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Working Papers in Economics and Statistics

2019-21

University of Innsbruck
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University of Innsbruck
Working Papers in Economics and Statistics

The series is jointly edited and published by

- Department of Banking and Finance
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Delegation Decisions in Finance

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Abstract

Based on an online experiment with a sample of finance professionals and participants from the general population (acting as clients), we examine drivers and motives of clients' choices to delegate investment decisions to agents. We find that clients favor delegation to investment algorithms, followed by delegation to finance professionals compensated with an aligned incentive scheme, and lastly to finance professionals receiving a fixed payment for investing on behalf of others. We show that trust in investment algorithms or finance professionals, and clients' propensity to shift blame on others increase the likelihood of delegation, whereas clients' own decision-making quality is associated with a decrease in delegation frequency.

JEL: C93, G11, G41.
Keywords: Experimental finance, finance professionals, delegation decisions.

We thank Pol Campos-Mercade, Alain Cohn, Dawei Fang, Håkan J. Holm, Christian König-Kersting, Michael Razen, Markus Walzl, seminar participants at the Center of Social and Economic Behavior in Cologne, the Berlin Behavioral Economics Colloquium and Seminar, the Helsinki GSE Colloquium, as well as conference participants of the Organizations and Society Workshop at the University of Innsbruck, the NOeG Conference (Austrian Economic Association) in Graz, the SFB Workshop in Innsbruck, the Experimental Finance Conference in Copenhagen, the Decision-Making for Others Workshop in Nijmegen, and the Workshop on Trust and Cooperation in Markets and Organizations in Stavanger for very valuable comments. We particularly thank Fredrik Bergdahl at *Statistiska centralbyrån* for the excellent collaboration on the project. Financial support from the Austrian Science Fund FWF (START-grant Y617-G11 and SFB F63), the Austrian National Bank (grant 17788) and the Swedish Research Council (grant 2015-01713) is gratefully acknowledged. This study was ethically approved by the review boards at *Statistiska centralbyrån* (*SCB*; Statistics Sweden) and in Gothenburg (Sweden).

1. Introduction

Given the complexity of financial products and markets, private investors frequently opt for delegating decisions to finance professionals. This delegation involves decisions about portfolio investments, insurance and pension plans, and seeking advice on various other financial aspects. The economic importance of delegated decision-making in finance is indicated by the large and growing market for financial advice and decision-making on behalf of clients. As of 2021, the financial planning and advice industry in the United States comprises more than 144,700 businesses and employs more than 230,000 personal financial advisors. The industry revenues have been increasing at an annualized rate of 3.6% over the last five years, to an estimated $56.9 billion in 2021.[1]

When exploring the manifold motives for financial delegation and the relationship between investor characteristics and the demand for financial advice, many studies focus on clients' financial sophistication (see, e.g., Bhattacharya et al., 2012; Hackethal et al., 2012; Calcagno and Monticone, 2015; Kim et al., 2021), but other motives might play an important role too. Among the set of intuitively evident motivations for delegating financial investments, trust in the agent appears to be one of the most obvious one (Guiso et al., 2008). Gennaioli et al. (2015) hypothesize that if money managers are trusted, principals have confidence to let money managers take risk on their behalf. Gennaioli et al. argue that principals (clients) prefer to hire money managers over investing on their own because they are too nervous or anxious to make risky decisions themselves. Another motive for delegating investments is the possibility to blame the agent if the investment should not turn out as expected.[2] Yet another potential determinant of whether or not to delegate investment decisions is an agent's confidence in his/her own skills and sophistication, particularly relative to the expected proficiency of the agent. It stands to reason to hypothesize that overconfident clients are less likely to delegate their investments (see, e.g., Guiso and Jappelli, 2006; Bobadilla-Suarez et al., 2017).

Whether or not individual-level motives and characteristics actually drive the propensity to delegate investment decisions remains an empirical question. In the same vein, the question whether clients' delegation propensity varies with different types of agents and with the incentive schemes faced by the agents arises naturally and calls for empirical investigation. Regarding the latter, misaligned incentives between clients and agents have been portrayed as a driver of excessive risk taking (Rajan, 2006; Diamond and Rajan, 2009; Dewatripont and Freixas, 2012). When this debate has spilled over to the public, trust in the financial industry has been suffering (Sapienza and Zingales, 2012). This debate might also be one of the reasons why robo advisors have gained relevance in recent years. They promise affordable advice (D'Acunto et al., 2019), although algorithm aversion might hinder their acceptance (Germann and Merkle, 2019). Given the economic relevance of delegated decision-making in finance, it seems pivotal to better understand the determinants of delegation decisions and their implications. Particularly, we deem it relevant to empirically examine whether clients prefer human money managers—conditional on their incentive structure—over investment algorithms and which personal characteristics drive delegation choices.[3]

[1] https://perma.cc/9JXP-N83N

[2] For indirect empirical evidence on blame shifting motives in delegation decisions, refer to Shefrin (2007) and Chang et al. (2016). For a general account on blame shifting, see, e.g., Bartling and Fischbacher (2012).

[3] Note that we use the terms "principal" and "client" synonymously throughout the paper for our sample of participants from the general population; likewise, we apply the terms "money manager" and "agent" interchangeably for our sample of finance professionals.

It is our aim to investigate the interplay of clients' delegation decisions—moderated by their preferences, skills, and individual characteristics—and the type of money manager (investment algorithm or finance professionals facing different incentive structures) they can delegate to. We report from a delegation experiment with a sample of Swedish finance professionals and a representative sample of the Swedish general population. We implemented six experimental conditions, varying (i) the pool of participants enrolled (finance professionals or general population) and (ii) the type of money manager the principal can delegate to (investment algorithm, finance professional with aligned incentives, and finance professional with fixed payment). In 25 investment decisions, participants had to allocate an endowment across two or five investment alternatives that differed in their expected payout, riskiness, and diversification potential. Participants from the general population were thereafter given the opportunity to delegate their decisions by replacing their own investments with those of an agent (a finance professional or an investment algorithm, depending on the treatment condition) for determining their payout from the experiment. Invitations were sent out via *Statistiska centralbyrån* (*SCB*; Statistics Sweden). A total of 408 finance professionals and 550 people from the general population completed the experiment. The collaboration with *SCB* allowed accessing a stratified sample of finance professionals, restricted to financial analysts, investment advisors, traders, fund managers, and financial brokers. To the best of our knowledge, we are first to experimentally examine the drivers of delegation decisions in a financial context utilizing a specialized sample of finance professionals—i.e., skilled employees that take financial decisions on behalf of clients in their day-to-day work—and a general population sample instead of student participants. Additionally, we obtained a set of predefined variables of the participants' register data for those who completed the experiment from *SCB*.

Our study provides the following insights: First, we show that clients are most likely to delegate their investment decisions to an investment algorithm, followed by professionals facing aligned incentives and professionals compensated with a fixed payment. However, for clients who decide to delegate, we do not find systematic differences in their willingness to pay attributable to the varying treatment conditions. The latter may suggest that clients who choose to delegate, on average, assign the same value to the virtue of delegation, irrespective of the type of agent. Second, on the aggregate level, we observe that trust is a key determinant for delegating investments. This result is in line with the conjecture of trust being a consistent and major explanatory factor of financial market participation (see, e.g., Guiso et al., 2008). In particular, we find that trust in the agent is explanatory for delegation propensity, irrespective of the type of agent. Moreover, we report that blame shifting motives increase and clients' own decision-making quality decreases delegation propensity. Third, we find that principals tend to ask the agent to take higher levels of risk as compared to the perception of risk they took in their own decisions. This result is in line with the theoretically postulated explanation of finance professionals acting as "money doctors" (Gennaioli et al., 2015). Fourth, we comment on the external validity of our experimental results based on comprehensive robustness analyses. The analysis of principals' self-reported survey responses on real-life delegation decisions to finance professionals and investment algorithms further emphasizes the key role of trust in the agent for delegation decisions. Finally, we discuss potential limitations of our study in the conclusion.

Our study adds to several strands of related research. First, we contribute to the expanding literature on delegation decisions to investment algorithms and robo advice. This emerging literature offers important insights on the impact of robo advice on investors (see, e.g., D'Acunto et al., 2019; Rossi and Utkus, 2020b).

Previous research, focusing on various fields of applications, provides evidence on algorithm aversion, with people distrusting the advice from algorithms more than advice based on human judgement (see, e.g., Dietvorst et al., 2014; Harvey et al., 2017; Longoni et al., 2019). Yet, evidence on algorithm aversion is mixed: Germann and Merkle (2019), for instance, find that investors do not have strong preferences whether to tie their incentives to a human fund manager or an investment algorithm; other studies provide evidence on algorithms being preferred over human advice (e.g., Logg et al., 2017). Our findings join the rank of previous indications of algorithm appreciation. We show that participants are more likely to delegate risky decisions to an investment algorithm than to finance professionals in the context of an abstract allocation decision task. In previous studies on the demand of algorithm-based advice, individual-level characteristics play a rather minor role in explaining the heterogeneity in delegation decisions. Instead, clients' financial characteristics (such as, e.g., prior holdings or trading volume) have been more relevant to gauge the reliance on algorithmic advice (see, e.g., Rossi and Utkus, 2020b). One noteworthy exception is the survey evidence in a hypothetical setting provided in Oehler et al., 2021, showing that students with, among others, a higher willingness to take risk are more likely to report using robo advice. Similar to our agenda, Bhattacharya et al. (2012) examine the demand side of unbiased algorithmic brokerage. They find that clients who most need financial advice are least likely to obtain it. We contribute to the literature by not only focusing on delegation to investment algorithms, but comparing (determinants of) delegation decisions to investment algorithms and (human) finance professionals facing different incentive schemes. We show that trust in investment algorithms and a lack of clients' decision-making quality are important channels of delegation decisions to investment algorithms.[4]

Second, we add to the literature on incentives of money managers. A considerable body of theoretical literature has demonstrated that information asymmetries between clients and advisors allow advisors to act in their own interest—to the detriment of their clients (see, e.g., Bolton et al., 2007; Carlin and Manso, 2011; Inderst and Ottaviani, 2012b). The agency conflicts in the market for financial advisory services modelled in this literature have been supported by various empirical studies (see, e.g. Bergstresser et al., 2009; Hackethal et al., 2012; Mullainathan et al., 2012). In addition, misaligned incentives have been portrayed as major contributors to the financial crisis in 2008 and as main drivers of excessive financial risk taking in general (Rajan, 2006; Diamond and Rajan, 2009; Bebchuk and Spamann, 2010; Financial Crisis Inquiry Commission, 2011; Dewatripont and Freixas, 2012). Yet another strand of experimental studies suggests that even strong financial incentives hardly interfere with agents' attempt to adhere to their clients' preferences (see, e.g., Rud et al., 2018; Ifcher and Zarghamee, 2019; Kling et al., 2019). Since the debate about agency conflicts and misaligned incentives has gained widespread public attention and has negatively affected trust in the finance industry (Sapienza and Zingales, 2012), we hypothesize that clients' knowledge about the incentive scheme faced by the agent might play a role for their decisions whether or not to delegate their investment decisions. As there is virtually no research on the impact of incentives on customers' demand for financial advice, we contribute to the literature by showing that clients prefer delegating to advisors with aligned incentives rather than advisors compensated with a flat payment.

[4] Note that Bhattacharya et al. (2012) provide evidence that a lack of trust in and/or familiarity with the algorithmic advisor (measured by the length of relationship with the brokerage) and a lack of financial sophistication (proxied by poor past portfolio performance) explain why clients do not seek advice. To the best of our knowledge, we are first to examine individual-level drivers of delegating investments to robo advisors in a controlled experimental environment, which allows to forgo the necessity to rely on proxies of potentially relevant factors in observational studies. Yet, our results on trust and decision-making quality being important factors of delegation decisions to robo advisors appear to integrate well with the findings of Bhattacharya et al.

Finally, we add to the literature on trust in the finance industry (Sapienza and Zingales, 2012; Zingales, 2015) and related concepts like blame shifting and risk delegation. As noted in the survey by Rossi and Utkus (2020a), hiring financial (human or robo) advice is not only a decision to maximize returns, but to "satisfy a broader set of needs." For instance, Chang et al. (2016) show that delegation reverses the disposition effect by allowing the investor to blame the money manager, making it easier for the investor to sell losing assets. Moreover, stock market participation and financial development have been shown to be conditional on individuals' trust in general and in the finance sector in particular (Guiso et al., 2004, 2008). While trust has also been shown to be an important determinant of financial advice seeking (Lachance and Tang, 2012), the results by Georgarakos and Inderst (2011) suggest that trust in financial advice only matters for stock market participation when clients' perceived financial capabilities are low. With respect to delegated decision-making in finance, Gennaioli et al. (2015) argue that trust is an important factor for delegating financial decisions in a theoretical framework, and show that increased risk-taking is one of the potential motives of clients to delegate their investments to "money doctors." To the best of our knowledge, we are first to empirically examine Gennaioli et al.'s theoretical account on delegation being driven (partly) by clients' desire to increase risk-taking based on incentivized experiments. Our results second the argument by Bucciol et al. (2019) that trust acts as a substitute for risk tolerance: Trust in the agent might compensate for a lack in clients' risk-bearing capacities, such that even individuals who shy away from risk can—through delegation—engage in risky investments.

2. Experimental Design

Please note that there is a companion paper (Stefan et al., 2021) which is based on the *same experiment*. An earlier working paper version presented the results on various aspects related to the delegated investment decisions in a single manuscript (Holzmeister et al., 2019). During the revision process, we split our contribution in two separate papers to discuss our objectives, our results, and their implications in a more targeted manner. In particular, the companion paper (Stefan et al., 2021) focuses on a different set of variables to address risk communication in delegating risky investments and whether delegation to finance professionals is beneficial for clients in terms of the risk-return trade-off. Since both papers are based on the same experimental implementation and data, substantial parts of the description of the experimental design and the relevant variables as well as their construction are identical in both papers.

Allocation decision task. The workhorse of our experiment is the allocation decision task as used by Banks et al. (2018). The task consisted of 25 investment decisions, in which participants were asked to allocate an endowment of 100 SEK on either two or five assets.[5] Participants were informed about the assets' returns per 1 SEK invested, depending on whether a coin toss shows up heads or tails. The returns used in the experiment were adopted from Banks et al. (2018), multiplied by a factor of 1.5, and rounded to one decimal place. The returns for each asset in the 25 investment decisions are depicted in Table 1, and the corresponding opportunity sets are illustrated in Figure B1 in Appendix B.

[5] By the end of February 2019, the exchange rate between US dollars and SEK was about 1:9; the exchange rate between the Euro and SEK about 1:10.5.

Table 1: Return distributions of the available assets in the 25 opportunity sets. This table shows the returns (in SEK) per 1 SEK invested for the different assets in the 25 opportunity sets, depending on whether the coin toss shows up heads or tails. Within the blocks of two (sets #1–10) and five assets (sets #11–25, the decision problems were randomized in order.

	Asset A		**Asset B**		**Asset C**		**Asset D**		**Asset E**	
Set	*Heads*	*Tails*	*Heads*	*Tails*	*Heads*	*Tails*	*Heads*	*Tails*	*Heads*	*Tails*
#1	0.00	1.20	3.60	0.00						
#2	3.60	0.00	0.00	1.80						
#3	4.80	0.00	0.00	1.20						
#4	2.30	0.00	0.00	4.50						
#5	0.00	2.40	2.40	0.00						
#6	1.20	0.00	0.00	4.80						
#7	0.00	2.30	4.50	0.00						
#8	0.00	3.60	1.80	0.00						
#9	0.00	2.70	3.00	0.00						
#10	1.20	0.00	0.00	3.60						
#11	0.30	2.70	0.90	0.90	1.20	0.00	0.60	1.80	0.00	3.60
#12	0.80	1.50	2.40	0.00	0.40	2.10	1.80	0.80	0.00	3.00
#13	2.30	0.60	0.40	1.50	0.00	2.40	1.50	0.90	3.00	0.00
#14	0.50	4.10	1.80	0.00	0.00	5.40	0.90	2.70	0.50	0.50
#15	2.70	0.30	3.60	0.00	0.00	1.20	0.90	0.90	1.80	0.60
#16	2.00	1.20	3.50	0.40	4.50	0.00	0.00	3.00	1.10	2.30
#17	1.40	0.20	0.00	1.80	0.50	1.40	0.80	0.80	1.80	0.00
#18	2.70	0.50	3.60	0.00	0.90	1.40	0.00	1.80	1.80	0.90
#19	0.00	2.40	2.40	0.00	1.80	0.60	0.60	1.80	1.20	1.20
#20	0.00	4.50	3.00	0.00	2.00	0.80	0.40	3.50	1.50	2.30
#21	0.00	3.60	2.70	0.90	3.60	0.00	1.50	1.50	0.60	2.70
#22	2.40	0.40	1.80	0.80	0.00	2.40	3.60	0.00	0.90	1.80
#23	0.30	2.70	1.50	0.60	1.20	1.80	2.40	0.00	0.00	3.60
#24	5.40	0.00	2.70	0.90	0.50	0.50	0.00	1.80	4.10	0.50
#25	0.50	2.70	1.80	0.00	1.40	0.90	0.90	1.80	0.00	3.60

The task consists of 10 decisions with two binary assets in a first block, and 15 decisions with five binary assets in a second block. Participants were first presented with the task instructions for the first block. After reading the instructions, participants could only continue once they had correctly answered three comprehension questions.[6] After the first ten decisions, participants were informed that five rather than two assets would be available for the remaining 15 decisions. The order of the two blocks was fixed for all participants, but the order of decisions was randomized in each of the two blocks. Figure A1 in Appendix A shows two screenshots of the main experimental task, i.e., the allocation decision task, with two and five assets, respectively.

At the end of the experiment, one of a participant's own or—in case a client opts for delegating the decisions—one of the agent's decisions was randomly chosen, and a simulated coin toss determined the participant's payoff. Returns were paid on top of the endowment, i.e., final payments could not fall below 100 SEK.

[6] Participants who did not answer the comprehension questions correctly, had the opportunity to look at the instructions again until they got the answers right. In addition, they received hints on the correct answers.

Decision-making quality index: Similar to Banks et al. (2018), we determine four measures of decision-making quality (*DMQI*) based on the allocation decision task in our experiment. In the following, we provide an overview over the four measures, while details are provided in Appendix C.

First, violations of first order stochastic dominance (*FOSD*, Hadar and Russell, 1969) are measured by the difference between the expected return of a portfolio chosen by the participant and the maximum expected return of a feasible portfolio that has the same same minimum payoff as the chosen one. This idea can be illustrated using the example of opportunity set #1 as described in Table 1 above: Asset A_1 (B_1) yields a payoff of 1.20 SEK (3.60 SEK) if the coin shows up tails (heads) and 0.00 SEK otherwise. Suppose a participant i chooses portfolio $x_{i,1} = (80, 20)$, i.e., she allocates 80 SEK of the endowment to asset A_1 and 20 SEK to asset B_1. If the coin shows up heads, participant i receives a payoff of $20 \cdot 3.60 = 72$ SEK and if the coin shows up tails, she receives $80 \cdot 1.20 = 96$ SEK, thus yielding an expected portfolio return of 84 SEK. Apparently, the portfolio $x_{i,1}$ is first-order stochastically dominated: Choosing the allocation $x'_{i,1} = (60, 40)$ would guarantee the same minimum payoff ($60 \cdot 1.20 = 72$ SEK) but a higher maximum payoff ($40 \cdot 3.60 = 144$ SEK). The expected return of this alternative portfolio $x'_{i,1}$ is 108 SEK, thus exceeding the expected return of portfolio $x_{i,1}$ by 24 SEK. This difference in expected returns constitutes our measure of the *FOSD* violation. Note that choosing portfolio $x_{i,1}$ rather than $x'_{i,1}$ implies that a sizeable fraction of the prospective reward—conditional on the fact that both portfolios result in the same minimum payoff—will be left on the table, namely 24 SEK $\div$ 108 SEK $= 22.2\%$.

Second, violations of the *Generalized Axiom of Revealed Preferences* (*GARP*) are measured using the *Money Pump Index* (Echenique et al., 2011). The intuition behind our measure is that a decision-maker violating *GARP* could be exploited as a "money pump" by an arbitrageur who replicates the chosen portfolios at lower cost and sells them to the decision-maker at higher prices. Again, this measure can be illustrated extending the example of participant i choosing portfolio $x_{i,1} = (80, 20)$ in opportunity set #1 as described above. Now consider opportunity set #10, which is a mirror versions of set #1: Asset A (B) yields a payoff of 1.20 SEK (3.60 SEK) if the coin shows up heads (tails) and 0.00 SEK otherwise. Suppose participant i chooses portfolio $x_{i,10} = (90, 10)$. The portfolio choices $x_{i,1} = (80, 20)$ and $x_{i,10} = (90, 10)$ are not only first-order stochastically dominated (as per the argument sketched above), but also violate the *Generalized Axiom of Revealed Preferences*. The portfolio $x_{i,10}$ yields a payoff of $90 \cdot 1.20 = 108$ SEK if the coin shows up heads and a payoff of $10 \cdot 3.60 = 36$ SEK if the coin shows up tails. Note that the same portfolio is feasible in opportunity set #1 without spending the entire endowment. Allocating 30 SEK of the endowment into asset A_1 and 30 SEK into asset B_1 would replicate the very same portfolio (with payoffs $30 \cdot 3.60 = 108$ SEK in case the coin shows up heads and $30 \cdot 1.20 = 36$ SEK in case it shows up tails). An arbitrageur could thus buy the replicated portfolio in opportunity set #1 yielding an expected return of 72 SEK at a "price" of only 60% of the endowment in opportunity set #1 and sell it to participant i at a "price" of 100% in opportunity set #10, involving a risk-free profit $0.40 \cdot 72$ SEK $= 29$ SEK. Following the same line of reasoning, $x_{i,1}$ can be constructed in opportunity set #10. To obtain a payoff of 72 SEK (heads) and 96 SEK (tails), respectively, in opportunity set #10, one would need to invest 60 SEK of the endowment in asset A_{10} and $26.\dot{6}$ SEK in asset B_{10}. Again, a hostile arbitrageur could buy the replicated portfolio (yielding an expected return of 84 SEK) at a "price" of $86.\dot{6}$ SEK in opportunity set #10 and sell it to participant i at a "price" of 100 SEK in opportunity set #1, involving a risk-free reward of 14 SEK. Since participant i's allocation decisions $x_{i,1}$ and $x_{i,10}$ can be replicated at an expense of less than 100% of the endowment in the respective other opportunity set each, she leaves a total of $29 + 14 = 43$ SEK—or 24% of the expected returns of the two portfolio decisions—in prospective rewards to a fictive arbitrageur.

Third, financial competence (*FC*) is a measure of a participant's ability to understand the task at hand: In our experiment, four opportunity sets were presented in both the two-assets- (sets #1, #2, #8, and #10) and the five-assets-frame (sets #11, #15, #18, and #25). Moreover, two of the four opportunity sets presented in the two-assets- and five-assets-frame, respectively, were constructed as mirror images of one another, i.e., only the payoffs for heads and tails were interchanged. Consequently, two opportunity sets were presented four times each (#1 = #10 = #11 = #15 and #2 = #8 = #18 = #25). Assuming that a financially competent investor would understand the opportunity sets and invest consistently, we measure a participant's financial competence for identical opportunity sets as the absolute differences between the expected returns of the chosen portfolios.

Fourth, failure to minimize risk (*FMR*) is based on the assumption of a risk averse investor:[7] In our experiment for two opportunity sets (#5 and #19 in Table 1), the expected return per 1 SEK invested was the same for all assets k. Consequently, all feasible portfolios in these opportunity sets share the same expected return. Thus, choosing a fully-hedged portfolio (second-order) dominates all other feasible portfolios in these two opportunity sets. A participant's failure to minimize risk in these opportunity sets is measured as the standard deviation $SD_{i,j}$ of the particular portfolio allocation.

Each measure is averaged across all (relevant) opportunity sets. To construct a composite measure of decision-making quality, we conduct a principal component analysis of the four measures described above (see Table C1 in Appendix C). In particular, the first principle component constitutes our decision-making quality index (*DMQI*). Table C2 in Appendix C shows the pairwise correlations between the decision-making quality measures for both the general population sample and the finance professionals sample. Violations of *FOSD* and *GARP* and, to a lesser extent, the *FC* measures are positively correlated for both groups. While the *FMR* measure is also correlated with the other measures for the general population sample, the correlations for the finance professionals turn out to be insignificant.[8]

Experimental treatments. Depending on the subject pool, participants were randomly assigned to one of the treatments listed in Table 2. Common to all treatments, both for finance professionals and for the general population sample, is the 25-item allocation decision task, which is described in detail above.

After having completed all items of the allocation decision task, participants from the general population (principals) had the opportunity to delegate their decisions to an agent. If principals opted for delegating their decisions, the experimental payoff depended on the agent's rather than their own decisions. The design choice that principals made the investment decision first, but were informed about the opportunity to delegate the investment decisions only afterwards, warrants further discussion. While potential "clients" that do not want to engage in financial matters at all might have dropped out at the outset of

[7] Note that the assumption of risk aversion is explicitly made in this particular measure of decision-making quality. Risk-aversion seems a reasonable assumption to make for our subject pool. In the survey response on financial risk tolerance (see Table 3), participants from the general population provide mean (median) responses of 4.28 (4) on a Likert-scale from 0 to 10 and, thus, can be classified as risk averse on average.

[8] It could be argued that this might be driven by the fact that the *FMR* measure does not necessarily capture decision mistakes, as failures to minimize risk could also be driven by preferences for risk-seeking. However, we follow Banks et al. (2018) in their choice of components for decision-making quality. Moreover, we use the first principal component of the four measures (in contrast to Banks et al. (2018); see SectionC in the Appendix) as our *DMQI* variable, with the consequence that uncorrelated influences—such as risk preference—should not pose a systematic issue for our measure.

the experiment,[9] our design involves two advantages: First, the opportunity to delegate without prior decisions could have lead to delegation in order to receive an experimental payment without spending any effort. Second, our experimental design allows studying whether or not delegation decisions depend on the decision-making quality of the participants, since investment choices are observed for all participants, irrespective of their decision whether or not to delegate.

Table 2: Treatment overview. This table illustrates the randomly assigned between-subjects treatment conditions for both samples, finance professionals and participants from the general population. The sample sizes per condition are indicated in Figure 1.

	Finance professionals *… make decisions …*		**General population** *… can delegate decisions to …*
FP-OWN	on one's own account	*GP-ALGO*	investment algorithm
FP-ALIGNED	for third party (linear incentives)	*GP-ALIGNED*	finance professional (linear incentives)
FP-FIXED	for third party (flat payment)	*GP-FIXED*	finance professional (flat payment)

Depending on the treatment, the principals' had the opportunity to delegate *either* to an investment algorithm programmed by the experimenters (*GP-ALGO*), a finance professional facing aligned, i.e., linear, incentives (*GP-ALIGNED*), or a finance professional receiving a flat payment of 200 SEK for deciding on behalf of one or more clients (*GP-FIXED*). Note that, compared to the baseline condition *GP-FIXED*, treatment *GP-ALIGNED* modifies the incentive structure of the agent, while holding the type of agent constant. In contrast, treatment *GP-ALGO* modifies the type of agent from a human to an investment algorithm.

If principals chose to delegate, they were asked to specify the risk (on a scale from 1 [no risk] to 4 [maximum return]) they wanted to be taken on their behalf by the agent,[10] as well as their willingness to pay for delegating the investment decisions (between 0 and 50 SEK, in steps of 5 SEK). At the end of the experiment, a "price" for delegating the decision to the agent (between 0 and 50 SEK) was randomly determined: If a participant's willingness to pay was higher than this random number, his/her decisions were delegated to the agent at the randomly determined price (i.e., the agent's decisions were payoff-relevant for the principal); if not, the principal's own decisions were relevant for the payment in the experiment.

Finance professionals were randomly assigned to one of three treatments in which they either made decisions on their own account (*FP-OWN*), or on behalf of (one or more) participant(s) from the general population sample. When deciding on principals' account, finance professionals either faced aligned incentives (i.e., they received exactly the same monetary payoff as (one of) their client(s); *FP-ALIGNED*), or were paid a flat fee of 200 SEK (*FP-FIXED*). Moreover, when deciding on behalf of others, finance professionals were

9 For a comprehensive response rate analysis and a discussion of potential self-selection effects, please refer to Appendix E.

10 The investment algorithm was programmed to construct investment portfolios, given the particular risk level, as follows: In each investment decision, the minimum variance portfolio and the maximum return portfolio were mapped to the endpoint options of the risk level scale, i.e., 1 and 4, respectively. Thus, risk level 1 was always associated with a sure payoff, whereas risk level 4 always involved a 100% investment in the asset with the highest expected return. For risk levels 2 and 3, portfolio weights were determined in equally sized steps between these fixed endpoints. For instance, if payoffs were 2.40 SEK / 0.00 SEK for asset A and 0.00 SEK / 0.80 SEK for asset B, then the risk-free portfolio was characterized by an investment of 25% in A and 75% in B, whereas the maximum return portfolio corresponded to an investment of 100% in A. Risk levels 2 and 3 were associated with portfolios investing 50% and 75% in A, respectively. To the participants the algorithm was described to be "programmed in such a way that it maximizes your expected profit conditional on the risk level you indicate below". Please note that participants still have to trust that the algorithm operates as described in this statement.

asked to comply with a randomly assigned risk level (between 1 [no risk] and 4 [maximum return]). In case a participant from the general population delegated his/her decisions, he/she was matched with a participant from the finance professional sample, based on the particular treatment *and* the stated risk level. All details about the delegation decision itself, the risk levels as a means to communicate the desired riskiness of the allocation decisions, the matching modalities, as well as the payment procedures were common knowledge.

Questionnaires. After the allocation decisions (but prior to the choice whether or not to delegate), all participants were asked to self-assess the overall level of risk taken across the 25 items of the allocation decision task on a scale from 1 to 4, i.e., on the same scale as when choosing the risk level when delegating the risky decisions. In addition, we included the following set of non-incentivized survey items at the end of the experiment: All participants were asked about (i) their self-assessed risk attitude in general and in financial decisions (Dohmen et al., 2011; Falk et al., 2016), (ii) their willingness to abstain from something today for a future benefit (Falk et al., 2016), (iii) their trust in mankind in general, in persons from the finance industry, and in investment algorithms, (iv) their proneness to shift blame on others (Wilson et al., 1990), and (v) their level of prosociality in a hypothetical charitable giving setting (Falk et al., 2018). The survey items included in the regression analysis (reported in Table 4 below) are summarized in Table 3.[11]

Table 3: Selected survey questions. This table summarizes the Likert items used in the regression analysis below. The table depicts the variable description, the wording of the question/statement, and the corresponding labelling of the minimum and maximum values for each item.

		Likert Scale	
Variable	***Question / Statement***	*min (0)*	*max (10)*
Risk Tolerance in Financial Matters	I am generally willing to take risks in financial matters.	does not describe me at all	describes me perfectly
Trust in Finance Professionals	I generally trust employees from the finance industry.	does not describe me at all	describes me perfectly
Trust in Investment Algorithms	I generally trust robo-advisors (i.e. computer programs) in financial matters.	does not describe me at all	describes me perfectly
Blame Shifting (Others)	If you hurt yourself accidentally, do you sometimes blame somebody who happens to be nearby even though you realize, on reflection, that they were not responsible?	I never blame others	I often blame others
Blame Shifting (Temptation)	Can you easily resist the temptation to blame others for the accidents that happen to you?	I can resist easily	I cannot resist at all

Furthermore, we included a 5-item questionnaire on delegation and advice-seeking in financial decisions, which was only posed to participants that indicate that they have been active in the financial market. Afterwards, all participants had four minutes to answer an eight-item Rasch-validated numeracy inventory (Weller et al., 2013), including two questions on cognitive reflection. In addition, participants had to provide their self-assessment of the number of correct answers in the numeracy questionnaire as well as their ranking compared to a random sample of the Swedish population. These assessments allow us constructing

[11] The full list of survey items is provided in Table D1 in Appendix D.

two measures of overconfidence, i.e., overestimation and overplacement. Finally, participants had three minutes to answer a six-item financial literacy questionnaire based on van Rooij et al., 2011. For further details regarding the survey items, please refer to Appendix D.

Recruitment and data collection. We conducted the online experiment in Sweden in cooperation with *Statistiska centralbyrån* (*SCB*; Statistics Sweden), who invited participants for the experiment and provided a set of predefined variables of the participants' register data for those who completed the experiment. *SCB* sent out invitations (including a hyperlink to the online experiment and a personalized alphanumeric identifier serving as login credentials) to 8,215 finance professionals and a randomly selected representative sample of 8,215 participants from Sweden's working general population, excluding finance professionals. The sample of finance professionals consists of financial analysts and investment advisors, traders and fund managers, and financial brokers. For the general population, following Edin and Fredriksson (2000) and Böhm et al. (2018), we only include people with a declared labor income exceeding the minimum amount that qualifies for the earnings related part of the public pension system. Invitations were sent out in two waves. In the first week of 2019, 20% of the sample were invited. Since no technical issues had arisen, the remaining 80% of the sample were invited in the third week of 2019.

Once participants logged in to the online software, programmed in *oTree* (Chen et al., 2016), using their personal identifier, they were presented with a detailed outline of the experiment. In particular, on the first screen, participants were informed that register data provided by *SCB* will be matched with the data collected in the experiment. Moreover, participants were informed that the study has been approved by the ethical review boards in Gothenburg and at *Statistiska centralbyrån*. Participants agreed upon the conditions and were directed to the instructions of the experiment. The data handling procedures ensured full pseudonymity of all participants. Further details and additional information on the recruitment, data collection, and experimental implementation are provided in Appendix A. Complete versions of the experiment and all treatments (in English) are available online via http://hea-2019-01-en.herokuapp.com.

In total, 408 finance professionals and 550 people from the general population completed the experiment. The experiment was conducted in Swedish and took on average 45 minutes to complete. The average payment to participants was 238.9 Swedish Krona (SEK; *SD* = 122.3), which is approximately $30 given the exchange rate at the beginning of 2019.[12] The experimental data was collected between January 4 and February 10, 2019.[13]

The sequence of tasks within the experiment is graphically summarized in Figure 1. For further information on the main task, please refer to Appendix B. Details on the side tasks and questionnaires are provided in Appendix D. Analyses on participants' decision times conditional on subject pools, treatments, and tasks are summarized in Appendix F.

[12] Thus, the average hourly salary for experimental participants amounts to approximately $40. This is comparable to other studies with general population samples (e.g., Andersson et al., 2016, 2019). This average annual salary is also comparable—although on the lower end—to other studies with finance professionals (e.g., Haigh and List, 2005; Kirchler et al., 2018, 2020; Weitzel et al., 2019).

[13] In total, only a relatively small fraction of participants—especially for an online experiment—dropped out during the experiment. In sum, 68.9% of all participants that started actually finished the experiment (i.e., 958 out of 1.391). The fraction of completes was 66.3% among the general population and 72.7% among finance professionals, hinting at relatively low and comparable attrition rates across subject pools.

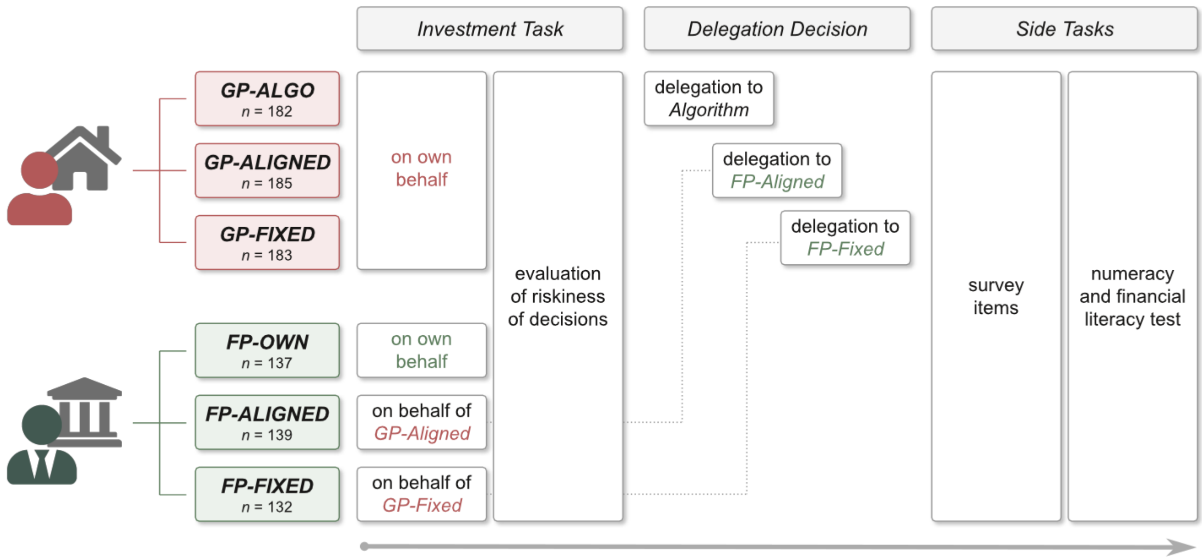

Figure 1: Flow chart of the experiment. This figure illustrates the sequence of tasks for participants in our experiment. First, participants were randomly assigned to one treatment and completed 25 investment decisions. Then, participants from the general population could delegate their investment decision to an agent in a delegation decision stage. Finally, all participants completed several side tasks, including self-reported items on economic preferences and supplementary survey questions, a financial literacy test, and a numeracy inventory.

Register data. In addition to the data collected in the online experiment, we obtained register data from *SCB* for each participant who completed the experiment. In particular, we received data on demographics (e.g., age, gender, income), occupational history (e.g., workplace, firm size), participants' education, their wealth history, and military records (e.g., scores of the military suitability tests). See Appendix A for further details on these variables. In the analysis of experimental results we only use part of the registry data as control variables—in particular, participants' gender (binary indicator for female), age (in years), net income from major employment in 2017 (in thousand SEK's), and maximum education level (dichotomous indicators for high school education or less, university education smaller or equal to three years, and university education larger than three years).[14]

3. Results

Sample Descriptives. Participants in both the finance professional and general population sample are, on average, 42 years old $sd = 11$ years). The ratios of male participants among finance professionals and participants from the general population are 75.3% and 55.4%, respectively. The high fraction of males in the finance industry is typical for the job functions under investigation and has also been found in previous studies (see, e.g., Kirchler et al., 2018; Weitzel et al., 2019). The average gross income (from major employment) among finance professionals amounts to SEK 722,046 ($sd = 547{,}815$) and close to 80% hold a University degree. The mean income of the general population is SEK 393,706 ($sd = 259{,}726$) with

[14] Please note several restrictions in the register data: First, the records for wealth data of *SCB* end in 2007 and other potentially relevant data such as portfolio holdings of assets or bank account data is not tracked by *SCB*. Second, adding data from the military suitability tests would have lowered the sample size by close to 40 percent, as all female participants would have been dropped from the analyses. Focusing only on male participants would have lowered the generalizability of our findings for both the general population and the finance industry.

about 50% holding a University degree. On average, participants from the general population took 5.5 and 15.2 minutes for the two-assets and the five-assets decisions in the investment tasks, respectively; the corresponding numbers for finance professionals are 7.0 and 18.4 minutes in treatment *FP-FIXED*, 7.3 and 19.1 minutes in treatment *FP-ALIGNED*, and 5.2 and 15.7 minutes in treatment *FP-OWN*, respectively. Further summary statistics for both samples and details on the times spent on the experimental tasks are presented in Appendix E and Appendix F.

In the following, we first examine principals' decisions to delegate across treatments and then identify potential drivers of delegation decisions. Finally, we discuss the robustness and external validity of our experimental results based on an analysis of participants' self-reported delegation behavior in real-life financial decisions. Note that throughout the presentation of the results, we indicate effect sizes in terms of marginal effects at the means (*MEM*) and/or odds ratios (*OR*) for non-linear models.

Result 1. *Delegation rates are highest when principals can delegate to the investment algorithm, followed by the treatment in which finance professionals face aligned incentives and the treatment in which professionals are compensated with a flat payment. Principals' willingness to pay for delegation (provided they chose to delegate) does neither depend on the type of the agent nor on the agent's incentives.*

Support: Panel (A) of Figure 2 shows the fractions of principals delegating their investment decisions to the agent for each of the three treatments. The estimates are based on logit regressions ($n = 550$; robust standard errors) of the binary delegation choice on treatment indicators. Compared to the delegation rate of 16.9% in treatment *GP-FIXED*, we find that participants are significantly more likely to delegate their investment decision to the agent in treatment *GP-ALIGNED* (25.9%; *MEM* $= 0.090$, $p = 0.034$), and in treatment *GP-ALGO* (37.9%; *MEM* $= 0.210$, $p < 0.001$). The difference in delegation rates between the treatments *GP-ALIGNED* and *GP-ALGO* is statistically significant too (*MEM* $= 0.120$, $p = 0.013$). These results suggest that principals take into account both the type of the agent (i.e., whether the agent is an algorithm or a finance professional) and the agents' incentives (fixed or aligned compensation) in their delegation decisions.

Panel (B) of Figure 2 depicts the principals' mean willingness to pay for delegating their investment decisions to the agent, conditional on the treatment. Note that the willingness to pay is only elicited for participants that chose to delegate ($n = 148$). The estimates are based on an ordinary least squares regression of principals' willingness to pay on treatment indicators (with robust standard errors). Although delegation rates vary considerably across treatments, we report no statistically significant differences between treatments in principals' willingness to pay for delegation. That is, we do neither find evidence that the willingness to pay for delegation (of principals who chose to delegate) differs conditional on whether the agent is a finance professional or an investment algorithm, nor on whether finance professionals face a flat compensation or a linear incentive scheme. While there is a clear ranking of delegation preferences to agents (i.e., investment algorithms $\succ$ finance professionals with linear incentives $\succ$ finance professionals with flat compensation), those clients that actually delegated their decisions might consider delegation as having equal merit, independent of the agents' incentives and whether the agent is an investment algorithm or a human. This might be the reason why the willingness to pay for delegation does not differ across treatments.

Result 1 provides a clear indication that principals' delegation decisions are affected by both the type of the agent and the incentives faced by human agents—i.e., factors that have been *exogenously* varied between

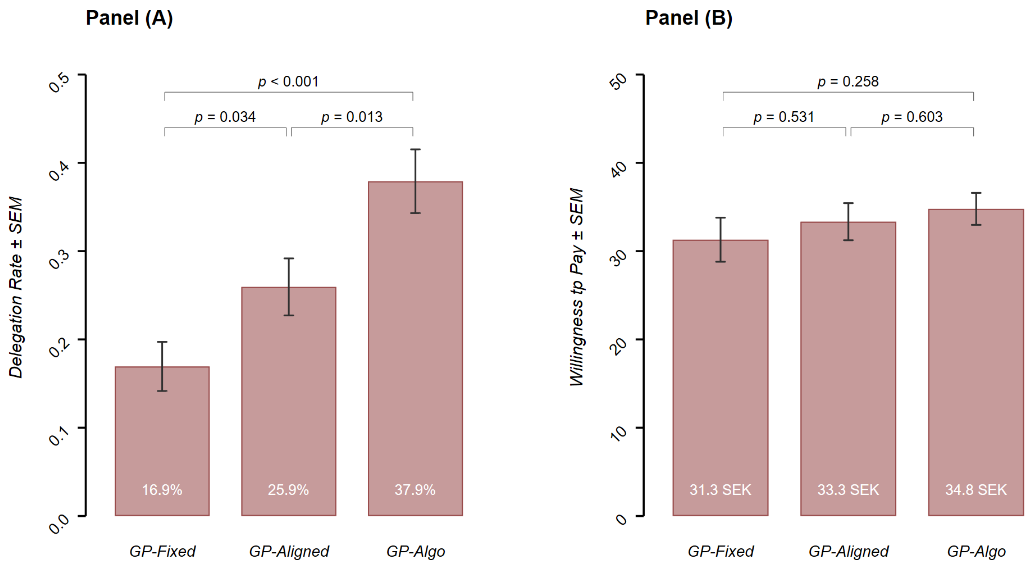

Figure 2: Delegation decisions separated by treatment conditions. **Panel (A)** depicts the share of principals opting for delegating their investment decisions to the agent conditional on the treatment. Error bars indicate standard errors of the mean (*SEM*); p-values are based on a logit regression of delegation on treatment indicators (with robust standard errors; $n = 550$). **Panel (B)** shows the mean willingness to pay for delegating the investment decisions of principals who chose to delegate. Error bars indicate standard errors of the mean (*SEM*); p-values are based on an ordinary least squares regression of willingness to pay on treatment indicators (with robust standard errors, $n = 148$).

the treatments in our experiment. The question whether the variation across treatments can be explained by the heterogeneity on the participant level arises naturally. Thus, in a next step, we investigate whether the *endogenous* variability in individual-level characteristics of principals have a systematic effect on the likelihood of delegating financial investment decisions, and whether these effects explain the observed differences in delegation rates between treatments.

Result 2. *Principals' propensity to delegate their investment decisions increases with trust in the agent. On aggregate, blame shifting motives tend to increase the propensity to delegate whereas the principals' decision-making quality tends to decrease the likelihood of delegation. Principals that delegate their investment decisions, on average, request the agent to take more risk than they perceive to have taken in their own decisions.*

Support: Table 4 reports the estimates obtained from logit regressions of principals' delegation decisions on various experimental and self-reported measures, conditional on the treatment (models 1–3) and pooled across all treatments (model 4). We find that the principals' decision whether to delegate is significantly driven by the principals' trust in the agent.[15] However, we find that the effect size estimates of the trust variable vary considerably across treatments. The odds of delegating one's decision to a finance professional compensated with a fixed payment (*GP-FIXED*; model 1) are expected to increase by 75.7% (*MEM* $= 0.068$, $p = 0.028$) for a one standard deviation increase in principals' trust in finance professionals. If the agent is a finance professional facing linear incentives (*GP-ALIGNED*; model 2), the odds of delegation, on average, increase by 103.3% (*MEM* $= 0.121$, $p < 0.001$) for a one standard deviation increase

[15] Refer to the table notes of Table 4 for details on the construction of the trust variable.

in trust. For the treatment in which clients can delegate their decisions to an investment algorithm (*GP-ALGO*; model 3) the effect of trust turns out to be largest: a one standard deviation increase in principal's trust in investment algorithms, on average, gives rise to an increase of 170.5% (*MEM* $= 0.174$, $p < 0.001$) in the odds of delegating the investment decision to the agent. Notably, however, the differences in the effect size estimates of trust between treatments—just as for all other coefficient estimates in the multi-variate regression analyses—turns out not to be statistically different from zero for all pairwise comparisons (see the table notes of Table 4 for details). In light of these findings, illustrating the relevance of trust for delegating financial decisions, experimental participants' self-reports on trust are particularly revealing: As reported in Appendix H, trust levels are, on average, higher towards the general population than towards finance professionals—even among their peers—as well as robo advisors.

Apart from trust being a significant driver of delegation decisions, we find that, pooled across treatments (model 4), blame shifting is significantly and positively associated with delegation propensity, whereas clients' decision-making quality is significantly and negatively related to the likelihood of delegation. On the treatment level, we find that the odds of delegating to a finance professional compensated with a fixed payment (*GP-FIXED*), on average, are expected to increase by 60.5% (*MEM* $= 0.057$, $p = 0.041$) for a one standard deviation in blame shifting. While the effect of blame shifting turns out being positive for the other treatments as well, the magnitudes are smaller and the effect is not statistically different from zero (*GP-ALIGNED*: *OR* $= 1.163$, *MEM* $= 0.026$, $p = 0.389$; *GP-ALGO*: *OR* $= 1.360$, *MEM* $= 0.053$, $p = 0.108$). With respect to the impact of principals' decision-making quality, we report a significant effect for treatment *GP-ALGO*: for a one standard deviation increase in decision-making quality, the odds of delegating to the investment algorithm are expected to decrease by 36.2% (*MEM* $= -0.078$, $p = 0.019$). However, the effect of *DMQI* on the likelihood of delegation turns out being smaller in magnitude and not statistically significant for the other two treatments (*GP-FIXED*: *OR* $= 0.695$, *MEM* $= -0.044$, $p = 0.140$; *GP-ALIGNED*: *OR* $= 0.963$, *MEM* $= -0.006$, $p = 0.803$).

Finally, we report that neither clients' numeracy skills and financial literacy scores, nor the two measures of overconfidence, nor participants' (self-reported) attitudes towards risk in financial matters are statistically significantly associated with principals' delegation decisions—irrespective of the treatment condition.[16]

One potential goal behind delegating investment decisions—related to our finding on trust as a driver of delegation—is to increase risk-taking. If principals trust the agents, delegation may permit them to take higher risk (Gennaioli et al., 2015). Indeed, our experimental results indicate that principals seek to increase risk-taking through delegation. Figure 3 illustrates the principals' desired levels of risk (when delegating their decision to the agent) conditional on the risk perception of their own investment decision. On average, principals tend to ask the agent to take higher levels of risk ($m = 2.84$, $sd = 0.69$) than they perceive they implemented themselves when deciding on their own behalf ($m = 2.58$, $sd = 0.76$; paired t-test: $t(147) = 4.081$, $p < 0.001$, $n = 148$). This result provides experimental support of the argument by Bucciol et al. (2019) that trust (at least partly) compensates for limited risk-bearing capacities of clients,

[16] Note that our finding, that delegation decisions cannot be explained by overconfidence, is in line with the results by Bobadilla-Suarez et al. (2017), arguing that a lack of explanatory power of overconfidence seems to reflect an intrinsic value for choice. Such a "choice premium" may indeed account for part of the unexplained variation in our data as well. Also note that the results by Guiso and Jappelli (2006) indicate that the propensity to delegate relates negatively to risk tolerance. In contrast to these results, we do not find support for a systematic direct effect of risk attitudes on participants' delegation decisions in our experimental setting.

Table 4: Determinants of delegation choices. This table reports marginal effects estimates based on logit regressions of the binary choice whether to delegate the investment decision to the agent on a set of experimental and self-reported measures, conditional on the treatment (models 1–3) and pooled across all treatments (model 4). Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.005$.

	(1) *GP-FIXED*	(2) *GP-ALIGNED*	(3) *GP-ALGO*	(4) *Pooled*
Experimental Measures:				
Decision Making Quality Index	−0.044	−0.006	−0.078*	−0.041*
	(0.028)	(0.026)	(0.033)	(0.016)
Financial Literacy Score (Std.)	0.027	−0.004	0.003	0.003
	(0.055)	(0.062)	(0.058)	(0.034)
Numeracy Score (Std.)	−0.038	−0.030	−0.028	−0.064
	(0.062)	(0.075)	(0.089)	(0.044)
Overestimation (Std.)	−0.039	−0.017	−0.047	−0.033
	(0.035)	(0.035)	(0.038)	(0.021)
Overplacement (Std.)	0.003	−0.022	−0.015	−0.023
	(0.039)	(0.042)	(0.052)	(0.026)
Self-Reported Measures:				
Risk Tolerance (Std.)	0.019	0.011	0.001	0.011
	(0.029)	(0.036)	(0.036)	(0.020)
Blame Shifting (Std.)	0.057*	0.026	0.053	0.045*
	(0.028)	(0.030)	(0.033)	(0.018)
Trust in Agent (Std.)	0.068*	0.121**	0.174**	0.123**
	(0.031)	(0.033)	(0.036)	(0.019)
Controls	*yes*	*yes*	*yes*	*yes*
Wald χ^2	16.238	18.535	42.016	54.050
$p > \chi^2$	0.236	0.138	0.000	0.000
Pseudo R^2	0.141	0.101	0.218	0.112
Observations	183	185	182	550

Notes: All self-reported measures are standardized scores. "Trust in Agent" refers to a combined variable of trust in finance professionals and financial algorithms, conditional on the treatment. "Blame Shifting" refers to the mean of two standardized survey items on shifting blame on others and resisting the temptation to shift blame on others. "Controls" include gender (binary indicator for female), age (in years), net income from major employment in 2017 (in thousand SEK's), and maximum education level (dichotomous indicators for high school education or less, university education smaller or equal to three years, and university education larger than three years).

To examine whether the effects systematically differ between treatments, we conduct Wald tests on each covariate after a seemingly unrelated regression with robust standard errors in pairwise comparisons of models. Notably, none of the differences are statistically significant with a p-value smaller than the 0.05 threshold.

such that even risk averse individuals—provided that they are sufficiently trusting—might engage in risky investment decisions through delegation.

Survey Evidence and Discussion. We examine the robustness of the above described results and provide tentative evidence for their external validity using survey responses on real-life delegation decisions instead of the experimental measures. All Likert-scale measures used in the post-experimental survey are described in Table D1 in Appendix D. In the following, we focus on the two self-reported measures of delegation of investment decisions to professionals and investment algorithms in the general population

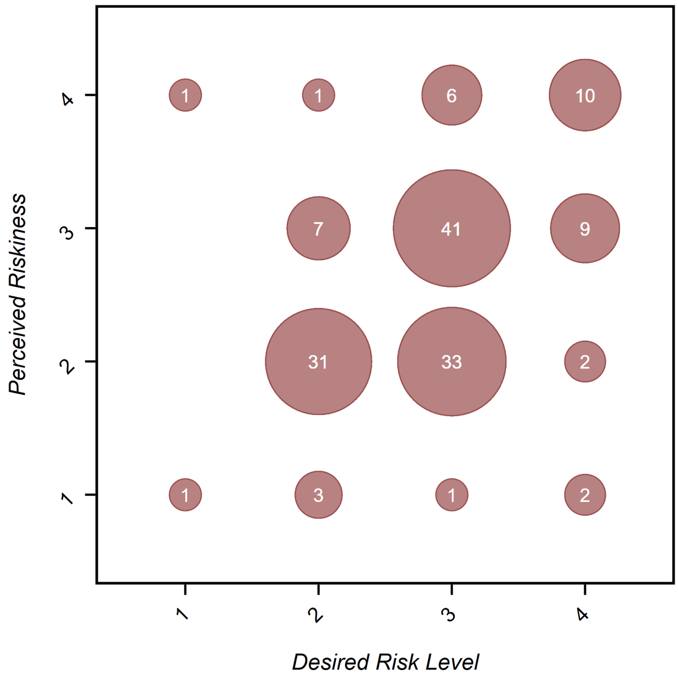

Figure 3: Risk Communication. The figure shows principals' perceived riskiness of their own decisions vs. principals' desired risk levels when delegating their investment decisions to the agent ($n = 148$).

sample.[17]

Panel (a) of Figure 4 shows the mean responses to the two survey items, emphasizing that the general population sample indicates to delegate more to finance professionals ($m = 3.360$, $sd = 0.154$) as compared to investment algorithms ($m = 1.711$, $sd = 0.118$). The difference in means is statistically significant (paired t-test: $t(404) = 10.494$, $p < 0.001$; $n = 405$). Apparently, this result is in contrast to the results of the main experiment, where participants from the general population delegate their decisions significantly more often to the investment algorithm (37.9% in treatment *GP-ALGO*) than to finance professionals (16.9% and 25.9% in the treatments *GP-FIXED* and *GP-ALIGNED*, respectively). There are several potential reasons for this difference in revealed algorithm appreciation between the experiment and the survey responses. First, probably few people have been offered to invest using algorithms in real life, as these products are still not too common and usually not strongly promoted by banks.[18] Second, as put forward by our findings and previous contributions to the literature (Gennaioli et al., 2015; Bucciol et al., 2019), trust is an important determinant for delegation decisions, and some participants might put more trust in algorithms constructed by researchers than those designed by financial institutions. Trusting an investment algorithm constructed by a financial intermediary likely calls for an additional "trust premium" to compensate for the frictions (e.g., uncertainty, asymmetric information, technological illiteracy, etc.) that evolve above and beyond trusting a finance professional hired by a financial institution. Third, in the experimental in-

[17] Note that the two survey questions on delegation were only answered by those participants who did *not* answer the question on frequent Investments with 0 (i.e., "does not describe me at all"). Since 145 participants of the general population sample indicated not to invest frequently at all, the sample size in all analysis presented in this section is $n = 405$ instead of $n = 550$.

[18] As argued, for instance, by Maucijauskaitė (2018), over the course of the last decades the market for robo advice has been on a rapid rise in Sweden—and the European Union in general—due to considerable technological advances. Yet, the actual use of algorithmic advisory services has been limited so far due to comparably slower behavioral adaption of investors.

structions, the algorithm was described to be “programmed in such a way that it maximizes your expected profit conditional on the risk level you indicate [..].” The term “maximizing profit” in the realms of an abstract experimental decision setting could trigger delegation, whereas in real life such a clear promise is beyond reach.[19]

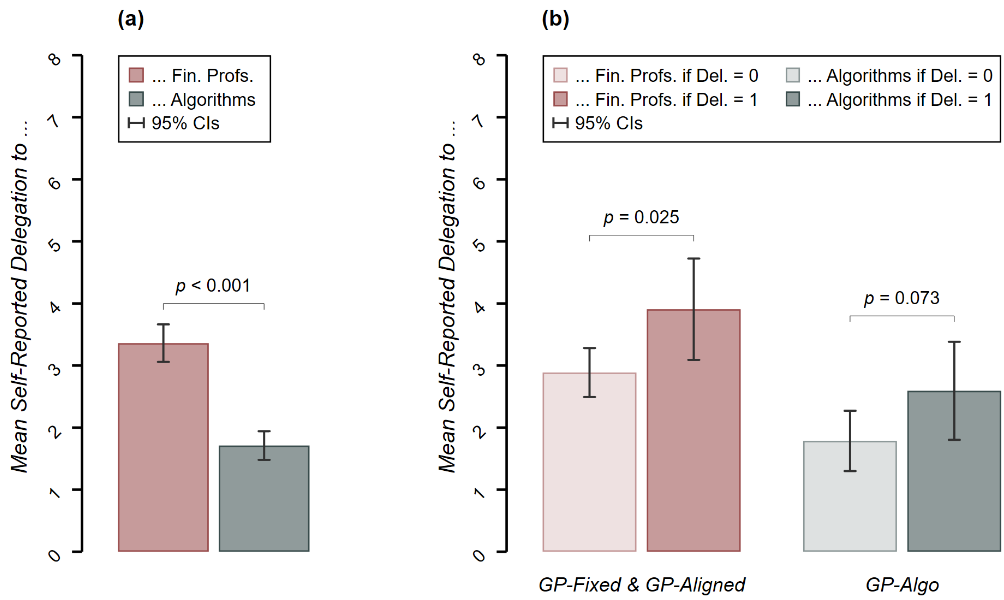

Figure 4: Self-reported propensity to delegate decisions to finance professionals and investment algorithms. Panel (a) Mean levels of self-reported real-life delegation decisions to finance professionals and investment algorithms ($n = 405$). The p-value is based on a paired t-test. **Panel (b)** Mean levels of self-reported real-life delegation decisions to finance professionals conditional on the decision whether (“Del. = 1”) or not (“Del. = 0”) to delegate the allocation decision in the experiment to a finance professionals in treatments *GP-FIXED* and *GP-ALIGNED* ($n = 267$), and mean self-reported levels of real-life delegation decisions to investment algorithms conditional on the decision whether (“Del. = 1”) or not (“Del. = 0”) to delegate the allocation decision in the experiment to the algorithm in treatment *GP-ALGO* ($n = 138$). The p-values are based on two-sample t-tests.

Panel (b) of Figure 4, however, demonstrates that the delegation decisions in the experiment correlate with self-reported real-life delegation behavior from the survey. In particular, it illustrates the mean survey responses to the two questionnaire items regarding self-reported real-life delegation behavior conditional on the delegation decision in the experiment (“Del. = 0” and “Del. = 1,” respectively), separated by the type of agent participants could delegate to in the experiment (i.e., finance professionals in the treatments *GP-FIXED* and *GP-ALIGNED* and an investment algorithm in treatment *GP-ALGO*). Regarding delegating investment decisions to finance professionals, we observe that the mean survey response of participants choosing to delegate in the experiment ($m = 3.907$, $sd = 0.417$) is significantly higher than the mean response of participants choosing not to delegate ($m = 2.887$, $sd = 0.201$) in the experiment (two-sample t-test: $t(265) = 2.261, p = 0.025; n = 267$). This difference corresponds to a point-biserial correlation coefficient of $r = 0.138$ between the survey response on delegation to professionals and the observed delegation decision in the experimental treatments *GP-FIXED* and *GP-ALIGNED*. With respect to delegation to investment algorithms, we observe a similar pattern: participants in the *GP-ALGO* treatment who choose to delegate

[19] For the sake of completeness, we also examine whether the difference between delegation choices in the experiment and the responses to the two survey questions differs between the three treatment conditions. Results are discussed in Appendix I.

to the algorithm in the experiment tend to report to delegate more to algorithms in real-life ($m = 2.593$, $sd = 0.403$) than participants who choose not to delegate ($m = 1.786$, $sd = 0.248$) in the experiment (two-sample t-test: $t(136) = 1.805$, $p = 0.073$; $n = 138$). While this comparison is not statistically significant—potentially driven by the relatively small sample size of $n = 138$—, the corresponding point-biserial correlation of $r = 0.153$ is comparable to the one on delegation to finance professionals. To sum up, we find that participants reporting to delegate real-life investments to finance professionals (investment algorithms) are more likely to delegate to finance professionals (investment algorithms) in our incentivized experimental task. We consider this as suggestive evidence that the delegation decisions in the experiment involve externally valid patterns.

Finally, we examine individual determinants of self-reported delegation in Table 5. Models (1) and (2) investigate the impact of the set of covariates used in Table 4 on participants' self-reported real-life delegation to finance professionals and investment algorithms, respectively. Just as for the experimental delegation decision, trust turns out to play a key role—in terms of statistical significance as well as effect size—, for delegating investment decisions to both finance professionals and algorithms. With respect to blame shifting motives, we, again, find qualitatively similar results as for the analysis of the experimental data. In particular, we observe that participants that are more prone to shift blame on others tend to be more likely to delegate investment decisions to finance professionals (yet, the effect is not statistically significant; $p = 0.065$). As opposed to the effect on delegation decisions observed in the experimental task, we find that both overestimation and risk tolerance significantly decrease participants' self-reported propensity to delegate investments to finance professionals. The negative association of the likelihood of delegation and participants' risk-tolerance (in light of the positive effect of trust) integrates well with the argument of Bucciol et al. (2019) that trust (at least partly) compensates for risk aversion: while individuals who are willing to bear risk tend to delegate less (and rather make investment decisions on their own), those who shy away from risk appear to be more likely to delegate their investments.

Turning to determinants of real-life delegation decisions to investment algorithms, we find a significantly negative effect of our measure of decision-making quality (*DMQI*), supporting the result identified in the experimental data.[20] As opposed to delegation to finance professionals, delegation decisions to investment algorithms turn out not to be significantly associated with participants' overestimation and risk tolerance. The latter could be interpreted as suggestive evidence that trust only serves as a substitute for risk tolerance in the context of interpersonal relationships (of clients and human agents); in delegation decisions to algorithms, however, trust and risk tolerance may rather be complements Bucciol et al., 2019, since investors might still feel like investing on their own behalf, even when through an artificial intermediary.

While models (1) and (2) take a cross-sectional perspective on self-reported delegation decisions in real-life, model (3) in Table 5 employs a "within-subject" perspective by utilizing the fact that all participants answered both delegation questions. We use the difference between responses to the question on delegation to finance professionals and to the question on delegation to algorithms as an individual-level measure of

[20] Since *DMQI* is determined based on participants' choices in the experimental allocation task, the interpretation of the impact of *DMQI* on the answers in the survey questions on delegation is not straightforward. As *DMQI* is constructed as a compound measure of rationality violations in an artificial portfolio context, it might be considered a proxy for financial sophistication. If so, the negative effect of *DMQI* on participants' self-reports, could be interpreted as suggestive that more financially sophisticated participants are less likely to delegate their investments to algorithms. The interpretation of *DMQI* being a proxy of financial sophistication is supported by significantly positive Pearson correlations between *DMQI* and numeracy ($\rho = 0.137$, $p = 0.006$; $n = 405$) and between *DMQI* and financial literacy ($\rho = 0.159$, $p = 0.001$; $n = 405$).

Table 5: Determinants of self-reported delegation decisions. This table reports estimates from ordinary least squares regressions of the self-reported survey responses whether investment decisions are delegated to (1) finance professionals and (2) investment algorithms on a set of experimental and self-reported measures pooled across all treatments. Model (3) uses the difference between survey responses to the question on delegation to finance professionals and the question on delegation to investment algorithms as the dependent variable, adding a within-subject perspective on how much more likely they are to delegate to finance professionals as compared to algorithms in real-life. Robust standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.005$.

	(1) Del. to Prof.	(2) Del. to Algo.	(3) Prof. − Algo.
Experimental Measures:			
Decision Making Quality Index	-0.166	-0.310**	0.129
	(0.111)	(0.102)	(0.075)
Financial Literacy Score (Std.)	0.058	-0.070	0.189
	(0.264)	(0.212)	(0.254)
Numeracy Score (Std.)	-0.405	-0.130	-0.249
	(0.367)	(0.256)	(0.359)
Overestimation (Std.)	-0.446*	0.108	-0.552**
	(0.175)	(0.139)	(0.180)
Overplacement (Std.)	0.226	-0.025	0.261
	(0.237)	(0.175)	(0.234)
Self-Reported Measures:			
Risk Tolerance (Std.)	-0.499**	-0.081	-0.454**
	(0.164)	(0.122)	(0.160)
Blame Shifting (Std.)	0.266	0.158	0.123
	(0.144)	(0.122)	(0.144)
Trust in Finance Professionals (Std.)	0.955**		1.245**
	(0.155)		(0.172)
Trust in Investment Algorithms (Std.)		0.601**	-0.751**
		(0.137)	(0.177)
Controls	*yes*	*yes*	*yes*
Treatment Fixed Effects	*yes*	*yes*	*yes*
$F(15, 389)$	8.555	3.272	6.592
Prob. $> F$	0.000	0.000	0.000
Adj. R^2	0.160	0.086	0.168
Observations	405	405	405

Notes: All self-reported measures are standardized scores. "Blame Shifting" refers to the mean of two standardized survey items on shifting blame on others and resisting the temptation to shift blame on others. "Controls" include gender (binary indicator for female), age (in years), net income from major employment in 2017 (in thousand SEK's), and maximum education level (dichotomous indicators for high school education or less, university education smaller or equal to three years, and university education larger than three years).

how much more likely participants are to delegate to a professional than to an algorithm. Again, we find that trust in the respective agent is a main driver: the more (less) participants trust finance professionals (investment algorithms), the more likely they are to delegate investment decisions to finance professionals. It is worthwhile to emphasize the difference in effect sizes: in absolute terms, the (positive) effect of trust in finance professionals on the relative preference for delegating to finance professionals rather than investment algorithms is about 1.7 times as large as the (negative) impact of trust in algorithms. Finally, the difference between delegation frequencies to professionals and algorithms is negatively correlated with overestimation and risk tolerance. Apart from the results on trust, we find that the likelihood to delegate investments to finance professionals rather than to investment algorithms significantly decreases with participants' overestimation and risk tolerance.

Overall, the validity exercise using the self-reported survey responses on real-life delegation decisions to finance professionals and investment algorithms appears to support our main findings based on the incentivized experimental task to a certain extent. In particular, the survey evidence further emphasizes the key role of trust in the agent for delegation decisions.

4. Conclusion

In this paper, we reported from a lab-in-the field (online) experiment with finance professionals serving as money managers and participants from the general population acting as clients. We examined in a controlled experimental environment whether various motivations and individual characteristics discussed in the literature explain clients' propensity to delegate investment decisions to money managers. We showed that clients are most likely to delegate to an investment algorithm, followed by professionals with aligned incentives and professionals compensated with a fixed payment. However, for clients who chose to delegate their investment, we did not find evidence for systematic variation in their willingness to pay for delegation to different types of agents.

We also reported that the variation in clients' decision whether to delegated to the agent can be partly explained by individual-level characteristics. Pooled across conditions, principals' propensity to delegate increased with trust in the agent, no matter whether to a human agent or an investment algorithm. Moreover, we reported that blame shifting motives increased and own decision-making quality decreased delegation propensity in the full sample. Focusing our attention on the effect of individual characteristics per treatments, blame shifting motives appeared to be particularly important for clients' delegation propensity to finance professionals with a fixed payment, whereas client's own decision-making quality appeared to primarily affect the delegation propensity to investment algorithm. Finally, we observed that principals tend to ask the agent to take more risk as compared to their risk perception in their own decisions.

Since we chose to study our research questions in a controlled experimental setting, our findings are subject to several limitations. For this reason, we are careful with generalizing our results. Our experimental investment task is an abstraction from real-world investment choices and, thus, differs in several aspects: (i) At first sight, there is no option *not* to invest the endowment. However, the investment task is designed such that participants could perfectly hedge against all risk, resulting in risk-free allocations. We acknowledge that this implicit outside option might not to be obvious for clients in our setting, which could render delegation to the agent more attractive *per se.* (ii) Our (experimental) model of the decision environment disregards and (deliberately) bypasses potentially relevant aspects of real-life investment decisions. Likewise, potentially relevant factors for delegation, such as, e.g., time constraints, ambiguity aversion, loss-bearing capacities, or inertia, are explicitly ruled out by our design. While the downsides of selection and abstraction apply to basically all experiments and theoretical models in behavioral economics and finance alike, our experimental investigation has several benefits: An abstract investment task with key features of real-world investment situations (e.g., diversification potential, trade-off between risks and returns) offers the upside of detaching the decision in the experiment from a real-world context, including participants' knowledge about investment products, their beliefs about future developments of markets, and reputation concerns. Moreover, with our design, we can elicit empirically unobservable variables such as the preference for the type of money manager, financial literacy, trust in (human or artificial) agents, and clients propensity to shift blame on others. Hence, we do not have to rely on proxies, which is a common limitation of observational studies. (iii) We also abstract from introducing tournament components or social comparison—features that finance professionals have been shown to care about (Kirchler et al., 2020, 2018). Furthermore, in our study, we deliberately restrict our attention to aligned vs. non-aligned incentives as a first step to introduce information about agents' incentives for clients' delegation decisions as a signal of potential conflicts of interest. We leave the introduction of demand- or supply-side competition and variations and extensions related to the extent of conflicting interests for future research.

Despite these limitations, our study has implications for real-world delegation decisions: Delegating financial decisions is the primary route to financial market participation for many individual investors (see Shum and Faig, 2006, for further details). In contrast to theoretical predictions, households' financial market participation is far from universal (Campbell, 2006). Thus, if it is a policy maker's goal to promote financial market participation, our results highlight the importance of establishing trust in the finance industry in general and in money managers—including investment algorithms—in particular (Georgarakos and Pasini, 2011). Beyond clients' decision-making quality and their propensity to shift blame on others, trust appears to be a consistent and major motive for delegating financial decisions. However, it is not perfectly clear whether clients would actually be better off trusting money managers more: conflicts of interest between clients and money managers (Inderst and Ottaviani, 2012a,b), challenges in risk communication, as well as biased and subpar investments by agents (e.g., Kling et al., 2019; Stefan et al., 2021) could constrain the benefits of delegated decision-making in the context of financial investments. Thus, the implications of delegated financial decisions are an important area for future research to enrich policy makers' view on the pros and cons of increasing the fraction of delegated investment decisions taken by professional money managers.

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Appendices

for Online Publication

Delegation Decisions in Finance

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A. Data Collection and Recruitment

Experimental software. The experimental software—computerized in *oTree* (Chen et al., 2016)—including all instructions, treatment variations, as well as the Swedish/English translations has been pre-registered at https://osf.io/ubpr3/. Demo versions of the experiment and all treatments (in English) are available via http://hea-2019-01-en.herokuapp.com. The source code of the experimental software is available at https://osf.io/tfeh5/. Figure A1 shows two screenshots of the main experimental task, i.e., the allocation decision task, with two and five assets, respectively.

Data availability. All raw data generated in the online experiments is available at https://osf.io/quxmd/. Moreover, the OSF repository contains all script files used to generate the results presented in the paper and the appendices, together with the processed data files, the figures, and tables. Please note that the register data obtained from *Statistiska centralbyrån* (Statistics Sweden; *SCB*) may not be publicly shared.

Recruitment. *Statistiska centralbyrån* (Statistics Sweden; *SCB*) sent out hard copy invitations to participate in the anonymous online experiment. The receivers of the invitations logged in to our experiment using a personalized participant code, which was linked to a key only known to *SCB*. The participant code indicated whether a particular participant was recruited from the finance professional pool or the general population pool. After the data collection has been completed, we sent the identifiers of those participants who completed the experiment to *SCB*, who used their keys to match the experimental data with the requested register data (which is described in detail below). Participants were informed that the data gathered in the experiment is matched with their register data in the invitation letters and on the first screen of the experiment.

Payments. To ensure full privacy of the data collected during the experiment, payouts were handled by the third party survey firm *Enkätfabriken*. Once participants completed the online experiment, they were redirected to a dedicated form on the website of *Enkätfabriken*. Participants used the same participant code as in the experiment. For payment purposes, *Enkätfabriken* collected participants' names, email addresses, "personnummer" (personal identity number), and bank account details. The information collected was handled only by *Enkätfabriken* and has been used exclusively for sake of ordering the bank remittances.

Registry data. In addition to the data collected in the online experiment, we obtained the following register data from *SCB* for each participant who completed all tasks in the experiment:

- *Demographics*: year born, age, gender, county, municipality, and assembly of residence, marital status, year in marital status, family status, birth country, children living at home age 0–3, 4–6, 7–10, 11–15, 16–17, ≥ 18, highest finished education level, education orientation, education group, education county, graduation year, primary source of income, work place municipality and county, work place industry 1990–1992, 1993–2001, 2002–2010, and 2007–2014, occupation 2002–2013 and 2014, net income of own business 1991–2003, 2003–2014, and 2004–2014, capital income, disposable income 1990–2004 and 2004–2014, disposable income of family 1990–2004 and 2004–2014, country of birth, date of immigration.
- *Firm / workplace*: number of employees at firm / workplace, number of men / women at firm / workplace, number of men / women with short / long education at firm / workplace, total salaries paid by firm / workplace.

Figure A1: Screenshots of the allocation decision task. The figure shows screenshots of the main experimental task as displayed to participants. Note that the information in the top right corner ("Your clients' risk level") was only displayed to finance professionals in the treatments *FP-FIXED* and *FP-ALIGNED*. By clicking on the question mark icon in the top left corner, participants had the opportunity to reread the instructions at any time. The button to proceed to the next decision was only shown if investments to the available assets summed up to 100.

- *Education*: high school, high school program, high school grades point average, high school graduation year, university, university program, university major, university graduation year.
- *Assets*: net wealth, total debt, bank account, listed equity, fixed income funds, other funds, bonds and other securities, taxable insurances, houses, apartments, holiday homes.
- *Military records*: command suitability, non cognitive abilities score, muscle strength, physical capacity for work, length, weight, cognitive scores 1 and 2 in language and logic, one in spatial understanding, and one in technical understanding.
- *Parents*: adoptive / biological mother / father, occupation mother / father, primary income source mother / father, net income from own business mother / father, net wealth mother / father.

We only use a part of the available registry data as control variables in our analyses of observed behavior, in particular, participants' gender (binary indicator for female), age (in years), net income from major employment in 2017 (in thousand SEK's), and maximum education level (dichotomous indicators for high school education or less, university education smaller or equal to three years, and university education larger than three years). The restricted use of the register data has been pre-registered at the outset (see https://osf.io/ubpr3/ for the pre-registration).

After the experiment reported in this paper, participants were invited to a second, independent experiment for which the obtained registry data plays a more pivotal role. For details about the second experiment, please refer to the respective pre-registration at https://osf.io/6rdp8/ and the Working Paper (Holmen et al., n.d.).

B. Allocation Decision Task

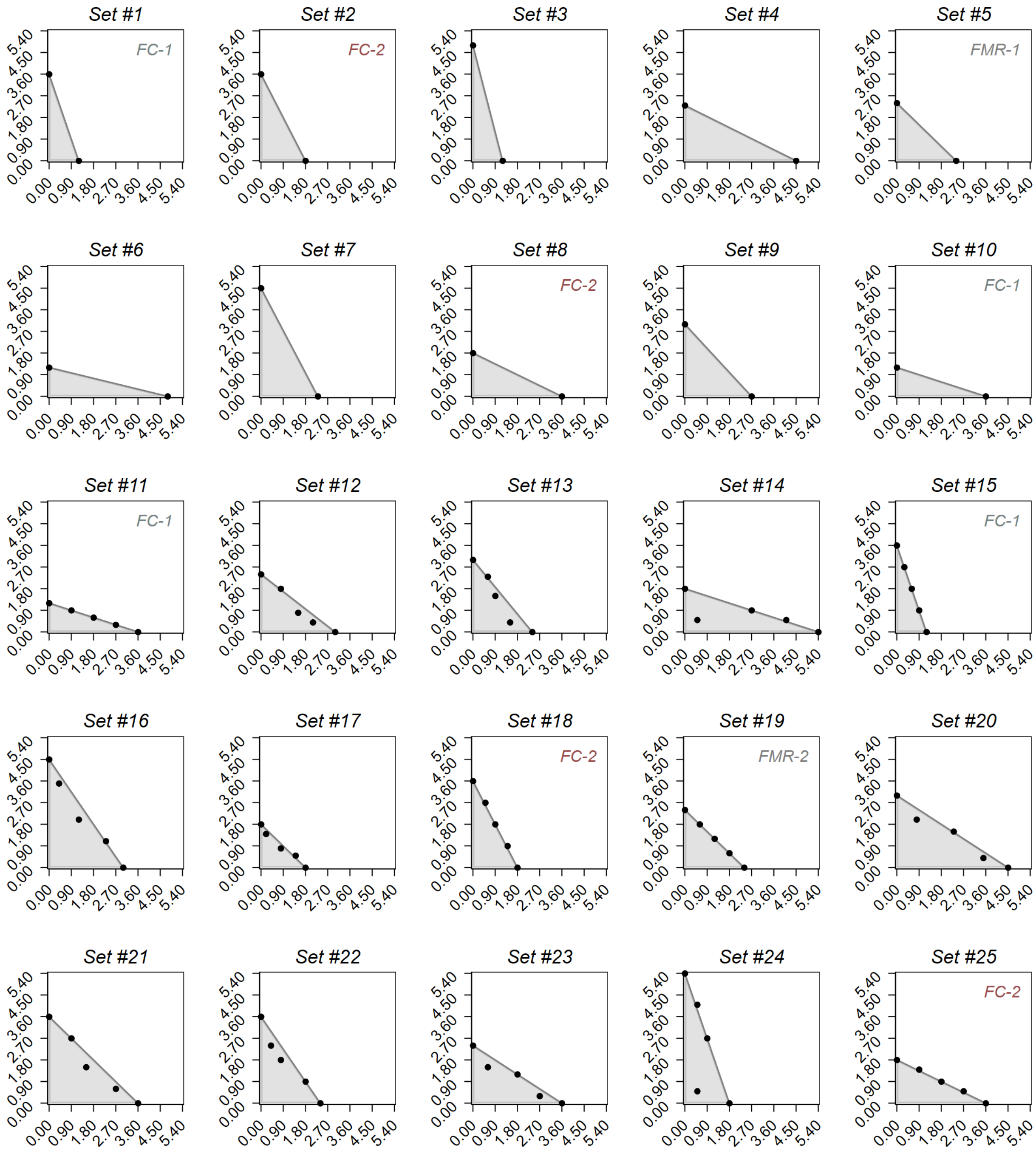

Figure B1: Opportunity sets in the allocation decision task. In each panel of this figure, the vertical (horizontal) axis indicates the return per 1 SEK invested if the coin shows up heads (tails). Each dot indicates a single asset. The labels *FC-1*, *FC-2*, *FMR-1*, and *FMR-2* denote particular opportunity sets used for constructing the decision-making quality measures "financial competence" (*FC*) and "failure to minimize risk" (*FMR*).

C. Decision-Making Quality Measures

In each opportunity set $j \in \{1, 2, \ldots, 25\}$, each participant i is endowed with 100 SEK to allocate on assets $k \in \{1, 2, \ldots, 5\}$. Let $a_{i,j,k}$ denote the fraction of the endowment allocated on asset k such that $\sum_k a_{i,j,k} = 1$.

The return per SEK invested in asset k if the coin comes up heads is denoted as $h_{j,k}$; the return per SEK invested if it comes up tails is denoted as $t_{j,k}$. Thus, the return of participant i's allocation in opportunity set j will either be

$$
\begin{aligned}
H_{i,j} &= \sum_k a_{i,j,k} \cdot h_{j,k} && \text{if the coin comes up heads, or} \\
T_{i,j} &= \sum_k a_{i,j,k} \cdot t_{j,k} && \text{if the coin comes up tails.}
\end{aligned}
$$

Let the tuple $\boldsymbol{x}_{i,j} = (H_{i,j}, T_{i,j})$ denote the portfolio of participant i in opportunity set j. In addition to the measures of expected return and standard deviation, following Banks et al. (2018) we also define four measures of decision-making quality: (i) violations of first order stochastic dominance (*FOSD*), (ii) violations of the generalized axiom of revealed preferences (*GARP*), (iii) financial competence (*FC*), and (iv) failure to minimize risk (*FMR*). Each of these measures is defined in detail below.

Expected Return. The expected portfolio return of participant i's investment in opportunity set j, i.e., the expected return from allocating the endowment on the available assets, is given by

$$ER_{i,j} = \frac{H_{i,j} + T_{i,j}}{2}.$$

Participant i's mean expected return, ER_i, is calculated as the average of $ER_{i,j}$ across 23 of the 25 opportunity sets, as the expected returns are identical for all portfolios in the two remaining opportunity sets (set #5 and #19; see Table 1 and Figure B1), i.e., $ER_i = 1/23 \cdot \sum_{j=1}^{23} ER_{i,j}$.

Standard Deviation. As a measure of portfolio risk, we calculate the standard deviation of participant i's portfolio in opportunity set j, i.e., the standard deviation of $H_{i,j}$ and $T_{i,j}$ occurring with a probability of 50% each:

$$SD_{i,j} = \sqrt{\frac{H_{i,j}^2 + T_{i,j}^2}{2} - \left(\frac{H_{i,j} + T_{i,j}}{2}\right)^2}.$$

The average portfolio risk for individual i, SD_i, is defined as the mean standard deviation across all 25 opportunity sets, i.e., $SD_i = 1/25 \cdot \sum_{j=1}^{25} SD_{i,j}$.

Violations of First Order Stochastic Dominance (*FOSD*). Following Banks et al. (2018), we use the difference between the maximum expected return of a portfolio that provides the same minimum payoff as the chosen portfolio and the expected return of the chosen portfolio as a measure of how closely participant i's choice in opportunity set j complies with the principle of *FOSD* (Hadar and Russell, 1969).

Given a chosen portfolio $\boldsymbol{x}_{i,j} = (H_{i,j}, T_{i,j})$, let $h_j^* = \max_k h_{j,k}$ be the maximum return across all assets k if the coin comes up heads and $t_j^* = \max_k t_{j,k}$ if the the coin comes up tails. By investing the fraction

$$w = \frac{\min(H_{i,j}, T_{i,j})}{\min(h_j^*, t_j^*)}$$

on the asset paying $\min(h_j^*, t_j^*)$ and 0 SEK otherwise, and investing the fraction $(1 - w)$ on the asset paying $\max(h_j^*, t_j^*)$ and 0 SEK otherwise, participant i maximizes the expected return but still guarantees a minimum return of $\min(H_{i,j}, T_{i,j})$. Thus, our measure of *FOSD* is:

$$FOSD_{i,j} = \left(w \cdot \frac{\min(h_j^*, t_j^*)}{2} + (1 - w) \cdot \frac{\max(h_j^*, t_j^*)}{2} \right) - \frac{(H_{i,j} + T_{i,j})}{2}.$$

To assess participant i's average violations of *FOSD*, we average the measure over all choices, except for the two opportunity sets for which any portfolio will yield the same expected returns (set #5 and #19; see Table 1 and Figure B1), i.e., $FOSD_i = 1/23 \cdot \sum_{j=1}^{23} FOSD_{i,j}$.

Violations of the General Axiom of Revealed Preferences (*GARP*). According to the Generalized Axiom of Revealed Preferences, for any two opportunity sets m and n ($m \neq n$), if participant i reveals to prefer $\boldsymbol{x}_{i,m}$ over $\boldsymbol{x}_{i,n}$, then $\boldsymbol{x}_{i,n}$ is not strictly preferred to $\boldsymbol{x}_{i,m}$.

An instance of a *GARP* violation occurs when a participant i chooses $\boldsymbol{x}_{i,m}$ in opportunity set m when $\boldsymbol{x}_{i,n}$ is affordable, and also chooses $\boldsymbol{x}_{i,n}$ in opportunity set n when $\boldsymbol{x}_{i,m}$ is affordable.

Let p_j denote the ratio of maximum returns for heads and tails in opportunity set j, respectively, i.e., $p_j = h_j^*/t_j^*$. The extent of violations of *GARP* is measured with the Money Pump Index (*MPI*), which is based on the idea that an arbitrageur can exploit violations in revealed preferences (Echenique et al., 2011): The arbitrageur could make profit by buying portfolio $\boldsymbol{x}_{i,m}$ at price p_n and then selling it at price p_m; likewise, the arbitrageur could buy portfolio $\boldsymbol{x}_{i,n}$ at price p_m and sell it at price p_n. The Money Pump Index is the total profit the arbitrageur could make, i.e.,

$$MPI_{i,m,n} = \alpha_{i,m,n} + \beta_{i,m,n}$$
$$MPI_{i,m,n} = p_m \cdot (\boldsymbol{x}_{i,m} - \boldsymbol{x}_{i,n}) + p_n \cdot (\boldsymbol{x}_{i,n} - \boldsymbol{x}_{i,m}).$$

We calculate the money pump for each violation of *GARP*, i.e., for $25 \cdot (25 - 1) \cdot 1/2 = 300$ pairwise combinations of opportunity sets. For each participant i, we determine the average money pump index, over all pairwise combinations, i.e., $MPI_i = 1/300 \cdot \sum_{m=1}^{25} \sum_{n=1}^{25} MPI_{i,m,n} \; \forall \; m > n$.

Financial competence (*FC*). Four opportunity sets were presented in both the two-asset- (sets #1, #2, #8, and #10) and the five-asset-frame (sets #11, #15, #18, and #25). In addition, two of these particular four opportunity sets were constructed as mirror images of one another, i.e., only the payoffs for heads and tails were interchanged. Thus, two opportunity sets (denoted as FC_1 and FC_2 in Figure B1) were effectively presented four times each (#1 = #10 = #11 = #15 and #2 = #8 = #18 = #25).

Let $J_1 = \{\#1, \#10, \#11, \#15\}$ and $J_2 = \{\#2, \#8, \#18, \#25\}$. Thus, J_1 and J_2 are sets containing identical opportunity sets presented as mirror images and/or in the two-asset- or the five-asset-frame, respectively. The financial competence of individual i is defined as the average absolute differences between the expected returns across the identical opportunity sets in J_1 and J_2, i.e.,

$$FC_i = \frac{1}{12} \cdot \left(\sum_{k,l \in J_1} |ER_{i,k} - ER_{i,l}| + \sum_{m,n \in J_2} |ER_{i,m} - ER_{i,n}| \right) \quad \forall\, k > l \;\; \& \;\; m > n.$$

Note that our definition of FC_i differs from the measure used by Banks et al. (2018), who average the absolute differences in expected returns across the two frames, but not across the mirrored versions of the sets.

Failure to minimize risk (*FMR*). In two opportunity sets (#5 and #19; see Figure B1), the expected return per 1 SEK invested was the same for all assets k, such that all feasible portfolios will share the same expected return. Choosing a portfolio that is fully hedged and, thus, has zero risk (second-order) dominates all other feasible portfolios in these two opportunity sets. The failure to minimize risk for participant i in opportunity set j, $FMR_{i,j}$, is measured as the standard deviation $SD_{i,j}$ of the particular portfolio allocation, which is then averaged over the two opportunity sets, i.e.,

$$FMR_i = \frac{1}{2} \cdot \sum_{j=1}^{2} SD_{i,j}.$$

Decision-making quality index (*DMQI*). The four measures address different aspects of decision making quality. We use the first principal component of the four measures as a proxy of decision-making quality (*DMQI*) for each participant (please note that this approach differs from the one in Banks et al., 2018). The underlying principal component analysis is summarized in Table C1. By construction, *DMQI* has a mean of zero and a standard deviation of unity. Thus, positive values can be interpreted as above average while negative values indicate that a participants' decision-making quality is below average.

The four measures tend to be correlated, as can be seen in Table C2 reporting Pearson correlation coefficients between *FOSD*, *MPI*, *FC*, and *FMR*, separated for the general population and finance professional sample. The study by Banks et al. (2018), conducted with participants from the general population, reports correlation coefficients of comparable magnitude.

Table C1: Principal component analysis of the four decision-making quality measures. This table outlines the four principal components of the *DMQI*, i.e., (i) violations of first order stochastic dominance (*FOSD*), (ii) money pump index (*MPI*), (iii) financial competence, and (iv) failure to minimize risk (*FMR*); $n = 958$. **Panel (A)** reports the eigenvalue and the proportion of explained variance for each of the four components. **Panel (B)** reports unrotated components.

Panel (A)

Component	*Eigenvalue*	*Difference*	*Proportion*	*Cumulative*
Comp. #1	2.034	1.046	0.509	0.509
Comp. #2	0.989	0.279	0.247	0.756
Comp. #3	0.710	0.444	0.178	0.933
Comp. #4	0.267	.	0.067	1.000

Panel (B)

Component	*Comp. #1*	*Comp. #2*	*Comp. #3*	*Comp. #4*
FOSD	0.621	−0.099	−0.318	0.710
MPI	0.618	0.026	−0.360	−0.698
FC	0.457	−0.228	0.859	−0.048
FMR	0.154	0.968	0.179	0.081

Table C2: Correlations between the decision-making quality measures. This table reports Pearson correlations between the decision-making quality measures (i) violations of first order stochastic dominance (*FOSD*), (ii) money pump index (*MPI*), (iii) financial competence (*FC*), (iv) failure to minimize risk (*FMR*), as well as the unified measure of decision-making quality (*DMQI*), separated for the general population sample (lower triangular matrix; $n = 550$) and the finance professionals sample (upper triangular matrix; $n = 408$). p-values are reported in parentheses.

	FOSD	*MPI*	*FC*	*FMR*	*DMQI*
FOSD		0.776	0.334	−0.084	0.740
		(< 0.001)	(< 0.001)	(0.090)	(< 0.001)
MPI	0.703		0.327	0.075	0.871
	(< 0.001)		(< 0.001)	(0.131)	(< 0.001)
FC	0.418	0.379		−0.131	0.685
	(< 0.001)	(< 0.001)		(0.008)	(< 0.001)
FMR	0.281	0.261	0.240		0.222
	(< 0.001)	(< 0.001)	(< 0.001)		(< 0.001)
DMQI	0.766	0.861	0.762	0.451	
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	

D. Questionnaires and Side Tasks

After the main experiment, participants were asked to answer a set of Likert items—all scaled from 0 (minimum) to 10 (maximum)—which are summarized in Table D1 below. The questions on risk tolerance and patience are based on Dohmen et al. (2011) and Falk et al. (2016, 2018); and the two statements addressing the proneness to shift blame are based on the inventory introduced by Wilson et al. (1990). In addition to the survey items reported in Table D1, participants were exposed to a hypothetical charitable giving setting based on Falk et al. (2018), asking how much they would donate to a good cause if they had unexpectedly received 10,000 SEK.[21] The 14 items were displayed on five separate screens: the first screen contained the questions regarding risk preferences in general and their willingness to give up something today in order to benefit more in the future; the second screen included the item of risk tolerance in financial matters as well as the three statements on trust; the third screen showed the hypothetical charitable giving task; the fourth screen comprised the two questions on blame shifting; and the fifth screen involved the five questions related to financial investments and the use of expertise.

Once the above questionnaires had been completed, participants answered eight questions allowing to determine their numeracy skills. As a measure of participants' numeracy, we used the number of correct answers. The numeracy task is based on the Rasch-validated inventory proposed by Weller et al. (2013). Two of the eight questions in the original set are well-known items from the Cognitive Reflection Test (CRT) introduced by Frederick (2005). Since this three-item test has been widely spread on the Internet, many people likely know the questions and the corresponding answers. Therefore, the two items on cognitive reflection skills have been replaced by items from the CRT proposed by Toplak et al. (2014). For answering the eight questions, participants faced a time constraint of four minutes. Since the items, by construction of the test, differ considerably in difficulty, the order of the questions has been randomized to avoid systematic effects arising from the time constraint. The questions used in the numeracy task are listed in Table D2.

After submitting their answers to the numeracy questions, participants were asked to self-assess their performance in the task in two different ways. The respective questions read as follows: (i) "How many of the eight questions you answered on the previous screen did you answer correctly?" (0 to 8), and (ii) "Compared to a random sample of the Swedish population, how did you score in terms of correct answers? Please estimate your position in the ranking." (Top 10%, Top 20%, …, Bottom 20%, Bottom 10%). While the first question allows for determining participants' overestimation of their own skills (as the difference between their estimates and actual performance), the second question allows for quantifying participants' tendency to "overplace" their performance relative to others. Question (ii) asked participants to evaluate their performance relative to a *random* sample of the Swedish population. However, our sample is not representative with respect to the level of education due to self-selection effects. For this reason we take a detour to derive a sensible measure of overplacement: The validated inventory proposed by Weller et al. (2013) is constructed in such a way that scores are approximately normally distributed among a general population sample. The fact that the numeracy scores in our general population sample are significantly different from a normal distribution (Shapiro-Wilk-Test; $W = 0.987$, $p < 0.001$, $n = 550$) somewhat confirms our conjecture of a self-selection effect in our sample. Thus, in a first step, we draw random integers from a normal distribution with a mean of 4.07 and a standard deviation of 1.83, the first and second moment reported for Study 2 in Weller et al. (2013), validating their Rasch-based measure. In a second step, we determine the percentiles associated with each possible score between 0 and 8. Finally, we deduct the percentile (from the sampled normal distribution) corresponding to participants' numeracy

[21] The question was presented to participants as follows: "Imagine the following situation: Today you unexpectedly received 10,000 SEK. How much of this amount would you donate to a good cause?"

score from their estimated decile, i.e., their answer to question (ii), to assess the degree of participants' overplacement.[22] As a final task of the experiment, participants were asked to answer six single-choice questions based on van Rooij et al. (2011), allowing to determine their financial literacy. In particular, three of the questions stem from their basic literacy inventory (*Q1–Q3*, corresponding to items (2), (3), and (5) in van Rooij et al. (2011)), and three questions are based on the advanced literacy inventory (*Q4–Q6*, corresponding to items (12), (16), and (7) in van Rooij et al. (2011)). As an index of financial literacy, we use the sum of participants' correct answers. The questions used in the financial literacy task are depicted in Table D3.

Descriptive results relating to the questionnaires are provided in Table G1; summary results of the side experiments on numeracy skills, financial literacy, and the two measures of overconfidence are provided in Table G2 in Appendix G.

[22] As we ask participants to estimate their performance relative to the general population in deciles rather than percentiles, we use the *minimum* difference to either of the bounds of the interval they implicitly provide as our measure of overestimation. That is, if the percentile (from the sampled normal distribution) lies within the interval participants estimate, the measure takes value 0; if the percentile is smaller than the lower bound (upper bound) of the estimated interval, we evaluate the percentile to the lower bound (upper bound) of the interval.

Table D1: Survey questions. This table summarizes the Likert items, all participants answered after the main experimental task. In particular, the table depicts the variable description as referred to in the main text, the wording of the question/statement, and the corresponding labelling of the minimum and maximum values for each item. The three items indicated with an asterisk were only displayed if the question “Frequent Investments” was not answered with 0 (“does not describe me at all”).

		Likert Scale	
Variable	***Question / Statement***	*min (0)*	*max (10)*
Risk Tolerance (in General)	Are you generally a person who is willing to take risks or do you try to avoid taking risks?	not at all willing to take risks	very willing to take risks
Patience (in General)	How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?	not at all willing to give up something today	very willing to give up something today
Risk Tolerance in Financial Matters	I am generally willing to take risks in financial matters.	does not describe me at all	describes me perfectly
Trust (in General)	I generally trust other people.	does not describe me at all	describes me perfectly
Trust in Finance Professionals	I generally trust employees from the finance industry.	does not describe me at all	describes me perfectly
Trust in Investment Algorithms	I generally trust robo-advisors (i.e. computer programs) in financial matters.	does not describe me at all	describes me perfectly
Blame Shifting (Others)	If you hurt yourself accidentally, do you sometimes blame somebody who happens to be nearby even though you realize, on reflection, that they were not responsible?	I never blame others	I often blame others
Blame Shifting (Temptation)	Can you easily resist the temptation to blame others for the accidents that happen to you?	I can resist easily	I cannot resist at all
Frequent Investments	I frequently invest in stocks and mutual funds myself (not through the national pension system).	does not describe me at all	describes me perfectly
*Delegate to Fin. Profs.**	I delegate my investment decisions (e.g., purchase of stocks, bonds, investment funds, real estate) to financial advisors at banks or other institutions and refrain from taking decisions myself.	does not describe me at all	describes me perfectly
*Delegate to Inv. Algos.**	I delegate my investment decisions (e.g., purchase of stocks, bonds, investment funds, real estate) to robo-advisors at banks or other institutions and refrain from taking decisions myself.	does not describe me at all	describes me perfectly
*Use Expertise of Fin. Profs.**	I use the expertise of financial advisers for my investments/pension savings.	does not describe me at all	describes me perfectly
Responsibility in Financial Matters	I am solely responsible for financial decisions in my household.	does not describe me at all	describes me perfectly

Table D2: Numeracy inventory based on Weller et al. (2013). This table summarizes the questions used to assess participants' numeracy and the correct answers to each of the questions. For answering all items, participants were given a maximum of four minutes. The inventory proposed by Weller et al. (2013) includes two questions from Frederick (2005). As these are likely to be known by many people, items *Q2* and *Q3* have been replaced by questions from Toplak et al. (2014).

ID	*Question*	*Correct Answer*
Q1	Suppose you have a close friend who has a lump in her breast and must have a mammogram. Of 100 women like her, 10 of them actually have a malignant tumor and 90 of them do not. Of the 10 women who actually have a tumor, the mammogram indicates correctly that 9 of them have a tumor and indicates incorrectly that 1 of them does not. Of the 90 women who do not have a tumor, the mammogram indicates correctly that 81 of them do not have a tumor and indicates incorrectly that 9 of them do have a tumor. Imagine that your friend tests positive (as if she had a tumor), what is the likelihood that she actually has a tumor?	50 percent
Q2	If John can drink one barrel of water in 6 days, and Mary can drink one barrel of water in 12 days, how long would it take them to drink one barrel of water together?	4 days
Q3	A man buys a pig for 600 SEK, sells it for 700 SEK, buys it back for 800 SEK, and sells it finally for 900 SEK. How much has he made?	200 SEK
Q4	In a lottery, the chance of winning a car is 1 in 1000. What percent of lottery tickets win a car?	0.1 percent
Q5	In a lottery, the chances of winning a 10.000 SEK prize are 1%. What is your best guess about how many people would win a 10.000 SEK prize if 1000 people each buy a single lottery ticket?	10 people
Q6	Imagine that we roll a fair, six-sided die 1000 times. Out of 1000 rolls, how many times do you think the die would come up as an even number?	500 times
Q7	If the chance of getting a disease is 20 out of 100, this would be the same as having a ... chance of getting the disease.	20 percent
Q8	If the chance of getting a disease is 10%, how many people would be expected to get the disease out of 1000?	100 people

Table D3: Financial literacy inventory based on van Rooij et al. (2011). This table summarizes the questions used to assess participants' literacy in financial matters and the corresponding choice options to each of the questions. Correct answers are highlighted in *italics*. For answering all items, participants were given a maximum of three minutes.

ID	*Question*	*Choices*
Q1	Suppose you had 1,000 SEK in a savings account and the interest rate is 20% per year and you never withdraw money or interest payments. After 5 years, how much would you have on this account in total?	○ *more than 2,000 SEK* ○ exactly 2,000 SEK ○ less than 2,000 SEK ○ do not know
Q2	Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?	○ more than today ○ exactly the same ○ *less than today* ○ do not know
Q3	Suppose that in the year 2025, your income after tax has doubled and prices of all goods have doubled too. In 2025, how much will you be able to buy with your income?	○ more than today ○ *exactly the same* ○ less than today ○ do not know
Q4	When an investor spreads his money among different assets, does the risk of losing money in general:	○ increase ○ *decrease* ○ stay the same ○ do not know
Q5	If the interest rate falls, what should happen to bond prices?	○ *rise* ○ fall ○ stay the same ○ none of the above ○ do not know
Q6	Which of the following statements is correct? If somebody buys the stock of firm B in the stock market:	○ *he owns a part of firm B* ○ he has lent money to firm B ○ he is liable for the firm B's debt ○ none of the above ○ do not know

E. Response Rate Analysis

A detailed summary of participants demographics compared to the characteristics of the sample invited is presented in Table E1. In particular, Table E1 reports the number of respondents and non-respondents per category of several socio-demographic characteristics, separated for both samples, as reported by *SCB*. Moreover, we report χ^2-tests comparing whether participants in our samples differ significantly from those who have been invited by *SCB* but did not participate in the experiment. We report self-selection effects in terms of gender, age, country of birth, income, and education for the general population sample, and self-selection effects with respect to gender, age, and education for the finance professionals sample.

For the finance professionals group, an analysis of response rates shows that men responded to a greater extent than women, and that finance professionals in the age group 45–59 years responded to a slightly lesser extent than other ages. Furthermore, the non-response analysis shows that those with the lowest income responded to a somewhat higher extent as compared to the others, and that those with a post-secondary education level of three years or more responded to greater extent than others. In the case of country of birth, the response rate was slightly higher for those born in Sweden compared to other countries. In the finance group, the response frequency was slightly lower (5%) in the group of traders and portfolio managers (job code “2414”) compared with analysts and advisers (code “2413”) and brokers (code“3311”) (6.4%).

For the general population group, the response rate analysis shows similar patterns regarding gender, i.e., men responded to a greater extent than women. The response rate was lowest among the elderly. Furthermore, the response rate analysis indicates that those with the lowest and highest income responded to a somewhat higher degree compared to other income groups. When it comes to the level of education, those with a post-secondary education of three years or more tend to be over-represented in our sample. In the case of country of birth, the response rate was slightly higher for the ones born in Sweden compared to other countries.

Table E1: Sample characteristics by subject pools. This table depicts the number (in %) of respondents (*"Resp."*), i.e., those who participated in our experiment, and non-respondents (*"No Resp."*), i.e., those who were invited but did not participate, for a number of different characteristics, separated for the general population and the finance profession sample. χ^2-tests (with k-1 degrees of freedom) and the corresponding p-values are reported.

	General Population			*Finance Professionals*		
	Resp.	*No Resp.*	χ^2 / p	*Resp.*	*No Resp.*	χ^2 / p
Gender:						
Male	55.35	49.36	9.322	75.30	68.47	10.169
Female	44.65	50.64	(0.002)	24.70	31.53	(0.001)
Age:						
20 – 29 years	11.55	10.28	37.789	11.85	8.73	14.062
30 – 39 years	31.69	23.18	(< 0.001)	31.12	28.79	(0.015)
40 – 49 years	26.62	26.39		28.51	30.04	
50 – 59 years	20.99	26.74		17.27	22.83	
60 – 69 years	9.15	13.41		10.04	8.60	
70 – 79 years	0.00	0.00		1.20	1.00	
Country of Birth:						
Sweden	88.17	82.84	13.248	89.76	88.95	0.311
Abroad	11.83	17.16	(< 0.001)	10.24	11.05	(0.577)
Citizenship:						
Swedish	97.04	95.64	3.132	97.59	96.53	1.604
Foreign	2.96	4.36	(0.077)	2.41	3.47	(0.205)
Marital Status:						
Married	46.90	46.26	2.247	52.21	56.31	4.910
Unmarried	41.41	40.49	(0.523)	40.36	35.46	(0.179)
Divorced	11.27	12.42		7.03	7.79	
Widowed	0.42	0.83		0.40	0.45	
Income:						
< 124,999 SEK	3.24	2.70	25.646	2.01	1.53	2.985
125,000 – 199,999 SEK	5.63	5.79	(< 0.001)	2.41	2.16	(0.560)
200,000 – 279,999 SEK	12.82	15.25		3.01	3.41	
280,000 – 369,999 SEK	24.08	31.16		5.22	6.85	
> 370,000 SEK	54.23	45.11		87.35	86.06	
Education:						
No High School	1.83	8.89	198.587	0.80	1.08	32.058
High School	28.45	46.89	(< 0.001)	7.83	17.06	(< 0.001)
University (< 3 years)	19.86	14.95		11.45	11.32	
University (> 3 years)	49.86	28.61		79.72	69.95	
Unknown, n/a	0.00	0.66		0.20	0.59	

F. Analyses of Time Spent

In the following, we examine the time spent per experimental task in the online experiment. Throughout the analysis, we truncate the time spent per task at the 99% percentile to avoid that outliers distort the results. In particular, for each task, durations exceeding this threshold are replaced by the value of the 99% percentile. Descriptive statistics of the time spent per task, separated for the general population and the finance professionals subject pools, are presented in Table F1. On average, the times spent in the experimental tasks appear to be sufficiently long to be confident that participants in both samples took the experiment very seriously, which is also confirmed by the high levels of decision-making quality (see Appendix C for details). Differences in the time spent between the two pools are reported in Table F2.

With respect to the main task, we examine learning effects by means of ordinary least squares regressions of the time spent on the 25 decisions on a linear time trend (with standard errors clustered at the participant level). The regressions reveal that the time spent per decision decreases with the progressing round numbers, in the decisions with both two and five assets, respectively. For the first two-asset item, participants from the general population take, on average, 57.1 seconds; for the subsequent decisions, the time spent, on average, decreases by 5.1 seconds per item ($t(548) = 13.916$, $p < 0.001$, $n = 5{,}500$). Finance professionals take, on average, 72.7 seconds for the first two-asset decision; for the following nine decisions with two assets, the time spent, on average, decreases by 6.5 seconds per item ($t(406) = 8.776$, $p < 0.001$, $n = 6{,}120$). Likewise, learning is observed for consecutive investment decisions with five assets. For the first five-asset item, participants from the general population take, on average, 3.5 minutes; for the subsequent decisions, the time spent, on average, decreases by 13.1 seconds per item ($t(548) = 2.065$, $p = 0.039$, $n = 5{,}500$). Finance professionals take, on average, 2.6 minutes for the first five-asset decision; for the following fourteen decisions with five assets, the time spent, on average, decreases by 7.7 seconds per item ($t(406) = 2.844$, $p = 0.005$, $n = 6{,}120$).

In addition, we investigate whether decision-making quality is systematically affected by time participants take to decide on the 25 investment decisions. Notably, ordinary least squares regression of *DMQI* on the time spent on the investment task (i.e., the sum of the time spent in the investment task with two and five assets) reveal that participants' proneness to poor investment decisions is not significantly driven by the time they spend on each decision, neither in the general population sample ($\beta = 0.005$, $t(548) = 1.314$, $p = 0.189$, $n = 550$), nor in the finance professionals sample ($\beta = 0.003$, $t(406) = 1.261$, $p = 0.208$, $n = 408$).

Table F1: Descriptive statistics of time spent per task. This table reports the means and standard deviations (in parentheses) as well as the median and interquartile ranges (*IQR*; in brackets) for the time spent per experimental task (measured in minutes), separated for the general population sample (all treatments) as well as the three treatments conducted among finance professionals.

	*GP-**		*FP-FIXED*		*FP-ALIGNED*		*FP-OWN*	
	m / sd	*q50 / iqr*	*m / sd*	*q50 / iqr*	*m / sd*	*q50 / iqr*	*m / sd*	*q50 / iqr*
Investment Task w/ Two Assets	5.53	4.30	7.00	5.13	7.26	5.07	5.16	4.25
	(4.17)	[3.10]	(5.37)	[4.56]	(5.23)	[5.98]	(3.94)	[3.08]
Investment Task w/ Five Assets	15.24	11.99	18.40	13.57	19.11	13.73	15.65	11.77
	(10.73)	[10.52]	(14.85)	[14.41]	(15.44)	[14.02]	(12.07)	[10.30]
Questionnaires (Self-Reported)	2.67	2.30	2.49	2.18	2.55	2.20	2.52	2.15
	(1.54)	[1.20]	(1.29)	[1.01]	(1.53)	[1.08]	(1.16)	[1.27]
Numeracy Inventory (8 Items)	3.65	4.00	3.68	4.00	3.58	4.00	3.60	4.00
	(0.55)	[0.62]	(0.57)	[0.58]	(0.64)	[0.92]	(0.62)	[0.77]
Financial Literacy Test (6 Items)	2.05	1.99	1.74	1.63	1.76	1.63	1.77	1.65
	(0.56)	[0.85]	(0.59)	[0.77]	(0.52)	[0.72]	(0.60)	[0.90]
Observations	550		132		139		137	

Table F2: Differences in time spent. This table reports the t-statistics from two-sample t-tests between the general population sample (pooled across all treatments) and the finance professionals sample separated for the treatment conditions for the time spent per experimental task (measured in minutes). Standard errors (se) are reported in parentheses. Means, standard deviations, medians, and interquartile ranges for the time spent per experimental task in all treatments are reported in Table F1. * $p < 0.05$, ** $p < 0.005$.

	*GP-** vs. *FP-FIXED*	*GP-** vs. *FP-ALIGNED*	*GP-** vs. *FP-OWN*	*FP-FIXED* vs. *FP-ALIGNED*	*FP-FIXED* vs. *FP-OWN*	*FP-ALIGNED* vs. *FP-OWN*
	t / se	*t / se*	*t / se*	*t / se*	*t / se*	*t / se*
Investment Task w/ Two Assets	−2.447*	−2.975**	0.988	−0.390	3.218**	3.754**
	(0.558)	(0.544)	(0.483)	(0.644)	(0.573)	(0.558)
Investment Task w/ Five Assets	−1.218	−1.676	0.676	−0.388	1.670	2.076*
	(1.509)	(1.523)	(1.353)	(1.842)	(1.648)	(1.670)
Obs.	315	322	320	271	269	276

G. Descriptive Results

In the following, we present a set of descriptive results for all measures elicited in the experiment. Many of these variables only enter our analyses as controls. Yet, while several results presented below back up our main findings, we also deem it interesting to compare our two subject pools—participants from the general population and professionals from the finance industry—along these measures.

Table G1: Descriptive statistics and comparisons between pools for the survey items. This table reports the means and standard deviations (in parentheses) for all survey items included in the experiment, separated for the general population and the finance professionals subject pool. The column "t-test" reports the differences in means and the t-values (in brackets) from two-sample t-tests based on $n = 958$. * $p < 0.05$, ** $p < 0.005$.

	Gen. Pop.	*Fin. Prof.*	*t-Test*
Altruism/Charitable Giving	0.79	0.85	-0.061
	(1.37)	(1.69)	[0.099]
Blame Shifting - Others	1.12	1.19	-0.065
	(1.56)	(1.59)	[0.103]
Blame Shifting - Temptation	1.55	1.78	-0.239
	(2.11)	(2.20)	[0.140]
Risk Tolerance in General	4.79	5.80	-1.017^{**}
	(2.14)	(1.94)	[0.135]
Risk Tolerance	4.28	6.09	-1.806^{**}
	(2.34)	(2.13)	[0.147]
Patience in General	6.03	7.21	-1.179^{**}
	(2.00)	(1.81)	[0.125]
Trust in General	5.71	5.79	-0.080
	(2.36)	(2.21)	[0.150]
Trust in Finance Professionals	4.16	4.63	-0.478^{**}
	(2.33)	(2.37)	[0.154]
Trust in Investment Algorithms	4.02	4.04	-0.024
	(2.25)	(2.45)	[0.153]
Frequent Investments	3.54	6.69	-3.149^{**}
	(3.31)	(3.25)	[0.215]
Responsibility in Fin. Matters	5.60	6.85	-1.249^{**}
	(3.67)	(3.33)	[0.231]
Use Expertise of Fin. Profs.	3.58	2.21	1.376^{**}
	(3.19)	(2.81)	[0.214]
Delegate to Fin. Profs.	3.36	1.32	2.039^{**}
	(3.11)	(2.16)	[0.192]
Delegate to Inv. Algorithms	1.71	0.85	0.865^{**}
	(2.37)	(1.67)	[0.147]
Observations	550	408	958

Notes: All items, except for "Altruism," were answered on Likert scales ranging from 0 (minimum) to 10 (maximum). The variable "Altruism" refers to the amount transferred (up to 10,000 SEK) in a hypothetical charitable giving setting. For reasons of comparison, the variable is re-scaled to thousands SEK.

Table G2: Descriptive statistics and comparisons between pools for numeracy, financial literacy, and overconfidence. This table reports the means and standard deviations (in parentheses) for participants' numeracy and financial literacy scores, their self-estimates regarding their numeracy scores (in terms of estimates of the score and their relative performance compared to the Swedish general population), and the two measures of overconfidence (overestimation and overplacement), separated for the general population and the finance professional subject pool. The column "t-test" reports the differences in means and the t-values (in brackets) from two-sample t-tests based on $n = 958$. * $p < 0.05$, ** $p < 0.005$.

	Gen. Pop.	*Fin. Prof.*	*t-Test*
Skills:			
Numeracy Score	4.44	5.31	-0.865^{**}
	(1.63)	(1.59)	[0.106]
Financial Literacy Score	4.29	5.39	-1.099^{**}
	(1.20)	(0.94)	[0.071]
Self-Assessment:			
Estimated Numeracy Score	5.34	6.17	-0.825^{**}
	(1.81)	(1.59)	[0.112]
Estimated Decile	0.56	0.68	-0.121^{**}
	(0.20)	(0.18)	[0.013]
Overconfidence:			
Overestimation	0.90	0.86	0.040
	(1.57)	(1.34)	[0.096]
Overplacement	-0.03	-0.03	0.005
	(0.23)	(0.19)	[0.014]

Notes: Overestimation refers to the difference between participants' estimate of their numeracy and their actual numeracy score. Overplacement refers to the (minimum) difference between participants' estimate of the decile, their performance in the numeracy task belongs to, and the percentiles of the numeracy scores evaluated based on a normal distribution (see Appendix D for further details).

H. Self-Reported Trust in Agents

We report from the post-experimental survey responses summarized in Table D1. Figure H1 reports mean levels of self-reported trust in the general population, in finance professionals, and in investment algorithms, respectively, separated for participants from the finance professional and the general population sample. For both subject pools, mean trust is highest in people from the general population. When it comes to human finance professionals and robo advisors, there is a drop in trust—even for the professionals who, on average, trust their peers less than the general population. While we can only speculate about the reasons for this, the results might be an echo of the effect of the financial crisis of 2008 on trust in the financial sector. See Sapienza and Zingales (2012). Still, finance professionals trust their peers significantly more than do other people from the general population. Interestingly, the difference between pools vanishes when it comes to trust in robo advisors.

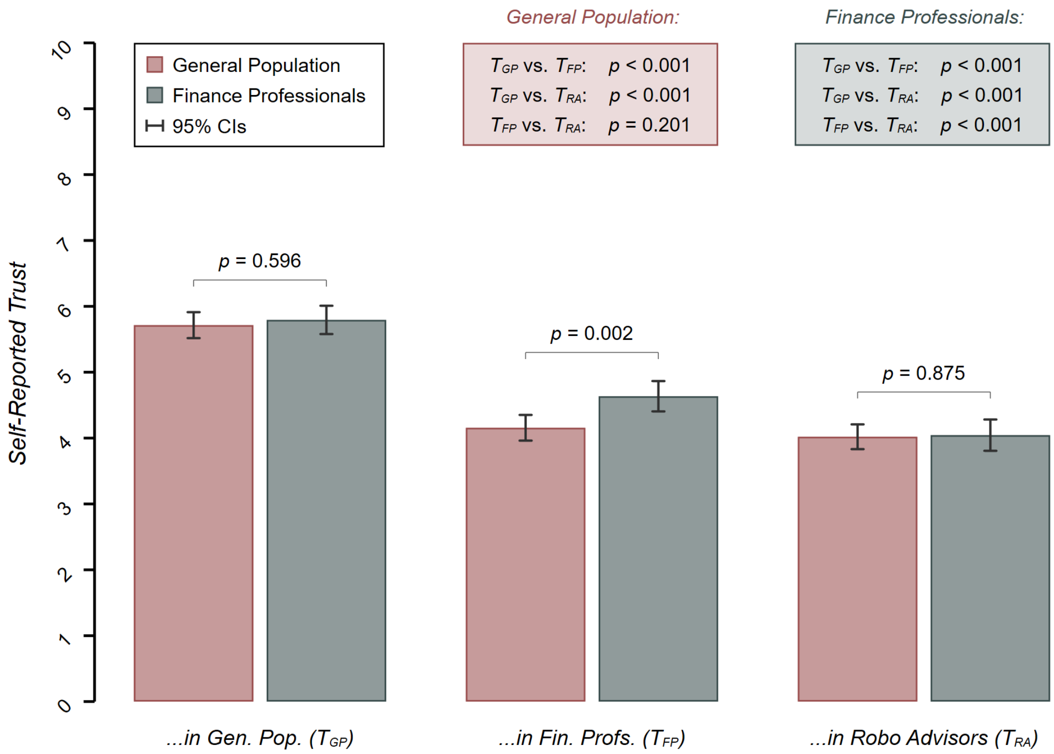

Figure H1: Self-reported trust. This figure depicts the mean levels of trust (self-reported on scales from 1 to 10) in the general population (T_{GP}), in finance professionals (T_{FP}), and in investment algorithm (T_{RA}), respectively, separated for the general population sample and the finance professionals sample. p-values reported above the bars indicate comparisons between the subject pools and are based on two-sample t-tests (n = 958). p-values reported in the boxes are based on paired t-tests with sample sizes of 550 (general population) and 408 (finance professionals), respectively.

I. Supplementary Analyses

In addition to the robustness/validity tests reported in the main text, we examine—for the sake of completeness—whether the difference in survey responses regarding real-life delegation decisions to finance professionals and investment algorithms differs between the three treatment conditions. The survey questions were asked after the incentivized task in the experiment and were identical in all three treatments. Therefore, answers are expected not to differ systematically between treatments.

Panel (a) of Figure I1 depicts the mean responses to the two question on real-life delegation decisions to finance professionals and investment algorithms, respectively, conditional on the three treatments. Apparently, the significant difference in mean responses regarding real-life delegations to finance professionals vs. investment algorithms maintains when conditioning the analysis on the three treatments. Notably, however, we observe that self-reported delegation rates to finance professionals (reddish bars) and algorithms (bluish bars) appear to depend on the treatments in a systematic way. In particular, the differences in self-reported delegation rates across treatments mimic the differences in the incentivized task displayed in Figure 2 of the main text. For both questions, we find the same rank order of delegation rates in real life as in the experimental task (i.e., *GP-FIXED* < *GP-ALIGNED* < *GP-ALGO*). To some extent, this impression is confirmed by two-sample t-test, which indicate statistically significant differences for the comparison of the mean survey responses to both questions (delegation to finance professionals and investment algorithms) between *GP-FIXED* and *GP-ALGO*, respectively.

Panel (b) of Figure I1 replicates the analysis in Panel (a) of Figure I1 but uses participant-level demeaned survey scores as the dependent measure, accounting for participant-level fixed effects. While the differences in means between the two questions are unaffected by demeaning, it is worthwhile to emphasize that the increasing pattern across treatments does not perpetuate. This indicates that the systematic treatment-induced impact on survey responses only affects the level of responses on the Likert-scale, but not the relative difference between responses with respect to the two questions. Thus, we conclude that the analyses presented in the main text are not systematically affected by treatment-induced effects in responses to the survey questions.

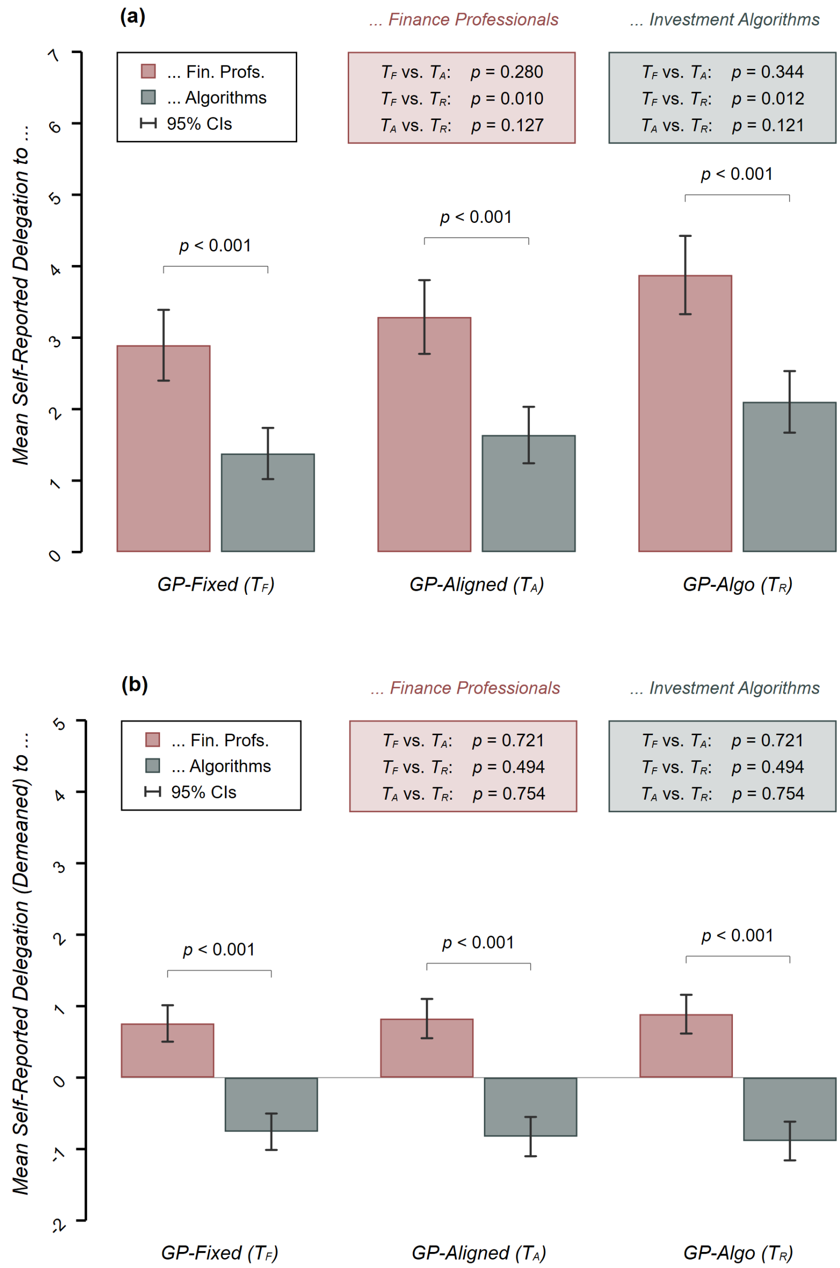

Figure I1: (a) Mean self-reported delegation to finance professionals and investment algorithms conditional on treatments. **(b)** Mean participant-level demeaned self-reported levels of real-life delegation decisions to finance professionals and investment algorithms conditional on treatments. p-values within treatment are based on paired t-tests; p-values between treatments (reported in the colored boxes) are based on two-sample t-tests.

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Delegated Decisions in Finance

Abstract
Based on an online experiment with a sample of finance professionals and participants from the general population (acting as clients), we examine drivers and motives of clients' choices to delegate investment decisions to agents. We find that clients favor delegation to investment algorithms, followed by delegation to finance professionals compensated with an aligned incentive scheme, and lastly to finance professionals receiving a fixed payment for investing on behalf of others. We show that trust in investment algorithms or finance professionals, and clients' propensity to shift blame on others increase the likelihood of delegation, whereas clients' own decision-making quality is associated with a decrease in delegation frequency.

ISSN 1993-4378 (Print)
ISSN 1993-6885 (Online)