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

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Abstract

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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 in industries where they have obtained experience on the job. 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, they pick better stocks, and 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.

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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))

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. 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.,

[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 decreasing unit costs as function of 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).

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 with an experienced fund manager outperform other ISPs by about 1.0% per quarter before fees on a four-factor risk adjusted basis. In regressions, using manager × date fixed effects, that spread widens to almost 1.4% per quarter. In addition, experienced managers make significantly better buying and selling decisions than inexperienced managers even if we use fixed effects to eliminate confounding variation on the manager-quarter, industry-quarter, and manager-stock level. These results suggest learning by doing and experience are first-order drivers of fund returns.

In deriving our experience measures, 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 Arrow's (1962) seminal work on learning by doing, 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." 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, for example, the healthcare industry than in the telecom industry, *at the same point in time.*

Several results support our learning story. First, the outperformance of experienced managers is particularly pronounced during subsequent industry shocks. Second, there are decreasing marginal benefits of experience. Third, difference-in-differences results show managers perform better than their peers after obtaining experience, but not before. Fourth, managers with greater exposure to a shock industry learn more. Finally, several placebo tests suggest our results are not spuriously induced by our methodology.

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.

As a final step, we develop an experience index (EDX), which aggregates experience measure across all industries for a given manager at the fund level. EDX is a purely backward looking measure that can be constructed in real time. Funds that score highest on EDX obtain significant 4-factor risk-adjusted returns of 1.4% per year before fees, while low EDX funds break even at best.

The next section summarizes the related literature. We describe our method and the dataset in detail in Section II. Section III presents our main results on fund manager experience and fund performance as well as robustness checks. In Section IV we examine trading by experienced fund managers. Section V discusses alternative explanations. Extensions are discussed in Section VI. The final section concludes.

I. 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 small number of papers 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." Our evidence of learning by doing is related to but different from the contemporaneous evidence of Pástor, Stambaugh, and Taylor (2014), who find that skill rises with fund age once they control for the size of the mutual fund industry. 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), Pástor 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), Pástor, Stambaugh, and Taylor (2014)). 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, but our effects are not subsumed by their fund-level measure. As we show that experience from industry shocks tends to be particularly valuable in future industry shocks, our findings can help explain why mutual funds on average do better in recessions (e.g., Moskowitz (2000), Glode (2011)), but the experience-performance relation we document cannot, by construction, be explained by recessions. Our trade-based results are in line with, and may even provide a learning-based economic rationale for, results in Chen, Jegadeesh, and Wermers (2000) and Schultz (2010) who find fund manager trading skill is observed predominantly for funds tilting towards growth stocks.

[2]This literature is too large for us to review it here. Papers include Daniel, Grinblatt, Titman, and Wermers (1997), Cohen, Coval, and Pástor (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.

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 because their employment histories exposed them to different learning opportunities. Our results show that experience can be (i) theoretically important for understanding the origins of 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, (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.

II. 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 we use in our empirical tests.

A. 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, 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 manager m_1 should outperform manager 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:

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

T denotes tenure and captures the idea that a 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. For example, they are more intelligent, or have received their education from an elite college. The subscript 0 indicates that baseline skill is time-invariant and fixed. In our formulation, managers with higher baseline skill receive more signals. E denotes experience.

The existing literature has mainly focused on the first two components. 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. 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 do this, we decompose the portfolio held by manager m in quarter q into its industry components, which comprise, for example all stocks held by the fund that belong to the banking industry, healthcare etc. We call these industry-related parts of the portfolio *industry sub-portfolios* (ISPs).

Consider then a reduced-form model of performance for ISP i:

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

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.[3] The model allows for an arbitrary set of variables, $\Gamma' B_{mq}$, that can vary across both managers and quarters. As discussed in the introduction, this set of variables includes a

[3] While we believe a linear specification in equation (3) is a plausible starting point, the true data generating process need not be linear. As shown in Angrist and Pischke (2009, Theorem 3.1.6), the linear specification in our benchmark model is the best linear approximation, in a minimum mean squared error sense, to the conditional expectation function of α given a level of experience E. We have explored cross-effects between T, S, and E in our empirical work, but could not find evidence for substantial non-linearities along those dimensions.

large range of covariates studied in the literature, such as manager age, fund characteristics, fund governance, and the state of the economy. As an empirical matter, the β's as well as Γ could be zero, in which case alphas would reflect pure luck.

Equation (3) shows that we can eliminate the effect of tenure, baseline skill, and all other, potentially time-varying, variables, B_{mq}, if we compare the performance of ISPs for the same manger at the same point in time. In our empirical work below, we implement this by estimating equation (3) with a full set of manager $\times$ quarter fixed effects. The coefficient of interest, β_3, is identified because experience varies within manager and date. Our main prediction is $\beta_3 > 0$, i.e. we conjecture that higher ISP alphas are a function of more ISP experience.

We assume here 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. We discuss the case of industry-specific skill and tenure in Section V below.

B. An 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.

We start by the definition of experience given in the American Heritage Dictionary of the English Language (2000). According to the dictionary, experience is "active participation in events or activities, leading to the accumulation of knowledge or skill," suggesting that a defining feature of experience is that it comes from having to act in a particular period or event. This feature is also highlighted in the quote by Arrow (1962) cited in the introduction. But when will a fund manager be particularly "active" and "working towards solving a problem"? We conjecture that managers are relatively active, and that problem solving becomes particularly relevant, when times are rough. Our proposed experience measure therefore counts the number of times a manager has experienced what we label *industry shocks*.

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 cross-sectional approach is in line with the fact that rankings and relative performance are of particular importance in the mutual fund industry (e.g., Brown, Harlow, and Starks (1996)).

Clearly, learning and experience are multi-dimensional, and fund managers may get experience from many different sources. Our objective is not to provide an all-encompassing measure of experience, but, more narrowly, to identify states of the world in which learning about one particular industry is particularly likely. We believe industry shocks are a natural candidate, and their use can be justified on at least three, not mutually exclusive, grounds.

First, the dictionary definition, Arrow's quote, and the pilot metaphor all suggest that problem solving is important in the accumulation of skill. Low returns, which come with industry shocks, are the central problem for fund managers. Second, industry shocks may capture underlying economic events that may make it more profitable to rationally direct attention to those industries in an attempt to understand the current set of industry-fundamentals better. Thus, there may be rational incentives to learn in industry shock periods. Third, the focus on learning in bad times is supported by a large literature on organizational learning. For example, in a widely-cited survey article on organizational learning, Lapré and Nembhard (2010) write:

> Failure experience is theorized to be a particularly effective stimulant for learning because it is highly salient, directly challenges the notion that current practices are adequate, and thereby provokes interest in identifying and developing alternative approaches. Failures [...] create an urgency to reflect, challenge old assumptions, and innovate to achieve aspirations. [...] Several studies have shown that organizations do not initiate change when their performance is satisfactory or successful, but do embrace change when their performance is poor.

There are some strong similarities to what we think is important in our fund manager setting. Low returns in industry shocks, and associated scrutiny by investors, are "highly salient" to fund managers and the low returns may plausibly "challenge the notion that current practices are adequate" and "provoke interest in identifying and developing alternative approaches." Experiencing low returns in industry shocks may "create an urgency to reflect, challenge old assumptions, and innovate to achieve aspirations." In sum, we believe all three of the above arguments provide support for the use of industry shocks in our experience measure.

Table I lists industry shock quarters from 1992 to 2012. 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 an industry return of 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 Internet Appendix that our results get stronger when we impose the additional restriction that an industry shock quarter must have a negative industry return.

With the definition of industry shocks in hand, we define our main experience measure for fund manager m in industry i and quarter q as:

$$E_{mqi} = \sum_{\tau<q} \mathrm{IS}_{i\tau} \times I[w_{m,\tau-1,i} > 0.1], \tag{4}$$

where IS stands for an industry shock in industry i in quarter τ. We update E_{mqi} after each industry shock quarter. E_{mqi} varies within a manager-quarter 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.

$I[w_{m,\tau-1,i} > 0.1]$ 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%. This captures the natural assumption that learning occurs predominantly in domains of interest for decision-makers. Intuitively, if I am not exposed to an industry, a negative return in that industry is not a problem that I need to solve. Of course it is possible that managers learn, in an absolute sense, from an industry shock even if they do not hold that industry, or even if they hold very little of it, perhaps by reading about the other industry or by talking to other fund managers. But we argue that managers learn more, in a relative sense, in industries to which they are more exposed. In terms of our pilot metaphor, a pilot learns more from actually experiencing heavy turbulence then from simply reading about it. We examine the relation between exposure and learning further in Section III.D 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. If the most skilled managers could anticipate the shock, they would scale back their exposure, and therefore be less likely to acquire experience by our measure. This would bias us against our hypothesis that managers with high values of E_{mqi} outperform.

C. Data

The starting piece of information is the fund manager's identity, obtained from Morningstar Direct. We combine this with information from the CRSP Mutual Funds Fund Summary table, and we manually screen the resulting merge.[4] 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 attention to funds 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

[4]An earlier version of this paper used CRSP as the only source for fund manager names. Since recent literature (e.g., Massa, Reuter, and Zitzewitz (2010), and Patel and Sarkissian (2014)) shows that Morningstar has a significantly more accurate coverage of fund manager names, we use this database as our main source for names, in line with recent related work (e.g., Berk and van Binsbergen (2012), Pástor, Stambaugh, and Taylor (2014)). To match Morningstar to CRSP, we follow the procedure described in Pástor, Stambaugh, and Taylor (2014). In a small number of cases where Morningstar Direct does not provide a fund manager name but CRSP does, we use the information from CRSP. Furthermore, 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 from 2006 to 2008.

managed equity funds with total net assets under management of at least \$5 million. We group together multiple share classes of the same fund using the Morningstar Direct and CRSP portfolio identifiers.

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 II, Panel A describes our sample, which covers the period from 1992Q1 to 2012Q1. We have a total of 81 quarters, 4,024 fund managers in 2,609 funds and 26,612 unique ISPs. Funds have on average 10.0 ISPs per quarter, and an ISP "lives" for, on average, 29.5 quarters (median = 27.0). Managers are on average in our sample (managing any ISP) for a total of 24.6 quarters (median = 20.0). Panel B presents summary statistics for the industry shock indicators (IS) and the experience measure across all 441,282 manager-industry-quarter observations. About 8% of our observations come from industry shock quarters. The average of the experience measure is 0.37, and the maximum number of industry shocks experienced by a manager in our sample for a given industry is 13.

D. Measuring Fund Manager Performance

We present results from two broad approaches to measure the performance of fund managers. The first approach is based on measuring ISP performance from holdings. The second approach is based on analyzing trades. Because we observe fund holdings only at quarterly frequency, our performance measures do not capture managerial actions and trading within the quarter. Throughout, performance is measured before fees.

D.1. Holdings-Based Approaches

We start by constructing a series of daily ISP returns for all ISPs in our sample. The raw ISP return, R_{mtiq}, is defined as a weighted average of the returns of stocks in that ISP:

$$R_{mtiq} = \sum_{j \in i} w_{mij,q-1} R_{jt}, \tag{5}$$

where m denotes the fund manager, t denotes the day within the quarter, i denotes the ISP's industry, q denotes the current quarter, and $w_{mij,q-1}$ is the weight of stock j in the ISP at the end of the quarter $q-1$.

Our main measure of performance is the standard 4-factor model (Fama and French (1993), Carhart (1997)). Specifically, ISP performance is the α from the following regression which we run across all days t for each ISP in quarter q:

$$R_{mtiq} - R_{ftq} = \alpha_{mqi} + b_{mqi}\text{RMRF}_t + s_{mqi}\text{SMB}_t + h_{mqi}\text{HML}_t + m_{mqi}\text{UMD}_t + \varepsilon_{mtiq}. \tag{6}$$

R_{mtiq} is the return from equation (5), R_{ftq} is the risk-free rate, and RMRF, SMB, HML and UMD are the standard factors obtained from Kenneth French's website. We multiply α_{mqi} by 63 trading days and refer to this number as the risk-adjusted 4-factor ISP return, or, for brevity, the FFC alpha.

To minimize concerns that our results are specific to any one performance measure, we also use several other measures proposed in the literature. First, we also report results based on raw returns. Second, we use the 3-factor model. Third, Cremers, Petajisto, and Zitzewitz (2013) argue that mutual fund performance measures based on the standard factors can be biased, and propose alternative factors. We therefore use their 4-factor model, which replaces the factors in equation (6) with proxies for those factors constructed from benchmark indexes. Fourth, we also use their 7-factor model.[5] Fifth, as we are using daily data, stale prices could potentially be

[5]Those models are labeled IDX4 and IDX7 in Cremers, Petajisto, and Zitzewitz (2013). We refer the reader to that paper for details on the factor construction. We obtain the factor return data from Antti Petaijsto's website.

an issue. We therefore also report results from a Dimson (1979) correction as implemented by Lewellen and Nagel (2006), who estimate equation (6) with the sum of three lags of the excess market return as an additional factor.

Next, as an alternative to factor models, we present results from the characteristic-adjusted holdings-based performance measure of Daniel, Grinblatt, Titman, and Wermers (1997) (DGTW). Gormley and Matsa (2014) point out that regressing the DGTW measure on other variables, in our case the experience proxy, will generally lead to biased estimates. Following a recommendation by those authors, we therefore present results also for a modified DGTW approach (DGTW*) in which we regress the FFC alpha on a full set of benchmark-quarter fixed effects. To get ISP benchmarks, we compute for each ISP-quarter the weighted average book-to-market, size, and momentum quintiles of all stocks in that ISP. DGTW* therefore combines the within-quarter risk-adjustment of the 4-factor model with the benchmark-adjustment of the DGTW approach.

Finally, we also use a performance measure due to Cohen, Coval, and Pástor (2005) that measures performance of a fund managers by their holdings of stocks that are concurrently held by other skilled managers. We use the 4-factor alpha as an input to constructing this measure.

D.2. Trading-Based Approach

Analyzing changes ("trades"), rather than levels of portfolio holdings, has been suggested as a potentially more powerful way of detecting skill (e.g. Chen, Jegadeesh, and Wermers (2000), Kothari and Warner (2001)). We therefore use a trading-based approach as a second way to measuring ISP performance. Section IV discusses the method and results in greater detail.

III. Measuring Performance from Holdings

A. Sample Splits

Table III presents summary statistics for key variables in our dataset when we split our sample of ISP-level observations by fund manager ISP-level experience. Experienced ISPs represent about

19% of our total observations, and the mean experience level in this group is 1.96.

The average ISP has a 4-factor alpha before fees of 41 basis points per quarter, which is roughly in line with the fund-level estimates reported in Kacperczyk, Sialm, and Zheng (2005). There is a considerable difference in alphas across subsamples: consistent with our main hypothesis, the 4-factor alpha for experienced ISPs is a full 100 basis points higher. The results are very similar for the other risk-adjusted performance measures we consider, including the Fama and French (1993) 3-factor alpha (FF), Cremers, Petajisto, and Zitzewitz (2013) 4- and 7-factor alphas (CPZ4, CPZ7), 4-factor alpha with Dimson correction as in Lewellen and Nagel (2006) (LND), and the Cohen, Coval, and Pástor (2005) "company you keep" 4-factor alpha (CCP). While the difference is not statistically significant in this simple sorting exercise, experienced managers also outperform their inexperienced counterparts using the DGTW measure.

Note that experienced and inexperienced ISPs hold different types of stocks. Experienced ISPs have similar market betas, but load significantly less on value, size and momentum. As exposure to those factors is associated with a risk premium, this explains why we see no meaningful difference in raw returns, but substantial differences in the other models. Controlling for value, size, and momentum is thus important to accurately compare managerial performance by experience.

Experienced ISPs are larger, older, and part of larger and older funds. Experienced ISPs have larger industry shares, i.e., funds hold more of their assets in experienced industries. As we require industry shares to exceed 10% in order for experience to increase, this difference is partly by construction.

Following Kacperczyk, Sialm, and Zheng (2005) we compute an Industry Concentration Index (ICI). ICI is the sum of what we label "ICI components". ICI components are for each fund-industry-quarter the squared deviation of the industry share of the fund from the average industry share across all funds in this industry and quarter. The data show that ICI, a fund-level variable, differs only little across experienced and inexperienced ISPs. By contrast, the ICI components of experienced ISPs deviate substantially from the average ISP. Note that the fixed effects we use in our main tests below eliminate any variable on the fun-level, including ICI, so the main results in this paper are orthogonal to the results presented in Kacperczyk, Sialm, and Zheng (2005).

Turning to managerial characteristics, managers of experienced ISPs have significantly longer tenure and industry tenure. Interestingly, we find no meaningful difference between experienced and inexperienced managers in terms of SAT score of their undergraduate institution, which we were able to collect for a subsample of 839 fund managers. This provides first evidence suggesting that the better performance of experienced managers we document is unrelated to baseline skill.

To provide some insight into how performance differences evolve over time, Figure I shows the cumulative 4-factor risk-adjusted performance from investing in a hypothetical portfolio of ISPs of experienced and inexperienced managers. Over our 20 year sample period, the experienced ISP portfolio has a performance of almost 110%, while the inexperienced ISP portfolio yields a risk-adjusted return close to zero over most of the sample period.

B. Regression-Based Evidence

The sorting results from the previous section show that experienced ISPs outperform by most standard performance measures. This is in line with managers learning from past industry shock experience. In this section we analyze if those sorting results carry over to a more rigorous multivariate setting.

As our baseline, we estimate the following version of equation (3):

$$\alpha_{mqi} = \lambda_{mq} + \beta_1 I(E_{mqi} > 0) + \beta_2 X_{mqi} + \varepsilon_{mqi}. \tag{7}$$

Here λ_{mq} are manager $\times$ quarter fixed effects; $I(E_{mqi} > 0)$ is an indicator 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 on ISP performance.

The manager $\times$ quarter fixed effects ensure that estimates are not driven by any variable that is fixed for the same manager in a given quarter. As highlighted above, this includes tenure, baseline skill, fund characteristics, and economy-wide effects. In most of our tests, X_{mqi} includes a dummy equal to one if the ISP's industry is going through an industry shock in the current quarter, because it is correlated with both experience and our performance measures. 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:

$$\varepsilon_{mqi} = \nu_{mq} + \nu_{qi} + \bar{\nu}_m + \bar{\nu}_q + \eta_{mqi}, \tag{8}$$

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 $\times$ quarter fixed effects parametrically 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)).[6]

Table IV, Panel A presents presents our main results. The difference in risk-adjusted performance between experienced and non-experienced ISPs is 1.38% per quarter using the FFC 4-factor risk adjustment. This effect is economically large and shows that the unconditional 4-factor alpha difference documented in Table II cannot be explained by time-invariant factors on the manager-quarter level, such as tenure and skill. While we find a statistically and economically meaningful difference even in raw returns, the fact that experienced ISPs load less on value, size, and momentum means that results get stronger once we adjust performance for these factors.

The alternative performance measures we consider also show a positive association between experience and performance. The CPZ and CCP measures yield an experience effect of about 1.1% to 1.3%, and the LND measure suggests that stale prices are not an issue in our setting. The return difference is smaller for the DGTW measure, but, at 56 basis points per quarter, still

[6]We show in the Internet Appendix that double-clustering by industry-date and fund yields very similar results as our baseline.

very large in absolute terms. The modified DGTW measure, which adds benchmark × quarter fixed effects to the FFC model, yields a difference of 1.3%, similar to our benchmark.[7]

In sum, Panel A presents strong evidence for a positive link between experience and performance that is robust to different methods of risk-adjusting returns and shows up even in the raw returns. 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.[8] This non-linear specification allows us to test the incremental impact of additional units of experience. The panel shows that, across all our performance measures, the first unit of experienced is most valuable and that additional units of experience tend to increase relative outperformance at a decreasing rate. Figure II presents the experience-performance relationship graphically for selected performance measures. This evidence is informative, since decreasing marginal benefit of experience is exactly what we would expect if the experience variable captures learning. By contrast, if the results in Panel A were somehow spuriously induced by our empirical method, it would not be obvious why the relation is concave.

In Panel C, we repeat the analysis from Panel A including an interaction term between experience and the industry shock indicator IS. The aim is to see if past experience is particularly valuable inside or outside future shock periods. We find that experience is valuable outside industry shock quarters for all our performance measures. With the exception of raw returns, experience is even more valuable in future shock periods. Conditional on being in an industry shock quarter, experienced ISPs outperform inexperienced ISPs by 4.63% for the 4-factor risk adjustment.

Overall, the results from Table IV confirm the results from the univariate sorts. Experienced managers outperform inexperienced managers across a range of performance measures, experience

[7] The number of observations drops in columns (4) and (5) due to the availability of CPZ portfolio returns and in columns (8) and (9) due to the availability of DGTW-benchmark-assignments from Russ Wermers' website.

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

is beneficial at a decreasing rate, and relative outperformance is particularly pronounced in industry shock quarters. Because of the manager $\times$ quarter fixed effects, the experience effect cannot be driven by tenure, baseline skill, or any other variable that does not vary within manager and date.

C. Placebo Tests

We run two placebo tests to make sure our findings are neither spuriously induced by how we construct the experience measure, nor by how we run our regressions. In the first test, we generate 10,000 sets of placebo industry shocks, where we randomly choose one industry every quarter and assign it an industry shock. 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 positively correlated ($\rho = 0.4$ in our sample) because they can only go up. To make sure we are not picking up this correlation, we include both true and placebo experience measures in our regressions. The aim of the placebo test is then 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 III summarizes the results. The placebo coefficients are centered near zero and are often negative. By contrast, the coefficient on the true experience variable is centered near the baseline estimate of 1.38. The distribution of the true estimates is much tighter than the distribution of the placebo estimates. Even the largest coefficient we see on the placebo measure across all 10,000 runs is smaller than our baseline estimate of 1.38. These results are reassuring. They show that it is very unlikely that our experience measure is large and significant by chance. There is nothing in the construction of the variable, or the econometric approach, that would mechanically 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.

As a second robustness test, we follow the bootstrap method of Kosowski, Timmermann, Wermers, and White (2006) and simulate 1,000 samples of ISP returns, imposing that alpha in the simulated data is zero. This procedure, described in greater detail in the Internet Appendix, is using as an input 4-factor model residuals obtained from estimating equation (6). The results presented in the Internet Appendix show that *all* alphas and *all* t-statistics across the 1,000 placebo runs are substantially below the alphas and t-values we find in our baseline analysis. There are two implications. First, those results show there is nothing mechanical in our procedure that would lead us to obtain higher alphas for experienced ISPs. We correctly fail to detect alpha in a placebo test where there is none by construction. Second, and more importantly, those results show that the large difference in alphas between experienced and inexperienced ISPs we find in our main tests cannot be induced by sampling variation ("luck") alone, even if we account for the fact that the cross-sectional distribution of alphas may be distinctly non-normal.

D. Exposure and Learning Intensity

Our definition of experience in equation (4) requires managers to hold at least 10% of their portfolio in a given industry before they can acquire experience.[9] In this section, we analyze the role of the weighting term in greater detail.

We start by asking if it is important to include a weighting term in the first place. We therefore run a horserace between our experience measure and an otherwise identical measure without the 10% requirement. The alternative measure, which we call Past IS, is then a simple count of the past industry shocks experienced by a manager. Table V, specification (1) – which is otherwise identical to Table IV, specification (3) – shows that the Past IS measure has essentially no power to explain performance, while our baseline experience measure is largely unchanged. This directly

[9]In the Internet Appendix we show that the specific functional form of the indicator function is not very important. We obtain very similar results when we replace the 10% cutoff with an indicator that is one for the largest three industries for each fund, or with an indicator that is one if the weight assigned to an industry in a given fund is higher than the median industry weight across all funds.

shows that the weighting term is important: completely consistent with a learning story, putting more weight on an industry shock when managers are more exposed to that industry increases our ability to explain ISP performance.

The 10% threshold is close to an equal-weighted portfolio across the 12 industries we use. Managers may research more intensively industries with higher weights in their portfolios, so larger exposures may imply more learning. To investigate this, we replace the 10%-threshold indicator by the raw industry weight. Specifications (2) and (3) show that this modified measure has very similar properties to our baseline. The key advantage of the modified measure is that it allows us to isolate the impact of the industry weight from the number of industry shocks, which we do in specification (4). To conduct this test, we group all ISPs with positive modified experience measure into three groups by industry weight *conditional on* the number of shocks experienced. Hence, the high group contains ISPs that, for a given number of shocks, have been substantially exposed to the shock industry (the average industry weight in this group is 21.6%), while the average exposure in the low group is positive, but closer to zero (average industry weight is 4.0%).

The results in specification (4) show that experience depends strongly on the industry weight. Managers with very large exposures have significantly better subsequent performance, while managers with small positive exposures have effects very close to the inexperienced group. These findings are consistent with the idea that larger exposure to a shock industry increases learning intensity.

E. Difference-In-Differences Results

We use a difference-in-differences approach as an alternative way to document learning effects. We start with all ISPs that go through an experience shock in quarter q and do not have any industry shock in the preceding and subsequent 4 quarters. The event window is then $t \in [-4, +4]$ around the experience shock quarter q.[10] To get a clean comparison group, we retain all other

[10]Similar results obtain with $[-5, +5]$ and $[-6, +6]$ event windows.

ISPs managed by the same manager in the same quarter with complete data in the event window (i) if they have the same industry tenure and (ii) if they do not go through an industry shock in the event window. We then test if the performance of the ISP that goes through an industry shock improves relative to the other ISPs managed by the same manager over the same period, by estimating:

$$\alpha_{mti} = \lambda_{mqt} + \beta I(\Delta E_{m0i} > 0) + \varepsilon_{mti}, \tag{9}$$

separately for each quarter t , where $I(\Delta E_{m0i} > 0)$ is an indicator function equal to one for ISPs that go through an experience shock in quarter $t = 0$ ("treated ISPs"), and where λ_{mqt} are manager $\times$ quarter $\times$ event-quarter fixed effects.

Table VI presents the results. As expected, ISPs that go through a shock do worse than other ISPs during the shock quarter. More interestingly, while there is no evidence of outperformance before the shock, there is strong evidence of outperformance after the shock. The difference-in-differences of performance between $t \in [-4, -1]$ and $t \in [+1, +4]$ is 2.53 percentage points ($t = 2.78$). Figure IV presents the results graphically. Consistent with learning from the industry shock experience, ISPs that go through a shock perform persistently better.

The results raise the bar for alternative explanations, because any omitted variable not captured by manager $\times$ date fixed effects would have to change precisely around the shock, and it would need to induce a long lasting performance differential between experienced and inexperienced ISPs. Note that both treatment and control group in our test live for the entire event period, so selection effects cannot explain those findings.

IV. Measuring Performance from Trades

Analyzing changes in holdings ("trades"), rather than levels, may be a more powerful way to detect skill, because trades more closely reflect active managerial decisions (e.g., Chen, Jegadeesh, and Wermers (2000)). Finding a positive relation between experience and performance using trades would be useful in our setting because it would further minimize concerns that our previous results are due to misspecified regression models. We provide two sets of trade–based results.

A. Performance of Buys versus Sells

In our first set of trade-based results, we compare the performance of buys and sells. For each ISP and holdings report date, we classify a stock in that ISP as a net buy, if the observed change in the portfolio weight from beginning to end of the holdings period is larger than what would be predicted from stock price appreciation alone; it is a net sell if the observed change is smaller than the predicted change. Specifically, we follow Kacperczyk, Sialm, and Zheng (2005) and define:

$$\mathrm{NB}_{siq} = 1 \quad \text{if} \quad w_{siq} - \frac{w_{siq-1}(1+R_{siq})}{\sum_s w_{siq-1}(1+R_{siq})} > 0 \tag{10}$$

where w_{siq} is the weight of a given stock s belonging to industry i in the fund's portfolio between two fund reporting dates $q-1$ and q, and R_{siq} is the stock's return between those dates. Net sells are defined analogously. This 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 regress, for each stock in each ISP and report date, its next-quarter return on a dummy equal to one if the stock was a net buy, an $E > 0$ dummy, an interaction term between the two, as well as different sets of fixed effects. We present results for three performance measures: raw returns, FFC returns, and DGTW returns and find overall very similar results.

The top panel in Table VII, specification (1) uses raw returns without any fixed effects. This is equivalent to computing the performance of a hypothetical equal-weighted portfolio long in stocks bought by each ISP and short in the stocks sold. Consistent with Chen, Jegadeesh, and Wermers (2000), buys outperform sells by a considerable margin for all fund managers. Importantly, buys outperform sells more for experienced managers. The difference is 38 basis points over the next quarter and highly statistically significant (t-statistic = 4.77).

The remaining two panels show we obtain very similar results when we use FFC or DGTW-adjusted returns instead of raw returns. This highlights an important advantage of the buy-sell approach: because we are essentially focusing on the difference between stocks bought and sold, risk-adjusting the individual returns does not matter much as long as buys and sells have similar risk characteristics. Omitted risk factors are therefore particularly unlikely to be an issue for those results.

Specifications (2) to (4) show that results are largely unchanged when we include manager $\times$ date, industry $\times$ date, or manager $\times$ industry fixed effects. This is reassuring and highlights that effects cannot be driven by any variable, observable or unobservable, that does not vary on those levels. The richness of the data allows us to even include manager $\times$ firm fixed effects alongside manager $\times$ date fixed effects. The resulting specification (5) indicates that trades by *the same manager* become better predictors of subsequent returns for *the same stock* after the manager obtains experience. This constitutes direct evidence in support of a learning story.

B. Trading around Earnings Announcements

In our second set of trade-based results, we analyze trades before earnings announcements. Earnings announcements are important corporate events in which fundamental information is revealed to the market. In addition, they are recurrent, and thus provide the fund manager with a natural opportunity to apply her experience. The literature has already established that some managers 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)).

To implement the test, we collect all earnings announcements occurring in our sample period from IBES. We then estimate:

$$CAR_{msiq} = \lambda + \beta_1 I(E_{mqi} > 0) + \beta_2 I(\mathrm{NB}_{siq} > 0) + \beta_3 I(E_{mqi} > 0) \times I(\mathrm{NB}_{siq} > 0) + \beta_4' X_{msiq} + \varepsilon_{msiq} \tag{11}$$

where CAR denotes the cumulative abnormal return over a three-day $(-1, +1)$ window around the following earnings announcement date, $I(\mathrm{NB}_{siq} > 0)$ is one for net buys and zero for net sells, X_{msiq} are control variables, and λ are fixed effects. The announcement return is defined as:

$$CAR = \prod_{t=-1}^{+1}(1 + R_{st}) - \prod_{t=-1}^{+1}(1 + \bar{R}_{st}) \tag{12}$$

where $\bar{R}_{st}$ is the return on a matching size and book-to-market portfolio, as in Hirshleifer, Lim, and Teoh (2009). Following those authors, we control for book-to-market, firm size, turnover, institutional ownership, reporting lag, and the number of analysts covering the stock in IBES.

Prior literature has documented $\beta_2 > 0$ in equation (11). Our key prediction is $\beta_3 > 0$ as, under the learning hypothesis, experienced managers are better able to trade in anticipation of earnings surprises. This is indeed what we find in Table VIII. Across the different specifications, net buying by experienced managers is associated with announcement returns that are 12 to 24 basis points higher. Relative to the baseline effect on the buy variable of between 28 and 44 basis points, this is an economically large increase. Consistent with the prior literature, the baseline effect on buys indicates that buying by mutual funds is a strong predictor of positive subsequent abnormal announcement returns.

As before, we include additional fixed effects. Specifications (2) to (4) show that results are largely unchanged when we include manager × date, industry × date, or manager × industry fixed effects. We also include manager × firm fixed effects alongside manager × quarter fixed effects in specification (5). The results provide again direct evidence of a learning story: the same manager becomes better at predicting earnings surprises for the same stock upon obtaining experience.

While we do not suggest trading before earnings surprises is the *only* channel through which learning could translate into higher returns, this analysis is useful for two additional reasons. First, the dependent variable in those tests are three-day returns and our results therefore unlikely depend on any specific risk-adjustment. 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.

In sum, Table VIII shows that experienced managers are better at predicting announcement returns, consistent with the hypothesis that the experience proxy captures learning by the fund managers. The robustness of the results to the inclusion of fixed effects make alternative stories particularly unlikely.

V. Industry-Specific Alternative Explanations

Our approach of comparing returns within manager across industries at the same point in time rules out a confounding impact of a large range of variables suggested to be important by the prior literature, including managerial baseline skill and tenure. In this section, we address three potential industry-specific concerns. First, are our results reflecting industry-specific managerial skill? Second, are there any omitted industry-level variables that drive our results? Third, can industry-specific attrition from the sample induce our results?

A. Industry-Specific Baseline Skill

The baseline model in equation (3) uses manager $\times$ quarter effects to eliminate the potentially confounding impact of unobserved managerial baseline skill, but *industry-specific* baseline skill would not be captured by those fixed effects.[11] To be clear about the difference, industry-specific baseline skill is a skill managers are endowed with *before* they enter our sample, perhaps because of a prior career in that industry, while experience is obtained in an industry *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, that is, there needs to be within-manager-and-date variation in skill across industries.

Our results in the previous section make an industry-specific baseline skill explanation unlikely. First, industry-specific baseline skill, a time-invariant difference in ability across industries within manager, by definition cannot explain our difference-in-differences results in Section III.E. Second, industry-specific baseline skill does not predict a decreasing marginal benefit of expe-

[11] Industry-specific tenure would also not be captured, but, since it is observable, it is easy to control for. We find all our results go through when we control for industry-specific tenure (see Internet Appendix).

rience. Third, it cannot explain why managers make better trades after obtaining experience in Table VII. Specifically, we find unchanged trading results when we control for manager $\times$ industry effects in specification (4) of Table VII, and therefore compare the same manager in the same industry before and after obtaining experience, which eliminates any manager-industry specific time-invariant variation, including industry-specific skill. Fourth, industry-specific skill cannot explain why the same manager gets better in predicting earnings surprises in specification (4) in Table VIII. In short, while industry-specific baseline skill could explain stable differences in performance, it does not explain the changes in performance after obtaining experience we document.

We provide additional results here. We start by using observable variables that should be highly correlated with industry-specific skill. The first one is the ICI component, measuring how much the industry share for a given ISP deviates from the average. Table IX, specification (1) 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.

The second industry skill proxy 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 might on average overweight it in her portfolio. Skilled managers might therefore be more likely to pass the threshold required to get an experience shock. Specification (2) shows that industry share indeed has a positive impact on fund performance. But, again, the experience coefficient is quite similar to our baseline model.

As a final proxy, we use the manager's pre–experience alphas (the average alpha while $E = 0$) as a direct estimate of industry-specific baseline skill. Specification (3) shows that the pre-experience alpha is strongly positively related to subsequent performance. The experience coefficient is lower when controlling for the pre-experience alpha but, at 88 basis points per quarter, still economically large and highly statistically significant.

All three variables above are positively related to performance, so there might be a role for industry-specific baseline skill. However, it is also possible that the variables themselves are mainly driven by learning effects, rather than industry-specific baseline skill. Because our focus is on identifying the role of experience, and because we have shown that our conclusions on the experience effect are largely unchanged by the inclusion of those variables, we do not pursue the issue further here.

A final concern could be that those ISPs that never obtain experience are short-lived underperformers. Our results could therefore be driven by the survival of more skilled ISPs. To address this issue, we repeat our analysis but drop all ISPs that never obtain experience from the sample. We are therefore comparing the same set of ISPs before and after obtaining experience. The results in specification (4) indicate that ISPs perform 1.2% per quarter better after becoming experienced, so ISPs that never become experienced are not a concern. We address further issues about attrition of underperforming ISPs from the sample in Section V.C.

Overall, we conclude that industry-specific baseline skill is not spuriously inducing our earlier results.

B. Omitted Industry-Level Variables

A second potential concern could be omitted industry variables, such as industry-level risk factors not captured by the four-factor model. Some of our evidence above should already attenuate this concern. For example, it is not obvious how omitted industry variables would explain a decreasing marginal benefit of experience. It is also not obvious how they would explain the larger experience effect for larger holdings, given that alpha is a percentage measure. Finally, it is not obvious how they would explain the long-lived effects documented in the difference-in-differences results. While those results raise the bar for explanations based on industry variables, we further examine the issue in this section.

We first examine the issue for the trade-based performance tests. For the buy-sell analysis, specification (3) of Table VII shows that the results are not materially affected by the inclusion of industry $\times$ date fixed effects. Hence, the outperformance of stocks bought by experienced managers is larger than the outperformance of stocks bought by inexperienced managers even if we compare trades in the same industry and quarter. For the earnings announcement tests (Table VIII), we find the same result: including industry-date fixed effects leaves our results almost completely unchanged. Hence, any omitted industry-level variable, including time-varying and unobservable ones, cannot induce those results.

Next, we examine the issue for the holdings-based performance measures. We first consider industry return as an additional control variable to our main holdings-based regression. Table X, specification (1) shows that our baseline results are essentially unaffected. Specification (2) adds 8 lags of industry returns to investigate if industry return dynamics, such as mean-reverting industry returns, impact our findings. Results indicate that industry dynamics are not driving our findings. This is consistent with our results in Table IV, Panel C: there we have shown that experienced managers tend to do particularly well in future industry shocks. If our documented outperformance were due to industry-level reversals, we should instead see that managers do especially well *outside* industry shocks, and poorly in industry shock quarters.

We next include industry volatility and 8 lags of industry volatility, measured as the standard deviation of daily returns in the quarter. Industries with greater uncertainty may be industries where smarter managers have an opportunity to apply their skill, which may be unrelated to experience. The results in specifications (3) and (4) show that this does not affect our estimates. Specification (5) shows that including both industry returns and industry volatility, together with their lags, is equally inconsequential for the experience effect.

Industry returns and industry volatility are observable variables that should capture a large fraction of potentially confounding variation at the industry level. It is still possible, however, that there are omitted unobservable time-varying variables, orthogonal to industry returns and volatility, driving some of our holdings-based results. We therefore include industry $\times$ quarter fixed effects along with the manager $\times$ quarter effects in specification (6). The coefficient on experience is now lower. It is important to note that, even then, the remaining effect of a risk-adjusted 88 basis points annually means that experience is a first-order driver of ISP returns. With a t-value of 2.96, the estimate is also highly statistically significant.

To sum up, we find for the holdings-based performance measures that the relation between experience and performance is unrelated to the observable variables industry return and industry volatility. If we include industry $\times$ quarter fixed effects, effects are attenuated, but still highly significant, both economically and statistically. The results from trading-based performance measures are completely immune to industry-specific factors. Overall, then industry-specific omitted variables are not affecting our main conclusion: experience is a first-order driver of fund manager performance.

C. Industry-Specific Attrition

Funds and fund managers decide in which ISPs they invest. Hence, a potential concern could be that worse ISPs leave our sample, thereby inflating our estimates. More technically, we have assumed in our baseline tests that exit is exogenous, while it may be endogenous.

Note that attrition is not an issue for us if selection is based on baseline skill, i.e. general ability that does not vary by industry. This is because the manager-date fixed effects control for any factor on that level, including any inverse Mills ratio from a well-specified Heckman selection model. Managers leaving the sample with *all* their ISPs is therefore not a problem for our estimates.

Theoretically, industry-specific attrition can affect our results in two ways. First, it could lead us to spuriously find learning effects when none are present. Second, it could point to a different learning mechanism, namely managers and funds learning about industry-specific fund manager skill (e.g., Seru, Shumway, and Stoffman (2010)).

While those concerns are theoretically valid, attrition after industry shocks is actually extremely rare in our data. The average probability of exiting an industry after going through an industry shock is 0.85%, relative to ISPs that went through a shock, and 0.07% relative to all ISPs in a quarter. For the average quarter in our data, this implies that 4 ISPs drop out of our sample following the shock. Those numbers are simply too small to have a meaningful impact on our findings. We present supporting evidence from a simulation exercise in the Internet Appendix.

A second argument why selection and attrition is unlikely a significant driver of our results comes from the difference-in-differences estimates in Section III.E. Those tests show that managers get better after a shock, i.e, alpha changes within manager, which is consistent with learning. By contrast, a selection/attrition story is about stable within-manager alphas spuriously correlating with experience through sample composition effects. Importantly, we condition on managers that stay in the sample for 9 quarters in the difference-in-differences regressions, so selection and attrition is, by construction, not a concern for those tests.

VI. Extensions

A. Learning from Industry Booms and Other Periods

The above findings support the idea that fund managers gain experience during industry shock quarters, i.e., in bad times. But managers might also learn from other periods, and in particular from booms. This may not be implausible 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 disproportionately 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. Consistent with the idea that learning in booms and busts are not symmetric, Lapré and Nembhard (2010) write in their survey of the organizational learning literature:

> "In contrast [to failure], success encourages preservation of the status quo, complacency about experimenting with new ideas, and risk aversion. Thus, success inspires a narrower scope of learning and change than failure." (our addition to the text in square brackets)

Hence, while there is reason to believe learning would be more pronounced in bad times than in booms, this is ultimately an empirical issue.

In Table XI we rerun our baseline regressions, including IS_n and E_n, where subscripts n denote IS and experience measures constructed on the n-th industry return rank, ordered from 1 (bust) to 12 (boom). We show results from 12 different regressions, one in each line. We include the baseline parameters IS_1 and E_1, the shock and experience measures we have been using all along, in all regressions as additional controls. This is necessary, because the other experience measures are correlated with our baseline experience measure (between 0.27 and 0.47 in our data). If we did not include baseline experience, we could not tell if an observed effect would obtain because it is actually in the data, or because the used experience measure is correlated with our baseline experience measure.

The first line in Table XI reproduces Table IV, Panel A, specification (3). 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. 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, while there is no clear pattern for the sign, size, and significance of the alternative experience measures.

Both the point estimate and statistical significance increases slightly for the highest industry ranks 11 and 12, although the pattern is not monotonic, and results for E_{11} are actually stronger than results for E_{12}. 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 other periods is at best weak.

B. 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. 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. 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 (4), using IS^{TS} instead of IS.

Table XII shows results that are qualitatively similar to the baseline case. Panel A splits the sample into experienced and non-experienced ISPs. Also here, we see that for most measures, experienced ISPs outperform inexperienced ISPs. Panel B replicates our regressions from Table IV. We again obtain qualitatively similar, quantitatively somewhat weaker, results. Specification (3) shows 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 86 basis points (t-value = 3.73). Overall, the data are consistent with the view that managers learn also from the time-series of industry-returns.

One interesting implication of the time-series findings is that they add a new dimension to the literature cited in the introduction that finds mutual funds tend to do better in recessions. 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 ($\rho = -0.54$).[12]

[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

C. Learning Spillover Effects

Our identification strategy is based on comparing ISPs for the same manager at the same point in time. This prevents us from identifying learning spillovers where what a managers learns in one ISP can be used to more profitably manager ISPs in other industries.

Importantly, any experience effect we uncover with our method is likely understating the true benefit of obtaining experience in a model which allows for experience spillovers. To see this, consider the polar case: if spillovers effects were very large, such that what a manager learns on one ISP can be transferred to another ISP one-for-one, we would not observe any experience effect using our method; both the ISP in the shock industry and all other ISPs for the same manager at the same point in time would have higher alphas, leaving the difference unchanged.

D. Experience at the Fund Level: EDX

In our last test, we investigate if the documented superior stock-picking ability of experienced managers at the ISP level shows up also at the fund level. We implement this in the simplest way by looking at a weighted average of the individual industry experience measures (equation (4)), 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: $\text{EDX}_{mq} = \sum_i w_{mi,q-1} E_{mqi}$. An advantage of EDX is that it is, in principle, 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 groups every month, low (bottom quintile of EDX), mid (quintiles 2 to 4), and high (top quintile). 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 XIII, Panel A, shows that some of our findings from the ISP-level carry over also to

zero correlation with the market factor ($\rho = -0.02$).

the fund-level. Specifically, high EDX funds outperform low EDX funds by an economically substantive margin, based on the point estimates. Before fees, the difference based on on the point estimates is 14.4 basis points per month, or 1.7 percentage points per year. High EDX funds have an alpha of 11.3 basis points per month (1.4% per year) which is statistically significant (t-statistic = 2.24). While before fees high EDX funds outperform and low EDX funds break even, we find that after fees high EDX funds break even, while low EDX funds underperform. Panel B shows that sorting on tenure does not have EDX's ability to produce a meaningful spread in alphas.

All portfolio alphas in Table XIII are measured quite imprecisely. Since EDX is a weighted average of ISP level experience, finding high standard errors may not be surprising. Moreover, this pure sorting exercise does not allow us to control for the fixed effects we used above in our ISP analysis. Given these caveats, it is remarkable that the results line up as expected based on the ISP-level analysis. In sum, we conclude that experience effects can be detected also on the fund level. Refining the measurement of experience on the fund level may be a promising topic for future research.

VII. 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 1.4% 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, including previously studied variables like age, tenure, education, IQ, corporate governance, fund characteristics, and the business cycle. Experience is associated with better trades, and the experience-performance relationship is increasing and concave. We find some supporting evidence that these results aggregate to the fund level. Measuring experience by a new EDX index that aggregates a manager's experience across ISPs, we find that high EDX funds outperform before fees, whereas low EDX funds do not.

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 in bad times, consistent with earlier investigations into learning by doing (e.g., Arrow (1962)). An important implication of our study for empirical researchers is that tenure might not be a powerful 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 investors should care about.

Our findings suggest a number of potentially fruitful areas for future research. For example, we have used a rather restrictive definition of experience on the industry-level, so our estimates of the value of experience may be lower bounds. It seems plausible that managers would also be able to obtain experience from other sources that do not vary by industry, and there may be learning spillovers across industries. It would be interesting to quantify the value of those other forms of experience. More broadly, it may be interesting to further explore how our ISP-level results can be optimally translated to the fund-level.

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Table I: 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	-0.131	2002q2	Business Equipment	-0.255
1992q2	Health	-0.059	2002q3	Business Equipment	-0.255
1992q3	Consumer Durables	-0.092	2002q4	Shops	0.002
1992q4	Oil, Gas, and Coal	-0.042	2003q1	Telecom	-0.109
1993q1	Health	-0.146	2003q2	Chemicals	0.054
1993q2	Consumer NonDurables	-0.075	2003q3	Telecom	-0.064
1993q3	Health	-0.024	2003q4	Shops	0.073
1993q4	Oil, Gas, and Coal	-0.072	2004q1	Consumer Durables	-0.023
1994q1	Health	-0.104	2004q2	Telecom	-0.026
1994q2	Consumer Durables	-0.065	2004q3	Business Equipment	-0.096
1994q3	Consumer Durables	-0.018	2004q4	Health	0.038
1994q4	Shops	-0.049	2005q1	Consumer Durables	-0.140
1995q1	Consumer Durables	0.005	2005q2	Chemicals	-0.054
1995q2	Oil, Gas, and Coal	0.032	2005q3	Consumer Durables	-0.025
1995q3	Oil, Gas, and Coal	0.020	2005q4	Oil, Gas, and Coal	-0.081
1995q4	Business Equipment	-0.037	2006q1	Utilities	-0.005
1996q1	Telecom	-0.022	2006q2	Business Equipment	-0.091
1996q2	Chemicals	-0.003	2006q3	Oil, Gas, and Coal	-0.028
1996q3	Telecom	-0.081	2006q4	Health	0.020
1996q4	Shops	-0.027	2007q1	Banks	-0.025
1997q1	Business Equipment	-0.044	2007q2	Utilities	0.003
1997q2	Utilities	0.055	2007q3	Consumer Durables	-0.061
1997q3	Chemicals	0.017	2007q4	Banks	-0.112
1997q4	Business Equipment	-0.109	2008q1	Banks	-0.153
1998q1	Utilities	0.048	2008q2	Banks	-0.166
1998q2	Manufacturing	-0.036	2008q3	Oil, Gas, and Coal	-0.265
1998q3	Banks	-0.212	2008q4	Consumer Durables	-0.397
1998q4	Oil, Gas, and Coal	0.004	2009q1	Banks	-0.234
1999q1	Utilities	-0.111	2009q2	Shops	0.082
1999q2	Health	-0.033	2009q3	Utilities	0.070
1999q3	Banks	-0.154	2009q4	Banks	-0.004
1999q4	Utilities	-0.077	2010q1	Utilities	-0.019
2000q1	Chemicals	-0.209	2010q2	Banks	-0.146
2000q2	Telecom	-0.142	2010q3	Banks	0.046
2000q3	Telecom	-0.118	2010q4	Utilities	0.038
2000q4	Business Equipment	-0.347	2011q1	Consumer Durables	0.007
2001q1	Business Equipment	-0.261	2011q2	Oil, Gas, and Coal	-0.057
2001q2	Telecom	-0.019	2011q3	Consumer Durables	-0.312
2001q3	Business Equipment	-0.347	2011q4	Business Equipment	0.080
2001q4	Telecom	-0.020	2012q1	Utilities	0.000
2002q1	Telecom	-0.089			

Table II: 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. The sample is based on all single–manager mutual funds in the union of the CRSP Mutual Funds and Morningstar Direct databases, with available information identifying the fund manager, over the period 1992Q1–2012Q1.

Panel A: Sample

Number of Quarters	81
Number of Managers	4,024
Number of Funds	2,609
Number of ISPs	26,612
Number of Manager-ISP combinations	46,366
Avg. Number of ISPs per Fund (Median)	10.0 (11.0)
Avg. Life of ISP in Quarters (Median)	29.5 (27.0)
Avg. Life of Manager in Quarters (Median)	24.6 (20.0)

Panel B: Experience and Industry Shock Variables

Variable	Mean	St.Dev.	Min	Median	Max	N
IS	0.08	0.27	0.00	0.00	1.00	441,282
Experience	0.37	1.03	0.00	0.00	13.00	441,282

Table III: Sample Splits

The table reports sample splits by experience for the main variables of interest. We report the sample average (All), the average for the subgroup of inexperienced managers ($E = 0$) and experienced managers ($E > 0$), as well as the t–statistic for the difference between the two subsamples. Reported t–statistics are based on standard errors that allow for clustering around industry × date in all rows except for fund manager characteristics, where standard errors allow for clustering at the manager level. Performance measures used are: Raw ISP returns, calculated as in equation (5), Fama and French (1993) three-factor alpha (FF), Fama–French–Carhart (1997) four-factor alpha (FFC), Cremers, Petajisto, and Zitzewitz (2013) four- and seven-factor alphas (CPZ4, CPZ7), four-factor alpha with Dimson correction as in Lewellen and Nagel (2006) (LND), Cohen, Coval, and Pástor (2005) "company you keep" four-factor alpha (CCP), and Daniel, Grinblatt, Titman, and Wermers (1997) CS-measure (DGTW).

Variable	All	$E = 0$	$E > 0$	t-stat
Experience	0.37	0.00	1.96	38.15
Performance Measures				
Raw ISP Return	3.19	3.20	3.12	-0.13
Market-Adjusted ISP Return	0.72	0.68	0.91	0.72
FF Alpha	0.43	0.30	1.00	3.74
FFC Alpha	0.41	0.22	1.23	5.32
CPZ4 Alpha	0.74	0.60	1.34	3.92
CPZ7 Alpha	0.76	0.60	1.46	4.55
LND Alpha	0.43	0.24	1.29	5.57
CCP Alpha	0.37	0.19	1.14	5.30
DGTW-Adjusted Return	0.39	0.35	0.54	0.82
Portfolio Characteristics				
4-Factor Loading MKT-RF	1.02	1.03	1.01	-2.26
4-Factor Loading HML	0.15	0.19	-0.01	-6.62
4-Factor Loading SML	0.25	0.26	0.20	-5.09
4-Factor Loading UMD	-0.01	0.01	-0.10	-5.41
Fund Age (quarters)	16.16	14.74	22.39	24.69
ISP Age (quarters)	14.77	13.22	21.51	27.96
Fund Size ($m)	959.51	894.79	1,241.82	16.10
ISP Size ($m)	94.55	68.82	207.84	24.47
Industry Share	0.11	0.10	0.19	32.48
Industry Concentration Index (ICI)	7.45	7.41	7.61	1.68
ICI Component	1.43	1.14	2.70	18.04
Fund Manager Characteristics				
Tenure (quarters)	12.78	10.82	21.32	30.91
Industry Tenure (quarters)	11.63	9.63	20.36	31.71
SAT Score	2,019	2,020	2,014	-0.53
N	441,282	358,993	82,289	

Table IV: Impact of Experience on ISP Returns

The table presents results from regressing ISP performance on experience. All regressions include manager × date fixed effects as well as an indicator variable equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). Panel A presents the baseline results for the performance measures described in Table III. In column (9), we assign each ISP-quarter to a DGTW-benchmark and then regress the ISP's FFC alpha on benchmark × date fixed effects. Panel B replaces the experience dummy by dummies indicating one, two, and above two units of experience. Panel C presents results for the interaction of experience with the industry shock indicator. t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

Panel A: Baseline

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.566	1.005	1.376	1.091	1.180	1.369	1.303	0.561	1.304
	(2.01)	(4.48)	(5.75)	(4.69)	(5.19)	(5.97)	(5.82)	(2.38)	(6.19)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel B: Incremental Benefit of Experience

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience = 1	0.408	0.850	1.185	0.812	1.006	1.239	1.121	0.453	1.124
	(1.87)	(4.05)	(5.60)	(4.05)	(5.02)	(5.61)	(5.68)	(2.26)	(5.90)
Experience = 2	0.686	1.280	1.621	1.391	1.536	1.548	1.523	0.576	1.507
	(1.56)	(4.00)	(4.42)	(3.89)	(4.61)	(4.58)	(4.48)	(1.60)	(4.86)
Experience ≥ 3	0.905	1.176	1.687	1.631	1.336	1.565	1.608	0.849	1.627
	(1.57)	(2.99)	(3.82)	(3.58)	(3.08)	(3.95)	(3.89)	(1.80)	(4.32)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel C: Effect on the Next Industry Shock

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.664	0.682	0.999	0.754	0.894	1.074	0.960	0.532	0.964
	(2.27)	(3.13)	(4.42)	(3.54)	(4.18)	(4.75)	(4.53)	(2.11)	(4.77)
IS × Experience	-0.944	3.103	3.627	3.226	2.735	2.829	3.290	0.285	3.307
	(-1.21)	(3.68)	(3.80)	(3.11)	(2.78)	(3.32)	(3.72)	(0.55)	(4.20)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.17	0.19	0.26

Table V: Industry Exposure and Learning Intensity

The table reports results when managers' learning intensity from industry shocks can vary with industry exposure. Column (1) reports the estimates of a regression of Fama–French–Carhart (FFC) alphas on fund manager experience and Past IS, the number of industry shocks that the fund manager has been exposed to throughout her career. Columns (2) to (4) present results for a modified experience measure: $E'_{mqi} = \sum_{\tau<q} w_{i,\tau-1} \times IS_{i\tau}$. Specification (4) sorts ISPs with positive modified experience measure into terciles based on industry weight, conditional on the number of industry shocks experienced. All specifications include manager × date fixed effects as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

		Modified E		
	(1)	(2)	(3)	(4)
Experience	1.317	1.124	0.878	
	(5.80)	(4.28)	(3.44)	
Past IS	0.039		0.091	
	(0.56)		(1.13)	
Experience: Low				0.207
				(0.83)
Experience: Medium				0.812
				(3.26)
Experience: High				1.792
				(5.75)
Manager × Date FE	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	441,282
R^2	0.16	0.16	0.16	0.16

Table VI: Difference-In-Differences Approach

The table reports differences in Fama–French–Carhart alphas between experienced and inexperienced ISPs in event time around an experience shock. ISPs with an experience-shock event are all ISPs that go through an experience shock in quarter q ($t = 0$ in event time) and do not have any industry shock in the preceding and subsequent 4 quarters. The control group consists of all other ISPs managed by the same manager in the same event quarter q with complete data in the event window $t \in [-4, +4]$ (i) if they have the same industry tenure and (ii) if they do not go through an industry shock in the event window. All specifications include manager $\times$ quarter $\times$ event-quarter fixed effects and t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry $\times$ date.

	Event time relative to industry shock quarter									
	-4	-3	-2	-1	0	+1	+2	+3	+4	Post - Pre
Experience	0.489	-0.265	-0.225	-1.440	-3.512	2.421	1.028	0.959	4.264	2.528
	(0.41)	(-0.30)	(-0.21)	(-1.08)	(-2.91)	(1.94)	(1.28)	(0.77)	(2.64)	(2.78)
N	6,052	6,052	6,052	6,052	6,052	6,052	6,052	6,052	6,052	48,416
R^2	0.31	0.30	0.31	0.27	0.30	0.29	0.31	0.29	0.29	0.29

Table VII: Returns on Buys and Sells

The table reports next-quarter returns of stocks purchased and sold by experienced and inexperienced managers. Each stock is classified at each fund-report date as either a net buy, or a net sell. A stock is a net buy (sell) if the fund manager increases (decreases) the weight of the stock in the overall portfolio net of price appreciation. Quarterly raw stock returns, Fama–French–Carhart (FFC) alphas, and DGTW-adjusted stock returns are regressed on a dummy equal to one if the stock is a net buy in a given fund, an $E > 0$ dummy, an interaction term, as well as different sets of fixed effects. The unit of observation is the fund-stock-date level and t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the stock level.

	Next-Quarter Outperformance of Stocks Bought vs. Stocks Sold				
	(1)	(2)	(3)	(4)	(5)
Raw returns					
$E = 0$	2.521	3.034	2.785	3.095	4.620
$E > 0$	2.899	3.347	3.169	3.424	5.057
Difference	0.378	0.313	0.384	0.329	0.437
t-statistic	(4.77)	(4.65)	(6.06)	(4.88)	(4.92)
FFC-Alphas					
$E = 0$	2.080	2.268	2.113	2.300	3.575
$E > 0$	2.479	2.685	2.517	2.657	3.949
Difference	0.399	0.417	0.404	0.357	0.374
t-statistic	(6.25)	(6.48)	(6.49)	(5.64)	(4.77)
DGTW-Adjusted Return					
$E = 0$	2.499	2.812	2.566	2.858	4.119
$E > 0$	2.844	3.154	2.968	3.211	4.632
Difference	0.345	0.342	0.402	0.353	0.513
t-statistic	(4.82)	(4.75)	(5.86)	(4.88)	(5.34)
Manager × Date FE	No	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	Yes	No	No
Manager × Industry FE	No	No	No	Yes	No
Manager × Firm FE	No	No	No	No	Yes

Table VIII: Experienced Managers' Trades Around Earnings Announcements

The table reports next-quarter earnings announcement returns of stocks bought and sold by experienced and inexperienced managers. Each stock is classified at each fund-report date as either a net buy, or a net sell. A stock is a net buy (sell) if the fund manager increases (decreases) the weight of the stock in the overall portfolio net of price appreciation. Cumulative abnormal returns over a 3-day window $(-1, +1)$ around the earnings announcement are regressed on a dummy equal to one if the stock is a net buy in a given fund, an $E > 0$ dummy, an interaction term, firm controls, and different sets of fixed effects. 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). The unit of observation is the fund-stock-date level and t-statistics, reported in parentheses, are based on standard errors allow for clustering at the stock level.

	(1)	(2)	(3)	(4)	(5)
Net Buy	0.281	0.313	0.296	0.321	0.441
	(13.86)	(14.31)	(14.43)	(14.81)	(15.47)
Experience	-0.031	-0.016	-0.017	-0.049	-0.108
	(-0.77)	(-0.32)	(-0.63)	(-0.60)	(-0.98)
Experience × Net Buy	0.120	0.130	0.143	0.135	0.237
	(3.34)	(3.57)	(3.98)	(3.67)	(4.95)
B/M	0.021	0.029	0.038	0.029	0.226
	(0.84)	(1.13)	(1.47)	(1.15)	(3.27)
Size	-0.047	0.008	0.007	0.007	-0.720
	(-1.60)	(0.27)	(0.24)	(0.24)	(-6.62)
Turnover	-0.027	-0.027	-0.027	-0.028	-0.020
	(-3.06)	(-3.16)	(-3.08)	(-3.26)	(-1.18)
IO	1.125	1.043	1.000	0.971	-1.180
	(5.66)	(5.27)	(5.09)	(4.96)	(-2.17)
Reporting lag	-0.019	-0.019	-0.018	-0.019	-0.008
	(-4.52)	(-4.63)	(-4.64)	(-4.79)	(-1.08)
$\log(1 + Analysts)$	0.037	0.048	0.038	0.037	0.092
	(0.64)	(0.84)	(0.67)	(0.65)	(0.96)
Manager × Date FE	No	Yes	Yes	Yes	Yes
Industry × Date FE	No	No	Yes	No	No
Manager × Industry FE	No	No	No	Yes	No
Manager × Firm FE	No	No	No	No	Yes
N	1,511,560	1,511,560	1,511,560	1,511,560	1,511,560
R^2	0.01	0.03	0.05	0.05	0.37

Table IX: Experience and Industry-Specific Skill

The table reports various checks for the potential effects of industry–specific baseline skill. In all specifications, the dependent variable is the Fama–French–Carhart alpha. In column (1), we include the average ICI component over the prior four quarters as an additional control variable. The ICI component is defined as the squared deviation of the industry share of a given ISP from the average industry share across all ISPs in a given quarter and industry. In column (2), the average industry share over the prior four quarters is included as a control variable. In column (3), we include the pre–experience alpha, defined as the average alpha obtained by a given manager in a given industry, as long as the value of the manager's experience in the industry equals 0. In column (4), the sample is restricted to all manager–industry combinations that at some point have a value of E greater than 0. All specifications include manager $\times$ date fixed effects as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry $\times$ date.

	(1)	(2)	(3)	(4)
Experience	1.378	1.237	0.875	1.201
	(5.66)	(5.23)	(3.54)	(3.73)
Average ICI component	0.013			
	(2.21)			
Average industry share		1.580		
		(2.11)		
Pre-experience alpha			0.762	
			(53.19)	
Manager $\times$ Date FE	Yes	Yes	Yes	Yes
N	262,839	262,839	436,169	134,233
R^2	0.17	0.17	0.33	0.36

Table X: Experience and Omitted Industry-Level Variables

The table reports estimates of our baseline model, using the Fama–French–Carhart alpha as the dependent variable, while controlling for additional industry-level variables. In column (1) we control for the current industry return. Column (2) adds 8 lags of industry returns. Columns (3) and (4) include industry return volatility, as well as 8 lags of industry volatility, respectively, as control variables. Industry volatility is defined as the standard deviation of the daily industry returns net of the market return in a given quarter. Column (5) simultaneously includes all previsouly used controls. Column (6) includes industry $\times$ date fixed effects. All regressions include manager $\times$ date fixed effects as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter (coefficient not shown). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry $\times$ date.

	(1)	(2)	(3)	(4)	(5)	(6)
Experience	1.233	1.091	1.240	1.219	1.093	0.220
	(4.77)	(4.64)	(5.34)	(5.41)	(5.00)	(2.96)
Industry Return	0.245	0.258			0.267	
	(5.46)	(5.80)			(6.28)	
Industry Volatility			1.368	1.238	1.504	
			(1.72)	(0.67)	(0.82)	
8 Lags of Industry Return	No	Yes	No	No	Yes	No
8 Lags of Industry Volatility	No	No	No	Yes	Yes	No
Manager $\times$ Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry $\times$ Date FE	No	No	No	No	No	Yes
N	441,282	205,960	441,282	205,960	205,960	441,282
R^2	0.18	0.20	0.16	0.17	0.20	0.34

Table XI: Learning from Industry Booms and Other Periods

This table reports results when learning can come from other periods. It shows coefficient estimates when the Fama–French–Carhart alpha is regressed on E_1, E_n, IS_1, IS_n and manager $\times$ date fixed effects. 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 (4). t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry $\times$ date.

Rank n	E_n	t-stat	E_1	t-stat	IS_n	t-stat	IS_1	t-stat
1 (Low)			1.376	(5.75)			-3.335	(-4.46)
2	0.172	(0.77)	1.351	(5.38)	-2.573	(-4.55)	-3.570	(-4.79)
3	0.367	(1.76)	1.306	(5.66)	-2.958	(-7.25)	-3.598	(-4.83)
4	0.029	(0.15)	1.330	(5.44)	-1.515	(-3.59)	-3.472	(-4.64)
5	-0.063	(-0.33)	1.372	(5.56)	-1.149	(-2.70)	-3.438	(-4.59)
6	0.268	(1.45)	1.287	(5.10)	-0.651	(-1.38)	-3.384	(-4.53)
7	-0.352	(-1.70)	1.448	(5.80)	0.244	(0.59)	-3.336	(-4.47)
8	-0.390	(-2.35)	1.467	(5.92)	0.071	(0.16)	-3.344	(-4.47)
9	-0.174	(-1.04)	1.442	(5.92)	0.986	(2.14)	-3.255	(-4.35)
10	-0.315	(-1.97)	1.449	(5.95)	0.951	(1.76)	-3.256	(-4.34)
11	0.350	(1.61)	1.202	(5.09)	2.859	(6.66)	-3.102	(-4.15)
12 (High)	0.239	(0.99)	1.113	(4.79)	3.096	(6.05)	-3.062	(-4.12)

Table XII: 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 (4), 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 presents regression results from Table IV using the time-series based experience measure. t-statistics, reported in parentheses, are based on standard errors that allow for clustering around industry × date.

Panel A: Summary Statistics by Experience

Variable	All	$E^{TS} = 0$	$E^{TS} > 0$	t-stat
Experience^{TS}	0.44	0.00	2.01	54.68
Performance Measures				
Raw ISP Return	3.19	3.24	2.98	-0.53
Market-Adjusted ISP Return	0.72	0.69	0.84	0.55
FF Alpha	0.43	0.34	0.74	2.26
FFC Alpha	0.41	0.27	0.91	3.54
CPZ4 Alpha	0.74	0.63	1.11	2.77
CPZ7 Alpha	0.76	0.66	1.11	2.62
LND Alpha	0.43	0.30	0.90	3.50
CCP Alpha	0.37	0.24	0.84	3.59
DGTW-Adjusted Return	0.39	0.38	0.42	0.22
N	441,282	344,630	96,652	

Panel B: Regression-Based Results

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience^{TS}	0.165	0.604	0.856	0.635	0.544	0.790	0.818	0.179	0.688
	(0.64)	(2.80)	(3.73)	(2.84)	(2.58)	(3.67)	(3.89)	(0.87)	(3.68)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.15	0.16	0.16	0.19	0.26

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

The table presents fund level results using the experience index EDX. For each quarter and fund, EDX is the weighted average of the individual ISP experience measures. In each quarter, funds are sorted into portfolios based on the value of EDX, low (below 20th percentile), medium (between the 20th and 80th percentiles), and high (above the 80th percentile). We compute the return of the respective portfolio as the TNA-weighted monthly return before and after expenses reported in CRSP. The abnormal return is the intercept from regressing the fund returns on the four FFC factors. Panel B repeats the analysis using tenure instead of EDX as a sorting variable. Below each coefficient estimate, the *t*-statistic is reported in parentheses. The table reports t-statistics in parentheses and the average number of individual funds in each 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	-0.031	-0.116	0.947	-0.090	0.148	0.097	271
	(-0.42)	(-1.58)	(42.51)	(-2.42)	(4.44)	(5.32)	
Mid EDX	0.026	-0.049	0.958	0.018	0.012	0.009	534
	(0.52)	(-0.97)	(54.11)	(1.16)	(0.86)	(0.85)	
High EDX	0.113	0.031	0.956	-0.132	0.090	0.015	210
	(2.24)	(0.62)	(55.98)	(-7.66)	(4.79)	(1.18)	
High – Low	0.144	0.147	0.010	-0.042	-0.057	-0.082	
	(1.62)	(0.10)	(0.34)	(1.02)	(-1.49)	(-3.72)	

Panel B: Sort on Tenure

	Abnormal return (% per month)		Factor loadings before expenses				
	Before expenses	After expenses	Market	Value	Size	Mom.	Avg. N
Low EDX	0.048	-0.039	0.948	-0.105	0.092	0.013	196
	(0.69)	(-0.57)	(36.54)	(-3.67)	(2.90)	(0.91)	
Mid EDX	0.061	-0.019	0.952	-0.064	0.061	0.021	600
	(0.99)	(-0.31)	(39.21)	(-3.30)	(3.82)	(1.94)	
High EDX	0.076	0.000	0.930	0.003	0.011	0.024	219
	(1.52)	(0.00)	(45.69)	(0.18)	(0.93)	(2.13)	
High – Low	0.027	0.040	-0.019	0.108	-0.081	0.011	
	(0.32)	(0.64)	(0.57)	(3.28)	(2.38)	(0.63)	

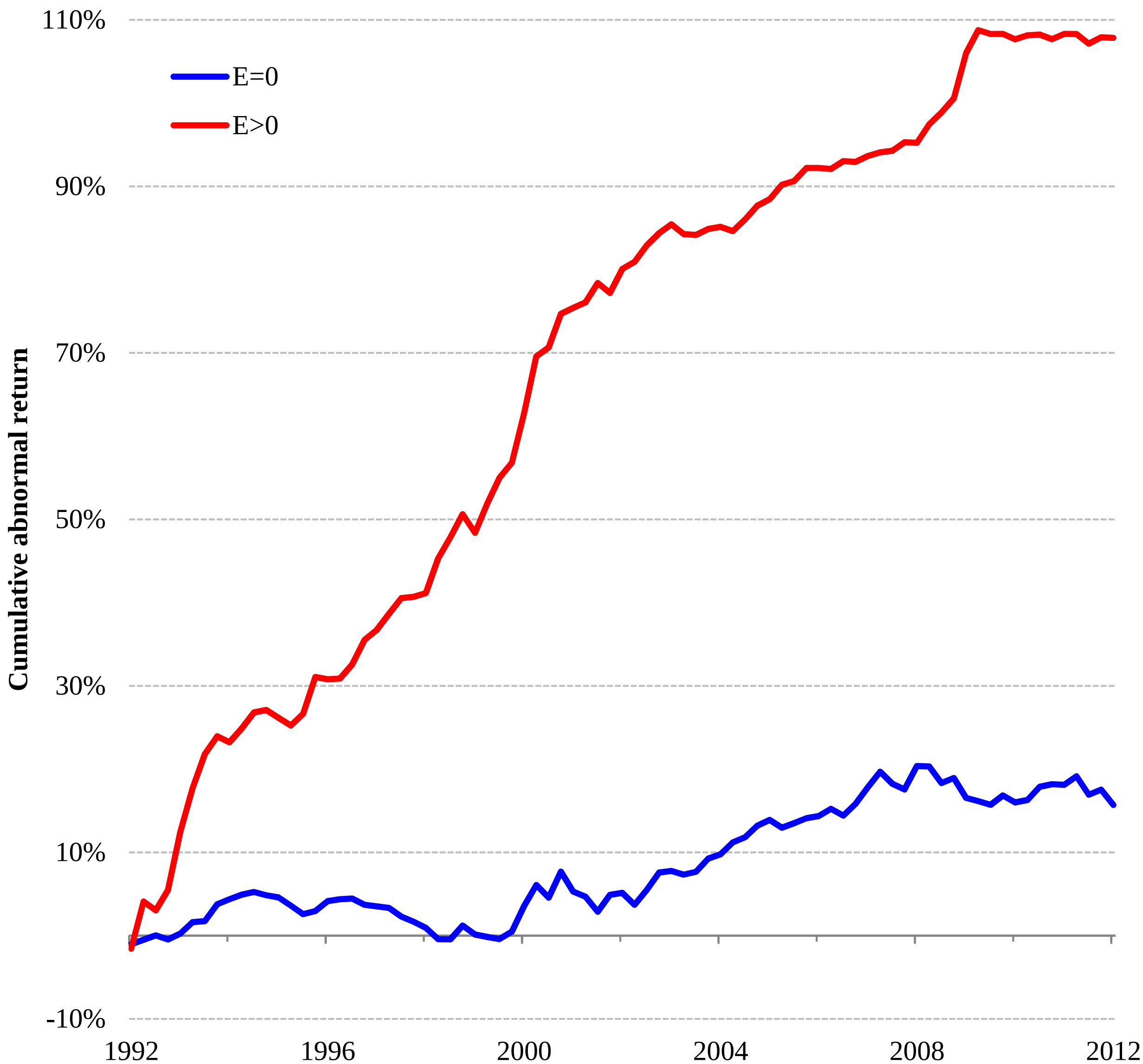

Figure I: Investing with experienced and inexperienced managers. The graph shows the cumulative abnormal return of a (hypothetical) equal-weighted, quarterly rebalanced, portfolio of ISPs by managers that have experienced an industry shock in the past ($E > 0$) as well as a portfolio of the remaining, inexperienced, ISPs. Returns are risk-adjusted using the Fama–French–Carhart model.

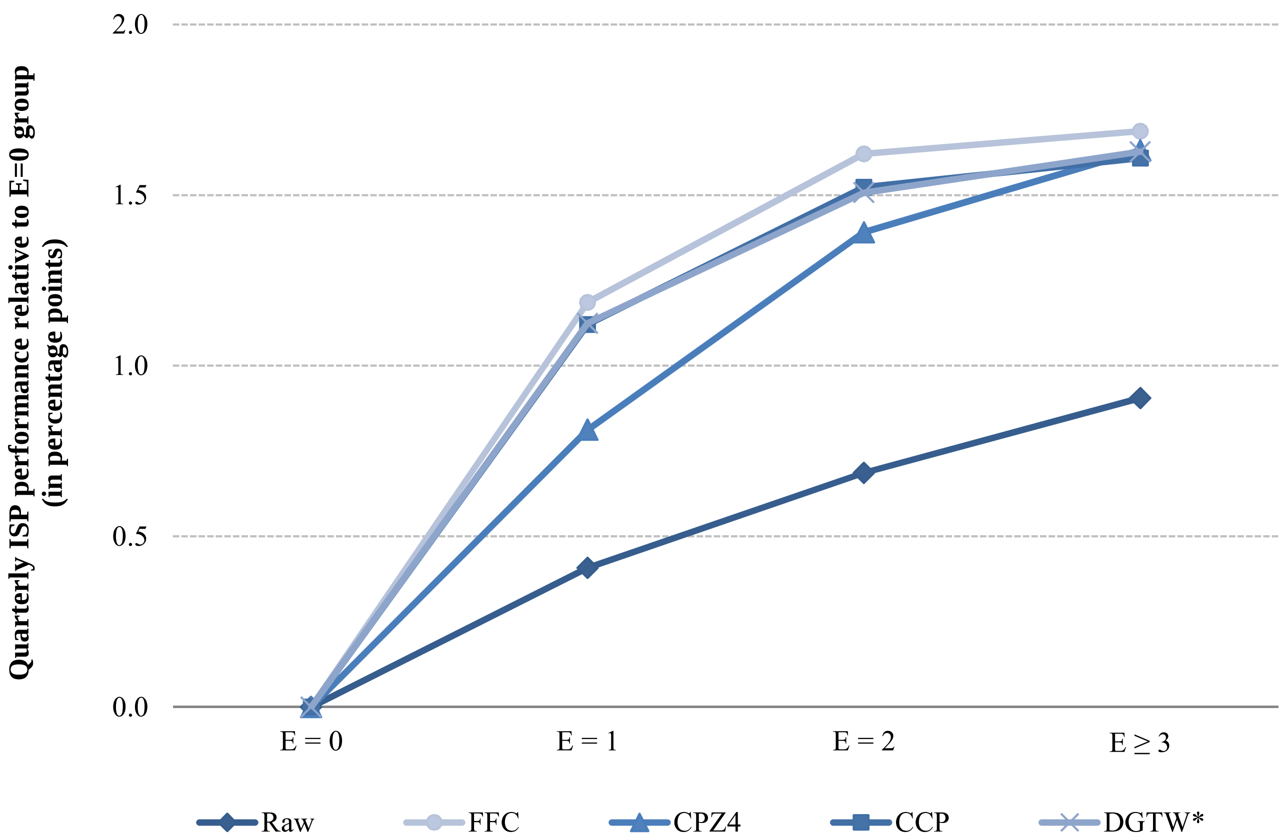

Figure II: The Experience-Performance Relationship. The graph plots the effect of one incremental unit of experience as estimated in Panel B of Table IV.

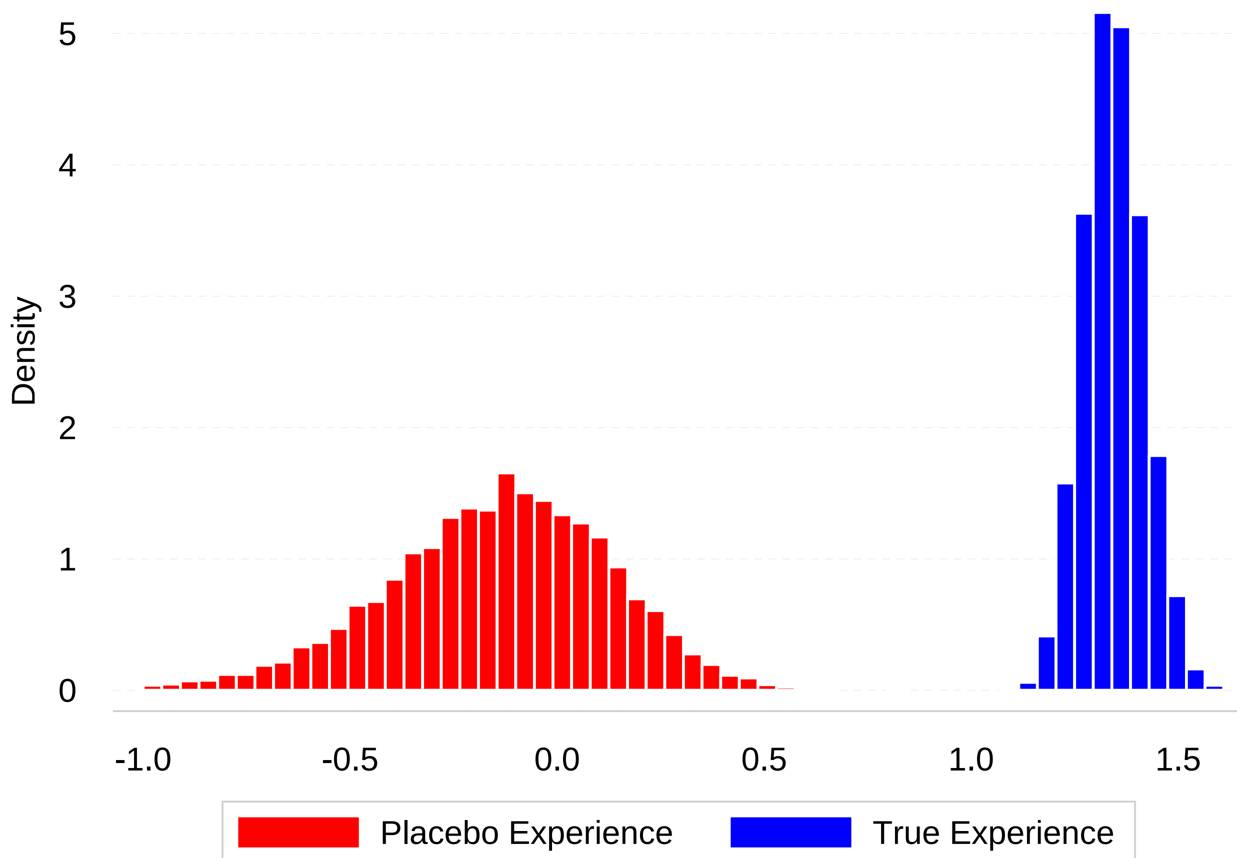

Figure III: Placebo Test. The figure presents results for the placebo test described in section III.C. A sequence of placebo industry shocks is generated by randomly selecting one Fama-French 12 industry every quarter as an industry shock quarter. Next, a pseudo experience measure is computed as in equation (4) based on this placebo industry shock series. We then rerun our baseline regression from Table IV, specification (3) with the placebo experience measure as an additional regressor. This procedure is repeated 10,000 times. The "True Experience" distribution is the histogram of the 10,000 coefficients on our baseline experience measure from the regression. The "Placebo Experience" distribution is the histogram of the 10,000 coefficients on the pseudo experience measure from the regression.

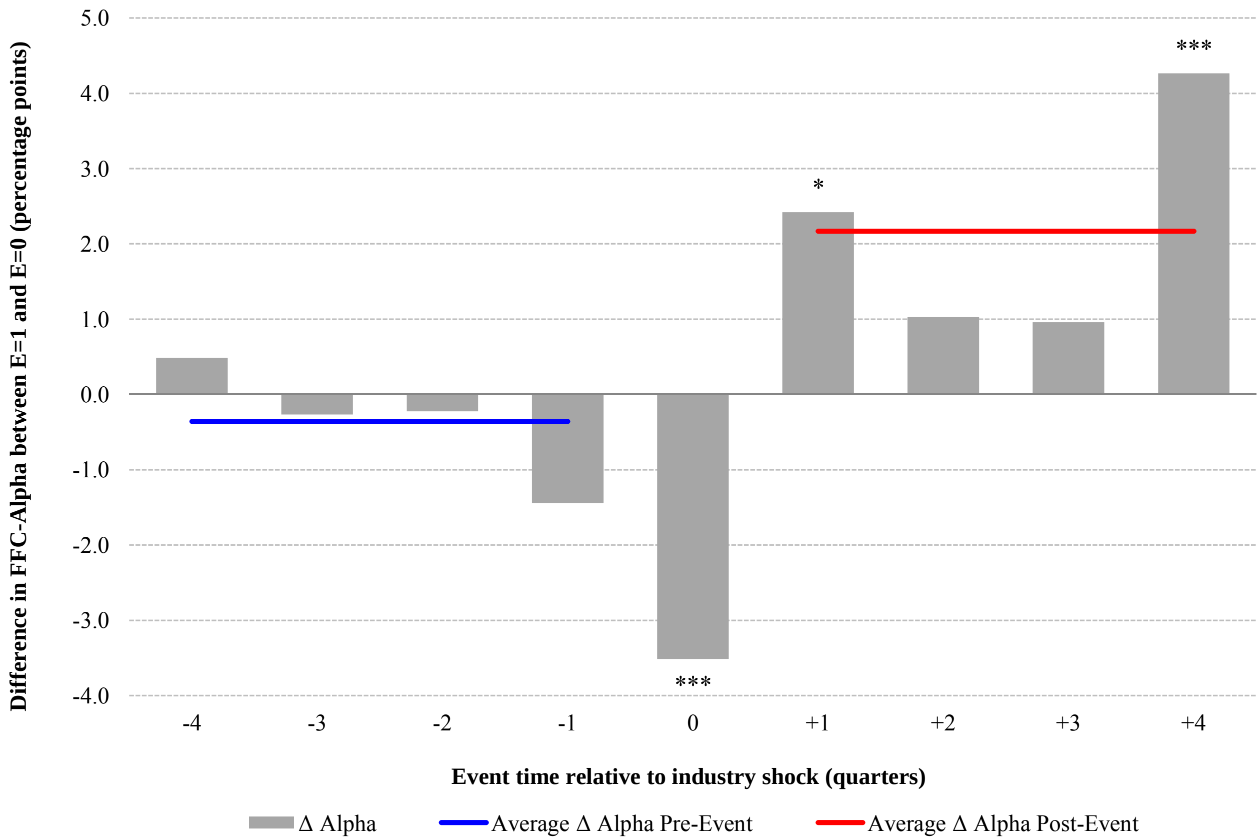

Figure IV: The Impact of Experience on Performance in Event Time. The graph plots the difference in Fama–French–Carhart alphas between experienced and inexperienced ISPs around an experience shock as reported in Table VI. ISPs with an experience-shock event are all ISPs that go through an experience shock in quarter q ($t = 0$ in event time) and do not have any industry shock in the preceding and subsequent 4 quarters. The control group consists of all other ISPs managed by the same manager in the same event quarter q with complete data in the event window $t \in [-4, +4]$ (i) if they have the same industry tenure and (ii) if they do not go through an industry shock in the event window. Asterisks ***, **, * indicate statistical significance of the difference between the high and low groups on the 1%, 5%, and 10% level, and are based on standard errors that allow for clustering around industry $\times$ date.

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

Internet Appendix

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October 24, 2014

Learning By Doing:

The Value Of Experience And The Origins Of Skill For Mutual Fund Managers

This appendix presents additional results to accompany the paper "Learning By Doing: The Value Of Experience And The Origins Of Skill For Mutual Fund Managers". The contents are as follows:

Table IA.1 reestimates the baseline regressions presented in Panel A of Table IV in the main paper while allowing standard errors to cluster at the fund level.

Table IA.2 reestimates the baseline regressions presented in Panel A of Table IV in the main paper using alternative measures of experience.

Table IA.3 reestimates the baseline regressions presented in Panel A of Table IV in the main paper while controlling for the manager's industry tenure. Industry tenure is defined as the number of quarters the manager has been managing stocks in a given industry.

Table IA.4 explains the bootstrap procedure described in section III.C of the main paper, which follows Kosowski, Timmermann, Wermers, and White (2006). **Figure IA.1** presents the estimated experience coefficients after 1,000 bootstrap iterations.

Table IA.5, Panels A and B, explain the simulation of an artificial dataset where industry-specific manager attrition is not random. Panel C and **Figure IA.2** present the estimated experience coefficients after 1,000 simulations.

Table IA.1: Alternative Clustering of the Standard Errors

The table repeats Panel A of Table IV in the main paper while clustering standard errors at the fund level (Panel A) and double-clustering standard errors around industry × date and fund (Panel B).

Panel A: Clustering at the fund level

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.566	1.005	1.376	1.091	1.180	1.369	1.303	0.561	1.304
	(12.99)	(20.11)	(28.25)	(25.13)	(25.54)	(26.86)	(36.52)	(12.92)	(27.10)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel B: Double–clustering around industry × date and fund

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.566	1.005	1.376	1.091	1.180	1.369	1.303	0.561	1.304
	(1.98)	(4.25)	(5.51)	(4.60)	(5.03)	(5.67)	(5.53)	(2.36)	(5.96)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Table IA.2: Alternative Experience Measures

The table repeats Panel A of Table IV in the main paper using alternative measures of experience. Panel A shows results for a modified cross-sectional industry shock measure that restricts industry shock quarters to quarters with negative industry returns. Panel B replaces the restriction $\mathrm{I}[w_{m,\tau-1,i} > 0.1]$ in equation (4) in the main paper by a dummy variable equal to one if industry i is among the top 3 industries held by manager m. Panel C replaces the same restriction by $\mathrm{I}[w_{m,\tau-1,i} > w_{all,\tau-1,i}]$, where $w_{all,\tau-1,i}$ refers to the median weight of industry i in the portfolio of all fund managers invested in that industry.

Panel A: Only Negative Industry Returns

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.662	1.096	1.455	1.160	1.278	1.428	1.381	0.631	1.394
	(2.12)	(4.36)	(5.42)	(4.46)	(5.04)	(5.55)	(5.50)	(2.43)	(5.92)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel B: Top 3 Holdings

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.624	1.082	1.439	1.137	1.223	1.415	1.382	0.582	1.337
	(2.05)	(4.56)	(5.67)	(4.63)	(5.09)	(5.82)	(5.82)	(2.34)	(6.17)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Panel C: Weight above Industry Median

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.441	0.544	0.839	0.694	0.790	0.875	0.756	0.437	0.804
	(2.06)	(2.95)	(4.53)	(3.68)	(4.33)	(4.68)	(4.41)	(2.47)	(5.06)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

Table IA.3: Industry Tenure

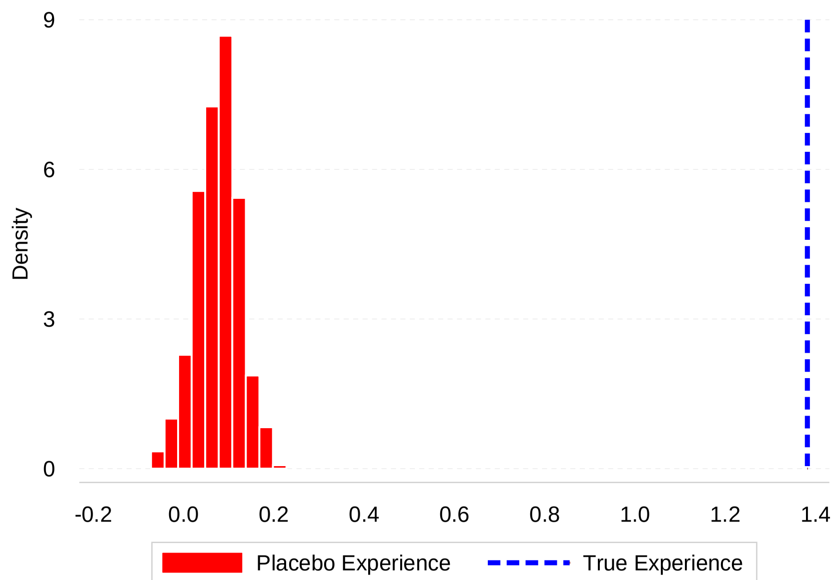
The table repeats Panel A of Table IV in the main paper while controlling for the manager's industry tenure.

	Raw	FF	FFC	CPZ4	CPZ7	LND	CCP	DGTW	DGTW*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience	0.595	1.007	1.374	1.112	1.202	1.367	1.289	0.593	1.314
	(2.11)	(4.50)	(5.73)	(4.76)	(5.28)	(5.93)	(5.73)	(2.52)	(6.21)
Industry Tenure	-0.018	-0.001	0.001	-0.013	-0.014	0.001	0.008	-0.022	-0.008
	(-1.40)	(-0.10)	(0.10)	(-0.99)	(-1.08)	(0.09)	(0.79)	(-1.73)	(-0.82)
Manager × Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	441,282	441,282	441,282	417,364	417,364	441,282	441,282	391,434	391,444
R^2	0.55	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.26

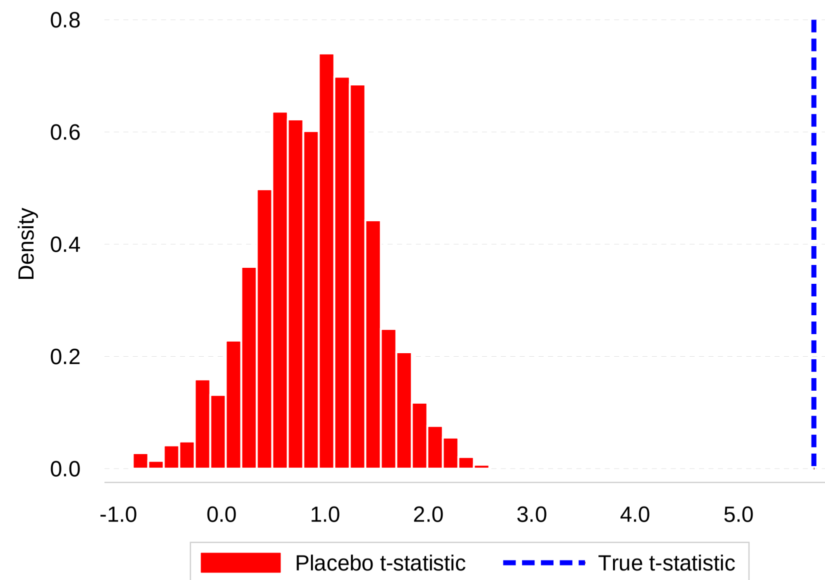
Table IA.4: Bootstrap Procedure

The table explains the steps for the bootstrap procedure described in section III.C of the main paper, which follows Kosowski, Timmermann, Wermers, and White (2006), to generate 1,000 bootstrapped alphas, under the null that all ISPs have zero alpha. The bootstrapped alphas are then regressed on fund manager experience, an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter, and manager $\times$ date fixed effects.

Step	Description
1	We start by estimating equation (6) in the text for each ISP-quarter, saving the resulting FFC alphas, factor loadings, and residuals.
2	For each ISP-quarter, we draw a random sample (indexed by b) with replacement from the realized ISP residuals that were saved in step 1.
3	We compute hypothetical excess ISP returns as: $R^{(b)}_{mqi,t} = \hat{b}_{mqi}RMRF_t + \hat{s}_{mqi}SMB_t + \hat{h}_{mqi}HML_t + \hat{m}_{mqi}UMD_t + \hat{\varepsilon}^{(b)}_{mqi,t}$
4	We regress $R^{(b)}_{mqi}$ on the FFC factors, and estimate a bootstrapped $\hat{\alpha}^{(b)}_{mqi}$. In a given bootstrap replication b, $\hat{\alpha}^{(b)}_{mqi}$ need not equal 0, because the residuals $\hat{\varepsilon}^{(b)}_{mqi,t}$ from step 1 are sampled with replacement.
5	Using the panel of bootstrapped ISP alphas $\hat{\alpha}^{(b)}_{mqi}$, we estimate our baseline regression by regressing the bootstrapped alphas on the fund manager experience indicator, an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter, and manager $\times$ industry fixed effects.
6	We repeat steps 1 to 5 for 1,000 bootstrap replications, storing the resulting estimates of the experience coefficient as well as the corresponding t-statistic.



(a) Distribution of Placebo Coefficients



(b) Distribution of Placebo t–statistics

Figure IA.1: Results from bootstrap procedure. The figure illustrates the output of the bootstrap based on the procedure by Kosowski, Timmermann, Wermers, and White (2006), as described in Table IA.4. Panel IA.1a displays the histogram of the estimated experience coefficients after 1,000 bootstrap replications, and Panel IA.1b shows the histogram of the corresponding t–statistics.

Table IA.5: Simulation of Non-Random Manager Attrition

The table presents the assumptions and results of the simulation of non-random manager attrition. An artificial dataset is simulated that mirrors our real dataset as closely as possible using the assumptions summarized in Panels A and B. The attrition rate varies between 0 and 30 percent and the simulation is repeated 1,000 times for each attrition rate. Simulated ISP-alphas are regressed on fund manager experience, manager $\times$ date fixed effects, as well as an indicator function equal to one if there is a shock in the industry of the ISP in the current quarter. Panel C reports the average magnitude and t-statistic of the experience coefficient for different attrition rates. t-statistics are based on standard errors allow for clustering around industry $\times$ date.

Panel A: Simulation Steps

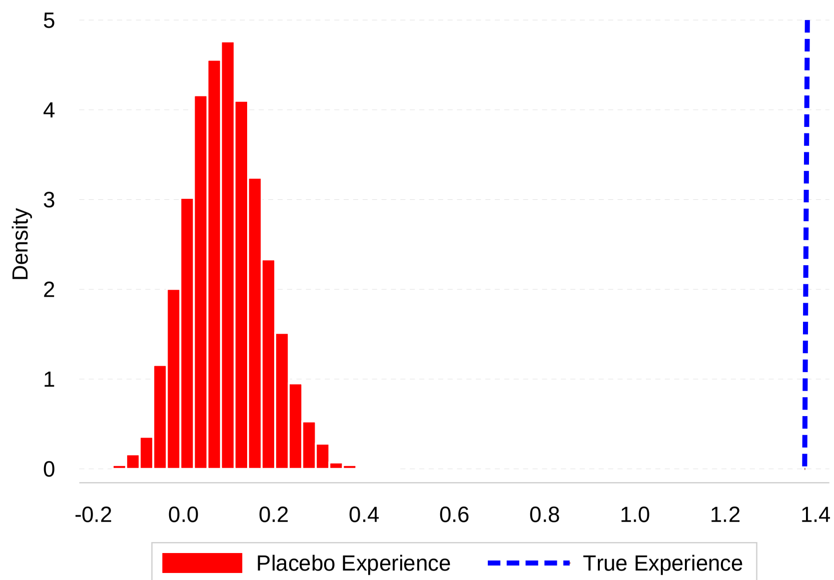
Step	**Description**
1	We start with 500 managers each managing 12 ISPs (6000 observations in total).
2	Each manager has a constant baseline skill α_{mi} in a given industry. α_{mi} is distributed $N(0.41, 10.89)$ (consistent with our data).
3	The quarterly ISP alpha is equal to the manager's baseline skill in the industry and an error term.
4	Every quarter one industry randomly experiences an industry shock, which lowers the alphas of all ISPs in that industry by 3 percentage points (consistent with our data).
5	The worst x% of all ISPs are removed at the end of every quarter, where x is varied between 0 and 30 percent.
6	Managers have a limited overall tenure T_m that follows a Poisson distribution with $\lambda = 20$ (consistent with our data). They stop managing all ISPs at the end of their tenure.
7	At the beginning of each quarter, all the ISPs that have been removed in steps (5) and (6) are replaced by new ISPs with average characteristics.
8	Experience E_{miq} sums up the number of industry shocks manager m has been exposed to in industry i up to time q. If an industry shock occurs, the manager learns from it with a probability of 40%.
9	After 80 quarters the simulation ends.

Panel B: Key Variable Assumptions

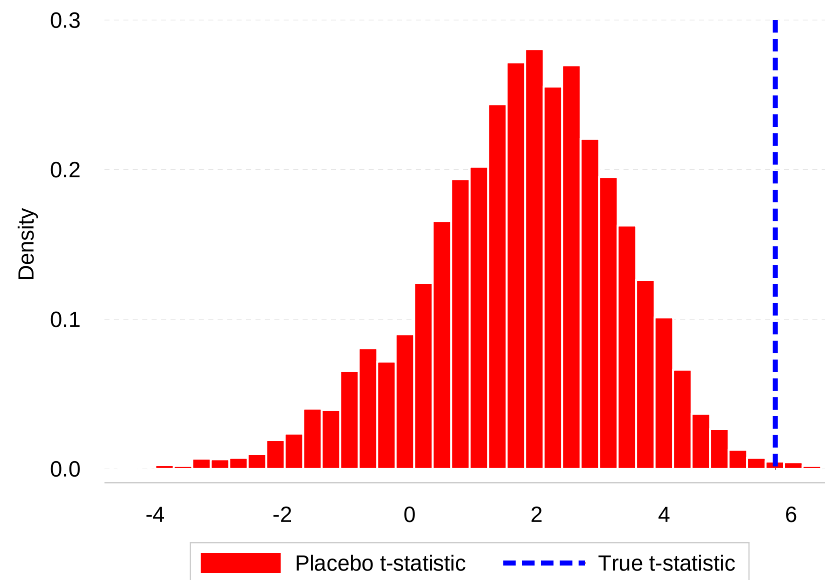
Variable	Definition
Quarterly ISP-alpha	$\alpha_{miq} = \alpha_{mi} + IS_{iq} \times (-3) + \varepsilon_{miq}$
Manager-industry skill	$\alpha_{mi} \sim N(0.41, 10.89)$
Error term	$\varepsilon_{miq} \sim N(0, 9)$
Manager tenure	$T_m \sim Pois(20)$
Experience	$\mathrm{E}_{mqi} = \sum_{\tau<q} \mathrm{IS}_{i\tau} \times \mathrm{I}[u_{m,\tau,i} \leq 0.4]$, where $u_{m,\tau,i} \sim U(0,1)$.

Panel C: Regression Results

	Excluding fixed effects		Including fixed effects	
	Average Coefficient	Average t-statistic	Average Coefficient	Average t-statistic
Attrition (in %)	(1)	(2)	(3)	(4)
0	0.000	0.01	0.001	0.03
1	0.177	5.55	0.036	1.01
5	0.599	16.81	0.077	2.00
10	0.984	24.01	0.106	2.37
15	1.284	27.40	0.126	2.42
20	1.517	28.49	0.141	2.32
30	1.858	27.64	0.172	2.19



(a) Distribution of Simulated Experience Coefficients



(b) Distribution of Simulated Experience t–statistics

Figure IA.2: Results from the simulation of attrition. The figure illustrates the output of the simulation described in IA.5, assuming an attrition rate of 1%. Panel A plots the histogram of the estimated experience coefficients after 1,000 simulation runs, and Panel B shows the histogram of the corresponding t-statistics.