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Choosing Investment Managers

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Abstract

Investment managers connected to plans sponsors are more likely to be hired than not-connected managers. The magnitude of the selection effect is comparable to that of prior performance. Ex post, connections do not result in higher post-hiring returns. Relationships are thus conducive to asset gathering by investment managers but do not generate commensurate pecuniary benefits for plan sponsors.

I. Introduction

At the end of 2018, the combined assets of pension funds, endowments, foundations, and sovereign wealth funds stood at approximately $37 trillion.[1] The investment of these assets is mostly delegated to external investment management firms that specialize in particular asset classes. Lakonishok, Shleifer, and Vishny (1992) argue that this delegation process is rife with conflicts of interest and generates large inefficiencies. They predict (pages 341 and 379) that the industry cannot exist as is because these inefficiencies demand change. In rebuttal,

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[1]U.S. pension fund assets are approximately $27 trillion (http://www.oecd.org/daf/fin/private-pensions/?globalpensionstatistics.htm), endowments and foundations assets are $1.5 trillion (https://www.pionline.com/?article/20180508/interactive/180509883/global-foundation-assets-reach-1-5-trillion), and sovereign wealth funds are $8.2 trillion (https://www.swfinstitute.org/fund-rankings/sovereign-wealth-fund).

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Hart (1992) and Perry (1992) argue that it is entirely possible, even likely, that agency costs and inefficiencies are part of a natural and stable equilibrium that is unlikely to change; the so-called cost of doing business. Gennaioli, Shleifer, and Vishny (2015) offer a complementary view, one in which investors tolerate inefficiencies because trust in investment managers offers countervailing benefits.

At the core of the above arguments reside issues of choice. Investment committees and external investment consultants are tasked to choose investment managers, presumably based on their expertise. We contend that understanding the nature of aforementioned equilibrium requires knowledge of the choice set and the selection mechanism: how investment managers are chosen from the set of managers who could have been chosen (the opportunity set). Examining choice provides novel economic insights in two ways. First, it sheds light on the mechanisms that generate frictions inherent in hiring. One cannot fully understand the choice mechanism solely by looking at chosen firms, because choice necessarily requires discriminating among firms in an opportunity set. For example, if a characteristic X is used to choose from a group, one can only know something about the role of X if there is variation in X between the chosen and the nonchosen. Second, outcomes from the nonchosen represent the counterfactual, which is a powerful tool to help quantify opportunity costs.[2] In our setting, this counterfactual also addresses the criticism in Berk and van Binsbergen (2015) that performance benchmarks must be investable at the time of the investment decision. Thus, looking at "what could have been" is important to understanding both choices and costs.

Relative to the extensive literature on choice pioneered by McFadden (1974), there are two key advantages to examining the choice mechanism and the counterfactual in the delegation of institutional assets. First, the opportunity set is collectively exhaustive, finite, and readily identifiable. This is because of the formulaic nature of the search process for investment managers. Once asset allocations (e.g., 40% allocation to domestic equity) are set, plan sponsors disseminate request for proposals (RFPs), which are a form of directed search. The set of investment managers who could respond to the RFP is identifiable, and the winner is often publicly disclosed. Second, in contrast to other situations in which understanding efficacy requires subjectivity and/or decades to observe (e.g., employee happiness or monetary returns to occupational choice), portfolio returns are objective and readily observable.

We study 5,245 decisions made by 1,336 U.S. plan sponsors delegating over \$1.1 trillion in assets to 644 unique investment managers between 2002 and 2017.[3] We restrict our attention to mandates in public equity and fixed income because the investment vehicles in these asset classes (typically separately managed accounts) are homogenous and returns are standardized. For each mandate, we construct an opportunity set from nonselected investment managers that offer live investment products in the same geographic region, style, and year. For

[2]A long literature exploits the counterfactual and links it to opportunity costs. See Imbens and Wooldridge (2009) for a comparison of methods used to evaluate public programs and choice. In the spirit of Heckman (2001), we make no causal claims from the counterfactual.

[3]We consider a more expansive sample that includes almost 7,000 decisions from over 2,000 plan sponsors originating from 36 countries in an earlier version of the article. To streamline the analysis, we focus here exclusively on U.S. plan sponsors. Readers interested in the global sample can find details at https://papers.ssrn.com/abstract=3651476.

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example, an endowment delegating \$100 million to a manager in emerging markets small-cap value, may choose from many investment managers that offer investment products in that group. The size of the opportunity set mechanically determines the unconditional probability of being chosen, which in most of our tests, is about 1%.

Our primary interest is in the influence of relationships on manager choice, although in doing so we also quantify the effect of return chasing (Guercio and Tkac (2002), Goyal and Wahal (2008)).[4] Relationships could be important because officials from plans, investment consultants, and investment managers have ample opportunity to interact with each other, described as "schmoozing" by Lakonishok et al. ((1992), p. 375). There are at least three nonmutually exclusive views on the influence of relationships. The first suggests that because relationships are a conduit for information sharing, they solve asymmetric information problems, reduce moral hazard, and lower search costs, all of which can improve outcomes (Cohen, Frazzini, and Malloy (2008), (2010), Engelberg, Gao, and Parsons (2012), Ozsoylev, Walden, Yavuz, and Bildik (2014), and Rossi et al. (2018)). A second view is that relationships are used to extract rents and result in adverse outcomes (Cohen and Schmidt (2009), Blanes-ì-Vidal, Draca, and Fons-Rosen (2012), Cohen and Malloy (2014), Haselmann, Schoenherr, and Vig (2018), and Schoenherr (2019)). A third view is that relationships embody trust, which offers benefits such as security from expropriation or reducing investor anxiety about risk without generating excess returns (Gennaioli et al. (2015)).

We measure connections between plans sponsors, investment managers, and consultants using proprietary data from Relationship Science, a firm that specializes in identifying relationships between individuals, especially among those employed by financial institutions below the C-suite (e.g., "relationship directors" assigned by investment managers to prospects and clients). We estimate selection regressions using mandate-specific fixed effects so that the counterfactual in each decision is its designated opportunity set. The influence of connections on the probability of an investment manager being hired is striking. Investment managers connected to plan sponsors are about 0.17% more likely to be hired than unconnected managers. Relative to the unconditional probability of hiring of 0.96%, this represents an 18% increase. The increase in the selection probability implied by connections between investment managers and investment consultants (who act as gatekeepers, shepherd the investment process, and provide headline-risk cover) is also about 18%. Interestingly, there is substantial nuance underneath these results. For instance, in regressions that use connection strength (as opposed to merely recognizing the presence of connections), incremental selection probability rises by 69% in the case of *strong* connections. And when we scale connection frequencies by the number of *possible* connections (because larger investment managers have more connections), the incremental selection probability increases by 63%. By way of comparison,

[4]Our goal is quite different from that of Rossi, Blake, Timmermann, Tonks, and Wermers (2018). They study connections *within* investment managers while we study connections *between* investment managers and plan sponsors. Our approach is also quite different from Jaiswal (2021) who examines recommendations by consultants who also own asset management firms (e.g., coowned firms such as the case for Graystone Consulting or the Russell Group), or recommendations when a consultant and asset manager pair have subadvisory or broker payment arrangements.

moving from the 25th percentile to the 75th percentile of past performance in the same investment style increases the probability of selection by about 29%. The headline result, therefore, is that the magnitude of the selection effect from connections is comparable to that of past performance.

Connections are endogenous, posing important challenges to inference. It is possible that high "quality" investment managers are more likely to be connected to plan sponsors, which would imply that it is not relationships that drive selection but omitted investment manager characteristics. Our regressions rule out this possibility because we also include investment manager fixed effects in our regressions. However, it is still possible that high-quality plan sponsors are more likely to be connected to high-quality investment managers, so that selection is because of some unobservable *joint* characteristic rather than connections. For example, if manager size is correlated with quality, then we expect our results to be driven by large investment managers and their connections. In contrast, if connections matter for selection, we expect them to matter more for small investment managers, whose growth (and survival) is predicated on asset gathering; and for a plan sponsor's first investment in an investment manager, where information asymmetries are likely larger. We find precisely that. Connections substantially increase selection probabilities for small investment managers but have no discernible impact for large investment managers. Similarly, connections are important when a plan selects an investment manager for the first time. And combining the two, we find that connections play an important role in the first-time selection of small investment managers. Together, these results suggest that connections matter precisely where we expect them to matter.

To understand the opportunity costs of the counterfactual, we study post-hiring returns. Average 3-year post-hiring cumulative excess returns for hired investment managers are lower than those in the opportunity set by 0.93% (t-stat. = 2.66). On the surface, such inferior selection presents a puzzle. But the negative post-selection returns are driven entirely by the choices of public pension systems for whom the 3-year excess returns of chosen investment managers are lower than those of in the opportunity set by 1.17% (t-stat. = 3.16), and where agency problems are well-documented. Indeed, the difference in returns for all other plan sponsors is indistinguishable from zero.

To infer whether connections convey information that is helpful in selecting investment managers with higher post-hiring returns, we estimate regressions that quantify the value of connections. We do so in a variety of ways, but the cleanest measure comes from a difference-in-difference in returns between hired and not-hired funds across connected and unconnected investment managers. If connections convey information relevant for future returns, then plan sponsors should display the better ability to select among connected than among unconnected funds. We do not find such evidence.

The mechanics of the selection process permit an even more refined test albeit in a small sample. In evaluating RFP respondents, plans whittle their opportunity set down to a short list of three to five candidates. These candidates are invited to do formal presentations, which are referred to as a "finals" presentation. For a small sample of mandates, we observe the exact finals list and replicate both the selection equations and the post-hiring return regressions. Even though power is limited by

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the small sample, the data still show that connections between consultants and investment managers matter for selection. And similar to the large sample evidence, connections do not generate positive post-hiring returns for plan sponsors.

The overall picture that emerges is that selection is related to social connections and the economic magnitude is about as important as past performance. Ex post, these selection criteria do not result in higher future returns relative to counterfactual investable investment choices.[5] The fact that connections are not associated with higher future returns implies that the pecuniary gains to trade are asymmetrically shared between investment managers and plan sponsor claimholders – the former receive flows and fees, but the latter do not get improved gross-of-fee performance. There could be countervailing benefits for plan sponsors. For example, it could be that connected investment managers deliver returns with lower volatility. However, we find that information ratios (which scale excess returns by volatility) are also not higher in the presence of connection. It is also possible that fees charged by connected investment managers are lower than those for not-connected investment managers. We find that they are not. Finally, it is possible that investment committees or their trustees receive countervailing personal benefits. For instance, it may be that by hiring connected managers, trustees lower legal or headline risk, incur lower search costs, or receive private nonpecuniary benefits. These are unmeasurable with our data but potentially relevant.

Our article is related to two prior studies that examine selection of investment managers. Goyal and Wahal (2008) study the selection and termination of investment management firms by plan sponsors, and Jenkinson, Jones, and Martinez (2016) investigate recommendations by investment consultants. Relative to those studies, our key result is that selection is related to connections between individuals at plan sponsors, investment managers, and consultants. Methodologically, the use of an opportunity set to study both choice and the counterfactual in this area is unique. As we argued earlier, this approach is important from an economic perspective because one cannot understand choice and importance of connections solely by looking at outcomes. Our article is also related to the literature on conflicts of interest arising from business ties in the investment management industry. For example, Pool, Sialm, and Stefanescu (2016) find that mutual fund families that are service providers in 401(k) plans favor their own funds, and Pool, Sialm, and Stefanescu (2022) report that funds which share revenue with record keepers are more likely to be added and less likely to be deleted from investment menus.

The remainder of the article is as follows: Section II briefly describes the search process. Section III describes the various data sources that we use and the construction of two samples. Descriptive statistics on data are presented in Section IV. We examine investment manager choice in Section V, and post-hiring returns in Section VI. Section VII discusses the sample of investment managers who make it to the finals list. Section VIII concludes.

[5]Our analysis focuses on asset classes composed of publicly traded securities. It is possible that connections are beneficial in other contexts such as private equity where matching is potentially more important.

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II. The Search Process

The search process for most pension systems, endowments, foundations, and sovereign wealth funds is relatively standardized. Investment committees, often in conjunction with a consultant, decide allocations to asset classes and investment styles. These allocations trigger a search process for investment managers specific to that asset class and style. Most plans engage their existing investment consultant to assist in the search, or in the case of private equity retain a (different) consultant specific to the asset class. Consultants can be viewed as informed experts in the sense of Krishna and Morgan (2001), although they undoubtedly fulfill other economic functions such as information gathering and protection from headline risk.

Most searches are conducted using RFPs. Details of the RFP are often, but not always, in the public domain.[6] The typical search resembles an inverted pyramid. The starting point is formal responses to RFPs by investment managers in the form of a Due Diligence Questionnaire. The responses include information about the firm including a description of personnel and their qualifications, assets-under-management, investment product information, references, fees, and so forth. Each response is evaluated by the plan and/or consultant – a process that may involve extended written exchanges of more detailed information. The final evaluation criteria are confidential but may involve measurable "hard" factors as well as soft considerations that require judgment. The former includes historical performance relative to benchmarks, attributions, measurement of trading costs, and so forth. The latter includes an assessment of key personnel risk (i.e., the risk that the CIO or a key portfolio manager may leave), consistency of investment philosophy, uniqueness relative to other investment managers, and other such attributes. Investment managers have a fairly good idea of who their competitors are, and therefore craft a narrative designed to distinguish themselves from each other.

Plans winnow the candidate list down from those responding to the RFP to a smaller subset referred to as a finals list. The number of managers in this list can vary but often ranges from two to five. Finalists are invited to do presentations to the plan's investment committee. Either before or after those presentations, plan officials or consultants may also conduct site visits to investment management firms to enhance their understanding of the investment process, infrastructure, and personnel. At the completion of this process, the investment committee makes a hiring recommendation.

This search, evaluation, and selection process is not the only way information is exchanged. There are repeated interactions between individuals at plans, consultants, and investment management firms in other industry settings. For example, a large number of investor conferences are designed to enhance social networks between these groups of participants, generating a nexus that can enhance

[6]RFPs for public institutions, particularly in the U.S., are equivalent to an open tender and generally posted online. For private institutions, the process is similar but less transparent. Some RFPs require that responding firms meet a set of criteria such as a minimum age, assets under management, and so forth. We investigate the influence of such restrictions in Section V.C.

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information flow or aggravate agency costs.[7] Such conferences receive sponsorship from investment management firms, which suggests that they may be valuable in terms of future flows. As in other parts of the financial industry, employees of investment managers, consultants, and plan sponsors also exchange human capital, generating information networks that could be to the flow of capital between these organizations.

III. Data Sources and Sample Construction

Our tests require information from multiple data sources. In this section, we describe the sources, as well as procedures to match and combine databases.

A. Mandate Information

We obtain public equity and fixed income mandate information from Fundmap, a firm that tracks RFPs generated by global institutional investors as well as selection decisions without public RFPs. Fundmap's database is comparable to those provided by other sources (e.g., Pensions and Investments) but with a relatively long time series. For each mandate, the database includes information on the value of the mandate, the geographic focus of the investment vehicle (e.g., U.S. emerging markets), and the investment style. Style definitions follow market conventions. In public equity, investment styles are defined using a size and value/core/growth grid. Fixed-income investment styles are based on duration, credit quality, and security type information (e.g., mortgages, convertibles). Plan information includes the name of the organization, location details, and the name and title of the key decision maker, typically a CIO or treasurer. The database also records the lead consultant for the plan, and if used, a search consultant. A "comments" field populated by a reporter covering each plan contains contextual information. This field sometimes identifies investment managers that qualified as finalists for the mandate, a data item that we exploit in Section VII. We exclude selection decisions pertaining to 401 K plans, 403(b) plans, 457 plans, 529 plans, and other Direct Contribution systems because they do not allocate specific dollar amounts. We also exclude all pure index fund mandates because selection is less consequential.[8]

[7]Even a cursory examination of conference brochures and websites reveals clear networking intentions. For example, one conference touts "Our events boast an interactive engagement model that allows for productive networking and meaningful discussions among a private group of your peers, experts, academics, and influencers in the field." The list of such conferences is large but prominent ones include those organized by the Money Management Institute, Institutional Investor Conferences, the Q-Group, and the Chicago Quantitative Alliance. An amusing account of the "worst conference ever" provided by McDaniel (2017) describes the structure and format of such interactions.

[8]Index fund mandates are potentially interesting as a placebo test because superior performance cannot be related to hiring decisions, and connections cannot be a source of information about future performance. However, there are a few reasons why the implementation of such a placebo test is problematic. First, the vast majority of such mandates are not pure index fund mandates but so-called passive mandates that contain some sort of overlay (e.g., preferred securities lending programs), or are enhanced indexes that hug but attempt to beat an index. Second, as a practical matter, in our data, there are only 60 pure index mandates, of which 42 are directed to one investment management firm (the remaining mandates are directed to two other firms).

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B. Investment Manager Data

We obtain information on returns and assets under management for each product offered by investment managers from eVestment. eVestment is a firm that specializes in the institutional marketplace and widely used by all parties engaged in the screening and selection process. The database is free of survivorship bias. All returns are gross of fees.

Three aspects of the data require special attention. First, the unit of observation in our analysis is a product which can be thought of as an investment strategy. A product can be provided in several "vehicles," which may be associated with separately managed or comingled accounts. All vehicles correspond to the same investment approach. The returns to a product are typically compliant with respect to GIPS. Second, each product is associated with a geographical focus and investment characteristics. For example, a U.S. small cap value product is assigned "U.S." as its geographical focus, "small" as its size characteristic, and "value" as its style characteristic. We use these features to assign a benchmark for each product. Although the database identifies manager-selected benchmarks, we opt to assign benchmarks ourselves to maintain consistency and avoid issues related to benchmark gaming (Sensoy (2009)). In the above example, the benchmark is the Russell 2000 Value Index. Benchmark assignments follow standard industry practices. A complete list of benchmarks is in Appendix A. Third, some of our tests require information on product fees. Institutional fees are typically a downward-sloping step function, with fee levels declining with the size of the mandate. For example, a manager might charge 0.60% for mandates under $20 million, 0.50% for mandates between $20 and $40 million, and 0.40% thereafter. We observe the entire step function and use the mandate size to measure the applicable fee.

C. Relationship Data

We acquire proprietary data on connections between individuals at plans, consultants, and investment managers from Relationship Science.[9] Many existing studies of connections exploit linkages between individuals serving on corporate boards or via educational networks (e.g., BoardEx). We employ Relationship Science for four reasons. First, coverage is better; BoardEx covers 3 million profiles while Relationship Science covers 10.1 million individuals. Second, Relationship Science caters particularly to nonprofits (including endowments and foundations) and financial institutions, which improves coverage among the organizations of interest to us. Third, Relationship Science tracks individuals in senior management and decision-maker roles beyond the board of directors or the C-suite. This is especially important in our setting because the delegation of assets from plans to investment managers often involves personnel who are not part of the C-suite. For instance, relationship managers and investment committee members are the key point of contact between plans and investment managers. Fourth, the database tracks common elements between individuals such as board participation, overlapping career history, overlapping roles in nonprofit organizations, investments,

[9] Appendix B describes collection and processing procedures in detail.

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transaction participation, personal connections, relative seniority between the individuals who overlapped in the same organization, the duration of the overlap, etc., while recognizing that individuals can have multiple relationships. We do not observe these underlying common elements, but Relationship Science uses a proprietary algorithm that combines these elements to provide an indication of the strength of the connection. We use this strength variable in a subset of our tests.

The Relationship Science database is based on publicly verifiable data sources, including SEC records, court records, financial statements, and other such hard records. Notably absent are self-reported linkages and social networks such as LinkedIn. The downside of this approach is that some linkages are potentially missed, but the advantage is a lower false positive rate because spurious linkages that are not actionable are avoided. Potentially missing linkages makes it harder to detect the influence of connections, rendering our tests conservative.

Our tests require knowledge of links between individuals across all possible organization pairs. Since the Relationship Science database covers an extremely large group of individuals at each organization, we restrict the search to a list of employee designations that we pre-specify. These designations include C-Suite individuals, as well as those in marketing, sales, research, and other functions that are important for interactions between the two organizations. Pre-specifying designations have the advantage of excluding lower-level employees (e.g., an assistant portfolio manager at an investment manager) that are unlikely to be important for the selection process; a complete list of designations is in Appendix B. Based on these criteria, Relationship Science provides us with a customized extract from their database covering the employees and organizations of interest.

The data extract generates millions of records because it tracks all possible connections. We aggregate connections across individuals in each organization pair, summarizing connection information in three ways. First, we create an indicator variable if there is a connection between two organizations in a particular year. This is the primary connection variable used in most of our tests. It is general and, by definition, agnostic with respect to the type or nature of connections between individuals. Second, we exploit information on the strength of the connection (classified as "strong" or "not-strong"), by creating indicator variables corresponding to each group. Third, we compute a continuous variable that scales the number of connections between two organizations by the product of the number of covered individuals in the plan and investment manager. The last two variables allow us to assess whether connection strength (rather than just the presence of connections) matters.

D. Sample Construction

Our tests require two samples: the set of selected investment managers and the opportunity set. Before constructing these samples, we match organizations from the three data sources mentioned above. Since the databases all use their own unique and nonstandardized identifiers, we create a master file that links them using a combination of electronic text matching and manual adjustments. An example is helpful to understand the nuances. Suppose that the three databases record an asset manager as i) AJO Partners, ii) Aronson-Johnson-Ortiz, and

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iii) AJO+. Our process links all three name variations to one master identifier. A second complication arises from mergers and acquisitions in the asset management industry. Consider, for example, a mandate allocated to the Framlington Group by a plan sponsor in 2004. In 2005, the Framlington Group was acquired by AXA Investment Managers. Our tests require knowledge of returns for Framlington's portfolios both before and after acquisitions. Our master file tracks both dead and live firms, matching merged firms. The vast majority of asset managers are matched in all three databases so there is minimal loss of information.

To obtain pre- and post-hiring returns for a mandate, we also need to match the mandate from Fundmap to a product from eVestment. This matching is done both electronically as well as manually. We first use geographic focus and style characteristics to electronically match mandates to products. For example, a U.S. small-cap value mandate allocated to Dimensional Fund Advisors is assigned to the equivalent product in eVestment. In some cases, we generate multiple matches as it may happen that the investment firm offers multiple products even within the same style geography bucket. In these cases, we average the matched product returns. Finally, it can also be the case that some mandates do not fit the traditional style assignments. For example, a mandate assigned to AJO Partners for its Managed Volatility strategy would not be matched via electronic methods. For those, we use a manual matching method. We match almost 90% of mandates from FundMap to specific products (or a combination of products) in eVestment.

To construct the opportunity set, we employ a slightly different two-step process. The first step is, as above, to electronically find potential matches for the opportunity set based on the same geographic focus and style characteristic of the chosen product. Thus, for each equity mandate, we require that the opportunity set contain all products in the same geographic focus, equity capitalization, equity style (core, value, growth), and benchmark as assigned by eVestment. We construct the opportunity set for fixed income mandates in a similar way, except that we replace the equity capitalization and style requirements with duration (short, intermediate, long, all durations) and fixed income styles (core, high yield, mortgage, and others). It is possible that the opportunity set for one mandate can be matched with multiple products from the same firm (e.g., if a firm offers two small-cap value products). In such cases, we average the two product returns so that a firm is only included in an opportunity set once.

This electronic matching process can sometimes generate false matches. To eliminate false positives, we incorporate a second elimination criterion in which we require the style information to appear in the product name. For example, if a mandate allocated to Dimensional Fund Advisors was in U.S. small cap value, as above, then we require that the terms "small" and "value" appear in the product name of potential matches, thereby excluding poor matches. This strict criterion minimizes false positives, but it is possible that we do not include potentially viable products in the opportunity set (false negatives).

There is one other important aspect to the construction of the opportunity set. We require that the products in the opportunity set have 3-year returns prior to the date of the mandate. This requirement introduces a survivorship that is consistent with the screening and selection process described in Section II.

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IV. Data Description

A. Sources of Capital and Investment Styles

Although the original sample of mandates is global, we restrict our attention to U.S.-based plan sponsors. Table 1 shows the distribution of mandates originating from 1,336 unique plan sponsors over the period of 2002 to 2017. There are 5,245 mandates, of which 3,777 are equity and the remainder are fixed income. The aggregate dollar value of assets delegated over this period is over $1.1 trillion. Of that, public equity accounts for $700 billion. The size of the average fixed-income mandate is $300 million, substantially larger than for equity mandates ($187 million). There is considerable skewness in the size distribution of both equity and fixed-income mandates, medians are substantially smaller than the mean.

There are over 20 different categories of plans in our data representing considerable variation in the underlying constituents. For example, foundations include public foundations, private family foundations, independent private foundations, corporate private foundations, community foundations, and so forth. To prevent the analysis from becoming unwieldy, we classify all plans into four main groups: public plans, corporate plans, endowments and foundations, and a catch-all miscellaneous group. Almost 80% of mandates (4,180 out of 5,245) come from public plans, and by dollar value, the concentration is even higher ($0.96 trillion out of $1.15 trillion). Despite this concentration, a meaningful number of mandates are generated by corporate plans, endowments, foundations, and other types of plan sponsors. The average and median size of mandates from public plans is larger than those from other categories.

B. Opportunity Set Description

The median number of firms in the opportunity set varies from a low of 13 (in all cap growth) to a high of 157 (in large-cap growth). Across all equity and fixed income styles, the average number of firms in the opportunity set is 94 and

TABLE 1

Distribution of Investment Mandates from Plan Sponsors

Table 1 shows the sample of public equity and fixed-income investment mandates from U.S. plan sponsors delegated to investment managers between 2002 and 2017. We report the number of mandates (*N*), the total value of all mandates in billions of U.S. dollars (Sum), and the average and median mandate size in millions of U.S. dollars. Public plans include union plans as well as state, municipal, county, and city-level pension plans. Corporate plans include single- and multi-employer pension plans. Endowments and foundations (Endws and Fnds) include both public and private entities. The miscellaneous category includes permanent funds, surplus funds, trust funds, settlement funds, and health plans.

	Equity				Fixed Income			
	N	Sum $B	Average $M	Median $M	*N*	Sum $B	Average $M	Median $M
All	3,777	707	187	40	1,468	441	300	55
Public	3,049	594	195	50	1,131	370	327	75
Corporate	220	23	103	28	103	13	125	62
Endws and Fnds	340	15	44	11	128	3	24	11
Miscellaneous	168	75	449	45	106	55	522	72

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72, respectively, which implies that the unconditional probability of being chosen is about 1%. Narrowing the opportunity set further, as we do in Sections V.C and VII, mechanically increases the unconditional probability of being hired. In general, the average size of chosen firms is often larger than the opportunity set, which suggests that size may be part of the selection criteria. Although we do not tabulate statistics in a table, average fees for chosen firms are very similar to the opportunity set, within one or two basis points of each other. And as expected, past 3-year excess returns for chosen firms are larger than those of the opportunity set.

C. Relationship Frequency

Table 2 compares the frequency distribution of connections between chosen firms and the opportunity set. Panel A shows frequencies of connections (in %) between plan sponsors and investment managers, labeled PSxIM (*P*lan *S*ponsor cross *I*nvestment *M*anager). We also report connection frequencies between investment managers and consultants in Panel B, labeled IMxIC (*I*nvestment *M*anager cross *I*nvestment *C*onsultant). To provide a baseline for comparison, we first compute the distribution of connections between all possible pairs across the entire time series. This unconditional distribution of connections is reported in the first row of each panel. Roughly speaking, the unconditional frequency of connections is about 6%. In other words, in the cartesian product of connections between plan sponsors and investment managers, and between investment consultants and investment managers, there exist direct connections in a little over 1 out of 20 cases.

TABLE 2

Frequency Distribution of Connections Between Plan Sponsors, Investment Managers, and Consultants

Table 2 shows the frequency (in %) of connections between individuals at plan sponsors, investment managers, and investment consultants, separately for chosen firms and the opportunity set. Panel A shows connection frequencies based on connections between plan sponsors and investment managers (PSxIM). Panel B shows connection frequencies based on connections between investment managers and investment consultants (IMxIC). The unconditional frequency of connections is based on all plan sponsors, investment managers, and investment consultants over the entire sample period. Firms and plans are classified as large (small) if they are above (below) the median size.

	Chosen Firm	Opportunity Set
Panel A. Between Plans Sponsors and Investment Managers (PSxIM)		
Unconditional distribution	6.4	6.4
All	27.1	17.0
Large plans and large firms	48.8	47.3
Small plans and large firms	12.4	10.5
Large plans and small firms	29.5	17.1
Small plans and small firms	4.6	2.9
Panel B. Between Investment Managers and Investment Consultants (IMxIC)		
Unconditional distribution	5.8	5.8
All	22.5	15.0
Large plans and large firms	30.2	25.4
Small plans and large firms	26.4	24.1
Large plans and small firms	11.8	8.3
Small plans and small firms	16.2	9.6
Panel C. Connection Strength		
Strong connections	1.4	0.6
Not strong connections	25.8	16.3

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Panel A of Table 2 shows that the frequency of connections between plan sponsors and chosen investment managers is 27.1%, substantially higher than the equivalent frequency in the opportunity set of 17.0%. Similarly, in Panel B, the frequency of connections between investment consultants and chosen investment managers is 22.5%, but only 15.0% in the opportunity set.

The remainder of each panel explores variations in connections with respect to the size of the organization pairs. Unsurprisingly, the frequency of connections between large plans and large firms is substantially higher than between small plans and small firms, and various combinations thereof. For example, 48.8% of large plans and large investment managers that are chosen are connected. Among small plans and small firms, the equivalent number is only 4.6%. But the more interesting comparisons are the *differences* in connections between chosen firms and those in the opportunity set for small versus large investment managers. For large investment managers and large plans, the difference between connections among chosen firms and the opportunity set is only 1.5% (48.8–47.3). In stark contrast, for small investment managers (focusing again on large plans), 29.5% of chosen investment managers are connected versus only 17.1% in the opportunity set; the difference is 12.4%. A very similar pattern emerges in connection frequencies between investment managers and investment consultants (Panel B of Table 2). The data hint at the potential importance of connections for small investment managers to gather assets from plan sponsors, an issue that we tackle in Section V.B.

Panel C of Table 2 shows the frequency of strong versus not-strong connections between plan sponsors and investment managers. As might be expected, the percentage of strong connections is substantially smaller than not-strong connections. More importantly, the ratio of strong to not-strong connections in chosen firms is larger than the same ratio for the opportunity set, hinting at the possibility that the strength of the connection may matter for selection decisions.

V. Investment Manager Choice

A. Selection Regressions

Table 3 contains OLS regressions in which the dependent variable is equal to 1 when plan sponsor PS chooses investment manager IM, and 0 for the opportunity set.

$$\text{HIRED}_{\text{PS,IM}} = \beta_0 + \beta_1 \text{PSxIM} + \beta_2 \text{IMxIC} + \beta_3 \text{CER}_{\text{IM}}(-3) + \beta_4 \ln(\text{AuM}_{\text{IM}}) + \text{FE}. \quad (1)$$

All regressions include mandate-specific fixed effects so that the comparison set is always the designated opportunity set; an added advantage is that commonality associated with plans or investment styles is absorbed by mandate-specific fixed effects.[10] The regressions also include investment manager fixed effects. t-statistics are based on standard errors that are clustered at the cross-product of

[10]See Bertrand, Djankov, Hanna, and Mullainathan (2007) and Angrist and Pischke (2009) for why OLS regressions are appropriate in this setting with a large number of fixed effects.

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TABLE 3

Investment Manager Choice Regressions

Table 3 shows OLS regressions of investment manager choice. The dependent variable is equal to 1 for the investment manager (IM) chosen by a plan sponsor (PS), 0 for investment managers from the opportunity set. The plan-manager connection indicator variable, PSxIM, is equal to 1 if there is connection between a plan sponsor and investment manager, and 0 otherwise. Similar connection indicator variables are constructed for strong, PSxIM(Strong), and not strong, PSxIM(NotStrong), connections. The continuous connection variable, PSxIM(Pct) is the number of connections scaled by the product of the number of covered individuals in both the plan and investment manager. The manager-consultant connection indicator variable, IMxIC, is similarly equal to 1 if there is a connection between investment managers and the search consultant, and 0 otherwise. $CER_{IM}(-3)$ is the cumulative excess return over the matching benchmark measured in the prior 3 years. $\ln(AuM_{IM})$ is the logarithm of assets under management of the investment manager. Columns 1–3 report results for all mandates, columns 4–6 report results for mandates where a big firm was hired, and columns 7–9 report results for mandates where a small firm was hired (in all cases, the opportunity set consists of all firms). Big firm is defined as firm with AuM > $10 billion. All regressions include mandate and investment manager fixed effects. To generate *t*-statistics, which appear in parentheses, we cluster at the style × geography × year level.

Hired Firm →	All			Big			Small		
	1	2	3	4	5	6	7	8	9
PSxIM	0.17	–	–	−0.06	–	–	0.25	–	–
	(2.38)			(−0.74)			(2.53)		
PSxIM(Pct)	–	0.61	–	–	0.56	–	–	0.54	–
		(2.40)			(1.72)			(1.66)	
PSxIM(Strong)	–	–	0.67	–	–	1.01	–	–	0.30
			(2.13)			(1.99)			(1.49)
PSxIM(NotStrong)	–	–	0.16	–	–	−0.08	–	–	0.25
			(2.25)			(−1.02)			(2.52)
IMxIC	0.17	0.17	0.17	−0.03	−0.03	−0.03	0.37	0.38	0.37
	(2.08)	(2.09)	(2.09)	(−0.33)	(−0.32)	(−0.31)	(3.15)	(3.15)	(3.15)
$CER_{IM}(-3)$	3.08	3.08	3.08	2.60	2.60	2.61	3.20	3.21	3.20
	(11.16)	(11.15)	(11.17)	(6.86)	(6.86)	(6.88)	(7.83)	(7.83)	(7.83)
$\ln(AuM^{IM})$	0.31	0.31	0.31	0.43	0.43	0.43	0.19	0.19	0.19
	(5.96)	(5.96)	(5.96)	(5.38)	(5.37)	(5.38)	(2.96)	(2.96)	(2.96)
No. of mandates	3,433	3,433	3,433	2,097	2,097	2,097	1,336	1,336	1,336
No. of obs.	360,459	360,459	360,459	198,863	198,863	198,863	161,539	161,539	161,539
Within-R^2	0.11	0.12	0.12	0.08	0.09	0.09	0.16	0.16	0.16

year, style, and geography. All specifications control for past 3-year excess returns ($CER_{IM}(-3)$) and the size of the investment manager ($\ln(AuM_{IM})$).

The first model employs indicator variables, PSxIM and IMxIC, that are equal to 1 if the relevant connection exists in the hiring year, and 0 otherwise. In this model, the coefficients on both PSxIM and IMxIC are 0.17%, with *t*-statistics of 2.38 and 2.08, respectively. The unconditional probability of selection is 0.96%, implying that the incremental increase in the selection probability of a connected investment manager relative to an unconnected investment manager for both coefficients is about 18%.[11]

PSxIM condenses complex connection information into a convenient-to-interpret indicator variable but potentially throws away information. We resurrect such information in two ways. First, we scale the number of connections between plans and investment managers by the product of the number of individuals covered by Relationship Science in both organizations. We label this variable PSxIM(Pct) to

[11]One could also imagine a scenario in which connections attenuate the influence of past performance, similar in spirit to the influence of affiliations in menu additions to 401(k) plans studied by Pool et al. (2016). However, in unreported regressions, the coefficient of an interaction variable between PSxIM and $CER_{IM}(-3)$ is only 0.20 and the *t*-statistic is 0.47.

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indicate its continuous and bounded nature.[12] In model 2, the coefficient on PSxIM(Pct) is 0.61% with a *t*-statistic of 2.40 while the coefficient on IMxIC is unchanged. We calculate the marginal effect for this continuous variable by considering a move from a state of no connection to a state where all individuals are connected, which is an upper bound on the impact of PSxIM(Pct) on hiring probabilities. This represents an increase of 63%. Second, we include two different indicator variables that focus on the strength of the connections, labeled PSxIM(Strong) and PSxIM(NotStrong) (the omitted category remains unconnected organizations). In model 3, the coefficient on PSxIM(Strong) is 0.67% (*t*-stat. = 2.13), which represents a 69% increase in the probability of selection. Notably, this increase is substantially larger than for investment managers with not-strong connections (the coefficient is 0.16%, which represents a 16% increase).

In our view, the incremental increases in selection probabilities are economically meaningful. But another way to gauge the incremental effect of connections on selection probabilities is to compare them to the effects of performance chasing. In model 1, a movement from the 25th to the 75th percentile of past performance increases the probability of being selected by 29% over the baseline. Comparing this to the incremental probabilities described above, the influence of connections appears to be comparable to that of prior performance. This headline result is of considerable economic importance because the dollar value of assets moving from one investment manager to another is extremely large, and because it speaks to the frictions in asset management discussed in the introduction. In the next section, we focus on tightening inferences with respect to connections, paying particular attention to omitted variables.

B. Tightening Inferences

Connections are endogenous, complicating inferences. For example, connections could be correlated with the accumulated reputation of the investment manager, in which case the link between connections and selection could be due to reputation. Our regressions rule out this particular possibility because we also include investment manager fixed effects. However, it is still possible that high-quality plan sponsors are more likely to be connected to high-quality investment managers, so that selection is because of some unobservable *joint* characteristic rather than connections. One way to address this is to saturate the sample with plan manager by investment manager fixed effects (in addition to mandate-specific fixed effects), effectively relying on time variation in connections. Such a solution requires two prerequisites. First, it requires that the same plan sponsor and investment manager combinations (either in hiring decisions and/or in the opportunity set) occur often enough for the regression to be estimable. That does not happen very often.[13] Second, it requires adequate time series variation in connections. That requirement is also not fulfilled. The reason is that once connections are established, they are rarely severed. Once employee X of plan sponsor PS knows employee Y of

[12] Conditional on connections, the 25th, 50th, and 75th percentiles of PSxIM(Pct) are 0.04, 0.10, and 0.24, respectively.

[13] The median number of times a unique plan sponsor and investment manager combination appears in our data is 2.

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an investment manager IM_1, it is impossible for the pair to "unknow" each other. Of course, if employee Y were to move from IM_1 to IM_2, the connection would be transformed from $PSxIM_1$ to $PSxIM_2$, but, as an empirical matter, employee turnover is not large. Moreover, connections between a plan sponsor and an investment manager are established via many employees, not just X and Y. So even if employee Y were to move, other employees of the same investment manager would remain connected to the plan sponsor. The upshot is that we cannot use plan sponsor by investment manager fixed effects.

A related issue arises with respect to connections between investment managers and investment consultants.[14] It is possible that consultants are hired for their connections, particularly by small plan sponsors trying to access certain investment managers. While this access issue is of great importance in private equity, it is less of a concern in public equity and fixed income. However, the other side of the coin, namely, small investment managers attempting to access plan sponsor assets via connections with consultants, remains a possibility.

While we cannot completely eliminate the possibility of endogenous matching on unobservable characteristics, we investigate two settings in which connections are more likely to be the driver of selection decisions. The first comes from the idea that connections are likely to be more important for small investment managers where matching on unobservable quality is less probable. In other words, to the extent that investment manager size is correlated with quality, a matching story implies that the importance of connections will be driven by large investment managers in our tests. Columns 4–6 and 7–9 of Table 3 report separate regressions mandates won by big and small investment managers, respectively.[15] In these regressions, much (but not all) of the traction appears to come from small investment managers. For example, for big investment managers (column 4), the coefficients on PSxIM and IMxIC are −0.06 and −0.03 with *t*-statistics of −0.74 and −0.33, respectively. But for small investment managers (column 7), the coefficients on PSxIM and IMxIC are 0.25 and 0.37 with *t*-statistics of 2.53 and 3.15, respectively. The differences are less apparent when we use continuous connection variables PSxIM(Pct) or use connection strength but persist for IMxIC. These results are inconsistent with an unobserved quality-matching explanation.

The second setting in which connections likely matter and assortative quality matching is less likely to apply is for a plan sponsors' first investment with an investment manager. Here an important challenge is to separate the role of connections from repeat business relationships established via prior hiring decisions. To investigate this, we create an indicator variable "PreviouslyHired" which is equal to 1 if the plan sponsor has previously hired an investment manager. The first column of the regressions in Table 4 includes this variable, along with PSxIM and IMxIC. The coefficient on PreviouslyHired is 4.71 (*t*-stat. = 10.99), which represents an almost fivefold increase in hiring probability relative to the unconditional

[14]General investment consultants (i.e., those not specific to private equity) are very rarely hired or terminated. Andonov, Bonetti, and Stefanescu (2022) report only 180 consultant hiring events and only 7 instances where a pension fund hires a consultant for the first time in the 2001 to 2020 sample period.

[15]We use \$10 billion in AUM as a breakpoint for these definitions but the results are largely insensitive to this choice.

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TABLE 4

Investment Manager Choice Regressions with Prior Hiring

Table 4 shows OLS regressions of investment manager choice similar to those in Table 3. We add an indicator variable "PreviouslyHired" which is equal to 1 if a firm in the opportunity set or the firm being hired has previously been hired by the plan sponsor. The "NotPreviouslyHired" indicator variable is equal to 1 if a firm in the opportunity set (or the hired firm) has not been hired by the plan sponsor in the past (i.e., NotPreviouslyHired = 1 − PreviouslyHired). We also include an interaction of NotPreviouslyHired and PsxIM (the coefficients on PSxIM and IMxIC itself are not identified; please see text). Subsample definitions are as in Table 3. All regressions include mandate and investment manager fixed effects. To generate *t*-statistics, which appear in parentheses, we cluster at the style × geography × year level.

Hired Firm →	All	All	Big	Small
$CER_{IM}(-3)$	3.03 (11.10)	3.03 (11.11)	2.57 (6.87)	3.14 (7.75)
$\ln(AuM_{IM})$	0.29 (5.53)	0.29 (5.53)	0.41 (5.09)	0.17 (2.66)
PreviouslyHired	4.71 (10.99)	–	–	–
PSxIM	0.13 (1.81)	–	–	–
IMxIC	0.17 (2.15)	–	–	–
NotPreviouslyHired	–	−4.84 (−11.20)	−4.45 (−8.06)	−4.34 (−7.28)
PSxIM×NotPreviouslyHired	–	0.23 (3.12)	−0.05 (−0.63)	0.39 (3.74)
IMxIC×NotPreviouslyHired	–	0.16 (2.08)	−0.08 (−0.83)	0.45 (3.74)
No. of mandates	3,433	3,433	2,097	1,336
No. of obs.	360,459	360,459	198,863	161,539
Within-R^2	0.38	0.38	0.31	0.40

probability. The large magnitude is not particularly surprising; in the context of hiring of private equity managers, Lerner, Schoar, and Wongsunwai (2007) show that plan sponsors make repeat investments about 50%–60% of the time, and Goyal, Wahal, and Yavuz (2023) find that prior hiring experience increases the probability of selection about five to six times. More importantly, the coefficients on PSxIM and IMxIC remain positive and significant, 0.13 and 0.17, respectively.

Our primary interest, however, is in understanding how much prior relationships help in the first-time hiring of an investment manager. To do so, we flip the PreviouslyHired indicator variable to "NotPreviouslyHired" so it is equal to 1 if an investment manager in the opportunity set has not been hired by the plan sponsor in the past (i.e., PreviouslyHired = 1 − NotPreviouslyHired), and then interact it with PSxIM and IMxIC. The interaction effects inform us of the importance of relationships *within* the group of investment managers who have not been previously hired by the plan sponsor. In the second column, the coefficient on the interaction effects with PSxIM and IMxIC are 0.23 and 0.16, with *t*-statistics of 3.12 and 2.08, implying that connections are more important for first-time hiring decisions. More revealingly, when we estimate separate regressions for big versus small investment managers, the results appear to be entirely driven by the selection of small investment managers.[16] In other words, connections are an important mechanism for

[16]In these regressions, the coefficients on PSxIM and IMxIC are not identified because these measure the impact of connections among investment managers that were previously hired by plan sponsors (by definition, prior hiring results in a connection).

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small investment managers to gather assets the very first time they are hired by a plan sponsor. This is the same for both connections between the plan sponsor and investment manager (PSxIM) and for connections between the investment manager and investment consultant (IMxIC).

C. Alternative Opportunity Sets

It is possible that the opportunity set includes investment managers who are infeasible for some unobservable reason. If that is the case, the regressions in Table 3 may overstate selection effects. In this section, we restrict the opportunity set in several ways to explore the sensitivity of selection mechanism to the size of the opportunity set.

Plan sponsors sometimes require that investment managers meet a set of pre-specified hurdles before responding to the RFP. Some requirements are trivial, such as registration under the Investment Advisors Act of 1940, whereas others may be more binding. Examples of the latter include requiring a minimum of assets under management, GIPS compliance, and so forth. These requirements serve to shrink the opportunity set and can potentially influence selection probabilities. We use three commonly used requirements to restrict the opportunity set: i) that firms in the opportunity set have at least $1 billion in assets, ii) that they are GIPS compliant at the time of the mandate, and iii) that at least half of the firm's assets derive from institutional clients. We then reestimate the baseline regression in Table 3 with these restricted opportunity sets.

Columns 2–4 in Table 5 show these regressions. To facilitate comparison, we also reproduce estimates from the original baseline regression in column 1. As expected, shrinking the opportunity set raises unconditional probability in each of

TABLE 5

Choice of Alternative Opportunity Sets

Table 5 shows results from OLS regressions of investment manager choice like those in Table 3 but with various restricted opportunity sets. The first regression (labeled Baseline) is identical to column 1 in Table 3. Columns 2–4 restrict the opportunity set based on whether and investment management firm has assets under management greater than $1 billion, whether it is GIPS compliant at the time of the mandate, and whether more than 50% of the firm's AuM is based on institutional clientele. Column 5 imposes all three restrictions together. Column 6 uses a bootstrap in which we randomly select five firms from the unconstrained opportunity set (i.e., the Baseline) and repeat the process 5,000 times. The results in column 6 show the average marginal effects across replications. All regressions include mandate and investment manager fixed effects. To generate t-statistics, which appear in parentheses, we cluster at the style × geography × year level.

	Baseline	AuM ≥ $1b	GIPS = 1	InstAuM ≥ 50%	AuM ≥ $1b GIPS = 1 InstAuM ≥ 50%	Bootstrap
	1	2	3	4	5	6
PSxIM	0.17	0.19	0.20	0.26	0.32	0.09
	(2.38)	(2.35)	(2.49)	(2.77)	(2.74)	
IMxIC	0.17	0.19	0.19	0.22	0.28	0.06
	(2.08)	(2.09)	(2.17)	(2.00)	(2.12)	
CERIM(−3)	3.08	3.99	3.55	4.17	5.49	0.20
	(11.16)	(11.30)	(11.33)	(9.85)	(9.77)	
ln(AuMIM)	0.31	0.12	0.32	0.46	0.13	–
	(5.96)	(1.51)	(5.24)	(5.24)	(1.05)	
No. of mandates	3,433	3,432	3,432	3,392	3,386	–
No. of obs.	360,459	309,148	323,378	255,396	206,477	–
Within-R^2	0.11	0.13	0.13	0.15	0.18	–

these specifications. But the coefficients on prior performance, PSxIM, and IMxIC, remain statistically significant and the increase in probability of being hired implied by these coefficients remain similar. In column 5, we impose all three requirements together. The opportunity set shrinks by roughly half in this specification as the unconditional probability jumps to almost 2%. In this quite stringent specification, the probability of hiring changes associated with our variables of interest is somewhat larger than those in Table 3.

Following Kuhnen (2009), we also implement a bootstrap procedure. For each mandate, we randomly select five firms from the unconstrained opportunity set and repeat the process 5,000 times. We reestimate the baseline regression (column 1 in Table 3) and calculate the average marginal effect across replications. For PSxIM and IMxIC, the averages are 9% and 6%, respectively. Both are smaller than the baseline model but still reliability positive; the standard deviations of PSxIM and IMxIC across replications (not reported in the table) are 3% so that the standard error across replications is extremely small.

VI. Post-Hiring Returns and the Opportunity Set

If connections reveal information that is helpful to predict future performance, then we expect higher future returns relative to the opportunity set when plan sponsors hire connected investment managers. An important feature of our analysis is that the opportunity set used to understand the counterfactual is investable.

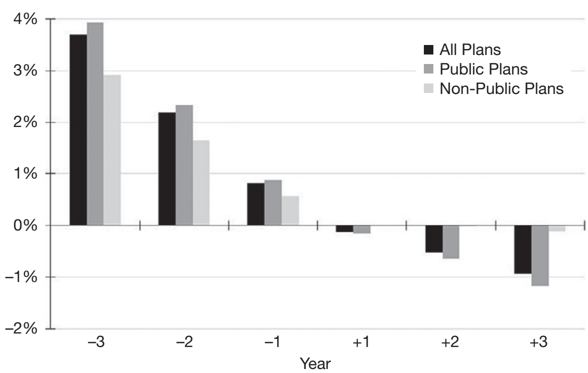
A. Average Post-Hiring Returns of the Counterfactual

We first tabulate cumulative excess returns of hired investment managers and those in the opportunity set. Figure 1 contains the difference in these excess returns (i.e., the difference between the excess returns of hired managers and those in the

FIGURE 1

Average Cumulative Excess Returns Differences Between Hired Firms and the Opportunity Set

Figure 1 shows average cumulative excess returns 1, 2, and 3 years before and after a selection decision. All returns are reported as difference in excess returns between hired investment managers and the opportunity set. Subsample definitions are as in prior tables.



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opportunity set), before and after the selection, computed using a calendar time approach. They can be interpreted as the return that a plan earned relative to the return that it could reasonably have earned. We report *t*-statistics based on standard errors that allow for autocorrelation and heteroscedasticity (Jegadeesh and Karceski (2009)).

Across the entire sample, the cumulative 3-year return difference prior to hiring is 3.71% (*t*-stat. = 11.97). This return chasing is evident in other horizons as well and is prominent in both public and nonpublic plans. Post-selection, however, plan sponsors do not show discernible selection ability. For the full sample, the post-selection 3-year return relative to the opportunity set is −0.93% (*t*-stat. = −2.66). Negative selection ability for a large sample is surprising. However, it is completely driven by the choices of public plans where the 3-year return relative to the opportunity set is −1.17% (*t*-stat. = −3.16), and where governance issues and agency problems are well-documented.[17] For nonpublic plans, average returns at every horizon are indistinguishable from zero.

B. Post-Hiring Return Regressions

Since both hired firms and those in the opportunity set can be connected (or not), it is possible to extract the influence of connections by controlling comparison groups in a regression-based approach. Specifically, we estimate regressions of the form:

$$
\begin{aligned}
\text{(2)} \qquad \mathrm{CER_{IM}}(+3) = {} & \beta_0 + \beta_1 \mathrm{HIRED_{PS,IM}} + \beta_2 \mathrm{PSxIM} + \beta_3 \mathrm{IMxIC} \\
& + \beta_4 \mathrm{PSxIM} \times \mathrm{HIRED_{PS,IM}} + \beta_5 \mathrm{IMxIC} \times \mathrm{HIRED_{PS,IM}} \\
& + \beta_6 \ln(\mathrm{AuM_{IM}}) + \mathrm{FE},
\end{aligned}
$$

where the dependent variable is the post-hiring 3-year cumulative excess returns. $\mathrm{Hired_{PS,IM}}$ is an indicator variable equal to 1 when the investment manager IM is hired by plan sponsor PS, and 0 for the opportunity set. Given this setup, various combinations of βs allow for comparisons between hired investment managers and the opportunity set, turning on and off connections in each group. All regressions include mandate-specific and investment manager fixed effects and *t*-statistics are based on standard errors clustered at the cross-product of year, style, and geography.

Following the format of the selection regression in Table 3, Panel A of Table 6 shows regressions specifications that use PSxIM, PSxIM(Pct), and PSxIM (Strong/NotStrong) as well as IMxIC. Generating various combinations of connected and unconnected managers among both hired managers and the opportunity set requires summing betas from equation (2). To facilitate inference, we provide the relevant comparisons in Panels B–D of Table 6. The nomenclature in these panels derived

[17]Some readers have suggested that diseconomies of scale might cause hired managers to underperform the opportunity set. Pástor, Stambaugh, and Taylor (2015) do not find any diseconomies of scale at the fund level. Rather, diseconomies, if they exist, do so only at the industry level or the style level. Since our opportunity set is constructed at style-year level, it is unlikely that diseconomies drive the difference in returns. In addition, the median ratio of mandate size to assets under management in our sample is only 0.1%, unlikely to generate performance differentials of the magnitude observed in the data.

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TABLE 6

Regressions of 3-Year Post-Hiring Returns on Connections

Panel A of Table 6 presents regression coefficients of regressions of 3-year post-hiring returns of the hired firms and the opportunity set on the listed independent variables. The other independent variables are defined in Table 3. Panels B and C show differences in returns between hired firms (H) and the opportunity set (OS) based on appropriate combinations of the coefficients on indicator variables in Panel A. Panel D shows differences in returns within various groups of hired firms, again based on combinations of coefficients from Panel A. Mnemonic NC stands for nonconnected. All regressions include mandate and investment manager fixed effects. To generate *t*-statistics, which appear in parentheses, we cluster at the style × geography × year level.

	1	2	3
Panel A. Regressions of 3-Year Return of Hired Firms and the Opportunity Set			
$\text{Hired}^{PS,IM}$	−1.261	−1.201	−1.260
	(−6.55)	(−6.43)	(−6.56)
PSxIM	0.013	–	–
	(0.19)		
PSxIM(Pct)	–	0.226	–
		(1.18)	
PSxIM(Strong)	–	–	−0.111
			(−0.45)
PSxIM(NotStrong)	–	–	0.016
			(0.24)
IMxIC	0.084	0.084	0.084
	(1.11)	(1.11)	(1.11)
$\ln(\text{AuM}^{IM})$	−1.787	−1.787	−1.787
	(−9.21)	(−9.22)	(9.21)
$\text{Hired}_{PS,IM}$ × PSxIM	0.074	–	–
	(0.25)		
$\text{Hired}_{PS,IM}$ × PSxIM(Pct)	–	−0.785	–
		(−0.90)	
$\text{Hired}_{PS,IM}$ × PSxIM(Strong)	–	–	0.531
			(0.57)
$\text{Hired}_{PS,IM}$ × PSxIM(NotStrong)	–	–	0.048
			(0.16)
$\text{Hired}_{PS,IM}$ × IMxIC	0.178	0.204	0.172
	(0.51)	(0.59)	(0.49)
No. of mandates	3,417	3,417	3,417
No. of obs.	314,652	314,652	314,652
Within-R^2	27.4	27.4	27.4
Panel B. Regression-Implied Return Differences Between Connected Hired (H) Investment Managers and NonConnected Opportunity Set, OS (NC)			
H (Connection ≡ PSxIM = 1, IMxIC = 0) − OS(NC)	−1.17	–	–
	(−3.97)		
H (Connection ≡ PSxIM(Pct) = 1, IMxIC = 0) − OS(NC)	–	−1.76	–
		(−2.09)	
H (Connection ≡ PSxIM(Strong) = 1, IMxIC = 0) − OS(NC)	–	–	−0.84
			(−0.92)
H (Connection ≡ PSxIM(NotStrong) = 0, IMxIC = 0) − OS(NC)	–	–	−1.20
			(−3.86)
H (Connection ≡ IMxIC = 1, PSxIM = 0) − OS(NC)	−1.00	−0.91	−1.00
	(−2.81)	(−2.67)	(−2.82)
Panel C. Regression-Implied Return Differences Between Connected Hired (H) Investment Managers and Connected Opportunity Set (OS)			
H − OS: Connection ≡ PSxIM = 1, IMxIC = 0	−1.19	–	–
	(−4.03)		
H − OS: Connection ≡ PSxIM(Pct) = 1, IMxIC = 0	–	−1.99	–
		(−2.35)	
H − OS: Connection ≡ PSxIM(Strong) = 1, IMxIC = 0	–	–	−0.73
			(−0.80)
H − OS: Connection ≡ PSxIM(NotStrong) = 1, IMxIC = 0	–	–	−1.21
			(−3.94)
H − OS: Connection ≡ IMxIC = 1, PSxIM = 0	−1.08	−1.00	−1.09
	(−2.99)	(−2.85)	(−2.99)

(continued on next page)

TABLE 6 (continued)

Regressions of 3-Year Post-Hiring Returns on Connections

Panel D. Regression-Implied Return Differences Between Connected Hired Investment Managers and Nonconnected (NC) Hired Investment Managers			
H (Connection ≡ PSxIM = 1, IMxIC = 0) − H(NC)	0.09 (0.29)	–	–
H (Connection ≡ PSxIM(Pct) = 1, IMxIC = 0) − H(NC)	–	−0.56 (−0.64)	–
H (Connection ≡ PSxIM(Strong) = 1, IMxIC = 0) − H(NC)	–	–	0.42 (0.45)
H (Connection ≡ PSxIM(NotStrong) = 1, IMxIC = 0) − H(NC)	–	–	0.06 (0.21)
H (Connection ≡ IMxIC = 1, PSxIM = 0) − H(NC)	0.26 (0.76)	0.29 (0.84)	0.26 (0.74)

from equation (2) is as follows: H refers to a hired investment manager, OS refers to the opportunity set, C refers to a connection which can be PSxIM or IMxIC, and NC refers to no connection.

C. Assessing Selection Ability

We start by comparing the returns of hired investment managers and the opportunity set when there are no connections in Panel A of Table 6. This difference is given by beta_1, the coefficient on $\text{Hired}_{\text{PS,IM}}$. The return difference ranges from −1.20 to −1.26% with *t*-statistics above 6.00 (in absolute value), and similar in magnitude to the unconditional difference of −0.93% in Figure 1.

Panel B of Table 6 shows return differences between connected hired investment managers and unconnected opportunity set, when connections are measured using the indicator variable PSxIM, using the percentage variable (PSxIM(Pct)), using connection strength (PSxIM(Strong/NotStrong)), or using IMxIC. For PSxIM, the return difference is −1.17% (*t*-stat. = −3.97), and for PSxIM(Pct) it is −1.76% (*t*-stat. = −2.09). When the connection is strong (PSxIM(Strong)), the difference in returns is only −0.84% (*t*-stat. = −092) but rises to −1.20% (*t*-stat. = −3.86), when the connections are not strong. Finally, when connections are between IMxIC, the return difference varies from −0.91 to −1.00%. The above results imply that plan sponsors display no ability in investment manager selection with respect to gross investment returns.

D. Assessing the Value of Connections

We now ask whether connections are associated with higher post-hiring returns. In the introduction, we noted three different views of the value of relationships. Connections could provide information about ability or be used to extract rents. It is also possible that connections are neither beneficial nor harmful for performance but are a means of establishing trust, which offers other benefits. We examine the empirical content of these hypotheses in this subsection, assessing the value of connections in three different ways.

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1. Return Differences Between Connected Hired Managers and Connected Opportunity Set

If connections are a conduit for information, one would expect to see higher returns for chosen managers. Using the notation defined earlier, the information hypothesis posits that returns of H&C (connected hired) are higher than those of OS&C (connected opportunity set).

Panel C of Table 6 shows return differences between hired investment managers and the opportunity set when connections are present in both (in the same five ways as Panel B). The return differences between hired managers and the opportunity set are about negative 1% (−1.19%, −1.99%, −0.73%, and −1.21% for PSxIM, PSxIM(Pct), PSxIM(Strong), and PSxIM(NotStrong), respectively. And when the connections are to investment consultants (IMxIC), the return differences vary from −1.00% to −1.09%. The fact that all the return differences in Panel C are negative is evidence against the hypothesis that connections are valuable source of information.

2. Differences in Differences

It may be the case that plan sponsors display singular inability to choose investment managers regardless of whether they are connected or not. In fact, the results from Panel B of Table 6 suggest that the return differences in Panel C of Table 6 may be "inflated" by the fact that, on average, plan sponsors choose investment managers who underperform relative to those in the opportunity set. To sharpen inferences, we examine the difference between return differences of the hired and opportunity set across the connected and unconnected. Using the earlier notation, we are interested in the difference of differences [(H&C − OS&C) − (H&NC − OS&NC)], captured by the coefficients of the interaction terms (β_4 and β_5) in equation (2).

If connections provided information on investment managers' ability, we expect plan sponsors to choose more skilled managers amongst the set of connected managers, with limited selection ability amongst the set of unconnected managers. In other words, the information hypothesis implies that (H&C − OS&C) > (H&NC − OS&NC). In contrast, the rent-extraction hypothesis, in which plan sponsors hire connected investment managers with poor ability, implies (H&C − OS&C) < (H&NC − OS&NC). Finally, the trust hypothesis implies no double difference in returns, (H&C − OS&C) = (H&NC − OS&NC).

Panel A of Table 6 shows that when using PSxIM, the β_4 coefficient is 0.07% but with a *t*-statistic of only 0.25. When we use PSxIM(Pct), PSxIM(Strong), and PSxIM(NotStrong) the β_4 coefficients are similarly small and all of them are statistically indistinguishable from zero. Moreover, the interaction term between the hired indicator variable and IMxIC is indistinguishable from zero in every specification in Panel A. In other words, controlling for general selection inability, selection decisions with connections between plan sponsors and investment managers are no better than selection decisions absent connections.

3. Return Differences Between Connected-Hired and Unconnected-Hired Managers

As a final test on the value of connections, we ignore the opportunity set altogether and focus on return differences within hired investment managers.

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The advantage of this test is that it does not rely on the empirical choices that we make in constructing the opportunity set. The disadvantage is that since it does not use the counterfactual, we are unable to rule out unobserved heterogeneity across different mandates. Therefore, we interpret the results in this subsection as additive to the overall evidence rather than definitive.

The first row of Panel D of Table 6 compares the returns of hired and connected investment managers (using PSxIM) with hired investment managers that have no connections. As before, the remaining rows use PSxIM(Pct), PSxIM(Strong), PSxIM(NotStrong), and IMxIC. In each and every case, the return differences between hired-and-connected investment managers and hired-but-unconnected investment managers are indistinguishable from zero.

E. Information Ratios

We also use information ratios, computed by scaling excess returns by their volatility. This serves two purposes. First, it addresses the variation in volatilities of excess returns across asset classes and/or investment styles. Second, it could be that connected investment managers deliver returns with lower volatilities. We use these information ratios in regressions equivalent to those in Table 6. Table 7 contains the results, presented in a format that parallels Table 6, with Panel A containing the regression estimates, and Panels B–D comparing hired and connected managers to various groups. Panel B of Table 7 shows that the difference in information ratios between hired and connected investment managers and the opportunity set is −0.21 (t-stat. = −4.97).

When connections are present in both the hired investment managers and the opportunity set, the differences in information ratios continue to be negative and large. For example, Panel C of Table 7 shows that the difference in information ratios of hired and connected investment managers compared to connected investment managers in the opportunity set is −0.21 (t-stat. = −5.11). As with the returns analysis, the difference-in-difference estimates from the coefficients on the interaction terms in the regressions also indicate that connections do not deliver higher information ratios; the interaction terms between the hired indicator variable and connection variables PSxIM, PSxIM(Pct), PSxIM(Strong/NotStrong), and IMxIC are all indistinguishable from zero. Finally, if we ignore the opportunity set entirely and focus only on selection decisions, Panel D of Table 7 shows that connections are not associated with high information ratios.

F. Fees

It is possible that the differences in returns (to the extent that there are any) are offset by lower fees. In unreported tests, we verify that there are no differences in fees between selected investment managers and the opportunity set. We also estimate regressions similar to those in Table 6 but with fees as the dependent variable. We find that connections are unrelated to fee levels, implying that lower fees are not a compensating differential.

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TABLE 7

Regressions of 3-Year Post-Hiring Information Ratios on Connections

Panel A of Table 7 presents regression coefficients of regressions of 3-year post-hiring information ratio of the hired firms and the opportunity set on the listed independent variables. The other independent variables are defined in Table 3. Panels B and C show differences in information ratio between hired firms (H) and the opportunity set (OS) based appropriate combinations of the coefficients on indicator variables in Panel A. Panel D shows differences in information ratio within various groups of hired firms, again based on combinations of coefficients from Panel A. Mnemonic NC stands for nonconnected. All regressions include mandate and investment manager fixed effects. To generate *t*-statistics, which appear in parentheses, we cluster at the style × geography × year level.

	1	2	3
Panel A. Regressions of 3-Year Information Ratios of Hired Firms and the Opportunity Set			
$\text{Hired}^{PS,IM}$	−0.167 (−5.90)	−0.175 (−6.61)	−0.167 (−5.89)
PSxIM	0.005 (0.51)	–	–
PSxIM(Pct)	–	0.029 (1.17)	–
PSxIM(Strong)	–	–	0.003 (0.10)
PSxIM(NotStrong)	–	–	0.005 (0.52)
IMxIC	0.026 (1.97)	0.026 (1.97)	0.026 (1.97)
$\ln(\text{AuM}^{IM})$	−0.214 (−10.41)	−0.214 (−10.41)	−0.214 (−10.41)
$\text{Hired}_{PS,IM}$ × PSxIM	−0.047 (−1.03)	–	–
$\text{Hired}_{PS,IM}$ × PSxIM(Pct)	–	−0.064 (−0.42)	–
$\text{Hired}_{PS,IM}$ × PSxIM(Strong)	–	–	0.111 (0.78)
$\text{Hired}_{PS,IM}$ × PSxIM(NotStrong)	–	–	−0.057 (−1.23)
$\text{Hired}_{PS,IM}$ × IMxIC	−0.001 (−0.02)	−0.005 (−0.10)	−0.004 (−0.08)
No. of mandates	3,417	3,417	3,417
No. of obs.	314,652	314,652	314,652
Within-R^2	40.5	40.5	40.5
Panel B. Regression-Implied Information Ratio Differences Between Connected Hired (H) Investment Managers and Nonconnected Opportunity Set, OS (NC)			
H (Connection ≡ PSxIM = 1, IMxIC = 0) − OS(NC)	−0.21 (−4.97)	–	–
H (Connection ≡ PSxIM(Pct) = 1, IMxIC = 0) − OS(NC)	–	−0.21 (−1.43)	–
H (Connection ≡ PSxIM(Strong) = 1, IMxIC = 0) − OS(NC)	–	–	−0.05 (−0.39)
H (Connection ≡ PSxIM(NotStrong) = 0, IMxIC = 0) − OS(NC)	–	–	−0.22 (−5.10)
H (Connection ≡ IMxIC = 1, PSxIM = 0) − OS(NC)	−0.14 (−2.69)	−0.15 (−3.09)	−0.14 (−2.72)
Panel C. Regression-Implied Information Ratio Differences Between Connected Hired (H) Investment Managers and Connected Opportunity Set (OS)			
H − OS: Connection ≡ PSxIM = 1, IMxIC = 0	−0.21 (−5.11)	–	–
H − OS: Connection ≡ PSxIM(Pct) = 1, IMxIC = 0	–	−0.24 (−1.61)	–
H − OS: Connection ≡ PSxIM(Strong) = 1, IMxIC = 0	–	–	−0.06 (−0.40)
H − OS: Connection ≡ PSxIM(NotStrong) = 1, IMxIC = 0	–	–	−0.22 (−5.22)
H − OS: Connection ≡ IMxIC = 1, PSxIM = 0	−0.17 (−3.18)	−0.18 (−3.57)	−0.17 (−3.21)

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TABLE 7 (continued)

Regressions of 3-Year Post-Hiring Information Ratios on Connections

Panel D. Regression-Implied Information Ratio Differences Between Connected Hired Investment Managers and Nonconnected (NC) Hired Investment Managers

H (Connection ≡ PSxIM = 1, IMxIC = 0) − H(NC)	−0.04 (−0.93)	–	–
H (Connection ≡ PSxIM(Pct) = 1, IMxIC = 0) − H(NC)	–	−0.03 (−0.23)	–
H (Connection ≡ PSxIM(Strong) = 1, IMxIC = 0) − H(NC)	–	–	0.11 (0.82)
H (Connection ≡ PSxIM(NotStrong) = 1, IMxIC = 0) − H(NC)	–	–	−0.05 (−1.14)
H (Connection ≡ IMxIC = 1, PSxIM = 0) − H(NC)	0.02 (0.53)	0.02 (0.46)	0.02 (0.47)

VII. Finalists

The analysis thus far relies on the construction of the counterfactual opportunity set, whether in its entirety or restricted in some way. In this section, we exploit the richness of the Fundmap data to examine the restricted opportunity set generated by plan sponsors themselves.

As plan sponsors do their due diligence, they narrow the opportunity set to a handful of managers referred to as a "finals list" in industry jargon. We use the text information in the comments field of the Fundmap database to extract the list of finalists for each mandate. For instance, a typical entry might state: "The system has hired Investec Asset Management to handle a $12.6 million international equity emerging markets strategy. Dimensional Fund Advisors and Westwood Global Investments were the other finalists." This type of data is not systematized or available for the full sample, so we manually identify the finalists and their investment products. We then replicate our analysis using nonselected finalists as the opportunity set. Our sample consists of 183 mandates, quite small relative to the main sample. As a result, we expect little power in being able to detect selection effects or differences in returns. On average, there are three firms in the finals stage, of which one is selected.

Panel A of Table 8 shows estimates from OLS regressions that predict selection relative to other finalists. The regressions exhibit notable consistency with the much larger sample in Table 3. Prior returns are still positively related to the selection decision, even though finalists are likely already selected on performance. The coefficient on PSxIM is positive but given the extremely small sample, has large standard errors. Connections between investment managers and consultants (IMxIC) are positively related to selection with a coefficient of 0.21% (*t*-stat. = 1.82). Thus, despite the small sample, the selection regression offers a degree of comfort – even in an extremely small but well-defined sample, connections seem to matter.

Panel B of Table 8 shows average cumulative excess returns in the 3 years before and after hiring for the selected firm as well as for the other finalists. Again, the pattern is remarkably consistent with the results in Figure 1. Three-year pre-hiring returns of hired managers are 2.82% higher than those of the other finalists. In

TABLE 8

Choice and Return Regressions for Finalists

Table 8 shows the sample of 183 mandates for which we identify the hired investment manager and the set of finalists. Panel A reports the OLS choice selection equation for this sample, equivalent to model 1 in Table 3. Panel B contains average cumulative excess returns 1, 2, and 3 years before and after a selection decision for hired firms and nonhired finalists. Panel C contains post-hiring return regressions equivalent to those in model 1 of Table 6. Panel D shows regression-implied connection return differences. To generate *t*-statistics, which appear in parentheses, we cluster at the style × geography × year level.

Panel A. OLS Selection Equation

	PSxIM	IMxIC	$CER_{IM}(-3)$	$\ln(AuM_{IM})$
Coefficient	0.053	0.213	1.525	−0.052
	(0.47)	(1.82)	(3.93)	(−2.64)

Panel B. Prehiring and Posthiring Cumulative Excess Returns

	Prehiring Years			Posthiring Years		
	−3	−2	−1	+1	+2	+3
Hired	7.18	3.81	0.77	0.24	0.09	0.59
	(8.35)	(5.77)	(2.20)	(0.86)	(0.32)	(1.64)
Nonhired	4.36	2.68	0.80	0.30	0.55	0.91
	(10.38)	(7.44)	(2.76)	(0.86)	(1.67)	(2.07)
Difference	2.82	1.14	−0.04	−0.05	−0.46	−0.31
	(4.09)	(2.33)	(−0.08)	(−0.18)	(−1.07)	(−0.46)

Panel C. Posthiring Return Regression

	Intercept	Hired	PSxIM	IMxIC	Hired × PSxIM	Hired × IMxIC	$\ln(AuM_{IM})$
Coefficient	1.76	−0.55	−1.51	−0.30	1.94	−0.09	−0.04
	(0.52)	(−0.45)	(−0.91)	(−0.21)	(0.98)	(−0.04)	(−0.13)

Panel D. Regression-Based Connection Return Differences

H (PSxIM = 1, IMxIC = 0) − OS (NC)	−0.13
	(−0.07)
H (PSxIM = 0, IMxIC = 1) − OS (NC)	−0.94
	(−0.59)
H (PSxIM = 1, IMxIC = 0) − OS (PSxIM = 1, IMxIC = 0)	1.39
	(0.77)
H (PSxIM = 0, IMxIC = 1) − OS (PSxIM = 0, IMxIC = 1)	−0.64
	(−0.35)
H (PSxIM = 1, IMxIC = 0) − H (PSxIM = 0, IMxIC = 0)	0.42
	(0.28)
H (PSxIM = 0, IMxIC = 1) − H (PSxIM = 0, IMxIC = 1)	−0.39
	(−0.20)

this precisely identified comparison group, hired managers do not outperform other finalists: the 3-year difference in post-hiring returns is −0.31% (*t*-stat. = −0.46).

We also estimate post-hiring return regressions equivalent to those in Table 6. Panel C of Table 8 shows these return regressions and Panel D shows regression implied differences in returns. The difference in 3-year returns between hired-and connected investment managers and other finalists who are unconnected is −0.13% (*t*-stat. = −0.08) using PSxIM and −0.94% (*t*-stat. = −0.59) using IMxIC. Even when both the hired investment manager and another finalist are connected (third and fourth rows of Panel D), the difference in returns remains statistically indistinguishable from zero. Finally, the return difference between hired-and-connected investment managers and hired but unconnected investment managers is also statistically insignificant. The small sample inevitably plays a role here, but regardless, the inference remains the same as the larger sample in Tables 6 and 7; there is no robust evidence that connections deliver higher returns.

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VIII. Conclusion

We study how plan sponsors including pension plans, endowments, foundations, and sovereign wealth funds choose public equity and fixed-income investment managers from an investable opportunity set. In addition to other factors such as size and past performance, personal connections between individuals employed by these institutions are related to selection probabilities. Importantly, the magnitude of the effect is comparable to that of past performance. Post-selection, there are no positive excess returns to selection based on connections. Viewing the selection process as a trade between plan sponsors and investment managers, it appears as if the gains to trade associated with connections are unequally shared – investment managers benefit from connections by larger received flows and, therefore, fees, but even at best, plan sponsors do not receive higher returns or lower fees. They may, of course, receive nonpecuniary or other compensating benefits that we are unable to measure.

Appendix A. Investment Mandates and Indices

Domestic Equity		
Capitalization	Style	Index
All	Core	Russell 3000 Core
	Value	Russell 3000 Value
	Growth	Russell 3000 Growth
Mega/large/mid-large	Core	Russell 1000 Core
	Value	Russell 1000 Value
	Growth	Russell 1000 Growth
Mid	Core	Russell Midcap Core
	Value	Russell Midcap Value
	Growth	Russell Midcap Growth
Small-mid/small/micro	Core	Russell 2000 Core
	Value	Russell 2000 Value
	Growth	Russell 2000 Growth

International Equity		
Capitalization	Style	Index
All	Core	MSCI ACWI All Core
	Value	MSCI ACWI All Value
	Growth	MSCI ACWI All Growth
Mega/large/mid-large/mid	Core	MSCI ACWI Large Core
	Value	MSCI ACWI Large Value
	Growth	MSCI ACWI Large Growth
Small-mid/small/micro	Core	MSCI ACWI Small Core
	Value	MSCI ACWI Small Value
	Growth	MSCI ACWI Small Growth

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Domestic Fixed Income		
Duration	Style	Index
All/long	Aggregate	Barclays Aggregate
Intermediate/short	Aggregate	Barclays Aggregate Inter/Short
All/long	Government	Barclays Government
Short	Government	Barclays Treasuries
All/long	Corporate	Barclays Corporate
Intermediate/short	Corporate	Barclays Corporate Inter/Short
All/long/intermediate/short	High yield	Barclays High Yield
All/long/intermediate/short	Mortgage	Barclays Mortgage
All/long/intermediate/short	Municipal	Barclays Municipal
All/long/intermediate/short	Convertibles	Barclays Convertibles
All/long/intermediate/short	Inflation	Barclays Inflation

International Fixed Income		
Style	Region	Index
Aggregate/government	Global	Barclays Global Aggregate
Corporate	Global	Barclays Global Corporate
High yield	Global	Barclays Global High Yield
Aggregate	Europe	Barclays Europe Aggregate
Government	Europe	Barclays Europe Government
Corporate	Europe	Barclays Europe Corporate
High yield	Europe	Barclays Europe High Yield
Aggregate/government/corporate/high-yield	U.K.	Barclays UK Aggregate
Aggregate/government/corporate/high-yield	Japan	Barclays Japan Aggregate
Aggregate/government/corporate/high-yield	Emerging	Barclays Emerging Aggregate
Aggregate/government/corporate/high-yield	Asia	Barclays Asia

Same for ACWI ex-U.S., EAFE, Emerging, Europe, North America, Japan, and the U.K.

Appendix B. Relationship Science Connections

We acquire data on connections from Relationship Science, a firm that specializes in relationships between individuals at three types of organizations:

- For-profit corporations, including Fortune 100 companies, law firms, accounting firms, and consulting firms.
- Financial institutions, including commercial banks, investment banks, private equity firms, wealth managers, and hedge funds.
- Nonprofits, including but not limited to public organizations, educational institutions, charities, foundations, endowments, and cause-based organizations.

At the time of acquiring the data, the database covered over 10.1 million individuals. The data are sourced from publicly verifiable data sources, including SEC records, court records, financial statements, and other such hard-coded information. Self-reported linkages and social network mediums such as LinkedIn are excluded.

The data cover a large range of individuals at each of these organizations. To construct our sample, we restrict the search in two ways.

- We provide a list of plan sponsors, investment consultants, and investment management firms derived from our Fundmap sample. That list includes 5,713 plan sponsor names, 5,564 investment manager names, and 800 consultant names. This is not a unique list because it allows for variations in organization names and locations.[18]

[18]For global investment management firms and consulting firms in which individuals have considerable interoffice mobility, it is possible that individuals in one geographic region are connected to

- We restrict the search for connections between two organizations to investment manager individuals with the following designations:
  - CEO
  - Chief Development Officer
  - Chief Investment Officer
  - Chief Marketing Officer
  - Chief of Staff
  - Chief Operating Officer
  - CFO
  - General Partner
  - Managing Partner
  - Department or Division Head
  - President
  - Senior Vice President
  - Vice Chairman
  - Director of Research
  - Senior Managing Director
  - Chief Officer
  - Executive Vice President Business Development / Corporate Development
  - Sales
  - Growth
  - Revenue
  - Solutions
  - Relationship Management

Relationship Science tracks common elements between individuals such as board participation, overlapping career history, investments, transaction participation, personal connections, seniority of the individuals, the duration of the overlap, and so forth. We do not observe these underlying common elements. But Relationship Science uses a proprietary algorithm that combines these elements to provide an indication of the strength of the connection (codified as "strong" in the data received by us).

Based on the above parameters, Relationship Science provides us with a customized data extract that identifies all possible connections. We are not permitted to disclose individual examples or connections but can provide a *hypothetical* example to explain the data and how we process it. Suppose that we seek to understand connections between Blackrock and the Government Pension Investment Fund (GPIF) of Japan. Relationship Science generates linkages between various individuals at these organizations. Let us suppose that there are 40 individuals at Blackrock, labeled B1 through B40, and 10 individuals at GPIF, labeled G1 through G10. The Cartesian product therefore potentially includes 400 connections. If there are no common elements between individuals, no data are recorded. In this example, suppose we observe that B3 and G3 establish a connection in 2008. Based on this setup, we regard Blackrock and

individuals in a different region. For example, suppose an asset management with headquarters in the U.S. but a regional New Zealand office is interacting with a Superannuation fund in Australia. If one considers only headquarters personnel in assessing relationships, one could easily miss connections between individuals in the New Zealand office and the Superannuation fund. Relationship Science permits the use of corporate hierarchies in generating connections, circumventing this issue.

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GPIF as having no connection before 2008, and a connection thereafter. Implicit in this process is the idea that once a connection is established it cannot be undone (i.e., it is not possible to “unfriend” someone). However, if the employee B3 leaves BlackRock to join AQR in 2015 and we observe that, then the connection between BlackRock and GPIF is terminated in 2015. The Relationship Science database also then records a connection between B3 (now at AQR) and G3 (at GPIF). Accordingly, we establish a connection between AQR and GPIF in 2015 (assuming that there was no connection between AQR and GPIF before). Thus, while it is not possible to unfriend a “person,” an organization might lose its connections.

The above description is based on direct linkages between individuals. Direct linkages occur, for example, when two individuals A and C are linked because they participated in the same transaction, served on the same board, or had other such closely tied relationships. Relationship Science also tracks indirect linkages. Indirect relationships are those in which A is linked to B, and B is linked to C. We use only the direct linkages in the article. In unreported tests, we rerun our tests with indirect linkages and find it has no material impact on our results. This is unsurprising because when there are direct connections, there are almost always indirect connections as well.

For a subsample, we observe the name of the organization that links two individuals as well as their formal roles in the connecting organization. The data contain over 13,000 connecting organizations with enormous variety in terms of their geography, organizational form, size, and other such attributes. To provide some color to such a large and diverse group, we classify them into four categories: nonprofit entities, financial institutions, educational institutions, and for-profit organizations. The percentage of observations in each group is 29%, 42%, 6%, and 23%, respectively. The data also contain over 10,000 formal roles of covered individuals. To provide a sense of the roles, the word clouds below provide a visual representation of the frequency and heterogeneity of roles for plan sponsors and investment managers, respectively. We also classify these roles into four parsimonious groups: chair-level, director-level, office-level, and member-level.

Using the data extract, we aggregate connections across individuals in each organization pair, summarizing connection information in three ways. First, we create an indicator variable if there is any connection between two organizations in a particular year. This is the primary connection variable used in most of our tests. It is general and,

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by definition, agnostic with respect to the type or nature of connections between individuals. Second, exploiting information on the strength of the connection (classified as "strong" or "not-strong"), we use indicator variables corresponding to each group. Third, we compute a continuous variable that scales the number of connections between two organizations by the product of the number of covered individuals in the plan and investment manager.

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