) enters Stage 1 only and is the exclusion restriction. $\bar{N}$ (uncensored) is the average number of RA-adopter months per cross-section in Stage 2; $N$ (selection) is the total number of at-risk investor-months in Stage 1. In the first-adoption (Panel A) and continuation (Panel C) designs the recovered $\hat{\rho} = \hat{\lambda}/\hat{\sigma}$ exceeds one in magnitude in several months, because $\hat{\lambda}$ is large relative to the within-month residual standard deviation; the inverse Mills ratio should be read as a Heckman selection diagnostic rather than a strict correlation in those panels. Newey–West HAC $t$-statistics (lag = $\min(3, T-1)$) in parentheses. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed).

# A. Online Appendix

This appendix collects supplementary material referenced in the main text. It serves three purposes. First, it documents sample construction, estimation choices, and variable construction details that are too long for the main text but are necessary for evaluating the empirical design. Second, it reports robustness checks for the two-margin Heckman estimates, the authority ladder, the reclaiming-control specification, performance predictors, and the post-adoption bias-trajectory evidence. Third, it records auxiliary evidence on external validity, age-bias correlations, and the structure of the self-directed sleeve. The appendix is descriptive and interpretive in the same sense as the main text: the estimates document revealed demand patterns and mechanism-consistent heterogeneity, but they are not presented as causal treatment effects of robo-advising.

## A.1. External Validity and Single-Platform Representativeness

The analysis is conducted on a single platform in one country, which raises a natural external-validity concern. We do not use the Alipay setting to estimate a universal adoption level for robo-advising. The levels of adoption, the age profile of users, and the role of platform discovery are likely to differ in Western brokerage markets, retirement-plan markets, and advisory markets. The portable object is instead the two-margin logic. Delegated financial products can have fixed entry costs and marginal authority costs, the same investor characteristic can move the adoption margin and the conditional-delegation margin in opposite directions, and persistence responds to its own forces. Three institutional features make the setting useful for that purpose. First, Alipay is a large mobile payments and wealth-management platform in China, and its account structure records mutual-fund holdings, delegated robo-advisor assets, automatic investment plan activity, cash-management balances, platform engagement, transactions, and returns in one place. This integrated observation reduces the external-holding problem that would arise in a single-brokerage dataset where the researcher observes only one slice of the investor's portfolio. Second, BangNiTou is a fully delegated mutual fund robo-advisor that constructs and rebalances portfolios for the investor. Its economic content is similar to delegated portfolio products in other markets even though its institutional setting is Chinese. Third, automatic investment plans coexist in the same account. They automate scheduled fund purchases but leave fund choice with the investor. The coexistence of self-directed funds, automatic investment plans, and robo-advising is what allows the paper to distinguish execution automation from delegation of fund-selection authority. The limitations are equally important. The Alipay investor base is younger than many Western brokerage populations; the Chinese mutual fund market has its own regulatory and distribution structure; and the super-app environment makes platform tenure and product discovery more central than they may be in a fragmented brokerage environment. We therefore interpret demographic magnitudes and adoption levels as context-specific. The stronger claim is about signs, cross-margin differences, and the authority-versus-automation distinction, not about the level of robo-advisor participation in other markets.

### A.2. Additional Methodological and Estimation Details

The main Heckman estimates are run month by month and then averaged across months. In each monthly cross-section, the first stage is a probit for robo-advisor participation and the second stage is an ordinary least squares regression of the conditional robo-advised share on observed covariates and the inverse Mills ratio. The reported coefficients are Fama–MacBeth averages, and inference uses Newey–West HAC standard errors with maximum lag three. This design keeps the identifying variation cross-sectional, which is appropriate for investor characteristics that move slowly over time, while allowing the slope coefficients to vary across market environments. The monthly estimation pipeline applies several safeguards. We drop regressors that are constant within month, verify the rank of the probit and outcome design matrices, remove variables that perfectly separate the adoption outcome, drop the joint set of province dummies when the effective parameter count approaches the number of adopters, and skip monthly cross-sections in which the probit fails to converge. Province dummies are included as controls when the monthly design matrix supports them, but their individual coefficients are not reported. These guards are mechanical and are applied before interpreting any coefficient. The number of retained months differs across specifications because richer specifications require more within-month variation and because behavioral measures rely on rolling windows that are not populated in the earliest months. The baseline and skills specifications retain 48 and 47 monthly cross-sections, and the bias-augmented specification 36. All continuous regressors are winsorized within month at the first and ninety-ninth percentiles and then standardized within month. Binary indicators are not standardized. Except for variables that are time-invariant or near-invariant, covariates are lagged one month so that they are predetermined relative to the month-$t$ participation and allocation decisions. Returns are never forward filled. Behavioral measures are forward filled across non-trading months only after the investor's first observed trading month, reflecting the premise that a measured behavioral tendency remains informative until new trading behavior updates it. Selection-stage coefficients in tables without interactions are rescaled to approximate marginal effects using the observed event rate for the relevant sample,

$$\mathrm{ME}_k = \phi\big(\Phi^{-1}(\bar{P})\big)\,\hat{\gamma}_k, \tag{15}$$

where $\bar{P}$ is the relevant monthly event rate. The main Heckman uses panel-specific RA-active rates of 0.326 in the baseline panel, 0.344 in the skills panel, and 0.372 in the biases panel, giving scaling factors of 0.360, 0.368, and 0.378. Outcome-stage coefficients are reported in units of the delegated share. In tables with interactions, the selection-stage coefficients are left on their native scale because the marginal effect of an interaction in a probit is not the coefficient on the interaction term and depends on the full covariate vector (Ai & Norton, 2003). The authority-ladder specifications avoid this issue by using a stacked linear probability design with investor-month fixed effects.

Online Appendix Table A.3 provides a benchmark that does not impose a selection correction. It re-estimates the outcome equations by monthly cross-sectional OLS on the same RA-active investor-month samples used in the corresponding panels of Table 2. Neither the inverse Mills ratio nor platform tenure enters these regressions, so the coefficients can be compared directly with the outcome-stage coefficients in the main table.

The purpose of this exercise is narrow. The positive coefficients on underdiversification and total turnover and the negative coefficient on confirmation bias remain when the correction is removed. The focal signs are therefore present in the observed allocation choices of adopters rather than being mechanically introduced by the inverse Mills ratio or the tenure exclusion. Because the regressions condition on RA-active observations, they do not address nonrandom selection into adoption and cannot validate the exclusion restriction. They complement rather than replace the selection-adjusted estimates.

### A.3. Choice-Based Sampling and Unweighted Estimation

The main sample is stratified on ever adoption. The purpose of the stratification is statistical power, not prevalence estimation. First adoption and re-entry are rare monthly events, and the conditional delegation share is observed only when the investor holds robo-advised assets. A uniform random sample of the same size would deliver many non-user months and too few robo-active observations for precise estimation of the conditional-share equation, lifecycle margins, and post-adoption dynamics. The main estimates are unweighted because the object of interest is the slope relationship between investor characteristics and the two margins, conditional on the active mutual-fund investor population, not the platform-level prevalence of robo-advisor use. Population-calibration weights would rescale ever-adopters and never-adopters back toward their population frequencies, but in thin monthly cross-sections they also amplify the influence of rare observations and increase the variance of the Fama–MacBeth slopes. We therefore read the unweighted coefficients as describing the sampled population, and report the weighted estimates as the check on whether the sign pattern is specific to it. We do not assert that the unweighted slopes are consistent for the platform population; establishing that would require a retrospective-sampling argument for a probit selection stage with full-window stratification, which we do not derive. We nevertheless re-estimate the main Heckman specifications with calibrated inverse-probability weights in Table A.1. Within each month, adopter observations receive weight $p_{pop}/p_{samp,t}$ and non-adopter observations $(1 - p_{pop})/(1 - p_{samp,t})$, normalized to mean one, with $p_{pop} = 1/101$. The headline sign patterns are unchanged: fund experience remains a positive participation predictor with a negative conditional-share relation; underdiversification and turnover preserve the negative-adoption, positive-delegation signature; and confirmation preserves the opposite pair of signs. Because the weights center the fitted adoption probability on the population rate, the reported selection marginal effects are evaluated at a one-percent base rate and are not comparable in magnitude with the main table; significance should be read from the $t$-statistics. The inverse Mills coefficient turns negative under weighting, and the selection-on-unobservables discussion in the main text therefore rests on the unweighted panel. The weighted estimates support the conclusion that the main sign findings are not artifacts of the unweighted stratified design.

### A.4. Variable Construction Details

This section records the construction of the behavioral and skill measures used in the main text. The measures are designed to capture distinct investment patterns, not a single behavioral type. Whenever possible, the measure is constructed from information available before the decision month and avoids mechanical overlap with the outcome being explained. Underdiversification is measured primarily by

the Herfindahl index of mutual fund portfolio weights,

$$\text{HHI}_{it} = \sum_{f} w_{ift}^2,$$

where $w_{ift}$ is investor $i$'s end-of-month weight in fund $f$. Higher HHI means a more concentrated fund portfolio. We also compute related diversification measures for robustness, including the effective number of funds, $1/\text{HHI}_{it}$, pairwise return-correlation measures, and a risk-adjusted diversification ratio. HHI is the main text measure because it is defined whenever the investor holds at least one mutual fund, whereas correlation-based measures require multiple holdings with sufficient return history. Total turnover is an active-trading intensity measure. It is computed from monthly buys and sells relative to average holdings between $t-1$ and $t$, using a Modified-Dietz-style denominator. Speculative turnover isolates a narrower form of active trading. A trade is classified as speculative when the investor held the fund at $t-1$, the position had a positive lagged one-month return, the investor reduced the position in month $t$, and the investor repurchased either the same fund or a fund of the same type within one month. The same-type version is the primary measure because same-fund round trips are rare in mutual fund data; the strict same-fund version is used as a robustness variant. Confirmation bias is the capped retention rate of lagged winner positions. We first identify positions held at the end of $t-1$ whose trailing twelve-month return was positive. For each such position, retention is the fraction of the $t-1$ value still held at $t$, capped at one so that new purchases do not inflate the measure. The investor-level confirmation-bias measure is the value-weighted average retention rate across lagged winners. Disconfirmation bias is constructed analogously from lagged loser positions as an exit rate. Because winners and losers are disjoint sets, the two measures capture different realized-holding responses. The disposition effect follows the standard PGR-minus-PLR logic. PGR is the proportion of gains realized and PLR is the proportion of losses realized. The position panel is rooted in holdings observed at $t-1$ and then follows the investor's decision during month $t$. This timing matters because complete exits between $t-1$ and $t$ are invisible if the measure is constructed only from end-of-month-$t$ holdings. Attention bias is the share of trading value directed to salient funds. Salience flags include funds with extreme trailing twelve-month returns, funds with top-decile current-month returns within style or across the fund universe, and newly launched funds. We exclude top-Sharpe and low-volatility flags from the primary measure because those variables can reflect informed attention rather than limited attention. Extrapolation bias is the share of trading value directed to funds whose lagged one-month return lies in the top decile, computed within Morningstar style and across the fund universe and combined through the union of the two criteria. The lagged construction is important because contemporaneous returns can be mechanically affected by flows. Gambling bias adapts the lottery-stock logic to mutual funds. A fund is lottery-like when it has low net asset value, high idiosyncratic volatility, and high idiosyncratic skewness, measured using lagged return information. Because diversified mutual funds rarely satisfy all three criteria at once, the primary measure uses an any-two-of-three rule and the strict triple-condition version is retained for robustness. Anchoring captures trading near prior price extremes. Anchored sells are sales of funds whose lagged price is close to the previous twelve-month high. Anchored buys are purchases of funds whose lagged price is close to the previous twelve-month low. The two directional ratios are combined as their mean

in the main measure. Herding is the share of the investor's trading value directed to funds with high lagged aggregate flows. Aggregate flow is computed with a leave-one-out correction so that investor $i$'s own trading does not mechanically enter the fund-level flow signal used to classify her behavior. Selection ability and timing ability decompose realized mutual-fund performance into two pieces. Selection ability is the value-weighted excess return of the investor's held funds relative to Morningstar category benchmarks. Timing ability captures active profit and loss associated with within-month position changes, orthogonal to the passive return on beginning-of-month holdings. Fund experience is the cumulative number of months in which the investor has active mutual fund holdings. Experience is therefore a stock of market participation, not a realized-performance measure.

### A.5. Full Specification and Alternative Intensive Outcomes

Online Appendix Table A.4 reports the joint Heckman specification with demographics, skills, and all behavioral variables included simultaneously. This specification is not the main table because the rolling-window construction of the behavioral variables restricts the retained month sample and because the nested specifications in the main text make clear which block absorbs which relation. The joint table is nevertheless the most direct check that the headline sign patterns are not artifacts of estimating skills and biases in separate panels. The three strongest behavioral sign patterns survive jointly: underdiversification remains negative on adoption, at $-0.065$, and positive on conditional delegation, at $+0.016$; turnover remains negative on adoption, at $-0.028$, and positive on conditional delegation, at $+0.039$; and confirmation remains positive on adoption, at $+0.019$, and negative on conditional delegation, at $-0.011$. Fund experience retains a positive participation coefficient, at $+0.024$, but its conditional-share coefficient attenuates to $-0.005$ once biases are included jointly. The interpretation in the main text follows from this pattern. Experience is robustly related to participation, while part of its conditional-share relation is mediated by behavioral correlates of experience. The inverse Mills ratio remains positive in the joint specification, at $+0.277$, so the paper treats selection on unobservables as an economic object rather than a nuisance term fully absorbed by the observed controls. Online Appendix Table A.5 reports alternative intensive-margin outcomes. Log RA assets capture absolute delegated scale, while RA assets over total financial assets capture delegation relative to the full on-platform balance sheet. These alternatives check whether the main results are mechanical consequences of the RA-plus-mutual-fund denominator in the main outcome. The core sign patterns are preserved across the alternative outcomes: turnover and underdiversification are positive on both alternatives, and confirmation negative on both. The inverse Mills coefficient changes across outcomes, however. It is positive on the main compositional share, smaller and only marginally significant on the total-asset share, and strongly negative on the log-level outcome. The pattern locates the channel, since the positive selection channel is strongest when delegation is measured as a composition of the investor's fund holdings instead of as the absolute level of assets inside the robo-advisor.

### A.6. Panel Fixed-Effects Control-Function Estimates

The main estimates use a month-by-month Heckman with Fama–MacBeth aggregation and Newey–West inference. A natural question is whether the behavioral cross-margin signs depend on that estimator or on its cross-sectional identification. Online Appendix Table A.6 re-estimates the conditional-

share equation with a completely different design: the control-function panel selection estimator of Wooldridge (1995) and Semykina and Wooldridge (2010). The first stage forms the inverse Mills ratio from per-month probits augmented with Mundlak per-investor time averages and the months-since-registration exclusion restriction, and the second stage is a linear panel regression that adds the inverse Mills ratio to investor fixed effects, in one variant, and to investor and month fixed effects, in the other, with two-way clustered standard errors. This swaps Fama–MacBeth cross-sectional averaging for within-investor identification and swaps Newey–West for two-way clustering, so agreement across the two estimators bears directly on whether the signs are an artifact of either choice. The three focal intensive-margin behavioral signs are preserved. In the biases specification, total turnover and underdiversification carry positive and highly significant coefficients on the conditional share, 0.040 and 0.045 with investor fixed effects and 0.041 and 0.032 with investor and month fixed effects, and confirmation bias is negative and highly significant, near $-0.015$ in both variants. Panel B repeats the exercise on the risky-RA-share outcome and recovers the same three signs at similar magnitudes, 0.045 and 0.046 for turnover, 0.052 and 0.038 for underdiversification, and $-0.015$ for confirmation, so the cross-margin behavioral pattern is not specific to the main denominator. The fixed-effects design cannot speak to the demographic and risk-preference gradients, because risk preference, age, and gender are time-invariant within investor and are absorbed by the entity fixed effect, so the table is read as a robustness check on the behavioral intensive-margin signs rather than on the extensive margin or the demographic cross-section. The inverse Mills coefficient is not stably signed across specifications in this design, and because the second stage treats the inverse Mills ratio as observed rather than as a generated regressor, its standard errors understate uncertainty and its $t$-statistics should be read as approximate. We therefore do not use the panel control-function selection term to corroborate the positive selection on unobservables documented in the main Fama–MacBeth tables; the contribution of the table is narrower and cleaner, namely that the behavioral cross-margin signs on the conditional share survive a fixed-effects estimator and two-way clustered inference, and are not produced by the estimator and the inference method of the main specification.

### A.7. AUTO, Authority, and Reclaiming-Control Robustness

Online Appendix Table A.7 reports a stand-alone AUTO Heckman, parallel to the main RA Heckman. AUTO denotes automatic investment plans, which automate purchase timing while preserving investor control over fund selection. The stand-alone AUTO estimates show that AUTO and RA are not the same product at different intensities. Underdiversification is strongly positive on AUTO intensity, at $+0.088$ against $+0.020$ for RA; confirmation is positive on AUTO entry and positive, not negative, on AUTO intensity; turnover is negative on both AUTO margins where it is positive on the RA conditional share; and the AUTO inverse Mills coefficient is negative in the biases specification. The opposite sign of selection on unobservables relative to RA is especially useful for interpretation, since it indicates that latent demand for intensive AUTO use is not the same residual object as latent demand for intensive RA use. Online Appendix Table A.10 reports the authority-ladder robustness checks and the baseline-control interactions suppressed in the main table. The first three rows of each block vary the pre-use horizon over which investor type is measured to three, nine, and twelve months, holding the identifying-only restriction fixed; the fourth row holds the horizon at the headline

six months and instead retains all eligible investor-months. The distinction between the automation step and the full-delegation step is preserved throughout. Engagement, underdiversification, anchoring, and attention reject equality of the two steps at every horizon and in the full panel; total turnover rejects at every horizon the table reports, its $L = 3$ block having not been supplied. The skill composite, marginal on the full-delegation step in the headline six-month window, reaches $+0.016$ at nine and twelve months while its automation-step coefficient turns negative, though its equality test never rejects at five percent. Selection ability is already significant at five percent in the headline window, holds at $+0.013$ at nine months, and weakens to $+0.012$ and ten percent significance at twelve. The sample shrinks as the horizon lengthens, from 766,254 stacked observations at $L = 3$ to 496,575 at $L = 12$, so comparisons across horizons are across partly different investors, and we read the horizon ladder as a stability check rather than as independent evidence. Online Appendix Table A.11 reports reclaiming-control robustness under three alternative adopter-sample restrictions: consecutive RA-months only, no-gap stayers, and dropping the first RA-active month per investor. The baseline specification studies RA-active adopter-months and relates delegated share to RA tenure, pre-RA fund experience, and their interaction. Across these variants, the pre-RA-experience by tenure interaction remains negative, at $-0.036$, $-0.040$, and $-0.036$, and the tenure main effect remains negative at $-0.042$ to $-0.045$. The result should be read as a tenure-share gradient among active adopter months, not as a randomized effect of experience, but its stability supports the interpretation that experienced investors use delegation less persistently.

### A.8. Selective Awareness and Bias-Trajectory Robustness

Online Appendix Table A.12 reports additional bias-trajectory specifications over robo-advisor tenure for attention, the disposition effect, gambling, and herding. These specifications complement the main selective-awareness evidence. The main text emphasizes the behaviors most directly tied to corrective portfolio management, underdiversification and turnover. The appendix expands the trajectory evidence to related behavioral measures and interaction variants. The purpose is not to claim that adoption causally eliminates biases. The purpose is to ask whether the behaviors that generate the negative-adoption, positive-delegation signature move after exposure in a direction consistent with learning from, or adjusting to, the delegated portfolio. The evidence is strongest for underdiversification and turnover. These behaviors decline with robo-advisor tenure, and the decline is more pronounced among investors with high pre-adoption bias or high platform engagement. Engagement is interpreted cautiously. It may raise exposure to portfolio composition and therefore salience, but it may also proxy for sophistication or general financial attention. The appendix therefore treats the interaction as mechanism-consistent heterogeneity, not as unique identification of awareness. The four additional biases behave in ways that support, but do not add to, the focal reading. Attention is effectively null: its adopter-by-tenure term is indistinguishable from zero in both panels, and its engagement triple is significant in the smoothed panel but shrinks to insignificance without the smoothing window, which is what a smoothing artifact looks like. Gambling is not null in the matched design, where the adopter-by-tenure term is $-0.0006$ and $-0.0008$ and significant in both panels, though it is flat within adopters; its engagement triple is positive, so it does not show the focal pattern in any case. The disposition effect declines with tenure in both panels, but the interpretation is mechani-

cal not psychological. The measure is built from investor-initiated realizations on the self-directed book, and delegation moves wealth into a sleeve the investor does not trade, so the denominator of the statistic falls with tenure. Herding rises with tenure in the smoothed panel, at $+0.0004$ in the matched design, which is the opposite of the focal prediction, though the raw monthly counterpart is insignificant and slightly negative; the natural reading of the smoothed result is a tracking channel, in which the investor observes the robo-advisor's portfolio adjustments and partially copies them in her own book. None of the four is a clean placebo, which is why the main table uses anchoring, extrapolation, and confirmation instead. The appendix also clarifies the relation between selective awareness and the other behavioral channels. Selective awareness is the paper-level mechanism for correctable active-opinion mistakes. Attachment and cognitive constraints are signatures inside the behavioral taxonomy. Confirmation-bias patterns are read through attachment to validated positions or narratives. Attention, gambling, and herding are read as constraints or preferences that robo-advising does not directly relax. The trajectory evidence is not expected to be uniform across all biases, because the framework predicts different demand signatures for different behavioral mechanisms.

### A.9. Performance Appendices

Online Appendix Table A.8 adds single fund-performance measures to the RA Heckman. The outcome-stage coefficients are economically very small, with absolute values no larger than 0.003. Category-adjusted fund returns at longer horizons are statistically significant but too small to explain the main cross-margin patterns. The result matters because a pure performance-chasing account would predict recent self-directed fund performance to explain a large part of delegation intensity. It does not. The inverse Mills ratio in these specifications remains positive and significant for the excess-return and category-adjusted arms, between 0.087 and 0.098, consistent with the main tables. Online Appendix Table A.9 adds RA-performance measures. Broad RA excess returns and beta-adjusted returns are small, no larger than 0.005 in absolute value at any horizon, while strategy-adjusted RA returns are positive, at 0.007 to 0.008. The main text interprets this as within-RA strategy selection rather than broad evidence that investors increase RA allocation because the RA channel beats the market. This distinction is important because investors may choose among risk profiles or strategy sleeves within RA even when broad RA performance does not explain whether they delegate more in the first place. Online Appendix Tables A.13, A.14, and A.15 extend the performance analysis to first adoption and lifecycle margins. Before first RA adoption, investor-specific RA returns are mechanically undefined, so the relevant observed signal is recent self-directed fund performance and broader platform-level return information. After adoption, both the self-directed and delegated sleeves can generate realized return signals. The lifecycle return tables therefore separate first adoption, continuation, exit-related prediction, and conditional allocation. Across these exercises, performance feedback matters in some decomposed specifications, especially when positive and negative components are separated, but it does not absorb the main characteristic-based cross-margin patterns. The first-adoption return tables are estimated on short monthly panels, fifteen to twenty-seven cross-sections, and their coefficients take opposite signs between the pre-window and lag-1 hazard constructions, so we treat them as evidence of absence of a strong return channel, not as precise estimates.

### A.10. Dose-Response and Self-Directed-Sleeve Outcomes

Online Appendix Table A.16 complements the main dose-response table by reporting three secondary outcomes: the auto-invest share of fund purchases, gambling bias, and the disposition effect. The omitted group is no lagged RA share, so the coefficients compare investor-months with different lagged RA-share bins to investor-months without active RA exposure, conditional on the fixed effects and controls specified in the table. The auto-invest share rises at low RA intensity, by 0.064 within-month standard deviations in the lowest bin, and turns sharply negative at the top bin, at $-0.103$; the outcome is z-scored within month, so these are not percentage points. Gambling bias and the disposition effect, by contrast, are essentially unmoved by RA intensity: their within-$R^2$ is zero to three decimals and almost every bin coefficient is insignificant. They function as placebo tests. The restructuring of the sleeve documented in the main table, across substitution, concentration, turnover, and risk, is not accompanied by a shift in these underlying biases, which sharpens the reading that the main effects reflect portfolio reorganization rather than a general change in investor type. The pattern in the main table is intentionally described as descriptive. Lower and moderate RA shares are associated with broader and less heavily traded self-directed sleeves. Very high RA shares are associated with a smaller residual self-directed sleeve that can look more concentrated or more actively traded. This non-monotonicity is consistent with the main paper's distinction between entry and intensity. A higher RA share does not simply scale up the same portfolio adjustment. Once a large share of fund wealth is delegated, the remaining self-directed sleeve is a selected residual portfolio, and its composition need not move monotonically with RA intensity.

### A.11. Stability, Age Correlations, and Self-Directed Sleeve

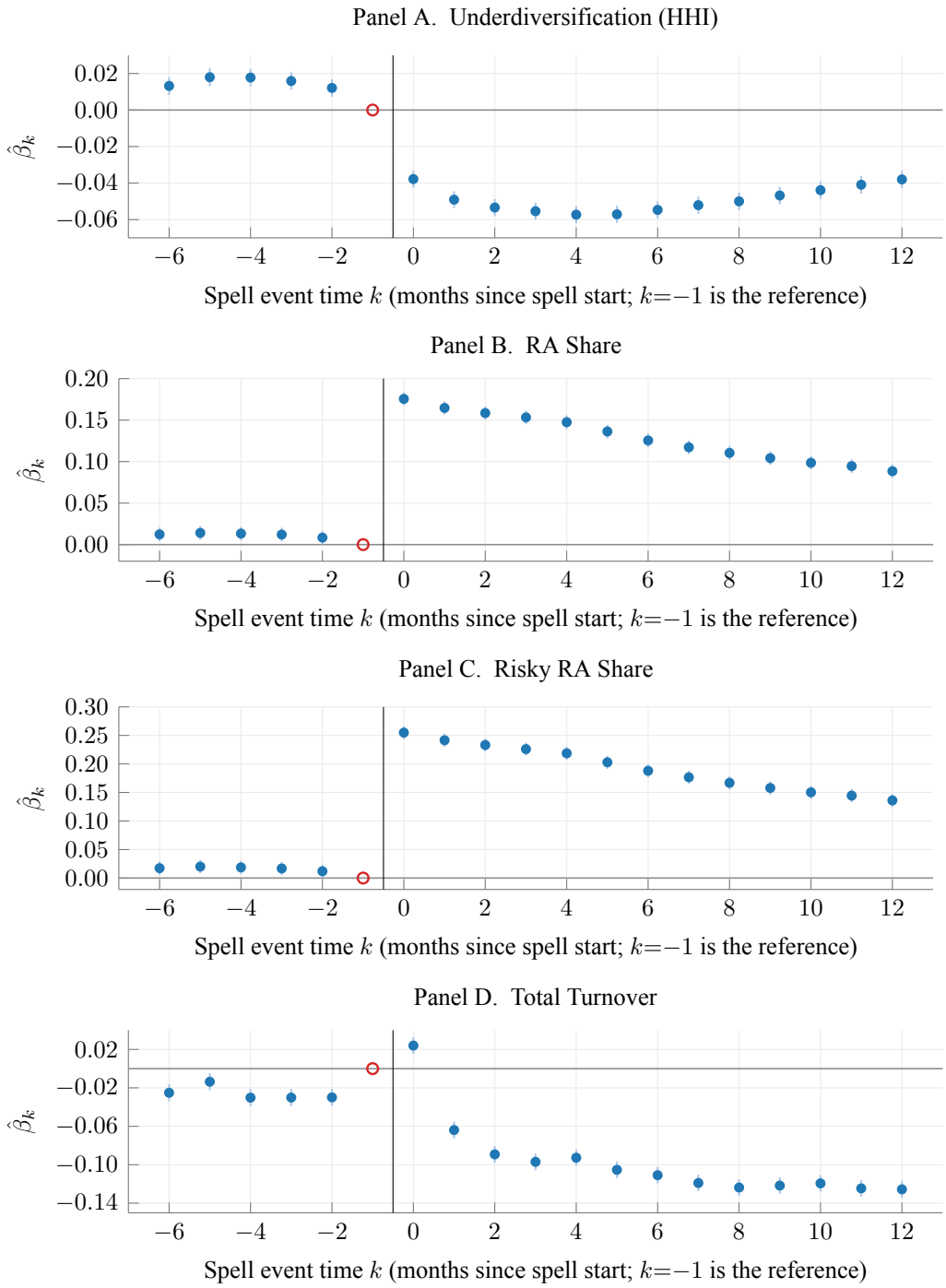
Online Appendix Table A.17 and Figure A.3 report stability checks across early and late sample windows and adoption cohorts. These exercises are scope checks; the main monthly Fama–MacBeth estimates remain the evidence. The early months of the sample correspond to a newly available product and a more selected adopter base, so some demographic coefficients are less stable in short windows; the early-period and late-cohort columns average only six and twenty-three monthly cross-sections respectively. Signs of the demographic effects are mostly, but not uniformly, stable. In the RA-share panel, log total financial assets is negative on the conditional share in all four splits, age is negative in all four, and cash-management assets are positive in all four; the female coefficient is negative everywhere except the early period, and risk preference moves from $-0.019$ early to $+0.008$ late, both significant. The risky-RA-share panel is less stable still: female moves from $+0.050$ to $-0.002$, risk preference from $-0.059$ to $+0.005$, and age from an insignificant $+0.006$ to $-0.007$, all between the early and late windows. The table is best read as a scope check on the wealth and age gradients; it does not support a stability claim for the demographic block as a whole. The magnitude of the wealth effect shrinks over the sample, from $-0.216$ in the early period to $-0.088$ in the late period, which is consistent with the wealth gradient on intensive delegation weakening as the technology matures. The main sign patterns are evaluated in the longer monthly panels and in the joint specification, not in six-month subsamples alone. Online Appendix Table A.18 reports correlations between age and the behavioral-bias, skill, and experience measures. These correlations support the interpretation of the

age coefficient in the main text. Older investors trade less, with a panel correlation of $-0.068$ between age and total turnover; are less concentrated by HHI, at $-0.034$; and display stronger confirmation bias, at $+0.060$. They also hold more funds and have longer fund experience. Once these behavioral correlates are controlled for, the residual age coefficient operates mainly on adoption rather than on conditional delegation. The appendix table is therefore not a separate mechanism test; it explains why the raw age coefficient changes when the behavioral block enters. Online Appendix Table A.19 reports self-directed-sleeve composition within the RA-adopter sample. The lagged combined-advisor indicator, which captures use of delegated advisory products other than the focal BangNiTou RA product, is associated with more self-directed funds, lower self-directed concentration, a higher self-directed share, and lower total and speculative turnover. This evidence is descriptive and should not be read as a causal effect of advisory use on self-directed discipline. It supports a more limited point: active delegated-product use can coexist with substantial and disciplined self-directed investing, so delegation does not mechanically replace the self-directed sleeve.

### A.12. Appendix Figures

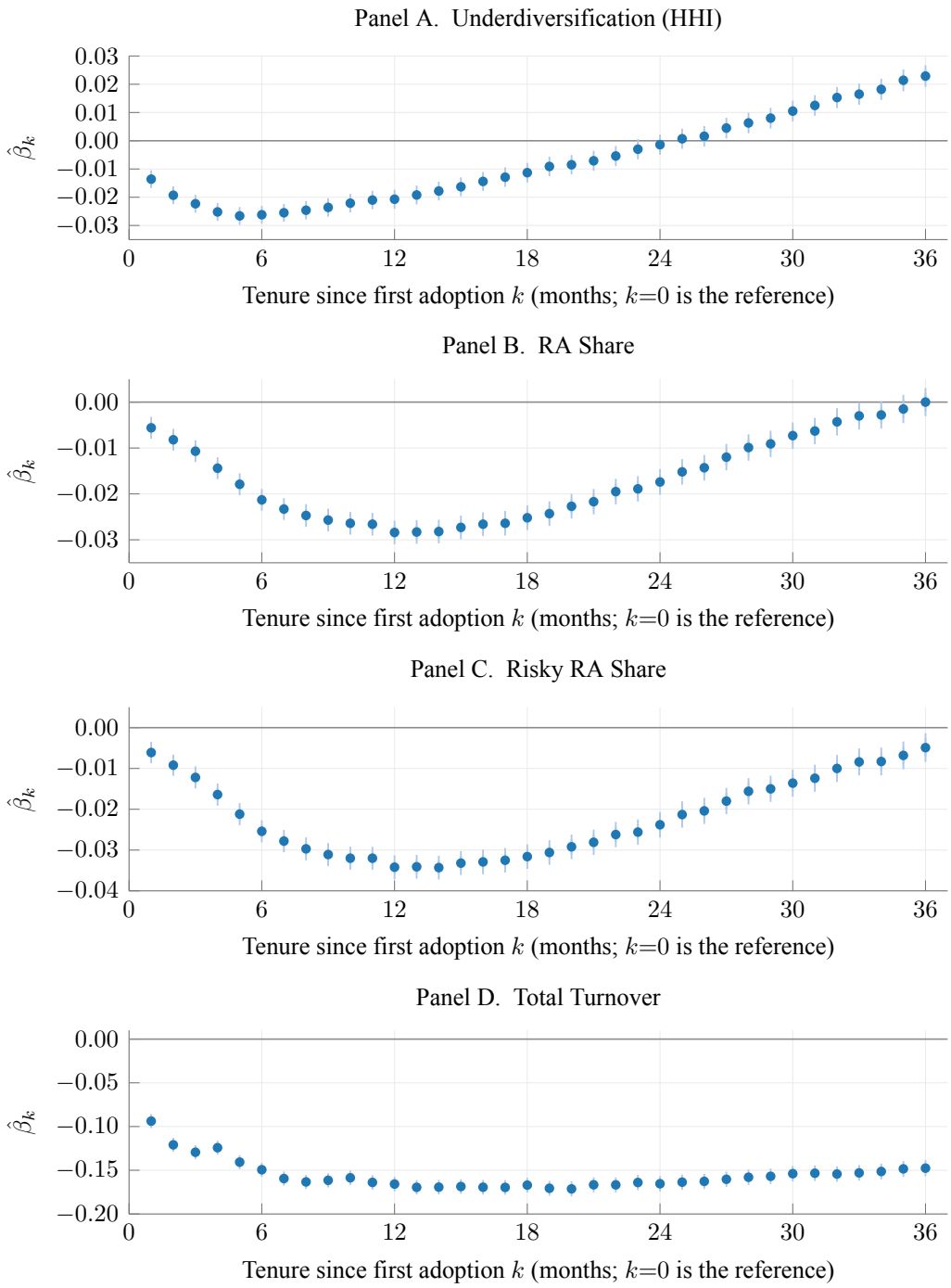
Figure A.1 repeats the event-study analysis for all adoption spells, including re-entry spells, instead of only first-ever adoption. Figure A.2 reports the post-adoption tenure event study for allocation and self-directed-sleeve outcomes. Figure A.3 plots time-stability and cohort-split coefficients. These figures are included to show that the main visual patterns are not driven by a single event definition, sample window, or return specification.

Figure A.1: Event Study for All Adoption Spells (Not Just the First)



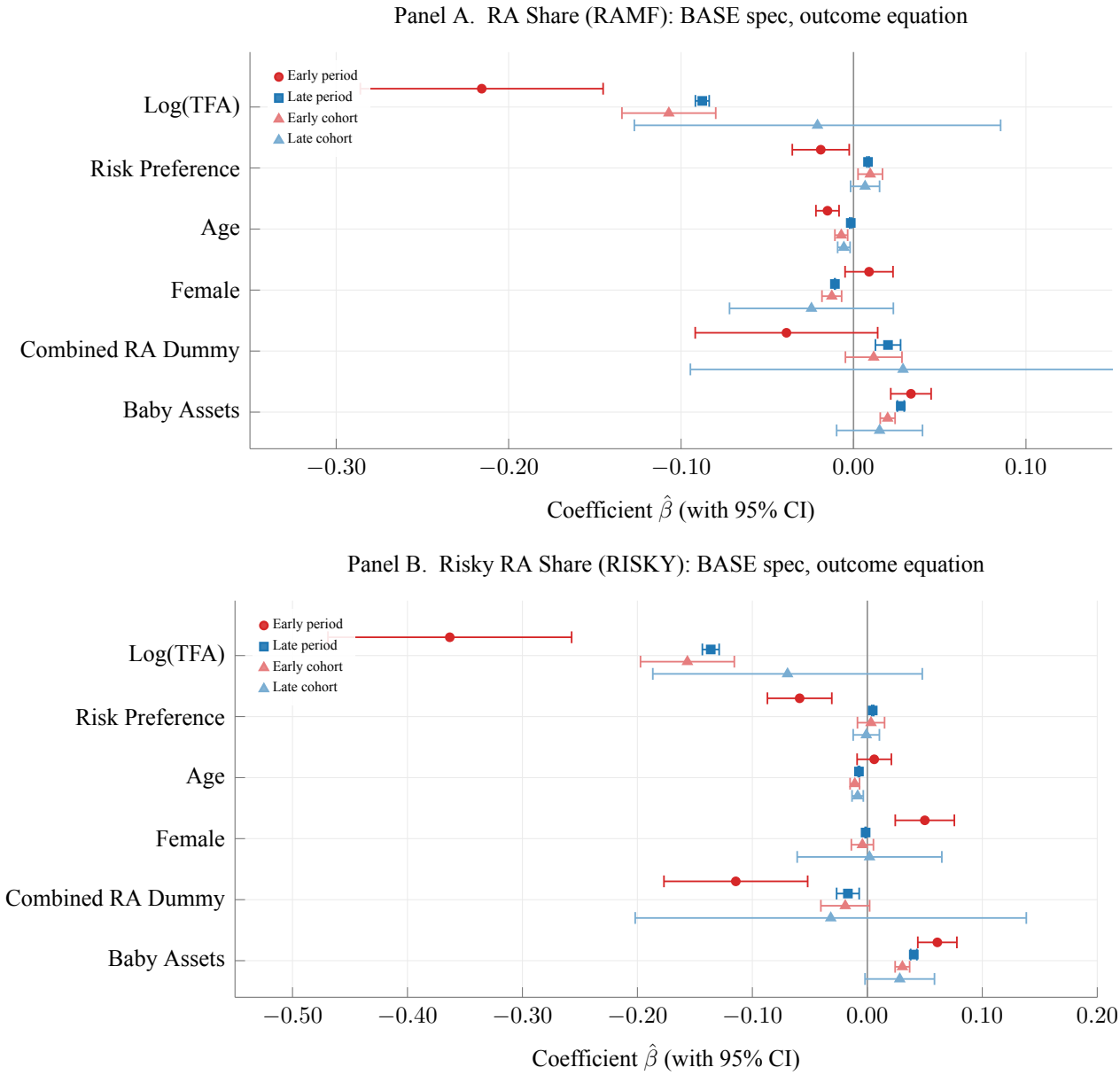

*Notes.* Event-study coefficients from **spell** fixed-effects regressions $y_{it} - \bar{y}_s = \sum_k \beta_k \cdot \mathbf{1}\{k_{st} = k\} + \varepsilon_{it}$, where $s$ indexes RA-adoption *spells* (every $0 \to 1$ transition of $\text{RA}_{it}$ opens a new spell). Demeaning is done within spell rather than within investor, so investors who exit and re-enter RA contribute multiple observations. Window is shorter than the first-adoption event study in Figure 4 ($k \in [-6, +12]$ vs. $[-12, +24]$) because spells can be re-entered; the relevant comparison is short-horizon. The spell sample is capped at 100,000 randomly drawn spells (seed 42) for computational tractability. Points are $\hat{\beta}_k$; vertical bars are 95% heteroskedasticity-robust CIs; the hollow red marker at $k = -1$ marks the reference period. Cross-panel consistency with the first-adoption event study (Figure 4) in the overlap $k \in [-6, +12]$ confirms that the within-investor first-adoption pattern is not driven by first-adoption effects alone.

Figure A.2: Post-Adoption Tenure Event Study




*Notes.* Event-study coefficients from within-investor fixed-effects regressions $y_{it} - \bar{y}_i = \sum_k \beta_k \cdot \mathbf{1}\{\text{tenure}_{it} = k\} + \varepsilon_{it}$, where tenure is months elapsed since an investor's first RA adoption. Sample restricted to the post-adoption window of first adopters; the reference period is $k = 0$ (month of first adoption). In contrast to Figure 4, which plots absolute event-time coefficients and shows the dominant level shift at adoption, this figure traces how the post-adoption gap from the adoption baseline *evolves with tenure*. Points are $\hat{\beta}_k$; vertical bars are 95% heteroskedasticity-robust CIs. All four outcomes drift back toward the adoption baseline as tenure grows: HHI recovers from $-0.027$ at month 5 to $+0.023$ at month 36 (a clear reconcentration); RA share recovers from $-0.028$ to $\approx 0$; risky RA share from $-0.034$ to $-0.005$; turnover partially recovers from $-0.170$ to $-0.148$. The pattern is the within-adopter analogue of the "reclaiming control" result in Figure 3.

Figure A.3: Time-Varying Stability and Cohort-Split Coefficient Plot




*Notes.* BASE-spec Heckman outcome-equation coefficients $\hat{\beta}_k$ across four sample splits: (i) *Early period* — the first 6 months of the panel; (ii) *Late period* — the final 6 months; (iii) *Early cohort* — investors whose first RA adoption occurred in the first 6 months; (iv) *Late cohort* — investors whose first RA adoption occurred in the final 6 months. Panel A uses RAMF $=$ RA$/$(RA $+$ MF) as the outcome; Panel B uses the risky RA share. Points are $\hat{\beta}$ and horizontal bars are 95% CIs. The BASE spec includes only demographics (Log(TFA), Risk Preference, Age, Female, Combined RA Dummy, Baby Assets) — the most comparable subset across the four splits. Wide CIs in the Early-period and Late-cohort bins reflect small samples (only one cohort observed for less than 6 months), but the *sign pattern* of the demographic effects is stable across splits: Log(TFA) is consistently negative, Baby Assets consistently positive, and the Combined RA Dummy consistently positive in the late period and late cohort (consistent with adoption persistence).

## A.13. Appendix Tables

Table A.1: Main Heckman Results: Weighted Estimation

| | Panel 1: Baseline (T=48) | | Panel 2: +Skills (T=47) | | Panel 3: +Biases (T=36) | |
|---|---|---|---|---|---|---|
| Variable | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) | Probit ME | Outcome ($RA_{share}$) |
| Constant | -0.063*** | 0.756*** | -0.063*** | 0.639** | -0.063*** | 0.458*** |
| | (-184.010) | (2.715) | (-143.929) | (2.480) | (-333.818) | (10.688) |
| Log(Total Financial Assets)$_{t-1}$ | 0.006*** | -0.175*** | 0.006*** | -0.128*** | 0.003*** | -0.058*** |
| | (10.722) | (-4.389) | (10.938) | (-5.478) | (5.935) | (-10.843) |
| Risk Preference | 0.001*** | -0.012 | 0.001*** | 0.012* | 0.001*** | 0.003*** |
| | (5.548) | (-0.969) | (4.474) | (1.792) | (7.218) | (4.707) |
| Age | 0.000 | -0.004 | 0.000 | -0.019*** | 0.001*** | -0.011*** |
| | (0.333) | (-0.538) | (0.284) | (-3.061) | (15.540) | (-14.330) |
| Female | -0.002*** | 0.014 | -0.002*** | 0.002 | -0.002*** | 0.010*** |
| | (-12.853) | (1.229) | (-15.712) | (0.227) | (-18.635) | (8.898) |
| Combined RA Dummy$_{t-1}$ | 0.011*** | -0.088*** | 0.010*** | -0.095*** | 0.010*** | -0.037*** |
| | (25.807) | (-4.006) | (28.284) | (-3.626) | (67.314) | (-6.831) |
| Baby Assets$_{t-1}$ | -0.002*** | 0.037*** | -0.001*** | 0.030*** | -0.001*** | 0.012*** |
| | (-16.439) | (5.403) | (-15.119) | (6.552) | (-7.976) | (8.476) |
| Fund Experience (Months)$_{t-1}$ | | | 0.002*** | -0.056*** | | |
| | | | (9.752) | (-10.974) | | |
| Selection Ability$_{t-1}$ | | | 0.000* | -0.004*** | | |
| | | | (1.770) | (-2.888) | | |
| Timing Ability$_{t-1}$ | | | 0.000 | -0.002 | | |
| | | | (0.819) | (-1.566) | | |
| Anchoring Bias$_{t-1}$ | | | | | 0.001*** | -0.010*** |
| | | | | | (11.006) | (-7.365) |
| Total Turnover$_{t-1}$ | | | | | -0.001*** | 0.062*** |
| | | | | | (-13.757) | (7.196) |
| Speculative Turnover$_{t-1}$ | | | | | 0.000*** | -0.018*** |
| | | | | | (-3.699) | (-8.071) |
| Attention Bias$_{t-1}$ | | | | | 0.000*** | -0.001 |
| | | | | | (-6.242) | (-1.637) |
| Disposition Effect$_{t-1}$ | | | | | 0.000*** | -0.003*** |
| | | | | | (5.265) | (-7.054) |
| Underdiversification HHI$_{t-1}$ | | | | | -0.003*** | 0.055*** |
| | | | | | (-98.504) | (26.405) |
| Extrapolation Bias$_{t-1}$ | | | | | 0.000*** | 0.001 |
| | | | | | (-3.139) | (0.998) |
| Gambling Bias$_{t-1}$ | | | | | 0.000*** | 0.002*** |
| | | | | | (-5.926) | (3.253) |
| Confirmation Bias$_{t-1}$ | | | | | 0.001*** | -0.039*** |
| | | | | | (24.914) | (-8.100) |
| Herding Bias$_{t-1}$ | | | | | 0.000*** | 0.000 |
| | | | | | (-7.486) | (0.833) |
| Months Since Registration$_{t-1}$ | 0.001*** | | 0.000*** | | 0.001*** | |
| | (9.771) | | (7.564) | | (8.828) | |
| Inverse Mills Ratio ($\lambda$) | | -0.197** | | -0.164* | | -0.098*** |
| | | (-2.054) | | (-1.718) | | (-6.119) |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| (Pseudo) $R^2$ | 0.044 | 0.132 | 0.041 | 0.146 | 0.046 | 0.228 |
| Adjusted $R^2$ | | 0.131 | | 0.145 | | 0.227 |
| $\hat{\rho}$ (mean / median) | | -0.364 / -0.472 | | -0.702 / -0.844 | | -0.463 / -0.463 |
| $\hat{\sigma}$ (mean / median) | | 0.348 / 0.243 | | 0.386 / 0.352 | | 0.203 / 0.200 |
| $\hat{\lambda}$ (mean / median) | | -0.197 / -0.113 | | -0.164 / -0.287 | | -0.098 / -0.097 |
| N (monthly mean) | 99,992 | 32,587 | 95,129 | 32,747 | 91,845 | 34,180 |
| N (total) | 4,799,616 | 1,564,155 | 4,471,044 | 1,539,119 | 3,306,408 | 1,230,489 |

*Notes.* This table reports a robustness check on the main (unweighted) results of Table 2, re-estimating the same specifications with calibrated inverse-probability weights. Estimates are from weighted Heckman two-stage (Tobit-2) selection models estimated monthly and averaged following Fama and MacBeth (1973). Both stages apply weights that align the sample RA-adoption rate with an assumed population rate of $p_{pop} = 1/101 \approx 0.99\%$: within each month, RA-adopter observations receive weight $p_{pop}/p_{samp,t}$ and non-adopter observations $(1-p_{pop})/(1-p_{samp,t})$, normalised to mean one. The selection stage is a weighted Probit and the outcome stage is WLS

on the adopter subsample. Reported selection-stage values are *approximate marginal effects* on the probability of RA adoption, computed as $\text{ME}_k = \phi(\Phi^{-1}(p_{pop})) \cdot \hat{\beta}_k$, where $\hat{\beta}_k$ is the Fama–MacBeth average of the monthly weighted Probit coefficients. Because the calibrated weights centre the fitted adoption probability on the population rate rather than the sample rate, the marginal effects are evaluated at $p_{pop} = 1/101$, giving a scaling factor $\phi(\Phi^{-1}(1/101)) = 0.026$. Marginal effects are computed from the unrounded coefficients and scaling factor and then rounded to three decimals for display; because the scaling factor is small, several marginal effects round to $0.000$ even where the underlying Probit coefficient is precisely estimated, and significance should be read from the $t$-statistics rather than from the displayed magnitudes. These marginal effects are evaluated at a population base rate of roughly one percent and are therefore not directly comparable in magnitude to those in Table 2, which are evaluated at the sample adoption rate of the estimation panel. Outcome-stage coefficients are reported as estimated (no scaling). Newey–West HAC $t$-statistics (max lag = 3), computed from the time series of monthly coefficients, are in parentheses; $t$-statistics, stars, and significance are invariant to the scaling. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Panel 1 reports the baseline specification. Panel 2 adds investor skills (fund experience, selection ability, timing ability). Panel 3 adds behavioral biases. The selection (Probit) dependent variable is $\text{RA}_{it} = 1$ if the investor holds robo-advisor assets; the outcome dependent variable is $\text{RA}_{share}$ = RA assets / (RA + mutual fund assets), observed only for RA-adopter months. All right-hand-side variables except Age, Female, and Risk Preference are lagged one period. Months Since Registration enters the selection equation only and serves as the exclusion restriction; the Inverse Mills Ratio, constructed from the monthly weighted Probit, enters the outcome equation only. Province dummies are included in both equations but not reported. The (Pseudo) $R^2$ row reports the McFadden pseudo-$R^2$ in the selection columns and the OLS $R^2$ in the outcome columns, averaged across months. T is the number of monthly cross-sections with successful estimation in each specification. N (total) is the sum of monthly observations across the T months; N (monthly mean) is the corresponding per-month average; reported N are unweighted observation counts. $\hat{\rho}$ is the average estimated correlation between the selection and outcome errors, $\hat{\sigma}$ the average standard deviation of the outcome error, and $\hat{\lambda} = \hat{\rho}\hat{\sigma}$ the average coefficient on the Inverse Mills Ratio; means and medians are taken over the T monthly estimates.

Table A.2: Exclusion-Restriction Sensitivity: Alternative Placements of Platform Tenure

| | Arm B: Tenure in both equations (T=36) | | | | | | Arm C: Tenure in neither equation (T=36) | | | | | | Arm E: Benchmark—tenure in selection only (T=36) | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Baseline | | +Skills | | +Biases | | Baseline | | +Skills | | +Biases | | Baseline | | +Skills | | +Biases | |
| Variable | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. |
| Constant | -0.408*** | -0.637*** | -0.416*** | -0.542*** | -0.401*** | -0.605*** | -0.402*** | -0.656*** | -0.412*** | -0.569*** | -0.395*** | -0.618*** | -0.408*** | -0.002 | -0.416*** | 0.001 | -0.401*** | -0.073 |
| | (-13.834) | (-9.974) | (-13.476) | (-7.290) | (-12.728) | (-9.799) | (-13.445) | (-9.942) | (-13.163) | (-7.309) | (-12.377) | (-9.925) | (-13.834) | (-0.036) | (-13.476) | (0.014) | (-12.728) | (-1.578) |
| Log(Total Financial Assets)$_{t-1}$ | 0.358*** | 0.091*** | 0.342*** | 0.070*** | 0.209*** | 0.063*** | 0.363*** | 0.102*** | 0.346*** | 0.082*** | 0.215*** | 0.070*** | 0.358*** | -0.055*** | 0.342*** | -0.052*** | 0.209*** | -0.008 |
| | (9.957) | (4.496) | (10.221) | (3.315) | (6.140) | (3.326) | (10.144) | (4.645) | (10.367) | (3.493) | (6.312) | (3.480) | (9.957) | (-6.615) | (10.221) | (-5.273) | (6.140) | (-1.279) |
| Risk Preference | 0.054*** | 0.030*** | 0.038*** | 0.024*** | 0.050*** | 0.028*** | 0.060*** | 0.034*** | 0.041*** | 0.027*** | 0.056*** | 0.031*** | 0.054*** | 0.012*** | 0.038*** | 0.015*** | 0.050*** | 0.014*** |
| | (6.885) | (11.854) | (4.737) | (15.392) | (6.611) | (10.516) | (7.890) | (14.884) | (5.403) | (18.344) | (7.627) | (13.343) | (6.885) | (7.238) | (4.737) | (8.028) | (6.611) | (9.872) |
| Age | 0.043*** | 0.014*** | 0.034*** | 0.010*** | 0.044*** | 0.012*** | 0.048*** | 0.018*** | 0.039*** | 0.013*** | 0.049*** | 0.016*** | 0.043*** | -0.002 | 0.034*** | 0.000 | 0.044*** | 0.001 |
| | (15.922) | (4.140) | (13.468) | (2.921) | (19.385) | (3.953) | (16.508) | (4.844) | (14.155) | (3.488) | (19.730) | (4.631) | (15.922) | (-0.833) | (13.468) | (0.151) | (19.385) | (0.416) |
| Female | -0.104*** | -0.058*** | -0.107*** | -0.052*** | -0.138*** | -0.064*** | -0.098*** | -0.056*** | -0.102*** | -0.051*** | -0.133*** | -0.062*** | -0.104*** | -0.016*** | -0.107*** | -0.016*** | -0.138*** | -0.018*** |
| | (-20.358) | (-11.162) | (-22.027) | (-9.603) | (-19.661) | (-9.820) | (-17.933) | (-10.579) | (-19.689) | (-9.326) | (-17.841) | (-9.504) | (-20.358) | (-4.919) | (-22.027) | (-4.188) | (-19.661) | (-4.243) |
| Combined RA Dummy$_{t-1}$ | 0.660*** | 0.280*** | 0.633*** | 0.239*** | 0.576*** | 0.251*** | 0.663*** | 0.290*** | 0.634*** | 0.250*** | 0.578*** | 0.257*** | 0.660*** | 0.044** | 0.633*** | 0.048** | 0.576*** | 0.084*** |
| | (77.541) | (10.692) | (74.834) | (8.280) | (90.326) | (11.166) | (76.757) | (10.657) | (74.400) | (8.245) | (89.735) | (11.236) | (77.541) | (2.502) | (74.834) | (2.379) | (90.326) | (5.318) |
| Baby Assets$_{t-1}$ | -0.094*** | -0.027*** | -0.089*** | -0.020*** | -0.054*** | -0.018*** | -0.094*** | -0.028*** | -0.089*** | -0.022*** | -0.054*** | -0.019*** | -0.094*** | 0.011*** | -0.089*** | 0.010** | -0.054*** | -0.001 |
| | (-14.191) | (-3.910) | (-12.379) | (-3.074) | (-9.797) | (-3.357) | (-14.110) | (-4.075) | (-12.268) | (-3.271) | (-9.715) | (-3.442) | (-14.191) | (2.852) | (-12.379) | (2.370) | (-9.797) | (-0.151) |
| Fund Experience (Months)$_{t-1}$ | | | 0.108*** | 0.017*** | | | | | 0.111*** | 0.022*** | | | | | 0.108*** | -0.022*** | | |
| | | | (20.231) | (4.297) | | | | | (20.195) | (5.924) | | | | | (20.231) | (-3.653) | | |
| Selection Ability$_{t-1}$ | | | 0.018*** | 0.008*** | | | | | 0.019*** | 0.008*** | | | | | 0.018*** | 0.002 | | |
| | | | (3.603) | (2.689) | | | | | (3.598) | (2.743) | | | | | (3.603) | (1.042) | | |
| Timing Ability$_{t-1}$ | | | 0.004 | 0.000 | | | | | 0.004 | 0.000 | | | | | 0.004 | -0.002 | | |
| | | | (1.012) | (0.058) | | | | | (1.003) | (0.113) | | | | | (1.012) | (-1.595) | | |
| Anchoring Bias$_{t-1}$ | | | | | 0.056*** | 0.022*** | | | | | 0.056*** | 0.022*** | | | | | 0.056*** | 0.003* |
| | | | | | (12.206) | (7.242) | | | | | (12.027) | (7.252) | | | | | (12.206) | (1.648) |
| Total Turnover$_{t-1}$ | | | | | -0.083*** | 0.009 | | | | | -0.084*** | 0.007 | | | | | -0.083*** | 0.039*** |
| | | | | | (-14.562) | (1.125) | | | | | (-14.479) | (0.944) | | | | | (-14.562) | (5.475) |
| Attention Bias$_{t-1}$ | | | | | -0.023*** | -0.013*** | | | | | -0.023*** | -0.014*** | | | | | -0.023*** | -0.006*** |
| | | | | | (-7.107) | (-6.631) | | | | | (-7.275) | (-6.785) | | | | | (-7.107) | (-4.689) |
| Disposition Effect$_{t-1}$ | | | | | 0.007*** | 0.001 | | | | | 0.008*** | 0.001 | | | | | 0.007*** | -0.002*** |
| | | | | | (5.256) | (0.859) | | | | | (5.293) | (1.090) | | | | | (5.256) | (-4.422) |
| Underdiversification HHI$_{t-1}$ | | | | | -0.172*** | -0.042*** | | | | | -0.170*** | -0.043*** | | | | | -0.172*** | 0.018*** |
| | | | | | (-95.296) | (-5.121) | | | | | (-91.973) | (-5.177) | | | | | (-95.296) | (3.153) |
| Extrapolation Bias$_{t-1}$ | | | | | -0.013*** | -0.006** | | | | | -0.013*** | -0.006** | | | | | -0.013*** | -0.002* |
| | | | | | (-2.859) | (-2.493) | | | | | (-2.904) | (-2.548) | | | | | (-2.859) | (-1.755) |
| Gambling Bias$_{t-1}$ | | | | | -0.024*** | -0.013*** | | | | | -0.024*** | -0.013*** | | | | | -0.024*** | -0.004*** |
| | | | | | (-5.861) | (-5.043) | | | | | (-5.862) | (-5.057) | | | | | (-5.861) | (-3.471) |
| Confirmation Bias$_{t-1}$ | | | | | 0.080*** | 0.006 | | | | | 0.081*** | 0.007* | | | | | 0.080*** | -0.023*** |
| | | | | | (25.986) | (1.339) | | | | | (25.842) | (1.692) | | | | | (25.986) | (-5.717) |
| Herding Bias$_{t-1}$ | | | | | -0.017*** | -0.009*** | | | | | -0.017*** | -0.009*** | | | | | -0.017*** | -0.004*** |
| | | | | | (-7.484) | (-5.254) | | | | | (-7.575) | (-5.239) | | | | | (-7.484) | (-3.766) |
| Months Since Registration$_{t-1}$ | 0.038*** | 0.023*** | 0.030*** | 0.019*** | 0.036*** | 0.021*** | | | | | | | 0.038*** | | 0.030*** | | 0.036*** | |
| | (12.403) | (9.932) | (10.383) | (7.295) | (10.968) | (8.933) | | | | | | | (12.403) | | (10.383) | | (10.968) | |
| Inverse Mills Ratio ($\lambda$) | | 0.769*** | | 0.684*** | | 0.750*** | | 0.791*** | | 0.713*** | | 0.765*** | | 0.180*** | | 0.182*** | | 0.255*** |
| | | (10.829) | | (8.312) | | (11.009) | | (10.654) | | (8.177) | | (10.965) | | (3.596) | | (3.193) | | (5.403) |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| (Pseudo) $R^2$ | 0.070 | 0.110 | 0.074 | 0.127 | 0.090 | 0.234 | 0.069 | 0.109 | 0.074 | 0.127 | 0.089 | 0.234 | 0.070 | 0.105 | 0.074 | 0.123 | 0.090 | 0.230 |
| Adjusted $R^2$ | | 0.109 | | 0.126 | | 0.233 | | 0.108 | | 0.126 | | 0.233 | | 0.104 | | 0.122 | | 0.229 |
| $\hat{\rho}$ | | 3.889 | | 3.452 | | 4.116 | | 3.991 | | 3.590 | | 4.196 | | 0.860 | | 0.875 | | 1.381 |
| $\hat{\sigma}$ | | 0.195 | | 0.198 | | 0.180 | | 0.195 | | 0.193 | | 0.180 | | 0.196 | | 0.194 | | 0.181 |
| $\hat{\lambda}$ (IMR coefficient) | | 0.769 | | 0.684 | | 0.750 | | 0.791 | | 0.713 | | 0.765 | | 0.180 | | 0.182 | | 0.255 |
| IMR-on-$X$ $R^2$ | | 0.995 | | 0.995 | | 0.995 | | 0.995 | | 0.995 | | 0.995 | | 0.985 | | 0.990 | | 0.988 |

*Continued on next page*

*Table A.2 (continued)*

| | Arm B: Tenure in both equations (T=36) | | | | | | Arm C: Tenure in neither equation (T=36) | | | | | | Arm E: Benchmark—tenure in selection only (T=36) | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Baseline | | +Skills | | +Biases | | Baseline | | +Skills | | +Biases | | Baseline | | +Skills | | +Biases | |
| Variable | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. | Sel. | Out. |
| Condition number | | 141.5 | | 143.3 | | 137.7 | | 142.2 | | 143.1 | | 137.7 | | 90.1 | | 106.9 | | 96.1 |
| N (monthly mean) | 91,813 | 34,173 | 91,813 | 34,173 | 91,813 | 34,173 | 91,813 | 34,173 | 91,813 | 34,173 | 91,813 | 34,173 | 91,813 | 34,173 | 91,813 | 34,173 | 91,813 | 34,173 |
| N (total) | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 | 3,305,265 | 1,230,240 |

*Notes.* This table assesses the sensitivity of the Heckman two-step estimates to the placement of the exclusion restriction (Months Since Registration, i.e. platform tenure). Arm B includes tenure in both the selection and outcome equations; Arm C includes it in neither; Arm E is the benchmark with tenure in the selection equation only, corresponding to the specification of Table 2. For each specification, Sel. reports the selection equation (monthly Probit, raw coefficients, not marginal effects and therefore not directly comparable in magnitude to the marginal effects of Table 2) and Out. reports the outcome equation (adopter-month OLS including the inverse Mills ratio). Coefficients are Fama–MacBeth means of monthly estimates, reported to three decimals; Newey–West $t$-statistics with lag $\min(3, T-1)$, computed from the time series of monthly coefficients, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Because Arms B and E share an identical selection equation, their Sel. columns coincide by construction; Arm C differs only through the omission of tenure. The sample is identical across the three arms, estimation is unweighted, and all right-hand-side variables except Age, Female, and Risk Preference are lagged one month; province dummies enter both equations but are not reported. Arm C is identified only through the Probit functional-form assumptions (the nonlinearity of the inverse Mills ratio), with no excluded instrument. The (Pseudo) $R^2$ row reports the McFadden pseudo-$R^2$ in Sel. columns and the OLS $R^2$ in Out. columns, averaged across months. $\hat{\sigma}$ is the average standard deviation of the outcome error, $\hat{\lambda}$ the average coefficient on the inverse Mills ratio, and $\hat{\rho}$ the average of the monthly ratios $\hat{\lambda}_t/\hat{\sigma}_t$. Values of $\hat{\rho}$ far outside the admissible correlation range $[-1, 1]$ in Arms B and C indicate that the selection correction is not separately identified when the exclusion restriction is absent from (or duplicated in) the outcome equation. Consistent with this, the IMR-on-$X$ $R^2$ — the $R^2$ from regressing the inverse Mills ratio on the outcome regressors — is near one in all arms and highest in B and C, and the condition number of the outcome design matrix rises relative to the benchmark, confirming that identification in Arms B and C rests on residual nonlinearity alone. In Arm E, the Months Since Registration selection coefficient is the exclusion-relevance statistic and is strongly significant in all specifications. N (total) is the sum of monthly observations across the T months; N (monthly mean) is the corresponding per-month average.

Table A.3: Allocation Among Adopters: OLS Without Selection Correction

| Variable | Panel 1: Baseline (T=48) Outcome ($RA_{share}$) | Panel 2: +Skills (T=47) Outcome ($RA_{share}$) | Panel 3: +Biases (T=36) Outcome ($RA_{share}$) |
|---|---|---|---|
| Constant | 0.216*** | 0.211*** | 0.193*** |
| | (12.936) | (15.197) | (15.585) |
| Log(Total Financial Assets)$_{t-1}$ | -0.123*** | -0.112*** | -0.047*** |
| | (-11.699) | (-12.414) | (-14.020) |
| Risk Preference | 0.003* | 0.009*** | 0.006*** |
| | (1.689) | (6.249) | (13.564) |
| Age | -0.011*** | -0.008*** | -0.009*** |
| | (-7.584) | (-5.275) | (-12.103) |
| Female | -0.003*** | -0.003*** | 0.003** |
| | (-3.030) | (-4.755) | (2.273) |
| Combined RA Dummy$_{t-1}$ | -0.030*** | -0.021*** | -0.004*** |
| | (-9.369) | (-10.787) | (-5.852) |
| Baby Assets$_{t-1}$ | 0.025*** | 0.022*** | 0.009*** |
| | (12.548) | (12.554) | (5.867) |
| Fund Experience (Months)$_{t-1}$ | | -0.038*** | |
| | | (-7.866) | |
| Selection Ability$_{t-1}$ | | -0.001** | |
| | | (-2.040) | |
| Timing Ability$_{t-1}$ | | -0.001** | |
| | | (-2.191) | |
| Anchoring Bias$_{t-1}$ | | | -0.007*** |
| | | | (-5.556) |
| Total Turnover$_{t-1}$ | | | 0.057*** |
| | | | (6.308) |
| Speculative Turnover$_{t-1}$ | | | -0.018*** |
| | | | (-8.241) |
| Attention Bias$_{t-1}$ | | | -0.003*** |
| | | | (-3.864) |
| Disposition Effect$_{t-1}$ | | | -0.003*** |
| | | | (-5.642) |
| Underdiversification HHI$_{t-1}$ | | | 0.045*** |
| | | | (25.127) |
| Extrapolation Bias$_{t-1}$ | | | -0.000 |
| | | | (-0.228) |
| Gambling Bias$_{t-1}$ | | | 0.001** |
| | | | (2.126) |
| Confirmation Bias$_{t-1}$ | | | -0.034*** |
| | | | (-6.752) |
| Herding Bias$_{t-1}$ | | | -0.000 |
| | | | (-0.875) |
| Province Dummies | Yes | Yes | Yes |
| $R^2$ | 0.132 | 0.145 | 0.228 |
| N (total) | 1,564,155 | 1,539,119 | 1,230,489 |

*Notes.* This table reports OLS regressions of the delegated share on investor characteristics, estimated among RA-adopter months only ($RA_{it} = 1$), with no selection correction. Unlike the Heckman specifications of Table 2, no inverse Mills ratio and no exclusion variable enter the regression; platform tenure enters only through the sample screen (adopter-months with tenure non-missing), so the estimation sample coincides with the outcome-stage sample of Table 2 panel for panel, and the coefficients are directly comparable to its Outcome columns. Estimates are monthly cross-sectional OLS coefficients averaged following Fama and MacBeth (1973). The dependent variable is $RA_{share}$ = RA assets / (RA + mutual fund assets), observed only for RA-adopter months and winsorised at the 1st and 99th percentiles within month. Newey–West HAC $t$-statistics (max lag = 3), computed from the time series of monthly coefficients, are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Panel 1 reports the baseline specification. Panel 2 adds investor skills (fund experience, selection ability, timing ability). Panel 3 adds behavioral biases, with total and speculative turnover entering together. All right-hand-side variables except Age, Female, and Risk Preference are lagged one period. Province dummies are included but not reported. The $R^2$ row reports the OLS $R^2$ averaged across months. T is the number of monthly cross-sections with successful estimation in each specification. N (total) is the sum of monthly observations across the T months; N (monthly mean) is the corresponding per-month average.

Table A.4: Heckman Two-Stage Estimates: Full Specification (Baseline + Skills + Biases)

| Variable | Probit ME | Outcome ($RA_{share}$) |
|---|---|---|
| Constant | -0.152*** | -0.095* |
| | (-12.461) | (-1.870) |
| Log(Total Financial Assets)$_{t-1}$ | 0.079*** | -0.005 |
| | (6.081) | (-0.664) |
| Risk Preference | 0.015*** | 0.015*** |
| | (5.587) | (12.132) |
| Age | 0.015*** | 0.000 |
| | (21.152) | (0.011) |
| Female | -0.052*** | -0.021*** |
| | (-19.747) | (-4.393) |
| Combined RA Dummy$_{t-1}$ | 0.214*** | 0.090*** |
| | (93.140) | (5.316) |
| Baby Assets$_{t-1}$ | -0.020*** | -0.002 |
| | (-9.001) | (-0.460) |
| Fund Experience (Months)$_{t-1}$ | 0.024*** | -0.005 |
| | (10.302) | (-1.413) |
| Selection Ability$_{t-1}$ | 0.007*** | 0.003* |
| | (3.412) | (1.896) |
| Timing Ability$_{t-1}$ | 0.001 | -0.001 |
| | (0.867) | (-0.823) |
| Anchoring Bias$_{t-1}$ | 0.022*** | 0.003 |
| | (12.667) | (1.362) |
| Total Turnover$_{t-1}$ | -0.028*** | 0.039*** |
| | (-12.421) | (6.129) |
| Attention Bias$_{t-1}$ | -0.008*** | -0.006*** |
| | (-6.684) | (-4.577) |
| Disposition Effect$_{t-1}$ | 0.001** | -0.000 |
| | (2.275) | (-0.719) |
| Underdiversification HHI$_{t-1}$ | -0.065*** | 0.016*** |
| | (-91.321) | (2.610) |
| Extrapolation Bias$_{t-1}$ | -0.004*** | -0.002* |
| | (-3.075) | (-1.707) |
| Gambling Bias$_{t-1}$ | -0.009*** | -0.004*** |
| | (-5.798) | (-2.820) |
| Confirmation Bias$_{t-1}$ | 0.019*** | -0.011*** |
| | (21.864) | (-3.264) |
| Herding Bias$_{t-1}$ | -0.006*** | -0.004*** |
| | (-6.519) | (-3.807) |
| Months Since Registration$_{t-1}$ | 0.012*** | |
| | (10.176) | |
| Inverse Mills Ratio ($\lambda$) | | 0.277*** |
| | | (5.404) |
| Province Dummies | Yes | Yes |
| Pseudo $R^2$ / $R^2$ | 0.092 | 0.226 |
| N | 3,214,651 | 1,199,341 |

*Notes.* Estimates are from an unweighted Heckman two-stage (Tobit-2) selection model estimated monthly. Reported selection-stage values are *approximate marginal effects* on the probability of RA adoption, computed as $\text{ME}_k = \phi(\Phi^{-1}(\bar{P})) \cdot \hat{\beta}_k$, where $\bar{P} = 0.373$ is the average monthly RA-active rate in the sample and $\hat{\beta}_k$ is the Fama–MacBeth average Probit coefficient. The scaling factor is $\phi(\Phi^{-1}(0.373)) = 0.379$. Outcome-stage coefficients are reported as estimated (no scaling). Newey–West HAC $t$-statistics (max lag = 3) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). The full specification includes baseline demographics and assets, investor skills (fund experience, selection ability, timing ability), and behavioral biases (anchoring, turnover, attention, disposition effect, underdiversification, extrapolation, gambling, confirmation, herding). The selection (Probit) dependent variable is $\text{RA}_{it} = 1$ if the investor holds robo-advisor assets; the outcome dependent variable is $\text{RA}_{share}$ = RA assets / (RA + mutual fund assets). Months Since Registration serves as the exclusion restriction (selection-only). All right-hand-side variables except Age, Female, and Risk Preference are lagged one period. Province dummies are included but not reported. $\hat{\sigma} = 0.182$, $\hat{\lambda} = 0.277$ (FM averages). The two-step estimator does not constrain the implied $\hat{\rho} = \hat{\lambda}/\hat{\sigma}$ to $[-1, 1]$; its average exceeds one, reflecting the heavily loaded inverse-Mills term rather than a literal error correlation, so we report $\hat{\lambda}$ as the selection statistic. N for the outcome equation is the number of RA-adopter months; N for the selection equation is the full sample of investor-months. The $\phi(\Phi^{-1}(\cdot))$ approximation is accurate to within 1–5% of a properly computed average marginal effect for Probit models with well-behaved $X\hat{\beta}$ dispersion.

Table A.5: Alternative Outcome Definitions for the Intensive Margin

| **Panel A: DV-A** $= \log(1 +$ **RA assets**$)$ (level-form intensive margin) | | | | | | |
|---|---|---|---|---|---|---|
| | BASE | | SKILL | | BIAS | |
| Variable | Selection ME | Outcome | Selection ME | Outcome | Selection ME | Outcome |
| Constant | -0.197*** | 6.391*** | -0.190*** | 7.424*** | -0.150*** | 8.099*** |
| | (-6.745) | (5.278) | (-7.655) | (10.299) | (-12.626) | (17.804) |
| Log(TFA) (t-1) | 0.141*** | 1.720*** | 0.134*** | 1.566*** | 0.080*** | 1.591*** |
| | (12.559) | (6.428) | (11.754) | (9.478) | (6.036) | (19.073) |
| Risk Preference | 0.029*** | 0.221** | 0.022*** | 0.166*** | 0.019*** | 0.092*** |
| | (5.616) | (2.308) | (4.261) | (3.666) | (6.626) | (5.682) |
| Age | 0.004 | -0.200*** | 0.003 | -0.157*** | 0.017*** | -0.137*** |
| | (0.770) | (-3.284) | (0.612) | (-4.086) | (25.518) | (-5.148) |
| Female | -0.042*** | -0.003 | -0.043*** | 0.080 | -0.052*** | 0.182*** |
| | (-17.144) | (-0.026) | (-19.983) | (1.324) | (-19.617) | (8.010) |
| Combined RA Dummy (t-1) | 0.224*** | -0.292 | 0.222*** | -0.471*** | 0.219*** | -0.554*** |
| | (24.914) | (-1.312) | (29.157) | (-3.061) | (98.397) | (-4.039) |
| Baby Assets (t-1) | -0.037*** | -0.077 | -0.034*** | -0.038 | -0.020*** | -0.048*** |
| | (-18.857) | (-1.408) | (-16.497) | (-1.235) | (-9.378) | (-2.607) |
| Fund Experience (mos) | | | 0.035*** | -0.216*** | | |
| | | | (9.546) | (-10.657) | | |
| Selection Skill | | | 0.004* | -0.011 | | |
| | | | (1.886) | (-1.432) | | |
| Timing Rate | | | 0.001 | -0.005 | | |
| | | | (0.968) | (-0.940) | | |
| Anchoring Bias | | | | | 0.022*** | -0.052*** |
| | | | | | (12.976) | (-3.618) |
| Total Turnover | | | | | -0.031*** | 0.069*** |
| | | | | | (-14.565) | (4.835) |
| Attention Bias | | | | | -0.009*** | 0.015 |
| | | | | | (-6.870) | (1.456) |
| Disposition Effect | | | | | 0.001* | 0.013*** |
| | | | | | (1.751) | (3.508) |
| Underdiversification (HHI) | | | | | -0.066*** | 0.204*** |
| | | | | | (-99.536) | (4.328) |
| Extrapolation Bias | | | | | -0.005*** | 0.011* |
| | | | | | (-2.731) | (1.869) |
| Gambling Bias | | | | | -0.009*** | -0.010* |
| | | | | | (-6.546) | (-1.769) |
| Confirmation Bias | | | | | 0.022*** | -0.048*** |
| | | | | | (29.161) | (-3.161) |
| Herding Bias | | | | | -0.006*** | 0.029*** |
| | | | | | (-7.322) | (4.698) |
| Months Since Reg. (t-1) | 0.012*** | | 0.008*** | | 0.014*** | |
| | (10.538) | | (7.800) | | (11.047) | |
| Inverse Mills Ratio | | -0.155 | | -0.838 | | -1.411*** |
| | | (-0.196) | | (-1.623) | | (-3.894) |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ / $R^2$ | — | 0.251 | — | 0.258 | — | 0.250 |
| N | 4,799,616 | 1,564,155 | 4,471,044 | 1,539,119 | 3,214,908 | 1,199,373 |

*Notes to Panel A.* Outcome variable is $\log(1 +$ ra_assets_raw$)$, the log of (one plus) the level of RA assets. This preserves variation in absolute RA holdings that is normalised out by the RA-share outcome in the main paper. Three nested specifications: BASE (demographics), SKILL (adds Fund Experience, Selection Skill, Timing Rate), and BIAS (adds 9 biases). Heckman two-stage with Fama–MacBeth monthly estimation and Newey–West HAC $t$-statistics (max lag $= 3$) in parentheses. The outcome is estimated only on the sample with RA $= 1$. Selection-stage values are approximate marginal effects on the probability of RA adoption, $\text{ME}_k = \phi(\Phi^{-1}(\bar{P}))\hat{\beta}_k$, with $\bar{P} = 0.326, 0.344, 0.373$ (factors $0.360, 0.368, 0.379$) for BASE/SKILL/BIAS on the 4.9-million investor-month panel; outcome coefficients are unscaled and $t$-statistics and significance are invariant. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

**Panel B: DV-B = RA / Total Financial Assets** (share-of-wealth intensive margin)

| | BASE | | SKILL | | BIAS | |
|---|---|---|---|---|---|---|
| Variable | Selection ME | Outcome | Selection ME | Outcome | Selection ME | Outcome |
| Constant | -0.197*** | 0.000 | -0.190*** | -0.045 | -0.150*** | 0.056* |
| | (-6.745) | (0.000) | (-7.655) | (-0.759) | (-12.626) | (1.758) |
| Log(TFA) (t-1) | 0.141*** | -0.025*** | 0.134*** | -0.010 | 0.080*** | -0.011*** |
| | (12.559) | (-4.533) | (11.754) | (-0.812) | (6.036) | (-2.725) |
| Risk Preference | 0.029*** | 0.014*** | 0.022*** | 0.018*** | 0.019*** | 0.008*** |
| | (5.616) | (5.600) | (4.261) | (3.997) | (6.626) | (8.215) |
| Age | 0.004 | -0.008*** | 0.003 | -0.010** | 0.017*** | -0.004*** |
| | (0.770) | (-2.908) | (0.612) | (-2.064) | (25.518) | (-2.780) |
| Female | -0.042*** | -0.012*** | -0.043*** | -0.015*** | -0.052*** | -0.004* |
| | (-17.144) | (-4.005) | (-19.983) | (-2.832) | (-19.617) | (-1.737) |
| Combined RA Dummy (t-1) | 0.224*** | 0.014 | 0.222*** | 0.024* | 0.219*** | 0.010 |
| | (24.914) | (1.429) | (29.157) | (1.952) | (98.397) | (0.883) |
| Baby Assets (t-1) | -0.037*** | -0.010*** | -0.034*** | -0.011*** | -0.020*** | -0.010*** |
| | (-18.857) | (-4.385) | (-16.497) | (-4.062) | (-9.378) | (-6.239) |
| Fund Experience (mos) | | | 0.035*** | -0.013*** | | |
| | | | (9.546) | (-5.581) | | |
| Selection Skill | | | 0.004* | 0.000 | | |
| | | | (1.886) | (-0.405) | | |
| Timing Rate | | | 0.001 | -0.001 | | |
| | | | (0.968) | (-1.435) | | |
| Anchoring Bias | | | | | 0.022*** | 0.000 |
| | | | | | (12.976) | (-0.013) |
| Total Turnover | | | | | -0.031*** | 0.017*** |
| | | | | | (-14.565) | (7.329) |
| Attention Bias | | | | | -0.009*** | -0.002*** |
| | | | | | (-6.870) | (-2.581) |
| Disposition Effect | | | | | 0.001* | 0.002*** |
| | | | | | (1.751) | (5.882) |
| Underdiversification (HHI) | | | | | -0.066*** | 0.017*** |
| | | | | | (-99.536) | (4.912) |
| Extrapolation Bias | | | | | -0.005*** | 0.000 |
| | | | | | (-2.731) | (0.356) |
| Gambling Bias | | | | | -0.009*** | -0.002*** |
| | | | | | (-6.546) | (-2.621) |
| Confirmation Bias | | | | | 0.022*** | -0.006*** |
| | | | | | (29.161) | (-4.410) |
| Herding Bias | | | | | -0.006*** | -0.001 |
| | | | | | (-7.322) | (-1.631) |
| Months Since Reg. (t-1) | 0.012*** | | 0.008*** | | 0.014*** | |
| | (10.538) | | (7.800) | | (11.047) | |
| Inverse Mills Ratio | | 0.110*** | | 0.136*** | | 0.065* |
| | | (3.838) | | (3.209) | | (1.951) |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ / $R^2$ | — | 0.088 | — | 0.092 | — | 0.107 |
| N | 4,799,616 | 1,564,155 | 4,471,044 | 1,539,119 | 3,214,908 | 1,199,373 |

*Notes to Panel B.* Outcome variable is RA assets divided by total financial assets (RA/TFA) — the share-of-wealth intensive margin (as opposed to the main paper's RA/(RA+MF), which is the within-risky-portfolio share). Heckman two-stage with Fama–MacBeth monthly estimation and Newey–West HAC $t$-statistics (max lag = 3) in parentheses. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$. Relative to the main-paper RA/(RA+MF) outcome (Table 2), the RA/TFA formulation shows substantially smaller coefficient magnitudes because it dilutes variation over all financial assets rather than just the risky sleeve, but the sign patterns on Risk Preference (+), Age (−), Female (−), and Baby Assets (−) are preserved. The bias coefficients on Total Turnover and Underdiversification both enter with positive signs in the RA/TFA outcome, consistent with the main-paper finding that high-turnover and concentrated investors hold larger absolute RA positions.

Table A.6: Control-Function Panel Heckman Estimates of Robo-Advisor Allocation

| | Baseline ($T = 48$) | | +Skills ($T = 47$) | | +Biases ($T = 35$) | |
|---|---|---|---|---|---|---|
| Variable | Entity | Entity+Time | Entity | Entity+Time | Entity | Entity+Time |
| *Panel A: RA Share* $= RA/(RA + MF)$ | | | | | | |
| Log(Total Financial Assets)$_{t-1}$ | $-0.162^{***}$ | $-0.141^{***}$ | $-0.151^{***}$ | $-0.120^{***}$ | $-0.144^{***}$ | $-0.118^{***}$ |
| | $(-25.235)$ | $(-30.240)$ | $(-31.405)$ | $(-21.821)$ | $(-43.568)$ | $(-33.191)$ |
| Combined RA Dummy$_{t-1}$ | $-0.011^{***}$ | $-0.000$ | $-0.002$ | 0.006 | $-0.017^{***}$ | $-0.000$ |
| | $(-2.906)$ | $(-0.074)$ | $(-0.580)$ | (1.440) | $(-5.689)$ | $(-0.085)$ |
| Baby Assets$_{t-1}$ | $0.018^{***}$ | $0.016^{***}$ | $0.017^{***}$ | $0.014^{***}$ | $0.012^{***}$ | $0.010^{***}$ |
| | (23.442) | (22.398) | (26.690) | (19.443) | (24.477) | (19.864) |
| Fund Experience (Months)$_{t-1}$ | | | $0.010^{***}$ | $-0.259^{***}$ | | |
| | | | (5.415) | $(-17.450)$ | | |
| Selection Ability$_{t-1}$ | | | 0.000 | $-0.000$ | | |
| | | | (0.416) | $(-0.755)$ | | |
| Timing Ability$_{t-1}$ | | | $-0.000$ | $-0.000$ | | |
| | | | $(-0.092)$ | $(-1.078)$ | | |
| Anchoring Bias$_{t-1}$ | | | | | $-0.006^{***}$ | $-0.006^{***}$ |
| | | | | | $(-10.298)$ | $(-10.855)$ |
| Total Turnover$_{t-1}$ | | | | | $0.040^{***}$ | $0.041^{***}$ |
| | | | | | (25.156) | (26.169) |
| Attention Bias$_{t-1}$ | | | | | $-0.002^{***}$ | $-0.001^{***}$ |
| | | | | | $(-3.991)$ | $(-3.238)$ |
| Disposition Effect$_{t-1}$ | | | | | $-0.001$ | $-0.001^{*}$ |
| | | | | | $(-1.215)$ | $(-1.752)$ |
| Underdiversification HHI$_{t-1}$ | | | | | $0.045^{***}$ | $0.032^{***}$ |
| | | | | | (27.657) | (17.379) |
| Extrapolation Bias$_{t-1}$ | | | | | $-0.001^{***}$ | $-0.000$ |
| | | | | | $(-2.762)$ | $(-1.337)$ |
| Gambling Bias$_{t-1}$ | | | | | $0.002^{***}$ | $0.002^{***}$ |
| | | | | | (4.341) | (4.691) |
| Confirmation Bias$_{t-1}$ | | | | | $-0.015^{***}$ | $-0.014^{***}$ |
| | | | | | $(-15.545)$ | $(-14.724)$ |
| Herding Bias$_{t-1}$ | | | | | $-0.003^{***}$ | $-0.002^{***}$ |
| | | | | | $(-8.560)$ | $(-4.317)$ |
| Inverse Mills Ratio ($\lambda$) | $-0.014$ | $0.036^{***}$ | 0.020 | $0.064^{***}$ | $-0.118^{***}$ | $-0.020^{*}$ |
| | $(-0.774)$ | (2.967) | (1.462) | (3.961) | $(-19.540)$ | $(-1.887)$ |
| Demographics (risk, age, female) | | | Absorbed by entity FE | | | |
| Entity (investor) FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time (month) FE | No | Yes | No | Yes | No | Yes |
| $N$ (selected) | 1,564,155 | 1,564,155 | 1,539,119 | 1,539,119 | 1,199,373 | 1,199,373 |
| *Panel B: Risky RA Share* $= RA_{\text{risky}}/(RA_{\text{risky}} + MF_{\text{risky}})$ | | | | | | |
| Log(Total Financial Assets)$_{t-1}$ | $-0.178^{***}$ | $-0.159^{***}$ | $-0.171^{***}$ | $-0.140^{***}$ | $-0.160^{***}$ | $-0.135^{***}$ |
| | $(-24.818)$ | $(-30.313)$ | $(-31.635)$ | $(-24.986)$ | $(-44.668)$ | $(-34.180)$ |
| Combined RA Dummy$_{t-1}$ | $-0.013^{***}$ | $-0.004$ | $-0.004$ | 0.003 | $-0.021^{***}$ | $-0.005$ |
| | $(-2.698)$ | $(-0.920)$ | $(-1.077)$ | (0.662) | $(-6.013)$ | $(-1.507)$ |
| Baby Assets$_{t-1}$ | $0.021^{***}$ | $0.019^{***}$ | $0.020^{***}$ | $0.017^{***}$ | $0.014^{***}$ | $0.012^{***}$ |
| | (22.709) | (22.315) | (26.760) | (21.006) | (25.618) | (21.984) |
| Fund Experience (Months)$_{t-1}$ | | | $0.009^{***}$ | $-0.245^{***}$ | | |
| | | | (4.236) | $(-16.457)$ | | |
| Selection Ability$_{t-1}$ | | | 0.000 | $-0.001$ | | |
| | | | (0.314) | $(-1.279)$ | | |
| Timing Ability$_{t-1}$ | | | $-0.000$ | $-0.001$ | | |
| | | | $(-0.054)$ | $(-1.299)$ | | |
| Anchoring Bias$_{t-1}$ | | | | | $-0.007^{***}$ | $-0.007^{***}$ |
| | | | | | $(-10.757)$ | $(-11.302)$ |
| Total Turnover$_{t-1}$ | | | | | $0.045^{***}$ | $0.046^{***}$ |
| | | | | | (25.143) | (26.188) |
| Attention Bias$_{t-1}$ | | | | | $-0.002^{***}$ | $-0.001^{***}$ |
| | | | | | $(-4.035)$ | $(-3.464)$ |
| Disposition Effect$_{t-1}$ | | | | | $-0.000$ | $-0.001$ |
| | | | | | $(-0.684)$ | $(-1.120)$ |
| Underdiversification HHI$_{t-1}$ | | | | | $0.052^{***}$ | $0.038^{***}$ |
| | | | | | (29.067) | (18.500) |

*Continued on next page*

*Table A.6 (continued)*

| | Baseline ($T = 48$) | | +Skills ($T = 47$) | | +Biases ($T = 35$) | |
|---|---|---|---|---|---|---|
| Variable | Entity | Entity+Time | Entity | Entity+Time | Entity | Entity+Time |
| Extrapolation Bias$_{t-1}$ | | | | | $-0.001^{***}$ | $-0.000$ |
| | | | | | $(-2.583)$ | $(-1.461)$ |
| Gambling Bias$_{t-1}$ | | | | | $0.002^{***}$ | $0.002^{***}$ |
| | | | | | (4.377) | (5.031) |
| Confirmation Bias$_{t-1}$ | | | | | $-0.015^{***}$ | $-0.015^{***}$ |
| | | | | | $(-17.142)$ | $(-16.464)$ |
| Herding Bias$_{t-1}$ | | | | | $-0.004^{***}$ | $-0.002^{***}$ |
| | | | | | $(-8.579)$ | $(-4.922)$ |
| Inverse Mills Ratio ($\lambda$) | $-0.017$ | $0.024^{*}$ | 0.012 | $0.055^{***}$ | $-0.145^{***}$ | $-0.048^{***}$ |
| | $(-0.840)$ | (1.651) | (0.714) | (3.089) | $(-21.095)$ | $(-4.107)$ |
| Demographics (risk, age, female) | | | Absorbed by entity FE | | | |
| Entity (investor) FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time (month) FE | No | Yes | No | Yes | No | Yes |
| $N$ (selected) | 1,564,316 | 1,564,316 | 1,539,280 | 1,539,280 | 1,199,511 | 1,199,511 |

*Notes.* The table reports Wooldridge (1995) / Semykina and Wooldridge (2010) control-function panel selection estimates. The dependent variable in Panel A is the robo-advisor allocation share $ra_share_{i,t} = RA_{i,t}/(RA_{i,t}+MF_{i,t})$, observed on robo-advisor-active investor-months; in Panel B it is the risky allocation share $RA_{\text{risky}}/(RA_{\text{risky}}+MF_{\text{risky}})$, observed on months with positive risky robo-advisor holdings. The two dependent variables use different selection rules, so their selected samples differ slightly. Stage 1 is a per-calendar-month cross-sectional probit of the selection indicator on the specification regressors, months-since-registration (the exclusion restriction), and Mundlak per-investor time-averages of the time-varying regressors. The inverse Mills ratio $\widehat{\lambda}_{i,t} = \phi(Z\widehat{\gamma})/\Phi(Z\widehat{\gamma})$ is formed for selected observations and merged onto the panel. Stage 2 is a linear panel regression on the selected sample with $\widehat{\lambda}$ added as a regressor. For each specification the Stage-2 model is estimated twice on a common Stage-1 inverse Mills ratio: "Entity" includes investor fixed effects only; "Entity+Time" adds month fixed effects. Province controls are absorbed by the investor fixed effect and are not separately included. Each variable spans two rows: the coefficient and, in parentheses below, the two-way (investor and month) cluster-robust $t$-statistic. This $t$ is not the Newey–West / Fama–MacBeth statistic reported in the main tables; the estimator and the standard errors differ, so the notation deliberately differs. The inverse Mills ratio coefficient equals $\delta = \rho\sigma$ and is the control-function selection test: if insignificant, selection adds nothing beyond the fixed effects and the uncorrected fixed-effects estimator is consistent; if significant, its sign carries the direction of the selection bias. Risk preference, age, and the female indicator are time-invariant within investor and are absorbed by the entity fixed effect; they cannot be estimated in this design. Continuous regressors are winsorised at 1/99 and z-scored over the pooled sample (coefficients are per one pooled standard deviation), which differs from the within-month standardisation in the main Fama–MacBeth tables; levels are therefore not directly comparable across the two, though signs and significance are. All regressors enter at $t-1$ except the (absorbed) contemporaneous demographics. The +Biases panel survives in 35 of 50 months because its larger probit loses months to the pre-flight screens. The reported within $R^2$ is the relevant fit statistic; between and overall $R^2$ from the entity (and time) demeaning are out-of-transform and routinely negative, and are not reported. The Entity+Time within $R^2$ for the +Skills specification is omitted (marked n/a) as out-of-transform with the generated regressor. The Stage-2 clustered standard errors treat $\widehat{\lambda}$ as observed and do not account for its being a generated regressor; they understate uncertainty, most acutely on the inverse Mills ratio coefficient itself. Rigorous inference would require an investor-block bootstrap over both stages. The inverse Mills ratio $t$-statistics in particular should be read as approximate. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed).

Table A.7: Standalone Automated-Trading (Auto-Invest) Heckman Results

| | BASE | | SKILL | | BIAS | |
|---|---|---|---|---|---|---|
| Variable | Selection ME | Outcome | Selection ME | Outcome | Selection ME | Outcome |
| Constant | -0.185*** | 1.619*** | -0.177*** | 1.668*** | -0.205*** | 1.190*** |
| | (-8.840) | (17.826) | (-7.254) | (14.387) | (-7.689) | (66.054) |
| Log(TFA) (t-1) | 0.122*** | -0.305*** | 0.102*** | -0.303*** | 0.057*** | -0.173*** |
| | (11.100) | (-9.439) | (9.780) | (-8.129) | (4.752) | (-11.584) |
| Risk Preference | 0.015*** | -0.025*** | 0.008** | -0.018*** | 0.012*** | -0.005** |
| | (4.170) | (-4.825) | (2.473) | (-4.048) | (5.369) | (-2.026) |
| Age | 0.030*** | -0.024*** | 0.029*** | -0.024*** | 0.030*** | -0.007*** |
| | (14.492) | (-5.706) | (13.596) | (-5.802) | (12.002) | (-3.132) |
| Female | 0.000 | 0.032*** | 0.002 | 0.031*** | -0.026*** | 0.039*** |
| | (0.011) | (3.555) | (0.264) | (3.094) | (-9.515) | (19.958) |
| Combined RA Dummy (t-1) | 0.079*** | -0.093*** | 0.071*** | -0.088*** | 0.058*** | -0.018* |
| | (18.080) | (-18.587) | (12.955) | (-13.421) | (13.297) | (-1.882) |
| Baby Assets (t-1) | -0.038*** | 0.058*** | -0.030*** | 0.054*** | -0.012*** | 0.021*** |
| | (-14.895) | (14.384) | (-31.046) | (12.818) | (-11.225) | (13.481) |
| Fund Experience (mos) | | | 0.044*** | -0.043*** | | |
| | | | (8.838) | (-5.444) | | |
| Selection Skill | | | 0.005** | -0.011*** | | |
| | | | (2.353) | (-2.833) | | |
| Timing Rate | | | 0.001 | -0.001 | | |
| | | | (0.181) | (-0.115) | | |
| Anchoring Bias | | | | | 0.011 | -0.006** |
| | | | | | (0.874) | (-2.552) |
| Total Turnover | | | | | -0.088*** | -0.064*** |
| | | | | | (-21.371) | (-15.355) |
| Attention Bias | | | | | -0.023*** | 0.000 |
| | | | | | (-4.322) | (0.091) |
| Disposition Effect | | | | | -0.009*** | 0.006 |
| | | | | | (-4.911) | (1.382) |
| Underdiversification (HHI) | | | | | -0.034*** | 0.088*** |
| | | | | | (-18.115) | (24.700) |
| Extrapolation Bias | | | | | -0.008** | -0.003 |
| | | | | | (-2.020) | (-0.820) |
| Gambling Bias | | | | | -0.005 | -0.004 |
| | | | | | (-1.153) | (-1.309) |
| Confirmation Bias | | | | | 0.009*** | 0.018*** |
| | | | | | (4.277) | (5.015) |
| Herding Bias | | | | | 0.010*** | -0.006*** |
| | | | | | (3.287) | (-7.164) |
| Months Since Reg. (t-1) | 0.030*** | | 0.028*** | | 0.026*** | |
| | (16.686) | | (14.240) | | (13.069) | |
| Inverse Mills Ratio | | -0.714*** | | -0.757*** | | -0.342*** |
| | | (-16.883) | | (-13.292) | | (-20.045) |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Pseudo $R^2$ / $R^2$ | 0.057 | 0.064 | 0.053 | 0.072 | 0.096 | 0.142 |
| N | 4,799,616 | 1,583,226 | 4,471,044 | 1,528,452 | 3,214,908 | 984,505 |

*Notes.* Heckman two-stage model with **automated-trading (Auto-Invest) participation** as the selection variable and the **auto-invest share** of fund purchases as the outcome. First stage: $\text{Auto}_{it} = 1$ if the investor used auto-invest purchase plans in month $t$. Outcome: $\text{auto_share}_{it}$ = fraction of total fund purchases executed via auto-invest (conditional on Auto $= 1$). Three nested specifications mirror the main-paper Table 2: BASE (6 demographics), SKILL (adds Fund Experience, Selection Skill, Timing Rate), BIAS (adds 9 biases). Selection (first-stage) values are approximate marginal effects on the probability of auto-invest participation, $\text{ME}_k = \phi(\Phi^{-1}(\bar{P}))\hat{\beta}_k$, scaled per specification by $\bar{P} = 0.330$ (BASE), $0.342$ (SKILL), $0.306$ (BIAS) on the 4.9-million investor-month panel, with factors $0.362$, $0.367$, $0.351$; outcome-stage coefficients are unscaled and $t$-statistics and significance are invariant to the scaling. Estimated monthly via Fama–MacBeth with Newey–West HAC $t$-statistics (max lag $= 3$) in parentheses. The BIAS sample drops to 35 months because bias construction requires longer fund-holding histories. Comparison with the RA Heckman (main Table 2) reveals the sign-flipping pattern that motivates Table 4 in the main paper: on **total turnover**, the coefficient is strongly negative for Auto ($-0.064$, $t = -15.4$) but strongly positive for RA ($+0.041$, $t = 6.2$ large). Underdiversification enters positively in both the Auto-Invest outcome equation (+0.088) and the RA outcome equation (+0.020 in Table 1 Panel 3). The Auto-Invest outcome coefficient is much larger in magnitude, and the within-investor-month stacking design in Table 4 confirms a relative tilt toward Auto-Invest. That is, concentrated investors delegate meaningfully to RA when they adopt, but they delegate relatively more to Auto-Invest in product comparison. These sign flips in the stacked-product analysis identify behavioral biases as driving the auto-vs.-RA choice in opposite directions. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

Table A.8: Mutual Fund Performance Controls in the RA Heckman

| *DV (outcome):* RA share = RA / (RA + MF) / *Selection:* $\text{RA}_{it} = 1$ | | | |
|---|---|---|---|
| Fund-performance measure | Lag-1 month | Cumulative 3-month | Cumulative 6-month |
| **Panel A: Outcome-equation coefficient on fund performance (RA share)** | | | |
| Excess return vs CSI300 | −0.002 | −0.001 | −0.001 |
| | (−1.114) | (−0.358) | (−0.451) |
| $\beta$-adjusted excess vs CSI300 | 0.000 | 0.002* | 0.002* |
| | (0.545) | (1.929) | (1.721) |
| Category-adjusted return | 0.001 | 0.002** | 0.003** |
| | (1.522) | (2.089) | (2.180) |
| **Panel B: Selection-equation coefficient on fund performance (probit ME, Combined RA)** | | | |
| Excess return vs CSI300 | 0.001 | 0.003 | 0.004 |
| | (0.196) | (0.291) | (0.341) |
| $\beta$-adjusted excess vs CSI300 | 0.006 | 0.009 | 0.013 |
| | (0.983) | (0.883) | (1.051) |
| Category-adjusted return | 0.008 | 0.016** | 0.022** |
| | (1.400) | (1.973) | (2.397) |
| **Panel C: Heckman structural parameters (outcome equation)** | | | |
| Inverse Mills Ratio $\lambda$ (Exc CSI300) | 0.090** | 0.098** | 0.088*** |
| | (1.983) | (2.526) | (3.167) |
| Inverse Mills Ratio $\lambda$ ($\beta$-adj) | 0.006 | 0.026* | 0.040** |
| | (0.424) | (1.646) | (2.185) |
| Inverse Mills Ratio $\lambda$ (Cat-adj) | 0.088** | 0.098** | 0.087*** |
| | (2.101) | (2.552) | (3.069) |
| Demographic controls | Yes | Yes | Yes |
| Province dummies | Yes | Yes | Yes |
| T (months, Exc CSI300) | 48 | 47 | 45 |
| N outcome (Exc CSI300) | 1,498,676 | 1,486,037 | 1,447,264 |
| Pseudo $R^2$ / $R^2$ (outcome, Exc) | 0.085 | 0.088 | 0.087 |

*Notes.* Heckman two-stage with Fama–MacBeth monthly estimation and Newey–West HAC $t$-statistics (max lag $= 3$) in parentheses. Each cell in Panels A and B is a separate Heckman specification: the three performance measures (rows) interact with three horizons (columns) to yield 9 specifications. Each specification includes the full demographic control set (Log(TFA), Risk Preference, Age, Female, Combined RA Dummy, Baby Assets; months-since-registration as the exclusion restriction in the selection equation) plus province dummies; only the performance coefficient is shown. *Performance measures*: (i) Excess Return vs CSI300 $= r_t^{\text{fund}} - r_t^{\text{CSI300}}$; (ii) $\beta$-adjusted excess $= r_t^{\text{fund}} - \hat{\beta}_i \cdot r_t^{\text{CSI300}}$; (iii) Category-adjusted $= r_t^{\text{fund}} - \bar{r}_t^{\text{peer}}$ (peer-group excess). All returns winsorised at 1%/99% and standardised within month. Sample size and $T$ differ modestly across the three performance measures: $\beta$-adjusted specifications have shorter $T$ (37, 36, 34 for L1/C3/C6 respectively) because $\beta$ estimation requires a minimum history. Only the Excess-vs-CSI300 row's $T$ and $N$ are reported in the footer. *Headline.* Fund-performance coefficients in the RA share equation are economically tiny (magnitudes $\leq 0.003$) across all 9 specifications. Only category-adjusted returns at the 3- and 6-month horizons reach borderline 5% significance, and even then the point estimates are below 0.003. The Heckman selection term $\hat{\lambda}$ is consistently positive and significant for Exc-CSI300 and Cat-adj specifications, indicating selection on unobservables that positively affect both RA participation and RA share. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

Table A.9: Robo-Advisor Performance Controls in the RA Heckman

| *DV (outcome):* RA share = RA / (RA + MF) / *Selection:* $\text{RA}_{it} = 1$ | | | |
|---|---|---|---|
| RA-performance measure | Lag-1 month | Cumulative 3-month | Cumulative 6-month |
| **Panel A: Outcome-equation coefficient on RA performance (RA share)** | | | |
| Excess return vs CSI300 | 0.003 | 0.001 | −0.002 |
| | (0.950) | (0.485) | (−0.835) |
| $\beta$-adjusted excess vs CSI300 | 0.002 | −0.001 | −0.005* |
| | (0.535) | (−0.275) | (−1.685) |
| Strategy-adjusted return | 0.007*** | 0.008** | 0.008** |
| | (2.946) | (2.541) | (2.387) |
| Demographic controls | Yes | Yes | Yes |
| Province dummies | Yes | Yes | Yes |
| T (months) | 48 | 48 | 48 |
| N outcome (Exc CSI300) | 1,387,457 | 1,327,953 | 1,206,106 |
| Pseudo $R^2$ / $R^2$ (outcome, Exc) | 0.144 | 0.143 | 0.142 |

*Notes.* Same empirical design as Table A.8 but substituting **RA** return measures for fund return measures in the outcome equation. RA returns are observed only conditional on RA participation, so the RA-performance measure does not enter the selection equation (which is identical across the 9 specs because it has the same regressors as the baseline). Each cell is a separate Heckman specification; each contains the full demographic control set (Log(TFA), Risk Preference, Age, Female, Combined RA Dummy, Baby Assets; months-since-registration as exclusion restriction). Fama–MacBeth with Newey–West HAC $t$-statistics (max lag = 3) in parentheses. *Performance measures*: (i) Excess return vs CSI300; (ii) $\beta$-adjusted excess; (iii) Strategy-adjusted $= r_t^{\text{RA}} - \bar{r}_t^{\text{strategy peer}}$ where the peer group is all RA strategies of similar target-risk allocation. All returns winsorised at 1%/99% and standardised within month. *Headline.* As with fund performance (Table A.8), RA-return coefficients are small: Excess and $\beta$-adjusted returns have coefficients below $|0.005|$ across all horizons. Only **strategy-adjusted** RA returns have significantly positive coefficients (0.007–0.008), consistent with investors allocating more to RA *strategies* that beat their peer set — i.e., a within-RA selection effect — rather than to the RA channel as a whole beating the market. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

Table A.10: Decision Authority and Product Choice: Pre-Use Horizon and Sample Robustness

| Variable | Horizon | Product-step coefficients: DIRECT → AUTO ($\gamma_T$) | Product-step coefficients: AUTO → RA ($\gamma_S$) | Equality test: $H_0 : \gamma_T = \gamma_S$: $\gamma_T - \gamma_S$ | $\chi^2(1)$ | $p$-value |
|---|---|---|---|---|---|---|
| **Panel A. Composite indices** | | | | | | |
| Skill composite, authority-bias composite, placebo-bias composite, and pre-use engagement. | | | | | | |
| *Mechanism variables* | | | | | | |
| Skill composite index | $L = 3$ | 0.001 (0.005) | 0.008 (0.006) | -0.007 | 0.477 | 0.490 |
| | $L = 9$ | -0.004 (0.006) | 0.016** (0.007) | -0.020 | 3.073 | 0.080 |
| | $L = 12$ | -0.008 (0.006) | 0.016** (0.007) | -0.023 | 3.760 | 0.053 |
| | $L = 6$, full panel | 0.004 (0.005) | 0.011* (0.006) | -0.006 | 0.409 | 0.522 |
| Authority-bias composite index | $L = 3$ | -0.005 (0.005) | -0.019*** (0.005) | 0.015 | 2.338 | 0.126 |
| | $L = 9$ | -0.004 (0.006) | -0.023*** (0.006) | 0.019 | 2.848 | 0.092 |
| | $L = 12$ | -0.002 (0.006) | -0.023*** (0.007) | 0.020 | 2.993 | 0.084 |
| | $L = 6$, full panel | 0.000 (0.005) | -0.022*** (0.005) | 0.022 | 5.085 | 0.024 |
| Placebo-bias composite index | $L = 3$ | -0.013** (0.006) | 0.006 (0.006) | -0.019 | 3.107 | 0.078 |
| | $L = 9$ | -0.010 (0.007) | -0.001 (0.007) | -0.009 | 0.539 | 0.463 |
| | $L = 12$ | -0.014* (0.007) | 0.004 (0.007) | -0.018 | 1.839 | 0.175 |
| | $L = 6$, full panel | -0.009 (0.006) | 0.003 (0.006) | -0.012 | 1.099 | 0.294 |
| Pre-use login frequency | $L = 3$ | -0.040*** (0.006) | 0.011* (0.006) | -0.051 | 25.263 | <0.001 |
| | $L = 9$ | -0.040*** (0.006) | 0.015** (0.006) | -0.056 | 25.354 | <0.001 |
| | $L = 12$ | -0.038*** (0.006) | 0.012* (0.007) | -0.050 | 17.890 | <0.001 |
| | $L = 6$, full panel | -0.035*** (0.006) | 0.013** (0.006) | -0.048 | 21.274 | <0.001 |
| *Product-step intercepts* | | | | | | |
| Automation ($c_T$) / delegation ($c_S$) | $L = 3$ | -0.596*** (0.018) | 0.297*** (0.017) | — | — | — |
| | $L = 9$ | -0.601*** (0.019) | 0.325*** (0.018) | — | — | — |
| | $L = 12$ | -0.622*** (0.019) | 0.370*** (0.017) | — | — | — |
| | $L = 6$, full panel | -0.543*** (0.015) | 0.287*** (0.016) | — | — | — |
| *Baseline-control interactions* | | | | | | |
| Log total financial assets | $L = 3$ | -0.023** (0.010) | 0.084*** (0.008) | — | — | — |
| | $L = 9$ | -0.035*** (0.011) | 0.091*** (0.008) | — | — | — |
| | $L = 12$ | -0.047*** (0.012) | 0.095*** (0.009) | — | — | — |
| | $L = 6$, full panel | -0.057*** (0.006) | 0.080*** (0.006) | — | — | — |
| Risk preference | $L = 3$ | -0.013*** (0.004) | 0.031*** (0.006) | — | — | — |
| | $L = 9$ | -0.018*** (0.005) | 0.034*** (0.006) | — | — | — |
| | $L = 12$ | -0.024*** (0.005) | 0.035*** (0.007) | — | — | — |
| | $L = 6$, full panel | -0.013*** (0.004) | 0.030*** (0.006) | — | — | — |
| Age | $L = 3$ | 0.005 (0.004) | 0.017*** (0.006) | — | — | — |
| | $L = 9$ | -0.007 (0.005) | 0.029*** (0.006) | — | — | — |
| | $L = 12$ | -0.012** (0.005) | 0.034*** (0.006) | — | — | — |
| | $L = 6$, full panel | 0.001 (0.004) | 0.022*** (0.005) | — | — | — |
| Female | $L = 3$ | 0.033*** (0.011) | -0.043*** (0.015) | — | — | — |
| | $L = 9$ | 0.038*** (0.013) | -0.043** (0.017) | — | — | — |
| | $L = 12$ | 0.047*** (0.013) | -0.054*** (0.018) | — | — | — |
| | $L = 6$, full panel | 0.027** (0.011) | -0.036** (0.015) | — | — | — |
| Baby assets | $L = 3$ | 0.010* (0.005) | -0.005 (0.006) | — | — | — |
| | $L = 9$ | 0.014** (0.006) | -0.006 (0.006) | — | — | — |
| | $L = 12$ | 0.014** (0.006) | -0.005 (0.006) | — | — | — |
| | $L = 6$, full panel | 0.020*** (0.005) | -0.006 (0.005) | — | — | — |
| *Model fit* | | | | | | |
| Within $R^2$ | $L = 3$: 0.2634; $L = 9$: 0.2719; $L = 12$: 0.2943; $L = 6$, full panel: 0.2470 | | | | | |
| Overall $R^2$ | $L = 3$: -0.6263; $L = 9$: -0.6032; $L = 12$: -0.5924; $L = 6$, full panel: -0.5673 | | | | | |
| Within-$X$ condition number | $L = 3$: 5.312; $L = 9$: 5.447; $L = 12$: 5.599; $L = 6$, full panel: 5.390 | | | | | |
| **Panel B. Disaggregated skill and authority biases** | | | | | | |
| Selection and timing ability; confirmation, turnover, speculative-turnover, herding, and disposition measures. | | | | | | |
| *Mechanism variables* | | | | | | |
| Selection ability | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.011* (0.006) | 0.013** (0.006) | -0.024 | 5.309 | 0.021 |
| | $L = 12$ | -0.018*** (0.006) | 0.012* (0.007) | -0.030 | 6.610 | 0.010 |
| | $L = 6$, full panel | -0.004 (0.005) | 0.012** (0.005) | -0.015 | 2.702 | 0.100 |
| Timing ability | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.002 (0.005) | 0.010 (0.006) | -0.007 | 0.510 | 0.475 |
| | $L = 12$ | 0.001 (0.006) | 0.010 (0.006) | -0.010 | 0.751 | 0.386 |
| | $L = 6$, full panel | 0.008 (0.005) | 0.003 (0.005) | 0.005 | 0.265 | 0.607 |
| Confirmation bias | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.040*** (0.007) | 0.007 (0.008) | 0.033 | 5.395 | 0.020 |
| | $L = 12$ | 0.045*** (0.008) | -0.004 (0.009) | 0.049 | 10.449 | 0.001 |
| | $L = 6$, full panel | 0.029*** (0.007) | 0.012* (0.007) | 0.018 | 2.003 | 0.157 |
| Total turnover | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.038*** (0.008) | -0.036*** (0.008) | 0.074 | 30.484 | <0.001 |
| | $L = 12$ | 0.047*** (0.008) | -0.041*** (0.009) | 0.088 | 35.074 | <0.001 |
| | $L = 6$, full panel | 0.032*** (0.007) | -0.031*** (0.006) | 0.062 | 30.975 | <0.001 |

*Continued on next page*

*Table A.10 (continued)*

| Variable | Horizon | Product-step coefficients: DIRECT → AUTO ($\gamma_T$) | Product-step coefficients: AUTO → RA ($\gamma_S$) | Equality test: $\gamma_T - \gamma_S$ | $\chi^2(1)$ | $p$-value |
|---|---|---|---|---|---|---|
| Speculative turnover | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.036*** (0.006) | -0.008 (0.006) | -0.028 | 6.973 | 0.008 |
| | $L = 12$ | -0.037*** (0.006) | -0.005 (0.006) | -0.032 | 8.510 | 0.004 |
| | $L = 6$, full panel | -0.026*** (0.005) | -0.012** (0.005) | -0.014 | 2.136 | 0.144 |
| Herding bias | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.015** (0.007) | -0.002 (0.006) | -0.013 | 1.218 | 0.270 |
| | $L = 12$ | -0.017** (0.007) | 0.001 (0.007) | -0.018 | 2.006 | 0.157 |
| | $L = 6$, full panel | -0.012** (0.006) | -0.001 (0.006) | -0.011 | 1.106 | 0.293 |
| Disposition effect | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.006 (0.006) | -0.009 (0.006) | 0.015 | 1.701 | 0.192 |
| | $L = 12$ | 0.006 (0.006) | -0.011 (0.007) | 0.017 | 1.918 | 0.166 |
| | $L = 6$, full panel | 0.004 (0.005) | -0.008 (0.006) | 0.012 | 1.398 | 0.237 |
| *Product-step intercepts* | | | | | | |
| Automation ($c_T$) / delegation ($c_S$) | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.604*** (0.009) | 0.333*** (0.011) | — | — | — |
| | $L = 12$ | -0.627*** (0.007) | 0.378*** (0.009) | — | — | — |
| | $L = 6$, full panel | -0.546*** (0.007) | 0.295*** (0.012) | — | — | — |
| *Baseline-control interactions* | | | | | | |
| Log total financial assets | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.035*** (0.011) | 0.080*** (0.028) | — | — | — |
| | $L = 12$ | -0.045*** (0.012) | 0.083*** (0.028) | — | — | — |
| | $L = 6$, full panel | -0.058*** (0.010) | 0.069*** (0.020) | — | — | — |
| Risk preference | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.021*** (0.006) | 0.034*** (0.007) | — | — | — |
| | $L = 12$ | -0.027*** (0.006) | 0.034*** (0.007) | — | — | — |
| | $L = 6$, full panel | -0.016*** (0.006) | 0.030*** (0.006) | — | — | — |
| Age | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.008 (0.006) | 0.030*** (0.006) | — | — | — |
| | $L = 12$ | -0.013** (0.006) | 0.034*** (0.007) | — | — | — |
| | $L = 6$, full panel | 0.001 (0.005) | 0.022*** (0.006) | — | — | — |
| Female | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.045** (0.020) | -0.053*** (0.020) | — | — | — |
| | $L = 12$ | 0.056*** (0.021) | -0.064*** (0.021) | — | — | — |
| | $L = 6$, full panel | 0.034* (0.017) | -0.046** (0.018) | — | — | — |
| Baby assets | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.014*** (0.005) | -0.004 (0.004) | — | — | — |
| | $L = 12$ | 0.014*** (0.005) | -0.004 (0.005) | — | — | — |
| | $L = 6$, full panel | 0.019*** (0.004) | -0.004 (0.004) | — | — | — |
| *Model fit* | | | | | | |
| Within $R^2$ | $L = 3$: —; $L = 9$: 0.2749; $L = 12$: 0.2973; $L = 6$, full panel: 0.2495 | | | | | |
| Overall $R^2$ | $L = 3$: —; $L = 9$: -0.6025; $L = 12$: -0.5920; $L = 6$, full panel: -0.5660 | | | | | |
| Within-$X$ condition number | $L = 3$: —; $L = 9$: 5.591; $L = 12$: 5.739; $L = 6$, full panel: 5.533 | | | | | |
| **Panel C. Placebo biases** | | | | | | |
| Under-diversification, gambling, anchoring, attention, and extrapolation biases. | | | | | | |
| *Mechanism variables* | | | | | | |
| Under-diversification (HHI) | $L = 3$ | — | — | 0.084 | 67.654 | <0.001 |
| | $L = 9$ | 0.056*** (0.007) | -0.036*** (0.006) | 0.092 | 64.257 | <0.001 |
| | $L = 12$ | 0.055*** (0.007) | -0.034*** (0.006) | 0.089 | 53.878 | <0.001 |
| | $L = 6$, full panel | 0.050*** (0.006) | -0.030*** (0.005) | 0.080 | 58.697 | <0.001 |
| Gambling bias | $L = 3$ | — | — | 0.003 | 0.097 | 0.756 |
| | $L = 9$ | 0.002 (0.007) | -0.015** (0.007) | 0.017 | 1.709 | 0.191 |
| | $L = 12$ | -0.001 (0.008) | -0.007 (0.007) | 0.005 | 0.153 | 0.695 |
| | $L = 6$, full panel | 0.000 (0.006) | -0.012** (0.006) | 0.012 | 1.211 | 0.271 |
| Anchoring bias | $L = 3$ | — | — | -0.087 | 67.767 | <0.001 |
| | $L = 9$ | -0.044*** (0.007) | 0.053*** (0.006) | -0.097 | 61.550 | <0.001 |
| | $L = 12$ | -0.049*** (0.007) | 0.063*** (0.007) | -0.111 | 66.223 | <0.001 |
| | $L = 6$, full panel | -0.036*** (0.006) | 0.045*** (0.006) | -0.081 | 57.155 | <0.001 |
| Attention bias | $L = 3$ | — | — | -0.064 | 32.332 | <0.001 |
| | $L = 9$ | -0.042*** (0.007) | 0.023*** (0.007) | -0.065 | 25.254 | <0.001 |
| | $L = 12$ | -0.041*** (0.008) | 0.021*** (0.007) | -0.062 | 19.683 | <0.001 |
| | $L = 6$, full panel | -0.041*** (0.006) | 0.025*** (0.006) | -0.066 | 31.635 | <0.001 |
| Extrapolation bias | $L = 3$ | — | — | 0.023 | 4.242 | 0.039 |
| | $L = 9$ | 0.006 (0.007) | -0.019*** (0.007) | 0.025 | 4.070 | 0.044 |
| | $L = 12$ | 0.004 (0.008) | -0.020*** (0.007) | 0.024 | 2.958 | 0.086 |
| | $L = 6$, full panel | 0.010 (0.006) | -0.018*** (0.006) | 0.028 | 6.045 | 0.014 |
| *Product-step intercepts* | | | | | | |
| Automation ($c_T$) / delegation ($c_S$) | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.618*** (0.019) | 0.340*** (0.020) | — | — | — |
| | $L = 12$ | -0.639*** (0.019) | 0.387*** (0.019) | — | — | — |

*Continued on next page*

*Table A.10 (continued)*

| Variable | Horizon | Product-step coefficients | | Equality test: $H_0 : \gamma_T = \gamma_S$ | | |
|---|---|---|---|---|---|---|
| | | DIRECT → AUTO ($\gamma_T$) | AUTO → RA ($\gamma_S$) | $\gamma_T - \gamma_S$ | $\chi^2(1)$ | $p$-value |
| | $L = 6$, full panel | -0.556*** (0.015) | 0.299*** (0.019) | — | — | — |
| *Baseline-control interactions* | | | | | | |
| Log total financial assets | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.011 (n.a.) | 0.069*** (0.013) | — | — | — |
| | $L = 12$ | -0.023 (n.a.) | 0.070*** (0.013) | — | — | — |
| | $L = 6$, full panel | -0.039*** (0.001) | 0.064*** (0.010) | — | — | — |
| Risk preference | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.018*** (0.005) | 0.034*** (0.006) | — | — | — |
| | $L = 12$ | -0.023*** (0.005) | 0.034*** (0.007) | — | — | — |
| | $L = 6$, full panel | -0.013*** (0.004) | 0.030*** (0.005) | — | — | — |
| Age | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | -0.005 (0.005) | 0.028*** (0.006) | — | — | — |
| | $L = 12$ | -0.009* (0.005) | 0.032*** (0.006) | — | — | — |
| | $L = 6$, full panel | 0.003 (0.004) | 0.021*** (0.005) | — | — | — |
| Female | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.057*** (0.009) | -0.057*** (0.013) | — | — | — |
| | $L = 12$ | 0.066*** (0.009) | -0.069*** (0.013) | — | — | — |
| | $L = 6$, full panel | 0.043*** (0.009) | -0.047*** (0.012) | — | — | — |
| Baby assets | $L = 3$ | — | — | — | — | — |
| | $L = 9$ | 0.011** (0.005) | -0.003 (0.005) | — | — | — |
| | $L = 12$ | 0.011** (0.005) | -0.003 (0.005) | — | — | — |
| | $L = 6$, full panel | 0.017*** (0.005) | -0.005 (0.005) | — | — | — |
| *Model fit* | | | | | | |
| Within $R^2$ | $L = 3$: —; $L = 9$: 0.2758; $L = 12$: 0.2991; $L = 6$, full panel: 0.2505 | | | | | |
| Overall $R^2$ | $L = 3$: —; $L = 9$: -0.6035; $L = 12$: -0.5925; $L = 6$, full panel: -0.5674 | | | | | |
| Within-$X$ condition number | $L = 3$: —; $L = 9$: 5.558; $L = 12$: 5.724; $L = 6$, full panel: 5.477 | | | | | |
| **Panel D. Sample construction** | | | | | | |
| Stacked observations | $L = 3$: 766,254; $L = 9$: 593,190; $L = 12$: 496,575; $L = 6$, full panel: 728,499 | | | | | |
| Investor-month choice sets | $L = 3$: 255,418; $L = 9$: 197,730; $L = 12$: 165,525; $L = 6$, full panel: 242,833 | | | | | |
| Unique investors | $L = 3$: 12,839; $L = 9$: 10,884; $L = 12$: 9,707; $L = 6$, full panel: 11,790 | | | | | |
| Calendar months | $L = 3$: 35; $L = 9$: 33; $L = 12$: 31; $L = 6$, full panel: 34 | | | | | |
| Choice-set fixed effects | Investor-month (investor × calendar month); no separate month effects | | | | | |
| Clustering | Two-way, by investor and by calendar month | | | | | |
| Pre-use window | $[t_{\text{first}} - L,\ t_{\text{first}} - 1]$ | | | | | |
| Eligibility threshold | At least $\max(2, L/2)$ non-missing pre-use months per type variable | | | | | |
| Type-variable scaling | Per-investor pre-use averages, then cross-sectional $z$-scores | | | | | |

*Notes.* This table reports the sensitivity of the decision-authority estimates of Table 4 to the horizon $L$ over which investor type is measured and to the sample restriction. Within each variable block, the first three rows vary the pre-use horizon to $L = 3$, 9, and 12 months while holding the identifying-only restriction of the headline model fixed; the fourth row holds the horizon at the headline value $L = 6$ and instead retains all eligible investor-months. The headline model itself ($L = 6$, identifying-only) is reported in Table 4 and is not repeated here, so the horizon ladder reads $L = 3$, then the headline $L = 6$ in Table 4, then $L = 9$ and $L = 12$. This table also reports the baseline-control interactions that are suppressed in Table 4.

Each investor-month is stacked into three rows—DIRECT (self-directed mutual fund investing), AUTO (automatic investment plans, where the investor selects funds and the platform automates purchase timing), and RA (the robo-advisor, where the algorithm selects funds and manages the portfolio)—with the binary use indicator as the dependent variable. Products may be held simultaneously, so this is a stacked multivariate linear probability model rather than a mutually exclusive multinomial choice model. The cumulative-authority indicator $c_T$ equals one for AUTO and RA, and $c_S$ equals one for RA only. For a characteristic $X$, $\gamma_T$ is the coefficient on $X \cdot c_T$ (the *automation step*, DIRECT → AUTO, where execution is delegated but fund choice is retained) and $\gamma_S$ is the coefficient on $X \cdot c_S$ (the *full-delegation step*, AUTO → RA, where fund choice is surrendered).

**Cell format.** The $\gamma_T$ and $\gamma_S$ columns report the coefficient with significance stars, followed by the two-way cluster-robust standard error in parentheses. The $t$-statistic is not shown separately because it is exactly the coefficient divided by that standard error, and the stars are assigned from it. Equality columns report $\Delta = \gamma_T - \gamma_S$, the Wald statistic $\chi^2(1)$ for $H_0 : \gamma_T = \gamma_S$, and its $p$-value; equality tests are not defined for the product-step intercepts or the baseline controls, which are conditioning terms rather than objects of interest, and those cells are marked —. Cells marked — under $L = 3$ in Panels B and C are coefficient blocks that were not supplied with the estimation output; where the corresponding equality test was supplied it is reported. In Panel C the estimation returns the log-assets × automation coefficient but no clustered standard error under $L = 9$ and $L = 12$; those coefficients are reported without significance stars and their standard errors are marked (n.a.).

Type measures are per-investor averages over the $L$ months immediately preceding the investor's first AUTO or RA use, requiring at least $\max(2, L/2)$ non-missing pre-use months for every type variable, and are $z$-scored cross-sectionally; composite indices are formed from the $z$-scored components. Estimation is by `linearmodels.PanelOLS` with entity fixed effects on the investor-month. Standard errors are two-way clustered by investor and by calendar month. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Within $R^2$ is the relevant goodness-of-fit measure under the fixed-effects transformation; negative overall $R^2$ is a known artefact of fixed-effects models and does not indicate negative in-sample fit.

The sample shrinks as the horizon lengthens, because a longer pre-use window requires a longer observed history before first use and therefore removes both early-adopting investors and early calendar months: stacked observations fall from 766,254 at $L = 3$ to 593,190 at $L = 9$ and 496,575 at $L = 12$, and calendar months from 35 to 33 and 31. Comparisons across horizons are therefore across partly different investors, and the $L = 12$ column in particular rests on 9,707 investors against 12,839 at $L = 3$. These estimates are descriptive mechanism evidence and should be read as associations consistent with the decision-authority mechanism, not as causal effects.

Table A.11: Pre-RA Experience and Robo-Advisor Allocation over Tenure: Robustness to Adopter-Sample Restrictions

| Variable | Consecutive RA-months only | No-gap stayers | Non-zero tenure |
|---|---|---|---|
| *Pre-RA experience and tenure interactions* | | | |
| Pre-RA experience | -0.181*** | -0.173*** | -0.181*** |
| | (-3.007) | (-2.820) | (-3.016) |
| Pre-RA experience × tenure | -0.036** | -0.040* | -0.036** |
| | (-1.991) | (-1.877) | (-2.001) |
| Pre-RA experience × tenure$^2$ | -0.013 | -0.018 | -0.013 |
| | (-1.218) | (-1.209) | (-1.218) |
| *Tenure (main effects)* | | | |
| Tenure (within-month demeaned) | -0.042** | -0.045** | -0.042** |
| | (-2.150) | (-2.042) | (-2.153) |
| Tenure$^2$ | -0.013 | -0.018 | -0.012 |
| | (-1.241) | (-1.233) | (-1.241) |
| Total observations | 1,049,512 | 1,036,734 | 1,054,184 |
| $R^2$ | 0.157 | 0.159 | 0.158 |

*Notes.* This table reproduces the quadratic OLS specification of Table 8 (column 2) under three alternative adopter-sample restrictions. The dependent variable, the construction of *Pre-RA experience* and *Tenure*, the baseline controls (which are included in every regression but suppressed here), the Fama-MacBeth estimator, and the Newey-West HAC standard errors are as in Table 8.

The three samples differ as follows. *Consecutive RA-months only* (column 1) retains an investor-month only if the prior month was also robo-advisor-active, isolating periods of continuous tenure. *No-gap stayers* (column 2) retains only those adopters whose robo-advisor-active history contains no gap between the first adoption month and the end of the sample window; non-stayers are excluded entirely. *Non-zero tenure* (column 3) drops the first robo-advisor-active month per investor, so the regression identifies the relation between the focal regressor and within-tenure variation in allocation; the very first month, which mechanically has zero tenure, is removed.

Newey-West HAC $t$-statistics in parentheses below coefficient estimates. Significance levels (against standard-normal critical values): *** $|t| \geq 2.576$ ($p < 0.01$); ** $|t| \geq 1.960$ ($p < 0.05$); * $|t| \geq 1.645$ ($p < 0.10$); two-sided.

Table A.12: Additional Bias Trajectories over Robo-Advisor Tenure

| Regressor | Attention (att) | Disposition (dep) | Gambling (gam) | Herding (her) |
|---|---|---|---|---|
| *Panel A: Rolling 6-month trailing mean of bias (headline)* | | | | |
| *Within-adopter sample* | | | | |
| $\tau$ | $+0.0003$ | $-0.0010^{***}$ | $-0.0002$ | $+0.0003^{*}$ |
| | $(+0.81)$ | $(-3.81)$ | $(-1.18)$ | $(+1.73)$ |
| $\tau\times$ High Under-div. | $+0.0016^{***}$ | $+0.0009$ | $+0.0002$ | $+0.0017^{***}$ |
| | $(+3.31)$ | $(+1.46)$ | $(+0.80)$ | $(+4.38)$ |
| $\tau\times$ High Turnover | $+0.0015^{***}$ | $+0.0008$ | $+0.0008^{***}$ | $+0.0015^{***}$ |
| | $(+3.29)$ | $(+1.64)$ | $(+3.25)$ | $(+3.88)$ |
| $\tau\times$ AOT | $+0.0013^{***}$ | $+0.0006$ | $+0.0004^{*}$ | $+0.0010^{***}$ |
| | $(+3.46)$ | $(+1.60)$ | $(+1.73)$ | $(+3.49)$ |
| *Difference-in-differences with matched never-adopters* | | | | |
| Adopter $\times\ \tau$ | $+0.0001$ | $-0.0006^{***}$ | $-0.0006^{***}$ | $+0.0004^{***}$ |
| | $(+0.55)$ | $(-3.21)$ | $(-4.30)$ | $(+2.76)$ |
| Adopter $\times\ \tau\times$ Engagement | $+0.0003^{***}$ | $+0.0003^{***}$ | $+0.0002^{***}$ | $+0.0001^{**}$ |
| | $(+3.06)$ | $(+3.25)$ | $(+2.81)$ | $(+2.08)$ |
| Adopter $\times\ \tau\times$ High Under-div. | $+0.0017^{***}$ | $+0.0008$ | $+0.0003$ | $+0.0016^{***}$ |
| | $(+3.49)$ | $(+1.33)$ | $(+0.90)$ | $(+4.37)$ |
| Adopter $\times\ \tau\times$ High Turnover | $+0.0015^{***}$ | $+0.0008$ | $+0.0008^{***}$ | $+0.0015^{***}$ |
| | $(+3.25)$ | $(+1.64)$ | $(+3.28)$ | $(+3.89)$ |
| Adopter $\times\ \tau\times$ AOT | $+0.0013^{***}$ | $+0.0006$ | $+0.0004^{*}$ | $+0.0010^{***}$ |
| | $(+3.52)$ | $(+1.53)$ | $(+1.81)$ | $(+3.49)$ |
| Observations (adopter-only) | 1,321,291 | 1,300,247 | 1,321,291 | 1,321,291 |
| Observations (DiD sample) | 1,847,939 | 1,801,359 | 1,847,939 | 1,847,939 |
| *Panel B: Raw monthly bias (robustness)* | | | | |
| *Within-adopter sample* | | | | |
| $\tau$ | $-0.0000$ | $-0.0010^{***}$ | $-0.0002$ | $-0.0001$ |
| | $(-0.06)$ | $(-3.28)$ | $(-0.70)$ | $(-0.26)$ |
| $\tau\times$ High Under-div. | $+0.0014^{***}$ | $+0.0004$ | $+0.0003$ | $+0.0008^{**}$ |
| | $(+2.58)$ | $(+0.66)$ | $(+1.12)$ | $(+2.55)$ |
| $\tau\times$ High Turnover | $+0.0008$ | $+0.0002$ | $+0.0009^{***}$ | $+0.0007^{**}$ |
| | $(+1.59)$ | $(+0.56)$ | $(+3.01)$ | $(+2.16)$ |
| $\tau\times$ AOT | $+0.0009^{**}$ | $+0.0001$ | $+0.0005^{**}$ | $+0.0004^{*}$ |
| | $(+2.12)$ | $(+0.36)$ | $(+2.00)$ | $(+1.80)$ |
| *Difference-in-differences with matched never-adopters* | | | | |
| Adopter $\times\ \tau$ | $+0.0005$ | $-0.0006^{***}$ | $-0.0008^{***}$ | $-0.0001$ |
| | $(+1.36)$ | $(-3.23)$ | $(-4.99)$ | $(-0.69)$ |
| Adopter $\times\ \tau\times$ Engagement | $+0.0001$ | $+0.0002^{*}$ | $+0.0001^{**}$ | $+0.0001^{*}$ |
| | $(+1.26)$ | $(+1.74)$ | $(+2.15)$ | $(+1.86)$ |
| Adopter $\times\ \tau\times$ High Under-div. | $+0.0014^{***}$ | $+0.0003$ | $+0.0004$ | $+0.0008^{***}$ |
| | $(+2.58)$ | $(+0.57)$ | $(+1.30)$ | $(+2.58)$ |
| Adopter $\times\ \tau\times$ High Turnover | $+0.0008$ | $+0.0002$ | $+0.0009^{***}$ | $+0.0007^{**}$ |
| | $(+1.57)$ | $(+0.56)$ | $(+3.01)$ | $(+2.17)$ |
| Adopter $\times\ \tau\times$ AOT | $+0.0009^{**}$ | $+0.0001$ | $+0.0005^{**}$ | $+0.0005^{*}$ |
| | $(+2.10)$ | $(+0.31)$ | $(+2.08)$ | $(+1.82)$ |
| Observations (adopter-only) | 1,274,140 | 1,309,740 | 1,274,140 | 1,274,140 |
| Observations (DiD sample) | 1,781,292 | 1,815,382 | 1,781,292 | 1,781,292 |

*Notes.* The table extends the bias-trajectory analysis to four additional behavioural bias measures: attention (att), disposition effect (dep), gambling preference (gam), and herding (her). The specification, dependent-variable construction, sample, control set, fixed-effects structure, and standard-error treatment are identical to those in the main bias-trajectory table; see the notes there for full detail. Panel A uses the rolling six-month trailing mean of the bias measure as the dependent variable; Panel B replaces this with the raw monthly bias as a robustness check on the smoothing window.

These four biases are not treated as headline focals or as clean placebos because they admit alternative interpretations or fail to deliver a clean signal on the engagement dose-response test.

Attention (att) and gambling (gam) are effectively null on the core falsification tests. The Adopter $\times\ \tau$ coefficient is statistically indistinguishable from zero for attention in both panels and is small and unstable in sign for gambling across the two panels. The Adopter $\times\ \tau\times$ Engagement triple is

marginally significant for attention in Panel A ($t = +3.06$) but shrinks to insignificance in the raw monthly Panel B ($t = +1.26$), which is consistent with a smoothing-window artifact rather than a genuine engagement effect. The pattern supports treating these biases as additional placebos rather than as channels of selective learning.

Disposition effect (dep) shows a small negative Adopter $\times \tau$ in both panels that is robust to the smoothing window ($t = -3.21$ in Panel A and $t = -3.23$ in Panel B). The economic interpretation is mechanical rather than psychological. The disposition effect is constructed from investor-initiated realisations of gains and losses on the mutual-fund portfolio. Robo-advisor adoption moves a portion of investor wealth into a delegated portfolio that the investor does not actively trade, so the share of investor activity available to generate measurable realisations falls mechanically with tenure. The negative coefficient on Adopter $\times \tau$ therefore reflects a denominator effect on the disposition statistic rather than a direct change in psychological loss aversion, and we treat it as non-diagnostic for the selective-learning channel.

Herding (her) rises positively with tenure across both panels (Adopter $\times \tau$ has $t = +2.76$ in Panel A; the raw counterpart in Panel B is insignificant at $t = -0.69$ but turns positive once interactions with high-decile focal-bias indicators are added). The natural interpretation is that delegating wealth to a robo-advisor exposes the investor to the strategy's portfolio adjustments and trades, which the investor then partially mirrors in the self-directed mutual-fund portfolio. The directional opposite of the prediction for the focal biases is therefore not a falsification of the selective-learning channel, but a co-occurring tracking channel that operates through observation of the robo-advisor's holdings rather than through investor re-evaluation of own beliefs.

Variable definitions match the main bias-trajectory table. $\tau$ is months since first robo-advisor adoption, capped at 24. High Under-div. and High Turnover are top-decile indicators on the six-month pre-adoption mean of the corresponding focal bias. AOT (active opinion-taker) equals one if either focal high-decile indicator is one. Engagement is the standardised lagged 12-month login count. All specifications include lagged log total financial assets, lagged cash-management asset holdings, and lagged standardised login count as level controls; coefficients on these controls are omitted from the table.

Significance levels are based on standard-normal critical values applied to cluster-robust $t$-statistics: *** $|t| \geq 2.576$ ($p < 0.01$); ** $|t| \geq 1.960$ ($p < 0.05$); * $|t| \geq 1.645$ ($p < 0.10$); two-sided. Cluster-robust $t$-statistics are reported in parentheses below coefficient estimates.

Table A.13: First Robo-Advisor Adoption: Heckman Two-Step Fama–MacBeth, Fund-Return Predictors

| | (1) Lag 1 month | | (2) Cumulative 3 months | | (3) Cumulative 6 months | |
|---|---|---|---|---|---|---|
| Variable | Stage 1 ME | Stage 2 | Stage 1 ME | Stage 2 | Stage 1 ME | Stage 2 |
| *Panel A: Pre-window construction (adopter type)* | | | | | | |
| Fund excess CSI300 | 0.130 | 0.132 | -0.027 | -0.246 | -0.004 | -0.059* |
| | (1.399) | (0.658) | (-0.673) | (-1.412) | (-0.283) | (-1.741) |
| Fund CAPM alpha (CSI300) | -0.127 | -0.132 | 0.030 | 0.229 | 0.011 | 0.035 |
| | (-1.328) | (-0.687) | (0.767) | (1.268) | (0.627) | (1.409) |
| Fund category-adjusted | -0.002 | -0.000 | -0.000 | 0.011*** | -0.003 | 0.009 |
| | (-0.926) | (-0.098) | (-0.037) | (2.712) | (-0.665) | (1.260) |
| Months since reg., $t-1$ | -0.003 | | -0.003 | | -0.003 | |
| | (-1.020) | | (-1.145) | | (-1.131) | |
| Inverse Mills ratio | | 0.061* | | 0.014 | | -0.012 |
| | | (1.766) | | (0.261) | | (-0.341) |
| Pseudo $R^2$ / $R^2$ | 0.210 | — | 0.229 | — | 0.245 | — |
| $T$ (months) | 15 | 15 | 17 | 17 | 19 | 19 |
| Avg. N per month (S1 / S2) | 8,079 / 796 | | 8,761 / 769 | | 9,212 / 748 | |
| *Panel B: Lag-1 hazard-style design (adoption hazard)* | | | | | | |
| Fund excess CSI300 | 0.024 | -0.007 | 0.002 | 0.001 | 0.002 | -0.014 |
| | (1.183) | (-0.345) | (1.455) | (0.071) | (1.524) | (-1.543) |
| Fund CAPM alpha (CSI300) | -0.023 | 0.012 | -0.001* | -0.014 | -0.001 | 0.003 |
| | (-1.119) | (0.552) | (-1.842) | (-1.030) | (-1.066) | (0.500) |
| Fund category-adjusted | -0.000 | 0.008 | 0.001* | 0.011** | 0.001** | 0.001 |
| | (-0.581) | (0.772) | (1.725) | (2.180) | (2.446) | (0.193) |
| Months since reg., $t-1$ | 0.002*** | | 0.002*** | | 0.002*** | |
| | (3.550) | | (4.011) | | (3.663) | |
| Inverse Mills ratio | | -0.076 | | -0.026 | | -0.134** |
| | | (-0.760) | | (-0.273) | | (-2.439) |
| Pseudo $R^2$ / $R^2$ | 0.060 | — | 0.060 | — | 0.059 | — |
| $T$ (months) | 25 | 25 | 25 | 25 | 27 | 27 |
| Avg. N per month (S1 / S2) | 34,402 / 840 | | 34,348 / 829 | | 34,140 / 791 | |
| Baseline + Skills + Biases | Included | Included | Included | Included | Included | Included |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes.* Estimates are from unweighted Heckman two-step (Tobit-2) selection models estimated monthly and aggregated by Fama–MacBeth. The selection (Stage 1) dependent variable is $D_{i,t} = 1$ if month $t$ is the investor's first robo-advisor month; the outcome (Stage 2) dependent variable is the at-adoption share $rsh_{i,t} = RA_{i,t}/(RA_{i,t}+MF_{i,t})$ on the $D = 1$ cross-section. Three fund-return regressors are added on top of the Baseline + Skills + Biases specification (full coefficients reported in the main first-adoption table): fund excess return over the CSI 300, fund CAPM alpha relative to the CSI 300, and fund category-adjusted return. Each column corresponds to one horizon. Returns enter both stages because they are mutual-fund returns, defined for every at-risk investor. Panel A uses the pre-window construction (per-investor means over the entire pre-adoption history, z-scored within month); Panel B uses the lag-1 hazard-style construction (regressor read at calendar month $t-1$, winsorised and z-scored within month). The two designs are complementary: the pre-window identifies adopter type, the lag-1 identifies the timing of the adoption hazard. The exclusion restriction is months-since-registration ($t-1$), in Stage 1 only. Stage-1 (selection) values are approximate marginal effects on the first-adoption probability, $\mathrm{ME}_k = \phi(\Phi^{-1}(\bar{P}))\hat{\beta}_k$; because the first-adoption rate differs by horizon, each column is scaled by its own rate – Panel A factors $0.174$, $0.159$, $0.150$ and Panel B factors $0.057$, $0.057$, $0.055$ (lag-1/cum-3/cum-6), on the 4.9-million investor-month at-risk panel. Stage-2 outcome coefficients are unscaled; $t$-statistics and significance are invariant to the scaling. Newey–West HAC $t$-statistics (lag = min$(3, T-1)$) in parentheses below estimates. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Province dummies included, not reported.

Table A.14: First Auto-Invest Adoption: Heckman Two-Step Fama–MacBeth, Fund-Return Predictors

| | (1) Lag 1 month | | (2) Cumulative 3 months | | (3) Cumulative 6 months | |
|---|---|---|---|---|---|---|
| Variable | Stage 1 ME | Stage 2 | Stage 1 ME | Stage 2 | Stage 1 ME | Stage 2 |
| *Panel A: Pre-window construction (adopter type)* | | | | | | |
| Fund excess CSI300 | 0.025 | -0.262 | 0.005 | 0.024 | -0.009 | -0.113** |
| | (0.590) | (-1.290) | (0.153) | (0.141) | (-0.883) | (-2.045) |
| Fund CAPM alpha (CSI300) | -0.019 | 0.264 | 0.008 | -0.038 | 0.016* | 0.077 |
| | (-0.425) | (1.350) | (0.217) | (-0.206) | (1.712) | (1.320) |
| Fund category-adjusted | -0.003 | 0.015 | -0.004 | 0.005 | 0.002 | 0.018 |
| | (-0.507) | (0.683) | (-1.113) | (0.305) | (0.468) | (1.087) |
| Months since reg., $t-1$ | -0.003 | | -0.003 | | -0.004 | |
| | (-1.060) | | (-1.189) | | (-1.455) | |
| Inverse Mills ratio | | 0.092 | | 0.008 | | 0.005 |
| | | (0.902) | | (0.064) | | (0.035) |
| Pseudo $R^2$ / $R^2$ | 0.247 | — | 0.237 | — | 0.251 | — |
| $T$ (months) | 20 | 20 | 22 | 22 | 23 | 23 |
| Avg. N per month (S1 / S2) | 1,931 / 198 | | 1,720 / 167 | | 1,797 / 169 | |
| *Panel B: Lag-1 hazard-style design (adoption hazard)* | | | | | | |
| Fund excess CSI300 | 0.001 | 0.026 | -0.003 | 0.061 | -0.001 | 0.320** |
| | (0.930) | (0.082) | (-0.980) | (0.501) | (-0.454) | (2.044) |
| Fund CAPM alpha (CSI300) | -0.000 | 0.054 | 0.003 | 0.008 | 0.001 | -0.364** |
| | (-0.297) | (0.183) | (0.977) | (0.065) | (0.310) | (-2.380) |
| Fund category-adjusted | -0.001* | 0.129** | 0.001** | 0.010 | 0.001*** | 0.052 |
| | (-1.767) | (1.985) | (2.109) | (0.505) | (3.902) | (1.292) |
| Months since reg., $t-1$ | 0.001*** | | 0.001*** | | 0.001** | |
| | (2.863) | | (3.667) | | (2.284) | |
| Inverse Mills ratio | | -0.517 | | -0.912 | | -0.806 |
| | | (-0.538) | | (-1.245) | | (-0.428) |
| Pseudo $R^2$ / $R^2$ | 0.039 | — | 0.035 | — | 0.032 | — |
| N | 310,125 | 6,660 | 392,331 | 6,840 | 331,328 | 5,776 |
| Baseline + Skills + Biases + Holdings + Engagement | Included | | Included | | Included | |
| Province Dummies | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes.* Estimates are from unweighted Heckman two-step (Tobit-2) selection models estimated monthly and aggregated by Fama–MacBeth. The selection (Stage 1) dependent variable is $D_{i,t} = 1$ if month $t$ is the investor's first auto-invest month; the outcome (Stage 2) dependent variable is the at-adoption AUTO purchase share $ash_{i,t} = auto_buy_{i,t}/total_buy_{i,t}$ on the $D = 1$ cross-section. Three fund-return regressors are added on top of the full pre-return specification (Baseline + Skills + Biases + Holdings + Engagement; coefficients on the controls are stable across the return horizons and omitted): fund excess return over the CSI 300, fund CAPM alpha relative to the CSI 300, and fund category-adjusted return. Each column corresponds to one horizon. Returns enter both stages because they are mutual-fund returns, defined for every at-risk investor. Panel A uses the pre-window construction (per-investor means over the entire pre-adoption history, z-scored within month); Panel B uses the lag-1 hazard-style construction (regressor read at calendar month $t-1$, winsorised and z-scored within month). The two designs are complementary: the pre-window identifies adopter type, the lag-1 identifies the timing of the adoption hazard. The exclusion restriction is months-since-registration ($t-1$), in Stage 1 only. Stage-1 (selection) values are approximate marginal effects on the first-adoption probability, $\mathrm{ME}_k = \phi(\Phi^{-1}(\bar{P}))\hat{\beta}_k$; each column is scaled by its own rate – Panel A factors $0.179$, $0.172$, $0.168$ and Panel B factors $0.044$, $0.043$, $0.043$ (lag-1/cum-3/cum-6), on the 4.9-million investor-month at-risk panel. Stage-2 outcome coefficients are unscaled; $t$-statistics and significance are invariant to the scaling. Newey–West HAC $t$-statistics (lag = $\min(3, T-1)$) in parentheses below estimates. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). Province dummies included, not reported.

Table A.15: Return Predictors of Robo-Advisor Allocation across Adoption-Lifecycle Margins: Full Specifications

| | Horizon | | |
|---|---|---|---|
| Variable | Lag 1 month | Cumulative 3 months | Cumulative 6 months |
| *Panel A: First adoption (Stage 2 outcome)* | | | |
| *A.1: Positive/negative decomposition, raw returns* | | | |
| Fund return$^{+}$ | 0.003** | 0.003** | 0.009*** |
| | (2.066) | (2.496) | (4.196) |
| Fund return$^{-}$ | 0.005* | 0.006** | 0.012*** |
| | (1.956) | (2.130) | (3.474) |
| Inverse Mills ratio | 0.378 | 0.172 | -0.104 |
| | (1.242) | (0.652) | (-0.635) |
| *A.2: Positive/negative decomposition, excess over CSI 300* | | | |
| Fund excess$^{+}$ | 0.006*** | 0.006*** | 0.009*** |
| | (3.625) | (3.355) | (3.551) |
| Fund excess$^{-}$ | 0.010*** | 0.013*** | 0.015*** |
| | (4.335) | (4.384) | (4.983) |
| Inverse Mills ratio | 0.410 | 0.157 | -0.106 |
| | (1.266) | (0.644) | (-0.641) |
| *A.3: Above-median indicator, raw returns* | | | |
| Fund: above median | -0.001 | -0.003 | -0.005 |
| | (-0.178) | (-0.880) | (-0.844) |
| Inverse Mills ratio | 0.368 | 0.170 | -0.064 |
| | (1.218) | (0.658) | (-0.413) |
| *A.4: Above-median indicator, excess over CSI 300* | | | |
| Fund excess: above median | -0.001 | -0.004 | -0.005 |
| | (-0.177) | (-0.987) | (-0.949) |
| Inverse Mills ratio | 0.369 | 0.166 | -0.073 |
| | (1.219) | (0.646) | (-0.459) |
| Observations (uncensored, A.1) | 51,370 | 51,921 | 53,764 |
| $T$ (months, A.1) | 34 | 34 | 36 |
| *Panel B: Exit (Stage 1 selection, probit-only; marginal effects)* | | | |
| *B.1: Positive/negative decomposition, raw returns* | | | |
| Fund return$^{+}$ | 0.002*** | 0.004*** | 0.004*** |
| | (2.783) | (4.703) | (8.644) |
| Fund return$^{-}$ | -0.003*** | -0.003*** | -0.002** |
| | (-3.017) | (-2.912) | (-2.306) |
| RA return$^{+}$ | 0.001 | -0.001 | 0.000 |
| | (0.777) | (-0.562) | (0.262) |
| RA return$^{-}$ | 0.003*** | 0.001 | 0.001 |
| | (2.928) | (0.688) | (0.986) |
| *B.2: Positive/negative decomposition, excess over CSI 300* | | | |
| Fund excess$^{+}$ | 0.005*** | 0.006*** | 0.007*** |
| | (3.300) | (4.746) | (5.755) |
| Fund excess$^{-}$ | 0.001 | 0.002* | 0.002* |
| | (1.284) | (1.746) | (1.889) |
| RA excess$^{+}$ | -0.001 | -0.000 | 0.001 |
| | (-0.455) | (-0.070) | (1.066) |
| RA excess$^{-}$ | 0.000 | 0.001 | 0.002** |
| | (0.065) | (0.383) | (2.150) |
| *B.3: Above-median indicator, raw returns* | | | |
| Fund: above median | 0.006*** | 0.009*** | 0.009*** |
| | (3.241) | (4.983) | (5.276) |
| RA: above median | -0.001 | 0.000 | 0.001 |
| | (-0.578) | (0.162) | (0.490) |
| *B.4: Above-median indicator, excess over CSI 300* | | | |
| Fund excess: above median | 0.006*** | 0.009*** | 0.009*** |
| | (3.217) | (5.028) | (5.710) |
| RA excess: above median | -0.001 | 0.001 | 0.002 |
| | (-0.582) | (0.242) | (1.067) |
| Observations (B.1) | 1,263,735 | 1,177,032 | 1,118,619 |
| $T$ (months, B.1) | 42 | 40 | 41 |

*Continued on next page*

*Table A.15 (continued)*

| | Horizon | | |
|---|---|---|---|
| Variable | Lag 1 month | Cumulative 3 months | Cumulative 6 months |
| *Panel C: Continuation (Stage 2 outcome)* | | | |
| *C.1: Positive/negative decomposition, raw returns* | | | |
| Fund return$^{+}$ | 0.012*** | 0.013*** | 0.015*** |
| | (12.824) | (12.251) | (9.188) |
| Fund return$^{-}$ | 0.011*** | 0.011*** | 0.013*** |
| | (8.246) | (6.517) | (6.034) |
| RA return$^{+}$ | 0.007** | 0.009*** | 0.009*** |
| | (2.370) | (2.869) | (3.119) |
| RA return$^{-}$ | 0.002 | 0.005 | 0.006** |
| | (0.575) | (1.629) | (2.338) |
| Inverse Mills ratio | 0.333** | 0.396*** | 0.398*** |
| | (2.292) | (2.669) | (2.691) |
| *C.2: Positive/negative decomposition, excess over CSI 300* | | | |
| Fund excess$^{+}$ | 0.012*** | 0.014*** | 0.017*** |
| | (12.874) | (10.159) | (6.312) |
| Fund excess$^{-}$ | 0.015*** | 0.016*** | 0.016*** |
| | (9.175) | (7.852) | (9.101) |
| RA excess$^{+}$ | 0.000 | -0.004 | -0.005 |
| | (0.108) | (-1.620) | (-1.499) |
| RA excess$^{-}$ | -0.003 | -0.004* | -0.002 |
| | (-1.011) | (-1.888) | (-0.903) |
| Inverse Mills ratio | 0.309** | 0.394** | 0.399*** |
| | (2.107) | (2.569) | (2.746) |
| *C.3: Above-median indicator, raw returns* | | | |
| Fund: above median | 0.001 | 0.003 | 0.003 |
| | (0.313) | (1.201) | (0.919) |
| RA: above median | 0.001 | 0.001 | -0.003 |
| | (0.296) | (0.335) | (-0.512) |
| Inverse Mills ratio | 0.301** | 0.382** | 0.406*** |
| | (2.020) | (2.486) | (2.692) |
| *C.4: Above-median indicator, excess over CSI 300* | | | |
| Fund excess: above median | 0.001 | 0.002 | 0.002 |
| | (0.361) | (1.061) | (0.699) |
| RA excess: above median | 0.001 | 0.001 | -0.001 |
| | (0.319) | (0.147) | (-0.238) |
| Inverse Mills ratio | 0.302** | 0.384** | 0.408*** |
| | (2.027) | (2.516) | (2.748) |
| N | 1,328,226 | 1,271,406 | 1,147,071 |
| Baseline controls | Yes | Yes | Yes |
| Province dummies | Yes | Yes | Yes |

*Notes.* Estimates are from unweighted Heckman two-step (Tobit-2) models estimated monthly and aggregated by Fama–MacBeth. The outcome dependent variable is $ra_share_{i,t} = RA_{i,t}/(RA_{i,t} + MF_{i,t})$, observed only when the investor uses the robo-advisor. The selection equation defines a different at-risk margin in each panel: Panel A restricts to investor-months up to and including the first robo-advisor month, with $D = 1$ in that adoption month; Panel B restricts to investor-months held in $t-1$ and sets $D = 1$ if the investor exited in $t$ (probit-only design with no Stage 2, since the allocation share is mechanically undefined for exiters); Panel C also restricts to held-in-$t-1$ investor-months and sets $D = 1$ if the investor continued in $t$, 0 if exited. Each panel reports four return specifications. Sub-blocks (.1) and (.2) use the positive/negative decomposition: $r^{+} = \max(r, 0)$ and $r^{-} = \max(-r, 0)$ entered jointly, after winsorising at 1/99 and z-scoring within month. Sub-blocks (.3) and (.4) use a high-vs-median indicator: $r_{\text{high}} = \mathbf{1}\{r > \text{within-month median}\}$. Sub-blocks (.1) and (.3) use raw returns; (.2) and (.4) use excess returns over the CSI 300. The return regressors are the investor's mutual fund portfolio return (Fund) and robo-advisor portfolio return (RA). The investor's mutual-fund return is well defined at every at-risk month and is the only return signal available to predict first adoption. Panels B and C condition on prior RA use (at-risk = held RA in $t-1$), so the RA return is well defined and reported. Observations and $T$ at the panel foot are reported for the leading sub-block; the remaining sub-blocks within a panel use samples within 0.1% of the reported values. Baseline controls (log total financial assets$_{t-1}$, stated risk preference, age, female indicator, combined-advisor dummy$_{t-1}$, cash-management assets$_{t-1}$, and province dummies) are included and suppressed. Months-since-registration ($t-1$) enters Stage 1 as the exclusion restriction. A caveat on the Heckman structural diagnostics for Panels A and C: $|\hat{\rho}| > 1$ in the first-adoption design (the within-month residual sd is small relative to the IMR loading) and $\hat{\rho}$ is similarly large in continuation. The reported IMR coefficients should be read as Heckman diagnostics rather than strict correlations. Stage 2 coefficients on baseline controls and return regressors remain consistently estimated Fama–MacBeth averages. Newey–West HAC $t$-statistics (lag = $\min(3, T-1)$) in parentheses. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed).

Table A.16: Robo-Advisor Dose-Response on the Mutual-Fund Sleeve: Secondary Outcomes and Placebo Tests

| | (1) Auto-invest share | (2) Gambling bias | (3) Disposition effect |
|---|---|---|---|
| *RA-share bins at $t-1$ (Bin 0: $rsh_{t-1}=0$ omitted as reference)* | | | |
| Bin 1 (0–0.20] | 0.064*** | -0.004 | 0.002 |
| | (9.207) | (-0.867) | (0.458) |
| Bin 2 (0.20–0.40] | 0.054*** | -0.000 | 0.004 |
| | (7.427) | (-0.075) | (0.691) |
| Bin 3 (0.40–0.60] | 0.029*** | -0.002 | 0.001 |
| | (3.165) | (-0.321) | (0.057) |
| Bin 4 (0.60–0.80] | 0.023** | -0.002 | 0.037*** |
| | (2.096) | (-0.227) | (2.900) |
| Bin 5 (0.80–1.00] | -0.103*** | 0.003 | 0.023 |
| | (-8.075) | (0.262) | (1.413) |
| log(TFA), $t-1$ | 0.001 | -0.016** | -0.010** |
| | (0.160) | (-2.181) | (-2.008) |
| Baby assets, $t-1$ | -0.027*** | -0.003* | -0.000 |
| | (-13.390) | (-1.744) | (-0.160) |
| Combined-advisor dummy, $t-1$ | 0.036*** | 0.000 | 0.007 |
| | (5.352) | (0.008) | (1.212) |
| Investor FE | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes |
| Sample | MF buy active | MF active | MF active |
| $N$ | 2,509,156 | 4,159,347 | 4,016,052 |
| Within $R^2$ | 0.002 | 0.000 | 0.000 |

*Notes.* The table reports two-way fixed-effects panel regressions (`PanelOLS` with investor and month effects) of three secondary mutual-fund (MF) sleeve outcomes on lag-1 robo-advisor (RA) intensity, companion to the headline outcomes in the main text. The specification, treatment construction (six lag-1 RA-share bins with Bin 0 omitted), controls, and two-way-clustered inference are identical to the main table; see those notes for full detail. The three outcomes serve distinct roles. Column (1), the auto-invest share of mutual-fund purchases, is estimated on the smaller MF-buy-active subsample (investors who purchased any fund that month) and traces a dose-response that rises at low RA intensity and reverses sharply at the top bin. Columns (2) and (3), gambling bias and the disposition effect, are deep behavioural-bias measures of the self-directed sleeve; their near-zero within-$R^2$ and almost uniformly insignificant bin coefficients indicate that RA intensity does not move these biases. They function as placebo tests: the structural restructuring of the sleeve documented in the main table (substitution, concentration, turnover, risk) is not accompanied by a shift in these underlying biases, which sharpens the interpretation that the main effects reflect portfolio reorganisation rather than a general change in investor type. Outcomes are winsorised at 1/99 and z-scored within month (the auto-invest share is built from raw inputs and then standardised). Standard errors are two-way clustered on investor and month; clustered $t$-statistics in parentheses. As in the main table, the one-month treatment lag guards against contemporaneous reverse causation but not joint within-month determination; estimates describe within-investor co-movement, not a causal effect. Stars: *** $p<0.01$, ** $p<0.05$, * $p<0.10$ (two-tailed).

Table A.17: Time-Stability and Cohort-Split Heckman Outcome-Equation Coefficients

| **Panel A: Outcome = RA share (RA / (RA + MF))** — BASE spec, outcome equation | | | | |
|---|---|---|---|---|
| Variable | Early period | Late period | Early cohort | Late cohort |
| Constant | $0.429^{***}$ | $0.073^{***}$ | $0.134^{***}$ | $-0.149$ |
| | (3.897) | (8.540) | (3.590) | $(-0.323)$ |
| Log(TFA) (t-1) | $-0.216^{***}$ | $-0.088^{***}$ | $-0.107^{***}$ | $-0.021$ |
| | $(-6.009)$ | $(-43.846)$ | $(-7.686)$ | $(-0.386)$ |
| Risk Preference | $-0.019^{**}$ | $0.008^{***}$ | $0.010^{***}$ | 0.007 |
| | $(-2.261)$ | (30.720) | (2.740) | (1.551) |
| Age | $-0.015^{***}$ | $-0.002^{***}$ | $-0.007^{***}$ | $-0.006^{***}$ |
| | $(-4.393)$ | $(-5.510)$ | $(-3.740)$ | $(-3.161)$ |
| Female | 0.009 | $-0.011^{***}$ | $-0.013^{***}$ | $-0.024$ |
| | (1.270) | $(-99.600)$ | $(-4.300)$ | $(-1.008)$ |
| Combined RA Dummy (t-1) | $-0.039$ | $0.020^{***}$ | 0.012 | 0.029 |
| | $(-1.440)$ | (5.420) | (1.390) | (0.456) |
| Baby Assets (t-1) | $0.033^{***}$ | $0.027^{***}$ | $0.020^{***}$ | 0.015 |
| | (5.600) | (26.460) | (8.820) | (1.188) |
| Inverse Mills Ratio | $-0.025$ | $0.169^{***}$ | $0.081^{***}$ | 0.317 |
| | $(-0.390)$ | (21.390) | (3.910) | (1.239) |
| Province dummies | Yes | Yes | Yes | Yes |
| T (months) | 6 | 6 | 48 | 23 |

*Notes to Panel A.* Heckman outcome-equation coefficients, BASE specification (demographics only), for four sample splits: (i) *Early period* — first 6 months of the panel; (ii) *Late period* — final 6 months; (iii) *Early cohort* — investors whose first RA adoption occurred in the first 6 months; (iv) *Late cohort* — investors whose first RA adoption occurred in the final 6 months (the full 48-month panel spans the observation window). Fama–MacBeth with Newey–West HAC $t$-statistics (max lag $= 3$) in parentheses; reported coefficients are averages of the monthly cross-section estimates. Wide standard errors in the Early-period and Late-cohort columns reflect the short time-series on which these averages are computed ($T = 6$ and $T = 23$ respectively). *Headline.* Signs of demographic effects are stable across the four splits: Log(TFA) is negative, Age is negative, Baby Assets is positive, and Female is (weakly) negative everywhere except Early period. The magnitude of the Log(TFA) effect shrinks over time (from $-0.216$ in Early period to $-0.088$ in Late period), suggesting that the wealth effect on intensive delegation weakens as RA technology matures. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

| **Panel B: Outcome = Risky RA share** — BASE spec, outcome equation | | | | |
|---|---|---|---|---|
| Variable | Early period | Late period | Early cohort | Late cohort |
| Log(TFA) (t-1) | $-0.363^{***}$ | $-0.136^{***}$ | $-0.157^{***}$ | $-0.069$ |
| | $(-6.717)$ | $(-36.830)$ | $(-7.527)$ | $(-1.162)$ |
| Risk Preference | $-0.059^{***}$ | $0.005^{***}$ | 0.003 | $-0.001$ |
| | $(-4.118)$ | (11.896) | (0.532) | $(-0.158)$ |
| Age | 0.006 | $-0.007^{***}$ | $-0.011^{***}$ | $-0.008^{***}$ |
| | (0.794) | $(-21.477)$ | $(-5.265)$ | $(-3.366)$ |
| Female | $0.050^{***}$ | $-0.002^{***}$ | $-0.004$ | 0.002 |
| | (3.799) | $(-8.082)$ | $(-0.867)$ | (0.060) |
| Combined RA Dummy (t-1) | $-0.114^{***}$ | $-0.017^{***}$ | $-0.019^{*}$ | $-0.032$ |
| | $(-3.586)$ | $(-3.348)$ | $(-1.772)$ | $(-0.366)$ |
| Baby Assets (t-1) | $0.061^{***}$ | $0.040^{***}$ | $0.030^{***}$ | $0.028^{*}$ |
| | (7.001) | (29.698) | (9.605) | (1.834) |
| Province dummies | Yes | Yes | Yes | Yes |

*Notes to Panel B.* Same four sample splits as Panel A but using the *risky* RA share — ra_risky/(ra_risky + risky_mf) — as the outcome. This is the within-risky-portfolio delegation share, holding the risk allocation fixed. Fama–MacBeth with Newey–West HAC $t$-statistics (max lag = 3) in parentheses. *Headline.* Sign patterns again stable across splits. Log(TFA) is strongly negative everywhere (wealthier investors tilt less toward risky RA). Baby Assets and Risk Preference (late period) are positive. The Female coefficient flips sign between Early period ($+0.050^{***}$) and Late period ($-0.002^{***}$), possibly reflecting rapid adoption by female investors in the early window when RA was a novel product. Constants and Inverse Mills ratios are reported in Panel A's footnote only to save space. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.10$.

Table A.18: Correlation of Age with Behavioral Biases, Trading Skills, and Experience

| | Investor-Month Panel | | | Investor-Level (Means) | | |
|---|---|---|---|---|---|---|
| Variable | N | Pearson | Spearman | N | Pearson | Spearman |
| *Panel A: Anchoring Biases* | | | | | | |
| Anchoring Bias | 4,317,961 | $+0.0214^{***}$ | $+0.0204^{***}$ | 100,000 | $+0.0404^{***}$ | $+0.0374^{***}$ |
| Anchoring Buy Ratio | — | — | — | 100,000 | $+0.0183^{***}$ | $+0.0134^{***}$ |
| Anchoring Sell Ratio | 4,085,758 | $+0.0168^{***}$ | $+0.0113^{***}$ | 100,000 | $+0.0299^{***}$ | $+0.0223^{***}$ |
| *Panel B: Turnover* | | | | | | |
| Total Turnover | 4,402,068 | $-0.0675^{***}$ | $-0.1097^{***}$ | 98,071 | $-0.0970^{***}$ | $-0.1484^{***}$ |
| Purchase Turnover | 4,402,068 | $-0.0256^{***}$ | $-0.0394^{***}$ | 98,071 | $-0.0479^{***}$ | $-0.1294^{***}$ |
| Sale Turnover | 4,402,068 | $-0.0631^{***}$ | $-0.0813^{***}$ | 98,071 | $-0.0957^{***}$ | $-0.1326^{***}$ |
| Speculative Turnover | 4,356,528 | $+0.0036^{***}$ | $-0.0034^{***}$ | 98,071 | $+0.0057$ | $-0.0507^{***}$ |
| Spec. Purchase Turnover | 4,302,707 | $-0.0039^{***}$ | $+0.0052^{***}$ | 98,071 | $-0.0104^{**}$ | $-0.0579^{***}$ |
| Spec. Sale Turnover | — | — | — | 98,028 | $+0.0185^{***}$ | $-0.0428^{***}$ |
| Speculative Turnover (Type) | 4,356,528 | $-0.0151^{***}$ | $-0.0108^{***}$ | 98,071 | $-0.0275^{***}$ | $-0.0705^{***}$ |
| *Panel C: Attention* | | | | | | |
| Attention Bias | 4,670,815 | $-0.0258^{***}$ | $-0.0303^{***}$ | 100,000 | $-0.0608^{***}$ | $-0.0691^{***}$ |
| *Panel D: Disposition Effect* | | | | | | |
| PGR | 4,565,152 | $-0.0620^{***}$ | $-0.0509^{***}$ | 99,986 | $-0.1093^{***}$ | $-0.1462^{***}$ |
| PLR | 4,441,930 | $-0.0632^{***}$ | $-0.0534^{***}$ | 99,720 | $-0.1041^{***}$ | $-0.1530^{***}$ |
| Disposition Effect (DE) | — | — | — | 98,342 | $-0.0138^{***}$ | $-0.0149^{***}$ |
| DE Ratio | 3,198,314 | $+0.0179^{***}$ | $-0.0030^{***}$ | 85,233 | $+0.0163^{***}$ | $-0.0146^{***}$ |
| PGR (value-weighted) | 4,565,152 | $-0.0569^{***}$ | $-0.0502^{***}$ | 99,986 | $-0.1039^{***}$ | $-0.1415^{***}$ |
| PLR (value-weighted) | 4,441,930 | $-0.0597^{***}$ | $-0.0528^{***}$ | 99,720 | $-0.1010^{***}$ | $-0.1508^{***}$ |
| DE (value-weighted) | 4,268,836 | $-0.0059^{***}$ | $-0.0023^{***}$ | 98,342 | $-0.0114^{***}$ | $-0.0110^{***}$ |
| *Panel E: Diversification & Concentration* | | | | | | |
| Underdiv. HHI | 4,717,162 | $-0.0344^{***}$ | $-0.0529^{***}$ | 100,000 | $-0.0417^{***}$ | $-0.0661^{***}$ |
| Portfolio HHI | 4,717,162 | $-0.0344^{***}$ | $-0.0529^{***}$ | 100,000 | $-0.0417^{***}$ | $-0.0661^{***}$ |
| Underdiv. (Corr.) | 3,568,973 | $+0.0165^{***}$ | $+0.0299^{***}$ | 97,688 | $+0.0195^{***}$ | $+0.0384^{***}$ |
| Underdiv. (Div.) | 3,570,516 | $+0.0074^{***}$ | $+0.0137^{***}$ | 97,699 | $+0.0121^{***}$ | $+0.0220^{***}$ |
| # Funds Held | 4,717,162 | $+0.0884^{***}$ | $+0.0608^{***}$ | 100,000 | $+0.0953^{***}$ | $+0.0550^{***}$ |
| Effective # Funds | 4,717,162 | $+0.0699^{***}$ | $+0.0516^{***}$ | 100,000 | $+0.0789^{***}$ | $+0.0462^{***}$ |
| Diversification Ratio | 3,570,516 | $-0.0064^{***}$ | $-0.0134^{***}$ | 97,699 | $-0.0106^{***}$ | $-0.0280^{***}$ |
| Correlation-Weighted Conc. | 3,568,973 | $+0.0165^{***}$ | $+0.0299^{***}$ | 97,688 | $+0.0195^{***}$ | $+0.0384^{***}$ |
| *Panel F: Extrapolation* | | | | | | |
| Extrapolation Bias | 4,612,051 | $-0.0163^{***}$ | $-0.0214^{***}$ | 100,000 | $-0.0430^{***}$ | $-0.0533^{***}$ |
| Extrapolation Bias (3m) | 4,545,865 | $-0.0228^{***}$ | $-0.0289^{***}$ | 100,000 | $-0.0543^{***}$ | $-0.0732^{***}$ |
| *Panel G: Gambling* | | | | | | |
| Gambling Bias | 4,475,220 | $-0.0173^{***}$ | $-0.0150^{***}$ | 100,000 | $-0.0371^{***}$ | $-0.0460^{***}$ |
| Gambling Bias (Strict) | 4,213,337 | $-0.0037^{***}$ | $+0.0042^{***}$ | 100,000 | $-0.0095^{**}$ | $-0.0198^{***}$ |
| *Panel H: Confirmation* | | | | | | |
| Confirmation Bias | 3,520,292 | $+0.0603^{***}$ | $+0.0572^{***}$ | 99,705 | $+0.0981^{***}$ | $+0.1232^{***}$ |
| Disconfirmation Bias | 3,319,970 | $-0.0687^{***}$ | $-0.0665^{***}$ | 98,235 | $-0.1021^{***}$ | $-0.1465^{***}$ |
| *Panel I: Herding* | | | | | | |
| Herding (Buy) | 4,596,186 | $-0.0307^{***}$ | $-0.0349^{***}$ | 100,000 | $-0.0542^{***}$ | $-0.0613^{***}$ |
| Herding (Sell) | 4,273,876 | $-0.0243^{***}$ | $-0.0173^{***}$ | 100,000 | $-0.0401^{***}$ | $-0.0052$ |
| Herding Bias | 4,612,051 | $-0.0265^{***}$ | $-0.0299^{***}$ | 100,000 | $-0.0497^{***}$ | $-0.0474^{***}$ |
| *Panel J: Skills & Experience* | | | | | | |

*(Continued)*

| | Investor-Month Panel | | | Investor-Level (Means) | | |
|---|---|---|---|---|---|---|
| Variable | N | Pearson | Spearman | N | Pearson | Spearman |
| Selection Skill ($S_t$) | — | — | — | 99,898 | $+0.0357^{***}$ | $+0.0486^{***}$ |
| Timing Rate | 4,571,144 | $+0.0005$ | $-0.0045^{***}$ | 99,891 | $+0.0014$ | $+0.0026$ |
| Fund Experience (months) | 4,645,675 | $+0.0807^{***}$ | $+0.1180^{***}$ | 100,000 | $+0.0835^{***}$ | $+0.1417^{***}$ |
| RA Experience (months) | 2,377,855 | $+0.0401^{***}$ | $+0.0571^{***}$ | 70,049 | $+0.0311^{***}$ | $+0.0518^{***}$ |

*Notes.* Pearson and Spearman rank correlations between investor age and each behavioral, skill, or experience measure. The investor-month panel uses the winsorised (1%/99%) and within-month standardised series; the investor-level columns collapse the panel to one observation per investor by averaging across months. Age is in years (raw, not standardised). N is the number of pairwise non-missing observations. Significance reflects the test that the population correlation is zero: $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$. In the investor-month panel the very large $N$ (≈2.4–4.7 million) drives most $p$-values below $10^{-50}$; the magnitude of the correlation, not statistical significance, is the relevant economic statistic. Investor-level correlations are systematically larger in absolute value (typically by a factor of 2–3) because aggregating to the investor mean removes within-investor month-to-month noise. Variable definitions follow Table 1.

Table A.19: The Structure of the Self-Directed Sleeve Within the RA-Adopter Sample

*Sample:* adopter-month panel (investors who hold RA in at least one sample month). *Estimator:* Fama–MacBeth OLS.
*Dependent variables* measured contemporaneously at month $t$.

| Regressor | (1) # Funds Held | (2) Portfolio HHI | (3) Self-Directed Share | (4) Total Turnover | (5) Speculative Turnover |
|---|---|---|---|---|---|
| Constant | 10.281*** | 0.403*** | 0.787*** | 0.283*** | 0.014*** |
| | (34.167) | (37.252) | (65.420) | (17.078) | (8.136) |
| Log(TFA)$_{t-1}$ | 7.117*** | -0.158*** | 0.114*** | -0.147*** | -0.003*** |
| | (40.125) | (-32.466) | (14.882) | (-14.298) | (-8.374) |
| Risk Preference | 0.759*** | -0.003*** | -0.010*** | 0.011*** | 0.000* |
| | (13.925) | (-3.525) | (-7.324) | (6.558) | (1.920) |
| Age | 0.755*** | 0.011*** | 0.007*** | 0.005*** | 0.001*** |
| | (29.920) | (11.470) | (5.249) | (5.809) | (7.522) |
| Female | -1.247*** | -0.009*** | 0.004*** | -0.035*** | -0.003*** |
| | (-21.829) | (-12.534) | (5.151) | (-20.842) | (-10.202) |
| Combined RA Dummy$_{t-1}$ | 8.927*** | -0.065*** | 0.022*** | -0.043*** | -0.004*** |
| | (18.519) | (-10.890) | (11.185) | (-12.959) | (-8.133) |
| Baby Assets$_{t-1}$ | -1.773*** | 0.031*** | -0.023*** | 0.042*** | 0.002*** |
| | (-31.001) | (41.477) | (-14.432) | (31.614) | (5.484) |
| Fund Experience (std)$_{t-1}$ | 2.158*** | -0.051*** | 0.041*** | -0.065*** | -0.001* |
| | (27.856) | (-15.822) | (9.171) | (-14.957) | (-1.740) |
| Selection Skill (std)$_{t-1}$ | -0.061 | 0.002 | 0.002** | 0.004 | 0.001** |
| | (-1.020) | (1.373) | (2.001) | (1.602) | (2.393) |
| Timing Rate (std)$_{t-1}$ | 0.010 | -0.000 | 0.001** | -0.000 | 0.001*** |
| | (0.266) | (-0.600) | (2.062) | (-0.058) | (3.214) |
| Province Dummies | Yes | Yes | Yes | Yes | Yes |
| N | 1,530,328 | 1,530,328 | 1,530,328 | 1,485,248 | 1,485,248 |
| Avg. $R^2$ | 0.152 | 0.206 | 0.145 | 0.142 | 0.009 |

*Notes.* Fama–MacBeth OLS on the *adopter-month panel* (investors who hold RA in at least one sample month); no selection correction. Each month, a cross-sectional OLS is estimated on the adopter panel; reported coefficients are the time-series mean of the monthly betas, with Newey–West HAC $t$-statistics (max lag $= 3$) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (two-tailed). *Dependent variables* at month $t$: (1) # Funds Held — count of distinct mutual funds in the self-directed book; (2) Portfolio HHI — Herfindahl concentration of self-directed fund weights; (3) Self-Directed Share $=$ MF assets / (RA $+$ MF); (4) Total Turnover — (buys $+$ sells)/lagged holdings in the self-directed book; (5) Speculative Turnover — turnover in speculative/short-horizon positions. *Controls* (all lagged one month): Log(Total Financial Assets), Combined RA Dummy, Baby Assets, Fund Experience, Selection Skill, Timing Rate. Age, Female, and Risk Preference are time-invariant and not lagged. Selection Skill and Timing Rate are standardised within month. Province dummies are included but not reported. *Key finding.* The coefficient on the lagged Combined RA Dummy describes the self-directed sleeve of an adopter in months with RA activity relative to adopter-months without it. In these months, adopters hold more self-directed funds ($+8.93$), exhibit lower portfolio concentration ($-0.065$), carry a larger self-directed share ($+0.022$), and trade less in both overall ($-0.043$) and speculative ($-0.004$) terms. The evidence is consistent with RA use coexisting with a broader, less concentrated, and more disciplined self-directed portfolio, rather than serving as a direct substitute for self-directed investing.