Learning By Doing: The Value Of Experience And The Origins Of Skill For Mutual Fund Managers

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Abstract

Learning by doing matters for professional investors. We develop a new methodology to show that mutual fund managers outperform by a risk-adjusted 1.5% per quarter in industries where they have experience. The key to our identification strategy is that we look "inside" funds and exploit heterogeneity in experience for *the same manager at a given point in time* across industries. As fund managers become more experienced, their trades become better predictors for abnormal stock returns around subsequent earnings announcements. Our approach identifies experience as a first-order driver of observed mutual fund manager skill.

JEL Classification: G02, G23, D83, J24

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When the markets act up like this, one natural reaction is to rely on the insights of experienced managers. The argument goes that, because they have been around the block a few times, they'll be able to navigate their funds better this time around. (From: Wall Street Journal (2010))

1. Introduction

Driving a car, flying an airplane, or writing an academic paper, are examples of activities in which learning by doing is important.[1] Most people are not born natural drivers, pilots, or scholarly writers – instead, they acquire the skill as they drive, fly, or write. Even controlling for general ability, there are likely large differences in performance between someone who, say, drives very little, and someone who drives a lot. As consumers, we value experience highly, and often prefer an experienced pilot (or dentist) to an inexperienced one. While learning by doing and experience obviously play a role in many contexts, little work exists that analyzes the value of experience for top-level economic decision makers. Our paper aims to fill this gap by studying mutual fund managers. The mutual fund industry is a market segment of first-order economic significance, which as of 2011 manages almost \$12 trillion dollars of investor wealth, or, alternatively, 23% of all assets of U.S. households (2012 Investment Company Fact Book). We exploit unique features of the mutual fund industry, and the available mutual fund data, to provide novel, comparatively clean, evidence indicating that learning by doing effects matter for this important set of professional investors.

Identification is the main challenge for any study on the value of experience and the impact of learning on output, because learning is unobservable. For instance, at first glance tenure might seem a reasonable proxy for fund manager experience. However, tenure could also proxy for effort, because junior managers might need to work harder to signal their type (e.g., Chevalier and Ellison (1999)). Moreover, if bad managers are eliminated by competition, or if the best managers go work for hedge funds (e.g., Kostovetsky (2010)), tenure is correlated with general ability.

[1] Learning by doing as a concept has a long history. Early writings emphasized the effects of learning by doing on educational outcomes (e.g., Dewey (1897)) and increases in individual worker productivity (e.g., Book (1908)). Starting with Arrow (1962), the concept has been applied to the study of firms and often refers to unit costs being a decreasing function of prior output (e.g., Bahk and Gort (1993)). The economic literature on learning by doing is too large for us to review here; we refer the reader to available surveys, such as Thompson (2010).

Further, managers with longer tenure might have a different standing within their organization, leading to different agency issues and explicit or implicit contractual arrangements influencing investment behavior and performance. For example, they might be overly conservative (e.g., Prendergast and Stole (1996)) or subject to greater risk of being fired for underperformance (e.g., Dangl, Wu, and Zechner (2008)). Lastly, tenure is correlated with age, which is again correlated with many other variables including cognitive ability (e.g., Korniotis and Kumar (2011)). In sum, it is extremely hard to identify the incremental value of experience using simple proxies like tenure or age. This is a central difficulty in all empirical work on learning by doing.

We develop a new approach to identifying the marginal impact of experience on mutual fund manager performance, building on two main ideas. First, we construct measures of experience, discussed in detail below, that are not linear functions of time. Age and tenure change one-for-one with calendar time (exactly so for age; approximately so for tenure). A key source of the identification problems highlighted above is the fact that many other variables are also highly correlated with calendar time. Our experience measures get around this problem. Second, we decompose a mutual fund into a collection of smaller industry sub-portfolios (ISPs). For example, instead of thinking of manager m as managing fund f in quarter q, we think of her as managing a healthcare ISP (the stocks held by fund f belonging to the healthcare industry) and a telecom ISP (the stocks held by fund f belonging to the telecom industry). If the level of experience differs across ISPs, we can use variation in industry experience *within* fund managers *at a given point in time* to identify the impact of experience on fund returns. The advantage of this strategy is that we do not need to rely on variation across managers, or across time, leaving us less exposed to the sort of omitted variable concerns described above. Fixed effects allow us to eliminate the confounding impact of all variables that do not vary across ISPs for a given manager-date combination. Important confounding factors we can thus exclude are, for example, general ability, educational background, tenure, age, fund characteristics, fund family characteristics, corporate governance at the fund level, and the overall state of the economy.

Our main results are as follows. Unconditionally, ISPs under an experienced fund manager outperform other ISPs by about 1.2% per quarter net of risk. By construction, this difference reflects pure *stock-picking* skill and is *before fees*. Regressions using manager×date fixed effects indicate a risk-adjusted performance difference of 1.5% per quarter between ISPs managed by managers with and without experience, respectively. This suggests that learning by doing and experience are first-order drivers of fund returns.

While our main tests are based on risk-adjusted returns and pay particular attention to identification issues, even the raw data indicate that experienced managers outperform. As Figure 1 (Panel A) shows, investing $1 in a hypothetical portfolio of all experienced-manager ISPs in 1992Q1 delivers about $8.5 at the onset of the recent financial crisis (2007Q3). In contrast, investing with inexperienced managers yields only $6.7. As of 2008Q4 (the end of our sample), the experienced-managers portfolio yields about $5 while the inexperienced-managers portfolio only $4, i.e. 20% less.

In deriving the experience measure, our main conjecture is that experience builds up mostly in difficult environments. Hence, a fund manager who navigates through a period of severe underperformance in a given industry (henceforth, an "industry shock") will gain more experience in that industry than if nothing unusual happens. That is, intuitively, we assume that fund managers resemble airplane pilots who gain experience not from plain sailing, but from flying through turbulence. This conjecture is directly motivated by seminal work on learning by doing by Arrow (1962), who writes: "Learning is the product of experience. Learning can only take place through the attempt to solve a problem and therefore only takes place during activity." It is also consistent with Malmendier and Nagel (2011) who show, among other things, that individuals who are personally experiencing economic shock periods exhibit persistently different patterns in their financial decision making than otherwise similar individuals. We operationalize this idea by recording industry-wide shocks, defined in detail below, for each industry and quarter in our dataset. We then use the number of past industry shocks observed by a manager over her career as a proxy for her experience in a given industry. The important feature of our experience definition is that it is not a linear function of time, i.e. the same manager might have more experience in the healthcare industry than in the telecom industry, *at the same point in time*.

Our results show that outperformance during industry shocks accounts for about one third of the overall effect of fund manager experience. This accords well with a learning interpretation: if the manager has experienced bad times in an industry, she does better next time around (see also the opening quote). Also consistent with a learning interpretation, we document that obtaining more experience is beneficial at a decreasing rate.

Placebo tests, in which we randomly assign "learning" periods, suggest that our results are not spuriously induced by our methodology. Further tests rule out industry return reversals, industry fixed effects, or industry-specific managerial skill as alternative explanations for our findings. We provide empirical support for our conjecture that experience and learning build up in bad times in particular, by showing that there are no detectable effects when we base our experience measure on industries that have performed best, good, average, or below average, but not worst.

Exactly how does experience translate into higher returns? While fully answering this question is a topic for future research, we provide a partial answer by analyzing holdings changes in anticipation of earnings announcements. We find that experienced fund managers trade in the direction of subsequent earnings surprises, and that they increase their holdings more before large earnings announcement returns. This suggests that one channel through which experience leads to higher returns is an enhanced ability to interpret and act upon news around earnings announcements. Because changes in holdings reflect active managerial decisions this test provides direct evidence that our experience measure captures fund manager learning.

As a final step, we develop an experience index (EDX), i.e. an aggregate experience measure across all industries for a given manager. EDX is a purely backward looking measure that can be constructed in real time. Funds that score highest on EDX obtain positive risk-adjusted returns before fees, and break even after fees. Funds that score low on the index break even before fees, and underperform after fees. A long-short portfolio earns 4-factor risk-adjusted returns of 2.4% per year after fees. Figure 1, Panel B, shows again that these patterns can be observed even in the raw data. The figure shows after-fee performance of high and low EDX funds relative to the market across our sample period.

1.1 Contribution to the Literature

To the best of our knowledge, our paper is the first to focus exclusively on identifying the value of experience in the mutual fund industry. However, a number of papers in the mutual fund manager characteristics literature contain related results. Chevalier and Ellison (1999) find evidence that managers graduating from more prestigious colleges outperform, but they find no robust results for tenure. This is in contrast to earlier results by Golec (1996) who reported a positive tenure effect. Ding and Wermers (2009) find that managers with longer tenure outperform in large funds, which might have better governance structures, but underperform in smaller funds. Greenwood and Nagel (2009) document that young and old managers had different investment and return patterns for technology stocks during the late 1990s "tech bubble." Because our study uses variation within managers at a given point in time, our effects are orthogonal to the age, tenure, and skill effects that were the focus of these earlier studies.

Our study also contributes to the growing literature on investor learning. One strand of the literature analyzes rational learning theories (e.g., Mahani and Bernhardt (2007), Pastor and Veronesi (2009), Seru, Shumway, and Stoffman (2010), Linnainmaa (2011), Huang, Wei, and Yan (2011)). Another strand looks at alternative learning theories, such as, for example, naïve reinforcement learning (e.g., Kaustia and Knüpfer (2008), Barber, Lee, Liu, and Odean (2010), Chiang, Hirshleifer, Qian, and Sherman (2011), Bailey, Kumar, and Ng (2011), Campbell, Ramadorai, and Ranish (2013)). Malmendier and Nagel (2011) show that past macroeconomic shocks shape future financial decisions. This learning literature has mainly focused on individual investors and retail investors. Our study introduces new results on the relevance and profitability of learning for professional investors.

Lastly, our study contributes a new econometric approach to identifying fund manager skill (e.g., Berk and Green (2004), Fama and French (2010)). Our results show that experienced managers can outperform passive benchmarks via stock-picking, which adds to a body of work suggesting that at least some funds can systematically outperform.[2] Our study is related to Kacperczyk, Sialm, and Zheng (2005), who show that mutual fund managers who concentrate their holdings in some industries have higher alphas (we show below that our experience effects obtain even after controlling for industry concentration). As we show that experience from industry shocks is particularly valuable in future industry shocks, our findings can help explain why mutual funds on average tend to do better in recessions (e.g., Moskowitz (2000), Glode (2011)).

While many papers focus on identifying whether skill exists, fewer ask where it comes from. Skill could be related to time-invariant factors like IQ (e.g., Chevalier and Ellison (1999), Grinblatt and Keloharju (2012)) and measured skill could be time-varying because boundedly rational managers find it optimal to allocate attention differently over assets across the business cycle (e.g., Kacperczyk, Nieuwerburgh, and Veldkamp ((2011), (2012))). In this paper, we add a new dimension by proposing that two otherwise identical fund managers can have different skill, in terms of performance, because their specific employment histories have exposed them to different learning opportunities. Our results show that experience can be (i) theoretically important for understanding the origins of mutual fund manager skill and (ii) a powerful predictor of fund performance.

On a broader level, our work addresses two central problems for the empirical literature on learning by doing identified in a recent survey by Thompson (2010): How to separate learning by doing from pure time, age, and size effects?, and: How to surmount empirical problems due to the poor quality of productivity data typically available to researchers? Our study directly tackles both of these problems. By using variation within manager-date cells as a source of identification, our approach minimizes omitted variable concerns. Further, our mutual fund data are close to ideal in many respects: (i) fund managers make economically substantial decisions,

[2] This literature is too large for us to review it here. Papers include Daniel, Grinblatt, Titman, and Wermers (1997), Cohen, Coval, and Pastor (2005), Kacperczyk, Sialm, and Zheng (2005), Bollen and Busse (2005), Kacperczyk and Seru (2007), Cremers and Petajisto (2009), Baker, Litov, Wachter, and Wurgler (2010), Berk and van Binsbergen (2012), Koijen (2012). See e.g., Wermers (2011) for an excellent survey.

(ii) they are appropriately incentivized to do well, (iii) we observe the same individual repeatedly in an almost identical decision making environment, (iv) we can observe multiple decisions for the same manager at the same time, and (v) mutual fund performance measures provide a reasonably accurate real-time productivity gauge.

We describe our method and the dataset in detail in Section 2. Section 3. presents our main results on fund manager experience and fund performance as well as robustness checks. In Section 4. we examine trading by experienced fund managers around earnings announcements. In Section 5. we address potential alternative explanations and present two extensions: we investigate if learning occurs in booms and other periods and we show that our results are similar when we focus on the time-series of industry returns to define the industry shock measure. The final section concludes.

2. Method and Data

In this section, we first illustrate our approach and explain how we identify experience from looking at individual industry components of fund portfolios. We then describe in detail how we construct our main experience measure based on industry shocks. Finally, we explain how we measure performance for industry sub-portfolios, and describe the dataset.

2.1 Experience and Learning

To fix ideas, consider a simple Bayesian learning model. In order to optimize her portfolio, a fund manager needs to form a prediction of the expected return of a stock, denoted by $\tilde{r}$. Her prior beliefs are that the return is normally distributed with mean r_0 and variance σ_0^2. An essential part of the fund manager's job is to process signals about $\tilde{r}$ and to update her beliefs accordingly. Suppose the manager obtains N independent signals, $s_n = \tilde{r} + \eta_n$, where η_n is normally distributed, has zero mean, and variance σ^2. Posterior precision (the inverse of the posterior variance) is then given by:

$$\rho_N = \sigma_0^{-2} + N\sigma^{-2}. \tag{1}$$

The precision of the estimate therefore increases with the number of signals N, independently of the realization of the signals. In other words, learning reduces uncertainty.

If, all else equal, a manager who is less uncertain about the environment she operates in outperforms other managers, the returns will be a function the number of signals received. Specifically, if risk-adjusted fund returns α are an increasing function of the precision, i.e., $\alpha'(\rho_N) > 0$, then, all else equal, equation (1) predicts that a fund managed by manager m_1 should outperform a fund managed by manger m_2 if $N_{m_1} > N_{m_2}$.

To make the simplest possible assumption that allows us to separate our approach from alternatives in the literature, assume that N can be written as:

$$N = T + S_0 + E. \tag{2}$$

Here, T denotes tenure and captures the idea that the manager will mechanically observe more signals – and therefore have more precise beliefs about $\tilde{r}$ – if she has a longer tenure. The second component, S_0, captures that some managers will have higher baseline skill than others; e.g. they are more intelligent, or have received their education from an elite college, etc. The subscript 0 indicates that baseline skill is time-invariant and fixed. In our formulation, baseline skill is like receiving S_0 additional signals, i.e. an individual with higher IQ or better education starts with a more precise estimate of $\tilde{r}$. E denotes experience.

The existing literature has mainly focused on the first two components (see Section 1.1). The innovation in our study is the third one: E. It captures that managers will not learn equally in every period. In some periods, more information will be produced, and the manager therefore receives more signals. Using the example from the introduction, while a pilot may learn something from flying in perfect conditions, she might learn much more from successfully navigating her plane through turbulent conditions. Hence, E captures the cumulative impact of exposure to past periods that produced more signals than would be predicted by the passage of time alone (captured by T). Formally, $E = \sum_{\tau<t} e_\tau$, where e_τ is a period t indicator variable equal to 1 if period τ was a high-learning period and 0 otherwise. We refer to E as *experience*, with the implicit understanding that it is actually "excess" experience, unrelated to the pure passage of time.

Experience varies not only by time, but also by industry. For example, a fund manager who was exposed to bank stocks in the fourth quarter of 2007 (when bank stocks fell by almost 10%) might have a different learning experience compared to a manager in business equipment in the same quarter (the return on business equipment stocks was 0.1%). The central idea of our approach is to exploit variation of experience across industries i managed by manager m in quarter q. To illustrate, suppose that manager m has 50% of her fund's portfolio invested in industry 1 and the remaining 50% in industry 2. We call these industry-related parts of the portfolio *industry sub-portfolios* (ISPs). Writing the above in terms of a simple reduced-form model of ISP returns yields for each $i \in \{1, 2\}$:

$$\alpha_{mqi} = \beta_1 T_{mq} + \beta_2 S_{0,m} + \beta_3 E_{mqi} + \Gamma' B_{mq} + \varepsilon_{mqi}, \tag{3}$$

which states that the risk-adjusted ISP return α_{mqi} of manager m in quarter q is a function of the components of N in equation (2), with the key difference that experience is now allowed to vary on the ISP level. In addition, the model also allows for an arbitrary set of variables, $\Gamma' B_{mq}$, that can vary across both managers and quarters. As discussed, this set of variables includes a large range of covariates studied in the literature, including manager age, fund characteristics, fund governance, and the state of the economy. Of course, as an empirical matter, the β's as well as Γ could be zero, in which case alphas would reflect pure luck.

With only two ISPs, we can take the differences between them for manager m at quarter q to get:

$$(\alpha_{mq1} - \alpha_{mq2}) = \beta_3 (E_{mq1} - E_{mq2}) + (\varepsilon_{mq1} - \varepsilon_{mq2}). \tag{4}$$

Equation (4) shows that we can eliminate the effect of tenure and baseline skill entirely if we compare the performance of two ISPs for the same manger at the same point in time. In addition, this wipes out all other (potentially time-varying) variables, B_{mq}, that do not vary for a given manager across ISPs in a given quarter. The coefficient of interest is β_3 and our key prediction is $\beta_3 > 0$, i.e. we conjecture that higher ISP alphas are a function of more ISP experience. Equation (4) captures the intuition of our approach for two industries. Since most managers manage more than two ISPs in a given quarter, we implement the same idea by estimating equation (3) with a full set of manager$\times$quarter fixed effects.

Focusing on within-manager variation across industries at a given point in time is useful because it addresses some of the challenges for existing approaches. First, it ensures that many potentially important omitted variables captured in $\Gamma' B_{mq}$ are not affecting the estimates. Second, by looking within managers, we can completely eliminate the impact of baseline skill $S_{0,m}$, and are therefore not subject to measurement error in ability proxies. Third, the approach can minimize sample selection concerns since identification comes from variation within managers *at a given point in time* and is therefore independent of T_{mq}.

We assume that tenure of the fund manager and baseline skill do not vary across ISPs for the same manager and quarter. This is trivially satisfied for the tenure and skill variables used in the prior literature: the number of years worked for, say, Fidelity, or the fact that the manager obtained a degree from an elite college do not vary across ISPs. At least conceptually, however, we could think of the manager-industry-specific tenure and manager-industry-specific baseline skill. We discuss in Section 5. why we believe that these factors, if they exist, do not impact our inferences.

2.2 Experience Proxy Based on Industry Shocks

To implement our approach, we need an experience measure that is not a linear function of time and that varies across industries for a given manager-quarter combination. The American Heritage Dictionary of the English Language (2000) defines experience as: "Active participation in events or activities, leading to the accumulation of knowledge or skill," suggesting that a defining feature of experience relates to having to act in a particular period or event. This fits well with the quote by Arrow (1962) cited in the introduction: "Learning can only take place through the attempt to solve a problem and therefore only takes place during activity." Both definitions highlight that experience is not something that just accumulates with the passage of time.

When will a fund manager be particularly "active" and "working towards solving a problem"? We conjecture that managers are relatively active (in the sense of actively thinking about their industry portfolio, not in the sense of active fund management) during times of extreme market movements, and that problem solving becomes particularly relevant in downturns. Building on the idea that learning is likely to occur in bad times, our experience measure counts the number of times a manager has experienced what we label *industry shocks*.[3]

We consider different industry shock definitions. In our baseline definition, a shock occurs in a given industry and quarter, if the value-weighted industry return is the lowest across all 12 Fama-French industries in the quarter. This is in line with fact that rankings and relative performance are of particular importance in the mutual fund industry (e.g., Brown, Harlow, and Starks (1996)). Note that using market-wide shocks, like, for example, NBER recessions, is not possible in our setting, since our approach eliminates all shocks that do not vary across industries at a given point in time.

Several advantages come with this definition of industry shocks. First, there might be substantial new information released in industry shock quarters enabling fund managers to learn (e.g., the substantial new information about shadow banking in the recent financial crisis). To the extent that industry shocks are related to industry fundamentals, managers have a chance

[3]The idea that experience from past industry shocks would have a strong impact on future financial decisions resonates well with Malmendier and Nagel (2011) who document that individuals who have experienced periods of particularly low stock market returns show different investment patterns and stock market participation rates than individuals who have not. Our study is different from theirs because they focus on macroeconomic shocks, while we focus on industry-specific shocks. A second difference is that they focus on differences in risk-taking behavior across individuals conditional on past experience, while we focus on *risk-adjusted* returns across ISPs managed by the same individual at the same point in time. Malmendier and Nagel also analyze experience from positive shocks, which we investigate in our setting in Section 5.2.

to learn something from the shock that is useful next time a similar event occurs. Second, the worst performing industries are very salient to investors, and thus for fund managers too. Hence, industry shocks are times in which fund managers are likely to actively study ways to minimize the shock's impact. Third, the media generally focuses disproportionately on extreme events. This might both increase the amount of valuable new information produced and make the industry performance more salient to investors. Finally, having one industry shock per quarter gives us considerable statistical power.

Table 1 lists industry shock quarters from 1992 to 2008. The number of industry shocks is not the same for all industries. This is a desirable feature of the definition, since it is plausible that learning opportunities are greater in some industries than others. We will, however, also use alternative definitions in our robustness checks, with a more even distribution of shocks across industries. A second notable feature from the table is that we assign the label "industry shock" also to quarters with positive returns (e.g., utilities in 1997Q2, with industry return 5.5%). This is adequate if managers, investors, and the media care mostly about the relative ranking of industries. We leave these quarters in our sample to be conservative and minimize our degrees of freedom, but we show in the robustness checks that our results get stronger when we impose the additional restriction that an industry shock quarter must have a negative industry return. We further require that the shock has a meaningful effect on the overall fund return, and restrict the attention to industries that represent a portfolio weight greater than 10% of assets under management in the fund at the end of the previous quarter. (In the robustness section we show that using the top 3 industries yields essentially identical results.) Thus, the experience measure for fund manager m in industry i and quarter q is:

$$\mathrm{E}_{mqi} = \sum_{\tau<q} \mathrm{IS}_{i\tau} \times \mathrm{I}[w_{m,\tau-1,i} > 0.1], \tag{5}$$

where IS stands for an industry shock in industry i in quarter τ, $\mathrm{I}[w_{m,\tau-1,i} > 0.1]$ is an indicator equal to one if the weight of industry i in the fund managed by fund manager m at the end of quarter $\tau - 1$ exceeds 10%. Whenever there are two consecutive industry shock quarters for an industry, we update E_{mqi} after the second industry shock quarter. E_{mqi} varies within a manager-quarter (mq) cell because a fund typically invests in multiple industries and because a fund manager can have different levels of experience in different industries. It is precisely this variation that we are seeking to exploit in our tests below.

While the industry weight is in principle chosen by the manager, we argue that making the experience measure contingent on lagged industry weight is innocuous for three reasons. First, if the industry shock is an unanticipated event, exposure to it, and therefore E_{mq}, is exogenous. Second, if the most skilled or experienced managers could anticipate the shock, they would scale back their exposure. This would bias us against our null, which is that managers with high values of E_{mqi} outperform. Third, we can directly show, using the characteristics-timing measure of Daniel, Grinblatt, Titman, and Wermers (1997), that there is no evidence in the data to suggest managers can successfully engage in industry-market-timing. It should be noted that this does not contradict the hypothesis that some managers can pick stocks with superior performance *within* industries.

The main alternative definition of industry shocks we consider exploits the time-series instead of the cross-section of industry returns. There, we define an industry shock quarter as one in which the industry return is among the worst in the industry's recent history. We find both polar cases (pure "cross-sectional learning" and pure "time-series learning") plausible, and we obtain similar results with both. We focus on the cross-sectional definition in particular, because it sets our work most clearly apart from papers in the literature that documents higher alphas in recessions. By construction, our cross-sectional effects cannot be due to recessions and the business cycle.

2.3 Data

We merge a number of sources: the CRSP Survivorship-bias Free Mutual Funds Database, the Thomson Reuters Mutual Fund Holdings Database, and the CRSP Monthly Stocks database.

The starting piece of information is the fund manager's identity, provided by the CRSP

Mutual Funds Fund Summary table. Coverage of manager names is sparse before 1992, so we choose this year as the starting point. To be able to focus on individual fund manager experience, we restrict the attention to funds that are managed by a single manager, as opposed to a team, and we keep only managers that do not manage multiple funds. We further focus on actively managed equity funds (excluding index funds, identified by the CRSP mutual funds database's index fund flag), with total net assets under management of at least $5 million.

We manually screen manager names for different spellings, typos, etc. In some cases, a given fund is "intermittently" managed by a team: for example, the Dreyfus Premier S&P Stars Opportunities Fund is managed by Fred A. Kuehndorf in 2006, by a team including Fred A. Kuehndorf in 2007, and again by Fred A. Kuehndorf in 2008. In all such cases, we assign the long-run individual fund manager as the actual manager for the team-managed years, i.e. in our example Fred A. Kuehndorf is the fund's manager for 2007. As a result of this screening procedure, we obtain 3,197 unique fund manager identifiers. Individual funds are identified based on the CRSP Mutual Funds *CRSP_Portno* portfolio identifier, which eliminates "redundant" information about different classes of the same fund.

We merge these data, using the MFLinks database, to the mutual funds' quarterly holdings in the Thomson Reuters Mutual Fund Holdings Database. Further, we assign each stock in a given fund's portfolio to one of the Fama-French 12 industries, using the stock's historical SIC code (SICH) reported in the Compustat Fundamental Annual database (if available), or the SIC code reported in the CRSP Monthly Stocks database.

Table 2, Panel A describes our sample. We have a total of 68 quarters, 3,197 fund managers in 2,503 funds and 38,267 unique ISPs (i.e. fund-manager-industry links). Funds have on average 9.2 ISPs per quarter, and an ISP "lives" for, on average, 8.8 quarters (median = 6.0). Managers are on average in our sample (managing any ISP) for 11.4 quarters (median = 8.0), similar to the findings of Chevalier and Ellison (1999). Our baseline setting uses the cross-sectional definition of industry shocks from Table 1 and the experience measure defined in equation (5). Panel B presents summary statistics for the industry shock indicators (IS) and the experience measure across all 336,163 manager-industry-quarter observations. About 8% of our observations come from industry shock quarters. The average of the experience measure is 0.25, and the maximum number of industry shocks experienced by a manager in our sample for a given industry is 9.

2.4 Measuring ISP Performance

Fund holdings are only observed at the quarterly frequency and industry sub-portfolio returns are not separately reported. We therefore calculate ISP performance as follows.

We fix the weights for each stock in each fund's ISP in quarter q to be equal to the dollar amount the fund has invested in the stock at the end of quarter $q-1$ divided by the total dollar amount invested by the fund in all stocks in a given Fama-French industry. Using daily stock returns from CRSP, we calculate the daily ISP return by aggregating the individual stock returns at the industry level. This gives us a series of daily ISP raw returns for all ISPs in our sample. Formally, for every stock j, industry i, and day t in quarter q we define the ISP raw return as:

$$R_{mti} = \sum_{j \in i} w_{mij,q-1} R_{jt} \qquad \forall t \in q, \tag{6}$$

where $w_{mij,q-1}$ is the weight of stock j in the industry i ISP by manager m at the end of the quarter $q-1$.

The ISP performance measure is the α from the following regression which we run across all days t for each ISP in quarter q:

$$R_{mti} - R_{ft} = \alpha_{mqi} + b_{mqi}\mathrm{RMRF}_t + s_{mqi}\mathrm{SMB}_t + h_{mqi}\mathrm{HML}_t + m_{mqi}\mathrm{UMD}_t + \varepsilon_{mti}. \tag{7}$$

R_{mti} is the return from equation (6), R_{ft} is the risk-free rate, and RMRF, SMB, HML and UMD are the Fama and French (1993) and Carhart (1997) factors, respectively. We multiply α_{mqi} by 63 trading days and refer to this number as the risk-adjusted ISP return. In addition to the 4-factor model, we will also report CAPM and 3-factor results, as well as results using the characteristic-adjusted performance measures of Daniel, Grinblatt, Titman, and Wermers

(1997).

α_{mqi} is a measure of pure stock-picking skill. It does not capture market timing: moving money in and out of the industry will not affect the percentage returns we look at. Further, because we observe holdings only at quarterly frequency, our approach neglects managerial actions and trading within the quarter. This biases us towards understating the impact of experience, since we are implicitly limiting the channels through which experience can feed into returns. Finally, the measure is before fees and other items like trading costs or revenues from securities lending.

The weighting scheme in equation (6) implicitly assumes that managers rebalance their portfolio daily towards a target level proxied for by the previous quarter industry share. This is consistent with standard models of asset allocation, but likely overstates the (unobservable) frequency with which managers actually rebalance. To show that our results are not sensitive to the rebalancing assumption, we have rerun all our tests using the alternative assumption that managers do not rebalance at all within the quarter and that they let the portfolio weight float with the underlying stock returns. We find that the results are virtually unchanged (see robustness checks section).

3. Baseline Results

3.1 Sample Splits

Table 2, Panel C presents summary statistics for the main variables of interest when we split the sample between ISPs with no experience ($E = 0$) and experience ($E > 0$). Conditional on $E > 0$, the mean experience level is 1.56. This group represents about 15% of our total observations. The average ISP has an alpha before fees of 29 to 58 basis points per quarter depending on the risk-adjustment (CAPM, 3-factor, and 4-factor). This is roughly in line with the fund-level estimates reported in Kacperczyk, Sialm, and Zheng (2005). More interestingly, we find a considerable difference in alphas between "experienced" and "inexperienced" ISPs.

The 4-factor alpha for experienced ISPs is 119bps higher than for inexperienced ISPs. This is consistent with the hypothesis that fund managers learn from experience and provides the basis for our more detailed analysis in the following sections.

We also report the average ISP factor loadings. Experienced ISPs have somewhat higher betas and load significantly less on value, size and momentum. These loadings explain why we see the largest difference in risk-adjusted returns for the 4-factor model.

Average ISP size is \$90 million and the average fund in our sample has \$953 million assets under management. Experienced ISPs are larger, and are typically part of larger funds. They are also associated with larger industry shares, i.e., funds hold more of their assets in experienced industries. This is partly by construction, because we require that the industry share exceeds 10% in order for experience to increase. Funds with experienced ISPs are slightly less diversified across industries according to the Hirschman-Herfindahl Index (HHI) formed on industry shares across all industries of a fund in a given quarter.

We follow Kacperczyk, Sialm, and Zheng (2005) and compute an Industry Concentration Index (ICI), defined for each fund-quarter as the sum of the squared deviations of the industry share of the fund from the average industry share across all funds in this industry and quarter. Although there is quite some variation in the ICI across funds (std = 11.7), there is virtually no difference in ICIs across funds with experienced and non-experienced ISPs, which shows that our experience measure captures a different aspect of the data. Interestingly, experienced ISPs deviate more from the average fund holdings than their inexperienced counterparts, based on the ICI components (squared deviations of the industry share of an ISP from the average industry share across all ISPs in this quarter and industry). This is again consistent with a learning interpretation: more experienced managers would use their information advantage to deviate from the herd.

Finally, and not surprisingly, fund managers with more experience have longer tenure (quarters for which a manager is in our dataset) and longer industry tenure (quarters the manager is in industry i). Since most funds hold most industries in most quarters, the tenure variable is only slightly larger than the industry tenure for the average manager.

3.2 Sorts

In this section we further investigate the source of the relative outperformance of managers with past industry experience. To do this we sort alphas into groups by experience and industry shock quarters. Table 3 presents results. The ISPs of experienced managers perform better both in industry shock quarters and other quarters. The performance of inexperienced ISPs decreases on average by between 4.9% and 8.2% in industry shock quarters, depending on the risk-adjustment. This is in contrast to the experienced ISPs, which fall by much less. The impact of the different factor loadings documented in Table 2 proves to be important in this case as well, since the alpha increases from –6% to almost zero for experienced managers in industry shock quarters once we control for value, size and momentum.

Table 3 shows that a about one third of the documented performance differential between experienced and non-experienced ISPs comes from the fact that experienced ISPs are able to break even on a risk-adjusted basis during shock quarters, while non-experienced ISPs make larger losses. To see this, recall from Table 2 that about 8% of observations were industry shock quarters. Focusing on the 4-factor adjustment, on average 33bps ($= 0.08 \times 4.18$) come from outperformance in industry shock quarters. While outperformance in industry shock quarters is particularly high, industry shocks are rare events. Therefore, the largest part of the overall difference in ISP performance can be attributed to the fact that experienced managers do better in their ISPs outside industry shocks. Specifically, this amounts to 89bps ($= 0.92 \times 0.97$; the difference between the total here and the 119bps reported in the previous section is due to rounding).

3.3 Regression-Based Evidence

The sorting results from the previous section show that ISPs managed by experienced managers outperform. While this is in line with a learning interpretation in which managers learn from past industry shock experience, we need to make sure that the results are not driven by variables that are correlated with experience. For example, Table 2 shows that managers in experienced ISPs have longer tenure, and that they are managing larger funds that are more diversified according to the HHI. Moreover, if competition eliminates bad managers over time, and if the average ability of managers therefore increases with tenure, the results might reflect the effect of manager baseline skill, such as IQ or elite education. Skilled managers would have longer tenure and would therefore mechanically be exposed to industry shocks, which would increase their experience measure.

To show that these fund-level factors are not driving our results, we estimate the following version of equations (3) and (4):

$$\alpha_{mqi} = \lambda_{mq} + \beta_1 I(E_{mqi} > 0) + \beta_2' X_{mqi} + \varepsilon_{mqi}. \tag{8}$$

Here λ_{mq} is a full set of manager $\times$ quarter fixed effects, $I(E_{mqi} > 0)$ is a dummy variable equal to 1 if E_{mqi}, the experience of manager m in industry i in quarter q, is greater than zero, and X_{mqi} is a vector of control variables. The main coefficient of interest is β_1 which captures the impact of experience, i.e., having experienced at least one industry shock event in the past. We use $I(E_{mqi} > 0)$ to be consistent with the previous tables and show in the robustness checks that replacing the experience dummy with E_{mqi} itself yields very similar results.

In this model, β_1 is identified if experience levels vary for the same manager across ISPs. The manager $\times$ quarter dummies ensure that the estimates are not driven by any variable that is fixed for the same manager in a given quarter, which, as highlighted above, includes in particular the impact of tenure and baseline skill. We control for the presence of an industry shock in a quarter because experienced ISPs, by definition, have experienced more particularly low industry returns. We allow standard errors to be correlated across ISPs managed by the same manager and across ISPs in the same industry in a given quarter, i.e. they will be of the general form:

$$\varepsilon_{mqi} = \nu_{mq} + \nu_{qi} + \bar{\nu}_m + \bar{\nu}_q + \eta_{mqi}, \tag{9}$$

where $\bar{\nu}_m$ and $\bar{\nu}_q$ are manager and quarter fixed effects and ν_{mq} and ν_{qi} are idiosyncratic factors on the manager-quarter and industry-quarter level, respectively. The manager × quarter dummies parameterically control for $\bar{\nu}_m$, $\bar{\nu}_q$, and ν_{mq} (because neither of these variables varies within manager-quarter cell), and we capture ν_{qi} by clustering at the industry-date level (Petersen (2009)).[4]

Table 4, Panel A presents presents our main results. The difference in risk-adjusted performance between experienced and non-experienced ISPs is 1.50% per quarter using the 4-factor risk adjustment. This effect is economically large and shows that the unconditional 4-factor alpha difference of 1.19% documented in Table 2 cannot be explained by time-invariant factors on the manager-quarter level, such as tenure and skill. The fact that experienced ISPs have much lower loadings on value, size, and momentum shows up also here as results get stronger in magnitude and significance once we risk-adjust performance for these factors. Finally, an F-test shows that the null hypothesis of the manager × quarter dummies being jointly zero can be rejected at any conventional significance level (p-value < 0.001).

In Panel B, we replace the experience dummy by a set of dummies equal to unity if E_{mqi} is equal to one, two, or more than two, respectively.[5] This non-linear specification allows us to test the incremental impact of additional units of experience. The panel shows that the first unit of experienced is most valuable and that additional units of experience increase relative outperformance at a decreasing rate. This is exactly what we would expect if our experience variable captures learning effects.

We repeat the analysis from Panel A including an interaction term between the experience measure and IS and present the results in Panel C. We can therefore estimate the differential effect of experience inside and outside of industry shock quarters. As in the univariate sorts, managers with past industry shock experience do much better than inexperienced ones in subsequent industry shocks. Conditional on being in an industry shock quarter, this difference is 3.57% for the 4-factor risk adjustment.

[4] All results become much stronger if we cluster at the fund level, instead (omitted for brevity).

[5] We collect observations with experience levels ≥ 3 in one bucket since such high experience levels represent only a small fraction of our observations ($< 2\%$).

Overall, these results confirm the univariate sorts. Experienced managers outperform inexperienced managers, experience ins beneficial at a decreasing rate, and relative outperformance is particularly pronounced in industry shock quarters.

3.4 Baseline Robustness

This section provides baseline robustness checks. We present additional, more specific, sets of robustness tests in the next two sections. Table 5 shows several alternative estimations of equation (8), where we regress the 4-factor alpha on an experience dummy, a crisis dummy and a full set of manager $\times$ quarter dummies. For brevity, we only report the point estimate and significance of the experience coefficient.

Specification (1) shows that results are effectively unchanged when we replace the requirement that the industry at least a 10% a weight by the alternative requirement that the industry must be in the fund's top three by assets under management. Next, adding the requirement that only quarters with negative value-weighted return are counted as industry shocks, specification (2) shows that, if anything, results become stronger.

As a further check, we consider an alternative weighting scheme to construct the daily ISP raw returns in equation (6). Our baseline implicitly assumes rebalancing, using the weight of the stock going into the quarter. Specification (3) shows very similar results when we make the opposite assumption, namely that managers stay completely passive during the period, and let the weight float with stock returns.

One potential problem with our experience measure is that, by construction, all managers start with zero experience in the first quarter of the sample. This is purely data-induced because we only observe managers starting in 1992. To alleviate concerns about this cutoff, we drop the first five years of our sample.[6] Specification (4) shows that the results are effectively unchanged, suggesting that the cutoff is not biasing our results.

Specification (5) considers a finer partition of the stock market into the 48 Fama-French

[6] While the first five years are dropped in the estimation, we still use them to compute each manager's experience measure.

industries, as opposed to the 12 industries used so far. Focusing on more narrowly defined industries may estimate the effect of experience more precisely because it focuses more closely on the potential source of learning; at the same time, alpha estimates become more noisy because ISPs now contain fewer stocks. Specification (5) shows that the results are very similar to the 12 industries baseline.

Specification (6) uses the raw experience count variable as an alternative to the experience dummy. As with the dummy variable, more experience is associated with higher alphas. The point estimate indicates 67bps higher 4-factor alpha for every unit of experience. The conditional average of experience in the positive domain is 1.56 and the maximum is 9, which indicates again the the impact of experience on performance is economically large. We comment on specifications (7) and (8) below.

As a final check, we replace the factor-based risk adjustment with the characteristics-based adjustment proposed by Daniel, Grinblatt, Titman, and Wermers (1997). Panel B shows results for our baseline regression for their CS, CT, and AS variables.[7] The panel shows that we get the same message from looking at the characteristics-adjusted performance measures as we do for the factor-adjusted measures: experience is associated with a significantly higher stock picking ability (CS). We do not find evidence of superior characteristics *timing* ability of managers (CT) and we do not find that experience is related to persistent styles that are associated with higher returns (AS). Overall, these findings support our baseline results and show that we are capturing stock picking skill.

3.5 Placebo Runs

We run placebo tests to make sure our findings are neither spuriously induced by how we construct the experience measure, nor by how we run our tests. We generate 10,000 sets of placebo industry shocks, where we randomly choose one industry every quarter and assign it an industry shock.

[7]Following Wermers (2011) we use a version of these variables in which all weights are percentages of ISP assets. This is necessary as ISP performance would otherwise mechanically depend on the weight of the stocks in the ISP in the overall fund. We are thus answering the hypothetical question what the ISP performance would have been if the manager had invested 100% of her assets in this ISP.

Hence, for each ISP and trial we obtain a new experience measure, which we refer to as "placebo" experience. We then rerun our baseline regression with this placebo experience measure, using the 4-factor alpha as the dependent variable.

For brevity, we refer to the experience measure used so far, based on the actual industry shocks, as the "true" experience. Placebo and true experience are mechanically correlated. To see this, consider a manager who is in our data for twelve quarters. If that manager experiences one industry shock in quarter nine, her true experience measure would be zero in quarters zero to nine and unity from periods ten to twelve. Suppose now that the placebo randomly assigns quarter five as a placebo industry shock. Both the true and placebo experience measures will show a zero up to period six, and unity after period nine. Thus, they will be positively correlated. To make sure we are not picking up this correlation, we include both true and placebo experience measures in our regressions.

The aim of this placebo test is therefore twofold. First, we check if, conditional on our experience variable, a placebo variable would have a strong effect on fund returns. Second, we check if our experience measure is robust to the inclusion of other, potentially correlated, placebo experience measures.

Figure 2 summarizes the results. The placebo coefficients are centered near zero and often negative. By contrast, the coefficient on the true experience variable is centered near the baseline estimate of 1.5. The coefficient on true experience exceeds the placebo coefficient 99.96% of the runs (i.e. the placebo coefficient is larger only 4 times out of 10,000), and even then the true coefficient is always positive. Its minimum over the 10,000 runs is 0.83. The distribution of the true estimates is much tighter than the distribution of the placebo estimates.

These results are reassuring. They show that it is very unlikely that our experience measure is large and significant by chance. Even the largest coefficient we see on the placebo measure across all 10,000 runs is smaller than our baseline estimate of 1.5. The results also show that there is nothing in the construction of the variable, or the econometric approach, that would spuriously induce the effect. The explanation most consistent with these results is that the experience measure is picking up variation that is truly informative for predicting ISP performance.

3.6 Experience at the Fund Level: EDX

In this section we investigate if the documented superior stock-picking ability of experienced managers at the ISP level shows up also at the fund level: Would it be profitable to invest with experienced managers? We implement this in the simplest way by looking at a weighted average of the individual industry experience measures (equation (5)), with weights corresponding to the weight of each industry in the fund at the end of quarter $q-1$, for each manager and quarter across all ISPs, to get a fund-level measure of experience:

$$\text{EDX}_{mq} = \sum_i w_{mi,q-1} E_{mqi}. \tag{10}$$

Note that because we restrict attention to single-managed fund-quarters only, EDX is unique for a given fund and quarter. An advantage of EDX is that it is implementable in real time since it only depends on past holdings and past industry shocks.

To see if EDX is associated with higher returns, we sort funds into three EDX terciles every month.[8] We obtain monthly fund returns after expenses from CRSP. We also compute before-expenses returns by adding 1/12 of the fund's expense ratio to the fund's return each month as in Fama and French (2010). Finally, we compute the monthly EDX portfolio return as equal-weighted average return across all funds in the respective portfolio.

Table 6, Panel A, shows that high EDX funds outperform before fees and break even after fees, while low EDX funds break even before fees and underperform after fees. Both before and after fees, a portfolio that is long the high EDX funds and short the low EDX funds would earn a profit of 20bps per month, or 2.4% per year. In terms of factor loadings, both high and low EDX portfolios have a market beta close to one and very similar loadings on value and size. A main difference is in the momentum factor. Low EDX funds seem to follow momentum strategies,

[8] Since a number of funds have an EDX value of zero, the low EDX portfolio contains a slightly higher number of funds.

while high EDX funds do not.

In Panel B, we replicate the analysis using tenure instead of experience. While the overall pattern is somewhat similar (tenure and experience are, after all, positively correlated) the effects are strongly attenuated and the point estimate on the high minus low portfolio is positive but measured very imprecisely (t-statistic smaller than 0.9). Moreover the patterns are not monotonic: performance is lowest for intermediate values of tenure. This further strengthens the argument that tenure and experience are not substitutes and that tenure may not have enough power to detect any meaningful experience effects.

The results from Table 6 are striking because they suggest that a backward-looking measure, EDX, can be used to obtain superior risk-adjusted returns after fees if investors can short. Fund investors should care about fund manager experience even if they cannot short, because high EDX funds allow them to at least break even after fees, while low EDX funds lose money on a risk-adjusted basis.

4. Holdings Changes around Earnings Announcements

Our findings show that experienced fund managers earn higher risk-adjusted returns on their ISPs. This is consistent with learning by experienced managers. But *how* does managerial learning and experience lead to higher returns? We provide a partial answer by looking at whether fund managers learn to predict earnings surprises, and whether this is reflected in their trades in anticipation of the earnings announcement. Earnings announcements are an important corporate event in which fundamental information is revealed to the market. In addition, they are a recurrent event, and thus provide a fund managers with a natural opportunity to apply their experience. The literature has already established that some mangers possess can predict earnings surprises, so the notion that it is possible to trade profitably in anticipation of a surprise is not implausible (e.g., Baker, Litov, Wachter, and Wurgler (2010)). Further, as we detail below, this test has the added benefit of allowing us to separate active managerial decisions from the impact of industry dynamics which would affect even a completely passive mutual fund manager.

To implement the test, we collect all earnings announcements occurring in our sample period from IBES. To track fund trades in anticipation of an earnings announcement, we follow Kacperczyk, Sialm, and Zheng (2005) and look at changes in the weight w_{siq} of a given stock s belonging to industry i in the fund's portfolio between two fund reporting dates q and $q-1$, adjusted for price appreciation. Specifically, we define:

$$\Delta \text{Hold}_{siq} = w_{siq} - \frac{w_{siq-1}(1 + R_{siq})}{\sum_j w_{siq-1}(1 + R_{siq})} \tag{11}$$

where R is the stock's return between dates $q-1$ and q. The above definition ensures that we are focusing on active trading by the fund as opposed to a mechanically changing composition of the fund's portfolio due to price changes. We then estimate:

$$CAR_{msiq} = \lambda + \beta I(E_{mqi} > 0) + \gamma \Delta \text{Hold}_{siq} + \delta I(E_{mqi} > 0) \times \Delta \text{Hold}_{siq} + \xi' X_{msiq} + \varepsilon_{msiq} \tag{12}$$

where CAR denotes the cumulative abnormal return over a three-day $(-1, +1)$ window around the following earnings announcement date, X_{msiq} are control variables, and λ are fixed effects. The announcement return is defined as

$$CAR = \prod_{t=-1}^{+1} (1 + R_{st}) - \prod_{t=-1}^{+1} (1 + \bar{R}_{st}) \tag{13}$$

where $\bar{R}_{st}$ is the return on a matching size and book-to-market portfolio, as in Hirshleifer, Lim, and Teoh (2009).

Prior literature has documented $\gamma > 0$ in equation (12). Our key prediction is $\delta > 0$ as, under the learning hypothesis, experienced managers are better able to trade in anticipation of earnings surprises.[9] This is indeed what we find in Table 7. Across the different specifications,

[9] We control for firm and fund characteristics in all our regressions. Specifically, following Hirshleifer, Lim, and Teoh (2009) we include controls for book-to-market, firm size, turnover, institutional ownership, reporting lag, and the number of analysts covering the stock in IBES. We also control for total net assets and fund flows of the fund, as well as for ICI component, which is an alternative measure of manager sophistication defined in Section **3.1**. We show that results are robust to inclusion of date, industry, earnings surprise decile (SUE decile), manager × quarter, and firm fixed effects.

the impact of ΔHold increases by between 73% and 102% for experienced managers. That is, the return around the earnings announcement increases whenever managers increase their holdings, and substantially more so for experienced managers. The estimates of (12) imply that a one percentage-point increase in the stock's portfolio weight by an inexperienced fund manager is associated with a 12bp higher announcement return. For an experienced manager, the announcement return increases to 21bps. Relative to the unconditional average announcement return of 27bps, this is economically sizeable. In sum, Table 7 shows that experienced managers are better at predicting announcement returns, consistent with the hypothesis that the experience proxy captures learning by the fund managers.

This analysis is especially useful for two reasons. First, it provides evidence for one specific channel through which experience translates into higher future returns: fund managers can benefit from past experience around future earnings announcements, when the ability to obtain informative signals and to process available information is particularly valuable. Second, earnings surprises are always *relative* to earnings expectations. Because the stock price can drop substantially even after announcing high absolute earnings, observing that experienced fund managers are better at predicting earnings surprises is hard to justify by fundamentals and much more likely to be indicative of true skill. By analyzing holdings changes, we can directly observe active decisions by fund managers that are not induced by mechanical changes in the market values of their portfolios.

5. Alternative Explanations and Extensions

5.1 Alternative Explanation 1: Industry Return Reversals and Industry Fixed Effects

One potential concern is that industry performance could be mean-reverting. An industry shock in one period could thus be followed by abnormally high returns in the subsequent periods, and our tests might capture this reversal. Several pieces of our evidence are inconsistent with this view. First, Panel C of Table 4 shows that experienced managers do particularly well in future industry shocks. In contrast, if our documented outperformance were due to industry-level reversals, we should see that managers do especially well *outside* industry shocks, and poorly in industry shock quarters. Second, our holdings-based tests document an active decision by experienced managers to adjust their holdings in anticipation of earnings surprises, rather than a passive effect on portfolio holdings. Industry dynamics can therefore in general not explain the earnings announcement results. Finally, specification (7) in Table 5 shows that including eight lags of past industry returns, to capture industry return dynamics effects directly, does not affect our results. In sum, we conclude that industry-level reversal does not explain our findings.

Another concern may be industry-level unobserved heterogeneity. While unobservable heterogeneity can never be fully ruled out, note that any alternative story would have to simultaneously explain why experienced ISPs outperform, and why we see that managers actively change their portfolio holdings prior to earnings announcements in the direction predicted by a learning model. It is not obvious what omitted factor would be able to induce this pattern. Nevertheless, to be conservative and to rule out that unobservables on the industry level are driving what we see, specification (7) of Table 5 repeats our baseline results with industry fixed effects. While the point estimates attenuate somewhat, both economic and statistical significance stays very high.

Overall, we conclude that industry return reversals and industry fixed effects are not driving our results.

5.2 Alternative Explanation 2: Industry-Specific Tenure and Skill

Our approach is designed to rule out that manager-specific tenure and ability. It is still possible, however, that *industry-specific* tenure and baseline skill matter. To see this, rewrite our econometric model for ISP alphas in equation (8) using industry-specific tenure and skill, denoted by T_{mqi}, and $S_{0,mi}$, respectively:

$$\alpha_{mqi} = \lambda + \beta_1 E_{mqi} + \beta_2' X_{mqi} + \beta_3 T_{mqi} + \beta_4 S_{0,mi} + \varepsilon_{mqi}. \quad (14)$$

Since tenure and baseline skill vary across industry by manager, our manager $\times$ quarter fixed effects would not eliminate their impact on alpha. Therefore, if (i) industry-specific tenure and baseline skill vary within manager and quarter and if (ii) they are positively correlated with experience, the experience coefficient in the above might capture those other effects.

We start by looking at industry tenure. It could play a role because managers who are around longer might simply get more experienced over time, and have more chances to be hit by an industry shock. Because it is observable, it is straightforward to control for industry tenure, as the number of previous quarters a given manager has managed an ISP in industry i. Panel A in Table 8 shows the results. Industry tenure is insignificant across all specifications, while the impact of experience is effectively unchanged. This could be due to the fact that experience indeed builds up mostly in bad times and is therefore not a linear function of time. In that case, industry-specific tenure and our experience measure would not be highly correlated. It could also reflect the fact that most funds have ISPs in most industries in most quarters, which would leave very little variation in industry tenure on the manager-quarter-level to exploit (Table 2). Both explanations suggest that a useful feature of the experience measure is that it is not a linear function of time.

A second potential concern is industry-specific baseline skill. Because industry-specific baseline skill is unobserved, it is much harder to address. Before presenting formal tests, let us highlight the key difference between industry-specific baseline skill and experience: industry-specific baseline skill is a skill managers are endowed with *before* they enter our sample, while industry-specific experience can contribute to increasing managerial skill through learning effects *while* fund managers are in our sample. Industry-specific baseline skill can only matter in our context if it is not captured by overall IQ, education, or general ability of the manager. Hence, ability alone is not a problem for our tests. What would be required is different ability across industries. For example, that some managers are "born" stellar telecom fund managers while being mediocre in managing auto stocks. Alternatively, that managers switch careers from, say, working in the chemical industry, where they obtained industry-specific knowledge, to managing a fund that also holds stocks in the chemical industry (funds that *only* hold stocks in the chemical sector are by definition not in our sample, so pure-play industry funds are not an issue); that some managers receive systematically better education with respect to some industries; or that fund management companies have specific expertise in analyzing certain industries, but not others. Because these channels will operate for some, but almost certainly not for most managers, it is conceptually not clear that we should expect to see large variation in baseline skill across industries for the same manager. If so, industry-specific baseline skill would not be an issue for our results.

If, however, we assume that there is variation in industry-specific baseline skill, the second condition that needs to be satisfied is that there is a positive correlation between industry skill and our experience measure. While this is possible, we could also think of scenarios in which there would be a negative correlation. For example, if some managers had so much industry baseline skill that they could predict crises, this would lead to a downward bias in our experience coefficient. Likewise, if the most industry-skilled managers left to set up their own hedge fund after proving their superior abilities during industry shocks, we would observe a negative correlation between experience and industry skill among the remaining managers.[10]

In Table 8, we present three formal tests to minimize concerns that our findings are driven by industry-specific baseline skill. We exploit observable variables that should be highly correlated with industry-specific baseline skill. The first variable we focus on is industry share, i.e. the fraction of the fund's assets allocated to industry i. If a manager is inherently better at managing stocks in industry i, she should on average overweight it in her portfolio (see also Kacperczyk, Sialm, and Zheng (2005) for a similar idea). We therefore include the average industry share over quarters -5 to -1 as additional control variable. Panel B shows that industry share indeed has a positive impact on fund performance. These estimates might indicate a role for industry-specific baseline skill. They might also reflect the skill-enhancing effect of past experience. Importantly,

[10]Note that this type of selection mechanism is not an issue in our baseline setting. There, identification comes from variation in experience within manager at the same point in time. Hence, because the baseline skill variable $S_{0,m}$ is completely wiped out by the fixed effects, the estimation of the experience effects does not depend on the composition of the sample.

and irrespective of industry share, the experience variable is always significant. Based on the 4-factor model, the impact of experience is 1.29%. To the extent that industry share captures industry-specific baseline skill, we conclude that our experience results are not driven by industry-specific baseline skill. One interpretation of these findings is that conditional on average lagged industry share (which might actually be high because of learning from prior experience) an additional unit of experience is valuable.

The second industry skill proxy is a measure of how much the industry share for a given ISP deviates from the average industry share across all ISPs, the ICI Component. The sum of the ICI Components across all ISPs for a given fund-quarter produces the industry concentration index ICI developed by Kacperczyk, Sialm, and Zheng (2005). These authors show that a higher industry concentration, i.e., a higher ICI, is related to superior fund performance.[11] Panel C shows that the ICI Component variable is positively related to fund returns, consistent with the findings of Kacperczyk, Sialm, and Zheng (2005). However, an additional unit of experience is valuable even conditional on the ICI component. In fact, the size and significance of the experience coefficient are hardly different from the baseline case. This suggests again that we are capturing some meaningful variation in the data that is not captured by other variables proposed in the literature.

As a final test, we use past alphas to construct a direct estimate of industry-specific baseline skill. To do this, we analyze risk-adjusted ISP returns of managers in our sample before they are exposed to the first industry shock. Industry-specific baseline skill, if it exists, should be reflected in these *pre-experience* ISP alphas. We therefore use for each ISP in quarter q the average of all pre-experience ISP alphas for that ISP up to and including quarter $q-1$ as a direct proxy for the unobserved effects in our main regressions. Panel D presents the results. Most importantly, we find again that our experience effects are largely unchanged and, if anything, stronger. This is unlikely due to pre-experience ISP alphas being a noisy measure of industry skill, because, just as would be expected under the null of industry skill effects, the coefficient on industry alphas is

[11] Closely related is the Active Share variable by Cremers and Petajisto (2009). Because both ICI and Active Share are defined at the fund level, we cannot include the original measures in our tests. Hence, we focus on the ICI Component (more precisely: its average over the quarters -5 to -1).

positive and significant in all specifications.

Overall, the results presented in this section suggest that industry-specific tenure and industry-specific baseline skill are not driving our finding that experienced managers outperform.

5.3 Learning from Industry Booms and Volatile Industries

The above findings support the idea that fund managers experience gain experience during industry shock quarters, i.e. during bad times. Do they they also learn from booms? This may be plausible since some of the factors that motivate learning in industry shocks apply also to booms: industry booms are salient events that attract investor and media attention and, because of tournament incentives, managers might disproportionally care about booms for career and bonus reasons. On the other hand, booms may be the result of bubbles, and investor exuberance and media hype may make it harder to extract informative signals. Further, the literature on reinforcement learning cited in the introduction suggests that, because of the human tendency to credit yourself for success and blame others for failure (the self-serving attribution bias), there might be an increased tendency among fund managers in booms to confuse luck with skill. Both factors might hamper learning in booms. Relatedly, we might be picking up not the effect of bad times, but rather effects from volatile industries. If so, we should find very similar effects for both booms and industry shocks.

To investigate these issues, we now allow experience to come from booms and other periods as well. Specifically, we continue to compute an experience measure as in equation (5), but define the IS as a dummy equal to 1 if the industry return rank in this quarter is, for example, the highest across all 12 Fama-French industries (i.e. a "boom"). We do this for all 12 possible industry return ranks and denote IS measures based on rank n as IS_n. Ranking industries from 1 (lowest return) to 12 (highest return) then yields 12 different measures IS_n and E_n, where E_n is computed as in equation (5) using IS_n. This convention implies that IS_1 and E_1 are the industry shock and experience measures we used in previous sections, and IS_{12} and E_{12} are the corresponding measures for boom periods.

Table 9 presents results when we rerun our baseline regressions, including IS_n and E_n. We show results from 12 different regressions, one in each line. We include the baseline parameters IS_1 and E_1 in all regressions as additional controls because experience measures are correlated. For instance, an industry that often has the lowest return, also has a high probability of having the second lowest return in a given quarter. Therefore, effectively, we test if an experience measure based on any other industry rank has incremental explanatory power for fund returns over and above the experience measure based on the worst performing industry. The regression specifications are otherwise identical to the baseline, including the full set of manager $\times$ quarter dummies.

The first line in Table 9 reproduces Table 4, Panel A. The second line shows that the experience measure based on industry shocks E_1 is effectively unchanged while the experience measure E_2, constructed based on industry rank 2 (the second lowest rank), is much closer to zero and insignificant. Overall, a striking feature of the table is that the coefficient on E_1 is always highly significant and always markedly higher than the coefficients on alternative experience measures, which are often near zero and always insignificant.

Both the point estimate and statistical significance increases slightly for the highest industry rank 12 (t-statistic = 1.46). At the same time the industry shock experience measure gets slightly attenuated both in size and statistical significance (although both remain high). This might indicate some role for learning from booms or for learning in volatile industries. However, if they exist, those effects are very weak in our data.

Overall, the analysis shows that while experience in industry shock periods always has a strong impact on fund returns, the evidence for learning effects in booms and volatile industries is at best weak, and there is no evidence of learning effects in other periods.

5.4 Learning from the Time-Series of Industry Returns

Our baseline results have focused on fund manager experience based on industry shocks that are defined cross-sectionally, i.e. whenever an industry is the worst performing one in a given quarter.

While this is plausible given the focus on relative performance in the mutual fund industry, it is also plausible to think of industry shocks in terms of the time-series. For example, investors and the media frequently compare returns this period to past returns. In this section we investigate if we can find experience effects also when experience is gained from industry shocks defined from the time-series of industry returns.

We compute a time-series based industry shock dummy IS^{TS} as follows: for every industry and quarter, we set IS^{TS} to one if the industry return is below the 10th percentile of returns in this industry over the last 40 quarters. We then compute a time-series based experience measure E^{TS} exactly as in equation (5), using IS^{TS} instead of IS.

Table 10 shows results that are qualitatively similar to the baseline case. Panel A splits the sample into experienced and non-experienced ISPs. Our definition of IS^{TS} yields group sizes that are quite comparable to the benchmark case. On average, an experienced manager has seen 1.76 industry shocks. ISP performance, measured as CAPM, 3-factor, and 4-factor risk-adjusted returns, also lines up as predicted under the hypothesis that managers learn from past industry shocks. The differences are not as pronounced as in the cross-sectional industry shock setting, however. For example the difference in 4-factor alphas is 72bps in the time-series case, while it was 119bps in the cross-sectional case. In absolute terms, however, these numbers are still sizeable.

Panel B replicates the sorting from Table 3. For brevity, we only report results for sorting the 4-factor alphas by E^{TS} and IS^{TS}. Also here, a substantial fraction of the outperformance for experienced ISPs relative to inexperienced ISPs can be attributed to the fact that managers who have seen past industry shocks are performing much better in future shock than their inexperienced peers.

Finally, Panel C replicates our regressions from Table 4. We again obtain qualitatively similar results, but somewhat weaker for the time-series case. Specifications (1) and (2) show that, conditional on IS^{TS}, and net of any potentially confounding factor that does not vary within a manager across ISPs at a given quarter, experience increases ISP performance by 68bps and

86bps, for three and four factor adjustments, respectively. Experience from past industry shocks is again particularly valuable in future industry shocks (specification (3)).

Overall, the data are consistent with the view that managers learn also from the time-series of industry-returns. We stress that we do not necessarily find learning from the cross-section of industry returns more relevant than learning from the time-series. Rather, we view both polar cases as plausible benchmarks to uncovering learning effects. We find it reassuring to see that there is support for both in the data.

One particularly interesting implication of the time-series findings is that it adds a new dimension to the literature cited in the introduction that finds mutual funds tend to do better in recessions and downturns. While existing explanations have focused on the higher marginal utility of wealth for investors in downturns (e.g., Glode (2011)), or the idea that obtaining informative signals becomes more valuable in downturns (e.g., Kacperczyk, Nieuwerburgh, and Veldkamp (2011)), our theory implies that mutual funds outperform in downturns because some fund managers learn from past downturns. The correlation between IS^{TS} and the market factor is in line with this idea (corr = –0.53).[12]

6. Conclusion

We present a new approach to investigating the importance of learning by doing for fund managers. Our innovation is to exploit variation in experience across industry sub-portfolios (ISPs) *for a given manager at a given point in time*. We find that experience is valuable: ISPs managed by experienced fund managers outperform by 1.5% per quarter on a 4-factor risk-adjusted basis. Our approach ensures that this difference cannot be explained by factors that do not vary across ISPs for a given manager and quarter. The effects therefore cannot be explained by previously studied variables like age, tenure, education, IQ, corporate governance, fund characteristics, and the business cycle. These results aggregate to the fund level. Measuring experience by a new

[12]Note that our baseline effects are, by construction, not related to the business cycle as, there, we define industry shocks purely from the cross-section of industry returns. The cross-sectional IS measure has practically zero correlation with the market factor (corr = –0.02).

EDX index that captures individual fund manager experience, we find that a long-short portfolio on EDX generates risk-adjusted returns of 2.4% per year in our sample. More experience is beneficial, but at a decreasing rate.

Underlying our approach is the idea that experience and learning are not just linear functions of time. Specifically, we conjecture that investors learn relatively more after periods of abnormally low returns, consistent with earlier investigations into learning by doing (e.g., Arrow (1962)), as well as the literature showing that own experience of abnormal market conditions in the past influences financial decisions in the future (e.g., Malmendier and Nagel (2011)). An important implication of our study for empirical researchers is that tenure might not be a very good empirical proxy for experience. Overall, our results suggest that learning by doing is important for professional investors in highly competitive markets, and that experience is a valuable fund manager characteristic that investors should care about.

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Table 1: Worst Performing Industries By Quarter

This table reports the worst performing industries for each quarter among all stocks in the NYSE, AMEX, and NASDAQ. We use the Fama-French 12 industry classification. Returns are value-weighted industry averages.

Quarter	FF12 Industry	Return	Quarter	FF12 Industry	Return
1992q1	Health	-0.125	2000q3	Telecom	-0.103
1992q2	Health	-0.068	2000q4	Business Equipment	-0.342
1992q3	Consumer Durables	-0.073	2001q1	Business Equipment	-0.294
1992q4	Oil, Gas, and Coal	-0.056	2001q2	Utilities	-0.008
1993q1	Health	-0.148	2001q3	Business Equipment	-0.345
1993q2	Consumer NonDurables	-0.075	2001q4	Telecom	-0.018
1993q3	Health	-0.020	2002q1	Telecom	-0.124
1993q4	Oil, Gas, and Coal	-0.065	2002q2	Business Equipment	-0.260
1994q1	Utilities	-0.101	2002q3	Business Equipment	-0.257
1994q2	Utilities	-0.069	2002q4	Shops	0.000
1994q3	Consumer Durables	-0.011	2003q1	Telecom	-0.091
1994q4	Telecom	-0.051	2003q2	Chemicals	0.054
1995q1	Consumer Durables	0.021	2003q3	Telecom	-0.050
1995q2	Oil, Gas, and Coal	0.034	2003q4	Shops	0.074
1995q3	Oil, Gas, and Coal	0.023	2004q1	Telecom	-0.014
1995q4	Business Equipment	-0.035	2004q2	Banks	-0.028
1996q1	Utilities	-0.012	2004q3	Business Equipment	-0.101
1996q2	Chemicals	-0.004	2004q4	Health	0.041
1996q3	Telecom	-0.074	2005q1	Consumer Durables	-0.119
1996q4	Shops	-0.029	2005q2	Chemicals	-0.054
1997q1	Business Equipment	-0.037	2005q3	Shops	-0.021
1997q2	Utilities	0.055	2005q4	Oil, Gas, and Coal	-0.072
1997q3	Consumer NonDurables	0.018	2006q1	Utilities	-0.007
1997q4	Business Equipment	-0.122	2006q2	Business Equipment	-0.094
1998q1	Oil, Gas, and Coal	0.044	2006q3	Oil, Gas, and Coal	-0.044
1998q2	Manufacturing	-0.072	2006q4	Health	0.015
1998q3	Chemicals	-0.223	2007q1	Banks	-0.012
1998q4	Oil, Gas, and Coal	-0.018	2007q2	Utilities	0.009
1999q1	Consumer NonDurables	-0.116	2007q3	Shops	-0.047
1999q2	Health	-0.037	2007q4	Banks	-0.097
1999q3	Banks	-0.143	2008q1	Business Equipment	-0.155
1999q4	Utilities	-0.082	2008q2	Consumer Durables	-0.154
2000q1	Chemicals	-0.246	2008q3	Oil, Gas, and Coal	-0.270
2000q2	Telecom	-0.172	2008q4	Consumer Durables	-0.346

Table 2: Summary Statistics

The table presents summary statistics. Panel A provides key statistics about our sample. Panel B shows descriptive statistics of our main industry shock and experience measures. Panel C reports averages of key variables of interest across all industry sub-portfolios (ISPs) used in our analysis. We report the sample average (All), the average for the subgroup of inexperienced manager ($E = 0$), the average for the subgroup of managers with experience ($E > 0$), as well as t-statistics for the difference across the two subsamples. Reported t-statistics allow for clustering at the industry-quarter level. The sample is constructed based on all mutual funds in the CRSP Mutual Funds database, with available information identifying the fund manager, and with single-fund managers managing single-manager funds, over the period 1992–2008.

Panel A: Sample

Number of Quarters	68
Number of Managers	3,197
Number of Funds	2,503
Number of ISPs	38,267
Avg. Number of ISPs per Fund (Median)	9.2 (10.0)
Avg. Life of ISP (Median)	8.8 (6.0)
Avg. Life of Manager (Median)	11.4 (8.0)

Panel B: Experience and Industry Shock Variables

Variable	Mean	St.Dev.	Min	Median	Max	N
IS	0.08	0.28	0	0	1	336,193
Experience	0.25	0.70	0	0	9	336,193

Panel C: Summary Statistics by Experience

Variable	All	$E = 0$	$E > 0$	t-stat
Experience	0.25	0.00	1.56	-
CAPM Alpha	0.58	0.53	0.83	0.98
3-Factor Alpha	0.29	0.14	1.07	3.93
4-Factor Alpha	0.41	0.22	1.41	4.93
CAPM Beta	0.95	0.93	1.06	6.12
4-Factor Loading HML	0.17	0.21	-0.06	-7.78
4-Factor Loading SMB	0.23	0.23	0.19	-2.68
4-Factor Loading UMD	-0.03	-0.02	-0.13	-4.74
ISP Size ($m)	90.25	67.45	212.50	20.89
Fund Size ($m)	952.55	904.77	1,208.70	12.83
Industry Share	0.11	0.09	0.19	30.87
HHI (%)	19.53	19.47	19.85	0.84
Industry Concentration Index (ICI)	7.56	7.56	7.56	-0.01
ICI Component	1.34	1.11	2.55	14.66
Tenure	11.68	10.31	19.03	29.00
Industry Tenure	10.80	9.35	18.53	31.75
N	336,193	283,343	52,850	

Table 3: Sorts

The table reports risk-adjusted returns sorted into groups by experience and industry shocks. Risk-adjustments are computed using the CAPM, the Fama and French (1993) 3-factor model, and the Carhart (1997) 4-factor model. T-statistics allow for clustering at the industry-quarter level.

Panel A: CAPM Alpha

	Risk-adjusted return			*t*-statistic		
	IS = 0	IS = 1	Diff.	IS = 0	IS = 1	Diff.
E = 0	1.19	-7.03	-8.22	6.11	-11.13	-13.66
E > 0	1.59	-6.00	-7.59	4.39	-6.16	-8.40
Diff.	0.41	1.04	0.63	1.33	1.40	0.93

Panel B: 3-Factor Alpha

	Risk-adjusted return			*t*-statistic		
	IS = 0	IS = 1	Diff.	IS = 0	IS = 1	Diff.
E = 0	0.60	-5.13	-5.73	3.36	-6.12	-6.99
E > 0	1.38	-1.71	-3.09	4.55	-1.55	-2.90
Diff.	0.78	3.41	2.63	3.18	4.72	3.87

Panel C: 4-Factor Alpha

	Risk-adjusted return			*t*-statistic		
	IS = 0	IS = 1	Diff.	IS = 0	IS = 1	Diff.
E = 0	0.61	-4.25	-4.86	3.67	-5.77	-6.77
E > 0	1.58	-0.07	-1.65	5.32	-0.06	-1.35
Diff.	0.97	4.18	3.21	3.94	4.10	3.24

Table 4: Fund Manager Experience and Performance

The table reports results from regressing ISP alphas on experience, manager-quarter fixed effects and controls. Experience is a dummy equal to one if the fund manager has experience greater than zero according to equation (5). Panel A, controls for the presence of industry shocks (IS). Panel B includes separate dummies for experience levels of one, two, and higher than two. Panel C includes an interaction term between experience and industry shocks. Results are shown for CAPM, 3-factor, and 4-factor risk-adjustment, respectively. T-statistics are robust to clustering by manager and industry as defined in equation (9).

Panel A: Baseline Regressions

	CAPM	3-Factor	4-Factor
Experience	0.596	1.212	1.504
	(1.94)	(4.36)	(4.97)
IS	-7.942	-5.132	-4.184
	(-13.81)	(-5.76)	(-5.22)
Manager × Quarter FE	Yes	Yes	Yes
R^2	0.25	0.17	0.16
N	336,193	336,193	336,193

Panel B: Impact of Increasing Experience

	CAPM	3-Factor	4-Factor
Experience = 1	0.439	1.084	1.384
	(1.70)	(4.31)	(5.08)
Experience = 2	0.808	1.397	1.688
	(1.70)	(3.51)	(3.69)
Experience $\geq$ 3	1.134	1.626	1.883
	(1.79)	(2.85)	(3.34)
IS	-7.949	-5.138	-4.190
	(-13.83)	(-5.78)	(-5.24)
Manager × Quarter FE	Yes	Yes	Yes
R^2	0.25	0.17	0.16
N	336,193	336,193	336,193

Panel C: Experience-Industry Shock Interactions

	CAPM	3-Factor	4-Factor
Experience	0.503	0.922	1.158
	(1.52)	(3.23)	(3.95)
IS	-8.121	-5.689	-4.849
	(-13.80)	(-6.75)	(-6.55)
IS × Experience	0.964	2.993	3.574
	(1.46)	(3.66)	(3.13)
Manager × Quarter FE	Yes	Yes	Yes
R^2	0.25	0.17	0.16
N	336,193	336,193	336,193

Table 5: Robustness Checks

This table reports robustness checks. All regressions include an IS dummy as well as a full set of manager × quarter fixed effects (not reported). In Panel A, specification (1) uses a top 3 cutoff for the experience definition in equation (5). Specification (2) adds the requirement that only industry quarters with strictly negative returns count in the experience measure. (3) allows the industry weight in computing ISP alphas in equation (6) float with stock returns. (4) repeats the baseline analysis for the years 1997–2008. (5) replicates the baseline result, working with 48 Fama–French industries instead of 12 industries throughout. (6) uses E_{mqi} instead of $I(E_{mqi} > 0)$. (7) includes 8 lags of value-weighted industry returns. (8) includes industry fixed effects. In Panel B, characteristics-adjusted performance measures due to Daniel, Grinblatt, Titman, and Wermers (1997) are used. Measures are rescaled so that the weights of individual stocks in ISPs scale up to 100% following Wermers (2011). Unless otherwise indicated, t-statistics are robust to clustering by manager and industry as defined in equation (9).

Panel A: Baseline Robustness (Only coefficient on Experience is shown)

	Experience	t-statistic	N
Baseline			
Experience	1.504	(4.97)	336,193
Alternative specifications			
(1) Top 3 Industry	1.522	(4.81)	336,193
(2) Only Negative IS	1.684	(5.06)	336,193
(3) Floating Weight	1.422	(4.59)	336,138
(4) Excl. First 5 Years	1.498	(4.66)	262,595
(5) Fama–French 48 industries	1.376	(2.94)	754,473
(6) Raw Experience Measure	0.667	(4.12)	336,193
(7) 8 Lags of Industry Returns	1.329	(5.09)	117,548
(8) Industry Fixed Effects	0.626	(2.72)	336,193

Panel B: Characteristcs-Adjusted Returns (DGTW (1997))

	CS	CT	AS
Experience	0.644	-0.020	0.057
	(2.35)	(-0.17)	(0.56)
N	301,582	243,674	243,674

Table 6: Experience at the Fund Level: EDX and Performance

The table presents fund level results using the experience index EDX (Panel A) and tenure (Panel B). In Panel A, for each quarter and fund, EDX is the sum of the individual ISP experience measures. In each quarter, we sort funds into terciles by the value of EDX. The low EDX portfolio is made up of funds with an EDX value of zero. We compute the return of the respective portfolio as the the equal-weighted monthly return before and after expenses reported in CRSP. Abnormal return is the intercept from regressing the fund returns on the four standard factors. Panel B repeats the analysis using tenure, defined as the number of quarters the manager is in our dataset up to the current quarter. The table reports t-statistics in parentheses and the average number of individual funds in a respective portfolio.

Panel A: Sort on Experience

	Abnormal return (% per month)		Factor loadings before expenses				
	Before expenses	After expenses	Market	Value	Size	Mom.	Avg. N
Low EDX	-0.06	-0.15	0.94	0.10	0.18	0.05	217
	(-0.87)	(-2.34)	(37.83)	(3.45)	(8.57)	(3.79)	
Mid EDX	0.07	0.00	0.96	0.15	0.12	-0.02	161
	(1.03)	(0.01)	(57.42)	(6.71)	(5.88)	(-1.43)	
High EDX	0.14	0.04	0.96	0.09	0.17	-0.05	181
	(2.19)	(0.63)	(63.82)	(3.19)	(8.10)	(-2.36)	
High – Low	0.20	0.19	0.02	-0.01	-0.01	-0.10	
	(2.12)	(2.07)	(0.77)	(-0.36)	(-0.61)	(-4.13)	

Panel B: Sort on Tenure

	Abnormal return (% per month)		Factor loadings before expenses				
	Before expenses	After expenses	Market	Value	Size	Mom.	Avg. N
Low Tenure	0.00	-0.09	0.98	0.07	0.17	0.00	184
	(-0.02)	(-1.60)	(61.00)	(3.38)	(8.68)	(0.07)	
Mid Tenure	-0.03	-0.11	0.95	0.09	0.18	0.00	181
	(-0.52)	(-1.76)	(52.89)	(4.19)	(8.38)	(0.00)	
High Tenure	0.06	-0.03	0.93	0.15	0.15	-0.01	179
	(1.23)	(-0.58)	(58.06)	(6.65)	(7.95)	(-0.69)	
High – Low	0.07	0.06	-0.05	0.08	-0.01	-0.01	
	(0.87)	(0.77)	(-2.05)	(2.65)	(-0.49)	(-0.51)	

Table 7: Experienced Managers' Trades Around Earnings Announcements

The table reports the results from regressing cumulative abnormal returns over a 3-day window $(-1, +1)$ around the earnings announcement on fund manager experience, ΔHold (increase in fund holdings of the stock, adjusted for price appreciation) and controls. Firm controls include book-to-market, size (natural logarithm of market capitalization, in millions of dollars), stock turnover, percentage of institutional ownership (IO), reporting lag, and analyst coverage (natural logarithm of 1 plus the number of analysts covering the firm). Fund controls include log fund assets, fund flows, and industry concentration index. Earnings surprise (SUE) deciles include the current SUE decile and the SUE deciles of the last 3 earnings announcements. T-statistics are robust to clustering by firm.

	(1)	(2)	(3)	(4)	(5)
ΔHold	0.115	0.120	0.088	0.125	0.130
	(6.48)	(6.35)	(3.87)	(6.88)	(5.61)
Experience × ΔHold	0.082	0.088	0.090	0.090	0.103
	(2.85)	(2.84)	(2.64)	(3.06)	(2.62)
Experience	-0.051	-0.031	-0.024	-0.038	-0.048
	(-1.38)	(-0.81)	(-0.57)	(-0.84)	(-0.96)
B/M ratio		0.015	-0.603	0.035	-0.229
		(0.14)	(-4.09)	(0.33)	(-1.12)
Log size		-0.033	-0.018	-0.007	-0.816
		(-1.37)	(-0.60)	(-0.28)	(-6.00)
Turnover		-0.024	-0.032	-0.021	-0.015
		(-2.56)	(-2.59)	(-2.28)	(-0.82)
IO		1.018	0.971	0.938	-0.160
		(5.62)	(3.99)	(5.27)	(-0.38)
Reporting lag		-0.012	-0.007	-0.012	-0.007
		(-3.10)	(-1.36)	(-3.27)	(-1.60)
Log(1+analysts)		0.010	0.093	0.007	0.053
		(0.17)	(1.41)	(0.12)	(1.04)
Log TNA		-0.001	0.002		
		(-0.44)	(0.50)		
Fund flow		-0.015	-0.008		
		(-0.91)	(-0.46)		
ICI		-0.086	-0.050		
		(-1.30)	(-0.70)		
SUE decile control	No	No	Yes	No	No
Industry dummies	Yes	Yes	Yes	Yes	Yes
Date dummies	Yes	Yes	Yes	No	No
Manager × Quarter FE	No	No	No	Yes	Yes
Firm FE	No	No	No	No	Yes
N	1,311,414	1,117,341	783,880	1,290,284	1,290,284
R^2	0.002	0.003	0.078	0.023	0.086

Table 8: Industry-Specific Tenure and Baseline Skill

This table reports additional tests on industry-specific tenure and industry-specific baseline skill. Panels A to D add additional controls to the baseline regression in Table 4, Panel A. All regressions include an IS dummy as well as a full set of manager × quarter fixed effects (unreported). In Panel A, industry tenure is the number of quarters this manager has managed an ISP in this industry prior to the current quarter. In Panel B, average industry share is the average industry share of an ISP over quarters -5 to -1. In Panel C, average industry concentration is the average of the ICI Component measure over quarters -5 to -1. ICI Component is for each ISP the squared deviation of the industry share from the average industry share across ISPs in this quarter and industry. In Panel D, the pre-experience ISP alpha is for each ISP in quarter q the average of all alphas for that ISP up to and including quarter $q-1$ as long as $E=0$. If $E>0$, pre-experience alpha is the last available pre-experience alpha for $E=0$. T-statistics are robust to clustering by manager and industry as defined in equation (9).

Panel A: Control for Lagged Industry Tenure

	CAPM	3-Factor	4-Factor
Experience	0.638	1.226	1.528
	(2.07)	(4.32)	(4.93)
Industry Tenure	-0.016	0.005	0.005
	(-0.87)	(0.27)	(0.30)
N	290,635	290,635	290,635

Panel B: Control for Lagged Industry Share

	CAPM	3-Factor	4-Factor
Experience	0.732	1.078	1.286
	(2.50)	(3.91)	(4.48)
Average Industry Share	-0.057	2.005	1.972
	(-0.08)	(2.16)	(2.43)
N	171,920	171,920	171,920

Panel C: Control for Lagged Deviation in Industry Share from Industry Mean

	CAPM	3-Factor	4-Factor
Experience	0.704	1.262	1.466
	(2.29)	(4.53)	(4.97)
Average ICI Component	0.017	0.020	0.021
	(2.34)	(2.70)	(2.80)
N	171,920	171,920	171,920

Panel D: Control for Average Pre-Experience ISP Alpha

	CAPM	3-Factor	4-Factor
Experience	0.718	1.253	1.548
	(2.33)	(4.46)	(5.06)
Pre-Experience ISP Alpha	0.039	0.089	0.063
	(2.25)	(4.49)	(3.85)
N	290,635	290,635	290,635

Table 9: Learning from Industry Booms and Volatile Industries

This table reports results when learning can come from other periods. It shows coefficient estimates when the 4-factor alpha is regressed on E_1, E_n, IS_1, IS_n and a full set of manager × quarter dummies. Every line represents results from one single regression. E_1 and IS_1 are the experience and industry shock dummies used in the previous tables. E_n and IS_n are the experience and industry shock variables when an industry shock is not based on the lowest industry return in a quarter (rank = 1), but on rank = n, where $n = 12$ denotes the highest industry return in the quarter (booms). The experience measures E_n are constructed otherwise as dummies based on equation (5). T-statistics are robust to clustering by manager and industry as defined in equation (9).

Rank n	E_n	t-stat	E_1	t-stat	IS_n	t-stat	IS_1	t-stat
1 (Low)			1.504	(4.97)			-4.184	(-5.22)
2	0.046	(0.17)	1.544	(4.82)	-2.968	(-5.05)	-4.450	(-5.57)
3	0.355	(1.38)	1.440	(4.89)	-2.993	(-7.36)	-4.462	(-5.58)
4	0.032	(0.14)	1.432	(4.55)	-1.619	(-3.42)	-4.333	(-5.40)
5	-0.084	(-0.31)	1.501	(4.69)	-1.047	(-2.16)	-4.282	(-5.33)
6	0.316	(1.28)	1.389	(4.26)	-0.443	(-0.79)	-4.217	(-5.27)
7	-0.257	(-0.96)	1.569	(4.86)	0.741	(1.57)	-4.123	(-5.14)
8	-0.355	(-1.76)	1.592	(5.01)	-0.154	(-0.30)	-4.205	(-5.24)
9	-0.120	(-0.52)	1.551	(4.86)	1.073	(2.46)	-4.086	(-5.08)
10	-0.179	(-0.86)	1.543	(4.98)	1.293	(1.93)	-4.074	(-5.07)
11	0.212	(0.87)	1.331	(4.58)	2.689	(5.55)	-3.949	(-4.92)
12 (High)	0.401	(1.46)	1.173	(4.13)	3.520	(6.13)	-3.892	(-4.90)

Table 10: Experience from the Time-Series of Industry Returns

The table presents results when experience comes from the time-series of industry returns. Experience E^{TS} is calculated as in equation (5), but now based on IS^{TS} which is an industry shock measure based on the time-series of industry returns. IS^{TS} is a dummy equal to one if the industry return in the quarter is among the lowest four quarterly returns over the last 40 quarters. Panel A shows averages of E^{TS} and performance variables aver the whole sample and split by experience. Panel B replicates the experience sort from Table 3 for the time-series based experience measure. Panel C presents regression results from Table 4 using the time-series based experience measure. T-statistics that allow for clustering by industry-quarter are shown in Panels A and B. T-statistics robust to clustering by manager and industry as defined in equation (9) are reported in parentheses in Panel C.

Panel A: Summary Statistics by Experience

Variable	All	$E^{TS} = 0$	$E^{TS} > 0$	t-stat
Experience^{TS}	0.34	0.00	1.79	-
CAPM Alpha	0.58	0.49	0.93	1.51
3-Factor Alpha	0.29	0.18	0.76	2.51
4-Factor Alpha	0.41	0.27	0.99	3.19
N	336,193	272,102	64,091	

Panel B: Sorting using 4-Factor Alpha

	Risk-adjusted return			t-statistic		
	$IS^{TS} = 0$	$IS^{TS} = 1$	Diff.	$IS^{TS} = 0$	$IS^{TS} = 1$	Diff.
$E^{TS} = 0$	0.62	-2.24	-2.86	3.56	-3.47	-4.60
$E^{TS} > 0$	0.99	0.97	-0.03	3.48	0.93	-0.03
Diff.	0.37	3.20	2.83	1.65	3.97	3.66

Panel C: Regression-Based Results

	3-factor	4-factor	4-factor
Experience^{TS}	0.677	0.890	0.364
	(2.31)	(3.06)	(1.23)
IS^{TS}	-6.357	-4.805	-5.515
	(-6.05)	(-5.53)	(-6.42)
$IS^{TS} \times \text{Experience}^{TS}$			3.472
			(4.06)
Manager × Quarter FE	Yes	Yes	Yes
R^2	0.16	0.16	0.16
N	336,193	336,193	336,193

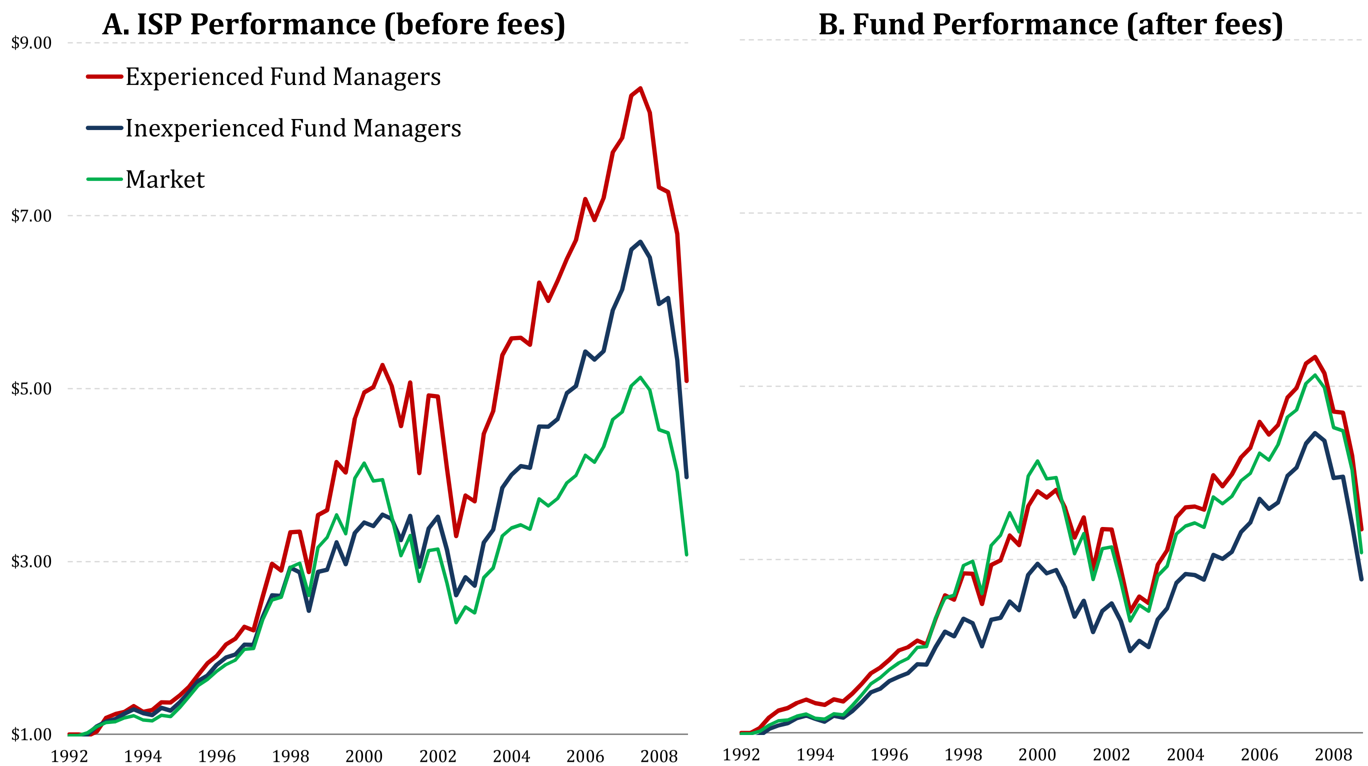

Figure 1: Investing $1 with experienced and inexperienced fund managers from 1992 to 2008. The two graphs show how an initial investment of $1 with experienced and inexperienced fund managers grows over our sample period. Performance is presented both at the industry-sub-portfolio (ISP) level *before fees* (Panel A) and at the fund level *after fees* (Panel B). Panel A shows the (hypothetical) performance of investing $1 in a value-weighted, quarterly rebalanced, portfolio of ISPs by managers that have experienced an industry shock in the past ($E > 0$) as well as the performance of the remaining, inexperienced, ISPs. In Panel B, funds with experienced managers are the funds belonging to the top tercile of the EDX-index distribution, and those with inexperienced managers to the bottom tercile. For comparison, both graphs also present the performance obtained by investing one dollar in the value-weighted CRSP market portfolio.

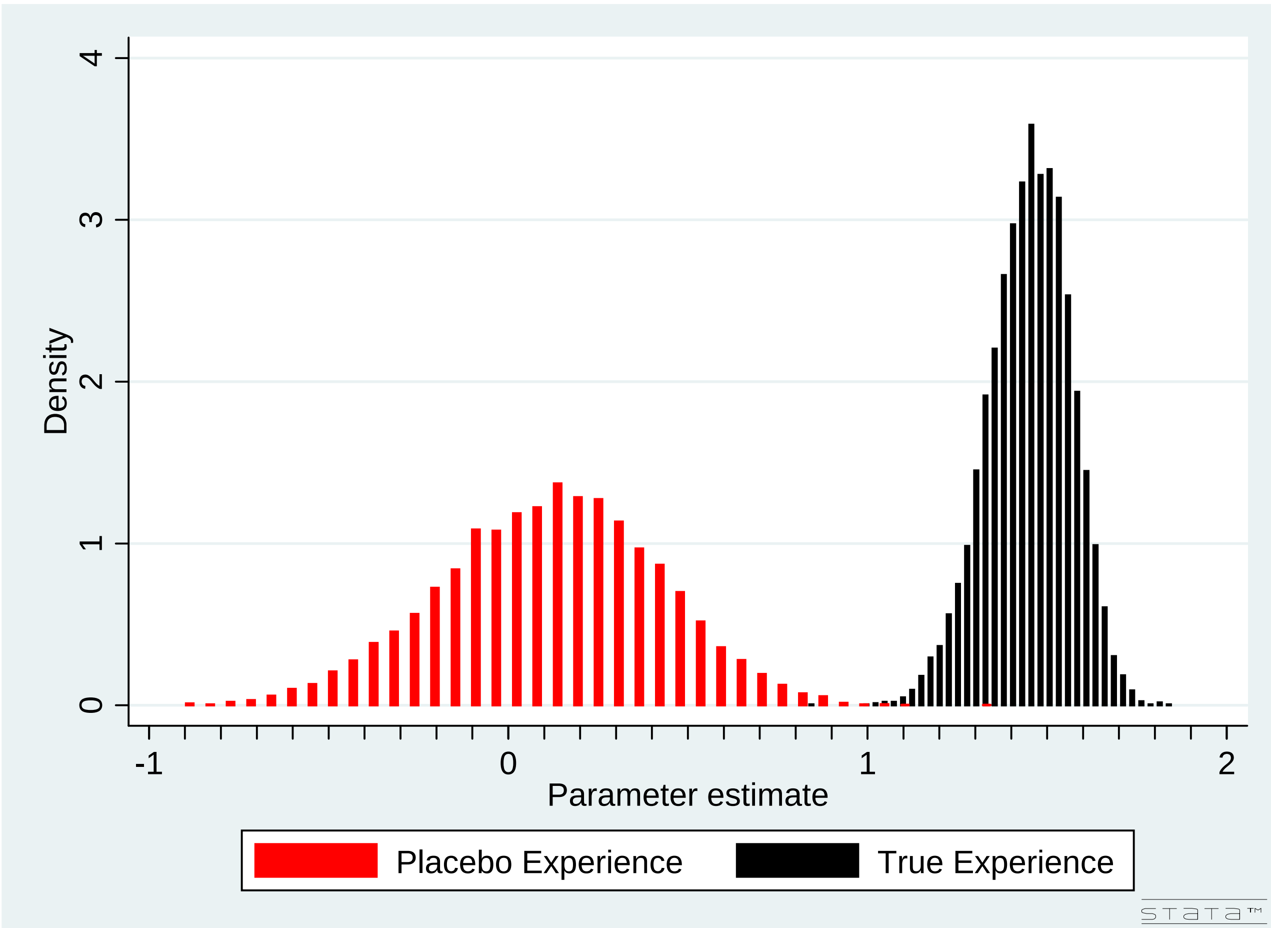

Figure 2: Results from 10,000 placebo runs. The figure shows parameter estimates for running our baseline regression in Table 4, Panel A, 10,000 times. For each run, we randomly generate a sequence of industry shocks by randomly selecting one Fama-French 12 industry every quarter as an industry shock quarter. We then recompute our experience measure based on this placebo industry shock series. We then regress the 4-factor alpha of an industry sub-portfolio (ISP) on the placebo experience measure, the "true" experience measure, a dummy for a placebo industry shock, a dummy for the true industry shock, as well as a full set of manager $\times$ quarter fixed effects. The figure shows the parameter estimates on the placebo and true experience measure, respectively.