How do firms appropriate value from employees with transferable skills?
A study of the appropriation puzzle in actively managed mutual funds

Victoria Sevcenko
London Business School
Sussex Place, Regents Park
London NW1 4SA, UK
vsevcenko@london.edu

and

Sendil Ethiraj
London Business School
Sussex Place, Regents Park
London NW1 4SA, UK
sethiraj@london.edu

How do firms appropriate value from employees with transferable skills?
A study of the appropriation puzzle in actively managed mutual funds

Abstract

How do firms benefit from employees with transferable skills? The prevailing view is that labor market frictions that impede employee mobility or strategies that constrain skill transferability are the primary instruments for firms to appropriate value from human capital. The empirical evidence, however, suggests that employees continue to be mobile, and firms pay premiums to attract and retain employees with transferable skills. To reconcile theory with data, we use data from the mutual-fund industry, where it is widely documented that active fund managers appropriate more value than they generate. We develop a theory of positive externalities stemming from transferable human capital that we argue accrue mostly to the firm, and provide evidence of such externalities in the mutual fund context. Empirically, we decompose the skills of mutual-fund managers into task- and firm-specific components, and argue that managers with task-specific skills generate positive externalities at the firm level that are not reflected in their performance measured at the fund level. We advance and test empirical hypotheses on the existence of these positive unmeasured externalities by examining whether managers with task-specific skills are more likely to be associated with activities such as mentoring, increased risk taking, and generating spillovers at the firm level. Our results show that managers with task-specific skills are indeed associated with greater positive externalities, compared with managers with firm-specific skills. We discuss the implications of our results for the literature on human-capital value creation and appropriation.

Keywords: human capital, spillovers from human capital, value appropriation, mutual funds

Acknowledgements: We would like to thank Gary Dushnitsky, Isabel Fernandez-Mateo, Martin Ganco, Panos Mavrokonstantis, Madan Pillutla, Henri Servaes, two anonymous reviewers, and seminar participants at London Business School, University of Minnesota and Washington University in St. Louis for their constructive feedback on earlier drafts of the paper. We would also like to thank Marcin Kacperczyk and Clemens Sialm for help with constructing the measures, Russ Wermers for providing the benchmark data and the industry experts who shared their time and deep knowledge of the context in interviews with us.

Introduction

How do firms derive value from human capital? The literature highlights two divergent qualities of human capital: its potential for value creation and value capture (Barney 1991; Becker and Gerhart 1996). On the value creation side, competitive advantage results from employing high-quality human capital on account of its rarity and disproportionate productivity (Crook et al. 2011) – an insight often echoed in CEOs' letters to shareholders praising human capital as the firm's most valuable asset (Brummet et al. 1968). On the value capture side, firms are disadvantaged when human capital is transferable across firm boundaries (Becker 1964). If employees can easily replicate their performance across firms, they can use outside labor markets to bid up wages and capture the value they create. Uniting the two literatures suggests that firms should employ high-quality employees only if they can limit their performance transferability.

The literature on this issue documents a range of mechanisms that firms can employ to achieve both high performance and non-transferability. One set of mechanisms looks at the role of labor market inefficiencies in driving workers' outside options. A central feature in such models is asymmetric information between current employers and external labor markets about workers' performance and ability (e.g., Acemoglu and Pischke 1999; Autor 2001). This results in wage discounting in the external labor market and allows the employer to capture the difference (Blundell et al. 1999). However, even in the absence of true asymmetric information, others have argued that as long as employees perceive their skills to be non-transferable, employers can gain from such skills (Coff and Raffiee 2015). Still others take a firm perspective and examine ways in which firms can limit transferability by redesigning organizations to include more teamwork (Ichniowski and Shaw 2003), increase reliance on complementary and proprietary assets under the firm's control (Wexley and Latham 1991), codify employee knowledge so that departure does not hurt the firm (Levitt et al. 2013), alter employment contracts to reduce employee bargaining power (Williamson et al. 1975) or elicit employee identification with the goals of the organization (Simon 1947).

These theories aside, a vexing puzzle is why in many human capital-intensive settings where employees' performance continues to be largely transferable[1] and publicly observable, firms still pay premiums to acquire transferable skills (Groysberg 2010). Similarly, it is unclear why external recruitment

[1] Extant approaches to limiting skill transferability also leave unquestioned the assumption that transferable skills are capable of creating equal value in otherwise different firms (Coff and Kryscynski 2011).

and temporary employment arrangements to exploit transferable skills are steadily replacing firms' internal labor markets that can foster firm-specific skills (Bidwell 2013). Even at the C-suite level, the largest 2,500 organizations in the world increased their external recruitment of CEOs by 57% between 2004 and 2015 (Aguirre et al. 2016). Some have celebrated these changes as evidence of the shift in bargaining power from the firm to the high-skilled workforce (Arthur and Rousseau 1996; Barley and Kunda 2006). This, however, presents a puzzle: if transferable human capital captures most of the value it generates, what drives the value that firms ascribe to it?

In this paper, we advance an alternative mechanism rooted in human capital externalities for how firms generate and appropriate value from employees with transferable skills. In particular, we suggest that there are two sides to transferable skills: 1) the negative effect of surplus dissipation due to skill transferability, which is extensively discussed in the literature, and 2) a positive effect of skill transferability that stimulates positive externalities for firms, which has been largely overlooked in prior work. Our proposed alternative explanation reflects this positive effect. We argue that employees with transferable skills relative to employees with firm-specific skills have a greater potential to generate positive externalities that accrue largely to the firm. This mechanism for value capture from human capital does not rely on limits to skill transferability.

The empirical setting for our study is the US mutual-fund industry, where the fund-manager appropriation puzzle is well documented. Active managers outperform their passive counterparts before expenses are factored in, and underperform them net of expenses (Elton et al. 1996a; Gruber 1996). While complete skill transferability could explain why managers are able to appropriate up to the value they create, it fails to explain why managers are able to appropriate over and above that value and why management companies do not or cannot limit managers' skill transferability by tying it to the firm's less mobile assets[2]. Our theory addresses this empirical puzzle by arguing for the presence of unmeasured and unpriced externalities created by employees with transferable skills.

[2] An alternative strategy that has gained some traction in the industry in recent years is the rise of anonymously managed funds, where fund companies intentionally refused to publicly disclose manager names in an attempt to curb their bargaining power in the firm (Massa et al. 2010). Nevertheless, over 70 percent funds continue to disclose manager names and offer solo-managed funds, which, coupled with the poor average performance of active funds net of expenses relative to their passive counterparts suggests that there may be other levers used by fund management companies to obtain value from human capital than organization design alone.

We present evidence for three sources of positive externalities from employees with transferable skills. First, we expect that, on the supply side, because transferable skills are often acquired on the job, employees with such skills are more likely to be asked to mentor employees without such skills. Second, because employees with transferable skills have more outside opportunities in the labor market, they are likely to be less risk-averse (and require less insurance) than employees with firm-specific skills. This increases the likelihood of employees with transferable skills being used in roles involving greater risk-taking, such as launching new products. Third, on the demand side, we argue that the external reputation of employees with transferable skills may extend beyond the employee's narrow task and benefit the entire firm by generating spillovers (e.g., attracting new customers and/or boosting their willingness to pay for products and services). Such activities (i.e., mentoring, risk taking, and generating spillovers,) constitute an externality because they are either unmeasured (i.e., not visible or metered as part of the duties of a mutual fund manager) or unpriced (e.g., the value of the activity is unknown due to selection effects). It is important to acknowledge that the goal of this paper is neither to exhaustively identify and measure all sources of positive human capital externalities nor to provide causal evidence. A more modest aim is to advance a theory of firm value capture from human capital that is rooted in positive externalities, and to provide robust (correlational) empirical evidence consistent with our proposed mechanism.

The rest of the paper develops these arguments in greater detail. We start with a brief overview of the literature on value appropriation from human capital and use it to develop our theory and empirical hypotheses. We then explain the decomposition of employee skills into transferable- and firm-specific components and perform validation checks. Finally, we present the results on the hypotheses tests, offer robustness checks, and discuss the implications of our findings for managing transferable human capital.

Value appropriation from human capital

Early treatments of human capital simply viewed labor as an input to production (Clark 1899). In perfectly competitive markets it implied that wages were equal to their marginal products. If a firm paid less than the marginal product, the employee would leave and seek employment in a rival firm. Because labor was unskilled, the cost of hiring, losing, and replacing employees was assumed to be zero. Becker (1964) sought to account for human capital as distinct from commoditized labor. His effort centered on the difference between general skills (or capital) and firm-specific skills. General skills can be used in any firm without the loss of productivity which makes them transferable. In contrast, firm-specific skills create a wedge between productivity in the firm and the outside market.[3] As a result, firms have no incentive to invest in training their workers in general skills. So workers bear the full cost of such training and reap all the associated returns (will be paid their marginal products). In contrast, firms have a strong incentive to invest in firm-specific skills training, and employees are willing to acquire such skills in return for receiving a guarantee of stable employment and a steady income.

Yet, these predictions are not always borne out in the data. In practice, firms often pay employees for acquiring transferable skills (Barron et al. 1999; Booth 1993; Lillard and Tan 1993) even if they are part-time or temporary employees (Autor 2001). Moreover, in contrast to Becker's original models which distinguished between the location of training – general skills were acquired through formal schooling, and firm specific skills were obtained from on-the-job training – subsequent work has recognized that many of the transferable skills that firms value are in fact learned on the job, rather than in educational institutions (Kambourov and Manovskii 2009; Neal 1995; Williamson et al. 1975; Winter and Szulanski 2001). In fact, on-the-job learning now accounts for a majority of the variation in productivity across organizations and even countries, at least some of which may be imputed to employer-sponsored training and mentoring programs (Syverson 2011). Examples include industry-specific (Neal 1995), occupation-specific (Kambourov and Manovskii 2009), and task-specific (Gibbons and Waldman 2004) skills. In this paper, we follow the literature and focus on task-specific skills, though we see the different types as broadly synonymous[4]. The trend, however, poses a puzzle: if firms are unable to appropriate returns from the skills that they pay to impart, what motivates them to do so?

The literature offers three theories to account for the observed patterns. First, labor market characteristics can affect a firm's willingness to pay for training in transferable skills (Blundell et al. 1999). The central insight is that labor market frictions can render transferable skills de-facto firm-specific. For instance, Acemoglu and Pischke (1998) and Autor (2001) show that when it is costly for external

[3] Firm-specific skills are similar to the notion of co-specialized assets (Ethiraj and Garg 2012). The skills may generate greater value in the presence of other assets tied to the firm such as routines, team of employees, knowledge embedded in capital and so on.
[4] Despite the broad similarities among the different types of transferable skills acquired on the job, we focus on task-specific skills in this paper due to the nature of our context where the skills needed to perform well within a task are relatively well defined, but vary substantially across tasks in the industry and occupation.

organizations to accurately observe an employee's performance (or ability), even transferable skills will be discounted in the labor market, allowing employers to profitably invest in and reap returns to transferable skill training. While these are certainly plausible accounts in many settings, the findings do not extend to cases where employee performance is publicly observable and external wage offers contain premiums. Moreover, these approaches model the value of human capital at the individual level, reflecting the theory's initial goal of accounting for labor market patterns (Becker 1962). Firms are largely missing from this picture.

Second, firms can structure employment contracts to capture value from transferable skills using legal deterrents in the form of intellectual property rights and non-compete agreements that limit employee mobility and bargaining power (Marx et al. 2009). However, available empirical evidence suggests that legal tools still fail to prevent exit among top performers (Ganco et al. 2015), and have important trade-offs for the rest of the firm. For example, firms in states with greater enforcement of non-compete agreements make investments in higher-risk R&D projects and display greater focus on short-term performance (Conti 2014). This suggests that relying solely on legal means to limit mobility may have competitive implications for the firm in resource- and product-markets. So, the net benefits of such legal means are not unambiguous.

Third, firms can shape human capital transferability by encouraging the co-specialization of human capital with the firm through in-house training in firm-specific knowledge and skills (Wexley and Latham 1991) and organization design instruments such as a greater reliance on team production (Ichniowski and Shaw 2003) or internal promotion to non-entry level jobs to incentivize employees to stay and acquire firm-specific skills (Doeringer and Piore 1971; Prendergast 1993). Yet, direct empirical evidence on co-specialization of human capital with the firm is sparse (Blundell et al. 1999). Instead, firms are increasingly filling senior roles with external hires (Bidwell and Keller 2014) and paying them premiums despite the known performance losses (Groysberg et al. 2008). And employees fail to see their human capital as non-transferable irrespective of their tenures in firms (Coff and Raffiee 2015).

In sum, the existing literature is unable to provide satisfactory answers to the question of whether or how firms can benefit from employees with task-specific skills in settings where employee performance is accurately observed and there is a wage premium for employees with transferable skills. In the following section, we outline a theory rooted in positive externalities arising from employees with task-specific skills.

Human Capital Externalities

Where do human capital externalities come from? Original treatments of human capital assumed that the impact of training is fully reflected in individual performance and firm performance is simply the sum of individual contributions. Even if surplus is created from interactions among human resources, the precise contribution of each individual can be captured in the notion of marginal product and bargained for accordingly. In contrast, there is ample empirical evidence on the importance of human capital complementarities for value creation at the firm level (Battu et al. 2003; Datta et al. 2005; Ethiraj and Garg 2012; Ichniowski et al. 1997). Acemoglu and Angrist (2001) estimate that human capital externalities explain 25-30 percent of the variance in growth rates across countries. This is in comparison with 6-10 percent private returns associated with an additional year of schooling (a direct measure of human capital). This implies that the holders of human capital do not capture all the returns from human capital, raising the possibility that the residual claimants may be the firms that employ them. The interesting question then is whether firms can deliberately aim to create and appropriate the value of such externalities.

To answer this question, we begin by distinguishing between supply-side and demand-side drivers of human capital externalities. On the supply-side, an externality-producing task is one that affects the performance of other workers in the firm, but not the job performance of the focal individual completing the task. Such tasks often have some of the following characteristics: the behaviors needed to complete a task are observable to colleagues, the completion of the task involves significant interaction between workers, or the task involves explicit mentoring. Several studies have tried to quantify the magnitude of such externalities. A study of teachers in elementary schools found that 20 percent of pupil performance can be attributed to the characteristics of other teachers who are teaching at the same school, but do not come into direct contact with the focal teacher’s pupils (Jackson and Bruegmann 2009). Similarly, a study of farmers in India found that the marginal return to adopting high yield seeds was 30-40 percent greater if one’s near neighbors also adopted the seeds than if they did not (Foster and Rosenzweig 1995).

On the demand-side, rather than affecting the performance of colleagues, an externality-producing task affects the returns to the same level of performance. Task characteristics that generate externalities include the task's (and the individual performing the task) visibility to customers and the extent to which a task creates or boosts complementarities among other tasks. For instance, Hausman and Leonard (1997) show that in the National Basketball League, superstar players create substantial externalities at the league level through increased ticket sales and spikes in television viewership even for away games. They estimated that the externality associated with Michael Jordan was about $53M for the *other* teams in the league. For task demand-complementarity, evidence from mutual funds suggests that creating new funds is often driven by the relationship between existing funds – the addition of a new type of fund may at times boost demand for other funds if customers are looking for a certain 'set' to invest into (Massa 2003).

The mere existence of externalities, however, does not ensure their appropriability by the firm. If such firm-level externalities can be traced back to specific tasks and employees, they can, at least in theory, be bargained away. In practice, though, estimating and pricing individual contributions to firm-level externalities is wrought with difficulties. The measurement problems arise due to the twin conditions of spatial and temporal separation of behavior and performance. By spatial separation we mean that the effects of the externalities are absorbed by and reflected in the performance of other workers in the firm, whose behavior cannot typically be held constant to identify the true value of the externality. By temporal separation we mean that most performance effects of externality-creating tasks are observed with a time lag that can span from weeks to years, depending on the performance metric used. As a result, both firms and employees are unable to fully disaggregate externalities into individual contributions.

While the measurement problem affects both employees and firms, its effects on their relative bargaining power may not be symmetric. First, firms are likely to have a more complete picture of their historical performance with and without specific employees, permitting a more accurate evaluation of an individual's value added. Second, a key source of a worker's bargaining power is the presence of outside wage offers, which are typically determined by the job and observable performance in it (Cahuc et al. 2006). In most cases, given the spatial and temporal separation of behavior and externality creation, neither the task nor observable performance will include the value of externalities that the employee creates. As a result, the employee is unlikely to be able to use it to bid up wages. Finally, to the extent that an employee's potential for externality creation varies across firms (e.g., due to job design differences), employees are less likely to be able to use multiple offers from firms to bid up their wages.

With this background to the source and significance of human capital externalities we turn to explicating the empirical implications of our theory linking human capital externalities with task-specific skills. Our theory starts with the assumption that for the relationship between the firm and the employee with task-specific skills to be sustained there must be a positive (or at least a non-negative) value that accrues to the firm. Our principal thesis is rooted in the existence of positive externalities for firms from the employment of workers with task specific skills. While such employees may indeed appropriate the measured surplus they create, we argue that they also create unmeasured and/or unpriced positive externalities that are mostly appropriated by their employer. Because human capital externalities are difficult to measure, we attempt to estimate them indirectly. Rather than testing the effects of individual-level skills on externalities at the firm level, we first identify three activities that help generate positive externalities at the firm level that are not measured at the task level. Second, we surmise that if the externalities associated with task-specific skills are greater than those associated with firm-specific skills we should observe more involvement of workers with task-specific skills in such externality-generating activities. Finally, we empirically confirm in our setting that the externality-generating activities we have identified do indeed lead to positive externalities at the firm level that are not fully reflected at the task level.

The three externality-generating activities that we focus on derive from prior work on the key challenges of managing human capital-intensive activity: (1) mentoring in task-specific skills (Acemoglu and Pischke 1998), (2) costs of risk-taking (Baker 1992), and (3) fostering spillovers (Moretti 2004). We theorize about the externalities associated with these elements and advance testable hypotheses. Our focus on these three sources of externalities is not meant to be exhaustive but, instead, to offer an existence proof for our theory that firms can benefit from externalities created by employees with task-specific skills.

<u>Mentoring</u>

The issue of maintaining task-specific skills in the firm has received much scrutiny in the literature. Because such skills are valuable and often essential for performance (Gibbons and Waldman 2004, 2006), firms cannot leave their acquisition entirely unmanaged. At the same time, however, the most common ways of acquiring human capital, such as purchasing it in the labor market or offering formal training programs in-house, are often inefficient and ineffective. First, direct purchase is costly because even when skills are fully transferable, employees still need to be compensated for the effort and risk involved in moving to a new firm. Second, task-specific skills are often tacit[5], narrowly applicable (Gibbons and Waldman 2006; Williamson et al. 1975), and subject to rapid change (Bessen 2015), rendering their codification and dissemination through firm-wide training programs difficult. Evidence from contexts as varied as shipbuilding (Argote et al. 1990), software development (Boh et al. 2007) and sales teams (Chan et al. 2014) shows that when skills have tacit components (Edmondson et al. 2003), are complex (Song et al. 2015) or change rapidly (Bessen 2015), employees learn more and faster if they have opportunities to observe colleagues and share their knowledge and practices, than if they learn by doing alone. As a result, firms frequently rely on peer mentoring as a means of transferring task-specific skills across cohorts in the firm and experienced employees with task-specific skills are in the best position to provide such mentoring (Covaleski et al. 1998).

In contrast, training in firm-specific skills can often be achieved through a variety of means other than direct mentorship. Many firm-specific practices are relatively stable over time and knowledge of them is distributed throughout the organization which makes them amenable to codification and dissemination without direct mentoring (Baker et al. 1994). Moreover, firm-specific skills are often best acquired through a wide range of interactions within the organization. It appears that newcomers assimilate faster and perform better if they have a broader and weaker network of ties to a multitude of organizational participants than if they have a smaller, stronger set of mentoring relationships (Fang et al. 2011). This is because all of the non-role-specific knowledge newcomers need to understand – such as what the organization does and how their jobs fit within it – is rarely possessed by a single mentor (or even a few mentors). Rather, such knowledge is typically widely dispersed across a range of participants. As a result, we expect that direct

[5] Becker's (1964) original models did not explicitly discuss tacitness, because he defined general skills as those obtained through formal schooling and firm-specific skills as those obtained on the job. Subsequent work has often equated general skills with codified knowledge and firm-specific skills with tacit knowledge (Hitt et al. 2001). However, this mapping need not be one-to-one. Once we accept that some transferable skills are obtained on the job rather than in formal schooling, the key difference between transferable and firm-specific skills becomes the extent to which they can be replicated by the *same person* across firm boundaries, not how easily they can be replicated by *anyone*.

mentorship of newcomers will be used more for imparting task-specific knowledge that often requires close collaboration and observation of performance to acquire, than firm-specific knowledge (Fang et al. 2011).

The value of mentorship often goes beyond the mentor-mentee relationship creating a positive externality, which accrues mostly to the firm. This occurs for at least three reasons. First, the value of mentorship permeates throughout the organization and has long-term consequences in the form of maintaining and disseminating best practices (Bartel et al. 2014), establishing knowledge-sharing norms (Di Stefano et al. 2014) and attracting workers willing to learn (Agrawal et al. 2017). These benefits can reach across successive cohorts and continue to yield benefits for firms even after the specific mentoring relationship ends. Second, neither the mentor nor the mentee typically know the true costs and benefits of mentorship. If these were known, the skilled employee could contract with and directly charge the rookie for providing the training, or could bargain with her employer for a greater share of the value. However, because the efforts and ability of the mentor and the mentee are, in general, unobservable and non-contractible (Morrison and Wilhelm 2004), the costs and benefits of training, while precisely known in theory, are stubbornly elusive in practice (Kaplan 1992; Luft and Libby 1997). As a result, mentorship cannot be mandated, but must arise as a by-product of regular activity – an externality – where the mentor's verifiable costs (if any) of providing the mentorship are minimized[6]. Finally, the costs and benefits of mentorship for the mentee are usually temporally separated because markets lag in their recognition of the mentee's newly acquired transferable skills (Acemoglu and Pischke 1998). As a result, firms can profitably exploit the (often short) window of time during which there is a chasm between the mentee's performance and the market's recognition of that performance. Thus, we hypothesize,

Hypothesis 1: *Employees with predominantly task-specific (firm-specific) skills are more (less) likely to mentor their colleagues.*

Risk-taking

A canonical problem in managing organizational adaptation to change is designing incentive structures that are able to effectively trade off risk and insurance (Prendergast 2002). In dynamic environments where survival depends on timely adaptation, risky exploration of alternatives may present

[6] One of the means of achieving such externalities is teamwork (Hamilton et al. 2003), where experienced workers are paired with rookie employees and their performance is evaluated jointly. This practice also helps ensure the mentor is motivated and the mentee can directly observe the operation and execution of the skills (de Grip et al. 2016).

high benefits and high costs at the firm-level, but the net effects may not be symmetric between the firm and its employees. For firms, exploration is justified if its costs can be offset and benefits multiplied by distributing them across the entire organization. For instance, whether or not the exploration of a new idea directly leads to new business generation, the knowledge gained through exploration in one part of the organization can often be usefully applied in other parts (Davis and Eisenhardt 2011). Moreover, firms can diversify their exploration efforts across multiple paths and reduce the risks associated with any given attempt (McGrath 2001). In contrast, for the individual employees who are exploring, the costs of each attempt are immediate and concentrated within the task itself, and the benefits are uncertain, distributed and temporally separated – all of which are difficult for firms to offset with incentives alone (Holmström 1999).

How to correctly structure the incentives for individuals has therefore been an important quest in this literature. If incentives were closely tied to performance, employees would become risk-averse because they would bear the costs of uncertainty (Hölmstrom and Milgrom 1991). In contrast, if the firm bears the cost of uncertainty, the negative side effects of insurance arise. Employees could take excessive risks, especially if they get a share of the upside but not the downside (Nickerson et al. 2001). Workers could also engage in shirking or low effort because it may be difficult for the firm to decompose signal from noise.

We surmise that employee skill transferability may help to partially alleviate some of the problems associated with incentivizing risk-taking behavior. Because skill transferability contributes to employees' outside labor market options and labor market perception of employee skill is cumulative and takes time to develop, employees possessing transferable skills may be better able to secure outside employment following poor performance than employees possessing mostly non-transferable skills. As a result, workers with transferable skills may be more willing to take on risky tasks, i.e., tasks with a high probability of failure, even when failure can lead to losing one's current employment. This is true even when their performance is publicly observable, because any specific case of underperformance does not render their transferable skills irrelevant. This sets up the possibility of discriminating behaviors from workers with task-specific and firm-specific skills. Note that we are not arguing that the incentive problem disappears for employees with task-specific skills. The more limited claim is that the firm's cost of incentivizing employees with transferable skills for risk taking would be lower than the corresponding cost of incentivizing employees with firm-specific skills. Thus, we hypothesize:

Hypothesis 2: *Managers with predominantly task-specific (firm-specific) skills are more (less) likely to be associated with risk taking.*

Spillovers

A third challenge of managing human capital is in capturing spillovers. Spillovers on the supply side account for a large component of economic growth (Romer 1990). Such spillovers constitute a leakage of private R&D from firms into the public domain. This spurs further innovation and lifts the aggregate value of technology in the economy. Evidence from aircraft production (Benkard 2000), ship-building (Argote et al. 1990), automotive manufacture (Levitt et al. 2013), and hospitals (Edmondson et al. 2001) supports this claim.

A second form of spillovers on the demand side also appears significant. These spillovers accrue from reputation in a world of imperfect information (Mayer 2006). For instance, when product quality is unobservable, reputation for quality can be an effective proxy for actual quality, and yield benefits such as increased sales (Diekmann et al. 2013), and higher willingness to pay (Shapiro 1983). Reputation effects can also carry over from products to the firm itself such that the known quality of one product can, via contagion, spread to other products where quality is unknown (Hagiu 2009). Such reputation effects are important in human capital-intensive contexts where past performance is observable, but there is uncertainty about future performance (Gompers et al. 2010).

We focus here on demand side spillovers. If past performance can generate demand-side spillovers, the remaining question is whether managers with task-specific skills are more likely than managers with firm-specific skills to generate such spillovers. Theoretically, there is at least one reason to expect managers with task-specific skills to acquire greater external reputations. Employees with task-specific skills are not only more mobile across firms, but also more likely to move in practice. Although precise measurement of transferable skills is difficult, empirical evidence suggests that higher levels of formal education and task-specific skills acquired on the job increase mobility rates (Gathmann and Schönberg 2010), while tenure, a common measure of firm-specific skills, reduces mobility (Topel 1991). The greater mobility of employees with task-specific skills, in turn, allows for the separation of employee skills from the firms within which they were employed. Consequently, markets can more accurately assess the ability of employees with task-specific skills than the ability of employees with firm-specific skills. This means that two managers with the same past performance may differ in their external reputations as a function of the extent to which their skills can be separated from their employing firm. In sum, holding past performance constant, we expect that managers with task-specific skills are likely to have greater external reputations as compared with managers with firm-specific skills. To the extent that demand-side spillovers are increasing in external reputations, we expect that employees with task-specific skills will generate greater demand-side spillovers than will similar employees with firm-specific skills. Thus, we hypothesize:

Hypothesis 3: *Managers with predominantly task-specific (firm-specific) skills are more (less) likely to be associated with positive spillovers at the firm level.*

Context & Data

We test our hypotheses in the context of the mutual fund industry, which provides an excellent foil for our theoretical arguments about the link between human capital surplus appropriation and the firm-level externalities that they generate. The mutual fund industry is comprised of many diverse firms, each offering at least one, but often multiple mutual funds, and employing or contracting with managers to make investment decisions. Following SEC reporting requirements, mutual funds are required to disclose their stock holdings for the preceding quarter. Therefore, quarterly performance and cost data is available at the firm-, fund-, manager- and even stock- (investment decision) levels. As a result, we can observe the compensation[7] of fund managers and their performance net of risk and relative to industry benchmarks. Further, the investment (buy and sell) decisions of managers are also observable, which affords the measurement of their skills as distinct from performance. Finally, because fund management companies usually offer several funds and employ several managers, manager performance can be separated from firm performance. This allows the identification of human capital externalities.

[7] An important limitation of our data is that management fees disclosed in the prospectus of a mutual fund often include both the fees paid to the fund manager and the share retained by the fund company. A priori, this makes it difficult to make any conclusive claims on whether fund managers or fund management companies are appropriating the surplus. That said, this limitation does not directly affect the main claim in the paper that managers with task-specific skills will generate externalities for the fund management company. It does, however, affect a validation check we perform to examine the wage effects associated with general skills and firm-specific skills.

Our data comes from two sources. First, from Thomson Reuters we obtain quarterly stock holdings[8] for all domestic, US-based mutual funds and their self-declared investment objectives (e.g. growth, income, balanced). Because our focus is on actively managed domestic equity funds, we drop from our sample funds with investment objectives of 'international', 'metals', 'municipal bonds', and 'bond and preferred'. We also omit funds based in countries other than the US and containing 'Index' in their names. Second, from the Centre for Research in Security Prices (CRSP) we obtain fund-level data on total net assets, expense ratios, management fees, marketing and distribution costs, the dates that the first share class was offered and each manager started managing the fund, manager names, and advising company names for all US-based mutual funds. The same data source also provides the unique identification codes, prices, shares outstanding, and returns for all listed stocks on NYSE, NASDAQ, AMEX and NYSE Arca.

We merge the Thomson holdings data with CRSP data on mutual funds using the Mutual Fund Links (MFLINKS) file provided by Wharton Research Data Services. To account for each stock's risk and momentum, we use benchmark returns based on the stocks' momentum, book-to-market ratio, and size, created by Wermers[9] (Daniel et al. 1997; Wermers 2000, 2003). To be included in our final sample, each fund must have valid holdings data from Thomson, total assets, expenses, returns, and other control variables listed in Table 1 from CRSP, and a valid benchmark return from Wermers' file. Following existing practice, we exclude stocks with prices below $1 (in 2013 prices), funds with total net assets falling below $1m in more than one quarter, and all funds with the number of shares held below ten. Funds are managed either by a single manager or by teams. In the skill identification part of the analysis we limit our analysis to single-manager funds so that we can assume a one-to-one mapping between managers and fund performance. When testing our hypotheses, we also include team-based funds. We use manager names[10] reported in CRSP to identify unique managers and rely on their fund's stock trading decisions to decompose their skills into task- and firm-specific components. The use of these filters results in a quarterly panel of, on average, 760 mutual funds in 192 asset management firms between 2002 and 2010.

[8] Thomson Holdings report most equity funds, but not all. Potential omissions include holdings that are small (under ten thousand shares or $200,000), confidential, or unmatched to Thomson's master file (Wermers 2003).
[9] The DGTW benchmarks and their construction are available via http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm. For further details, please see Online Supplement, Appendix B1.
[10] Details of our procedure to identify unique managers are available in the Online Supplement, Appendix A2.

Methods

Our main claim is that managers with task- as opposed to firm-specific skills generate positive externalities at the firm level that are unmeasured at the fund level. In providing evidence for this claim, we take a two-part approach. In the first part, we first decompose fund performance into manager skills. Second, we label each skill as chiefly task- or firm-specific. While there is an established literature to guide us in decomposing fund performance into separate skills, it does not label the skills as task- or firm-specific. To accomplish such labeling, we draw on findings in prior studies of mutual funds, the practitioner literature aimed at mutual-fund managers, and our interviews of mutual-fund managers and analysts. Third, we validate our labelling with a series of empirical tests that ensure our mapping conforms to established theoretical predictions regarding task- and firm-specific skills. In the second part, we use the firm- and task-specific skill measures we derived in the first step to test our hypotheses. To improve readability, we focus here only on the most essential results for Part I and direct interested readers to Appendices A, B and C in the Online Supplement, where we provide further details on the process we use to construct our dataset, create our measures, and validate them.

<u>Part I: Identifying task- and firm-specific skills of fund managers</u>

Step 1: Fund-manager performance decomposition

We assume that fund manager performance is a function of her skills and a stochastic component:

$$\textit{Manager Performance}_{ijt} = \textit{Task-Specific Component}_{it} + \textit{Firm-Specific Component}_{it} + \varepsilon_{ijt} \quad \ldots\ldots\ldots(1)$$

for manager *i*, in fund *j*, in year *t*, where each component reflects the performance effect of the underlying skill (task- or firm-specific) [11], and ε reflects all idiosyncratic characteristics of the fund, time-specific effects, and randomness associated with manager performance. We follow the method in Daniel et al. (1997) and use data on the quarterly buying and selling decisions of fund managers to decompose raw fund performance (quarterly Gross Return) into three distinct components of ability that reflect different manager skills, each of which is measured in relation to its benchmark. Thus, skill here is distinct from performance and stock characteristics (i.e., risk), although we separately control for fund-return volatility. The first component is the ability to select stocks that outperform their benchmarks, which Daniel et al. (1997) label

[11] Our skill measures assume substitutability between firm- and task-specific skills, and allow managers to possess high amounts of both skills.

"Characteristic Selectivity." The second component is the ability to time investments into particular stock types, as described by their benchmarks, and is labeled "Characteristic Timing." The third component is the skill in holding stocks of particular types, as defined by the benchmark portfolios, and is labeled "Average Style." Further details of our calculation of the skill components and our replication approach following Daniel et al. (1997) are included in Appendices B1 and B2 of the Online Supplement. Descriptive statistics and Kernel density plots of our measures are included in Appendix B3.

Step 2: Mapping the three components of ability to task-specific and firm-specific skills

We reviewed the literature and conducted interviews with mutual fund managers and analysts to map the three skill measures identified in Step 1 above to firm- and task-specificity. The process of portfolio construction begins with a detailed understanding of client needs that the fund aims to serve. The most common metrics used are: client risk/return preferences, their investment horizons, and the tax implications for their returns (Fabozzi and Markowitz 2011). Based on these inputs, the manager selects an investment style and picks stocks in three distinct, but often simultaneous, steps: stock type (bucket) selection (Average Style), individual stock selection within each bucket (Characteristic Selectivity), and the timing of the purchase and sale of individual stocks and buckets (Characteristic Timing). While active fund managers typically retain autonomy over these decisions, the fund company monitors managers by placing some constraints on their decisions (Almazan et al. 2004). These constraints vary widely between firms even for the same 'type' of fund, resulting in substantial learning curves for managers new to a firm (Almazan et al. 2004). While the restrictions reduce manager discretion, they do not dictate specific investment decisions. We use firm constraints to predict the mapping of the three skills into firm- and task-specific.

The most immediate impact of firm constraints is on Average Style. This measure captures the return to stock types that a manager holds in her portfolio, and is largely determined in the first step of portfolio construction – the selection of a pool of stocks. We learned from our interviews that because clients often wish to simultaneously invest in multiple funds at the same firm (where each fund meets only a part of the client's investment objectives), management companies are rigorous in enforcing strategies that achieve complementarity across funds. They employ a mix of formal (fund mandates) and informal (norms, culture) restrictions on funds' investments and hold managers accountable for adhering to them. As one of our interviewees notes, “If you [potential investment companies] don’t believe in our philosophy or if you’re [the company] not the kind of company that fits that philosophy, then we’re just not going to invest” (Fund Manager, Alliance Bernstein). However, these restrictions do not fully eliminate manager discretion (Fabozzi and Markowitz 2011) and managers remain responsible for the weights on each bucket captured by our Average Style measure. In the words of one equity fund manager, “The benchmark pre-supposes that you have certain places where you begin to sell out of a given position… but in reality you are not waiting for the moment when the index rules tell you something goes out… you might be doing it before or after… so there are plenty of opportunities for active management” (Fund Manager, Aviva Investors). Based on this evidence, we expect performance on Average Style to depend on detailed knowledge of the firm’s policies and aims, and largely reflect firm-specific skills.

Firm constraints also affect the second step in portfolio construction – timing the changes in stock characteristic weights, a skill captured with our Characteristic Timing measure. Timing is not deliberate in all funds (Avramov and Wermers 2006; Daniel et al. 1997), but when it exists, it manifests as the manager’s ability to shift holdings across stock types. Three fund characteristics drive this ability. First, the fund’s concentration (the number of stocks held in the portfolio), determines how timing is executed. In funds with many stocks, turnover is common and timing directly drives some investment decisions. In concentrated funds, as one of our interviewees noted, timing is used mainly to fine-tune investments[12]: “timing is more to better assess the entry or exit point – so more of ‘I like the idea, but you know, if you look at the volume, you look at the price…let’s wait a little bit, because I see that the market timing is not great. Likewise, I like the idea, I’m ready to press the button, but then there’s a profit warning – maybe that could be an entry point” (Equity Portfolio Specialist, Morgan Stanley Investment Management). Second, restrictions on stock turnover and investment horizon imply differences in how timing is achieved. In funds with shorter investment horizons and higher turnover, timing can react to short-term stock price fluctuations and momentum. In funds with longer horizons, timing implies trading only on broad, long-lasting shifts. While such timing skills can be replicated across firms, the wide variation of concentration and turnover restrictions between similar funds, but in different firms, requires significant adaptation by managers.

[12] Two funds with the same level of concentration may still have to be managed differently depending on the fund’s benchmark and the tolerable tracking error that dictate the kinds of shifts across stock characteristics that can be done, so managers need a deep understanding of the benchmarks of the fund and of other funds in the firm.

Finally, the manager's relationship with the fund company's trading desks dictate the effectiveness of timing strategies that require rapid or large shifts across stock positions. On balance, we expect timing skill to depend on the manager's experience in the firm and thus reflect more firm-specific skills.

The final step in portfolio construction is the selection of specific stocks from the consideration set – captured with our Characteristic Selectivity measure. Provided that the fund complies with the guidelines set out in its prospectus, active equity-fund managers are free to invest in any stock within these guidelines.[13] Prior research shows that their choices within these guidelines determine the bulk of the variation in the fund's relative performance within its category (Daniel et al. 1997). As one interviewee noted, "you are not paying our fee for us to time when the market is gonna go in and out; you are paying our fee because we are picking high quality companies for your portfolio" (Fund Manager, Alliance Bernstein). Moreover, we learned from our interviews that while the knowledge of specific stocks and relationships with top management is sometimes delegated to analysts in the firm, the ultimate responsibility for investment decisions lies with the fund manager. In sum, we label this skill component, which we capture as Characteristic Selectivity, to be manager-specific (task-specific) and transferable across firm boundaries.

Step 3: Validating the measures of task- and firm-specific skills

Having established a mapping of performance to skill, we use manager moves across firms to validate the capacity of the three skill measures – Characteristic Selectivity, Timing, and Average Style – to discriminate between firm- and task-specific skills. We examine the sign and magnitude of the effect of manager change on each skill component across the first three quarters post-change. Because funds change managers for different reasons (Khorana 1996, 2001), we have no expectations about the effect of manager change on Gross Return. We expect no change or a positive effect on components that rely mostly on task-specific skills – Characteristic Selectivity – and a fall in components that rely mostly on firm-specific skills – Average Style and Characteristic Timing. The logic is that an outside manager will not have the firm-specific skills to match the performance of her predecessor, but she will, on average, possess similar levels of task-specific skills. In all our models, we expect the effects of change to be strongest in the first quarter and to dissipate over time, as managers slowly acquire firm-specific skills.

[13] In some cases (e.g. sector funds), the mandates are very specific, leaving relatively little discretion to managers. However, such funds are not included in our sample.

We identify and code instances of manager change for each fund using historical manager names and dates noting the start of each manager's tenure. This variable is binary, taking the value of 1 if the manager is new to the fund and firm and 0 otherwise.[14] For robustness, we also code internal manager changes – taking the value of 1 if the new manager has previously managed a fund for the same firm, and zero otherwise[15]. If the effects of manager change on fund performance are driven by the manager's firm-specific skills, we should see that the effects are attenuated if the new manager is only new to the fund, but has managed a different fund in the same firm previously, than if she is also new to the firm. Both measures are extended to one, two, and three quarters separately.

Because manager change could be endogenous to performance or ability, we follow Wooldridge (2002) and adopt a two-stage residual inclusion estimation procedure where in the first stage we run a probit selection model with internal and external manager changes as the dependent variables regressed on two instruments and a set of controls. Compared with two-stage least squares (2SLS), this method also yields consistent estimates and is particularly appropriate in cases where the endogenous variable is binary, but relies on the additional assumptions of normally distributed errors in the first stage and a correctly specified first stage, both of which we are comfortable with (Stock and Yogo 2005; Wooldridge 2002; Wooldridge 2015). For robustness, we repeat our analyses with 2SLS included in Tables OS3 and OS4 in the Online Supplement, Appendix C1.

Our instruments for manager change include (1) changes in top marginal income tax rates in the focal state where the fund management company operates, and (2) the average of the top marginal state income tax rates in all other states excluding the focal state[16], both lagged by one year. We assign tax rate changes at the state level, using fund management company headquarters location. Because fund and parent firm headquarters locations may differ, it adds some noise to our measures of tax rate changes. However, we unaware of any systematic bias that such measurement error could introduce. Company locations change infrequently over the course of our observation period, with only 21% of firms changing location during this time in the full sample (including firms with only team-managed funds). No firm in existence for over a

[14] We can only observe if a manager has been with the firm if s/he held a fund-management position. In case a manager worked as an analyst prior to taking over a fund and this is her first assignment as manager, we code her as "external to the firm." However, this makes our test of the firm-specific skill component conservative because analysts with experience within the firm should possess comparable firm-specific skills to their predecessors.
[15] In our tables, we include internal and external manager change indicators in the same regression, but the results are robust if we include them separately.
[16] We obtain this data from http://www.nber.org/~taxsim. For more information, see Feenberg and Coutts (1993).

year, with at least one solo-managed fund and surviving our data filters ever changed its location during our observation period. All our skill identification regressions are estimated on this subsample.

We believe these instruments satisfy the twin conditions of validity and relevance. In terms of validity, changes in the marginal state income tax rates are unlikely to be systematically and directly related to mutual fund manager performance. However, tax changes have recently been shown to have significant effects on the mobility of high earners across states and countries (Kleven et al. 2013; Young and Varner 2011), and the effects should be even stronger for more proximate moves such as those between New York, New Jersey and Connecticut. Indeed, the first stage and reduced form results confirm this in our setting: the instruments are significantly correlated with manager changes (the first-stage F-statistic is above 10 in most specifications) and they are also significant predictors of the Characteristic Selectivity, Characteristic Timing and Average Style measures in the reduced form.[17] We then obtain the residuals from the first stage regressions, and use them as controls in the second stage OLS regressions with fund fixed effects regressing Gross Return and its decomposition into Characteristic Selectivity, Timing, and Average Style, on external and internal manager change, one-quarter lagged firm performance and control variables defined in Table 1. To account for the estimated variable in the second stage, we bootstrap and cluster standard errors at the fund level.

< Insert Table 1 about here >

The summary statistics for the main variables are included in Panel A in Table 2 and a complete correlation table is included in Table OS2 in the Online Supplement. Our results, in Table 3, contain simple OLS results regressing fund performance components directly on manager change and the two-stage residual inclusion results, each broken down by the number of quarters following manager change (Panels 1-3). Each pair of results therefore stems from a separate regression.

Our main goal in this table is to determine the direction of change in skill components following internal and external manager change. We, therefore, focus on differences in the signs and statistical significance of the results between OLS and residual inclusion methods, and between internal and external manager changes. As expected, the results are different when comparing the OLS and instrumented

[17] We omit including these results for the sake of brevity, but they are available from the authors.

regressions. This confirms that manager moves are likely endogenous. In addition, the magnitudes of the effects differ between the two methods, suggesting that either the OLS estimates are biased towards zero or that our instruments are weak. We address the latter issue in detail in Appendix C1 in the Online Supplement. The results from the residual inclusion approach show that across the dependent variables, the magnitudes of the coefficients drop as we move from the first quarter only (Panel 1) to an aggregate of the first three quarters (Panel 3). Model 6 shows a significant drop in Gross Return for external manager changes, but not internal ones. Models 7 and 8 show that neither Characteristic Selectivity nor Characteristic Timing react to either type of manager change, but the signs on coefficients for Characteristic Timing are negative. Finally, Average Style is negative following both internal and external changes, but only the coefficients on external change are consistently significant in all three quarters. Along with the qualitative evidence we obtained from our interviews and industry reports, we interpret these results as suggesting that Characteristic Selectivity relies more on task-specific rather than firm-specific skills, while Characteristic Timing and Average Style rely more on firm-specific skills. We therefore combine CT and AS into a single component labeled Firm-Specific Skills and retain CS as a measure of Task-Specific Skills.[18] As expected, we find in Model 10 that external manager changes affect Firm-Specific Skills negatively and significantly. In the rest of the paper we proceed with these two measures of task-specific and firm-specific skills.[19]

< Insert Table 2 and Table 3 about here >

<u>Robustness tests for task- and firm-specific skill measures</u>

We sought to further validate our task- and firm-specific skill measures by empirically testing predictions derived from human-capital theory. In the interest of space, we describe these tests only briefly and direct interested readers to the Online Supplement, Appendix C2. We perform three tests. First, we examine if our measures capture manager skill rather than firm-level policy. If managers retain discretion over their portfolios, we should see that manager changes lead to substantive strategic changes in fund management, not just performance changes. We measure strategic changes as changes in the underlying benchmarks that the portfolios track, and compare the effects following internal and external manager changes. If we assume that external replacements possess fewer skill similarities to their predecessors than

[18] Our results are robust to coding Characteristic Timing as a task-specific skill.
[19] All results in the paper are robust to testing the effects of characteristic timing and average style separately.

internal replacements, we should expect external changes to result in greater strategic changes than internal managerial changes. The results, included in Table OS5 in the Online Supplement, Appendix C2, confirm this expectation. Second, we examine how managers' skills affect their tenure at the firm. If our measures capture firm- and task-specific skills, we should observe an association between firm-specific (task-specific) skills and longer (shorter) firm tenures, because the outside options of managers with firm-specific skills should be inferior to those with task-specific skills. The results, in Table OS6, accord with our expectations. Finally, we examine how managers' wages vary with their skills. Drawing on Becker (1962), we expect that holding constant total surplus at the fund level, relative to firm-specific skills, task-specific skills will be associated with larger surplus capture by managers[20]. Our results, in Table OS6, confirm our predictions.

Measures

Dependent variables

Mentoring. Ideally, we would measure mentoring by observing the interactions among mutual fund managers in the same firm. Unfortunately, however, access limitations preclude us from doing this and we rely instead on proxy measures. One such proxy is team fund management. In the mutual fund setting, team-managed funds account for over half of all funds in the industry and their proportion has been growing steadily over the years (Ferreira et al. 2013). While there are multiple reasons for having team-managed funds, an important one is training the next generation of managers. A growing literature has documented the value of teams in encouraging mentorship and boosting productivity beyond what can be achieved through learning-by-doing alone. These studies cover a wide range settings such as sales teams (Chan et al. 2014), software development (Bessen 2015; Boh et al. 2007), steel minimills (Boning et al. 2007), and teaching (Jackson and Bruegmann 2009). We argue this pattern also holds in the mutual fund setting and is particularly important for acquiring task-specific knowledge, which is embedded in individuals and difficult to codify and teach through firm-wide training programs. Thus, we expect that fund managers with task-specific skills are more likely than managers with firm-specific skills to join a team of less-skilled managers who are expected to learn from and be mentored by the employee with task-specific skills. We code team

[20] The surplus created operates as a proxy for each manager's marginal product.

management as a binary variable with the value of 1 if an existing manager already managing a solo-fund joins a team-managed fund in the firm, and 0 otherwise.

Risk-taking.[21] We identify risk-taking as an act by an existing manager of starting a new solo-managed fund. Launching a new fund is a common but risky activity, both for the manager and the firm. Turnover among funds is high, with between 36% and 56% of equity funds liquidated between 1990-2010 (Bogle 2005). Yet, the benefits of success are significant, particularly for firms. Investors typically invest in multiple funds in the same firm. Thus, the benefits (in terms of attracting new clients) of new funds, especially in new categories, reverberate across the entire firm (Massa 2003; Nanda et al. 2004). Our binary variable *New Fund* is coded 1 in the fund's first quarter of existence and 0 otherwise.

Spillovers. Our measure of spillovers captures the effects of a manager's skill in managing a focal fund on her ability to draw in new investments to the firm. We thus define spillovers as the cash inflows accruing to all the funds offered by the management company, excluding those managed by the focal manager. We borrow a standard measure of cash inflows from the finance literature, defined as the change in the fund's total net assets between any two quarters net of growth in value (Gruber 1996).

$$Family\ Inflows\ (excl.\,focal\ funds)_{ft} = \sum_{i=1}^{I-k} \left(TNA_{it} - (TNA_{it-1} \times Fund\ Return_{it})\right)$$

where k are the focal manager's funds, f is the firm, i is the fund, and *TNA* denotes total fund net assets.

Explanatory variables

Task-specific and firm-specific skills. For our main results, we use a binary measure of task- and firm-specific skills derived from the fund-level dataset and aggregated to the manager level by averaging each manager's quarterly performance across all sole-funds she manages concurrently. We then create a cumulative average measure for each manager along each skill dimension and construct a binary variable taking the value of 1 if the manager falls into the top 50% of managers on the task-specific dimension and bottom 50% on the firm-specific dimension, and 0 if the opposite is true. Managers high or low on both skill types are omitted, to allow a clean comparison. The binary measures reduce our sample size, but allow for an easier interpretation of the coefficients. However, due to the arbitrary nature of the 50% cutoff, we replicate all results with continuous measures, included in the Online Supplement, Appendix D.

[21] Note that risk taking in launching new funds is distinct from risk as a fund characteristic. The former is the firm/career risk of fund failure; the latter is the investors' risk of losing investments. All our analyses control for fund risk with benchmark adjustment, fund volatility, and Lipper categories.

Controls. All models include the controls listed Table 1.

Econometric specification

For the binary dependent variables (mentoring and risk-taking), we use LPM, logit, and probit models with year, quarter and firm fixed effects. For spillovers, we use OLS with year, quarter and firm fixed effects.

Results

Panel B in Table 2 presents the summary statistics and correlation matrices for all variables in our analyses and Table 4 presents the results of the tests of our hypotheses, which we discuss below.

Mentoring

Models 1-3 in Table 4 present the results of our mentoring regressions. All three models support Hypothesis 1 and are robust across models. In our probit regressions, the average marginal effect of task-specific skills on a manager's probability of joining a team-managed fund is 1.6% and is statistically significant. Since in any given period the average predicted probability of joining a team fund is 2.18%, with only 12.3% of managers in our sample ever joining a team-managed fund, this represents a sizable magnitude over the baseline.

Risk-taking

Models 4-6 in Table 4 support Hypothesis 2: managers with task-specific skills are more likely to launch new funds than managers with firm-specific skills. From our probit regressions, the average marginal effect of task-specific (versus firm-specific) skills on the probability of starting new funds is 1.8% ($p<0.01$) relative to the average predicted probability of 2.03%. Moreover, only 13.4% of managers in our sample ever start a new fund. Therefore, this result again represents a sizable increase over the baseline.

Spillovers

Finally, Models 7-10 in Table 4 present the analysis of spillovers. Model 7 estimates the impact of skill type on generating spillovers to the fund family. In contrast with our expectation in Hypothesis 3, we find no statistically significant effect of our Task-Specific Skill dummy. The insignificant effect may in part be explained by the nature of our skill type proxies – our measure of Firm-Specific Skills captures a large proportion of total fund performance and may therefore overwhelm the effect of Task-Specific Skills on total inflows. However, existing theory and evidence also predict that past fund performance will trigger inflows into other funds in the family (Massa 2003). We, therefore, decided to also examine whether fund manager skills explain the chances of being a star fund, and whether stardom helps explain fund inflows. We code star performance as a binary variable taking the value of 1 when the fund falls in the top performing 5% of all funds in a quarter and 0 otherwise. We then regress star fund performance on the task-specific skill dummy and controls (see model 8). We find that managers with task-specific skills are significantly more likely to manage a star fund, with the average marginal probability increasing by about 1.6% (the average predicted probability in each period is 2.18%). Next, we regress firm inflows (excluding focal fund inflows) on one-quarter lagged star-fund performance and the same set of controls. We expect star performance to induce significant new firm inflows, and our results confirm that prediction. Model 9 shows that star performance of a fund adds, on average, an extra $1.7 billion of inflows to the firm, and the effect nearly doubles when we control for manager skill type. These are large effects considering the median firm size in our sample is about $6 billion (75th percentile: $32 billion). Model 10 re-estimates model 9 with the task-specific skill dummy. We observe an even larger impact of star funds on firm inflows here. We interpret this result as providing evidence for an indirect effect of task-specific skills on new firm inflows. It is important to acknowledge that we are simply documenting a correlation from task-specific skills to managing a star fund to fund inflows. We are unable to tease out any causal relationship between task-specific skills and managing a star fund.

< Insert Table 4 about here >

Robustness tests

Team management. Because we cannot observe mentorship directly and managers may be assigned to teams for other reasons, we performed several tests to rule out alternative explanations for the correlation between team management and task-specific skills. The first concern is that managers with task-specific skills join teams with (experienced) managers who do not require mentoring. If so, we should see no relation between task-specific skills and joining team funds with new-to-the-industry managers (i.e. have not managed a fund for any firm). To test this, we repeat our mentoring tests, but restrict our analysis to teams where at least one member is new to the industry. In our sample, fully 68% of cases where managers join team funds contain at least one such new manager. We run three models: a limited probability model, a probit and a logit, all with controls listed in the footnote to Table 5, firm, Lipper category and time dummies. All models show similar results, but due to space constraints we only report the results from the limited probability model in Column 6 of Table 5. We find that managers with task-specific skills are more likely to join teams with members that are new to the industry.

A second concern is the possibility that rookie managers fail to benefit from the mentorship they receive, which would suggest that the practice is either wasteful or, more worryingly, is not conducted to encourage mentorship, but to provide leverage to experienced managers. To test for this possibility, we examine how rookie managers benefit from having prior experience on team-managed funds. If teams are used for mentorship, we should see that team-management experience at the start of a manager's career results in an improvement in the manager's performance in subsequent funds. Further, if team management is used primarily to train new managers in task-specific skills, we should also see a correlation between early team experience and the manager's performance on task-specific skills. To test these expectations, we create a binary variable taking the value of 1 if the manager was part of a team-managed fund in the first year of her appearance in the industry (the first time she managed a fund) and zero otherwise. We then regress manager-level measures of total quarterly performance, task- and firm-specific skills on the Team Experience dummy and a set of controls (listed in the footnote to Table 5) with firm, Lipper category and time effects. The results, in Columns 7-9 in Table 5, show that team experience in the first year of the manager's career is positively associated with future returns across the funds managed by the manager. When we break skills down into task-and firm-specific sub-components, we see that team experience is associated with higher task-, but not firm-specific skills.

A final concern with our measure of mentoring is the possibility that managers are placed on team-managed funds not because they make good mentors, but because they are the least-valued managers in the firm[22]. This is plausible given that team-management dilutes the reputational benefits accruing to any one

[22] Note that our mentorship regressions with continuous skill measures (see Appendix D of the Online Supplement) already show that increases in manager performance increase rather than decrease the chances of joining a team-managed fund. Additionally, because our mentorship regressions only involve managers who are or were in the past year, managing a solo fund in addition to the team fund that they now join, team-management typically boosts rather than diminishes their assets under management.

manager in the team and reduces their bargaining power. We can infer the value of a manager to a firm with the total assets under her management. We therefore regress the natural log of total net assets under a manager's control[23] on the number of team-managed funds a manager joins along with manager fixed effects and a set of controls listed in the footnote to the table. Columns 1-5 in Table 5 include the results. Column 1 shows that each additional team fund a manager joins boosts her net assets by about 11.9%. We then test whether the effect holds for managers with both task- and firm-specific skills, using three types of measures: the Task-Specific Skill Dummy, the Career-Long measure of both skills (omitted for brevity) and the four-quarter average of each skill. We find in Columns 2-5 that while the interactions of Team Funds with task-specific skills are insignificant, the interactions with firm-specific skills are negative and significant for both skill measures, suggesting that while increases in task-specific skills (controlling for firm-specific skills) are not associated with manager assets, increases in firm-specific skills are associated with lower assets.

<u>Risk-Taking</u>. One concern with our risk-taking results may be that while managers with task-specific skills are easier to incentivize to pursue new fund launches, the total value generated from such activities for the rest of the organization is negative. To test this possibility, we aggregate our data to the firm-level. We then test the relation between the number of (new) funds that a firm launched in the previous quarter or year and the net New Cash Inflows (net of the new funds) and net changes in Quarterly TNA of the firm, controlling for firm-level covariates listed in Table 5, time and firm dummies. Columns 10-13 in Table 5 show the results. Across all models we see that additional (new) funds result in positive and significant increases in new cash inflows and TNA for the firm. The magnitudes are also sizable. Every additional new fund adds between 23 and 36 basis points (Columns 11) to New Cash Inflows to the firm net of the new fund, though only fourth-quarter effects are significant, and 2.15% to firm TNA in the first quarter after the launch (Column 13). The effects on firm TNA are short-lived, turning insignificant by year-end, suggesting that new cash inflows are offsetting lower performance across funds – a common occurrence in fund companies aiming to grow their assets, albeit at the cost of individual fund performance (Siggelkow 2003).

< Insert Table 5 about here >

[23] We split team-fund assets equally among the managers, only counting each manager's fraction in her total.

Discussion

This paper offers an initial answer to the puzzle of why firms continue to pursue workers with transferable human capital even though they appear to fully appropriate the surplus they generate. In explaining this puzzle, we offer a theory rooted in unmeasured externalities at the firm level that are not reflected in the task performance of individual employees. We submit that while employees with transferable skills appropriate the surplus they create at the task level, they create positive externalities at the firm level. We find that in the context of the US mutual-fund industry, while managers with task-specific skills capture a greater share of surplus than managers with firm-specific skills, we also find evidence that they are more likely to be used in mentoring roles and risky activities such as starting new funds. The use of managers with task-specific skills to train the next generation of managers creates an externality because such mentoring activity at the firm level is unmeasured at the fund level. Similarly, starting new funds is a risky activity because of the high failure rate of funds in their first three years and because the costs of failure are censored and not reflected in the performance of surviving funds (Elton et al. 1996b). Finally, we find indirect evidence that task-specific skills generate demand-side spillovers for the fund company. Together, these findings provide convergent evidence that fund-manager over-appropriation at the fund level may be offset by positive externalities at the firm level. Several implications are worth noting.

First, the wedge between fund-level and fund-company-level returns demands a reconciliation of the accounting of surplus. Our results imply that fund managers over-appropriate from investors in mutual funds but the beneficiaries are the investors in the fund management companies. In the aggregate, this means that there is a transfer of wealth from mutual fund investors to investors in mutual fund management companies. This raises the puzzle of why mutual fund investors are systematically more gullible than investors in fund management companies. Unfortunately, we don't have an answer to this question. Finance scholars have sought to examine this issue with a variety of conclusions ranging from investor myopia in focusing on gross rather than net returns after expenses (Gruber 1996) to aggregate informational benefits (Berk and Green 2004). That said, our finding supports the intuition for a famous quote by Paul Samuelson (Nobel laureate in economics in 1970) in a congressional testimony in 1967: *"I decided that there was only one place to make money in the mutual fund business, as there is only one place for a temperate man to be in a* *saloon: behind the bar and not in front of it. It made sense to invest in mutual fund companies, but not in mutual funds"* (Samuelson 1967). Consistent with this, we find (see Figure OS5 in the Online Supplement Appendix E) that publicly traded mutual fund companies have higher management fees compared with private fund companies. This is because public fund companies separate the interests of mutual fund investors from the fund company investors. In contrast, in private companies (e.g., mutuals), investors in the funds are also the owners in the fund company and we observe less transfer of wealth.

Second, our study confirms evidence on human-capital externalities. While there is little research measuring externalities at a micro-level, aggregate estimates suggest that human-capital externalities are at least three times as large as the private returns to human capital (Acemoglu and Angrist 2001: 11). This may be one reason why seemingly outsized compensation packages continue to be offered in human capital-intensive professions such as sports, movies and finance. Unfortunately, we are unable to quantify the dollar value of externalities that fund managers create and the share of it that the company retains. We show that managers who capture the most surplus also generate externalities (mentoring, risk-taking and spillovers) at the firm level, that remain unmeasured at the fund level. However, more direct evidence from other settings is needed to quantify the size and sharing arrangements of such externalities.

Third, our findings suggest that firms can benefit at least as much, if not more, from improving value capture from human-capital externalities as from improving employee performance. Extant research on human-capital focuses more on raising employee output than their externalities in the firm (Almeida and Carneiro 2009; Blundell et al. 1999). Yet, externalities may comprise a larger share of firm productivity and may be more easily appropriable by the firm. Finally, there is the important issue of the generalizability of our findings on externalities. We believe that externalities are not specific to mutual funds. In consulting and law firms, for example, a key task for partners is business development, where success is measurable with revenue growth, but effort costs of failed pitches are censored. While more research is needed, there is no a priori reason to expect that the mutual fund setting is idiosyncratic.

In sum, this paper offers an initial answer to the question of why firms continue to pursue transferable human capital despite its costs. The paper, however, is not without limitations. First, it is based on one industry with a set of idiosyncratic characteristics, including visibility of individual performance, high performance uncertainty, and high task measurability. Second, we are unable to directly document surplus transfer from investors in mutual funds to investors in fund companies. However, assuming that activities such as mentoring and risk-taking are not costless, the greater use of managers with task-specific skills suggests that they also bear some of the unmeasured costs. Nevertheless, our study is best considered a plausible rather than a conclusive account. Third, our decomposition of fund-manager returns into task- and firm-specific skills remains primarily empirical, leaving the possibility that flaws in our instruments may cause mismeasurement of the two types of skills. Finally, we infer mentorship from the team management of funds and therefore cannot directly observe that managers with task-specific skills are mentoring those without. Nonetheless, our qualitative interviews support our claim that mentorship is indeed occurring in team-based funds, and we are able to rule out several alternative explanations for why managers with task-specific skills may be teamed with managers without such skills. Moreover, while we are unable to provide affirmative evidence for mentoring, the claims in the paper do not hinge solely on the mentoring mechanism. Mentoring is just one example of positive externalities accruing from managers with task-specific skills. There may be other externalities in addition to the three that we have considered. In sum, we hope that this paper spurs further research to deepen our understanding both of the sources of human-capital externalities and how they affect surplus appropriation.

References

Acemoglu, D., J. Angrist. 2001. How large are human-capital externalities? Evidence from compulsory-schooling laws *Nber macroeconomics annual 2000, volume 15*. MIT Press, 9-74.
Acemoglu, D., J.S. Pischke. 1998. Why do firms train? Theory and evidence. *The Quarterly Journal of Economics* **113**(1) 79-119.
Acemoglu, D., J.S. Pischke. 1999. The structure of wages and investment in general training. *Journal of Political Economy* **107**(3) 539-572.
Agrawal, A., J. McHale, A. Oettl. 2017. How stars matter: Recruiting and peer effects in evolutionary biology. *Research Policy* **46**(4) 853-867.
Aguirre, D.M., P.-O. Karlsson, G.L. Neilson. 2016. 2015 ceo success study. Strategy&.
Almazan, A., K.C. Brown, M. Carlson, D.A. Chapman. 2004. Why constrain your mutual fund manager? *Journal of Financial Economics* **73**(2) 289-321.
Almeida, R., P. Carneiro. 2009. The return to firm investments in human capital. *Labour Economics* **16**(1) 97-106.
Argote, L., S.L. Beckman, D. Epple. 1990. The persistence and transfer of learning in industrial settings. *Management Science* **36**(2) 140-154.
Arthur, M.B., D.M. Rousseau. 1996. *The boundaryless career: A new employment principle for a new organizational era.* Oxford University Press.
Autor, D.H. 2001. Why do temporary help firms provide free general skills training? *The Quarterly Journal of Economics* **116**(4) 1409-1448.
Avramov, D., R. Wermers. 2006. Investing in mutual funds when returns are predictable. *Journal of Financial Economics* **81**(2) 339-377.
Baker, G., M. Gibbs, B. Holmstrom. 1994. The internal economics of the firm: Evidence from personnel data. *The Quarterly Journal of Economics* **109**(4) 881-919.
Baker, G.P. 1992. Incentive contracts and performance measurement. *Journal of Political Economy* **100**(3) 598-614.
Barley, S.R., G. Kunda. 2006. Contracting: A new form of professional practice. *The Academy of Management Perspectives* **20**(1) 45-66.
Barney, J. 1991. Firm resources and sustained competitive advantage. *Journal of Management* **17**(1) 99-120.
Barron, J.M., M.C. Berger, D.A. Black. 1999. Do workers pay for on-the-job training? *The Journal of Human Resources* **34**(2) 235-252.
Bartel, A.P., N.D. Beaulieu, C.S. Phibbs, P.W. Stone. 2014. Human capital and productivity in a team environment: Evidence from the healthcare sector. *American Economic Journal: Applied Economics* **6**(2) 231-259.
Battu, H., C.R. Belfield, P.J. Sloane. 2003. Human capital spillovers within the workplace: Evidence for great britain. *Oxford Bulletin of Economics and Statistics* **65**(5) 575-594.
Becker, B., B. Gerhart. 1996. The impact of human resource management on organizational performance: Progress and prospects. *The Academy of Management Journal* **39**(4) 779-801.
Becker, G.S. 1962. Investment in human capital: A theoretical analysis. *The Journal of Political Economy* **70**(5) 9-49.
Becker, G.S. 1964. *Human capital: A theoretical and empirical analysis with special reference to education. 3rd ed.* The University of Chicago Press, Chicago.
Benkard, C.L. 2000. Learning and forgetting: The dynamics of aircraft production. *American Economic Review* **90**(4) 1034-1054.
Berk, Jonathan B., Richard C. Green. 2004. Mutual fund flows and performance in rational markets. *Journal of Political Economy* **112**(6) 1269-1295.
Bessen, J. 2015. *Learning by doing: The real connection between innovation, wages, and wealth*. Yale University Press, New Haven and London.
Bidwell, M., J. Keller. 2014. Within or without? How firms combine internal and external labor markets to fill jobs. *Academy Of Management Journal* **57**(4) 1035-1055.
Bidwell, M.J. 2013. What happened to long-term employment? The role of worker power and environmental turbulence in explaining declines in worker tenure. *Organization Science* **24**(4) 1061-1082.
Blundell, R., L. Dearden, C. Meghir, B. Sianesi. 1999. Human capital investment: The returns from education and training to the individual, the firm and the economy. *Fiscal Studies* **20**(1) 1-23.
Bogle, J.C. 2005. The mutual fund industry 60 years later: For better or worse? *Financial Analysts Journal* **61**(1) 15-24.
Boh, W.F., S.A. Slaughter, J.A. Espinosa. 2007. Learning from experience in software development: A multilevel analysis. *Management Science* **53**(8) 1315-1331.

Boning, B., C. Ichniowski, K. Shaw. 2007. Opportunity counts: Teams and the effectiveness of production incentives. *Journal of Labor Economics* **25**(4) 613-650.
Booth, A.L. 1993. Private sector training and graduate earnings. *Review of Economics & Statistics* **75** 164-170.
Brummet, R.L., E.G. Flamholtz, W.C. Pyle. 1968. Human resource measurement - a challenge for accountants. *The Accounting Review* **43**(2) 217-224.
Cahuc, P., F. Postel-Vinay, J.-M. Robin. 2006. Wage bargaining with on-the-job search: Theory and evidence. *Econometrica* **74**(2) 323-364.
Chan, T.Y., J. Li, L. Pierce. 2014. Learning from peers: Knowledge transfer and sales force productivity growth. *Marketing Science* **33**(4) 463-484.
Clark, J.B. 1899. *The distribution of wealth: A theory of wages, interest and profit*. The Macmillan Company, New York.
Coff, R., D. Kryscynski. 2011. Invited editorial: Drilling for micro-foundations of human capital–based competitive advantages. *Journal of Management* **37**(5) 1429-1443.
Coff, R., J. Raffiee. 2015. Toward a theory of perceived firm-specific human capital. *The Academy of Management Perspectives* **29**(3) 326-341.
Conti, R. 2014. Do non-competition agreements lead firms to pursue risky r&d projects? *Strategic Management Journal* **35**(8) 1230-1248.
Covaleski, M.A., M.W. Dirsmith, J.B. Heian, S. Samuel. 1998. The calculated and the avowed: Techniques of discipline and struggles over identity in big six public accounting firms. *Administrative Science Quarterly* 293-327.
Crook, T.R., S.Y. Todd, J.G. Combs, D.J. Woehr, D.J. Ketchen, Jr. 2011. Does human capital matter? A meta-analysis of the relationship between human capital and firm performance. *Journal of Applied Psychology* **96**(3) 443-456.
Daniel, K., M. Grinblatt, S. Titman, R. Wermers. 1997. Measuring mutual fund performance with characteristic-based benchmarks. *The Journal of Finance* **52**(3) 1035-1058.
Datta, D.K., J.P. Guthrie, P.M. Wright. 2005. Human resource management and labor productivity: Does industry matter? *Academy Of Management Journal* **48**(1) 135-145.
Davis, J.P., K.M. Eisenhardt. 2011. Rotating leadership and collaborative innovation. *Administrative Science Quarterly* **56**(2) 159-201.
de Grip, A., J. Sauermann, I. Sieben. 2016. The role of peers in estimating tenure-performance profiles: Evidence from personnel data. *Journal of Economic Behavior & Organization* **126**(PA) 39-54.
Di Stefano, G., A.A. King, G. Verona. 2014. Kitchen confidential? Norms for the use of transferred knowledge in gourmet cuisine. *Strategic Management Journal* **35**(11) 1645-1670.
Diekmann, A., B. Jann, W. Przepiorka, S. Wehrli. 2013. Reputation formation and the evolution of cooperation in anonymous online markets. *American Sociological Review* **79**(1) 65-85.
Doeringer, P., M. Piore. 1971. *Internal labor markets and manpower analysis*. Heath, Lexington, MA.
Edmondson, A.C., R.M. Bohmer, G.P. Pisano. 2001. Disrupted routines: Team learning and new technology implementation in hospitals. *Administrative Science Quarterly* **46**(4) 685-716.
Edmondson, A.C., A.B. Winslow, R.M.J. Bohmer, G.P. Pisano. 2003. Learning how and learning what: Effects of tacit and codified knowledge on performance improvement following technology adoption. *Decision Sciences* **34**(2) 197-224.
Elton, E.J., M.J. Gruber, C.R. Blake. 1996a. The persistence of risk-adjusted mutual fund performance. *Journal of Business* 133-157.
Elton, E.J., M.J. Gruber, C.R. Blake. 1996b. Survivor bias and mutual fund performance. *Review of Financial Studies* **9**(4) 1097-1120.
Ethiraj, S.K., P. Garg. 2012. Division of gains from complementarities in human capital-intensive activity. *Organization Science* **23**(3) 725-742.
Fabozzi, F.J., H.M. Markowitz. 2011. *Equity valuation and portfolio management*. Wiley.
Fang, R., M.K. Duffy, J.D. Shaw. 2011. The organizational socialization process: Review and development of a social capital model. *Journal of Management* **37**(1) 127-152.
Feenberg, D.R., E. Coutts. 1993. An introduction to the taxsim model. *Journal of Policy Analysis and Management* **12**(1) 189-194.
Ferreira, M.A., A. Keswani, A.F. Miguel, S.B. Ramos. 2013. The determinants of mutual fund performance: A cross-country study. *Review of Finance* **17**(2) 483-525.
Foster, A.D., M.R. Rosenzweig. 1995. Learning by doing and learning from others: Human capital and technical change in agriculture. *Journal of Political Economy* **103**(6) 1176-1209.
Ganco, M., R.H. Ziedonis, R. Agarwal. 2015. More stars stay, but the brightest ones still leave: Job hopping in the shadow of patent enforcement. *Strategic Management Journal* **36**(5) 659-685.
Gathmann, C., U. Schönberg. 2010. How general is human capital? A task-based approach. *Journal of Labor Economics* **28**(1) 1-49.

Gibbons, R., M. Waldman. 2004. Task-specific human capital. *The American Economic Review* **94**(2) 203-207.
Gibbons, R., M. Waldman. 2006. Enriching a theory of wage and promotion dynamics inside firms. *Journal of Labor Economics* **24**(1) 59-107.
Gompers, P., A. Kovner, J. Lerner, D. Scharfstein. 2010. Performance persistence in entrepreneurship. *Journal of Financial Economics* **96**(1) 18-32.
Groysberg, B. 2010. *Chasing stars: The myth of talent and the portability of performance*. Princeton University Press, Princeton, NJ.
Groysberg, B., L.-E. Lee, A. Nanda. 2008. Can they take it with them? The portability of star knowledge workers' performance. *Management Science* **54**(7) 1213-1230.
Gruber, M.J. 1996. Another puzzle: The growth in actively managed mutual funds. *The Journal of Finance* **51**(3) 783-810.
Hagiu, A. 2009. Two-sided platforms: Product variety and pricing structures. *Journal of Economics & Management Strategy* **18**(4) 1011-1043.
Hamilton, B., J. Nickerson, H. Owan. 2003. Team incentives and worker heterogeneity: An empirical analysis of the impact of teams on productivity and participation. *Journal of Political Economy* **111**(3) 465-497.
Hausman, J., G.K. Leonard. 1997. Superstars in the national basketball association: Economic value and policy. *Journal of Labor Economics* **15**(4) 586-624.
Hitt, M.A., L. Biermant, K. Shimizu, R. Kochhar. 2001. Direct and moderating effects of human capital on strategy and performance in professional service firms: A resource-based perspective. *Academy Of Management Journal* **44**(1) 13-28.
Holmström, B. 1999. Managerial incentive problems: A dynamic perspective. *The Review of Economic Studies* **66**(1) 169-182.
Hölmstrom, B., P. Milgrom. 1991. Multitask principal agent analyses: Incentive contracts, asset ownership, and job design. *Journal of Law Economics & Organization* **7** 24-52.
Ichniowski, C., K. Shaw. 2003. Beyond incentive pay: Insiders' estimates of the value of complementary human resource management practices. *Journal of Economic Perspectives* **17**(1) 155-180.
Ichniowski, C., K. Shaw, G. Prennushi. 1997. The effects of human resource management practices on productivity: A study of steel finishing lines. *The American Economic Review* **87**(3) 291-313.
Jackson, C.K., E. Bruegmann. 2009. Teaching students and teaching each other: The importance of peer learning for teachers. *American Economic Journal: Applied Economics* **1**(4) 85–108.
Kambourov, G., I. Manovskii. 2009. Occupational specificity of human capital. *International Economic Review* **50**(1) 63-115.
Kaplan, R.S. 1992. *The evolution of management accounting*. Springer.
Khorana, A. 1996. Top management turnover: An empirical investigation of mutual fund managers. *Journal of Financial Economics* **40** 403-427.
Khorana, A. 2001. Performance changes following top management turnover: Evidence from open-end mutual funds. *Journal of Financial and Quantitative Analysis* **36**(3) 371-393.
Kleven, H.J., C. Landais, E. Saez, E. Schultz. 2013. Migration and wage effects of taxing top earners: Evidence from the foreigners' tax scheme in denmark. *The Quarterly Journal of Economics*.
Levitt, S.D., J.A. List, C. Syverson. 2013. Toward an understanding of learning by doing: Evidence from an automobile assembly plant. *Journal of Political Economy* **121**(4) 643-681.
Lillard, L.A., H.W. Tan. 1993. Private sector training: Who gets it and what are its effects? *Research in Labor Economics* **13**(1-62).
Luft, J.L., R. Libby. 1997. Profit comparisons, market prices and managers' judgments about negotiated transfer prices. *Accounting Review* 217-229.
Marx, M., D. Strumsky, L. Fleming. 2009. Mobility, skills, and the michigan non-compete experiment. *Management Science* **55**(6) 875-889.
Massa, M. 2003. How do family strategies affect fund performance? When performance-maximization is not the only game in town. *Journal of Financial Economics* **67**(2) 249-304.
Massa, M., J. Reuter, E. Zitzewitz. 2010. When should firms share credit with employees? Evidence from anonymously managed mutual funds. *Journal of Financial Economics* **95**(3) 400-424.
Mayer, K.J. 2006. Spillovers and governance: An analysis of knowledge and reputational spillovers in information technology. *Academy Of Management Journal* **49**(1) 69-84.
McGrath, R.G. 2001. Exploratory learning, innovative capacity and managerial oversight. *The Academy of Management Journal* **44**(1) 118-131.
Moretti, E. 2004. Workers' education, spillovers, and productivity: Evidence from plant-level production functions. *The American Economic Review* **94**(3) 656-690.
Morrison, A.D., W.J. Wilhelm. 2004. Partnership firms, reputation, and human capital. *The American Economic Review* **94**(5) 1682-1692.

Nanda, V., Z.J. Wang, L. Zheng. 2004. Family values and the star phenomenon: Strategies of mutual fund families. *The Review of Financial Studies* **17**(3) 667-698.
Neal, D. 1995. Industry-specific human capital: Evidence from displaced workers. *Journal of Labor Economics* **13**(4) 653-677.
Nickerson, J.A., B.H. Hamilton, T. Wada. 2001. Market position, resource profile, and governance: Linking porter and williamson in the context of international courier and small package services in japan. *Strategic Management Journal* **22**(3) 251-273.
Prendergast, C. 1993. A theory of 'yes men'. *The American Economic Review* **83**(4).
Prendergast, C. 2002. Uncertainty and incentives. *Journal of Labor Economics* **20**(2) S115-S137.
Romer, P.M. 1990. Endogenous technological change. *Journal of Political Economy* **98**(5) S71-S102.
Samuelson, P.A. 1967. Hearings before senate committee on banking and currency *Session 1659, 90th Congress*, 1659 ed, Washington, DC.
Shapiro, C. 1983. Premiums for high quality products as returns to reputations. *The Quarterly Journal of Economics* **98**(4) 659-680.
Siggelkow, N. 2003. Why focus? A study of intra-industry focus effects. *Journal of Industrial Economics* **51**(2) 121-150.
Simon, H.A. 1947. *Administrative behavior: A study of decision-making processes in administrative organization*. Free Press, New York.
Song, H., A. Tucker, K. Murrell, D. Vinson. 2015. Public relative performance feedback in complex service systems: Improving productivity through the adoption of best practices. *HBS Working Paper*. Harvard Business School, Boston.
Stock, J.H., M. Yogo. 2005. Testing for weak instruments in linear iv regressions. D.W.K. Andrews, J.H. Stock, eds. *Identification and inference for econometric models: Essays in honor of thomas rothenberg*. Cambridge University Press, New York, 80-108.
Syverson, C. 2011. What determines productivity? *Journal of Economic Literature* **49**(2) 326-365.
Topel, R. 1991. Specific capital, mobility, and wages: Wages rise with job seniority. *Journal of Political Economy* **99**(1) 145-176.
Wermers, R. 2000. Mutual fund performance: An empirical decomposition into stock picking talent, style, transactions costs, and expenses. *The Journal of Finance* **55**(4) 1655-1703.
Wermers, R. 2003. Is money really ‘smart’? New evidence on the relation between mutual fund flows, manager behavior, and performance persistence. *Working paper, University of Maryland*.
Wexley, K.N., G.P. Latham. 1991. *Developing human resources in organizations*. Pearson.
Williamson, O.E., M.L. Wachter, J.E. Harris. 1975. Understanding the employment relation: The analysis of idiosyncratic exchange. *The Bell Journal of Economics* **6**(1) 250-278.
Winter, S.G., G. Szulanski. 2001. Replication as strategy. *Organization Science* **12**(6) 730-743.
Wooldridge, J.M. 2002. *Econometric analysis of panel data*. MIT Press, Cambridge, MA.
Wooldridge, J.M. 2015. Control function methods in applied econometrics. *Journal of Human Resources* **50**(2) 420-445.
Young, C., C. Varner. 2011. Millionaire migration and state taxation of top incomes: Evidence from a natural experiment. *National Tax Journal* **64** 255–284.

Table 1 – Definitions of Control Variables

Variable	***Periodicity***	***Measurement & Description***
Cash	*Quarterly*	Cash holdings expressed as a percent of the quarterly total net assets of the fund.
Diversification	*Quarterly*	The number of distinct Lipper categories represented in the focal firm.
Expense Ratio	*Quarterly*	The sum of management, distribution, and administration fees, expressed as a fraction of quarterly total net assets of the fund.
Gender	*N/A*	A dummy variable containing the value of 1 if the manager in question is female and 0 otherwise. For more details on how this variable was coded, please see the Online Supplement, Appendix A2.
Firm Age	*Quarterly*	Number of quarters since the firm first appears in CRSP.
Firm Marketing Expense	*Quarterly*	Mean marketing expenditure across all funds in the firm.
Firm Performance	*Quarterly*	The average performance of all funds belonging to the same firm across the past four quarters expressed in percent of total net assets.
Firm Size	*Quarterly*	Total net assets across all funds in the firm.
Firm Turnover	*Quarterly*	Average tenure in the firm adjusted for firm age.
Firm Age	*Quarterly*	The number of quarters since the first fund in the firm was offered, as reported by CRSP.
Fund Age	*Quarterly*	The number of quarters since the fund was first offered, as reported by CRSP.
Front Load	*Quarterly*	Since each share class within the fund can charge a different load (including no load), we measure the presence of a front load using a binary variable, taking the value of 1 if one or more of the fund's share classes charge a front load and 0 otherwise.
Rear Load	*Quarterly*	Since each share class within the fund can charge a different load (including no load), we measure the presence of a rear load using a binary variable, taking the value of 1 if one or more of the fund's share classes charge a rear load and 0 otherwise.
Manager Tenure	*Quarterly*	The number of quarters since the manager took over the fund, as reported by CRSP. With multiple managers, the tenure of the longest tenured manager is used.
Marketing Expense	*Quarterly*	The maximum 12b1 fee set by the fund in its prospectus.
Negative Surplus	*Quarterly*	A binary variable taking the value of 1 if the surplus is negative in the current quarter and 0 otherwise.
No. of Funds per Manager	*Quarterly*	Number of funds the manager is reported to be managing.
Number of Stocks Held	*Quarterly*	The number of stocks held by the fund in the current quarter.
Portfolio Risk	*Quarterly*	Standard deviation of the portfolio's performance calculated over the previous 180 days from daily fund returns data.
Sub-advised	*Quarterly*	A binary variable taking the value of 1 if the fund is sub-advised by an external advisor and 0 otherwise.
Surplus	*Quarterly*	The total quarterly return for the fund calculated as a percentage return on the stocks held multiplied by the size of the fund (in millions of dollars).
Total Net Assets (TNA)	*Quarterly*	Assets under management are calculated by multiplying the number of stocks held at the end of each quarter by their prices as of the last trading day of that quarter. We use two types of controls: a set of five dummy variables, one for each quintile, and natural log of fund TNA.
Trade Volume	*Quarterly*	The number of stocks traded each quarter. Calculated as the sum of purchased and sold stocks in the current quarter as compared to the previous quarter.

Table 2 – Fund- and Manager-level summary statistics for Skill Identification & Main Results

Panel A: Fund-Level Sample		Mean	S.D.	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1)	Gross Return	2.644	12.124	-64.640	108.446	1.000											
(2)	Characteristic Selectivity	0.568	4.140	-28.951	61.006	0.456	1.000										
(3)	Characteristic Timing	-0.018	1.806	-18.099	22.773	0.123	0.067	1.000									
(4)	Average Style	1.037	5.917	-27.113	27.077	0.791	0.030	-0.105	1.000								
(5)	Firm Spec. Skill	0.992	5.852	-25.038	29.568	0.811	0.048	0.196	0.952	1.000							
(6)	Ext. Mgr Chg 1Q	0.016	0.124	0.000	1.000	-0.003	0.002	0.005	-0.011	-0.010	1.000						
(7)	Int. Mgr Chg 1Q	0.023	0.148	0.000	1.000	-0.017	-0.007	-0.005	-0.019	-0.019	-0.019	1.000					
(8)	Focal State Tax (lag)	5.747	2.237	0.000	10.920	0.000	-0.020	0.000	0.013	0.013	0.000	-0.018	1.000				
(9)	Avg. Out. State Tax (lag)	5.369	0.082	5.143	5.639	0.018	-0.004	0.023	0.004	0.010	-0.004	0.012	-0.657	1.000			
(10)	Mgr Tenure	28.321	21.926	1.000	208.000	-0.023	-0.008	-0.007	-0.012	-0.013	-0.117	-0.052	-0.007	0.007	1.000		
(11)	TNA	2.184	1.394	0.000	4.000	0.003	0.014	-0.003	0.004	0.004	-0.015	0.017	0.015	0.000	0.063	1.000	
(12)	Expense Rat.	0.013	0.005	0.001	0.097	0.026	0.018	-0.001	0.012	0.011	0.011	-0.012	0.105	-0.066	-0.046	-0.366	1.000

This table is based on a sample of 16,416 observations. Correlations larger in absolute value than 0.01 are significant at the 5% level.

Panel B: Manager-Level Sample		Mean	S.D.	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(1)	Mentoring	0.012	0.109	0.000	1.000	1													
(2)	Risk taking	0.021	0.143	0.000	1.000	0.127	1												
(3)	Spillovers (excl. focal mgr, $mil)	-164.0	6919.5	-0.001	34357	-0.013	0.000	1											
(4)	Star Fund	0.016	0.125	0.000	1.000	0.031	0.018	0.001	1										
(5)	Task-Specific Skill Dummy	0.505	0.500	0.000	1.000	0.003	0.020	0.013	0.031	1									
(6)	Firm Performance (%)	1.874	10.015	-93.238	98.238	0.013	0.026	-0.155	0.070	0.003	1								
(7)	Firm TNA ($mil)	58155	135000	0.026	624000	-0.012	-0.028	-0.075	0.044	-0.099	-0.003	1							
(8)	Firm Mrktng Exp. (% TNA)	0.050	0.057	0.000	0.296	0.017	-0.021	-0.104	0.035	-0.083	-0.018	0.708	1						
(9)	Diversification	7.387	7.121	1.000	33.000	0.026	-0.035	-0.095	0.040	-0.065	-0.062	0.773	0.759	1					
(10)	Sub-advised	0.088	0.388	0.000	8.000	0.048	-0.014	0.007	0.003	0.017	-0.003	-0.064	-0.062	-0.008	1				
(11)	Portfolio Risk	1.333	0.517	0.003	6.474	0.000	-0.001	-0.009	0.071	0.192	-0.067	0.046	0.049	0.065	-0.037	1			
(12)	Manager Tenure	24.046	21.331	-23.000	208.000	0.016	-0.073	0.000	0.002	-0.120	-0.029	-0.034	-0.050	-0.011	0.017	-0.092	1		
(13)	Gender	0.083	0.276	0.000	1.000	0.007	0.005	-0.001	-0.010	0.004	0.004	0.018	0.044	0.038	0.020	-0.002	-0.057	1	
(14)	No. Funds	1.338	0.983	1.000	27.000	0.038	0.043	-0.009	0.164	0.009	-0.006	0.050	0.059	0.092	0.065	0.066	0.093	0.011	1

This table is based on a sample of 9,293 observations. Correlations larger in absolute value than 0.019 are significant at the 5% level.

Table 3 - Skill Identification Table

		OLS					Residual Inclusion				
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Gross Return	Characteristic Selectivity	Characteristic Timing	Average Style	Firm Spec. Skill	Gross Return	Characteristic Selectivity	Characteristic Timing	Average Style	Firm Spec. Skill
Panel 1 (1Q)	New External Manager (1Q)	1.621*	0.00366	0.156	0.301	0.414	-12.01***	0.582	-0.406	-7.359***	-7.510***
		(0.657)	(0.237)	(0.117)	(0.326)	(0.333)	(1.669)	(0.588)	(0.302)	(0.882)	(0.881)
	New Internal Manager (1Q)	1.397**	0.192	-0.0758	0.524*	0.446*	0.287	0.270	-0.0442	-0.512	-0.606
		(0.423)	(0.151)	(0.0839)	(0.216)	(0.225)	(1.486)	(0.448)	(0.185)	(0.788)	(0.777)
	F- or Chi2-Test: External = Internal	0.0843	0.440	2.667	0.331	0.00639	22.52***	0.156	1.012	23.17***	25.49***
Panel 2 (2Q)	New External Manager (2Q)	-0.626	0.00602	0.173*	-0.712*	-0.519*	-5.332***	0.147	-0.0351	-3.351***	-3.271***
		(0.462)	(0.162)	(0.0714)	(0.231)	(0.225)	(0.733)	(0.279)	(0.121)	(0.386)	(0.379)
	New Internal Manager (2Q)	-0.718*	-0.00642	-0.0324	-0.424*	-0.446*	-1.835	0.0336	-0.00494	-1.454+	-1.483+
		(0.332)	(0.120)	(0.0592)	(0.172)	(0.172)	(1.451)	(0.384)	(0.163)	(0.824)	(0.799)
	F- or Chi2-Test: External = Internal	0.0265	0.00370	5.214*	1.022	0.0669	3.553+	0.0502	0.0202	3.813+	3.864*
Panel 3 (3Q)	New External Manager (3Q)	0.121	0.0105	0.0371	-0.209	-0.145	-3.607***	0.0757	-0.120	-2.238***	-2.258***
		(0.416)	(0.138)	(0.0616)	(0.201)	(0.201)	(0.627)	(0.211)	(0.0970)	(0.322)	(0.306)
	New Internal Manager (3Q)	-0.257	-0.0468	-0.0533	-0.105	-0.141	-0.862	-0.0140	-0.0285	-0.728	-0.762
		(0.295)	(0.0998)	(0.0466)	(0.158)	(0.151)	(0.952)	(0.259)	(0.112)	(0.529)	(0.533)
	F- or Chi2-Test: External = Internal	0.583	0.113	1.475	0.172	0.000292	4.048*	0.0497	0.441	4.959*	5.144*

Notes: *** Indicates significance at the 0.1% level, ** significance at the 1% level, * significance at the 5% level and + significance at the 10% level. Errors are bootstrapped and clustered at the fund level, and all regressions contain fund fixed effects. New External Manager is a dummy variable taking the value of 1 if the manager is new to the fund and new to the firm and zero otherwise. New Internal Manager is a dummy variable taking the value of 1 if the manager is new to the fund, but has managed funds in the same firm previously. The F-test (for OLS) or the Chi-Square test (for residual inclusion) statistics at the bottom of each panel tests the statistical equivalence of the coefficients on internal and external manager change. Each Panel reports the results of a separate regression with the key explanatory variables listed in the table and the same set of the following controls: fund size quintiles, stock turnover quintiles, manager tenure, fund expense ratio, fund cash reserves, fund performance volatility, front- and rear-loads, lagged average firm performance, number of funds per manager, manager gender, year and quarter dummies.

Table 4 – Main Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Mentoring	Mentoring	Mentoring	Risk taking	Risk taking	Risk taking	Spillovers	Star Fund	Spillovers	Spillovers
	LPM	Probit	Logit	LPM	Probit	Logit	OLS	Probit	OLS	OLS
Task-Specific Skill Dummy	0.005*	0.359**	0.783*	0.005**	0.444*	1.168*	-342.462	0.299**		-330.507
	(0.002)	(0.129)	(0.330)	(0.002)	(0.186)	(0.534)	(226.168)	(0.106)		(223.884)
Star Fund									1,764.376*	2,262.455+
									(761.902)	(1,269.118)
Firm Performance	0.000	0.003	0.001	0.000	0.007	0.014	-120.098***	-1.895	-164.098***	-175.448***
	(0.000)	(0.020)	(0.051)	(0.000)	(0.026)	(0.067)	(25.916)	(1.391)	(20.274)	(30.242)
Firm TNA	0.000	0.000	0.000	0.000	0.000	0.000+	0.025***	-0.000	0.023***	0.025***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)	(0.000)	(0.003)	(0.005)
Firm Marketing Expense	-0.045	-2.443	-3.782	-0.046	-2.885	-4.511	-20,766.147***	0.807	-16,572.131***	-22,872.481***
	(0.056)	(2.840)	(8.425)	(0.058)	(4.879)	(11.695)	(5,631.136)	(1.661)	(2,692.878)	(5,504.352)
Diversification	0.000	0.020	0.050	0.000	0.010	-0.019	-262.555***	0.019	-191.839***	-219.240***
	(0.001)	(0.034)	(0.087)	(0.001)	(0.063)	(0.153)	(60.593)	(0.019)	(35.579)	(60.310)
Sub-advised	0.020*	0.440**	1.068**	-0.001	-0.437	-0.964	91.743	0.018	-35.346	32.998
	(0.010)	(0.141)	(0.337)	(0.002)	(0.315)	(0.854)	(137.052)	(0.100)	(87.840)	(150.978)
Portfolio Risk	-0.001	-0.146	-0.331	-0.003	-0.238	-0.483	-755.678+	0.243*	-329.012+	-680.102+
	(0.004)	(0.213)	(0.517)	(0.003)	(0.218)	(0.552)	(396.908)	(0.098)	(178.722)	(383.449)
Manager Tenure	0.000	0.001	0.005	-0.000	-0.002	-0.002	-4.596	0.001	1.753	-3.010
	(0.000)	(0.004)	(0.010)	(0.000)	(0.006)	(0.020)	(4.823)	(0.003)	(2.468)	(4.756)
Gender	0.003	0.008	0.073	0.002	0.151	0.388	-546.313	-0.300	-95.082	-538.455
	(0.005)	(0.206)	(0.551)	(0.004)	(0.250)	(0.686)	(350.971)	(0.205)	(189.475)	(346.222)
Constant	-0.006	-2.140*	-4.515+	0.001	-1.743*	-3.493+	2,851.443**	1.056	744.934	2,473.986*
	(0.009)	(0.988)	(2.622)	(0.011)	(0.756)	(2.101)	(1,047.482)	(1.417)	(754.881)	(1,042.322)
Observations	8,086	2,987	2,987	8,086	1,961	1,961	6,028	4,829	11,619	6,018
R-squared	0.059			0.061			0.078		0.096	0.120
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Inv. Obj. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses are clustered at the manager level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

Table 5 – Robustness Tests for Main Results

Panel A	(1) Ln(Mgr TNA) OLS Mgr FE	(2) Ln(Mgr TNA) OLS Mgr FE	(3) Ln(Mgr TNA) OLS Mgr FE	(4) Ln(Mgr TNA) OLS Mgr FE	(5) Ln(Mgr TNA) OLS Mgr FE	(6) Rookie Team OLS Firm FE
Team Funds / Mgr	0.119***	0.0878**	0.148	0.107***	0.135***	
	(0.0339)	(0.0311)	(0.0995)	(0.0311)	(0.0346)	
Task-Specific Skill Dummy		0.0310	0.0379			0.005*
		(0.114)	(0.115)			(0.002)
Team Funds / Mgr * Task-Specific Skill Dummy			-0.0736			
			(0.101)			
Task-Specific Skill 4Q Avg				0.0271***	0.0273***	
				(0.00593)	(0.00599)	
Firm-Specific Skill 4Q Avg				0.0132**	0.0144**	
				(0.00496)	(0.00494)	
Task-Specific Skill 4Q Avg * Team Funds / Mgr					-0.00306	
					(0.0157)	
Firm-Specific Skill 4Q Avg * Team Funds / Mgr					-0.0136*	
					(0.00625)	
Observations	13,665	6,937	6,937	10,847	10,847	7,032

Panel B *All columns are estimated with OLS & contain firm FE*	(7) Mgr Return %	(8) Task-Specific Skill (cont.)	(9) Firm-Specific Skill (cont.)	(10) New Cash Inflows (% Firm TNA)	(11) New Cash Inflows (% Firm TNA)	(12) Qtrly TNA Chng (%)	(13) Qtrly TNA Chng (%)
Team Experience	0.526*	0.174+	0.162				
	(0.213)	(0.103)	(0.111)				
Number of Funds in Firm				0.151*		2.296***	
				(0.0599)		(0.520)	
New Funds in Firm Q1					0.226		2.151*
					(0.277)		(0.891)
New Funds in Firm Q4					0.356*		0.350
					(0.177)		(0.583)
Observations	13,564	13,225	12,469	6,603	6,603	6,577	6,577

Notes: *** Indicates significance at the 0.1% level, ** significance at the 1% level, * significance at the 5% level and + significance at the 10% level. Errors are clustered at the firm or manager level depending on the fixed effects that are used. Task-specific Skill Dummy takes the value of 1 if the manager falls in the top 50% of the distribution on task-specific skills and bottom 50% of the distribution on firm-specific skills, and zero otherwise. Task- and Firm-specific Skill Four-Quarter Average measures capture the performance of the manager on task- and firm-specific skills respectively over the past four quarters only. Team Funds per Manager is the number of team funds the manager is part of. Team Experience takes the value of 1 if the manager was in a team-managed fund in the first year of her experience in the industry and 0 otherwise. New Funds in Firm is the lagged (by one quarter (Q1) or four quarters (Q4)) number of new funds that the firm has launched. Columns 1-9 are at the manager level, while Columns 10-13 are at the firm level. All columns include controls for firm total net assets, firm performance, the number of funds a manager manages, firm marketing expense, firm diversification, the number of sub-advised funds, the performance volatility of each fund, manager tenure, manager gender, year, quarter and fund investment objectives. Regressions at the manager level also include the same controls averaged at the manager level, where appropriate.