Learning By Trading

Amit Seru, Tyler Shumway, and Noah Stoffman*

This version: October 5, 2007
First version: March 15, 2006

Abstract

Using a large sample of individual investor records over a nine-year period, we analyze both the disposition effect and trading performance at the individual level to determine whether and how investors learn from their trading experience. We find that performance improves and the disposition effect declines as investors become more experienced, suggesting that investors learn by trading. A substantial part of this learning occurs when investors stop trading after learning about their poor ability rather than staying in the sample and improving their ability over time. By ignoring investor attrition, the existing literature significantly overestimates how quickly investors become better at trading. Our learning estimates suggest that investors do not learn fast enough to prevent behavioral biases from affecting asset prices.

*We thank Brad Barber, Dan Bernhardt, James Choi, Juhani Linnainmaa, Uday Rajan, Mark Seasholes, Morten Sorensen, Ning Zhu, and seminar participants at the 2007 Western Finance Association meeting, the 2007 Wharton Household Portfolio Choice conference, Carnegie Mellon University, the University of California at Irvine, the University of Manchester, the University of Toronto, and the University of Michigan for helpful comments. Any remaining errors are ours. We are grateful to Jussi Keppo for helping us acquire the data used in this study, and to the Mitsui Life Financial Research Center at the University of Michigan for partial funding. Seru (amit.seru@chicagogsbs.edu) is at Chicago GSB, and Shumway (shumway@umich.edu) and Stoffman (stoffman@umich.edu) are at the Ross School of Business at the University of Michigan.
Academics have recently shown interest in the investment behavior and performance of individuals, a field that has been called ‘household finance’ by Campbell (2006). Over the past decade, several researchers have documented a number of behavioral biases among individual investors. More recently, researchers have found evidence that some individual investors are more informed or skilled than others. Considering these findings, it is natural to ask how skilled or informed investors acquire their advantage. For example, do investors learn by trading? If so, to what extent do investors improve their ability and to what extent do they learn about their inherent ability? And how quickly do investors learn? In this paper, we exploit trading records to study both average investor performance and the strength of the behavioral bias known as the disposition effect. We correlate performance and disposition with investor experience and investor survival rates to determine whether and how investors learn by trading.

Motivated by the existing economics literature on learning, we consider two specific ways in which investors can learn. First, in the spirit of classical learning-by-doing models (Arrow 1962, Grossman, Kihlstrom, and Mirman 1977), investors might improve their ability as they trade. Second, as investors trade they might realize that their inherent level of ability is low and decide to stop actively trading. This decision to either continue or stop trading is a feature of the recent learning model of Mahani and Bernhardt (2007). In our analysis we take either of these two scenarios as evidence of learning by trading. Although these types of learning are different, they are not mutually exclusive.

To clarify the model of learning we have in mind, consider the case of an individual who decides to begin trading. The investor must decide which of the myriad sources of market information and investment advice available to her to take seriously. She could consult

---

1 Coval, Hirshleifer, and Shumway (2005) document significant performance persistence among individuals. Ivković and Weisbenner (2005) find that individuals place more informed trades in stocks of companies located close to their homes, and Ivković, Sialm, and Weisbenner (2005) show that individuals with more concentrated portfolios tend to outperform those who are more diversified. Linnainmaa (2007) finds that individuals who trade with limit orders suffer particularly poor performance.

2 The disposition effect is the propensity of investors to sell assets on which they have experienced gains and to hold assets on which they have experienced losses. The effect was first proposed by Shefrin and Statman (1985), and was subsequently documented in a sample of trading records from a U.S. discount brokerage firm by Odean (1998). The effect has been found in other contexts, including in Finland (Grinblatt and Keloharju 2001), China (Feng and Seasholes 2005, Shumway and Wu 2006), and Israel (Shapira and Venezia 2001); among professional market makers (Coval and Shumway 2005), mutual fund managers (Frazzini 2006), and home sellers (Genesove and Mayer 2001); and in experimental settings (Weber and Camerer 1998). We focus on the disposition effect because it is a robust empirical finding and it is relatively easy to measure.
standard news sources, internet sites, investment newsletters, and neighbors or friends. She might also consider the advice of brokers, news analysts, authors of books and magazines, and finance professors. To the extent that these sources fail to agree completely, individuals must determine how much decision weight to assign to each source. Moreover, the quality of these sources is likely to differ across individuals: some investors may know executives at a firm, while others will not. As investors begin trading, they can learn to which of the various sources they should pay more attention. This can be thought of as improving ability through learning. Investors who only have access to poor sources of information cannot improve by focusing more on particular sources. Instead, they will learn that they have no useful information and will stop trading actively, choosing instead to invest in a passive investment such as an index fund.\footnote{This example is very similar in spirit to well-known ‘bandit’ problems. For example, Bolton and Harris (1999) study the strategic interaction of agents in an experimentation game. In our setting, we can think of investors as being randomly assigned slot machines with different unobservable expected payoffs, and experimenting to learn what the payoffs are. The random assignment is analogous to the assignment of inherent ability.} This is also a type of learning, but rather than improving her ability, the investor learns about her ability.

This example helps to elucidate the two types of learning we examine in this paper. First, investors may learn in such a way that their ability improves, perhaps by learning which of their sources of information are particularly useful. We measure this type of learning by correlating investors’ performance and susceptibility to the disposition effect with their experience. Second, investors may learn about their ability, perhaps by learning whether they have access to useful information sources. Because investors who learn that their ability is poor will cease to trade, we identify this type of learning by examining attrition in our data. Once we control for unobserved individual heterogeneity and attrition, any learning that remains will be of the first type, a direct improvement in ability. Since we track individuals over time—both their survival rates as well as their performance and their level of the disposition effect—we are able to differentiate between these two types of learning. Ours is the first paper to identify and measure both types of learning.

Differentiating between types of learning allows us to determine how quickly investors learn in a meaningful way. Estimating learning without controlling for heterogeneity and attrition results in inflated improvement estimates, which do not correspond to the experience of any particular type of investor. Measuring the speed of learning in a meaningful way is important for a number of reasons. If investors learn quickly and there is low turnover in
the population of investors, behavioral biases are unlikely to significantly affect asset prices. Moreover, if they learn relatively quickly then the ‘excessive’ trading documented by Odean (1999) and Barber and Odean (2001) may be justified, because investors may optimally choose to trade more actively if they know they will improve with experience. Finally, policy makers should consider the speed at which individuals learn when determining the costs and benefits of different trading mechanisms.

The type of learning we observe, besides being important from a theory perspective, also has significant implications for policy. For instance, if investors can significantly improve their ability then investor education initiatives may be worthwhile and perhaps individual investors should be encouraged to trade. However, if the ability to invest successfully is relatively fixed, then screening mechanisms, or tests that measure and reveal inherent ability, have more value than education. Finally, thinking about both the speed and the type of learning, has implications for market efficiency. For example, if many inexperienced investors begin trading around the same time, and they learn slowly, their trading could lead to time-varying market efficiency.

We test our hypotheses with a remarkable dataset that includes the complete trading records of investors in Finland from 1995 to 2003, including more than 22 million observations of trades placed by households. We use these data to estimate disposition and calculate performance at the account level. Our disposition estimates indicate that a median individual in our sample is 2.8 times more likely to sell a stock when its price has risen since purchase than when its price has fallen. We exploit the panel structure of our data to examine whether individual investors learn to avoid the disposition effect and improve their performance as they trade. In particular, we estimate the mean return and the disposition effect for each account and year in our sample and relate these estimates to experience, past returns, and various demographic controls.

We measure investing experience with both the number of years that an investor has been trading and with the cumulative number of trades that an investor has placed. Of course, investors may gain experience by actively trading securities and observing the results of each trade. If this is the primary way in which investors learn, then cumulative trades will predict future investment performance and the disposition effect. However, investors may also learn by observing market quantities and considering the outcomes of hypothetical trades based on, for example, a particular information source. If this is the primary way that investors
learn, then years of experience will be a better predictor of investment performance and the disposition effect than cumulative trades.

Our tests provide robust evidence of learning by trading at the individual level. We show that in a simple model of learning, performance improves and the disposition effect declines as investors become more experienced. An extra year of experience is associated with an improvement in average returns of approximately 40 basis points (bp) over a 30-day horizon, and a reduction in the disposition effect of about 5 percent. However, we argue that individual heterogeneity and survivorship effects are likely to significantly affect these simple estimates, making them difficult to interpret.

We adjust for survivorship and heterogeneity with a modified Heckman selection model that allows for individual fixed effects. The Heckman selection model is a two-stage instrumental variables model that adjusts for the possibility that the composition of the sample is endogenous. As instruments, we construct two variables that satisfy the necessary exogeneity conditions—that is, they are likely to affect the probability of an investor remaining in the sample, but are unlikely to affect changes in the investor’s performance or disposition effect except through their effect on survival. The first variable is an indicator for whether the individual inherited shares in the previous year due to the death of a relative. We conjecture that an individual who inherits shares is more likely to trade in the future, perhaps because their wealth has increased, or perhaps because the new shares cause them to pay more attention to the stock market. This satisfies the exogeneity condition since inheritance of shares from a relative is unlikely to directly affect changes in the performance or the disposition effect of an individual. The second variable is the variation in the returns across all positions taken by an account in the previous year. This variation is a measure of the consistency of an investor’s performance, and we conjecture that an investor with more variable performance is more likely to stop trading. While an investor’s previous average performance is likely to be related to her future performance, there is no reason to believe that the consistency of an investor’s past performance should directly affect changes in her future performance or disposition effect.

\footnote{It is also possible, of course, that investors learn by considering the returns to hypothetical trades before they ever start trading. If this is the only way in which investors learn then we will find no evidence of learning by trading.}
In our first-stage estimates we confirm that investors with poor performance are those who are more likely to cease trading. Moreover, more successful investors continue to trade actively. We also find that adjusting for survivorship and heterogeneity in our learning reduces our learning coefficients by about one-half to three-quarters. This suggests that while individuals do learn to improve their trading ability with experience, they primarily learn about their own inherent trading ability and they cease to trade if their ability is low.

Some evidence about learning by trading exists. Feng and Seasholes (2005) give evidence that investors, in aggregate, display significantly less disposition over time, estimating that for sophisticated investors the disposition effect is essentially attenuated after about 16 trades. These estimates do not appear to be consistent with the findings of Frazzini (2006), which shows that mutual fund managers, who trade substantially more than most individuals, display a significant disposition effect. Furthermore, since Feng and Seasholes (2005) do not adjust for heterogeneity and attrition, it is impossible to tell whether their estimates imply that a particular investor who trades 16 times will no longer exhibit the disposition effect or whether they imply that most investors who exhibit the disposition effect will cease trading before they place 16 trades. Nicolosi, Peng, and Zhu (2004) show that the trading performance of individuals appears to improve with trading experience, estimating that individuals can improve their risk-adjusted portfolio return by about two percent per year (or about 0.8 bp per day) over a 3-year period. This seems quite large. Again, it is not possible to tell whether this estimate is driven by the most successful investors surviving or by the least successful investors improving their ability.

While our tests have some features in common with existing papers, they differ from the literature in a number of important respects. First, unlike other papers, our tests use measures of performance and estimates of the disposition effect that are specific to individuals, allowing us to track particular individuals over time. This allows us to control for investor heterogeneity and survivorship effects and allows us to separate the two types of learning as well as to estimate the speed of learning. It also ensures that each observation in almost all of our tests is an average or regression coefficient for one individual in one particular year. Thus, we can be sure that our results hold for the average active investor. Put another way, investors who trade disproportionately often do not receive disproportionate weight in our

---

5Other papers that find some learning in various settings include List (2003), Barber, Odean, and Strahilevitz (2004), Linnainmaa (2006), and Choi, Laibson, Madrian, and Metrick (2007).
estimates. Finally, using only one observation per person per year reduces the likelihood that our standard error estimates are incorrect because of correlation among our regression residuals. Given the unique features of our data and our test methods, the results of our hypothesis tests add significantly to the literature on financial learning.

The rest of the paper is organized as follows. Section 1 describes the hypotheses we test and our statistical methods, while Section 2 provides detail on our data. Section 3 discusses our results and Section 4 concludes.

1 Hypotheses and Methods

We test three hypotheses in this paper. Our first hypothesis (H1) is that individual investors learn by trading. More importantly, we are interested in understanding how investors learn by trading. Our second and third hypotheses clarify the type of learning in H1. Our second hypothesis (H2) is that investors learn about their inherent ability by trading, and our third hypothesis (H3) is that investors learn to improve their ability over time. Of course, if H1 is false, then H2 and H3 will also be false. We test H2 by examining the importance of individual heterogeneity and attrition in our sample. The notion in H2 is that individuals stop trading if they realize their ability is low. Once we control for unobserved individual heterogeneity and attrition, any learning that remains will support H3—that is, it suggests that individuals learn to directly improve their ability. To examine our hypotheses we test a number of related predictions. Our prior belief for each of these predictions is that investors both learn about their inherent ability and learn to improve their ability. This section motivates and describes our tests in more detail. It also describes some of the statistical methods of our tests.

1.1 Measuring performance

Investor performance is the primary variable that we correlate with experience to test our hypotheses. Measuring the performance of individual investors is a significant challenge. Our data, like others that are available, do not include all non-equity securities that may be held by an investor, so it is impossible to measure the return for the investor’s entire portfolio. This is made more difficult by the fact that the amount of money an individual has invested
in equities often fluctuates significantly over time. Since we cannot accurately measure portfolio returns, we measure performance by examining the average return of stocks purchased. However, this generates a new problem—comparing the returns on holding periods of different lengths. For example, it is particularly difficult to compare the performance of one investor who holds a stock for one week and earns a holding period return of 3 percent to that of another investor who holds a stock for one year and earns a holding period return of 15 percent.

Given the challenges associated with calculating performance, we take a straightforward approach that is nevertheless likely to capture much of the relevant information in the individual’s returns. In particular, we calculate the returns earned by the purchased stock in the 30 trading days following each of an investor’s purchases. We choose to examine 30-day returns because the median holding period in our data is 39 trading days, but all of our findings remain unchanged if we use a 10-, 45-, or 60-day holding period. Importantly, we truncate this calculation window at the length of the actual holding if it is shorter than 30 days. That is, the 30-day return for investor \(i\) holding stock \(j\) is,

\[
R_{i,j}(t) = \frac{P_j(t + \min(s, 30))}{P_j(t)} - 1,
\]

where \(P_j(\cdot)\) denotes the stock’s closing price adjusted for splits and dividends, \(t\) denotes the purchase date, and \(s\) denotes the actual holding period.

Our approach is an attempt to deal with the problem of comparing returns over similar holding periods while ensuring that the actual selling decisions of investors affect their performance. By measuring returns this way, we hope to capture the value of short-term signals that the investor may have received. Looking over longer horizons would introduce considerable noise into our return estimates.

To adjust for risk, some of our results use risk-adjusted returns (or alphas). We use a four-factor model to adjust returns for known risk factors. In addition to a value-weighted market return, we construct three factor-mimicking portfolios (SMB, HML, and UMD; see Fama and French (1993) and Carhart (1997)). To construct the HML and SMB factors, we augment our data with information from Thomson Financial’s DataStream database. We take quarterly data on shares outstanding and book value of equity using definitions as similar as possible to those of Fama and French.
Each quarter, we sort firms into deciles independently along three dimensions: market capitalization (price per share times shares outstanding), market-to-book (price per share divided by book value of equity per share), and past returns (over months $t - 12$ to $t - 2$). The small-minus-big (SMB) portfolio consists of a long position in the top half of stocks by market capitalization, and a short position in the bottom half. The high-minus-low (HML) portfolio consists of a long position in those stocks in the top 30 percent of market-to-book, and short position in stocks in the lowest 30 percent. The up-minus-down (UMD) portfolio consists of a long position in stocks in the top decile of past return performance, and a short position in stocks in the bottom decile. We use the overnight interbank lending rate in Finland as a proxy for the risk-free rate. Beginning in 1999, this is equivalent to the Euribor rate.

To measure a firm’s risk in a particular year, we regress daily returns in excess of the risk-free rate on the daily returns of the four factor-mimicking portfolios and a constant. To ensure that our estimates are not contaminated by well-known problems associated with non-synchronous trading, we follow the two-stage least squares approach of Scholes and Williams (1977, pp. 316–319).

We use the estimated factor betas from these regressions to calculate risk-adjusted daily returns for each stock, and we use these adjusted returns in the same performance regressions we previously estimated with raw returns. The risk adjusted return is defined as

$$\alpha_{i,t} = R_{i,t} - (\beta^{M}_{i} \text{RMRF}_{t} + \beta^{S}_{i} \text{SMB}_{t} + \beta^{H}_{i} \text{HML}_{t} + \beta^{U}_{i} \text{UMD}_{t}) ,$$

where $R_{i,t}$ denotes the raw return of stock $i$ on date $t$, $\beta^{M}_{i}$ is stock $i$’s market beta, $\beta^{S}_{i}$ is the beta on the size factor (SMB), $\beta^{H}_{i}$ is the beta on the value/growth factor (HML), and $\beta^{U}_{i}$ is the beta on the momentum factor (UMD). The returns RMRF, SMB, HML, and UMD are the returns on the market portfolio, and the three factor-mimicking portfolios, respectively. Given the daily time-series of factor-mimicking portfolios constructed as described above, we have a daily series of risk-adjusted returns, which we can then use instead of raw returns in several of our tests. As with the raw returns, we calculate holding period returns over a fixed interval of 30 days, but truncate the holding period at the length of the actual holding period.
Given our measure of performance, we perform a few tests to be sure that it has similar properties to the measures used previously, such as poor investor performance on average and persistence in performance. However, our main prediction about performance relates directly to the central hypothesis of our paper:

**Prediction 1.** *Individuals with more investment experience have better investment performance.*

We test this conjecture at this point by regressing our performance measure on our experience measures. Performance is measured as an investor’s average return over the 30 trading days following each purchase. Our primary experience variables include the number of years that an investor has been in our data and the cumulative number of trades the investor has placed. We include a quadratic term for each experience variable to allow investors to learn more slowly over time. Notably, evidence for this prediction is consistent with investors learning by trading as predicted by H1, but it does not allow us to differentiate between H2 and H3. Later in the paper we adjust for individual heterogeneity and potential survivorship bias. Results of the tests related to performance are presented in Section 3.1.

1.2 Measuring disposition

Another outcome variable that we track to evaluate whether investors learn with experience is the disposition effect. Previous researchers have measured the disposition effect in a number of ways. Odean (1998) compares the proportion of losses realized to the proportion of gains realized by a large sample of investors at a discount brokerage firm. Grinblatt and Keloharju (2001) model the decision to sell or hold each stock in an investor’s portfolio by estimating a logit model that includes one observation for each position on each day that an account sells any security. Days in which an account does not trade are dropped from their analysis.

As Feng and Seasholes (2005) point out, a potential problem with these and similar approaches is that they may give incorrect inferences in cases in which capital gains or losses vary over time. Hazard models, which have been extensively applied in a number of fields including labor economics and epidemiology, are ideally suited to our setting. Feng and Seasholes (2005) implement a parametric hazard model. However, they pool their data and
estimate the hazard regression only once for all investors together, ignoring most individual-
level heterogeneity and any survivorship bias. Since our focus is on estimating disposition
at an individual level, we estimate the hazard regression for each investor and year. Imple-
mentation of the hazard model uses all data about the investor’s trading and the stock price
path, rather than just data on days when a purchase or sale is made. That is, it implicitly
considers the hold-or-sell decision each day. This improves our disposition estimates, and
gives us more power with which to investigate learning. More important, individual level
estimates enable us to test H2 and H3 by explicitly accounting for investor attrition and
heterogeneity.

We use a Cox proportional hazard model with time-varying covariates to model the
probability that an investor will sell shares that he currently holds. We count every purchase
of a stock as the beginning of a new position, and a position ends on the date the investor
first sells part or all of his holdings. Alternative definitions of a holding period, such as first
purchase to last sale, or requiring a complete liquidation of a position, do not substantively
alter our results. Our time-varying covariates include daily observations of some market-wide
variables and daily observations of whether each position corresponds to a capital gain or
loss.

Proportional hazard models make the assumption that the hazard rate is

\[ \lambda(t) = \phi(t) \exp(x(t)\beta). \]  

Here, the hazard rate, \( \lambda(t) \), is the probability of selling at time \( t \) conditional on holding a
stock until time \( t \), and \( \phi(t) \) is referred to as the baseline hazard. The term \( \exp(x(t)\beta) \) allows
the expected holding time to depend on covariates that vary over time. In our specification,
each of the covariates changes daily. Since we estimate the hazard model for each investor-
year, the baseline hazard rate describes the typical holding period of just one investor in
one particular year. The Cox proportional hazard model does not impose any structure on
the baseline hazard, and Cox’s (1972) partial likelihood approach allows us to estimate the
\( \beta \) coefficients without estimating \( \phi(t) \). The method also allows for censored observations,
which is important in our setting because investors have not always closed a position by the
end of a given year. Details about estimating the proportional hazard model can be found
The particular specification we use for investor $i$’s hazard to sell position $j$ is

$$
\lambda_{i,j}(t|x_{i,j}(t)) = \phi_i(t) \exp \left\{ \beta^d_i I(R_{i,j}(t) > 0) + \beta^r_i R_{M,t} + \beta^\sigma_i \sigma_{M,t} + \beta^V_i V_{M,t} \right\}.
$$

The key variable in this regression is $I(R_{i,j}(t) > 0)$, an indicator for whether the total return on position $j$ from the time of purchase up until time $t$ is positive. Investors who suffer from the disposition effect are more likely to sell when this condition is true, so they will have positive values of $\beta^d_i$. In the rest of the paper, we refer to $\beta^d_i$ as the ‘disposition coefficient.’ The total return variable includes any dividends or other distributions, and is calculated using closing prices on all days, including the date of purchase. We use the closing price on the purchase date instead of the actual purchase price to ensure that our results are not contaminated by microstructure effects. In addition, we include as controls three 5-day moving averages of market-level variables to ensure that we are not capturing selling related to market-wide movements: market returns ($\bar{R}_{M,t}$), squared market returns ($\sigma_{M,t}$), and market volume ($V_{M,t}$). We repeat this estimation via maximum likelihood each year from 1995–2003 for each individual $i$ who places at least seven round-trip trades in a year.

To investigate learning, we use our disposition estimates as dependent variables in cross-sectional regressions. Because these estimates are measured with noise, we use weighted least-squares (WLS) regressions where the weight given an observation is proportional to the reciprocal of the estimated variance of the disposition coefficients. The standard errors used in this calculation are calculated using the robust ‘sandwich’ estimator, with clustering by security.

We perform a few tests on our disposition coefficients to be sure that our measure has similar properties to the measures used previously. In particular, for investors to have an incentive to learn to avoid the disposition effect, it must be a behavioral bias that is costly to them. We examine whether disposition is a somewhat stable, predictable attribute of a particular investor and whether investors with more disposition have inferior investment performance. However, our main prediction about disposition is closely related to our central hypothesis:

**Prediction 2.** Individuals with more investment experience have a weaker disposition effect.

---

*In addition, our data do not include transaction prices during the first three months of 1995, but we do have closing prices during this period.*
We test this prediction initially by regressing disposition coefficients on our experience measures. As in our performance results, we use the number of years that an investor has been in our data, and/or the cumulative number of trades to measure experience. Also similar to our earlier tests on performance, support for this prediction is consistent with H1, but it does not allow us to differentiate between H2 and H3. Again, later in the paper we adjust for individual heterogeneity and survival to differentiate between the two types of learning.

Our empirical strategy differs from that of Feng and Seasholes (2005), who estimate one large hazard model with all investors pooled together. Their hazard model includes an experience variable, and an interaction term of \( I(R_{i,j}(t) > 0) \) with their experience variable. We avoid this approach because it gives inordinate statistical weight to investors who trade frequently. To see the sort of problem that this approach might generate, suppose that investors trade according to different styles. Some investors trade like day traders, exploiting short-term supply and demand imbalances, while others trade like fundamental analysts, looking for larger and longer-term mispricing. If we aggregate traders by assuming that each of an individual’s trades is a separate, conditionally independent observation, the inferences we make about learning will be driven by the types of investors who trade the most. Accordingly, we could mistakenly attribute the improvements of a select few individuals to the whole population. While we avoid this sort of problem by making an individual-year the unit of observation, we estimate a few hazard models with the Feng and Seasholes (2005) method to allow comparison with their results.

1.3 Survivorship and heterogeneity

Our predictions so far have ignored heterogeneity and survivorship. However, we need to consider heterogeneity and attrition to have persuasive inferences about learning and to test H2 and H3. Through wealth effects, significant heterogeneity in ability can plausibly make it appear as if there is learning when in fact there is none. More important, simple models that neglect survivorship may provide evidence consistent with investors learning, but they will not be able to determine whether poor investors actually improve their ability or simply drop out after learning about their inherent inability. To avoid these problems, we carefully control for investor heterogeneity and survivorship bias. We use two methods to disentangle the two effects. First, we control for time-invariant unobserved individual characteristics by
including individual fixed effects in our learning regressions. Any additional improvement over time is then likely attributable to improvement of ability. Second, we directly examine how much ceasing to trade (or learning about ability) affects our inferences about learning by using a modified version of the selection model introduced by Heckman (1976).

The classic Heckman model involves a two-stage procedure. In the first stage, a selection model is constructed to predict which observations will be observable in the second stage. In the second stage, the regression of interest is estimated with an adjustment for survivorship bias. We modify this procedure to account for both survivorship bias and individual heterogeneity, adopting the empirical strategy of Wooldridge (1995), which modifies the Heckman model to allow for fixed effects. Intuitively, this approach accounts for survivorship by estimating the selection model every year and including the inverse Mills ratios (the conditional probability that an individual would not cease to trade) of each selection equation in the learning regression model. Individual time-invariant heterogeneity is accounted for in this method by running the learning regression in first differences. More concretely, the learning regression model we estimate is

\[
\Delta y_{i,t+1} = \beta \Delta x_{i,t} + \rho_2 d_{2t} \lambda_{i,t} + \ldots + \rho_T d_{Tt} \lambda_{i,t} + \epsilon_{i,t},
\]

where \( \lambda_{i,t} \) are the Inverse Mills ratios from a year \( t \) cross-sectional probit model (the selection model) and \( d_{2t}, \ldots, d_{Tt} \) are year dummies. Including these variables in the learning regression accounts for the impact of the selection equation. Note that a joint test of \( \rho_t = 0 \) for \( t = 2, \ldots, T \) is a test of whether survivorship bias is a concern.

The first-stage uses cross-sectional probit regressions to predict whether or not the individual ceases to trade in a given period. The probit regressions include a constant, linear and quadratic experience terms, the number of different stocks the investor trades, the individual’s average return in the previous year, and the individual’s average daily marked-to-market total portfolio value. As instruments, we use the following variables: (1) a dummy variable for whether an investor inherited shares in the previous calendar year; and (2) the cross-sectional standard deviation of the individual’s previous-year 30-day return. As we will explain below, both these variables are likely to satisfy the necessary exogeneity conditions—that is, they are likely to affect the probability of remaining in the sample, but are unlikely to affect changes in an individual’s performance or disposition effect except through their effect on survival.
For our first instrument, we conjecture that an individual who inherits shares is more likely to trade in the future, perhaps because their wealth has increased, or because the new shares cause them to pay more attention to the stock market. This satisfies the exogeneity condition since inheritance of shares from a relative is unlikely to directly affect changes in the performance or disposition effect of an individual. In the data, when an investor dies and shares are transferred to an heir, it appears as a transaction with the account of the deceased selling shares and the account of the heir purchasing shares. A special code identifies the transaction as an inheritance. In our sample death transfers are evenly spread over the sample (ranging from 62 in 1995 to as high as 443 in 2003). Our second instrument is the variation in the returns across all positions taken by an account in the previous year. This variation is a measure of the consistency of an investor’s performance, and we conjecture that an investor with more variable performance is more likely to stop trading. Again, there is no reason to believe that the consistency of an investor’s past performance should directly affect changes in his future performance or disposition effect.

We construct the sample to be used in the selection model as follows. An account observation is added to the selection sample if it places one or more trades in a given year. This differs from our main sample, where we require investors to have placed at least seven round-trip trades in order to estimate either the average performance or the disposition coefficient. Once an account is added, it remains in the selection sample until 2003, which is the end of our data. In some years, an account will have placed enough round-trip trades to be included in our hazard regressions, so the data will include a performance average and disposition estimate for this account. However, each year we will also have data on many accounts for which we do not have performance and disposition estimates. If estimates are available, we treat the account as having been selected into our data.

1.4 Predictions about heterogeneity and survival

In order to differentiate between H2 and H3, we examine the role of investor heterogeneity and survival in our data. For these to be the important, the heterogeneity for which we want to control must be at least partially observable. This observation leads to our first prediction about heterogeneity:
Prediction 3. *There is substantial predictable heterogeneity in investors’ performance and behavioral bias. Some heterogeneity is correlated with experience.*

We test this conjecture by sorting investors into subsamples based on various characteristics that are ex-ante likely to be related to their financial sophistication. For example, we sort investors by their wealth (proxied by each investor’s average daily portfolio value), by whether or not they trade options, and by several other characteristics. We then estimate the average disposition and performance of each subsample, testing whether there is a significant difference between the ex-ante sophisticated and unsophisticated subsamples. We also use each subsample to estimate our simple learning regressions, and we look at the experience coefficients in these subsample regressions to test whether learning across groups occurs at the same rate.

Our next prediction examines the effect on learning of individual heterogeneity more directly:

**Prediction 4.** *Accounting for individual heterogeneity explains a significant portion of the learning implied by simple models.*

We examine this prediction by estimating our learning regressions once again, this time controlling for investor heterogeneity by including individual fixed effects. We also include year fixed effects to adjust for time-series variation in average market returns. We compare the coefficients of our fixed effects regression to the simple model that we previously estimated.

We go further, exploring our data for potentially important survivorship effects. For survivorship effects to be important, there must be significant attrition in the data, which we confirm by testing our next prediction:

**Prediction 5.** *Many new investors will cease trading within a short period of time. Those who remain will trade more over time.*

We examine this prediction by looking at the rate at which investors who are in our sample in one year (having placed seven or more round-trip trades that year) continue to be in

---

7Our proxy for wealth includes only the equity holdings of investors. This measure could be negatively correlated with risk aversion since for any level of total wealth an investor who allocates more money to equities may be more risk-averse. However, even if this is the case, cross-sectional variation in risk aversion cannot explain changes in the disposition effect, performance and survival rates over time.
the sample in subsequent years. The prediction that surviving traders will trade with more intensity is a feature of standard learning models like that of Mahani and Bernhardt (2007). In this model, investors do not initially know their type. As investors trade, they update their subjective probabilities of being skilled, learning about their inherent ability. Investors with a sufficiently low probability of being skilled eventually cease trading while those that survive increase the intensity of their trades. We examine the intensity part of this prediction by looking at the typical pattern of trading volume for a new account that continues to trade.

Finally, we examine how investor survivorship affects our learning estimates in our last prediction:

**Prediction 6.** *Accounting for survivorship in addition to individual heterogeneity explains a significant portion of the learning implied by simple models.*

We carefully control for survivorship bias by estimating our learning regressions with the modified Heckman (1976) correction. It is important for us to control for survivorship bias, since it is clear that investors with weaker performance will be less likely to continue trading long enough for us to estimate their performance in future periods. Comparing the results of our simple models to those that correct for survivorship allows us to estimate what fraction of learning by trading is driven by learning about inherent ability (H2) and what fraction is driven by improved ability (H3).

### 2 Data

The data used in this study come from the central register of shareholdings in Finnish stocks maintained by Nordic Central Securities Depository (NCSD), which is responsible for the clearing and settlement of trades in Finland. Finland has a direct holding system, in which individual investors’ shares are held directly with the CSD. Since our data come from the CSD, they reflect the official record of holdings and are therefore of extremely high quality. The data cover all trading in all Finnish stocks over a nine-year period. Grinblatt and Keloharju (2000, 2001a, 2001b) use a subset of the same data, comprising the first two years of our sample period. The data include the transactions of nearly 1.3 million individuals.

---

†These references provide a detailed discussion of the data.
and firms, beginning in January, 1995 and ending in December, 2003. In all, more than 22 million trades by individual investors are included.

While our dataset includes exchange-traded options and certain irregular equity securities, we focus on trading in ordinary shares. Trading in Finland is conducted on the Helsinki Stock Exchange, which is owned by OMX, an operator of stock exchanges in Nordic and Baltic countries. Trading on the Helsinki exchange begins with an opening call from 9:45–10:00 a.m., and ends with a closing call from 6:20–6:30 p.m. Continuous trading during regular hours is conducted through a limit order book.

Our transaction data include the number of shares bought or sold, corresponding transaction prices, and the trade and settlement dates, although trades are not time-stamped. Additional demographic data, such as the account-holder’s age, zip code, and language are also included, as are initial account holdings at the beginning of our sample period. We use the data to create proxies for wealth and measures of investor sophistication.

To construct a proxy for wealth, we use opening balances and subsequent trades to reconstruct the total portfolio holdings of each account on a daily basis. Using these holdings, we approximate wealth as the average daily marked-to-market portfolio value for each investor. We also calculate the average value of trades placed by an investor each year. To measure sophistication, we note that investors who trade options are likely to be more familiar with financial markets. This is particularly true in our setting because many of the options in our data are granted to corporate executives as part of compensation. Therefore, while we do not include options trades in our estimates of disposition, we use whether an investor ever trades options as a proxy for sophistication. We also count the number of distinct securities traded by an investor over the sample period, and use this as a measure of portfolio diversification.

Despite the impressive richness of these data, they are missing a few important features. Only the direct holdings and transactions of individuals are available. This means that for an individual who directly trades shares of Nokia and holds a Finnish mutual fund that owns shares of Nokia, we will observe only trades in the former. The trades of the mutual fund are included in the dataset, but are identified as holdings of the mutual fund company, and cannot be tied to the individual. However, our wealth calculations allow us to compare the importance of the individual investors as a group to that of other market participants. On average, individuals hold 12.6 percent of all equity held by Finnish investors,
including financial institutions, government funds, nonprofit organizations and nonfinancial corporations. This is more than financial institutions, which hold an average of 9.6 percent during our sample period. The majority of equity is held by the government (34.7%) and nonfinancial firms (33.4%), although these investors trade relatively less and may do so for strategic reasons that are not directly linked to profit-maximization.

Table 1 provides summary statistics for the new accounts in our dataset. New accounts are accounts that place their first trade in 1995 or a subsequent year. (That is, they have no recorded initial positions.) Panel A includes all new accounts that place at least one trade during our sample period (1995–2003), while Panel B gives results only for those new accounts for which we are able to estimate the disposition coefficient at least once. We only attempt to estimate the disposition coefficient if an individual has placed at least seven round-trip trades in a given year, although even with this restriction the procedure to maximize the likelihood function does not always converge. The last two rows of each panel are indicator variables, taking a value of one if the investor: (a) trades options; or (b) is female; and zero otherwise.

Comparing Panels A and B, it is apparent that the subset of investors for whom disposition coefficients are available is somewhat different than the larger population. By construction, the accounts in Panel B place more trades, but they also have larger portfolios, trade larger amounts of money, trade in a wider selection of securities, and are somewhat younger. As well, investors for whom we can estimate disposition are more likely to trade options (17%) than the overall sample (3%). Since we are only able to estimate disposition for investors who trade with some frequency, this likely results from the fact that investors who trade options are simply more likely to trade in general.

Figure 1 shows the number of accounts (including both new and existing accounts) that place one or more trades in each year. There is considerable variation in the number of accounts placing trades over time, from a low of 54,196 accounts in 1995 to a high of 311,013

---

9A complication of our data is that trading of shares held in an American Depository Receipt (ADR) or by certain foreigners who need not register directly with the CSD is hidden in the orders of certain institutions that serve as registrars. It is possible, however, to separate these ‘nominee’ accounts from other institutional holdings by carefully analyzing the trading history of each institutional account. We implement such a procedure, the details of which are available upon request. Therefore, throughout this paper when we write ‘institutions,’ we mean Finnish institutions and not nominee-registered accounts or trading in American- or Swedish-listed depository receipts.
accounts in 2000. Additions of new accounts follows a similar pattern. We discuss entry and exit from the sample in more detail in the next section.

3 Results

We present our empirical findings in this section. We begin by presenting the results of our tests relating to performance in Section 3.1 and to the disposition effect in Section 3.2. Section 3.3 examines whether there is heterogeneity in learning and whether our tests that adjust for heterogeneity change our learning estimates. Section 3.4 examines the importance and magnitude of survivorship effects, and a number of additional tests are presented in Section 3.5.

3.1 Performance tests and results

We start by performing a few tests on trader performance to be sure that our performance measure has similar properties to the measures used in the literature. Previous papers, in particular Odean (1998), have shown that average investor performance is worse than that of the market portfolio. Poor performance by average investors is also a prediction of the model in Mahani and Bernhardt (2007). This motivates our first test—on average, individuals do not earn returns in excess of the market return. We test this hypothesis by calculating the average return to a stock purchased by an individual investor net of the market return. Calculating this average at a 30-day horizon (using our convention of using a shorter holding period if the individual sells the stock before 30 days) yields an average return net of the market of $-4.9\%$. At a 60-day horizon, the average net return is $-10.1\%$. At both of these horizons, returns net of the market return are quite statistically significantly negative.

While the average performance of individual investors is likely to be quite poor, Coval, Hirshleifer, and Shumway (2005) shows that some individuals persistently outperform others. Again, performance persistence among individuals is an implication of Mahani and Bernhardt (2007). We test whether there is any persistence in investor performance in three related ways. For the first approach, we regress each investor’s average 30-day return in year $t$ on the investor’s average return in year $t - 1$ and year fixed effects. Using year fixed effects
adjusts for time series variation in average market returns. The estimated coefficient in this regression is 0.183 ($p < 0.0001$), very statistically and economically significant. Our second approach is to calculate each investor’s average return in two disjoint time periods, 1995–1999 and 2000–2003. We then calculate the Spearman rank correlation between the return series from the first period with that from the second period. This correlation is 0.164 ($p < 0.0001$), again quite statistically and economically significant. Our third test method involves sorting investors in each year into performance quartiles, and then plotting the average performance of each of those quartiles for the next several years. This plot, which appears in Figure 2, again gives evidence that the most successful investors in the past continue to outperform the least successful investors for at least a couple of years. Results calculated with alphas instead of raw returns are qualitatively the same. These results confirm that there is a degree of persistence in individual returns.

Our main prediction on performance is that more experienced investors have better investment performance. For now, we test this prediction by simply regressing performance on experience, experience squared (to allow learning to slow down over time) and some control variables, ignoring any individual heterogeneity or survivorship bias. Columns 1 and 2 of Table 3 report the results of this regression. When experience is measured either in number of years or cumulative trades, it is positively and significantly related to average returns. An additional year of experience increases average 30-day post-purchase returns by $41 - 4 = 37$ bp, or approximately 3 percent at an annualized rate. An additional 100 trades increases returns at slightly over one-fourth of this rate. Again, results estimated with alpha instead of raw returns are quite similar (unreported). While these estimates are encouraging, the speed of learning they imply seems almost implausibly large. Taking the regression parameters at face value, an investor with 8 years of experience should outperform a new investor by about 22 percent per year. While we observe some heterogeneity in investor ability (or some performance persistence) it is not nearly large enough to justify these large coefficients.

3.2 Disposition tests and results

The disposition effect is quite large in our data. To give an idea of the economic significance of the effect, we present some aggregate evidence of the effect in our data. Figure 3 is a plot of the relation between the propensity to sell an existing position (the hazard ratio) and the
position’s holding period return. To generate this plot, we group all investors and estimate one hazard model each year. We group the data for this procedure so we can estimate a model with many covariates, but almost all of the tests that follow are based on individual-level results. Rather than using only one indicator variable as in Equation (3), we use 20 dummy variables corresponding to different 1 percent return ‘bins’ in this model. In Figure 3 we plot the dummy variable coefficients by year. The sum of these coefficients times their corresponding dummy variables is multiplied by the baseline hazard rate to give the actual conditional hazard rate. The conditional hazard ratio is remarkably similar across years. The plot shows an obvious kink in the hazard ratio near zero: investors are clearly more likely to sell a stock if it has increased in value since the purchase date. This provides strong support for the presence of a disposition effect in aggregate, consistent with the extensive literature cited above.

Turning to our main individual-level disposition regressions, we require that an investor place at least seven round-trip trades in a year to be included in the sample, and we run the regression for each investor-year to generate a separate disposition coefficient whenever possible. While this filter drastically reduces our sample size, it is necessary to ensure that our coefficients of interest are identified.

Table 2 summarizes the distribution of our disposition estimates, which we use to investigate several of our predictions. Panel A provides information on all investors for whom we have estimates. There are 18,042 observations in our panel, and the number of observations each year rises considerably in the first half of the sample and then declines somewhat in the latter part of our sample. The median disposition coefficient is 1.04, which is economically quite large. This coefficient implies that the median new investor in our data is $e^{1.04} = 2.8$ times more likely to sell a stock whose price is above its purchase price than a stock that has fallen in value since the time of purchase.

Using the estimated standard errors for each investor, we can classify estimates as significant or not at any given confidence level. The last two columns of Panel A show the proportion of investors who have a significantly positive or negative disposition coefficient at the 10 percent level. Over our entire sample period, 41.8 percent of investors have a disposition coefficient that is statistically greater than zero. Panel B gives summary statistics for the other coefficients in the hazard model. None of the controls is statistically significant in the cross-section.
Before we can consider whether investors learn to avoid the disposition effect, we need to argue that the effect is in fact a behavioral bias. Theoretically, there is no particularly well accepted model, either rational or irrational, that produces the disposition effect. It is difficult to imagine a rational model that can produce the effect. It is not particularly difficult to think of a model in which the probability of selling a stock increases in the stock’s unrealized return, but it is difficult to think of a model in which the probability of selling rises dramatically as soon as the return becomes positive—that is, a model that predicts a ‘kink’ at zero, as show in Figure 3.

It is also difficult to design a model in which traders with the disposition effect have the characteristics that our traders have. In particular, one necessary condition for disposition to be a behavioral bias is that investors with more disposition have inferior investment performance. If disposition is unrelated to investment performance, investors with the effect would have little incentive to learn to avoid it. To get a sense of how returns vary with disposition, we examine average investor returns across quintiles of the disposition coefficient. In this sort, the disposition coefficients are always estimated one year before the average returns are calculated, so disposition coefficients and average returns are not mechanically correlated in any way. For each quintile, Figure 4 graphs the average return earned by investors over different horizons from the purchase date. Returns are substantially higher in the lowest disposition quintile than in the highest disposition quintile. For example, in the 30 days following a purchase, a stock’s price increases 46 bp on average when bought by an investor in the lowest disposition quintile, compared to a decline of 54 bp if purchased by an investor in the highest disposition quintile. The differences between high- and low-quintile average returns range from 17 bp at the 10-day horizon to 131 bp at the 45-day horizon. These differences are both economically and statistically large. These results are consistent with the claim that individuals with high disposition effect coefficients have relatively poor investment performance. They are also consistent with the disposition effect being a behavioral bias that investors want to learn to avoid.

Another necessary condition for disposition to be a behavioral bias is that disposition is a somewhat stable, predictable attribute of a particular investor. We test this conjecture by estimating the disposition effect at the investor level in adjacent time periods. Each set of estimates comes from a completely disjoint dataset. Any trades that are not closed at the end of the first period are considered censored in the model estimated with first period data.
Therefore, any trades that are not closed at the end of the first period are completely ignored in the model estimated with second period data. We explore the stability of disposition coefficients by estimating the rank correlation of account-level disposition coefficients over the two periods, testing whether the rank correlation is significantly different from zero. We estimate the rank correlation between an investor’s disposition coefficient in year $t$ and their coefficient in year $t - 1$ to be 0.364, suggesting that there is a fair degree of persistence in the individual’s disposition coefficient. This correlation is extremely statistically significant.

Taken together, Figures 3 and 4, Table 2 and the correlation in the previous paragraph provide strong evidence that the disposition effect is a widespread and economically important behavioral bias that is present in each year of our study.

Finally, we examine whether more experienced investors are more likely to avoid the disposition effect, testing our main prediction related to disposition. Again at this point, we simply regress disposition coefficients on our experience variables, those variables squared, and some control variables. Columns 3 and 4 of Table 3 present our results for the disposition learning regressions. To reduce the weight given to disposition coefficients that are not estimated very precisely, we estimate the regressions with weighted least squares, where the weights are proportional to $1/\hat{\text{Var(}} \beta_d \text{)}$ from our hazard regression in Equation (3). The base case (Column 3) shows that disposition declines with experience ($\beta_1 < 0$). Moreover, investors tend to slow down in their learning as they gain experience since $\beta_2 > 0$. Frequent traders, investors who trade more securities, and investors who earned higher returns in the previous year all have lower levels of disposition, but even with these controls our base results are qualitatively unchanged.

Column 4 indicates that an additional 100 trades reduces the disposition coefficient by 0.041, which is similar to the coefficient on Experience in Column 3. In other words, a year of experience or 100 trades have approximately the same effect on disposition. In each of the specifications the estimated YearsTraded and CumulTrades coefficients are statistically significant at the 1 percent level. Economically, however, our results suggest that investors learn relatively slowly. Specifically, the estimates in Column 3 suggest that an additional year of experience corresponds to a reduction in the disposition coefficient of approximately 0.05. To provide some context for this estimate, note that the unconditional median disposition coefficient in our sample is 1.04. An extra year of experience decreases this by about five percent.
As discussed in Section 1.2, we also estimate the yearly hazard models presented in Table 4, which are comparable to the model of Feng and Seasholes (2005). These estimates are from pooled hazard models estimated each year, in which all individuals are treated as if they were just one person. Experience is interacted with an indicator for whether the price is above the purchase price, and the coefficient on this interaction term is interpreted as a learning coefficient. These models again give evidence of learning. However, the learning coefficient estimates are quite variable over time (0.149 to 0.063), and they are statistically insignificant in three of the nine years. Furthermore, the average disposition coefficient of an investor is estimated in aggregate to be around 0.65 in this model. This suggests that, depending on the period chosen, an additional year of experience corresponds to a reduction in the disposition coefficient of approximately 10 percent to 25 percent, significantly higher than our regression estimate. Table 4 also lists the number of observations available each year, and the fraction of the observations that are censored, or the purchased stocks that are not sold by the end of each year.

The low level of the average disposition effect, the high variability in the annual learning estimates, and the high level of learning found both by Feng and Seasholes (2005) and in our implementation of their model suggest that there are significant differences between our approach and theirs. One important difference is that, since we estimate a different disposition coefficient for each individual, those who place a large number of trades have the same weight in our analysis as those who place seven or eight trades. In Feng and Seasholes (2005), an individual’s weight is proportional to her trading volume. Another difference is that we estimate learning over a much longer period of time, since Feng and Seasholes (2005) only have about two years of transactions data. Thus, the year-to-year variation in the annual estimates in Table 4 is much less of a concern for our analysis.

3.3 Heterogeneity in learning

To examine if there is significant predictable heterogeneity among individuals in our sample, we separate investors into a number of different groups by observable characteristics that we

---

10 These regressions are essentially the same as the empirical strategy employed by Feng and Seasholes (2005), except we use a proportional hazard regression rather than the parametric Weibull model. Our indicator variable is the same as their ‘Trading Gain Indicator’ (TGI). The experience variable we use for this model is the total number of trades placed before the current trade, rather than CumulTrades, which is the total number of trades placed in previous calendar years.
believe are related to their financial sophistication. We examine the average disposition and performance of each of these groups, confirming that our priors are correct and demonstrating that there is significant heterogeneity in the data. We also estimate the simple learning regression for disposition and returns for each group, predicting that the less sophisticated investor groups will learn faster than the more sophisticated investor groups. Importantly, we do not classify investors on the basis of the estimated disposition coefficient, \( \beta^d \), because of concerns about measurement error. That is, the most extreme disposition estimates are likely those with the most error, and we would therefore expect these accounts to see a decrease in disposition in future years, even if these investors are not really learning. To avoid sorting on measurement error, we focus instead on observable variables that are ex-ante related to performance and disposition.

Each row of Table 5 displays the mean of the disposition coefficient and the average returns (or performance) of each group, as well as the regression coefficient of these variables on YearsTraded and the number of observations used in the calculations. Results for disposition are shown in Columns 1 and 2, and for returns in Columns 3 and 4. We consider investors ex-ante likely to be relatively sophisticated if they trade options, have significant wealth, are men, or have had relatively good past performance.\(^{11}\) Looking at the table, it is clear that the means for each of our sophistication subgroups is significantly different, and each change in the mean across subgroups is of the sign we expect. We also expect that less sophisticated investors will learn faster than more sophisticated investors. In each pair of rows of the table this prediction is confirmed. In most cases there is a clear difference between the unsophisticated investors, who learn to avoid the disposition effect at a rate of about 10 percent per year, and sophisticated investors, for whom the learning coefficient is often insignificant.

It is also plausible that investors learn more when the market in general is not doing well. During periods of high market returns, investors’ incentives to learn about their biases could be reduced if they attribute their success to their ability, similar to the behavior modeled in Zingales and Dyck (2002) in the context of media and bubbles. Thus, we should find that investors are more likely to learn when the markets are not doing well rather than when they are. To test this we define the state of the market as an ‘up-market’ if the excess return on

\(^{11}\)We classify investors who make excess profits that are in the top quarter of the entire market in the first two years of their trading as ‘winners.’ Our results are not sensitive to alternative definitions of winners, such as using a one- or three-year classification period, or above-median excess returns.
a broad Finnish index is positive in a given year, and as a ‘down-market’ if the excess return is negative. In the last two rows of the table we re-estimate our base regressions for each of the two states of the market and find that, as we hypothesized, individuals learn to avoid the disposition effect primarily when the market is not doing well.

These results are consistent with Prediction 3 and suggest that there is substantial heterogeneity both in initial ability (performance and disposition) and in rates of learning. It is unsophisticated investors and investors who start out with poor returns who learn most. These results suggest that considering heterogeneity in our learning estimates will be important.

We control for time-invariant unobserved individual characteristics by including individual fixed effects in our learning regressions. We also control for market returns and any other time-varying features of performance or disposition by including year fixed effects in the regressions. Specifically, we estimate:

\[ y_{i,t+1} = \alpha_i + \beta_1 \text{Experience}_{i,t} + \beta_2 \text{Experience}^2_{i,t} + \delta X_{i,t} + \gamma_t + \epsilon_{i,t}, \]  

where \( \alpha_i \) is the individual specific fixed effect, \( \gamma_t \) is the time fixed effect and \( X_{i,t} \) are other controls. The performance results, reported in Columns 1 and 2 of Table 6, suggest that an investor with one year of experience will earn 22 bp more than an inexperienced investor over a 30-day horizon. Column 2 indicates that a similar increase in returns comes from an additional 400 trades. The disposition results appear in Columns 3 and 4. While YearsTraded is no longer significant in these regressions, Column 4 suggests that 100 trades reduces the disposition coefficient by approximately 0.03. Comparing these estimates with those reported in Table 3, we find that the learning estimates after controlling for individual fixed effects fall roughly by one-half. Consistent with Prediction 4, this suggests that though investor performance improves and disposition declines with experience, accounting for individual heterogeneity reduces the estimates by about 50 percent.

### 3.4 Survivorship effects

In this section, we explore our data for potentially important survivorship effects. For these effects to be important, there must be significant attrition in the data. In other words, many
new investors should cease trading within a short period of time. The model of Mahani and Bernhardt (2007), which suggests that attrition is driven by investors learning about their inherent ability, also predicts that those traders who remain will trade more intensely over time.

We first present some overall attrition evidence by examining the rate at which investors who are in our sample in one year (having placed 7 round-trip trades in one year) fail to place any trades during the rest of our sample period. Since the rest of the sample period changes from year to year, the earlier years of our sample period provide more reliable estimates of true exit rates than the later years. Figure 5 shows that attrition is a significant feature of our data. Approximately 25 percent of those traders who enter the sample in one year fail to ever trade again. Of traders who trade for two or three years, about 5 percent permanently exit the sample. This is consistent with our Prediction 5.

To check whether trading intensity changes over time, we estimate regressions with both the number of trades placed and total trade value as dependent variables, and experience and year dummy variables as explanatory variables. Including year dummies ensures that market-wide changes in stock characteristics will not contaminate our results. We plot the results of these regressions in Figure 6. The plot clearly shows that, conditional on survival, trading intensity increases over time. This supports the part of Prediction 5 that has to do with trading intensity, which is consistent with the model of Mahani and Bernhardt (2007).

In some of our last sets of results, we carefully account for a survivorship effect by estimating our learning regressions using the method proposed by Wooldridge (1995), which is a standard Heckman (1976) correction modified to account for individual time invariant heterogeneity. Results from the selection model, with two-step efficient estimates of the parameters and standard errors, are given in Table 7. The first-stage selection model uses 30,218 observations, while the second-stage regression (in first differences) use only 6,511 observations in the performance regression and 8,818 observations in the disposition regressions.

We estimate the first-stage regression for each year and construct inverse Mills ratios for each year. For brevity, we only report one set of pooled first-stage estimates in Column 1 of Table 7. Results for each of the years are qualitatively similar to those reported. We find strong evidence that as investors get bad returns they cease trading. In particular the estimate on $\bar{R}_{t-1}$ is positive and significant. The estimate is also economically meaningful.
and suggests that, keeping other explanatory variables at their mean levels, a decrease in returns of one standard deviation increases the probability that the individual will cease to trade next period by around 15 percent. This is strong evidence for H2. As low ability investors trade, they learn about their inherent ability and cease trading. More successful investors continue to trade actively.

The other coefficient estimates reported in the first column of Table 7 also seem sensible: investors are more likely to remain in the sample and trade if they hold relatively diversified portfolios and have relatively more trading experience. Importantly, coefficient estimates for both of our instruments are statistically significant and of the predicted sign. Specifically, inheriting shares increases the probability that the individual will continue trading the next period and higher variability in past performance increases the probability that the individual ceases to trade. Both the instruments also have an economically significant impact. For instance, keeping other variables at mean levels, inheriting shares increases the probability that the investor will continue trading in the next period by 5 percent.

In line with Prediction 6, we find that accounting for selection has a significant impact on our learning estimates. Column 2 uses performance as the dependent variable in a regression of the form of Equation (4). Comparing the estimates in Table 7 to the simple model reported in Table 3, the coefficient on YearsTraded is no longer statistically significant, and the coefficient on cumulative trades is reduced by about 90 percent. When we use disposition as the dependent variable in Column 3, the coefficient is reduced by slightly more than 50 percent. The joint tests of statistical significance of the inverse Mills ratios in Columns 3 and 4 also show that accounting for sample selection is important. Unreported results in which YearsTraded and CumulTrades are included in separate regression models yield almost the same coefficients, but regressions that include both variables are reported for brevity.

Our results suggest that accounting for selection is important and significantly affects inferences about learning. Investor heterogeneity and survivorship effects account for some-

---

12 The coefficients on YearsTraded in these regressions must be viewed with caution. Because the change in YearsTraded is always equal to exactly one year, the coefficient on YearsTraded can only be identified if we leave the fixed effect for one year out of the regression. We leave the fixed effect for 1997 out of the regression, so the coefficient we report can be thought of as the learning coefficient for that year. For robustness, we also estimate standard Heckman selection models without either individual or year fixed effects. The coefficients on YearsTraded in these models are 0.31 in the performance regression and -0.019 in the disposition regression. Both of these coefficients are marginally statistically significant (at 10 percent). The magnitude and significance of these coefficients is consistent with our other results.
thing on the order of one-half to three-quarters of the learning estimates found in simple and aggregate models. This translates directly into slower learning than that inferred from simpler models. Taking the disposition-learning coefficient, for example, 100 trades corresponds to an improvement of about 0.04 in the simplest model, an improvement of about 0.03 in the model with individual fixed effects, and an improvement of about 0.02 in the survivorship/fixed effects model. Roughly speaking, if it takes about 100 trades to improve about 4 percent in the simple model, it takes about 200 trades to achieve the same improvement after adjusting for survivorship and individual heterogeneity.

Our estimates suggest that the fraction of learning that is driven by investors learning about their inherent ability (i.e., H2) by trading is large. After adjusting for this type of learning, the portion of learning that is due to investors learning to improve their ability over time (i.e., H3) is significantly different from zero, but not excessively large. In other words, there is support for H3, though accounting for H2 significantly reduces estimates of how quickly investors become better at trading. Overall, our findings are consistent with the three hypotheses that were outlined in Section 1.

### 3.5 Other Tests

In this section we conduct some additional tests related to our main predictions. First, it is possible that the performance improvement with experience that we find is not entirely due to learning of the types we considered but rather due to a change in the risk preferences of investors over time. To address this possibility, we estimate our learning regressions with risk-adjusted returns (or 30-day alphas) instead of raw returns, and we regress the average factor betas of stocks purchased by investors on experience and our control variables. Our regressions also control for survivorship and individual and year fixed effects. The results of our regressions appear in Table 8. The table clearly shows that risk-adjusted returns improve with experience, with a coefficient that is actually larger than the coefficient we estimate for raw returns. Looking at the coefficients on average factor betas makes it clear why this is the case. With more experience, investors are actually both improving raw returns and taking less risk, or purchasing stocks with lower factor betas. This result is particularly strong for the market (RMRF) and size (SMB) factor betas. Thus, it appears very unlikely that our raw return learning results are driven by changes in risk preferences with experience.
Second, in unreported results, we substitute the market return for each stock’s return to see if individuals learn to time the market. If investors are learning to identify good times to buy then the market as a whole will tend to increase after their purchases; if instead they are learning to select stocks, we will not find evidence of learning when we look only at market returns. In fact, we find that the coefficient estimates on experience variables are insignificant, which suggests that performance improved because investors became better at stock selection. Third, we also conduct the tests on survivorship using the Wooldridge (1995) method, taking data on investors who resume trading after ceasing to trade for a few years (the tests reported in the last section had dropped such investors, using only observations in two consecutive years). Including these investors increases the sample by around 250 observations in the second stage but does not affect the nature of the results reported. Fourth, all of the results on disposition remain qualitatively unchanged if we include a ‘December dummy’ in (3) or remove partial sales from our sample. This rules out tax-motivated selling or rebalancing as possible explanations for the disposition effect. Finally, in all the fixed effect regressions that control for individual heterogeneity, we cluster the standard errors at the individual level and find that our results are unaffected.

4 Conclusion

We examine learning in a large sample of individual investors in Finland during the period 1995–2003. We correlate performance and disposition with investor experience and investor survival rates to determine whether and how investors learn by trading. We find that performance improves and the disposition effect declines as investors become more experienced, suggesting that investors learn by trading. We differentiate between investors learning about their inherent ability by trading and learning to improve their ability over time by accounting for investor attrition. We find that a substantial part of this learning occurs when investors stop trading after learning about their inherent ability rather than continuing to trade and improving their ability over time. By not accounting for investor attrition and heterogeneity, the previous literature significantly overestimates how quickly investors become better at trading.

Our results suggest a number of interesting implications. First, since investors who continue trading learn slowly and there is great deal of turnover in the investor population,
it is likely that behavioral biases are an important feature of financial markets. Agents do not learn fast enough to make it impossible for biases to affect asset prices. Second, while it would be wonderful to know how quickly investors who cease to trade would learn if they chose to continue trading, we have no way to estimate this speed. If we assume that those who continue trading learn more quickly than those who cease to trade, policy makers might enhance welfare by devising screening mechanisms, or tests that measure and reveal inherent investing ability. Allowing unskilled investors to learn of their poor ability without incurring significant costs might be more valuable than encouraging people to become active investors. Third, an open question in the literature is why there is such high trading volume, particularly among seemingly uninformed individual investors. Our results indicate that such trading may be rational; investors may be aware that they will learn from experience, and choose to trade in order to learn. Our results also suggest that differences in the expected performance of investors may arise from different experience levels. Finally, if many inexperienced investors begin trading around the same time, their trades could lead to time-varying market efficiency. Our evidence is therefore consistent with the recent results of Greenwood and Nagel (2006), and the more general discussion found in Chancellor (2000) and Shiller (2005).
References


Table 1: Summary Statistics

This table presents summary statistics for our data. Panel A includes all individual accounts in our data that started trading during the sample period. Panel B gives results just for those accounts for which we are able to estimate at least one disposition coefficient. We only estimate the disposition coefficient if an individual has placed at least seven round-trip trades in a given year. Number of trades is the total number of trades placed by an investor during the sample period. Average portfolio value is the average marked-to-market value of an investor’s portfolio using daily closing prices.

### Panel A: Entire Sample (322,454 accounts)

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>25th Pctl</th>
<th>Median</th>
<th>75th Pctl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of years with trades</td>
<td>1.9</td>
<td>1.0</td>
<td>1.0</td>
<td>2.0</td>
</tr>
<tr>
<td>Number of securities traded</td>
<td>3.5</td>
<td>1.0</td>
<td>1.0</td>
<td>3.0</td>
</tr>
<tr>
<td>Number of Trades</td>
<td>15.4</td>
<td>1.0</td>
<td>3.0</td>
<td>8.0</td>
</tr>
<tr>
<td>Average value of shares traded, EUR</td>
<td>3,447</td>
<td>808</td>
<td>1,653</td>
<td>3,310</td>
</tr>
<tr>
<td>Average portfolio value, EUR</td>
<td>11,588</td>
<td>1,470</td>
<td>2,794</td>
<td>5,856</td>
</tr>
<tr>
<td>Age in 1995</td>
<td>39.3</td>
<td>27.0</td>
<td>39.0</td>
<td>51.0</td>
</tr>
<tr>
<td>Gender (1=female)</td>
<td>0.39</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Trades options (1=yes)</td>
<td>0.03</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### Panel B: Accounts with Disposition Estimates (11,979 accounts)

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>25th Pctl</th>
<th>Median</th>
<th>75th Pctl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of years with trades</td>
<td>4.4</td>
<td>3.0</td>
<td>4.0</td>
<td>6.0</td>
</tr>
<tr>
<td>Number of securities traded</td>
<td>22.3</td>
<td>12.0</td>
<td>18.0</td>
<td>28.0</td>
</tr>
<tr>
<td>Number of trades</td>
<td>222.3</td>
<td>68.0</td>
<td>117.0</td>
<td>224.0</td>
</tr>
<tr>
<td>Average value of shares traded, EUR</td>
<td>5356</td>
<td>1855</td>
<td>3235</td>
<td>5759</td>
</tr>
<tr>
<td>Average portfolio value, EUR</td>
<td>58828</td>
<td>5102</td>
<td>11483</td>
<td>26147</td>
</tr>
<tr>
<td>Age in 1995</td>
<td>35.3</td>
<td>27.0</td>
<td>34.0</td>
<td>44.0</td>
</tr>
<tr>
<td>Gender (1=female)</td>
<td>0.15</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Trades options (1=yes)</td>
<td>0.17</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 2: Disposition Estimates

This table reports a number of summary statistics for our estimates of the disposition effect. \( \beta^d \) is the coefficient in the hazard regression,

\[
\lambda_{i,j}(t|x_{i,j}(t)) = \phi_i(t) \exp\{\beta^d_i I(R_{i,j}(t) > 0) + \beta^r_i \bar{R}_{M,t} + \beta^s_i \sigma_{M,t} + \beta^V_i V_{M,t}\}.
\]

We estimate this model for each account-year with seven or more round-trip trades. Panel A reports on the cross-section of coefficients estimated each year. The columns labeled ‘positive’ and ‘negative’ report the proportion of investors with a statistically significant coefficient, where significance is measured at the 10% level using standard errors obtained from the maximum likelihood estimation of the hazard model. Panel B provides information for all the variables in the hazard model, where \( \beta^r, \beta^s, \) and \( \beta^V \) are the coefficients on 5-day moving averages of market returns, market returns squared, and market volume, respectively.

### Panel A: All Accounts with Disposition Estimates

<table>
<thead>
<tr>
<th>Year</th>
<th>N Obs</th>
<th>( \beta^d ) estimate</th>
<th></th>
<th></th>
<th>Significant at 10%</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>10th Pctl</td>
<td>Median</td>
<td>90th Pctl</td>
<td>Positive</td>
</tr>
<tr>
<td>1995</td>
<td>25</td>
<td>-0.34</td>
<td>0.72</td>
<td>2.37</td>
<td>28.0%</td>
</tr>
<tr>
<td>1996</td>
<td>89</td>
<td>-0.38</td>
<td>0.99</td>
<td>2.39</td>
<td>38.2%</td>
</tr>
<tr>
<td>1997</td>
<td>248</td>
<td>-0.91</td>
<td>0.96</td>
<td>2.31</td>
<td>33.1%</td>
</tr>
<tr>
<td>1998</td>
<td>695</td>
<td>-0.69</td>
<td>1.05</td>
<td>2.66</td>
<td>35.4%</td>
</tr>
<tr>
<td>1999</td>
<td>1958</td>
<td>-0.51</td>
<td>1.08</td>
<td>2.57</td>
<td>40.4%</td>
</tr>
<tr>
<td>2000</td>
<td>5961</td>
<td>-0.37</td>
<td>1.00</td>
<td>2.54</td>
<td>41.5%</td>
</tr>
<tr>
<td>2001</td>
<td>3732</td>
<td>-0.29</td>
<td>1.01</td>
<td>2.53</td>
<td>42.7%</td>
</tr>
<tr>
<td>2002</td>
<td>2649</td>
<td>-0.31</td>
<td>1.10</td>
<td>2.70</td>
<td>45.6%</td>
</tr>
<tr>
<td>2003</td>
<td>2685</td>
<td>-0.33</td>
<td>1.09</td>
<td>2.53</td>
<td>41.6%</td>
</tr>
<tr>
<td>All years</td>
<td>18042</td>
<td>-0.36</td>
<td>1.04</td>
<td>2.57</td>
<td>41.8%</td>
</tr>
</tbody>
</table>

### Panel B: Hazard Function Estimates

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>t-stat</th>
<th>10th Pctl</th>
<th>Median</th>
<th>90th Pctl</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \beta^r )</td>
<td>-0.04</td>
<td>-0.27</td>
<td>-0.65</td>
<td>0.03</td>
<td>0.93</td>
</tr>
<tr>
<td>( \beta^s )</td>
<td>0.01</td>
<td>0.46</td>
<td>-0.05</td>
<td>0.00</td>
<td>0.04</td>
</tr>
<tr>
<td>( \beta^V )</td>
<td>0.25</td>
<td>9.14</td>
<td>-2.62</td>
<td>0.24</td>
<td>3.08</td>
</tr>
<tr>
<td>( \beta^d )</td>
<td>1.32</td>
<td>6.81</td>
<td>-0.36</td>
<td>1.04</td>
<td>2.57</td>
</tr>
</tbody>
</table>
Table 3: Simple Learning Model: Estimates at Individual Level

This table presents results for regressions of the form

\[ y_{i,t+1} = \alpha + \beta_1 \text{Experience}_{i,t} + \beta_2 \text{Experience}^2_{i,t} + \delta X_{i,t} + \epsilon_{i,t} \]

where the dependent variable is either the investor’s average 30-day return following purchases (\( \bar{R}_{i,t+1} \)) or the investor’s disposition coefficient (\( \beta_{d,t+1} \)). Experience is measured by either years of experience (YearsTraded) or cumulative number of trades placed (CumulTrades). \( X_{i,t} \) is a vector of controls including the number of trades placed by the individual in a given year (NumTrades), the number of securities held by the individual in a given year (NumSec), and the individual’s average total daily portfolio value (PortVal). Data are from the period 1995 to 2003. Standard errors are in parentheses, and ***, ** and * denote significance at 1%, 5% and 10%, respectively.

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>( \bar{R}_{i,t+1} )</th>
<th></th>
<th>( \beta_{d,t+1} )</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>( \text{YearsTraded}_t )</td>
<td>0.414</td>
<td>(0.160)**</td>
<td>-0.050</td>
<td>(0.014)**</td>
</tr>
<tr>
<td>( \text{YearsTraded}_t^2 )</td>
<td>-0.043</td>
<td>(0.027)</td>
<td>0.007</td>
<td>(0.002)**</td>
</tr>
<tr>
<td>( \text{CumulTrades}_t ) (( \div 10^2 ))</td>
<td>0.110</td>
<td>(0.043)**</td>
<td>-0.041</td>
<td>(0.003)**</td>
</tr>
<tr>
<td>( \text{CumulTrades}_t^2 ) (( \div 10^4 ))</td>
<td>-0.0005</td>
<td>(0.0003)*</td>
<td>0.0002</td>
<td>(0.00002)**</td>
</tr>
<tr>
<td>( \text{NumSec}_t )</td>
<td>0.096</td>
<td>(0.012)**</td>
<td>-0.011</td>
<td>(0.001)**</td>
</tr>
<tr>
<td>( \text{NumTrades}_t )</td>
<td>-0.031</td>
<td>(0.005)**</td>
<td>0.006</td>
<td>(0.006)***</td>
</tr>
<tr>
<td>( \text{PortVal}_t ) (( \div 10^6 ))</td>
<td>0.104</td>
<td>(0.061)*</td>
<td>-0.006</td>
<td>(0.003)**</td>
</tr>
<tr>
<td>( \bar{R}_{t-1} )</td>
<td>-0.005</td>
<td>(0.001)**</td>
<td>-0.005</td>
<td>(0.001)***</td>
</tr>
<tr>
<td>Observations</td>
<td>13404</td>
<td>13404</td>
<td>17715</td>
<td>17715</td>
</tr>
<tr>
<td>( R^2 ) (%)</td>
<td>1.6</td>
<td>1.7</td>
<td>1.1</td>
<td>1.6</td>
</tr>
</tbody>
</table>
Table 4: Simple Learning Model: Disposition Estimates at Aggregate Level

This table presents learning estimates from pooled proportional hazards models, using a method similar to that of Feng and Seasholes (2005). Each year we pool the trades of all investors, treating them as if they were just one individual and estimating one learning coefficient for the entire population. The model is,

$$\lambda(t) = \phi(t) \exp\{\beta_4 I(R_{i,j}(t) > 0) + \beta_5 \text{Exper}^2\} + \beta_5 I(R_{i,j}(t) > 0)\}$$,

where Exper is measured in years since first placing a trade and $I(R_{i,j}(t) > 0)$ is an indicator variable that takes a value of one when a stock has increased in price since its purchase date. For brevity, only the $\beta_4$ coefficient estimates are reported. Estimated $\beta_5$ coefficients are all insignificant. We also report the number of trades, or observations, considered by the model (in thousands of trades), the percentage of observations censored (trades not closed by the end of the year), and the number of accounts contributing observations to the model. Standard errors are in parentheses, and ***, ** and * denote significance at 1%, 5% and 10%, respectively.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$\beta_4$</td>
<td>-0.074</td>
<td>-0.093</td>
<td>-0.096</td>
<td>-0.149</td>
<td>-0.031</td>
<td>-0.013</td>
<td>-0.063</td>
<td>-0.069</td>
<td>-0.123</td>
</tr>
<tr>
<td>(0.039)*</td>
<td>(0.062)</td>
<td>(0.031)**</td>
<td>(0.022)***</td>
<td>(0.024)</td>
<td>(0.011)</td>
<td>(0.011)***</td>
<td>(0.012)***</td>
<td>(0.012)***</td>
<td></td>
</tr>
<tr>
<td>Trades</td>
<td>6.6</td>
<td>14.5</td>
<td>26.6</td>
<td>44.6</td>
<td>99.6</td>
<td>251.9</td>
<td>161.9</td>
<td>115.6</td>
<td>131.8</td>
</tr>
<tr>
<td>Censored</td>
<td>16%</td>
<td>19%</td>
<td>19%</td>
<td>19%</td>
<td>17%</td>
<td>16%</td>
<td>17%</td>
<td>18%</td>
<td>20%</td>
</tr>
<tr>
<td>Accounts</td>
<td>384</td>
<td>713</td>
<td>1360</td>
<td>2412</td>
<td>4953</td>
<td>11585</td>
<td>7445</td>
<td>5416</td>
<td>6056</td>
</tr>
</tbody>
</table>
Table 5: Heterogeneity in Learning

This table reports both means and simple learning coefficient estimates from regressions of the form,

\[ y_{i,t+1} = \alpha + \beta_1 \text{YearsTraded}_{i,t} + \beta_2 \text{YearsTraded}^2_{i,t} + \delta X_{i,t} + \gamma_t + \epsilon_{i,t}, \]

conditioned on a number of variables that ex-ante might be correlated with trader sophistication. The variable of interest is either the disposition coefficient \((\beta_d^d_{i,t+1})\) or returns \((\bar{R}_{i,t+1})\). For brevity we only report \(\beta_1\) coefficients in the table. We classify investors as ‘Trades options’ if they trade in options at any point during our sample. Similarly, investors are classified as ‘wealthy’ if they are in the top 25th percentile of average portfolio value. We define the state of the market as ‘up’ if the excess return on a broad market index is positive, and ‘down’ if it is negative. Finally, we classify investors who make excess profits that are at or above the 75th percentile of all investors in the first two years of their trading as ‘winners.’ We include year dummies in all the regressions. Data are from the period 1995 to 2003. Standard errors are in parentheses and \(*\), \(*\) and \(*\) denote significance at 1%, 5% and 10% respectively. All group means are significantly different at 1% level.

<table>
<thead>
<tr>
<th>Classification</th>
<th>(\beta_d^d_{i,t+1})</th>
<th>(\bar{R}_{i,t+1})</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>YearsTraded</td>
</tr>
<tr>
<td>No options trades</td>
<td>1.17</td>
<td>-0.096</td>
</tr>
<tr>
<td>Trades options</td>
<td>0.99</td>
<td>-0.031</td>
</tr>
<tr>
<td>Not wealthy</td>
<td>1.14</td>
<td>-0.101</td>
</tr>
<tr>
<td>Wealthy</td>
<td>1.11</td>
<td>-0.015</td>
</tr>
<tr>
<td>Females</td>
<td>1.24</td>
<td>-0.140</td>
</tr>
<tr>
<td>Males</td>
<td>1.11</td>
<td>-0.078</td>
</tr>
<tr>
<td>Not winners</td>
<td>1.15</td>
<td>-0.840</td>
</tr>
<tr>
<td>Winners</td>
<td>0.96</td>
<td>-0.039</td>
</tr>
<tr>
<td>Down market</td>
<td>1.14</td>
<td>-0.120</td>
</tr>
<tr>
<td>Up market</td>
<td>1.08</td>
<td>0.028</td>
</tr>
</tbody>
</table>

39
Table 6: Learning with Individual Fixed Effects

This table reports estimates of regressions of the form

\[ y_{i,t+1} = \alpha_i + \beta_1 \text{Experience}_{i,t} + \beta_2 \text{Experience}_{i,t}^2 + \delta X_{i,t} + \gamma_t + \epsilon_{i,t}, \]

where Experience is measured by either years of experience (YearsTraded) or cumulative number of trades placed (CumulTrades). The dependent variable in models (1) and (2) is individual \( i \)'s average return in the following year, \( \bar{R}_{i,t+1} \), and in models (3) and (4) it is the individual’s disposition coefficient, \( \beta_{d_{i,t+1}} \). \( X_{i,t} \) is a vector of controls including the number of trades placed by the individual in a given year (NumTrades), the number of securities held by the individual in a given year (NumSec), and the individual’s average total daily portfolio value (PortVal). We also include year dummies and individual-specific intercepts in each regression. Data are from the period 1995 to 2003. Standard errors are in parentheses and \(*\), \(*\) and \(*\) denote significance at 1%, 5% and 10% respectively.

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>( \bar{R}_{i,t+1} )</th>
<th>( \beta_{d_{i,t+1}} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \text{YearsTraded}_t )</td>
<td>0.249 (0.401)</td>
<td>-0.010 (0.038)</td>
</tr>
<tr>
<td>( \text{YearsTraded}_t^2 )</td>
<td>-0.022 (0.038)</td>
<td>0.010 (0.003)**</td>
</tr>
<tr>
<td>CumulTrades ( \div 10^2 )</td>
<td>0.058 (0.059)**</td>
<td>-0.030 (0.005)**</td>
</tr>
<tr>
<td>CumulTrades ( \div 10^4 )</td>
<td>-0.0009 (0.0004)**</td>
<td>0.0002 (0.00003)**</td>
</tr>
<tr>
<td>NumSec ( t )</td>
<td>0.003 (0.024)</td>
<td>-0.008 (0.002)**</td>
</tr>
<tr>
<td>NumTrades ( t )</td>
<td>-0.021 (0.010)**</td>
<td>-0.0007 (0.0009)</td>
</tr>
<tr>
<td>PortVal ( t ) ( \div 10^6 )</td>
<td>0.618 (0.602)</td>
<td>-0.067 (0.602)</td>
</tr>
<tr>
<td>( \bar{R}_{t-1} )</td>
<td>-0.002 (0.001)*</td>
<td>-0.002 (0.001)*</td>
</tr>
</tbody>
</table>

| Observations | 13404 | 13404 | 17715 | 17715 |
| Adjusted \( R^2 \) (%) | 45.7 | 45.9 | 38.7 | 38.6 |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Individual Fixed Effects | Yes | Yes | Yes | Yes |
Table 7: Learning with Survival Controls

This table reports estimates of selection model regressions with the fixed effects modification developed by Wooldridge (1995). The regressions are of the form

$$\Delta y_{i,t+1} = \beta \Delta X_{i,t} + \rho_2 d_2 \lambda_{i,t} + \ldots + \rho_T d_T \lambda_{i,t} + \epsilon_{i,t},$$

where $\lambda_{i,t}$ are the inverse Mills ratios from a year $t$ cross-sectional probit model (the selection model) and $d_2, \ldots, d_T$ are time dummies. Including these variables in the learning regression accounts for the impact of the selection equation. The joint test of $\rho_t = 0$ for $t = 2, \ldots, T$ is a test of whether survivorship bias is a concern. The probit model reported in Column 1 is estimated with data from all of the years of the sample pooled together, but the inverse Mills ratios in the second stage estimates are estimated separately each year. The regressions in Columns 2 and 3 are estimated with all the variables in first differences, except the inverse Mills ratios. These first differences add fixed effects to the model. Experience is measured by years of experience (YearsTraded) and cumulative number of trades placed (CumulTrades). The dependent variable in the second stage is either the individual’s average return in the following year, $\bar{R}_{i,t+1}$, or the individual’s disposition coefficient, $\beta_{d,i,t-1}$. $X_{i,t}$ is a vector of controls including the number of trades placed by the individual in a given year (NumTrades), the number of securities held by the individual in a given year (NumSec), and the individual’s average total daily portfolio value (PortVal). $\bar{R}_t$, the investor’s average return, is excluded from the return regression due to the econometric problems that arise when lagged dependent variables are included in fixed effects models. Additional variables in the first stage selection model are $I(\text{Inherit} = 1)$, an indicator variable for whether the investor inherited shares in year $t$, and $\sigma_{R_t}$, the standard deviation of the investor’s returns. Data are from the period 1995 to 2003. Standard errors are in parentheses, and ***, **, and * denote significance at 1%, 5% and 10% respectively.

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>First Stage (All Years)</th>
<th>Second Stage (First Difference)</th>
<th>Second Stage (First Difference)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CumulTrades$_t$ ($\div 10^2$)</td>
<td>0.355 (0.021)***</td>
<td>0.010 (0.004)***</td>
<td>-0.019 (0.002)***</td>
</tr>
<tr>
<td>CumulTrades$_t^2$ ($\div 10^4$)</td>
<td>-0.002 (0.0001)***</td>
<td>-0.0003 (0.0002)***</td>
<td>0.0002 (0.00003)***</td>
</tr>
<tr>
<td>YearsTraded$_t$</td>
<td>0.376 (0.0196)</td>
<td>0.894 (1.766)</td>
<td>-0.099 (2.04)</td>
</tr>
<tr>
<td>YearsTraded$_t^2$</td>
<td>0.028 (0.007)***</td>
<td>-0.015 (0.021)***</td>
<td>0.008 (0.006)</td>
</tr>
<tr>
<td>NumSec$_t$</td>
<td>0.187 (0.001)***</td>
<td>0.034 (0.009)***</td>
<td>-0.010 (0.002)***</td>
</tr>
<tr>
<td>NumTrades$_t$</td>
<td>0.963 (0.062)***</td>
<td>-0.100 (0.012)***</td>
<td>0.002 (0.003)</td>
</tr>
<tr>
<td>PortVal$_t$ ($\div 10^6$)</td>
<td>0.003 (0.003)</td>
<td>0.220 (0.221)</td>
<td>-0.007 (0.007)</td>
</tr>
<tr>
<td>$\bar{R}_t$</td>
<td>0.002 (0.001)***</td>
<td>-0.003 (0.002)*</td>
<td></td>
</tr>
<tr>
<td>$I(\text{Inherit} = 1)$</td>
<td>0.191 (0.074)***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\sigma_{R_t}$</td>
<td>-0.617 (0.061)***</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Joint Test of $\rho = 0$ (from 1996-2003)

F(8, 6490) = 3.91
Pr>F = 0.0000
F(7, 8799) = 5.29
Pr>F = 0.0000

Observations 30218 6511 8818
Year Fixed Effects Yes Yes
Individual Fixed Effects Yes Yes
Table 8: Risk Taking and Experience

This table reports the results of fixed effect selection model estimates of regressions of various performance and risk measures on experience measures. The method of these regressions and their associated first-stage estimates are described in Table 7. The dependent variables include each investor's average 30-day risk adjusted return (alpha), and each investor's average beta coefficient on four factors—RMRF, SMB, HML, and UMD. These betas are estimated in the standard way, as described in the text. Each regression includes the control variables and inverse Mills ratios described in Table 7 but only the experience variables coefficients are reported in this table. Standard errors are in parentheses, and ***, ** and * denote significance at 1%, 5% and 10% respectively.

<table>
<thead>
<tr>
<th>Dependent Variables</th>
<th>30-day α</th>
<th>β on RMRF</th>
<th>β on SMB</th>
<th>β on HML</th>
<th>β on UMD</th>
</tr>
</thead>
<tbody>
<tr>
<td>YearsTraded</td>
<td>0.58</td>
<td>-0.034</td>
<td>-0.016</td>
<td>0.0544</td>
<td>0.0073</td>
</tr>
<tr>
<td></td>
<td>(0.41)</td>
<td>(.0108)***</td>
<td>(.0039)***</td>
<td>(.0435)</td>
<td>(.0250)</td>
</tr>
<tr>
<td>YearsTraded^2</td>
<td>0.02</td>
<td>0.00246</td>
<td>0.00163</td>
<td>0.0037</td>
<td>-0.00076</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(.0012)*</td>
<td>(.0005)***</td>
<td>(.0028)</td>
<td>(.0019)</td>
</tr>
<tr>
<td>CumulTrades (÷10^2)</td>
<td>0.05</td>
<td>-0.01</td>
<td>-0.002</td>
<td>-0.002</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td>(0.02)***</td>
<td>(.002)***</td>
<td>(0.0009)***</td>
<td>(.004)</td>
<td>(.005)***</td>
</tr>
<tr>
<td>CumulTrades^2 (÷10^4)</td>
<td>-0.0002</td>
<td>0.00005</td>
<td>0.00001</td>
<td>0.00002</td>
<td>0.00008</td>
</tr>
<tr>
<td></td>
<td>(0.0001)**</td>
<td>(0.00001)***</td>
<td>(0.000006)***</td>
<td>(0.00002)</td>
<td>(0.00002)***</td>
</tr>
<tr>
<td>Dep Var Mean</td>
<td>0.07</td>
<td>0.663</td>
<td>0.028</td>
<td>-0.086</td>
<td>-0.054</td>
</tr>
<tr>
<td>Standard Dev</td>
<td>3.30</td>
<td>0.288</td>
<td>0.103</td>
<td>0.431</td>
<td>0.440</td>
</tr>
</tbody>
</table>
This graph shows the number of accounts that place one or more trades in each year, including both accounts that exist at the beginning of the sample and new accounts. There is considerable variation in the number of accounts placing trades over time, from a low of around 54,196 in 1995 to a high of 311,013 in 2000.

This figure plots the average 30-day returns earned by investors in years following their first purchase. Investors are grouped into quartiles in their first year of trading, and we then calculate average returns for each quartile in subsequent years. Returns are calculated using the approach discussed in the text, and are demeaned by calendar year, which removes the impact of any year fixed effects. Raw returns are reported here, but the results for risk-adjusted returns are not qualitatively different.
Figure 3: The Disposition Effect in Aggregate

This graph shows how the propensity to sell a stock depends on the stock’s return since purchase. Each line plots the regression coefficients from one hazard regression modeling the conditional probability of selling a stock. The coefficients correspond to dummy variables for return ‘bins’ ranging from [−10, −9) percent to [9, 10) percent. In each year, there is a pronounced kink near zero, and the hazard increases rapidly for positive returns.
Figure 4: Returns by Disposition Quintile

This figure shows average 10-, 20-, 30-, and 45-day returns following a purchase for each disposition quintile. Returns are calculated using the approach discussed in the text. Raw returns are reported here, but the results for risk-adjusted returns are not qualitatively different. Returns earned by the lowest quintile (1) are higher than those earned by the highest quintile (5).

Figure 5: Proportion of Accounts Who Exit

This graph shows the percentage of investors who exit in 1 year, 2 years, . . . , or 6 years following the first year in which they place at least seven trades. Exit is defined as placing no further trades during our sample period. Within each year group, the first bar indicates the percentage of investors who exited in the 1st year after placing a trade, the second bar indicates the percentage of investors who exited in the 2nd year, and so on. Data are missing for later years because we do not know how many investors stopped trading after 2003.
Figure 6: Trading Intensity and Experience

This figure shows how trading intensity changes with experience. Intensity is measured as the number of trades placed (dark blue) and the total value of trades placed (light blue; in 10,000's of EUR). Results are demeaned by calendar year to adjust for year fixed-effects. We report results beginning in the investor's first full calendar year in the sample.