Stakes and Investor Behaviors*

Pengfei Sui† and Baolian Wang‡

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Abstract

We examine how stakes affect investor behaviors. In our unique setting, the same investors trade stocks in their real accounts using their own money and, at the same time, trade in a simulated setting. Our *real-world within-investor* estimation produces strong evidence that investors exhibit stronger biases and perform worse in their higher-stakes real accounts than in their lower-stakes simulated accounts. Even with no monetary stakes, investors exhibit strong biases in their simulated accounts and biases in the two types of accounts are strongly positively correlated. The behavioral consistency between the two types of accounts suggests that low-stakes experimental methods, although imperfect, can be informative about real-world human behaviors. Using account data from two brokerage companies, we find that investors exhibit a stronger disposition effect on positions with greater portfolio weight. Hence, the finding that stakes-strengthening-biases may not be unique to the comparison between no-monetary and high-monetary stakes.

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† Sui is affiliated with the School of Economics and Management, Chinese University of Hong Kong, Shenzhen. Email: psui@cuhk.edu.cn.
‡ Wang is affiliated with the Warrington College of Business, University of Florida. Email: baolian.wang@warrington.ufl.edu.

1. Introduction

Understanding the effects of stakes on human behaviors is of great interest to economists. First, it informs us whether insights gained in laboratories can be generalized to the real world. Economists have increasingly turned to the experimental method to understand human behaviors. Lab experiments enable investigators to conduct research under highly controlled conditions. Critics of the generalizability of lab studies have argued, though, that human behaviors may be sensitive to various factors that are systematically different between the lab and the real world (Thaler, 1986; Levitt and List, 2007; Benjamin, 2019). One of these factors is stakes. Second, understanding the stakes effect can also shed light on the mechanisms that underlie the observed behaviors. For example, if a behavioral bias is driven largely by weak incentives, high stakes may increase the effort level such that the bias will be extinguished or significantly muted.

This paper explores the effects of stakes on behaviors in a real-world stock market environment in China. One unique feature of our setting is that many investors carry two types of stock accounts: real brokerage accounts in which investors trade using their own money and simulated accounts in which no real money is involved. Except for the fact that trades in simulated accounts do not involve real money, almost the same rules apply to these accounts as to real accounts. Such a unique setting enables us to construct a within-subject estimate of stakes effects. Using a sample of more than 4,000 investors over a three-year period, we compare the two types of accounts with reference to a standard set of prominent behavioral patterns that have been documented in the finance literature (Barberis and Thaler, 2003; Barber and Odean, 2013). As these patterns are typically considered suboptimal and most evidence shows that they are detrimental to investor performance (see Section 4.1), following the finance literature, we often refer to them as biases in this paper.

Our setting is nearly ideal for studying the effects of stakes on behaviors. First, real accounts undoubtedly involve more substantial stakes than simulated accounts. Our conservative estimate suggests that the average real-account value is worth several times China’s annual disposal income per capita. Second, critics believe that, in addition to the effects of high stakes, biases are likely to be extinguished or significantly muted by experience and competition in the real world, and it is plausible that the cognitive processes underlying belief formation and revision operate differently in natural environments than they do in abstract lab settings (Thaler, 1986; Levitt and List, 2007; Benjamin, 2019). Another criticism is that experimental subjects—typically students—may not be representative of the relevant population of investors. In other words, even if researchers can increase the stakes to the real-world level in labs, the stakes effects observed in the lab may still differ from the stakes effects in the real world due to other lab–field differences. Our real-world setting enables us to circumvent these criticisms.

Our headline result is surprising. In contrast to the conventional wisdom that higher stakes should reduce biases, we find that investors exhibit *stronger* behavioral biases in their real accounts than in their simulated accounts. We observe in the real accounts a stronger disposition effect (a stronger tendency to sell stocks that earn capital gains than to sell those that incur capital losses), stronger lottery preferences (buying lottery-type stocks, which are lower-priced, more volatile, and more positively skewed than non-lottery stocks), greater extrapolation (buying stocks with high past returns), greater underdiversification (holding smaller sets of stocks in portfolios), and more trading. Real accounts perform worse than their simulated accounts. The only behavioral pattern with respect to which we do not find a significant difference between real and simulated accounts is local bias (the tendency to hold stocks in firms located closer to where one lives).

Given our sample size and the advantage of the within-investor estimation strategy, we can estimate the effects of stakes precisely. Except for local bias, the differences in the strengths of biases between the real and simulated accounts are both statistically and economically significant. Even with respect to local bias, given the small standard error, we can confidently conclude that the difference between the two types of accounts is small. Consider extrapolation, underdiversification, and performance as examples. In real accounts, the average past six-month return on purchased stocks is 15.60% higher than the market, while it is 12.66% above the market in simulated accounts. On average, investors hold 2.96 stocks in their real accounts and 4.36 stocks in their simulated accounts. In the two months following purchases, the sample stocks deliver average returns for real and simulated accounts that are 0.66% and 0.15% lower, respectively, than returns on similar-sized stocks on the market.

These behaviors have been attributed to a range of cognitive biases. The disposition effect has been explained by an incorrect belief in the mean reversion of prices (Odean, 1998), the diminishing sensitivity of prospect theory (Shefrin and Statman, 1985; Barberis and Xiong, 2009), cognitive dissonance (Chang, Solomon, and Westerfield, 2016), or realization utility—whereby investors derive utility from realizing gains and losses on assets that they own (Barberis and Xiong, 2012; Ingersoll and Jin, 2013). Lottery preference is consistent with the probability weighting function of prospect theory (Barberis and Huang, 2008) or the salience theory (Bordalo, Gennaioli, and Shleifer, 2012). Extrapolative beliefs stem from the "representativeness heuristics" (Kahneman and Tversky, 1972; Barberis, Shleifer, and Vishny, 1998) or a belief in the "law of small numbers" (Tversky and Kahneman, 1971). Underdiversification and local bias are consistent with ambiguity aversion and the familiarity bias or the "mere exposure effect." The mere exposure effect is the finding that mere exposure to someone or something makes us like that person or thing more than is justified based on informational considerations alone (Zajonc, 1968; Bornstein, 1989). Strong probability weighting may also explain underdiversification because portfolio diversification is sometimes sacrificed to gain portfolio skewness (Barberis and Huang, 2008). Excessive trading is consistent with overconfidence and sensation-seeking (Odean, 1998; Barber and Odean, 2000; Scheinkman and Xiong, 2003; Grinblatt and Keloharju, 2009).

Although some heuristics and biases (e.g., prospect theory) can contribute to explaining more than one behavior (Barberis, 2013), the headline finding from this literature is that these behaviors are likely driven by distinct factors (Barberis and Thaler, 2003; Barberis, 2018). Consistent with this, we find that these biases are only weakly correlated with each other. That these biases are distinct and that we cover most of the widely studied investor behaviors in finance suggests the broad scope of findings indicating that stakes strengthen biases, at least in stock markets.

The evidence also reveals systematic individual differences in the stakes effect. In other words, across investors, a larger real–simulated difference in one bias is typically associated with larger real–simulated differences in other biases. The findings of cross-investor heterogeneities in stakes effects naturally suggest exploring individual characteristics to uncover the microfoundation of why stakes matter. As our data do not have many individual characteristics, we leave this for future study.

It is possible that stakes strengthen biases because, insofar as there is no real money involved in simulated accounts, investors may trade randomly and hence show no systematic biases. Early experimental economists made a point of studying biases by implementing financially incentivized designs drawn from the psychological literature that were previously studied using hypothetical questions (Grether and Plott, 1979; Grether, 1980). We present two sets of evidence that are inconsistent with this conjecture. First, even though biases are generally weaker in simulated accounts than in real accounts, the biases we find in simulated accounts are strong. Second, there are strong positive correlations between investor biases in the two types of accounts, suggesting behavioral consistency even with no monetary stakes involved. These findings indicate that low-stakes experimental methods, although imperfect, can be informative about real-world human behaviors.

The psychology and experimental economics literature has proposed several mechanisms that stakes may affect biases.[4] One widely-held view is that raising the stakes leads to greater attention and effort, and such an increase can lead to a more accurate translation of individuals' beliefs or preferences to their behaviors. The proposal of eliciting beliefs and preferences by financially incentivized designs is built upon this view (Grether and Plott, 1979; Grether, 1980).[5] The conventional wisdom that higher stakes should reduce biases assumes that an increase in attention and effort leads people to shift from heuristics toward more deliberative processing and form less noisy expectations and better decision-making (Stanovich and West, 2008; Gabaix and Laibson, 2017; Imas, Kuhn, and Mironova, 2022).

There are at least two nonexclusive mechanisms that higher stakes may strengthen biases in the financial context. The first mechanism challenges the assumption of the conventional wisdom. The marginal effect of stakes can be negative if people carry incorrect mental models and greater attention leads to the more careful application of the incorrect models (Dawes, 1979; Dawes, Faust, and Meehl, 1989; Hartzmark, Hirshman, and Imas, 2021). For example, if investors believe in price momentum, higher stakes can lead to more effort in locating stocks with higher past returns, strengthening the extrapolation bias. Similar arguments also apply to cases involving intrinsic but nontraditional preferences, such as prospect theory, realization utility, cognitive dissonance, and

[4] We review the stakes literature more comprehensively in Section 2.

[5] This mechanism relies on the assumption that decision-making is costly. If the cost of decision-making is zero, investors will always perfectly translate their beliefs and preferences to their behaviors, and actions will not differ by stakes level. That decisions in the financial context are costly seems obvious because investors need to collect information, form beliefs, and do the utility-maximization calculation.

sensation-seeking. Higher stakes may lead investors to make decisions that conform more closely to their preferences but deviate further from normative benchmarks.

Another mechanism by which stakes strengthen biases is that stakes can strengthen the features of nontraditional preferences. Rottenstreich and Hsee (2001) and Hsee and Rottenstreich (2004) show that the elements of prospect theory are more pronounced when a decision is more affective or emotional.[6] Compared with trading in simulated accounts, trading for real money is almost certainly more emotional and can potentially lead to a more pronounced value function and a more pronounced probability weighting function. A more pronounced value function and a more pronounced probability weighting function, in turn, can lead to a stronger disposition effect (Shefrin and Statman, 1985; Odean, 1998; Barberis and Xiong, 2009) and higher demand for lottery stocks (Barberis and Huang, 2008). A stronger lottery preference may also explain underdiversification because one may need to sacrifice diversification to gain portfolio skewness (Barberis and Huang, 2008).

The first mechanism predicts that higher stakes affect the translation of incorrect beliefs or nontraditional preferences to behaviors but does not predict a change in preferences per se. The second hypothesis predicts a change in preferences. The first mechanism can potentially apply to all the biases we study. Although we are not aware of evidence that stakes strengthen the features of nontraditional preferences other than prospect theory, such a possibility cannot be ruled out.

Although distinguishing between these mechanisms is beyond the scope of this paper, we provide some suggestive evidence. Both mechanisms predict that investors will exert more effort on and pay more attention to their real accounts than their simulated accounts. Consistent with results reported in the experimental literature (Enke et al., 2021), we find compelling evidence that higher stakes lead to greater effort/attention. Trading volume has been used as a proxy for attention (Barber and Odean, 2008). As discussed above, investors trade significantly more frequently in real accounts, suggesting that they pay greater attention to those real accounts. Investors are more likely to broadcast their real-account trades to their social networks than their simulated-account trades. Conditional on

[6] Faro and Rottenstreich (2006) find that people predict that others hold less pronounced prospect theory preferences than actually prevails, and Andersson et al. (2016) find that deciding for others reduces loss aversion; both of these findings are consistent with the notion that deciding for others or predicting what others will do is less emotional than deciding for oneself or predicting one's own behavior.

selling, investors are more likely to broadcast selling winners than selling losers. Such a winner–loser difference is more pronounced in real accounts than in simulated accounts, a finding that is consistent with the fact that trading for real money is more emotional.

We take advantage of another data feature to examine whether attention hurts performance further. Our main tests focus on the period during which an investor carries both types of accounts. Most of our sample investors open their simulated accounts before they start to trade for real money. We conjecture that investors in our sample will pay more attention to their simulated accounts when they hold only simulated accounts than when they carry both types of accounts. If attention hurts performance, we predict that investors in our sample will exhibit stronger biases in their simulated accounts and that those accounts will perform more poorly when they do not hold real accounts than when they do. Our empirical results support these conjectures.

The real−simulated account comparison is between high monetary stakes and no monetary stakes. Conceptually, the stakes effect does not have to be monotonic. The marginal effect of stakes on performance may be negative when stakes are zero or very low if low positive stakes reduce decision noise and lead investors with incorrect beliefs or nontraditional preferences to make decisions that conform more closely to how they think and prefer. When stakes are sufficiently high, the marginal effect may become positive if additional effort shifts people from using heuristics toward more deliberative processing. Although such a non-monotonicity possibility does not invalidate our identification's internal validity, it raises issues on the study's external validity, i.e., whether we can generalize the results to other settings.

The findings that investors do not trade randomly in their simulated accounts and their behavioral patterns are correlated between real and simulated accounts mitigate the external validity concern. We also examine another related possibility. Because simulated accounts involve no real money, some investors may use them for learning or as a “watch list” to keep track of stocks they are interested in buying but do not yet have enough conviction to buy. Perhaps stocks with past returns or lottery features above certain thresholds enter an investor’s real account and stocks that fall just short will enter her simulated accounts, which explains the real−simulated differences in extrapolation and lottery preferences. Investors may keep a bigger number of stocks on the “watch list” that they will buy, which explains the real−simulated difference in underdiversification. However, such interpretation is at odds with the results of local bias. If investors are more explorative in their simulated accounts, we may predict a lower local bias in their simulated accounts than the real accounts. It is also not obvious how such interpretation can explain the results of the disposition effect. Moreover, almost all trading platforms in China have a “watch list” function, and it is unclear why investors may use the simulated account function as a “watch list.” Nevertheless, we find similar results if we drop investors who are suspected of using the simulated accounts as “watch lists.” Such investors are defined as those who have *ever*, in our sample period, purchased any stock in their real account and the stock was in their simulated account at the time of the purchase. If a “watch list” helps with selecting stocks, we shall expect that having a simulated account will strengthen extrapolation and lottery preferences. However, the results are the opposite.

Ideally, we would like to gather information on investor behaviors with all stakes levels. We provide evidence using two other account-level datasets—one from a US brokerage company and the other from a Chinese brokerage company—to shed light on this question. In this setting, the variations in stakes come from distinct positions in an investor’s portfolio. Positions that carry greater portfolio weights are considered to involve higher stakes. We find that investors exhibit a strong disposition effect with regard to positions that carry greater portfolio weights. Positions with different portfolio weights all have positive and typically high monetary stakes. Hence, the evidence from the two brokerage datasets shows that within high monetary stakes, stakes strengthen biases. Overall, with the caveat that portfolio weights are not random, these findings provide additional evidence supporting higher stakes strengthening biases.

The paper is organized as follows. Section 2 provides an overview of related research. In Section 3, we describe our data and methodology. In Section 4, we define our measures of investor biases and report summary statistics for those measures. Section 5 presents the results. In Section 6, we report results based on the two brokerage account datasets. We conclude in Section 7.

2. Overview of related research

Our research contributes, in the first instance, to the literature that studies how stakes affect biases.[7] The great bulk of this literature consists of experimental studies conducted in labs. Due to budget considerations, the standard method for studying the effects of stakes on behaviors involves the use of payments that amount to a couple of hours of an individual's wages to incentivize experimental subjects—typically students—to commit time and effort to participate. In some studies, researchers are able to pay subjects up to several times their normal monthly incomes, which is typically done by experimenting in low-income countries.[8] Despite decades of research on the stakes effect, empirical evidence that compares biases under no or small stakes and very large stakes is scant (Enke et al., 2021). Our study provides novel evidence based on a non-experimental setting in which we compare no monetary stakes and high monetary stakes. We study a standard set of prominent behavioral biases of financial market investors. The great bulk of this literature examines behaviors different from ours.[9]

Field studies on stakes are almost always based on cross-individual comparisons. Chetty, Friedman et al. (2014) show that individuals in Denmark are inattentive with respect to their pension contributions regardless of the economic stakes. Gathergood et al. (2019) find evidence that individuals' credit card debt repayments are not allocated to higher interest rate cards, and the degree of misallocation is invariant with the economic stakes. Cross-individual comparisons are affected by

[7] See Enke et al. (2021) for a review of this literature.

[8] Most of these studies examine subjects' behaviors in strategic games such as the ultimatum game or coordination games. Slonim and Roth (1998), Cameron (1999), and Andersen et al. (2011) study the ultimatum game. Parravano and Poulsen (2015) study coordination games. Rapoport et al. (2003) study a three-person centipede game. Cooper et al. (1999) study gaming against managers. Binswanger (1980), Kachelmeier and Shehat (1992), and Holt and Laury (2002) study risk preference. Kachelmeier and Shehata (1992) conducted a study in China where they can offer very large monetary incentives relative to subjects' living costs. They report that the level of monetary incentives affects the revealed risk preferences. Even with extremely high monetary incentives (when subjects earned three times their normal monthly revenues in the course of a two-hour experiment), subjects exhibit probability weighting.

[9] There is also a body of literature documenting that some biases survive in the high-stakes real world. Individuals exhibit various biases in various game shows (Metrick, 1995; Berk et al. (1996), Levitt (2004), Post, Assem, Baltussen, and Thaler (2008), Belot et al. (2010), Van den Assem et al. (2012), and Jetter and Walker (2017)), in art auctions (Graddy (2009) and Graddy et al. (2014)), in professional golf competition (Pope and Schweitzer, 2011), and among asylum judges, loan officers, and baseball umpires (Chen et al., 2016). The behavioral finance field has documented many biases (Barberis and Thaler, 2003; Campbell, 2006; Barber and Odean, 2013; Beshears et al., 2018; Malmendier, 2018) that we study. Our contribution is not to document the existence of biases in high-stakes financial markets but to document that, in real-world financial markets, higher stakes can lead to stronger biases, while the studies cited in this note do not compare what occurs in high- and low- stakes situations.

omitted variables such as financial sophistication, wealth, and IQ. To the best of our knowledge, Andersen, Mukherjee, and Nielsen (2021) have conducted the only other field study that examines the causal effects of stakes. They exploit exogenous variation in stakes resulting from unexpected inheritances following sudden parental death and report economically small effects on biases, with most bias reductions passive (i.e., from the inheritance per se). For example, inheriting assets from parents mechanically reduces a beneficiary's underdiversification if the inherited assets are weakly correlated with the beneficiary's portfolio. In their setting, it is also difficult to differentiate between stakes effects and wealth effects, while wealth effects have been shown to affect stock market participation in the same setting (Andersen and Nielsen, 2011).

In contrast to the conventional wisdom that higher stakes should reduce biases, existing studies show that the effects of stakes on performance depend on the nature of the tasks involved. First, findings indicating that higher stakes improve performance concentrate on tasks where increased effort can improve performance (Camerer and Hogarth, 1999). Such tasks include memory or recall tasks and clerical tasks (e.g., coding words or building things). There is also some evidence that stakes improve academic performance (Levitt et al., 2016). In a recent study, Enke et al. (2021) find that higher stakes also help subjects taking the Cognitive Reflection Test, which is a task designed to measure a person's tendency to override an impulsive incorrect response and engage in further reflection to find a correct answer.[10] Second, stakes do not change average behavior substantially with respect to tasks involving high cognitive effort costs, such as tasks testing base rate neglect and anchoring (Camerer and Hogarth, 1999; Enke et al., 2021). Third, stakes can hurt performance. Such phenomena were typically found in tasks that involve creativity or tasks in which simple intuition or habit provides optimal answers and thinking harder makes things worse (Arkes, Dawes, and Christensen, 1986; Ashton, 1990; Hogarth et al., 1991; Ariely et al., 2009). These findings suggest that individuals may "think too much" (Wilson and Schooler, 1991) or "choke under pressure" (Baumeister, 1984).[11]

[10] Enke et al. (2011, p33-34) write that, ". . . reducing reliance on 'system 1' is not enough to overcome biases because these are often limitations of the more deliberative 'system 2.'"

[11] Ariely et al. (2009) discuss three mechanisms that individuals may "think too much" or "choke under pressure." First, the "Yerkes-Dodson law" posits that there is an optimal level of arousal for executing tasks and that departures from this level in either direction can lead to decreased performance (Yerkes and Dodson, 1980). Second, increased stakes can cause people, involuntarily, to consciously think about the task, shifting control of behavior from "automatic" to

Fourth, high stakes affect behaviors involving nonpecuniary considerations. High stakes seem to reduce the weight of nonpecuniary factors such as moral costs or reputational concerns and shift subjects' behaviors in strategic games toward the predictions of rational models (Smith and Walker, 1993; Camerer and Hogarth, 1999; Levitt and List, 2007; Andersen et al.. 2011). Andersen et al. (2011) find that, in the ultimatum game, sufficiently high stakes lead responder behaviors to converge almost perfectly on full acceptance of low offers. In some cases, extrinsic monetary incentives crowd out intrinsic incentives such as volunteering and other pro-social behaviors (Gneezy and Rustichini, 2000; Gneezy et al., 2011; Bowles and Polania-Reyes, 2012).

To the best of our knowledge, we are the first to document a negative effect of stakes in real-world financial investment tasks. "Thinking too much" or "choking under pressure" can explain why stakes hurt performance, but these mechanisms do not seem to be most relevant to our financial context. We propose two possible mechanisms for our findings. First, raising stakes leads to greater attention and effort, and such an increase facilitates the translation of incorrect beliefs or nontraditional preferences to behaviors. A recent experiment by Hartzmark, Hirshman, and Imas (2021) documents that individuals react in more extreme ways to information about a good they own than to the same information about a non-owned good, consistent with investors having incorrect beliefs and attention hurting. Second, raising stakes leads to higher emotion that strengthens the features of nontraditional preferences. These two mechanisms are not mutually exclusive, and both likely play some roles.

Our study also contributes to the literature that studies mechanisms that underlie investor biases. Any explanation of a bias should also be able to explain why the bias gets worse at higher stakes or at least consistent with this prediction. Cronqvist and Siegel (2014) document that genetic differences explain a large portion of cross-investor variations in biases.[12] Grinblatt, Keloharju, and Linnainmaa (2011, 2012) show that underdiversification and the disposition effect are stronger in individuals with lower IQs. Barberis (2013) argues that prospect theory can explain several investor

"controlled" mental processes even though it is well documented that automatic processes are more effective for tasks that are highly practiced and automated (Langer and Imber, 1979; Camerer, Loewenstein, and Drazen, 2005). Third, increased stakes tend to narrow individuals' focus of attention (Easterbrook, 1959), including the breadth of the solution set people consider. This can hurt tasks that involve creativity.

[12] Cronqvist and Siegel (2014) also review the literature on associating specific genes with biases.

biases. Gabaix (2014, 2019) provides a unifying framework for thinking about inattention as a common source of several biases, and Birru et al. (2021) provide evidence that is consistent with this framework, which assumes that individuals carry the correct mental models, but cognitive costs lead them to form noisy expectations. Such a framework predicts that higher stakes will lead to greater effort and hence reduce biases. Our findings and those of several recent studies (Enke et al., 2021; Hartzmark, Hirshman, and Imas, 2021) suggest that many individuals may not carry the correct mental models, and poor decisions are driven in part by deeply rooted biases that strong motivation, at least alone, cannot improve.[13]

Finally, we stress that we do not expect our stakes-strengthen-biases results to extend to all other biases or all other settings. As discussed above, stakes have effects that vary with the nature of tasks. When discussing the generalizability issue, it is useful to emphasize one important feature of financial markets. Stock returns are characterized by a low information-to-noise ratio. Such a feature may prevent investors from learning. Accurate learning takes place only when individuals receive timely and organized feedback (Thaler, 1986). Competition does not seem to work effectively at eliminating biased investors from financial markets (De Long et al., 1990). In addition, in financial markets, nonpecuniary factors may not exist or are unimportant factors in investor trading. As discussed above, evidence shows that high stakes tend to reduce the consideration of nonpecuniary factors. Last, our data are drawn from an investor social network and, as a result, our investors' trades can be observed by their peers. Although our within-subject estimates remain valid because both types of accounts are observable, we note that stakes effects may be a function of context. The biases in our real accounts are similar to what have been documented using other account-level datasets, mitigating this concern.

3. Data and methodology

Our primary data source is Snowball Finance (Xueqiu.com; Snowball hereafter), supplemented with stock price, return, and volume data from the China Stock Market & Accounting

[13] Evolutionary models of biases provide insights into this phenomenon. Proponents of these models argue that behaviors that represent investment biases today might have been advantageous in ancient times (Rayo and Becker, 2007).

Research Database (CSMAR).[14] In this section, we discuss the background information pertaining to Snowball and the data.

2.1 Snowball Finance

Snowball is one of the most popular social networks among Chinese investors. Founded in 2010, it has 12 million active users. The users range from small retail investors to professionals, including sell-side analysts and fund managers. On Snowball, investors can access real-time price and volume information on financial securities. Like Twitter, Snowball allows its users to share information and post their opinions on investment and financial issues and interact with other users.

One unique feature of Snowball is that users can carry two types of investment accounts: real and simulated. Users can register at Snowball and obtain one or more simulated accounts. They can also link their brokerage accounts with Snowball and reveal their transactions on the website.[15]

Users may link their brokerage accounts for several reasons, and some benefits and website functions are available only if they do. For example, only linked investors can observe other linked investors' trading records. Through collaboration with the brokerage firm, Snowball helps linked users participate in IPO subscriptions and enhance the returns on their cash balances (i.e., by participating in reverse repurchase agreements or money market mutual funds). Investors may also enjoy the experience of interacting with others and being influential.

Based on our conservations with Snowball representatives, when dealing with trading in the simulated accounts, Snowball tries to follow real stock market rules as closely as possible. There are, however, several differences. First, orders from simulated accounts are matched with the prevailing exchange order book, but simulated trades will not change the order book or have a price impact.[16] Second, Snowball does not consider brokerage commissions (about 0.25% of the value of a transaction) or stamp-duty taxes (0.1% of transaction value, for sales only) in calculating the values of positions in the simulated accounts. Third, cash dividends are automatically reinvested into those dividend-paying stocks.

[14] Sui and Wang (2022) also use Snowball data. They focus on real accounts and study self-enhancing transmission bias.
[15] Most of the brokerage accounts are held at Ping'an Securities, one of the largest securities firms in China.
[16] Both the Shanghai Stock Exchange and the Shenzhen Stock Exchange are pure limit-order markets.

We do not expect these differences to have an important effect on our results. The sample investors are retail investors. Treating them as price takers (i.e., with no impact on the order book) is approximately correct. Given that Snowball does not consider commissions or taxes with simulated accounts, we expect investors to trade more actively in those accounts. As we show later, however, investors trade significantly more frequently in real accounts.

2.2 Snowball data

The data we collected from Snowball are very similar to those used by Odean (1998) and Barber and Odean (2000). We created a user file and a trade file. We also collected a file with all the posts. The first real account was opened on June 24, 2016 and the first simulated account was opened on October 22, 2014. Our sample starts on June 24, 2016 and ends in May 2019, when we began collecting data.

At the user level, we collected information on user identifications, the list of accounts a user holds (including closed accounts), a real/simulated flag for each account, an active/closed flag for each account (measured at the time we collected data), an account's initiation date, an account's close date (if applicable), and user locations if available. About half of the users in our main sample reported their locations, either at the province level or the prefecture-city level.

We also created a trade file. The trade file contains information on investor identification, account identification, trade date, trade time, asset symbol, type of record (dividend payment, buy/sell, IPO subscription, etc.), and trade price. The data do not include information on the number of shares for each asset or the total value of an account. Instead, the data include the value of one asset as a fraction of the total portfolio value. For each trade, we collect the fractions before and after the trade. We infer the direction of a trade by comparing the change in fractions. For example, if the fraction of a stock decreases from 50% to 20% after a trade, we infer that this is a sale. Information indicating changes in fractions also enables us to calculate portfolio turnover. The sum of a change in fractions within a period gives us a measure of portfolio turnover.

Once the link between an investor's brokerage account and Snowball is set up, all subsequent real-account trades will be disclosed on Snowball. Trades that occur before the link are not disclosed, which avoids selective disclosure based on ex-post performance. All trades from the simulated accounts are disclosed.

The data are subject to several limitations. First, users can close their simulated accounts or real accounts at any time. Trading records and daily portfolio performance data for closed simulated accounts are kept on the website but are removed for closed real accounts. For closed real accounts, the website keeps account initiation dates, account close dates, and portfolio performance as of account close dates. The fact that the trading histories of closed real accounts are unavailable to us raises a sample-selection concern. We argue that this sample-selection concern may not be particularly problematic. First, Sui and Wang (2022) find that real account closings do not seem to correlate with performance. Second, as documented later, investors achieve *lower* performance and exhibit *stronger* biases in their real accounts. If an investor is more likely to close a real account when performance falls or when biases are stronger, we would expect the sample selection to lead to an underestimation of the stakes effects.

Second, Snowball sets limits on the total number of trades available on the website. If the number of trades from a real account is above 200, only the most recent 200 trades will be visible. The limit is 1,000 for simulated accounts. The majority of accounts do not reach these limits, and therefore we have data on their entire trading histories for most of the accounts in our sample. Truncation is more likely to affect users who trade very frequently. Our data overweight their trading in the period immediately before May 2019, when we collected our data. We evaluate how this truncation affects our results later in the paper.

2.3 *Summary statistics*

We focus on investors who hold both real and simulated accounts. In our main analyses, for each investor who holds both types of accounts, we restrict our analysis to the period when both types of accounts are active. In some tests, we examine whether the presence of the other account affects the biases. To collect sufficiently voluminous data to measure investor behaviors, we require that the overlapping period lasts for at least one year.

We start with 13,993 investors with real accounts. Of these investors, 8,732 also hold simulated accounts. To be included in our sample, an investor has to trade stocks listed on the two domestic exchanges in both types of accounts. As a result, 398 investors are excluded. Lastly, we exclude 3,921 investors whose real and simulated accounts overlap for less than one year. Our final sample includes 4,413 investors. Virtually every investor holds one real account, but many hold multiple simulated accounts. In total, the 4,413 investors hold 20,603 simulated accounts. Among the final sample, 1,081 investors hold only one simulated account. Our results are similar if we focus on these 1,081 investors.

In Table 1, we report the summary statistics for the sample investors. The average length of the overlapping period is 668 days. The average number of trades in the real accounts is 99.66. For investors with multiple simulated accounts, we first calculate the number of trades for each simulated account and then average it across all the simulated accounts an investor holds. We obtain an average number of trades in a simulated account of 39.77.

Although Snowball does not reveal account values, our conservative estimates suggest that the average real account value is worth four to six times the annual disposable income per capita in China. We estimate the value of each real account by taking advantage of the exchange trading rule according to which a stock's minimum trading unit is 100 shares. First, we estimate the value of each trade by assuming that the trade size is 100 shares. This is the lowest possible trade value. Second, Snowball reports the size of each trade as a fraction of a trader's total portfolio value. We divide the trade value by the fraction, obtaining an estimate of the lower bound of the portfolio value at the time of the trade. Third, we calculate the highest portfolio value across all the trades executed by a given investor and use this figure as our final measure of portfolio value. The estimated mean (median) portfolio value is 244,000 RMB (106,000 RMB) or approximately 35,030 US dollars (15,218 US dollars). The mean (median) annual disposable income per capita in China in 2019 (the last year of our sample) was 30,733 RMB (26,523 RMB) or approximately 4,412 US dollars (3,808 US dollars).

4. Investor behaviors and methodology

In this section, we introduce the investor behaviors that we examine in this paper. All the behaviors we examine have been widely studied in the finance literature (Barberis and Thaler, 2003; Barber and Odean, 2013). For some behaviors, there may be alternative measures. We discuss these alternative measures and report the results of robustness tests with these measures in Section A of the Appendix.

We calculate each investor behavior measure at the individual level and separately for real accounts and simulated accounts. Virtually all the investors in our sample hold just one real account, but as noted above, many hold multiple simulated accounts. We estimate the disposition effect by pooling data from all the simulated accounts together. We first calculate other bias measures for each simulated account and then take the average across all of an investor's simulated accounts. For diversification analysis, in addition to the average-across-accounts approach, we also measure diversification by treating all the simulated accounts as one and report this result in the Appendix. Our results are qualitatively similar if we focus on investors who hold just one simulated account. We winsorize all the measures at the 1% level and the 99% level separately for real accounts and simulated accounts to mitigate the effects of extreme values.

4.1 Investor behaviors

4.1.1 The disposition effect

The disposition effect, the tendency to sell stocks that earn capital gains rather than stocks that incur capital losses, is perhaps the most robust empirical finding on investor trading (Shefrin and Statman, 1985; Odean, 1998).

To examine how investors in our sample tend to sell winner stocks and hold loser stocks, we first use the transaction dataset to construct a holding sample that contains observations for all investor-stock-day pairs (Ben-David and Hirshleifer, 2012). For example, if an investor bought Stock A on January 2, 2018, and held it until January 31, 2018, there will be 22 observations (22 business days). Purchases and sales are aggregated on a daily basis for each account-stock. We flag the days when a position is opened and when shares are sold (including partial sales). We use the weighted average purchase price and take stock splits and dividends into account to calculate the holding-period returns. We exclude positions for which we do not have information on the initial purchase price, mainly because those positions were established before a Snowball link.

We estimate the disposition effect using the following model,

$$Sell_{i,j,t} = \alpha + \beta_i Gain_{i,j,t-1} + \varepsilon_{i,j,t}, \quad (1)$$

where i, j, and t denote investor i, position j, and day t, respectively. $Sell_{i,j,t}$ is a dummy variable that equals 1 if investor i sells position j (partially or fully) on day t and 0 otherwise. $Gain_{i,j,t\text{-}1}$ is a dummy variable that equals 1 if investor i experiences a gain in stock j at the end of day t-1 and 0 otherwise. We estimate Equation (1) using a linear probability model. The coefficient, β_i, is our measure of the disposition effect for investor i. To facilitate the presentation of the reported results, we multiply β_i by 100.

The individual-level disposition effect estimates may be noisy. We could, alternatively, estimate the disposition effect by pooling all the real accounts or all the simulated accounts together. We can also estimate Equation (1) using a hazard model instead of a linear probability model. We find qualitatively similar results with all these methods and report them in the Appendix.

4.1.2 Lottery preference

We define lottery stocks following Kumar (2009). Specifically, we consider three stock characteristics to identify stocks that might be perceived as lotteries: idiosyncratic volatility, idiosyncratic skewness, and stock price. At the end of month t, we compute both idiosyncratic volatility and idiosyncratic skewness using daily data from t-6 through t-1. The idiosyncratic volatility measure is the variance of the residuals obtained by fitting a one-factor model to the daily stock returns, where the factor is the market excess returns. We measure idiosyncratic skewness following Harvey and Siddique (2000). Specifically, idiosyncratic skewness is a scaled measure of the third moment of the residuals obtained by fitting a two-factor model to the daily stock returns, where the two factors are excess market returns and squared excess market returns.

We consider all listed domestic Chinese stocks and define stocks in the lowest 50^{th} stock price percentile, the highest 50^{th} idiosyncratic volatility percentile, and the highest 50^{th} idiosyncratic skewness percentile as lottery-type stocks (Kumar, 2009).

At the individual investor level, we calculate the strength of lottery preferences separately for real accounts and simulated accounts, using the percentage of lottery stocks an investor buys over the sample period. We weigh each purchase with a purchase's dollar amount as a fraction of the total portfolio value at the time of purchase. For example, if a purchase represents 20% of the total portfolio value at the time of purchase, and another purchase represents 40% of the total portfolio value at the time of purchase, the second purchase is assigned a weight that is twice that of the first purchase. We obtain qualitatively similar results if we weigh different purchases equally or measure lottery preference based on holdings instead of purchases.

We report the results based on the three characteristics (price per share, idiosyncratic volatility, and idiosyncratic skewness) in the Appendix.

4.1.3 Extrapolation

Investors tend to buy stocks with abnormally high past performance (Barber and Odean, 2013). We expect that investors harboring stronger extrapolation bias will buy stocks with higher recent past returns. For each stock an investor bought on day *t*, we calculate the excess return on this stock, defined as the return on the stock net of the market return over the preceding 126 trading days. We aggregate these excess returns (in percentages) across all an investor's purchases, separately for real and simulated accounts. As we do for the lottery preference measure, we weigh each purchase by its dollar amount as a fraction of the total portfolio value at the time of purchase. We use average past excess returns as the proxy for investors' tendency to follow extrapolation strategies. Our results are not sensitive to window choices and we report them in the Appendix.

4.1.4 Underdiversification

Our measure of diversification is the number of stocks in an investor's portfolio. We count the number of stocks in a portfolio every day and use the time-series average as our measure of diversification. For all other bias measures, a higher value is associated with a stronger bias. To be consistent and for ease of exposition, we use the negative value of the natural logarithm of the average number of stocks as our measure of underdiversification.

4.1.5 Local bias

It has been documented that investors prefer stocks in firms that are located geographically near to them (Ivkovic and Weisbenner, 2005; Massa and Simonov, 2006).

To test whether investors in our sample tend to hold local stocks, we collect the headquarters of all the listed firms and the residence cities of the investors.[17] We obtain firm headquarters from WIND. We then identify the latitude and longitude of each location by relying on the Baidu map Application Programming Interface and calculate the distance (in kilometers) between headquarter cities and investors' residence cities using the haversine formula. The haversine formula determines the great-circle distance between two points on a sphere, given their longitudes and latitudes.

Our primary measure of local bias is the benchmark-adjusted average distance (in kilometers) between an investor's location and the headquarters of the stocks the investor bought over our sample period. We first calculate the volume-weighted average distance between an investor and his/her purchased stocks. The benchmark is the market capitalization-weighted average distance between this investor's location and the headquarters of all the listed stocks. Investors for whom the average distance is shorter than the benchmark are more likely to exhibit local biases. Our main local bias measure is calculated as log (1 + benchmark distance) – log (1 + average distance). We add 1 to the average distance because there are cases where investors buy only stocks located in their resident cities and therefore exhibit an average distance of 0. Our results are also similar if we measure local bias using the excess weight by which an investor buys stocks in his/her own province or prefecture city.

4.1.6 Turnover

Barber and Odean (2000) find that investors trade too frequently. We investigate the frequency of trading for the investors in our sample using portfolio turnover.

We calculate turnover at the account level through the following steps. First, we define the date for the first and last observations in one portfolio within the overlapping period as the start and end dates. We obtain the duration of one portfolio as the total number of days between the end date and the start date. Notice that we measure duration in calendar days rather than trading days. We aggregate the corresponding fractions across all trades within the duration of one portfolio and divide

[17] About 50% of our sample investors report their location. Of the investors who report their locations, about 80% report the prefecture-city level and the other 20% report the province level. We drop the latter from the analysis when calculating the benchmark-adjusted distance and when calculating the excess weight of stocks in one's city.

the sum by two. Finally, we obtain the daily account turnover by dividing the aggregated fraction by the total number of duration days. We report the annualized turnover rate by multiplying the daily turnover by 365.

4.1.7 Investor performance

For each purchase, we calculate the stock's buy-and-hold returns (in percentages) over the following two months. We adjust it by size-decile portfolio returns. As we do for lottery preferences, extrapolation, and local bias, we weigh each purchase with a purchase's dollar amount as a fraction of the total portfolio value at the time of purchase. Our results are similar if we measure performance over the following one, three, or six months. Size-decile portfolios are formed based on market capitalization at the end of the previous month and rebalanced monthly.[18] Our performance measure is not affected by the difference in the treatments of commissions and taxes between the real and simulated accounts on Snowball.

4.1.8 Discussions

Most investor behavior literature considers these behaviors suboptimal. The theoretical literature has proposed various mechanisms to explain these behaviors based on irrational beliefs, non-traditional preferences, or bounded rationality. The empirical evidence predominately shows that these behaviors are associated with lower investor performance.[19] For example, Barber, Lee, Liu, and Odean (2009) show that the aggregate portfolio of individual investors in Taiwan suffers an annual performance loss equivalent to 2.2% of Taiwan's gross domestic product, and at least two-thirds of the losses (commissions and transaction taxes paid) are attributable to heavy trading.

[18] Snowball reports portfolio performance on its website. As stated on its website, though, its calculation "may be affected by special cases." Our calculation shows that there are an unusually high number of days with extreme returns. For example, about 14% of real portfolios experience a daily return that is higher than 22.22% or lower than −22.2%. With the daily ±10% price-limit rule, 22.22% and −22.2% are the maximum and minimum possible daily portfolio returns. Our further analysis suggests that, in some cases, Snowball does not properly observe cash flows into or out of an account. Our private communications with Snowball confirm this conjecture. Therefore, we decided to calculate portfolio returns by ourselves instead of using the reported returns.

[19] See the references in the Introduction, and Barberis and Thaler (2003) and Barber and Odean (2013) for reviews of both the theoretical and empirical contributions to this literature.

Evidence from Chinese data also points in the same direction. For the disposition effect, studies have argued that it could be motivated by a desire to rebalance portfolios or to avoid higher trading costs of low-priced stocks. These rational interpretations have been dismissed both in the US data and in Chinese data (Odean, 1998; An, Engelberg, Henriksson, Wang, and Williams, 2022). Lottery stocks underperform other stocks both in the US (Kumar, 2009) and in China (Jiang, Liu, Peng, and Wang, 2022) and therefore lottery preference is costly. Investors can exhibit extrapolation (i.e., buy past winners) if past winners continue to perform better (i.e., return momentum). The literature reports no momentum effect or a reverse-momentum effect in China (Liu, Stambaugh, and Yuan, 2019; Jiang, Liu, Peng, and Wang, 2022). Investors can exhibit local bias if they are more informed about local stocks, which is not supported by the data either in the US (Seasholes and Zhu, 2010) or in China (Seasholes, Tai, and Yang, 2010). Not surprisingly, as we show later, all these biases are negatively correlated with investor performance.

4.2 Summary statistics and correlation coefficients of biases

In Table 2, we report the summary statistics for the biases. For each behavioral bias measure, we report, separately for the real and simulated accounts, the first quartile, the median, and the third quartile, the number of investors for whom we can construct the measure as well as the means and standard deviations. It is evident that, in both simulated and real accounts, investors exhibit a strong disposition effect, extrapolation, and local bias. They also hold a small number of stocks in both types of accounts. The average fraction of lottery stocks in the entire stock market is 6.09%, so the investors exhibit a strong lottery preference in real accounts but not in simulated accounts. Investors earn negative abnormal returns in both types of accounts.

The biases in the real accounts are qualitatively similar to those documented in the literature: the disposition effect (Feng and Seasholes, 2005; Frydman and Wang, 2020; An et al., 2022), lottery preference (Liu et al., 2022), extrapolation (Liao, Peng, and Zhu, 2021), underdiversification (Feng and Seasholes, 2008; Frydman and Wang, 2020), local bias (Feng and Seasholes, 2008), turnover (Frydman and Wang, 2020; Liu et al., 2022), and performance (An et al., 2022). In cases where the estimates are comparable, the biases in the real accounts are also quantitatively similar: the disposition effect (Frydman and Wang, 2020), underdiversification (Feng and Seasholes, 2008; Frydman and

Wang, 2020), turnover (Liu et al., 2022), and performance (An et al., 2022). Such a finding mitigates the concern that our online investors may not be representative of the relevant population of investors. This finding also mitigates the concern that the observability of the trades on the online investor social network may dramatically change investors' behaviors.

In Table 3, we report the pairwise correlation coefficients between behavioral biases in the real accounts (Panel A) and in the simulated accounts (Panel B). First, in general, the biases are positively correlated with each other and are negatively correlated with performance. Most of the correlation coefficients are statistically significant. One exception is local bias, whose correlations with other variables are weak and do not exhibit a clear pattern. Local bias also does not strongly correlate with performance. The general negative correlations between the biases and performance are consistent with the view that these investor behaviors are costly mistakes. Second, the correlations are not very strong. They are all lower than 0.3, suggesting that these biases are distinct from each other.[20] Third, the correlation coefficients between biases/performance are higher in real accounts than in simulated accounts.

5. Results based on the Snowball data

5.1 Stakes effects: Differences between real and simulated accounts

Table 4 presents the main results of the paper. We conduct the within-subject comparison at the level of biases between real accounts and simulated accounts. For each behavioral bias measure, we report the means of real accounts and the means of simulated accounts. We also report the means of investor-level differences, their *t*-statistics, and Wilcoxon *p*-values.

Investors exhibit stronger biases in their real accounts than in their simulated accounts, with the exception that they do not exhibit a different local bias between the two types of accounts. These differences are highly statistically significant, as indicated by the high *t*-statistics. The differences are

[20] The generally weak positive correlations between investor biases and weak negative correlations between investor biases and performance is consistent with findings reported in the literature. See Bailey, Kumar, and Ng (2011) for an example. Liu et al. (2022) report a similar correlation table but their investor biases are calculated based on survey data. Attempts to consolidate many biases into fewer dimensions have also been made in the experimental economics literature (see Dean and Ortoleva, 2019; Chapman et al., 2022), but these studies typically examine biases other than those we examine.

also sizable. In real accounts, the probability that investors sell winners is, on a daily basis, 4.16% higher than the probability that they sell losers. In the simulated accounts, the difference is only 0.49%. In the real accounts, 6.49% of purchases involve lottery stocks, while in the simulated accounts, 4.26% of purchases involve those stocks. On average, investors hold 2.96 (exp(1.086)) stocks in their real accounts and 4.36 (exp(1.473)) in their simulated accounts. Annual turnover is 596% in real accounts and 84% in simulated accounts. In the two months after being purchased, the stocks investors hold in their real accounts underperform their size-decile portfolio by 0.656%, while the stocks they hold in their simulated accounts underperform that portfolio by 0.148%.

In Figure 1, we present the estimated kernel densities for behavioral biases in real and simulated accounts. Each plot uses a Gaussian kernel. The distributions of the biases reveal the contrast between real and simulated accounts. With respect to all the biases except local bias, the distribution of real accounts exhibits a clear rightward shift from the distribution of simulated accounts. The performance distribution of real accounts exhibits a clear leftward shift. These graphs make it clear that the differences between the two types of accounts are sizable and systematic, echoing the strong statistical power associated with Table 4.

We report the results of several robustness tests for the baseline results reported in Table 4. First, in Section A of the Appendix, we present the robustness tests using alternative measures of the behavioral biases and find qualitatively similar results. Second, in Section B of the Appendix, we report the results for the disposition effect using a pooled regression approach. The individual-level disposition effect estimates may be noisy. The pooled regression results confirm that the disposition effect is stronger in real accounts. Third, as discussed in Section 2.3, among the 4,413 investors, 3,332 hold multiple simulated accounts. In Section C of the appendix, we replicate the results reported in Table 4 by restricting the sample to investors who hold only one simulated account and find similar patterns. Fourth, as discussed in Section 2.2, Snowball sets limits on the total number of trades available on the website. We then consider the 19% of the sample whose trades are truncated on the Snowball website. We replicate Table 4 by restricting the sample to investors whose trades are not truncated or were executed in the last year of our sample, when such data truncation is least likely. The results reported in Section D of the appendix are similar to the main results.

Our within-subject design can identify the causal effects of stakes and have strong statistical power. Experimental results may however be sensitive to whether the design is within-subject or between-subject (Charness, Gneezy, and Kuhn, 2012). For example, consider ambiguity aversion. In Fox and Tversky (1995), one set of gambles is associated with clear odds of winning, while the other set of gambles is vague in this respect. The within-subject analysis (when individuals are given both clear and vague gambles) indicates ambiguity aversion while the between-subject analysis does not. To assess whether our results are sensitive to the choice between a within- and a between-subject design, in Section E of the Appendix, we calculate the biases for investors who hold only simulated accounts and for those who hold only real accounts. Again, we find similar results.

Some investors may use the simulated accounts for learning or as a “watch list” to keep track of stocks they are interested in buying but do not yet have enough conviction to buy. As discussed in the Introduction, this interpretation may explain the results of lottery preference, extrapolation, and underdiversification, but probably not local bias and the disposition effect. Almost all trading platforms in China have a “watch list” for investors, and our investors do not have to use the simulated accounts as “watch lists.” Hence, we view such the “watch list” alternative interpretation as unlikely.

Nevertheless, we conduct a robustness test in which we exclude the investors who are suspected of using the simulated accounts as watch lists. We define such investors as those who *ever* purchase, in our sample period, any stock in their real account and the stock was in their simulated account at the time of the purchase. Such a definition is probably too broad as we may exclude many false positives. We use this criterion to ensure the remaining sample is free of the “watch list” issue. The results in Section F of the Appendix indicate the same conclusion as in Table 4. In fact, the differences between real and simulated accounts are greater in this subsample than in Table 4. The “watch list” hypothesis also predicts that adding a simulated account should facilitate searching and stock selection and strengthen biases such as lottery preference, extrapolation, and underdiversification. In Section 5.4, we show that the results are exactly the opposite.

Overall, the results reported in Table 4 and Sections A–F of the Appendix indicate clearly that biases are stronger and performance is worse in the real accounts than in the simulated accounts. Such findings are inconsistent with the argument that higher stakes will weaken biases. In fact, they point in the opposite direction: biases are stronger when the stakes are higher.

5.2 *Behavioral consistency between real and simulated accounts*

The findings reported in Table 4 have strong implications for the experimental method as a means of understanding human behaviors. Consistent with many skeptical criticisms, our findings show that biases documented in low-stakes lab settings may differ from those that emerge in high-stakes real-world settings. Instead of observing that high stakes eliminate biases, however, as many skeptical scholars have argued, we observe that the low-stakes lab method may underestimate the biases harbored by real-world humans, at least in the case of stock markets. Can we learn anything, then, from the lab methodology about real-world human behaviors?

In Table 5, we examine whether and how biases are correlated between high-stakes real accounts and low-stakes simulated accounts. Although the lab method may underestimate or overestimate bias levels, it may still contain information about heterogeneity across investors. For example, if investor A holds a stronger lottery preference than investor B, she/he may exhibit a stronger lottery preference in both her/his real and simulated accounts than investor B, leading to positive correlations across investors between the real account lottery preference and the simulated account lottery preference.

This is indeed what we document. The columns labeled “Correlations” and “p-values” report the correlation coefficients of the biases between real accounts and simulated accounts as well as their p-values. It is evident that, between the same investors’ real and simulated accounts, the biases are strongly positively correlated. The correlation coefficients are all highly statistically significant, ranging from 0.132 for the disposition effect and 0.444 for local bias.

We then consider another method for testing the correlations. We first sort all investors into five quintiles by the value of a given bias in their real accounts. For each quintile, we then calculate the average value of the bias for both their real accounts and their simulated accounts. We also report the means of and t-statistics for the differences between biases in the highest quintile and the lowest quintile. For all seven measures, we see that sorting by the value of a bias in the real accounts leads to a sizable difference in the value of the bias in the simulated accounts. These differences are larger than the interquartile ranges of these biases (see Table 2), except with underdiversification and performance, for which the difference/interquartile range ratio is higher than 60%.

The results reported in Table 5 indicate strong behavioral consistency. These results also suggest that, even when there are low stakes in the simulated accounts, investors are not trading randomly and lab experiments can still help researchers study behaviors.

5.3 *Investor heterogeneity in stakes effects*

Do some investors exhibit stronger stakes effects than others? We examine this possibility and report the results in Table 6. For each bias, we calculate the individual-level differences between the real and simulated accounts. In Table 6, we report the correlation coefficients between these differences across investors. We expect the correlations to be positive if the stakes effects have an investor-level component. In other words, if higher stakes increase a bias of a given investor to a significant (small) extent, it may also increase other biases of this investor to a significant (small) extent.

The results reported in Table 6 show that, across all the bias measures, most of the correlation coefficients are positive, and all of them are negatively correlated with performance. As is the case with other tables, here local bias is an exception: the real–simulated difference in local bias is not strongly correlated with the real–simulated differences in other biases or performance. The real–simulated difference in underdiversification is also negatively correlated with the real–simulated differences in lottery preference and extrapolation, although this result is not statistically significant. Overall, the evidence suggests that there is an individual component of the stakes effects.

The findings regarding the cross-investor heterogeneities in stakes effects shed valuable light on the mechanisms underlying those effects. Given data limitations, we have very little demographic information to analyze, so future studies should examine whether individual characteristics correlated with stakes effects.

5.4 *Overlapping periods and non-overlapping periods*

In our main tests, we restrict our sample to periods when an investor holds both real and simulated accounts. This design provides a within-subject and a within-the-same-period estimate of the stakes effect. Our within-subject estimates avoid the impact of investor heterogeneities. The choice of the same period enables us to control for time-varying effects. The co-existence of both types of accounts may also, however, affect investor behaviors. For example, evidence shows that ambiguity aversion exists mainly when a more ambiguous choice and a less ambiguous choice co-exist (Fox and Tversky, 1995). Recall that our results are robust to a between-subject design (see Section E of the Appendix).

To further examine the co-existence effect, we take advantage of the fact that the real and simulated accounts are not opened at the same time and compare the biases between the overlapping periods and the non-overlapping periods, i.e., periods when an investor has not opened a second type of account. Such an estimate remains a within-subject estimate but it is calculated across periods. Most (3,807 out of 4,413) of the investors opened their simulated accounts first, with only 606 having opened their real accounts first. For the latter, most opened their real accounts not long before they opened their simulated accounts.

We exclude investors whose non-overlapping periods are shorter than 90 days. As is the case with our main results, such a requirement ensures that we can obtain meaningful estimates of the bias/performance measures. Differing from the main test in which we require a minimum 365-day window, however, here we use a 90-day window. Whether we require a 180-day or a 365-day window, the results derived by comparing the simulated accounts between non-overlapping and overlapping periods are similar. If we require a 180-day window, however, the sample for the real-account comparison will be roughly halved and become too small for a meaningful statistical comparison. With the 90-day requirement, we obtain 2,538 investors for the simulated-account comparison and 203 investors for the real-account comparison.

We report the results in Table 7. In Panel A, we compare the simulated accounts between non-overlapping and overlapping periods. In Panel B, we compare the real accounts. The existence of an account of the other type is associated with weaker biases in both simulated accounts and real accounts. For simulated accounts, the differences are statistically significant except for those pertaining to local bias. For real accounts, the differences are statistically significant for lottery preference, underdiversification, and turnover, perhaps reflecting the smaller sample. The existence of an account of the other type is associated with superior portfolio performance for both simulated accounts and real accounts.

These results also shed doubt on the above-discussed mechanism that investors use simulated accounts as “watch lists.” If simulated accounts are used as “watch lists,” we shall expect that having a simulated account strengthens the selection of lottery stocks and stocks with high past returns, leading to stronger lottery preferences and extrapolation. The results in Panel B are contradictory to these predictions.

5.5 *Stakes, attention, and emotion*

An often implicit assumption in discussions of the stakes–behaviors relationship is that raising the stakes will lead to greater attention and greater effort. This section presents analyses of whether and how stakes affect investor attention. We also present a preliminary analysis of whether and how stakes affect investor emotions.

Recall our findings that investors have a higher turnover in real accounts than in simulated accounts. Trading volume is a widely used proxy for attention (Barber and Odean, 2008). To the extent that trading volume is a valid proxy for investor attention, the results pertaining to turnover are consistent with the idea that higher stakes are associated with greater attention.

We conduct additional analyses by taking advantage of the feature in virtue of which Snowball is a social platform that allows investors to write posts. If investors pay more attention to their real accounts, we expect them to post more frequently on stocks in their real accounts than on stocks in their simulated accounts.

In Table 8, we report results pertaining to the probability that an investor writes a post within the next three days about a stock he/she has just traded.[21] We report the results for buying and selling separately. In real accounts, the probabilities of posting are 2.73% and 1.86% for buying and selling, respectively. In simulated accounts, the probabilities are 0.78% and 0.68%, respectively. The real–simulated differences are highly statistically significant.

In the next three rows of Table 8, we report the probability of posting conditional on selling a winner, selling a loser, and the winner–loser differences. Investors are significantly more likely to broadcast their investment successes than their failures. The winner–loser differences are 0.93% and

[21] The results are similar if we vary the window to measure posting activities to one day or seven days. Section G of the Appendix presents these robustness results.

0.38% for real and simulated accounts, respectively. The positive winner–loser differences are consistent with the presence of self-enhancing transmission bias (Hirshleifer, 2020; Han, Hirshleifer, and Walden, 2022; Sui and Wang, 2022). Interestingly, the winner–loser posting difference is larger in the real accounts than in the simulated accounts, suggesting a stronger self-enhancing transmission bias when trading for real money.

For many individuals, writing posts on social media is a means of expressing emotions. Posting more frequently on stocks in their real accounts than in their simulated accounts also indicates higher emotional involvement when trading for real money than when trading in the simulated setting. The finding that investors exhibit stronger self-enhancing transmission bias is also consistent with higher emotional involvement in real accounts.

6. Results based on two brokerage data sets

In this section, we present analyses using two brokerage account datasets. We collect one from a large US discount brokerage company that has been widely used in retail investor studies (Barber and Odean, 2000). The raw data include trading activity for roughly 78,000 households between January 1999 and November 1996. We collect a similar dataset for a sample period that runs from 2000 through 2009 from a large Chinese brokerage company (Frydman and Wang, 2020; An et al., 2022).

In this setting, we measure stakes by a position's portfolio weight and compare the disposition effect between different positions. If stakes strengthen investor biases, we expect to find a stronger disposition effect on positions with higher portfolio weight. As our empirical model, we use Equation (2),

$$Sell_{i,j,t} = \beta_1 Gain_{i,j,t-1} + \beta_2 Weight_{i,j,t-1} + \beta_3 Gain_{i,j,t-1} * Weight_{i,j,t-1} + controls + \varepsilon_{i,j,t}, \quad (2)$$

where $Sell_{i,j,t}$ and $Gain_{i,j,t-1}$ are defined in the same way as in Section 4.1.1. $Weight_{i,j,t-1}$ is position j's value as a percentage of the total portfolio value at the end of day t-1. The value of position j at the end of day t-1, if measured using the price at t-1, will be mechanically correlated with the

Gain dummy. To avoid such confoundedness, we measure the value of a position at the end of day *t*-1 by the product of the number of shares at *t*-1 and the *purchase* price instead of the price at *t*-1.[22]

We report the results in Table 9. For the US data (Panel A) and the Chinese data (Panel B), we estimate Equation (2) in six specifications using different fixed effects. The results reported in column (1) are obtained with no fixed effects. In columns (2)–(4), we include investor fixed effects, stock fixed effects, and date fixed effects, respectively. In column (5), we use all three sets of fixed effects. In column (6), we use the most stringent specification, which includes stock fixed effects, date fixed effects, and investor*gain fixed effects. We include investor*gain fixed effects to control for heterogeneities in the disposition effect across investors, enabling us to estimate the within-investor effects of portfolio weights.

In all the specifications, the coefficients of *Gain*PortWeight* are positive and highly statistically significant. The lowest t-statistic is above 7. The economic effect is also sizable. Based on the estimates reported in column (5), when *PortWeight* increases from 25% to 75%, the disposition effect increases from 0.20 to 0.32 in the US data and from 3.24 to 3.88 in the Chinese data. These changes represent a 60% increase and a 20% increase, respectively.

Overall, these results show that investors exhibit a stronger disposition effect with regard to positions with higher portfolio weights, consistent with our results based on the Snowball data indicating that higher stakes are associated with stronger biases. The results based on the two brokerage datasets provide external validity, showing that the higher-stakes-stronger-biases results are not restricted to data from the online investor platform.

7. Conclusions

The headline finding of our paper is that investors exhibit stronger biases and worse performance in higher-stakes real accounts than in lower-stakes simulated accounts. The results hold for both the within-subject design and the between-subject design. These results support the notion

[22] Calculating *PortWeight* using the Snowball data is difficult. As discussed in Section 2.2, we have data only on the fraction of trades at trade time, not at the end of every day. Inferring the portfolio weights at the end of each day requires information on stock price changes since the last trade as well as cash inflows and outflows. Unfortunately, data on cash outflows and inflows are not available. In Section H of the Appendix, we measure *PortWeight* with the *PortWeight* at the time a position was established. The coefficient of *Gain*PortWeight* is significantly positive in the Snowball data, which is consistent with the results based on the two brokerage company datasets.

that stakes affect behaviors. In contrast to the conventional wisdom that higher stakes should weaken biases, our evidence suggests that higher stakes can strengthen biases. We also find investors exhibit strong biases and perform worse in their simulated accounts even though these accounts do not involve real money. Across investors, biases are strongly correlated between the two types of accounts, suggesting behavioral consistency. These findings suggest that the low-stakes lab methods, although imperfect, can still be informative about real-world human behaviors.

Our results indicate that attention (and possibly also emotional involvement) should be considered when examining stakes effects. Evidence from investor trading and social media posting activities shows that investors pay greater attention to their real accounts than their simulated ones. Opening a real (simulated) account predicts weakening biases in an existing simulated (real) account, perhaps because attention is partially reallocated from the existing account to the new account.

The expectation that higher stakes will affect behaviors rests on two assumptions. First, raising the stakes leads to greater attention and effort. Second, such an increase in attention and effort can affect behaviors. Overall, our findings imply that the marginal effect of attention is negative in the stock market. Perhaps investors carry incorrect mental models and greater attention impairs decision-making (Dawes, 1979; Dawes, Faust, and Meehl, 1989). It is also possible that investors have non-traditional preferences, such as prospect theory, and higher stakes lead to behaviors that conform to investors' preferences more closely but deviate further from traditional benchmarks. Higher stakes can also strengthen the elements of non-traditional preferences, as Rottenstreich and Hsee (2001) and Hsee and Rottenstreich (2004) document for prospect theory. Disentangling these alternative interpretations is of great importance, suggesting a potentially fruitful research endeavor for the future.

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Figure 1. Graphic views of differences between real and simulated accounts

This figure presents the estimated kernel densities for behavioral biases in real and simulated accounts. The red lines are for real accounts and the blue lines for simulated accounts. Each plot uses a Gaussian kernel. The x-axis of turnover is the log scale.

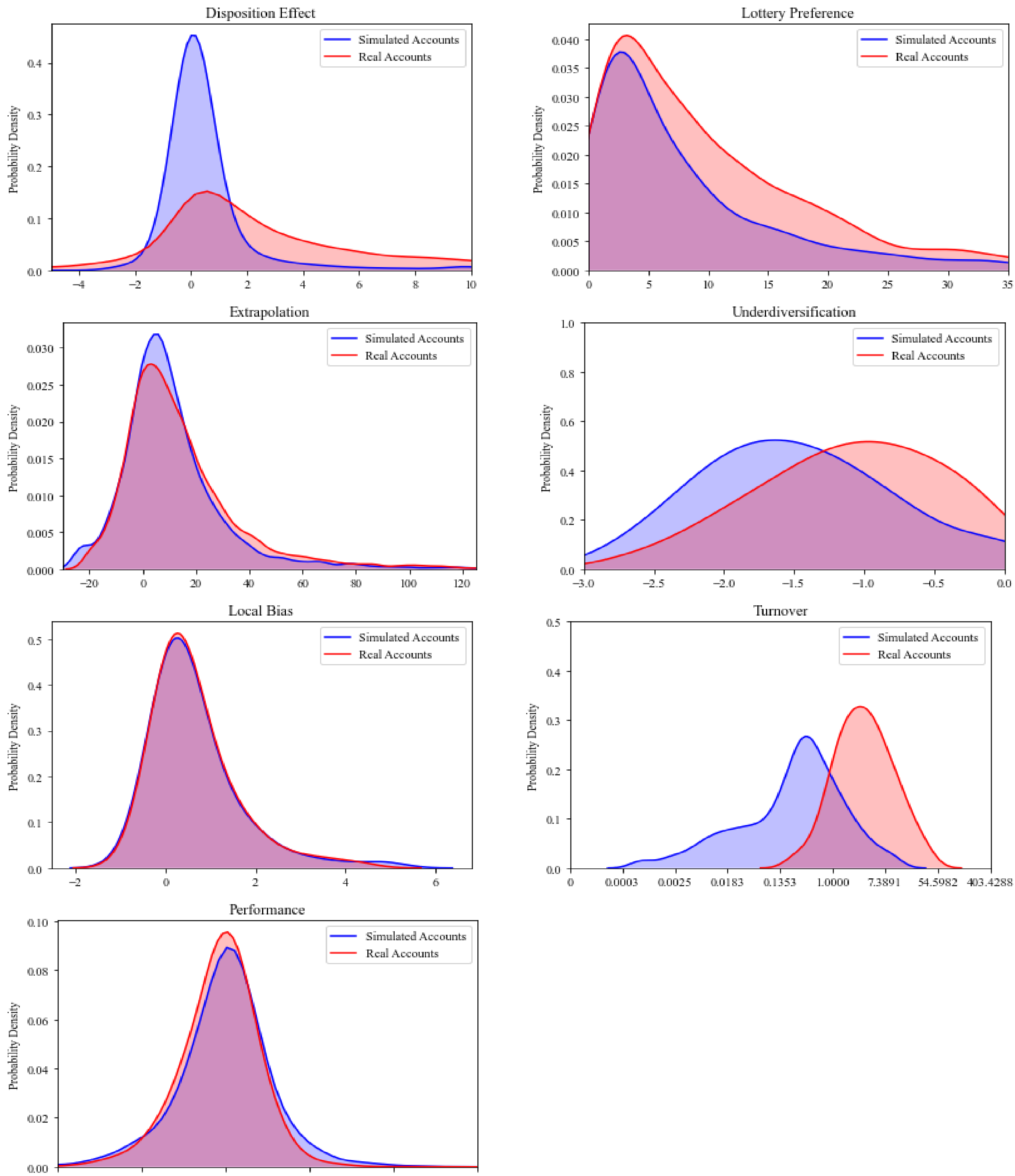

Table 1. Summary Statistics

In this table, we report summary statistics for the sample investors. These investors include all those whose real accounts and simulated accounts overlap for more than a year. We report summary statistics for these investors for the overlapping period. *Active period* is the length of an active period in days. *Number of trades* is the total number of trades in the overlapping period. We report the total numbers (N), means, standard deviations (STD), the first quartiles (Q1), the medians, and the third quartiles (Q3).

	N	Mean	STD	Q1	Median	Q3
Active period	4,413	667.867	197.555	491.000	642.000	833.000
Number of trades in real accounts	4,413	99.661	62.729	43.000	91.000	160.000
Number of trades in simulated accounts	4,413	39.772	158.106	6.429	14.000	33.667

Table 2. Summary statistics for biases

In this table, we report the mean, standard deviation (STD), Q1 (the first quartile), median, and Q3 (the third quartile) for each type of bias. We also report *t*-statistics for and Wilcoxon *p*-values of the investor-level bias measures. See Section 4.1 for the bias definitions. All biases are calculated at the individual investor level and separately for real accounts and simulated accounts. We winsorize each measure separately for the real accounts and the simulated accounts at the 1% level and the 99% level.

Panel A. Real accounts

	N	Mean	STD	Q1	Median	Q3	*t*	Wilcoxon *p*
Disposition effect	4,371	4.163	7.532	0.160	1.868	5.908	(36.54)	0.000
Lottery preference	4,369	6.491	9.130	0.000	2.495	9.708	(46.99)	0.000
Extrapolation	4,373	15.599	26.191	0.121	9.224	22.374	(39.39)	0.000
Underdiversification	4,394	-1.086	0.657	-1.536	-1.041	-0.580	(-109.61)	0.000
Local Bias	1,739	0.630	0.880	0.055	0.366	0.907	(29.84)	0.000
Turnover	4,386	5.960	7.382	1.501	3.180	7.166	(53.47)	0.000
Performance	4,375	-0.656	4.685	-3.181	-0.285	2.174	(-9.27)	0.000

Panel B. Simulated accounts

	N	Mean	STD	Q1	Median	Q3	*t*	Wilcoxon *p*
Disposition effect	4,371	0.489	1.575	0.000	0.000	0.353	(20.54)	0.000
Lottery preference	4,369	4.264	8.792	0.000	0.068	4.441	(32.06)	0.000
Extrapolation	4,373	12.660	25.200	-0.199	7.403	18.028	(33.22)	0.000
Underdiversification	4,394	-1.473	0.688	-1.967	-1.539	-1.073	(-141.96)	0.000
Local Bias	1,739	0.645	0.970	0.048	0.354	0.914	(27.74)	0.000
Turnover	4,386	0.839	1.624	0.070	0.304	0.784	(34.22)	0.000
Performance	4,375	-0.148	5.921	-2.858	0.219	2.936	(-1.65)	0.020

Table 3. Correlation coefficients of biases

In this table, we report the pairwise correlation coefficients between behavioral biases: Panel A lists the correlation coefficients of real accounts and Panel B lists the correlation coefficients of simulated accounts. See Section 4.1 for the bias definitions. Reported in parentheses are the *p*-values of the correlation coefficients.

Panel A. Real accounts

	Disposition effect	Lottery preference	Extrapolation	Underdiversification	Local bias	Turnover	Performance
Disposition effect	1						
Lottery preference	0.213	1					
	(0.000)						
Extrapolation	0.115	0.157	1				
	(0.000)	(0.000)					
Underdiversification	0.237	0.211	0.117	1			
	(0.000)	(0.000)	(0.000)				
Local bias	0.050	-0.068	-0.044	-0.007	1		
	(0.039)	(0.005)	(0.067)	(0.774)			
Turnover	0.306	0.234	0.269	0.368	-0.021	1	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.390)		
Performance	-0.209	-0.260	-0.259	-0.180	-0.003	-0.229	1
	(0.000)	(0.000)	(0.000)	(0.000)	(0.915)	(0.000)	

Panel B. Simulated accounts

	Disposition effect	Lottery preference	Extrapolation	Underdiversification	Local bias	Turnover	Performance
Disposition effect	1						
Lottery preference	0.119	1					
	(0.000)						
Extrapolation	0.137	0.045	1				
	(0.000)	(0.003)					
Underdiversification	0.104	0.063	0.042	1			
	(0.000)	(0.000)	(0.006)				
Local bias	-0.028	0.036	-0.030	0.048	1		
	(0.250)	(0.131)	(0.209)	(0.045)			
Turnover	0.413	0.124	0.230	0.052	-0.027	1	
	(0.000)	(0.000)	(0.000)	(0.001)	(0.268)		
Performance	-0.093	-0.099	-0.129	-0.094	-0.041	-0.082	1
	(0.000)	(0.000)	(0.000)	(0.000)	(0.088)	(0.000)	

Table 4. Differences between the real and simulated accounts

In this table, we report differences between real and simulated accounts. See Section 4.1 for the bias definitions. For each type of bias, we report the mean of the real accounts, the mean of the simulated accounts, and the mean of the differences between them. For the differences, we report *t*-statistics and Wilcoxon *p*-values. In column "N," we report the number of investors.

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	4,371	4.163	0.489	3.674	(32.43)	0.000
Lottery preference	4,369	6.491	4.264	2.228	(13.88)	0.000
Extrapolation	4,373	15.599	12.66	2.939	(6.49)	0.006
Underdiversification	4,394	-1.086	-1.473	0.386	(32.67)	0.000
Local Bias	1,739	0.630	0.645	-0.015	(-0.64)	0.810
Turnover	4,386	5.960	0.839	5.121	(47.44)	0.000
Performance	4,375	-0.656	-0.148	-0.509	(-4.95)	0.001

Table 5. Correlations between real and simulated accounts

In this table, we report how biases are correlated between real and simulated accounts. In the "Correlation" column, we report the correlation coefficients and their *p*-values between a type of bias in an investor's real account and the same type of bias in her/his simulated account. In the next five columns, we report the mean of each bias measure by quintiles sorted by the value of the bias in an investor's real account. In the High–Low column, we report the mean differences between the high and low groups as well as the *t*-statistics (in parentheses).

	Correlation		Low 1	2	3	4	High 5	High–Low	
	Coefficient	*p*						Mean	*t*-stat
Disposition effect	0.132	0.000							
Real accounts			-2.434	0.454	1.903	4.859	16.041	18.476	(62.00)
Simulated accounts			0.332	0.176	0.407	0.609	0.923	0.591	(6.62)
Lottery preference	0.300	0.000							
Real accounts			0.000	0.042	2.511	8.069	21.829	21.829	(75.26)
Simulated accounts			2.090	2.146	3.121	5.276	8.684	6.594	(14.03)
Extrapolation	0.320	0.000							
Real accounts			-7.638	1.964	9.338	19.306	55.014	62.652	(56.58)
Simulated accounts			3.478	7.507	11.035	16.170	25.110	21.631	(15.46)
Underdiversification	0.321	0.000							
Real accounts			-2.067	-1.432	-1.044	-0.671	-0.217	1.851	(168.35)
Simulated accounts			-1.797	-1.612	-1.421	-1.355	-1.177	0.620	(19.30)
Local bias	0.444	0.000							
Real accounts			-0.175	0.111	0.371	0.775	2.067	2.242	(48.03)
Simulated accounts			0.280	0.329	0.491	0.775	1.349	1.069	(13.21)
Turnover	0.251	0.000							
Real accounts			0.802	1.788	3.258	6.130	17.829	17.028	(57.03)
Simulated accounts			0.351	0.536	0.732	0.996	1.582	1.231	(13.89)
Performance	0.195	0.000							
Real accounts			-7.407	-2.524	-0.322	1.669	5.301	12.708	(87.78)
Simulated accounts			-2.010	-0.679	-0.268	0.727	1.491	3.501	(11.59)

Table 6. The effects of stakes across investors

We first calculate the difference between real and simulated accounts for each bias. In this table, we report the correlation coefficients between these differences across investors. The *p*-values of the correlation coefficients are reported in parentheses.

	Disposition effect	Lottery preference	Extrapolation	Underdiversification	Local bias	Turnover	Performance
Disposition effect	1						
Lottery preference	0.126	1					
	(0.000)						
Extrapolation	0.131	0.089	1				
	(0.000)	(0.000)					
Underdiversification	0.064	-0.002	-0.016	1			
	(0.000)	(0.897)	(0.305)				
Local bias	0.013	0.016	-0.017	0.030	1		
	(0.597)	(0.505)	(0.471)	(0.218)			
Turnover	0.296	0.096	0.187	0.125	0.008	1	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.728)		
Performance	-0.098	-0.077	-0.159	-0.034	-0.022	-0.086	1
	(0.000)	(0.000)	(0.000)	(0.025)	(0.367)	(0.000)	

Table 7. Differences between overlapping periods and non-overlapping periods

In this table, we report the results of comparisons between the overlapping periods and the non-overlapping periods for simulated accounts (Panel A) and real accounts (Panel B). An overlapping period occurs when an investor holds both types of accounts. A non-overlapping period occurs when an investor holds only one type of account. We require non-overlapping periods to extend at least 90 days.

Panel A. Simulated accounts

		Non-Overlapping	Overlapping	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	2,491	0.573	0.446	0.127	(2.33)	0.002
Lottery preference	2,422	4.466	3.853	0.613	(2.80)	0.302
Extrapolation	2,429	16.155	10.348	5.806	(8.69)	0.000
Underdiversification	2,505	-1.411	-1.495	0.084	(9.11)	0.000
Local Bias	1,068	0.617	0.625	-0.008	(-0.26)	0.168
Turnover	2,443	2.326	0.846	1.481	(17.18)	0.000
Performance	2,435	0.369	0.004	0.365	(2.05)	0.209

Panel B. Real accounts

		Non-Overlapping	Overlapping	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	205	2.650	2.294	0.356	(0.47)	0.141
Lottery preference	206	7.384	5.157	2.228	(2.15)	0.187
Extrapolation	206	15.473	13.851	1.622	(0.80)	0.444
Underdiversification	208	-0.886	-1.188	0.302	(7.63)	0.000
Local Bias	58	0.746	0.699	0.047	(0.36)	0.694
Turnover	208	7.640	3.599	4.041	(6.25)	0.000
Performance	206	0.732	-0.602	1.334	(2.31)	0.043

Table 8. Postings

In this table, we report results indicating the likelihood (in %) that an investor writes posts about his/her transaction on Snowball in the three days following a transaction. Each row corresponds to a different transaction type: buying, selling, selling winners, and selling losers. In the last row, we also report the difference between selling winners and selling losers. The analyses for real and simulated accounts are conducted separately. For each type of transaction, we report the mean value of real accounts, the mean value of simulated accounts, and the mean value of the differences between them. For the differences, we report *t*-statistics and Wilcoxon *p*-values. In column "N," we report the number of investors.

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Buying	4,409	3.741	1.140	2.607	(21.76)	0.000
Selling	4,400	2.413	1.084	1.577	(10.64)	0.000
Selling winners	4,337	3.093	1.271	2.203	(11.32)	0.000
Selling losers	4,306	1.763	0.724	1.303	(8.61)	0.000
Selling winners – Selling losers	4,243	1.332	0.531	0.948	(4.96)	0.000

Table 9. Evidence from two brokerage datasets

This table presents the pooled regression results of the disposition effect analysis. Specifically, we report the regression results of the following model,

$$Sell_{i,j,t} = \beta_1 Gain_{i,j,t-1} + \beta_2 PortWeight_{i,j} + \beta_3 Gain_{i,j,t-1} * PortWeight_{i,j} + controls + \varepsilon_{i,j,t}$$

where observations occur at the investor (*i*), position (*j*), and date (*t*) levels. $Sell_{i,j,t}$ is a dummy variable that equals 1 if investor *i* sells position *j* (partially or fully) on day *t*. $Gain_{i,j,t\text{-}1}$ is a dummy variable that equals 1 if investor *i* experiences a gain on stock *j* at the end of day *t*-1. $PortWeight_{i,j}$ is the portfolio weight of position *j* in investor *i*'s portfolio on day *t*. We measure the value of a position using the purchase price of a stock instead of the current price. We add three control variables, following Ben-David and Hirshleifer (2012): the log purchase price, the square root of the number of days since purchase, and the stock volatility calculated using daily returns during the 250 days preceding the purchase. We cluster standard errors at the investor, stock, and day levels. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. US

	(1)	(2)	(3)	(4)	(5)	(6)
Gain	0.113***	0.128***	0.135***	0.100***	0.135***	
	(16.76)	(15.15)	(17.80)	(15.54)	(16.07)	
PortWeight	0.006	-0.102***	-0.010	0.008	-0.159***	-0.093***
	(0.56)	(-7.29)	(-0.92)	(0.81)	(-11.96)	(-8.87)
Gain*PortWeight	0.116***	0.233***	0.146***	0.110***	0.250***	0.158***
	(7.69)	(11.50)	(9.81)	(7.44)	(12.09)	(10.43)
Log(Price)	0.005	0.002	0.047***	0.004	0.042***	0.028***
	(1.27)	(0.52)	(4.77)	(1.11)	(6.30)	(3.71)
Sqrt(Time owned)	-0.019***	-0.006***	-0.017***	-0.019***	-0.002***	0.000
	(-27.94)	(-14.44)	(-26.13)	(-25.95)	(-5.88)	(1.01)
Volatility	2.551***	0.144	1.203***	2.506***	0.064	0.074
	(9.31)	(0.86)	(4.87)	(9.39)	(0.32)	(0.34)
Investor FE	No	Yes	No	No	Yes	No
Stock FE	No	No	Yes	No	Yes	Yes
Date FE	No	No	No	Yes	Yes	Yes
Investor*Gain FE	No	No	No	No	No	Yes
Adj-R^2	0.001	0.012	0.002	0.002	0.013	0.016
Obs.	118,500,332	11,8500,331	11,8500,322	11,8500,332	118,500,316	11,8499,114

Panel B. China

	(1)	(2)	(3)	(4)	(5)	(6)
Gain	3.487***	3.711***	3.494***	2.683***	2.925***	
	(52.80)	(57.92)	(51.74)	(45.21)	(56.26)	
PortWeight	1.376***	1.190***	1.404***	1.138***	1.073***	1.240***
	(32.75)	(25.78)	(34.55)	(26.67)	(23.79)	(28.41)
Gain*PortWeight	1.641***	1.410***	1.617***	1.512***	1.274***	0.646***
	(16.06)	(15.43)	(15.83)	(15.53)	(14.51)	(7.32)
Log(Price)	0.288***	0.175***	0.322***	0.642***	0.797***	0.427***
	(6.53)	(4.75)	(5.78)	(13.60)	(18.97)	(11.67)
Sqrt(Time owned)	-0.220***	-0.136***	-0.220***	-0.241***	-0.170***	-0.161***
	(-52.86)	(-44.79)	(-52.27)	(-56.11)	(-54.80)	(-52.87)
Volatility	1.141**	0.570	0.019	1.697***	0.329*	0.471**
	(2.17)	(1.20)	(0.04)	(4.57)	(1.65)	(2.57)
Investor FE	No	Yes	No	No	Yes	No
Stock FE	No	No	Yes	No	Yes	Yes
Date FE	No	No	No	Yes	Yes	Yes
Investor*Gain FE	No	No	No	No	No	Yes
Adj-R^2	0.027	0.053	0.028	0.034	0.060	0.067
Obs.	80,443,217	80,443,217	80,443,212	80,443,217	80,443,212	80,441,574

Appendix

Section A. Alternative measures

In this section, we introduce alternative measures of the biases and in Table A1, we report the results of robustness tests based on these alternative measures. The Table A1 results are qualitatively similar to those reported in Table 4: relative to simulated accounts, behavioral biases (except local bias) are stronger in real accounts and performance is worse in real accounts.

A. The disposition effect

One weakness of the study's disposition effect measure is that it is mechanically correlated with an investor's turnover (Feng and Seasholes, 2005). Imagine two investors, Alex and Bob. On any given day, Alex's propensity to sell a winning stock (PGR) is 2%, and his propensity to sell a losing stock (PLR) is 1%. Suppose that the likelihood that Bob sells both his winning and losing stocks is twice the likelihood that Alex does so on any given day. In other words, Bob's PGR is 4% and PLR is 2%. Based on Equation (1), the disposition effect of Bob is stronger (2%) than that of Alex (1%).

To address this concern, we adopt an alternative method, following Seru, Shumway, and Stoffman (2010), and estimate a proportional hazard model,

$$h_{i,j}(t) = \phi_i(t)\exp\left(\gamma_i Gain_{i,j,t-1} + \text{controls} + \text{error}\right),$$

where $h_{i,j}(t)$ is the probability that investor *i* sells position *j* on date *t*, conditional on not having sold that position prior to date *t*. $Gain_{i,j,t\text{-}1}$ is defined in the same way as in Equation (1). Therefore, $\exp(\gamma_i)$ measure the PGR/PLR. Moreover, following Seru, Shumway, and Stoffman (2010), we add three additional controls—five-day moving averages of market returns, squared market returns, and market trading volume—to our Cox regression to control for market-driven confounding factors that may contaminate our results.

B. Lottery preference

In the paper, we construct our lottery measure based on three stock characteristics: price per share, idiosyncratic volatility, and idiosyncratic skewness. We use these three characteristics as alternative measures for lottery preference.

C. Extrapolation

In the paper, we measure extrapolation as returns in excess of the market over the past 126 trading days. As an alternative measure, we calculate size-decile adjusted returns over the preceding 252 trading days.

D. Underdiversification

In the paper, we measure underdiversification as the negative of the natural logarithm of the number of stocks in a given account. If an investor holds multiple simulated accounts, we calculate the average of the number of stocks across those simulated accounts before we take the natural logarithm. In the alternative measure, we treat all the simulated accounts as one and calculate the total number of stocks from all the simulated accounts.

E. Local bias

Alternatively, we measure local bias using the excess weight with which an investor buys stocks in his/her own province or prefecture cities. The benchmarks are the market capitalization of local stocks (for firms located in either the same province or the same prefecture city) as a fraction of the market capitalization of all the listed stocks.

F. Turnover

We do not consider any alternative measures of turnover.

G. Investor performance

In the paper, we measure investor performance over the following two months after purchases. As alternative measures, we measure investor performance over the following one, three, or six months.

Section B. Pooled regressions for the disposition effect analysis

The individual-level disposition effect estimates may be noisy. As an alternative, we estimate the disposition effect by pooling all real accounts or all simulated accounts together. Specifically, we estimate the following model,

$$Sell_{i,j,t} = \beta_1 Gain_{i,j,t-1} + \beta_2 Real_{i,j} + \beta_3 Gain_{i,j,t-1} * Real_{i,j} + controls + \varepsilon_{i,j,t}$$

where observations occur at the investor (i), position (j), and date (t) level. $Sell_{i,j,t}$ is a dummy variable that equals 1 if investor i sells position j (partially or fully) on day t. $Gain_{i,j,t\text{-}1}$ is a dummy variable that equals 1 if investor i experiences a gain on stock j at the end of day t-1. $Real_{i,j}$ is a dummy variable that equals 1 if position j is in a real account and 0 otherwise.

In addition to estimating a linear probability model that includes the pooled data, we also estimate a hazard model,

$$h_{i,j}(t) = \phi(t)\exp\left(\beta_1 Gain_{i,j,t-1} + \beta_2 Real_{i,j} + \beta_3 Gain_{i,j,t-1} * Real_{i,j} + controls + \vartheta_{i,j,t}\right),$$

where $h_{i,j}(t)$ is the probability that investor i sells position j on date t, conditional on not having sold that position prior to date t. Other variables are defined as they are in the linear probability model.

In Table A2, we report the linear probability model results, and in Table A3, we report the hazard model results. We multiply the coefficients listed in Table A2 by 100. The results indicate that, in both specifications, β_3 is consistently positive. The results are also robust across various high-dimensional fixed-effect specifications or stratification specifications.

Section C. Investors with one simulated account

Among the sample investors, 3,332 hold multiple simulated accounts. In Table A4, we report our main results based on investors who hold only one simulated account. There are 1,081 such investors. The results are qualitatively similar to those reported in the main text.

Section D. Data truncation

If an investor has traded more than 200 times in real accounts, Snowball keeps only the most recent 200 trades. In Table A5, we re-run the analysis to address this truncation issue. In Panel A, we report the results using the sample where we include only investors who executed fewer than 200 trades. In Panel B, we report the results using data for the year before we collect the data. The results reported in both panels are qualitatively similar to the main results.

Section E. Between-subject designs

In Table A6, we examine the stakes effects using a between-subject design. For real accounts, we focus on users who do not hold any simulated accounts. For simulated accounts, we use the period before an investor opens his/her real account. We require the account to be held for at least one year. Another difference between this table and Table 4 is that, for Table 4, we conduct paired statistical tests for the differences, while in this table the tests are not paired. The results are qualitatively similar to the results reported in Table 4.

Section F. Using simulated accounts as a "watch list"

In Table A7, we examine.

Section G. Postings—varying windows

In Table A8, we report the results of postings for varying windows. In Table 8, we measure postings within the three days following a transaction. In Table A7, we measure postings on the day following a transaction (Panel A) or within the seven days following a transaction (Panel B). The results are qualitatively similar to those reported in Table 8.

Section H. Portfolio weight and the disposition effect in the Snowball data

In Table A9, we examine how a position's portfolio weight is associated with the disposition effect using the Snowball data. The coefficient of *Gain*PortWeight* is positive, suggesting that the disposition effect is stronger when portfolio weight is higher.

Table A1. Differences between the real and simulated accounts – alternative measures

In this table, we report the differences in biases between investors' real and simulated accounts based on the alternative measures described in Section A of the Appendix.

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect						
Disposition effect (Hazard)	2,098	0.771	0.175	0.948	(3.83)	0.000
Lottery preference						
−Log(Price)	4,369	-2.793	-2.923	0.131	(17.08)	0.000
Idiosyncratic volatility	4,369	0.020	0.019	0.001	(12.29)	0.000
Idiosyncratic skewness	4,369	0.604	0.517	0.087	(13.30)	0.000
Extrapolation						
Extrapolation (252 days)	4,373	26.631	24.461	2.170	(3.55)	0.238
Underdiversification						
−Log(1+Number of stocks all simulated accounts)	4,394	-1.087	-2.183	1.097	(63.46)	0.000
Local Bias						
Adjusted same province ratio	1,748	0.014	0.011	0.003	(0.72)	0.109
Adjusted same prefecture ratio	1,695	-0.006	-0.005	-0.000	(-0.10)	0.145
Performance						
Performance (1m)	4,375	-0.532	-0.098	-0.434	(-5.73)	0.000
Performance (3m)	4,375	-0.630	0.006	-0.636	(-4.95)	0.001
Performance (6m)	4,375	-0.111	0.496	-0.607	(-3.26)	0.016

Table A2. Pooled regressions for the disposition effect analysis – linear probability model

In this table, we report the pooled regression results of the disposition effect analysis based on a linear probability model,

$$Sell_{i,j,t} = \beta_1 Gain_{i,j,t-1} + \beta_2 Real_{i,j} + \beta_3 Gain_{i,j,t-1} * Real_{i,j} + controls + \varepsilon_{i,j,t},$$

where observations occur at the investor (*i*), position (*j*), and date (*t*) levels. $Sell_{i,j,t}$ is a dummy variable that equals 1 if investor *i* sells position *j* (partially or fully) on day *t*. $Gain_{i,j,t-1}$ is a dummy variable that equals 1 if investor *i* experiences a gain on stock *j* at the end of day *t*-1. $Real_{i,j}$ is a dummy variable that equals 1 if position *j* is in a real account and 0 otherwise. We add three control variables, following Ben-David and Hirshleifer (2012): the log purchase price, the square root of the number of days since purchase, and stock volatility calculated using daily returns over the 250 days preceding the purchase. We cluster standard errors at the investor, stock, and day levels. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Real	1.071***	1.612***	1.041***	1.061***	1.613***	1.649***
	(7.56)	(20.88)	(8.07)	(7.69)	(20.84)	(20.82)
Gain	0.413***	0.545***	0.518***	0.380***	0.540***	
	(8.25)	(7.56)	(7.01)	(9.33)	(8.28)	
Gain*Real	1.019***	1.022***	1.016***	0.981***	0.963***	0.890***
	(6.58)	(6.40)	(6.43)	(6.46)	(6.09)	(6.28)
In(Price)	-0.114***	-0.067***	-0.103	-0.123***	-0.023	-0.168**
	(-3.82)	(-2.90)	(-1.27)	(-4.31)	(-0.20)	(-2.08)
Sqrt(Time Owned)	-0.140***	-0.104***	-0.132***	-0.147***	-0.109***	-0.104***
	(-14.66)	(-15.08)	(-15.04)	(-15.76)	(-17.31)	(-17.48)
Volatility	1.277***	0.846***	0.589**	1.397***	0.779***	0.770***
	(5.74)	(5.62)	(2.35)	(6.36)	(3.70)	(3.66)
Constant	2.587***					
	(10.04)					
Investor FE	No	Yes	No	No	Yes	No
Stock FE	No	No	Yes	No	Yes	Yes
Date FE	No	No	No	Yes	Yes	Yes
Investor*Gain FE	No	No	No	No	No	Yes
Adj-R^2	0.015	0.049	0.019	0.016	0.053	0.058
Obs.	31,323,672	31,323,672	31,323,667	31,323,672	31,323,667	31,323,663

Table A3. Pooled regressions for the disposition effect analysis – hazard model

In this table, we report the pooled regression results of the disposition effect analysis based on a Cox proportional hazard model,

$$h_{i,j}(t) = \phi(t)\exp\left(\beta_1 Gain_{i,j,t-1} + \beta_2 Real_{i,j} + \beta_3 Gain_{i,j,t-1} * Real_{i,j} + controls + \vartheta_{i,j,t}\right),$$

where observations occur at the investor (i), position (j), and date (t) levels. $h_{i,j}(t)$ is the probability that investor i sells position j on date t, conditional on not having that position sold prior to date t. $Gain_{i,j,t-1}$ is a dummy variable that equals 1 if investor i experiences a gain on stock j at the end of day t-1. $Real_{i,j}$ is a dummy variable that equals 1 if position j is in a real account and 0 otherwise. We add log purchase price and volatility as control variables. We cluster standard errors at the investor level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Gain	0.099***	0.151***	0.118***	0.078***	
	(5.60)	(10.15)	(7.06)	(5.16)	
Real	1.104***	1.182***	0.989***	0.968***	1.175***
	(25.31)	(41.14)	(24.78)	(23.79)	(40.74)
Gain*Real	0.160***	0.194***	0.188***	0.169***	0.186***
	(8.41)	(11.59)	(10.41)	(9.33)	(9.68)
In(Purchase Price)	-0.056***	-0.056***	-0.046*	-0.056***	-0.054***
	(-6.39)	(-9.83)	(-1.88)	(-7.18)	(-9.42)
Volatility	0.151***	0.084***	-1.037***	0.206***	0.087***
	(6.77)	(3.50)	(-14.48)	(9.13)	(3.18)
Investor stratification	No	Yes	No	No	No
Stock stratification	No	No	Yes	No	No
Date stratification	No	No	No	Yes	No
Investor*Gain stratification	No	No	No	No	Yes
Pseudo-R^2	0.013	0.032	0.029	0.022	0.034
Obs.	25,278,983	25,278,983	25,278,983	25,278,983	25,278,983

Table A4. Investors with one simulated account

In this table, we report the differences between real and simulated accounts for the subsample of investors who hold only one simulated account. See Section 4.1 for the bias definitions. For each bias, we report the mean value of real accounts, the mean value of simulated accounts, and the mean value of the differences between them. For the differences, we report *t*-statistics and Wilcoxon *p*-values. In column "N" we report the number of investors.

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	1,071	4.574	0.372	4.202	(17.78)	0.000
Lottery preference	1,067	7.085	3.615	3.470	(9.34)	0.000
Extrapolation	1,069	16.385	10.949	5.436	(5.24)	0.000
Underdiversification	1,081	-1.032	-1.234	0.203	(8.17)	0.000
Local Bias	353	0.661	0.696	-0.035	(-0.55)	0.166
Turnover	1,074	5.941	0.616	5.326	(23.48)	0.000
Performance	1,071	-1.068	0.155	-1.223	(-4.83)	0.001

Table A5. Data truncation issue

In this table, we report the differences between real and simulated accounts for the subsample of investors who execute fewer than 200 trades in their real account (Panel A) and the subsample of investors who were active in the last year of our sample (Panel B). See Section 4.1 for the bias definitions. For each bias, we report the mean value of real accounts, the mean value of simulated accounts, and the mean value of the differences between them. For the differences, we report *t*-statistics and Wilcoxon *p*-values. In column "N" we report the number of investors.

Panel A. Investors who execute fewer than 200 trades in their real accounts

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	3,540	3.780	0.470	3.309	(27.56)	0.000
Lottery preference	3,540	6.430	4.361	2.069	(11.45)	0.000
Extrapolation	3,543	14.722	12.657	2.066	(4.15)	0.330
Underdiversification	3,561	-1.051	-1.461	0.410	(31.23)	0.000
Local Bias	1,421	0.621	0.640	-0.019	(-0.73)	0.671
Turnover	3,555	5.078	0.817	4.262	(41.24)	0.000
Performance	3,544	-0.551	-0.112	-0.439	(-3.87)	0.008

Panel B. Investors in the last year of our sample

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	1,584	4.173	0.286	3.887	(18.67)	0.000
Lottery preference	1,566	4.273	2.421	1.853	(6.87)	0.000
Extrapolation	1,567	8.339	6.400	1.939	(3.27)	0.106
Underdiversification	1,610	-1.250	-1.562	0.313	(15.43)	0.000
Local Bias	640	0.638	0.640	-0.002	(-0.05)	0.286
Turnover	1,589	4.498	0.581	3.917	(24.14)	0.000
Performance	1,570	-1.144	-0.204	-0.924	(-4.15)	0.003

Table A6. Between-subject design

In this table, we report the differences in biases between investors' real and simulated accounts. For real accounts, we focus on users who hold only one real account and hold no simulated accounts. For simulated accounts, we use the period before an investor opens his/her real account. We require the accounts to have been held for at least one year. Another difference between this table and Table 4 is that, for Table 4, we conduct paired statistical tests of the differences, while for this table the tests are not paired.

	Real		Simulated		Difference		
	N	Mean	N	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	3,289	5.435	911	0.713	4.722	(26.44)	0.000
Lottery preference	3,299	9.727	908	5.333	4.394	(12.50)	0.000
Extrapolation	3,300	20.701	909	17.551	3.149	(2.68)	0.000
Underdiversification	3,302	-1.005	911	-1.424	0.419	(17.52)	0.000
Local Bias	591	0.673	415	0.608	0.065	(1.16)	0.973
Turnover	3,301	6.818	909	1.935	4.882	(25.38)	0.000
Performance	3,300	-1.371	909	0.050	-1.421	(-6.56)	0.000

Table A7. Excluding investors who may use simulated accounts as a "watch list"

In this table, we report the differences between real and simulated accounts for a subsample of investors. These investors, in our sample period, have never purchased any stocks for their real accounts, and at the time of the purchase, the stock was in their simulated accounts. See Section 4.1 for the bias definitions. For each bias, we report the mean value of real accounts, the mean value of simulated accounts, and the mean value of the differences between them. For the differences, we report *t*-statistics and Wilcoxon *p*-values. In column "N" we report the number of investors.

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Disposition effect	794	6.761	0.278	6.483	18.201	0.000
Lottery preference	788	9.062	4.736	4.326	8.621	0.000
Extrapolation	790	22.114	8.562	13.552	9.254	0.000
Underdiversification	798	-0.833	-1.054	0.222	7.061	0.000
Local Bias	278	0.576	0.625	-0.049	-0.621	0.509
Turnover	792	7.153	0.343	6.810	21.233	0.000
Performance	792	-2.000	-0.364	-1.635	-5.189	0.000

Table A8. Postings – one-day window or seven-day window

In this table, we report the likelihood (in %) that an investor posts about his/her transaction on Snowball on the day following the transaction (Panel A) or within the seven days following the transaction. Each row displays results for different types of transactions: buying, selling, selling winners, and selling losers. In the last row, we also report the differences between selling winners and selling losers. The analyses are conducted separately for real and simulated accounts. For each type of transaction, we report the mean values of real accounts, the mean values of simulated accounts, and the mean values of the differences between them. For the differences, we report *t*-statistics and Wilcoxon *p*-values. In column "N" we report the number of investors.

Panel A. One-day window

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Buying	4,409	2.731	0.776	1.959	(19.07)	0.000
Selling	4,400	1.860	0.676	1.370	(10.69)	0.000
Selling winners	4,337	2.337	0.771	1.811	(11.20)	0.000
Selling losers	4,306	1.411	0.435	1.216	(9.03)	0.000
Selling winners – Selling losers	4,243	0.926	0.381	0.560	(3.71)	0.000

Panel B. Seven-day window

		Real	Simulated	Difference		
	N	Mean	Mean	Mean	*t*-stat	Wilcoxon *p*
Buying	4,409	4.896	1.620	3.287	(23.35)	0.000
Selling	4,400	3.097	1.470	1.939	(11.62)	0.000
Selling winners	4,337	3.932	1.869	2.564	(11.56)	0.000
Selling losers	4,306	2.222	0.998	1.546	(9.02)	0.000
Selling winners – Selling losers	4,243	1.685	0.871	0.991	(4.46)	0.000

Table A9. The disposition effect and portfolio weights – Snowball data

In this table, we report the pooled regression results of the disposition effect analysis based on a linear probability model,

$$Sell_{i,j,t} = \beta_1 Gain_{i,j,t-1} + \beta_2 PortWeight_{i,j} + \beta_3 Gain_{i,j,t-1} * PortWeight_{i,j,t-1} + controls + \varepsilon_{i,j,t},$$

where observations occur at the investor (i), position (j), and date (t) levels. $Sell_{i,j,t}$ is a dummy variable that equals 1 if investor i sells position j (partially or fully) on day t. $Gain_{i,j,t-1}$ is a dummy variable that equals 1 if investor i experiences a gain on stock j at the end of day t-1. $PortWeight_{i,j,t-1}$ is the original weight of position j in the account when investor i established position j. We add three control variables, following Ben-David and Hirshleifer (2012): the log purchase price, the square root of the number of days since purchase, and stock volatility calculated using daily returns during the 250 days preceding the purchase. We cluster standard errors at the investor, stock, and day levels. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Gain	1.122***	1.500***	1.498***	0.949***	1.353***	
	(12.37)	(18.34)	(16.00)	(11.03)	(18.73)	
PortWeight	3.558***	-1.377***	3.434***	3.808***	-1.035***	-0.009
	(15.62)	(-6.62)	(16.56)	(17.49)	(-4.85)	(-0.05)
Ratio*PortWeight	1.453***	2.577***	1.334***	1.568***	2.523***	0.438**
	(3.25)	(8.86)	(3.51)	(3.77)	(9.04)	(2.25)
In(Price)	-0.221***	-0.084***	0.985***	-0.203***	0.887***	0.522***
	(-7.08)	(-4.39)	(3.61)	(-6.17)	(5.63)	(3.36)
Sqrt(Time Owned)	-0.424***	-0.197***	-0.352***	-0.477***	-0.194***	-0.159***
	(-22.17)	(-15.73)	(-21.40)	(-24.36)	(-17.18)	(-14.42)
Volatility	3.466***	1.607**	0.228	3.351**	0.109	0.100
	(2.62)	(2.10)	(0.77)	(2.53)	(0.37)	(0.37)
Constant	5.408***					
	(9.60)					
Investor FE	No	Yes	No	No	Yes	No
Stock FE	No	No	Yes	No	Yes	Yes
Date FE	No	No	No	Yes	Yes	Yes
Investor*Gain FE	No	No	No	No	No	Yes
Adj-R^2	0.023	0.072	0.037	0.033	0.086	0.095
Obs.	4,503,258	4,503,236	4,503,238	4,503,258	4,503,216	4,503,198