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

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January 6, 2013

Abstract

This paper provides evidence for substantial learning by doing effects among professional investors in a large and highly competitive segment of financial markets. We develop a new methodology to show that experienced mutual fund managers outperform their non-experienced counterparts by up to 67bp per quarter on a risk-adjusted basis. 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. In addition to highlighting a previously underemphasized source of observed mutual fund manager skill, our approach circumvents some of the main obstacles for the empirical literature on learning by doing effects in economics.

JEL Classification: G23, D03, D83, J24

Keywords: Learning by Doing, Mutual Funds

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

1. Introduction

Driving a car, flying an airplane, or writing an academic paper, are examples of activities in which learning by doing is important.[1] Most people are not born natural drivers, pilots, or scholarly writers – instead, they acquire the skill as they drive, fly, or write. Even controlling for general ability, there are likely large differences in performance between someone who, say, drives very little, and someone who drives a lot. As consumers, we routinely 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.

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, in our setting managerial tenure seems like a reasonable proxy for fund manager experience at first glance. However, tenure could also proxy for effort, because more junior managers might need to work harder to signal their type (e.g., Chevalier and Ellison (1999)). Moreover, if bad managers are eliminated by

[1] Learning by doing as a concept has a long history. Early writings emphasized the effects of learning by doing on educational outcomes (e.g., Dewey (1897)) and increases in individual worker productivity (e.g., Book (1908)). Starting with Arrow (1962) the concept has been applied to the study of firms and often refers to unit costs being a decreasing function of prior output (e.g., Bahk and Gort (1993)). The economic literature on learning by doing is too large for us to review here; we refer the reader to available excellent surveys, such as Thompson (2010). It comprises a substantial body of theoretical work in various domains, including industrial organization, trade theory, and endogenous growth theory in macroeconomics. Learning by doing effects have been documented in experimental settings and have been studied extensively in the context of medical decision making (e.g., Waldman, Yourstone, and Smith (2003)).

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 that would themselves influence investment behavior and performance. For example, they might be overly conservative (e.g., Prendergast and Stole (1996)) or subject to greater risk of being fired for underperformance (e.g., Dangl, Wu, and Zechner (2008)). Lastly, higher tenure is correlated with age, which is again correlated with many other variables including cognitive ability (e.g., Korniotis and Kumar (2011)). In sum, then, 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 procedure to identify 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 that 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, then 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, which leaves us less exposed to the sort of omitted variable concerns described above. Fixed effects allow us to eliminate the confounding impact of all variables that do not vary across ISPs for a given manager-date combination. Important confounding factors we can thus exclude are, for example, general ability, educational background, tenure, age, fund characteristics, fund family characteristics, corporate governance at the fund level, and the overall state of the economy.

Our main results are as follows. Unconditionally, ISPs under an experienced fund manager outperform other ISPs by about 120bp per quarter net of risk. By construction, this difference reflects pure *stock-picking* skill and is *before fees*. Regressions using manager×date fixed effects indicate that about half of this effect is explained by variation across fund managers, across funds, and by other factors that do not vary within a given fund manager at a given point in time. A remaining risk-adjusted performance difference of 67bp per quarter can be attributed to one additional unit of experience. This suggests that learning by doing and experience are first-order drivers of fund returns.

In deriving a measure of experience, 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." It is also consistent with Malmendier and Nagel (2011) who show that individuals who are personally experiencing economic shock periods exhibit persistently different patterns in their financial decision making than otherwise similar individuals.

We operationalize this idea by recording industry-wide shocks 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. We single out industry shocks based on the relative ranking of quarterly industry returns: Fund managers learn most from the worst performing industry in a given quarter. As we argue in detail below, this is plausible for at least three reasons: (1) an industry might end up in the bottom return rank because fundamentally new information becomes available, which, in turn, facilitates learning; (2) investors care about losers in their portfolio, so a fund manager who wants to keep her job has an incentive to learn about the reasons for underperformance; (3) the media tend to focus on extremes in their news coverage, which will amplify the previous two effects. The important feature of our experience definition is that it is not a linear function of time, i.e. the same manager might have more experience in the healthcare industry than in the telecom industry, *at the same point in time*.[2]

Our results show that outperformance during industry shocks accounts for about one third of the overall effect of fund manager experience. This accords well with a learning interpretation: if the manager has experienced bad times in an industry, she does better next time around (see also the opening quote). Placebo tests, in which we randomly assign quarters of increased learning, suggest that our results are not spuriously induced by our methodology. While our tests fully control for general ability (like IQ), we show in an extension to our baseline model that industry-specific ability, i.e., unobserved differences in industry-specific skill that already exist when fund managers enter our sample, is not inducing the experience patterns we see in the data. We provide empirical support for our conjecture that experience and learning build up in bad times in particular, by showing that there are no detectable effects when we base our experience measure on industries that have performed best, good, average, or below average (but not worst).

As a final step in our analysis, we develop an experience index (EDX), as an aggregate measure of experience across all industries for a given manager. EDX is a purely backward looking measure, that can be constructed in real time. Funds that score highest on the EDX index obtain positive risk-adjusted returns before fees, and break even after fees. Funds that score low on the index break even before fees, and underperform after fees. A long-short portfolio earns 4-factor risk-adjusted returns of 2.4% per year after fees.

We contribute to both the mutual fund literature and the broader literature on learning by doing in economics. We now discuss these contributions separately.

To the best of our knowledge, our paper is the first to focus exclusively on identifying the

[2]This purely cross-sectional industry shock definition most clearly sets our results apart from the existing literature. We therefore use it as a benchmark. We also test plausible alternative measures based on the time-series of industry-returns below.

value of experience in the mutual fund industry. However, a number of papers in the broader literature on mutual fund manager characteristics 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). 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 technology bubble of the late 1990s. 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 a growing literature that seeks to understand investor learning in financial markets. One strand of the literature analyzes the impact rational learning theories.[3] Another strand of the literature looks at alternative learning theories, such as, for example, naï̈ve reinforcement learning.[4] 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 *sophisticated* (institutional) investors.

Lastly, our study contributes a new econometric approach to identifying fund manager skill (e.g., Berk and Green (2004), Fama and French (2010)). Our results show that experienced managers can outperform passive benchmarks via stock-picking, which adds to a body of work suggesting that at least some funds can systematically outperform.[5] Our study is related to Kacperczyk, Sialm, and Zheng (2005), who show that mutual fund managers who concentrate their holdings in some industries have higher alphas. We show below that our experience effects

[3]E.g., Mahani and Bernahrdt (2007), Pastor and Veronesi (2009), Seru, Shumway, and Stoffman (2010), Linnainmaa (2011), Huang, Wei, and Yan (2011).

[4]E.g., Kaustia and Knüpfer (2008), Barber, Lee, Liu, and Odean (2010), Chiang, Hirshleifer, Qian, and Sherman (2011), Bailey, Kumar, and Ng (2011).

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

obtain even after controlling for industry concentration. As we show that experience from industry shocks is particularly valuable in future industry shocks, our findings can help explain why mutual funds on average tend to do better in recessions (e.g., Moskowitz (2000), Glode (2011)).

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

On a broader level, our work addresses two central problems for the empirical literature on learning by doing: 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?[6] 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, by using mutual fund data, we are using a dataset that is 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 gauge of productivity.

According to Thompson (2010), the "tenor" of the newer empirical literature on learning by

[6] For example, Thompson (2010) argues that "studies using large samples have provided extensive evidence on the effects of plant and firm age on size and growth. But because of the tenuous link between age and productivity, these studies provide at best indirect evidence that passive learning may be taking place". One reason for the lack of direct evidence are the "[...] considerable empirical difficulties caused in large part by the poor quality of data that have typically been available to researchers."

doing in industrial settings – which, importantly, is most often subject to the two central problems identified in the previous paragraph – is that passive learning effects are small, which would, in turn, suggest that "much of the theoretical work on passive learning might be barking up the wrong tree." In contrast, our findings suggest that learning by doing effects can be substantial in production contexts where highly skilled labor is a major input, which is an increasingly large part of the economy. A natural, potentially testable, hypothesis for industrial settings based on our results would be that managerial learning is a main channel to bring about efficiency increases on the firm level.

We describe our method and the dataset in detail in Section 2. Section 3 presents our main results on fund manager experience and fund performance as well as robustness checks. In Section 4 we present three extensions: we investigate if learning occurs in booms and other periods, we show that our results are similar when we focus on the time-series of industry returns to define the industry shock measure, and we develop the experience index EDX to analyze the impact of experience on fund performance. Section 5 concludes.

2. Method and Data

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

2.1 Experience and Learning

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

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

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

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

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

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

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

The existing literature has mainly focused on the first two components. Perhaps most prominently, Chevalier and Ellison (1999) estimate S_0 by using variation in the quality of the undergraduate college of mutual fund managers. Few papers in the literature focus on the tenure component T, overall with with mixed results. For example, Golec (1996) finds a positive relation between tenure and performance, Chevalier and Ellison (1999) find that tenure is unrelated to performance, while Ding and Wermers (2009) find a positive effect for well-governed large funds and a negative effect for small funds. As explained in the introduction, it is hard to draw conclusions on the value of experience from the tenure variable T.

The innovation in our study is the third component, E, in equation (2). It captures that managers will not learn the same in every period. In some periods, more information will be produced about the stock, and the manager therefore receives more signals. It may also be the case that in some periods, managers allocate more attention to the stock relative to other stocks in the portfolio (e.g., Kacperczyk, Nieuwerburgh, and Veldkamp (2012)), perhaps because of career concerns. Using the example from the introduction, while a pilot may learn something from flying in perfect conditions, she might learn much more from successfully navigating her plane through turbulent conditions. Hence, E captures the cumulative impact of exposure to past periods that produced more signals than would be predicted just by the passage of time alone (which is captured by T). Formally, $E = \sum_{\tau<t} e_\tau$, where e_τ is a period t indicator variable equal to 1 if period τ was a high-learning period and 0 otherwise.[7] It is this E component in equation (2) that we refer to as *experience*, with the implicit understanding that it is actually excess experience that is unrelated to the pure passage of time.

Experience not only varies by time, but also by industry. For example, a fund manager who was exposed to bank stocks in the fourth quarter of 2007 (where bank stocks fell by almost 10%) might have a different learning experience than a manager in business equipment in this quarter (return of business equipment stocks in this quarter was 0.1%). The central idea of our approach is to exploit variation of experience across industries i managed by manager m in quarter q. To illustrate, suppose that manager m has 50% of her fund's portfolio invested in industry 1 and the remaining 50% in industry 2. We call these industry-related parts of the portfolio *industry*

[7] One could also focus on the intensity, rather than on the number of periods. We opt to use the number of periods for the exposition, because it matches how we implement the idea empirically.

sub-portfolios (ISPs). Writing the above in terms of a simple reduced-form model of ISP returns yields for each $i \in \{1, 2\}$:

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

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

If the manager manages only two ISPs we can take the differences between the two for manager m at quarter q to get:

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

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

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

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

2.2 Experience Proxy Based on Industry Shocks

To implement the approach from the previous section, we need a measure of experience that is not a linear function of time and that varies across industries for a given manager-quarter combination. In order to obtain such a measure, we first look at the characteristic features of experience. The American Heritage Dictionary of the English Language (2000) defines experience as: "Active participation in events or activities, leading to the accumulation of knowledge or skill," suggesting that a defining feature of experience relates to having to act, in a particular period or event. This fits well with the quote by Arrow (1962) cited in the introduction: "Learning can only take place through the attempt to solve a problem and therefore only takes place during activity." Both definitions highlight the fact that experience is not something that just accumulates with the passage of time. It therefore also provides some justification for interpreting E_{mqi} as experience even though we eliminate the impact of tenure.

When will a mutual fund manager be particularly "active" and "working towards solving a problem"? We conjecture that managers are relatively active (in the sense of actively thinking about their industry portfolio, not in the sense of active fund management) during times of extreme market movements, and that problem-solving becomes particularly relevant in downturns. Building on the idea that learning is likely to occur in bad times, the experience measure that we will use is constructed from the number of times a manager has experienced what we label *industry shocks*. We consider different industry shock definitions. Our baseline definition states that an industry shock occurs in a given industry and quarter, if the value-weighted industry return is the lowest across all 12 Fama-French industries in this quarter. This is in line with fact that rankings and relative performance are of particular importance in the mutual fund industry (e.g., Brown, Harlow, and Starks (1996)).

Several advantages come with this industry shock definition. First, there might be substantial new information released in industry shock quarters that would enable fund managers to learn (e.g., the substantial new information about shadow banking in the recent financial crisis). To the extent that industry shocks are related to industry fundamentals, managers have a chance to learn something from the shock event that is useful next time a similar event occurs. Second, the worst performing industries are probably very salient to investors, and thus industry underperformance is salient for fund managers too. Hence, industry shocks are times in which fund managers with investments are likely to focus actively on ways to minimize the impact of the shock. Third, the media generally focuses disproportionately on extreme events. This might both increase the amount of valuable new information produced and make the industry performance more salient to investors. Finally, having one industry shock per quarter gives us considerable statistical power to detect effects.[8]

Table 1 shows industry shock quarters for each quarter from 1992 to 2008. As can be seen from the table, the number of industry shocks is not the same for all industries. This is a desirable feature of the definition, since it is plausible to assume that learning opportunities are greater in some industries than in others. We will, however, also use alternative definitions in our robustness checks, with a more even distribution of shocks across industries. A second

[8]Note that using market-wide shocks, like, for example, NBER recessions, is not possible in our setting since our approach eliminates all shocks that do not vary across industries.

notable feature from the table is that we assign the label "industry shock" also to quarters with positive returns (e.g., utilities has an industry shock 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, but might be less precise if industry returns need to be below some absolute benchmark to be considered a shock event. We leave these quarters in our sample to be conservative, and to minimize our degrees of freedom, but we show in the robustness checks that our results get stronger when we impose the additional restriction that an industry shock quarter must have a negative industry return.

Experience is based on the number of industry shocks a manager has experienced in the past, but we impose the additional condition that the shock must conceivably have a meaningful effect on the overall fund return. We therefore restrict attention to industries that represent a portfolio weight greater than 10% of assets under management in the fund at the end of the previous quarter.[9] Then, the experience measure for fund manager m in quarter q who manages an ISP in industry i is defined as:

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

where IS stands for an industry shock in a given industry i in quarter τ, $\mathrm{I}[w_{m,\tau-1,i} > 0.1]$ is an indicator function that is one if the weight of industry i in the fund managed by fund manager m at the end of quarter $\tau - 1$ exceeds 10%. Whenever there are two consecutive industry shock quarters for an industry, we update the experience measure at the end of the second industry shock quarter. E_{mqi} varies within a manager-quarter (mq) cell because a fund typically invests in multiple industries and because a fund manager can have different levels of experience in different industries, depending on whether or not she was sufficiently exposed to industry shocks in this particular industry in the past. It is precisely this variation that we are seeking to exploit in our tests below.

While the industry weight is in principle chosen by the manager, making the experience

[9] In the robustness section we show that using the top 3 industries yields essentially identical results.

measure contingent on lagged industry weight is innocuous for two reasons. First, if the industry shock is an unanticipated event, exposure to an industry shock, and therefore our experience measure, is exogenous. Second, if the most skilled or experienced managers could anticipate the shock, they would scale back their exposure. This would bias us against finding that the managers with high values of E_{mqi} outperform. Consistent with both reasons, using the characteristics-timing measure of Daniel, Grinblatt, Titman, and Wermers (1997), we do not find evidence that managers can successfully time industries by moving in and out of characteristics associated with temporarily high returns. Importantly, though, neither reason contradicts the hypothesis that some managers can pick stocks with superior performance *within* industries that are less exposed to industry shocks.

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

The idea that experience from past industry shocks would have a strong impact on future financial decisions resonates well with Malmendier and Nagel (2011) who document that individuals who have experienced periods of particularly low stock market returns show different investment patterns and stock market participation rates than individuals who have not.[10]

[10] Our study is different from theirs because they focus on macroeconomic shocks, while we focus on industry-specific shocks. A second difference is that they focus on differences in risk-taking behavior across individuals conditional on past experience, while we focus on *risk-adjusted* returns across ISPs managed by the same individual at the same point in time.

2.3 Data

Our data comes from a number of standard sources: the CRSP Survivorship–bias Free Mutual Fund Database, the Thomson Reuters Mutual Fund Holdings Database, and the CRSP Monthly Stocks database.

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

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

We merge these data, using the MFLinks database, to the mutual funds' stock 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. We obtain the Fama-French industry classification from Kenneth French's website.

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

2.4 Measuring ISP Performance

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

For every fund at the end of quarter $q-1$, we group all stocks in the fund portfolio in one of the 12 Fama-French industries. We fix the weights for each stock in these industry groups to be equal to the dollar amount the fund has invested in the stock at the end of quarter $q-1$ divided by the total dollar amount invested by the fund in all stocks in this Fama-French industry. Using the stock-level daily returns from CRSP, we calculate the daily return of the portfolio by aggregating the individual stock returns at the industry level. This gives us a series of daily ISP raw returns for all ISPs in our sample. Formally, for every stock j, industry i, and day t in quarter q we define the ISP raw return as:

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

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

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

$$R_{mti} - R_{ft} = \alpha_{mqi} + b_{mqi} RMRF_t + s_{mqi} SMB_t + h_{mqi} HML_t + m_{mqi} UMD_t + \varepsilon_{mti}. \quad (7)$$

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

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

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

3. Baseline Results

3.1 Sample Splits

Table 2, Panel C presents summary statistics for the main variables of interest when we split the sample into observations associated with no experience ($E = 0$) and experience ($E > 0$). Conditional on $E > 0$, the average experience level is 1.56. This group represents about 15% of our total observations. Looking at risk-adjusted performance of ISPs, we find that the average ISP has an alpha before fees of 29 to 58 basis points per quarter depending on the risk-adjustment used (CAPM, 3-factor, and 4-factor adjustments). This is roughly in line with the fund-level estimates reported in Kacperczyk, Sialm, and Zheng (2005). More interestingly, we find a considerable difference in alphas between "experienced" and "inexperienced" ISPs. The 4-factor alpha for experienced ISPs is 119bps higher than the alpha of inexperienced ISPs. This difference is statistically significant, using t-statistics that allow for clustering of observations on the industry-quarter level. These findings are consistent with the null hypothesis that fund managers learn from experience and they provide the basis for our more detailed analysis in the next sections.

We also report the average factor loadings across ISPs from the regressions in equation (7). Experienced ISPs have somewhat higher betas and load significantly less on value, size and momentum, which is consistent with the idea that experienced managers find it easier to deviate from the benchmark because they have better information. The different loadings also explain why we see the largest difference in risk-adjusted returns for the 4-factor model.

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

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

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

3.2 Sorting Results

In this section we further investigate the source of the relative outperformance of managers with past industry experience. To do this we sort alphas into groups by experience and industry shock quarters. Table 3 presents results. We first observe that the ISPs of experienced managers do better both in industry shock quarters and other quarters. The performance of inexperienced ISPs decreases on average by between 4.8% and 8.2% in industry shock quarters, depending on the risk-adjustment. This is in contrast to the experienced ISPs, which fall by much less. The impact of the different factor loadings documented in Table 2 proves to be important in this case as well since the alpha shrinks from 6% to almost zero for experienced manager in industry shock quarters once we control for value, size and momentum.

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

Since we have defined experience based on the number of industry shocks experienced in the past, one interpretation of the results is that managers who have been exposed to industry shocks, and therefore learning opportunities, can use this experience to outperform in the future. Specifically, managers who have seen industry shocks in the past are doing particularly well relative to their non-experienced peers (but not relative to the passive benchmarks) in future industry shocks.

3.3 Regression-Based Evidence

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

To show that these fund-level factors are not driving our results, we estimate several specifications of the following general model which nests equations (3) and (4) above:

$$\alpha_{mqi} = \lambda + \beta_1 E_{mqi} + \beta_2' X_{mqi} + \varepsilon_{mqi}. \tag{8}$$

Here λ stands for fixed effects (further specified below), E_{mqi} is the relevant experience of manager m in industry i in quarter q, and X_{mqi} is a vector of control variables. The main coefficient of interest is β_1 which captures the impact of one additional unit of experience, i.e., one additional industry shock event experienced in the past.

In our baseline specification, we include a full set of manager $\times$ quarter fixed effects, i.e. we define $\lambda \equiv \lambda_{mq}$. β_1 is identified if experience levels vary for the same manager across ISPs. The manager $\times$ quarter dummies ensure that the estimates are not driven by any variable that is fixed for the same manager in this quarter, which, as highlighted above, includes in particular the impact of tenure and baseline skill. We control for the presence of an industry shock in a quarter to make sure we compare ISPs on a fair basis (experienced ISPs, by definition, have experienced more particularly low industry returns). We allow standard errors to be correlated across ISPs managed by the same manager and across ISPs in the same industry in a given quarter, i.e. we allow standard errors in equation (8) to be of the general form:

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

where $\bar{\nu}_m$ and $\bar{\nu}_q$ are manager and quarter fixed effects and ν_{mq} and ν_{qi} are idiosyncratic factors on the manager-quarter and industry-quarter level, respectively. The manager $\times$ quarter dummies parameterically control for $\bar{\nu}_m$, $\bar{\nu}_q$, and ν_{mq} (because neither of these variables varies within manager-quarter cell), and we capture ν_{qi} by clustering at the industry-date level (Petersen (2009)). All results become much stronger when we cluster on the fund level, instead (not reported).

Table 4, Panel A presents presents our main results. We find that a fraction of the relative outperformance we observed in the simple sorting exercise above can be explained by factors that do not vary across ISPs. Hence, in line with the previous literature, tenure, skill, and other fund-level or economy-wide factors may partially explain alphas. The contribution of these regressions is to show that within-manager variation across different ISPs, captured by our experience measure, is also explaining a substantial part of the difference in performance between experienced and non-experienced ISPs. The difference in risk-adjusted performance between non-experienced ISPs and ISPs in which the manager has experienced one industry shock in the past is 66.7bps per quarter for the 4-factor alphas. Comparing this to the unconditional performance difference of 119bps shows that these effects are economically large. The fact that experienced ISPs have much lower loadings on value, size, and momentum shows up also here as results get stronger in magnitude and significance once we risk-adjust performance for these factors. An F-test shows that the null hypothesis of the manager $\times$ quarter dummies being jointly zero can be rejected at any conventional significance level (p-value < 0.001).

In Panel B we estimate the same regression including an interaction term between the experience measure and IS. We can therefore estimate the differential effect of shock experience inside and outside of industry shock quarters. As in the univariate sorts, we find that, in industry shocks, managers with past industry shock experience in an ISP do much better than inexperienced ones. Conditional on being in an industry shock quarter, based on the 4-factor model, this difference is as large as 169bps for an additional industry shock experience.

In a final version of our main regression model, we include a full set of industry $\times$ quarter dummies along with the manager $\times$ quarter fixed effects. We therefore define $\lambda \equiv \lambda_{mq} + \lambda_{qi}$ in equation (8). The industry $\times$ quarter dummies are flexible enough to accommodate different shocks for the same industry in different quarters (e.g., a different impact of an industry shock to bank stocks in 2000Q1 and 2007Q1), and different shocks for different industries in the same quarter (e.g., bank stocks in 2007Q1, versus Oil stocks in 2007Q1). We include these additional fixed effects to rule out that our results are driven by (potentially time-varying) omitted variables on the industry level. In particular, we want to minimize concerns that results are due to a fundamental industry-specific risk-factor that we do not capture by adjusting returns only for the four Carhart factors. One example would be long-term reversal effects in stock returns (e.g., DeBondt and Thaler (1985)) or industry momentum (e.g., Moskowitz and Grinblatt (1999)) that might not be captured in the momentum factor. The industry $\times$ quarter dummies can help minimize such concerns because they eliminate the impact of all variables that vary across industries as long as they do not vary across ISPs in the same industry.

Before we present results, we note that, while the industry $\times$ quarter effects help us to rule out these alternative explanations, it is not clear, conceptually, that we want to include them since they might eliminate a lot of the variation of interest. Opportunities to learn and the value of experience are likely to vary by industry (e.g., learning opportunities in business equipment or healthcare are larger than in utilities), and time (e.g., managers learn more, and the value of prior experience is greater, in business equipment in 2001 than in 1992). Hence, we would expect alphas *on average* to be higher in some quarters and industries *even if learning were the only driver of alphas*, as some managers can profitably use their experience exactly then. By including industry $\times$ quarter effects we are comparing the informative difference in alphas in industry-quarters where experience is valuable, with differences in alphas from periods where opportunities to learn are low, the value of experience is low, and the realized alphas are pure noise. If this noise component is large enough, we might not detect experience effects (the coefficient would be biased towards zero) even though there might be large effects in the true underlying model.

Panel C shows that, despite this caveat, our results are qualitatively unchanged and statistical significance stays high while the point estimate on experience decreases to 0.109.

Overall, the results in this section confirm our findings from the univariate sorts. Experienced managers outperform inexperienced managers, and relative outperformance is particularly pronounced in industry shock quarters. The results are not driven by omitted variables on the industry level.

3.4 Baseline Robustness

This section presents baseline robustness checks on our main result (we have mentioned several of them already above without showing the actual results). We will present additional, more specific, sets of robustness tests in the next two sections. Table 5 shows several alternative versions of our specification (3) in Table 4, where we regress the 4-factor alpha on a crisis dummy and a full set of manager $\times$ quarter dummies. For brevity, we only report the coefficient and significance of the experience coefficient.

We first show in specification (1) of Table 5 that results are effectively unchanged when we replace the requirement in our experience measure (equation (5)) that the industry has a weight above 10% of assets under management, by the alternative requirement that the industry must be in the fund's top three by assets under management. Next, while our baseline experience measure counts any industry quarter where the industry has the lowest value-weighted return across all industries as an industry-shock quarter, we now add the requirement that only quarters with negative value-weighted return are counted as industry shocks. Specification (2) shows that, if anything, results become stronger.

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

One potential problem with our experience measure is that, by construction, all managers start with zero experience in the first quarter of the sample. This is purely data-induced because we only observe managers starting in 1992. To alleviate concerns that this artificial cutoff is spuriously affecting our results, we drop the first five years of our sample from the estimation, but use them to construct experience measures. Since the average tenure of managers in our sample is significantly shorter than five years, the resulting experience measure should be more accurate. Specification (4) shows that the results are effectively unchanged when we start the estimation in year 1997 (and therefore drop a sizeable fraction of our data). This suggests that the data-induced cutoff is not biasing our results.

As an alternative to controlling for time-varying industry-factors, we present results from 5-factor alphas in specification (5). The five factor alphas are constructed by regressing daily raw ISP returns on the four Fama-French-Carhart factors as well as on the daily value-weighted industry-return in the ISP's industry. Because the industry return is parametrically controlling for time-variation in industry-returns, we report heteroskedasticity-robust standard errors. Specification (5) shows that the results are similar to the results in Table 4, Panel C, where we have controlled for industry $\times$ quarter effects.

Specification (6) includes 12 lags of industry returns to make sure that industry momentum is not influencing our results. While we lose more than 75% of our sample, the results show that industry momentum is not explaining the superior returns associated with fund manager experience.

Specification (7) considers a finer partition of the stock market into the 48 Fama–French industries, as opposed to the 12 industries used thus far. If experienced managers have superior industry information, focusing on more narrowly defined industries can estimate the effect of experience more precisely. The results are consistent with our baseline specification.

A a final robustness check, we replace the factor-based risk adjustment with the characteristics based adjustment method proposed by Daniel, Grinblatt, Titman, and Wermers (1997). Panel B shows results for our baseline regression for their CS, CT, and AS variables.[11] The panel shows that we get the same message from looking at the characteristics-adjusted performance measures as we do for the factor-adjusted measures: experience is associated with a significantly higher stock picking ability (CS). We do not find evidence of superior characteristics timing ability of managers (CT) and we do not find that experience is related to persistent styles that are associated with higher returns (AS). Overall, these findings support our baseline results and

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

show that we are capturing stock picking skill.

3.5 Placebo Tests

In this section, we run placebo tests to make sure our findings are neither spuriously induced by how we construct the experience measure, nor by how we run our tests. We generate 10,000 sets of placebo industry shocks, where we randomly choose one industry every quarter and assign it an industry shock. Hence, for each ISP and trial we obtain a new experience measure. We then rerun our baseline regression using this placebo experience measure. We use the 4-factor alpha as the dependent variable. Because the experience variables are somewhat persistent, and because managers with longer industry tenure mechanically have higher experience measures, the placebo experience variable and the "true" experience variable we used before will often be positively correlated. To make sure we are not capturing this correlation, we also include the true experience measure in the regression as well as the true industry shock variable. The aim of this placebo test is 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 1 summarizes the results. As can be seen from the figure, the placebo coefficients are centered close to zero and are often negative. By contrast, the coefficient on the true experience variable is centered close to the baseline estimate of 0.667. The coefficient on the true measure exceeds the coefficient on the placebo 98.75% of the time, and even then the true coefficient is always positive (its minimum over the 10,000 runs is 0.21). The distribution of the true estimates is much tighter than the distribution of the placebo estimates.

These results are reassuring. They show that it is very unlikely that our experience measure is large and significant by chance. Even the largest coefficient we see on the placebo measure across all 10,000 runs is smaller than our baseline estimate on 0.667. They also show that there is nothing in the construction of the variable, or the econometric approach, that would spuriously induce the effect, because then we should also see it for the placebo variable. The explanation most consistent with these results is that the experience measure is picking up variation that is truly informative for predicting ISP performance.

3.6 Industry-Specific Tenure and Baseline Skill

We have documented above that most of the related literature focuses on explanatory variables for fund returns that vary in the fund and/or time dimension. Because our approach is eliminating the impact of all variables that do not vary within manager at the same point in time, which includes fund level and time-specific variables, we argue that we are introducing a new effect to the literature. We attribute this effect to experience and learning by doing and have shown above, in particular, that the tenure and baseline skill variables that have been used in the prior literature cannot cause the patterns we find in the data. However, while *overall* manager specific tenure and ability seem to be ruled out by our approach, *industry-specific* tenure and baseline skill could present alternative mechanisms to explain our new results. To see this, rewrite our econometric model for ISP alphas in equation (8) using industry-specific tenure and skill, denoted by T_{mqi}, and $S_{0,mi}$, respectively:

$$\alpha_{mqi} = \lambda + \beta_1 E_{mqi} + \beta_2' X_{mqi} + \beta_3 T_{mqi} + \beta_4 S_{0,mi} + \varepsilon_{mqi}. \tag{10}$$

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

We start by looking at industry tenure. Industry tenure could play a role because managers who are around longer might simply get more experienced as time passes, or they might simply have more chances to be hit by an industry shock. Because it is observable, it is straightforward to control for industry tenure in the above setting. Specifically, we use lagged industry tenure, defined as the number of previous quarters a manager has managed an ISP in industry i in our sample as an additional control in our baseline regressions. Panel A in Table 6 shows results. The industry tenure variable is insignificant across all specifications, while the experience variable is effectively unchanged. This could be due to the fact that experience indeed builds up mostly in bad times and is therefore not a linear function of time. In that case, industry-specific tenure and our experience measure would not be highly correlated. It could also reflect the fact that most funds have ISPs in most industries in most quarters, which would leave very little variation in industry tenure on the manager-quarter-level to exploit (Table 2). Both explanations suggest that a useful feature of the experience measure is that it is not a linear function of time.

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

If we assume that there is variation in industry-specific baseline skill, the second condition that needs to be satisfied is that there is a positive correlation between industry skill and our experience measure. While this is possible, it is also possible to think about scenarios in which there would be a negative correlation. For example, if some managers had so much industry baseline skill that they could predict crises, this would lead to a downward bias in our experience coefficient. Likewise, if the most industry-skilled managers left to set up their own hedge fund after proving their superior abilities during a crisis event, we would observe a negative correlation between experience and industry skill among the remaining managers.[12] Hence, alternative explanations for our results based on industry-specific baseline skill require a considerable number of additional assumptions.

We present three formal tests to minimize concerns that the experience effects we document are really industry-specific baseline skill effects spuriously picked up by our experience measure. In these tests we exploit observable variables that should be highly correlated with industry-specific baseline skill. The first variable we focus on is industry share, i.e., the fraction of the fund assets allocated to industry i. If manager are inherently better at managing stocks in industry i they should *on average* overweight this industry in their portfolios (see also Kacperczyk, Sialm, and Zheng (2005) for a similar idea). We therefore include the average industry share over quarters -5 to -1 as additional control variable in our baseline regression. Panel B shows that industry share indeed has a positive impact on fund performance once we adjust returns for value and size factors. The economic magnitude is sizeable with a quarterly change of 26bps ($= 0.11 \times 2.422$) in fund returns for a one standard deviation change in the average industry share. These estimates might indicate a role for industry-specific baseline skill. They might also reflect the skill-enhancing effect of past experience. Importantly, and irrespective of industry share, the experience variable is always significant. Based on the 4-factor model, the impact of one additional unit of experience is 51bps. To the extent that industry share captures industry-specific baseline skill, we conclude that our experience results are not driven by industry-specific

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

baseline skill. One interpretation of these findings is that conditional on average lagged industry share (which might actually be high because of learning from prior experience) an additional unit of experience is valuable.

The second variable we consider as industry skill proxy is a measure of how much the industry share for a given ISP deviates from the average industry share across all ISPs in this quarter. We have called this variable ICI Component in Table 2. The sum of the ICI Components across all ISPs for a given fund-quarter produces the industry concentration index ICI developed by Kacperczyk, Sialm, and Zheng (2005). These authors show that a higher industry concentration, i.e., a higher value of ICI, is related to superior fund performance. Closely related is the Active Share variable by Cremers and Petajisto (2009), which also indicates that funds that actively deviate from their benchmark have stock-picking skill that allows them to outperform passive benchmarks. Because both ICI and Active Share are defined at the fund level, we cannot include the original measures in our tests. Hence, we include the ICI Component measure (more precisely: its average over the quarters -5 to -1), which is the squared difference between the industry share of an ISP and the average industry share across all ISPs in this industry and quarter.

Panel C shows that the ICI Component variable is positively related to fund returns, consistent with the findings of Kacperczyk, Sialm, and Zheng (2005) and Cremers and Petajisto (2009). However, as was the case with industry share, an additional unit of experience is valuable even conditional on lagged values of deviations from industry means. In fact, the size and significance of the experience measure is hardly different from the baseline case. This suggests again that we are capturing some meaningful variation in the data that is not captured by other variables proposed in the prior literature.

As a final test, we use past alphas to construct a direct estimate of industry-specific baseline skill. To do this, we analyze risk-adjusted ISP returns of managers in our sample before they are exposed to the first industry shock. By definition, industry-specific baseline skill, if it exists, should be reflected in these *pre-experience* ISP alphas. We therefore use for each ISP in quarter q the average of all pre-experience ISP alphas for that ISP up to and including quarter $q-1$ as a direct proxy for the unobserved effects in our main regressions. Panel D of Table 6 presents results. Most importantly, we find again that our experience effects are largely unchanged and, if anything, stronger. This is unlikely due to pre-experience ISP alphas being a noisy measure of industry skill, because, just as would be expected under the null of industry skill effects, the coefficient on industry alphas is positive and significant in all specifications.

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

4. Extensions

4.1 Learning from Booms and other Periods

The above findings support the idea that experience gained in industry shock quarters facilitates learning and leads to higher returns in the future. If fund managers learn from bad times, it is natural to ask if they also learn from booms. This may be plausible since some of the factors that motivated learning in negative industry shock periods apply also to booms: industry booms are salient events that attract investor and media attention and, because of tournament incentives, managers might disproportionally care about booms for career and bonus reasons. On the other hand, booms may be the result of bubbles, and investor exuberance and media hype may make it harder for fund managers 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 boom periods.

To investigate the issue, we modify our prior approach to allow experience to come from booms and other periods as well. Specifically, we continue to compute an experience measure as in equation (5), but define the IS as a dummy equal to 1 if the industry return rank in this quarter is, for example, the highest across all 12 Fama-French industries. We do this for all

12 possible industry return ranks and denote IS measures based on rank n as IS_n. Ranking industries from 1 (lowest return) to 12 (highest return) then yields 12 different measures IS_n and E_n, where E_n is computed as in equation (5) using IS_n. This convention implies that IS_1 and E_1 are the industry shock and experience measures we used in previous sections, and IS_{12} and E_{12} are the corresponding measures for boom periods.

Table 7 presents results when we rerun our baseline regressions in Table 4, Panel A, including IS_n and E_n. We show results from 12 different regressions, one in each line. We include the baseline parameters IS_1 and E_1 in all regressions as additional controls because experience measures are correlated. For instance, an industry that often has the lowest return, also has a high probability of having the second lowest return in a quarter. Therefore, effectively, we test if an experience measure based on any other industry rank has incremental explanatory power for fund returns over and above an experience measure that is based on the worst performing industry. The regression specifications are otherwise identical to the baseline setting. Importantly, they continue to include a full set of manager $\times$ quarter dummies.

The first line in Table 7 replicates the baseline results from Table 4, Panel A. The second line shows that the experience measure based on industry shocks E_1 is effectively unchanged while the experience measure E_2, constructed based on industry rank 2 (the second lowest rank), is much closer to zero and insignificant. Overall, a striking feature of the table is that the coefficient on the shock experience E_1 is always highly significant and always markedly higher than the coefficients on alternative experience measures which are often close to zero and always insignificant.

The analysis reveals interesting patterns for booms. Both point estimates and statistical significances increase for the highest industry ranks 11 and 12. At the same time the industry shock experience measure gets slightly attenuated both in size and statistical significance (although both remain high). This might be consistent with additional learning effects in boom periods that get picked up by E_1 in the baseline setting. The lower magnitude and significance of E_{12} compared to E_1 might be due to limits on learning because of exuberance and reinforcement learning effects as hypothesized above.

Overall the analysis shows that while experience in industry shock periods always has a strong impact on fund returns, we find at best weak evidence for learning effects in boom periods, and no evidence for learning effects in other periods.

4.2 Learning from the Time-Series of Industry Returns

Our baseline results have focused on the case in which fund manager experience is based on industry shocks that are defined cross-sectionally: whenever an industry is the worst performing one in a quarter, managerial learning opportunities are greatest, and we record an industry shock for this industry and this quarter. While we argue this is plausible given the focus on relative performance in the mutual fund industry, it is also plausible to think of industry shocks in terms of the time-series. For example, investors and the media frequently compare returns this period to returns in the past. In this section we investigate if we can find experience effects also when experience is gained from industry shocks defined from the time-series of industry returns.

We start by computing 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 (the value-weighted quarterly return across all stocks in this industry and quarter) is below the 10th percentile of industry returns in this industry over the last 40 quarters. We then compute a time-series based experience measure E^{TS} exactly as in equation (5) using IS^{TS} instead of IS.

Table 8 shows that we obtain results that are qualitatively similar to the baseline case when we use the time-series based experience measure instead of the cross-sectionally based experience measure. Panel A splits the sample into experienced and non-experienced ISPs. Our definition of IS^{TS} yields group sizes that are quite comparable to the benchmark case. On average, an experienced ISP manager has seen 1.56 industry shocks. ISP performance, measured as CAPM, 3-factor, and 4-factor risk-adjusted returns, also lines up as predicted under the hypothesis that managers learn from past industry shocks. The differences are not as pronounced as in the cross-sectional industry shock setting, however. For example the difference in 4-factor alphas is 73bps in the time-series case, while it was 119bps in the cross-sectional case. In absolute terms, these numbers are still sizeable.

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

Finally, Panel C replicates our regressions from Table 4. We again find the the results are qualitatively similar, but somewhat weaker for the time-series case. Specification (1) 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, one additional unit of experience increases ISP performance (4-factor alpha) by 37.6bps. Experience from past industry shocks is every valuable in future industry shocks (specification (2)) and controlling for industry $\times$ quarter effects eliminates a lot of variation, but still leaves a statistically significant outperformance of 6bps for experienced managers.

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

One particularly interesting implication of the time-series findings is that it adds a new dimension to the literature cited in the introduction that finds that mutual funds tend to do better in recessions and downturns. While existing explanations have focused on the higher marginal utility of wealth for investors in downturns (e.g., Glode (2011)), or the idea that obtaining informative signals becomes more valuable in downturns (e.g., Kacperczyk, Nieuwerburgh, and Veldkamp (2011)), our theory implies that mutual funds outperform in downturns because some fund managers learn from past downturns. The correlation between IS^{TS} and the market factor is in line with this idea (corr = –0.53).[13]

4.3 Experience at the Fund Level: EDX and Performance

In this section we investigate if the documented superior stock-picking ability of experienced managers on the ISP level shows up also at the fund level. Specifically, we ask if it would be profitable for investors to invest with experienced managers. We implement this in the simplest way by looking at a weighted average of the individual industry experience measures (equation (5)), where the weights correspond 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:

$$EDX_{mq} = \sum_{i} w_{mi,q-1} E_{mqi}. \tag{11}$$

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

To see if EDX is associated with higher returns, we sort funds into three EDX terciles every month. Since a large number of funds have an EDX value of zero, the low EDX portfolio contains a slightly higher number of funds. 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 9, Panel A, shows that high EDX funds outperform before fees and break even after fees, while low EDX funds break even before fees and underperform after fees. Both before and after fees, a portfolio that is long the high EDX funds and short the low EDX funds would earn a profit of 20bps per month, or 2.4% per year. In terms of factor loadings, both high and low EDX portfolios have a market beta close to one and very similar loadings on value and size. A main

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

difference is in the momentum factor. Low EDX funds seem to follow momentum strategies, while high EDX funds do not.

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

The results from Table 9 are striking because they suggest that a backward-looking measure, EDX, can be used to obtain superior risk-adjusted returns after fees if investors can short. Fund investors should care about fund manager experience even if they cannot short, because high EDX funds allow them to at least break even after fees, while low EDX funds lose money on a risk-adjusted basis.

5. Conclusion

We present a new approach to investigate 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, to the extent of 67bps per quarter on a risk-adjusted basis. Our approach ensures that this difference cannot be explained by factors that do not vary across ISPs for a given manager and quarter. The effects therefore cannot be explained by previously studied variables like age, tenure, education, IQ, corporate governance, fund characteristics, and the business cycle. These results aggregate to the fund level. Measuring experience by a new EDX index that captures individual fund manager experience, we find that a long-short portfolio on EDX generates risk-adjusted returns of 2.4% per year in our sample.

Underlying our approach is the idea that experience and learning are not just linear functions of time. Specifically, we conjecture that learning opportunities are greatest after periods of abnormally low industry returns, which is consistent with earlier investigations into learning by doing (e.g., Arrow (1962)), as well as the literature showing that experiencing bad market conditions in the past influences financial decisions in the future (e.g., Malmendier and Nagel (2011)). An important implication of our study for empirical researchers is that tenure might not be a powerful proxy for experience.

Overall, our results suggest that learning by doing is important for sophisticated investors, and that experience is a valuable fund manager characteristic that investors should care about. Given the strength and magnitude of our results, studying the impact of experience on financial decision making in other contexts is a potentially important topic for future research. More broadly, the market for mutual funds is among the most liquid and competitive financial markets, where feedback is noisy and the forces of arbitrage are strong. If experience matters there, it may be even more valuable in most other economic settings.

Finally, while our findings provide strong evidence that experienced fund managers outperform by picking stocks, some more specific questions about their investment strategies remain for future research. In particular, do experienced managers remain largely passive after making the right stock picks? Or does their experience enable them to better react to corporate news (e.g., earnings announcements, investment, and financing decisions)? These questions are beyond the scope of this paper, but we hope to address some of them in future work.

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Table 1: Worst Performing Industries By Quarter

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

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

Table 2: Summary statistics

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

Panel A: Sample

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

Panel B: Experience and Industry Shock Variables

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

Panel C: Summary Statistics by Experience

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

Table 3: Sorting results

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

Panel A: CAPM Alpha

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

Panel B: 3-Factor Alpha

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

Panel C: 4-Factor Alpha

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

Table 4: Fund manager experience and performance

The table reports results from regressing ISP alphas on manager-quarter fixed effects and controls. Panel A, controls for the presence of industry shocks (IS). Panel B includes and interaction term between experience and industry shocks. Panel C includes industry-quarter fixed effects. Results are shown for CAPM, 3-factor, and 4-factor risk-adjustment, respectively. T-statistics are robust to clustering by manager and industry as defined in equation (9).

Panel A: Baseline Regressions

	CAPM	3-Factor	4-Factor
Experience	0.297	0.543	0.667
	(1.77)	(3.63)	(4.12)
IS	-7.943	-5.131	-4.182
	(-13.81)	(-5.76)	(-5.21)
Manager × Quarter FE	Yes	Yes	Yes
R^2	0.25	0.17	0.16
N	336,193	336,193	336,193

Panel B: Regressions with Experience-Industry Shock Interaction

	CAPM	3-Factor	4-Factor
Experience	0.245	0.374	0.478
	(1.35)	(2.47)	(3.07)
IS	-8.086	-5.596	-4.705
	(-13.71)	(-6.43)	(-6.12)
IS × Experience	0.462	1.506	1.693
	(1.40)	(3.65)	(3.13)
Manager × Quarter FE	Yes	Yes	Yes
R^2	0.25	0.17	0.16
N	336,193	336,193	336,193

Panel C: Regressions with Industry-Quarter Fixed Effects

	3-Factor	4-Factor	4-Factor
Experience	0.088	0.109	0.080
	(3.06)	(3.80)	(2.65)
IS × Experience			0.243
			(2.90)
Industry × Quarter FE	Yes	Yes	Yes
Manager × Quarter FE	Yes	Yes	Yes
R^2	0.28	0.24	0.24
N	336,193	336,193	336,193

Table 5: Robustness Checks

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

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

	Experience	t-statistic	N
Baseline			
Experience	0.667	(4.12)	336,193
Alternative specifications			
(1) Top 3 Industry	0.706	(4.04)	336,193
(2) Only Negative IS	0.761	(4.21)	336,193
(3) Floating Weight	0.621	(3.74)	336,138
(4) Excl. First 5 Years	0.643	(3.87)	262,595
(5) 5-Factor Alpha	0.096	(4.82)	336,192
(6) 12 Lags of Past Industry Returns	0.432	(3.90)	74,674
(7) Fama–French 48 industries	0.875	(2.78)	754,473

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

	CS	CT	AS
Experience	0.317	0.014	0.025
	(2.13)	(0.22)	(0.46)
N	301,582	243,674	243,674

Table 6: Industry-Specific Tenure and Baseline Skill

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

Panel A: Control for Lagged Industry Tenure			
	CAPM	3-Factor	4-Factor
Experience	0.319	0.544	0.671
	(1.89)	(3.58)	(4.05)
Industry Tenure	-0.017	0.008	0.009
	(-0.89)	(0.40)	(0.50)
N	290,635	290,635	290,635
Panel B: Control for Lagged Industry Share			
	CAPM	3-Factor	4-Factor
Experience	0.314	0.406	0.503
	(2.00)	(2.89)	(3.39)
Average Industry Share	0.115	2.430	2.422
	(0.15)	(2.65)	(2.98)
N	171,920	171,920	171,920
Panel C: Control for Lagged Deviation in Industry Share from Industry Mean			
	CAPM	3-Factor	4-Factor
Experience	0.307	0.512	0.608
	(1.90)	(3.57)	(3.99)
Average ICI Component	0.017	0.022	0.023
	(2.40)	(2.89)	(3.01)
N	171,920	171,920	171,920
Panel D: Control for Average Pre-Experience ISP Alpha			
	CAPM	3-Factor	4-Factor
Experience	0.350	0.535	0.674
	(2.09)	(3.56)	(4.15)
Pre-Experience ISP Alpha	0.037	0.087	0.061
	(2.15)	(4.37)	(3.74)
N	290,635	290,635	290,635

Table 7: Learning from Booms and Other Periods

This table reports results when learning can come from other periods. It shows coefficient estimates when the 4-factor alpha is regressed on E_1, E_n, IS_1, IS_n and a full set of manager × quarter dummies. Every line represents results from one single regression. E_1 and IS_1 are the experience and industry shock variables 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 in equation (5). T-statistics are robust to clustering by manager and industry as defined in equation (9).

Rank n	E_n	t-stat	E_1	t-stat	IS_n	t-stat	IS_1	t-stat
1 (Low)			0.667	(4.12)			-4.182	(-5.21)
2	-0.104	(-0.67)	0.743	(3.92)	-2.973	(-5.05)	-4.441	(-5.53)
3	0.058	(0.40)	0.667	(4.00)	-2.988	(-7.28)	-4.454	(-5.52)
4	0.063	(0.51)	0.636	(3.84)	-1.654	(-3.50)	-4.332	(-5.39)
5	0.016	(0.12)	0.651	(3.92)	-1.036	(-2.13)	-4.274	(-5.31)
6	0.182	(1.47)	0.612	(3.57)	-0.433	(-0.76)	-4.212	(-5.24)
7	0.010	(0.07)	0.672	(3.93)	0.746	(1.57)	-4.111	(-5.11)
8	-0.090	(-0.96)	0.678	(4.08)	-0.174	(-0.33)	-4.204	(-5.23)
9	-0.048	(-0.35)	0.681	(3.78)	1.038	(2.37)	-4.087	(-5.07)
10	-0.113	(-0.90)	0.692	(4.03)	1.300	(1.94)	-4.072	(-5.07)
11	0.270	(1.70)	0.473	(2.79)	2.716	(5.58)	-3.955	(-4.93)
12 (High)	0.228	(1.46)	0.400	(2.42)	3.538	(6.16)	-3.897	(-4.90)

Table 8: Experience from the Time-Series of Industry Returns

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

Panel A: Summary Statistics by Experience

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

Panel B: Sorting using 4-Factor Alpha

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

Panel C: Regressions using 4-Factor Alpha

	(1)	(2)	(3)
Experience^{TS}	0.376	0.174	0.060
	(3.23)	(1.54)	(2.50)
IS^{TS}	-4.794	-5.386	
	(-5.50)	(-6.29)	
IS^{TS} × Experience^{TS}		1.830	
		(4.58)	
Manager × Quarter FE	Yes	Yes	Yes
Industry × Quarter FE	No	No	Yes
R^2	0.16	0.16	0.24
N	336,193	336,193	336,193

Table 9: Experience on the Fund Level: EDX and Performance

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

Panel A: Sort on Experience

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

Panel B: Sort on Tenure

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

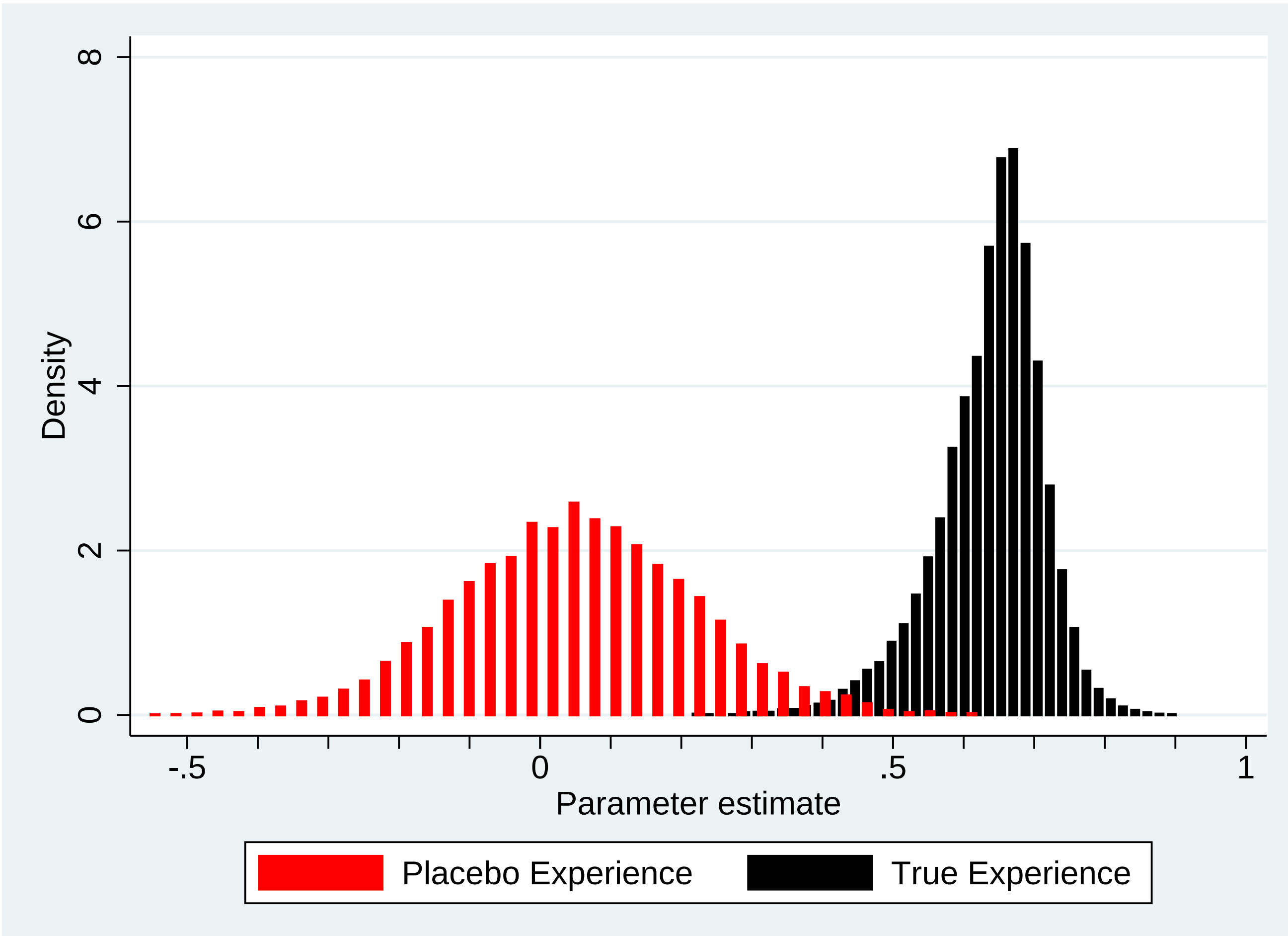

Figure 1: Results from 10,000 placebo runs. The figure shows parameter estimates for running our baseline regression in Table 4, Panel A, 10,000 times. For each run, we randomly generate a sequence of industry shocks by randomly selecting one Fama-French 12 industry every quarter as an industry shock quarter. We then recompute our experience measure based on this placebo industry shock series. We then regress the 4-factor alpha of an industry sub-portfolio (ISP) on the placebo experience measure, the "true" experience measure, a dummy for a placebo industry shock, a dummy for the true industry shock, as well as a full set of manager $\times$ quarter fixed effects. The figure shows the parameter estimates on the placebo and true experience measure, respectively.