Hiring Your Friends: Evidence from the Market for Financial Economists*

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ABSTRACT

We study connections in academic hiring in a sample of finance doctoral graduates. Departments hire individuals with school connections to other recently hired faculty at a significantly greater rate than would otherwise be predicted. Similarly, schools exhibit an elevated propensity to hire individuals with last names that indicate a similar ethnic background to incumbent department members. School-connected hires tend to publish at a significantly higher rate than expected, a finding that is robust to a large number of model modifications and is stronger in more research-intensive departments. The evidence on school connections appears highly consistent with an employer information benefit from hiring based on direct school connections. Ethnic-connected hires tend to publish at lower-than-predicted rates, a finding that is robust to many, but not all modeling alterations. We cautiously interpret the ethnic evidence as suggestive of possible negative favoritism effects arising from this type of connected hiring.

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1. Introduction

A rich academic literature considers the process by which individuals secure jobs (e.g., Granovetter (1974)). A prominent issue in many of these investigations concerns the role of personal networks and connections in the job matching process. The degree to which these factors aid or hinder the efficiency of job matching clearly has important implications for hiring practices and labor market design. Some authors have identified a beneficial role for relying on personal connections in hiring, often emphasizing the potential for a decrease in noise as candidate employees and employers evaluate a potential match. However, there may also be negative consequences to using personal networks in hiring, for example, a loss in objectivity.

It is empirically challenging to identify the role of connections in the quality of the employee-employer match, as information on personal connections and networks are often imprecise, while at the same time objective measures of employee productivity are rarely available. In a few cases, researchers have been able to locate both types of information for a specific labor market or firm, while in other cases researchers have made indirect inferences from information on career trajectories, wages, or subjective performance.

In this paper, we provide direct evidence on these issues by studying the academic labor market for newly minted financial economists. Similar to the goals of Oyer (2006, 2008) and Kim, Morse, and Zingales (2009), we hope to provide both specific evidence to those interested in academic labor markets, but also more general insights that are likely to apply in other labor market settings with similar features. The key advantage of the academic labor market is that rich and objective data on individual measures of productivity are available in the form of information on research output.

We study a comprehensive sample of top-100 ranked finance departments from the early 1990s until the late 2000s. Within this sample, we examine the research performance of finance
doctoral graduates during the six-year period after these individuals obtain their initial tenure-stream faculty position. After establishing baseline models predicting research success, we proceed to ask whether measures of connections between the employer and the hired employee offer incremental explanatory power.

As a measure of whether there is a potential direct connection between a hiring department and a hired individual, we examine whether an incumbent member of the hiring department was recently awarded their doctoral degree from the same school as the new hire. If so, we refer to this as a school-connected hire. Consistent with the notion that connections matter in hiring decisions, we find that the rate of school-connected hiring is approximately 50% higher than what would be predicted by chance, even after accounting for many of the stochastic features of the job matching process in this market.

To capture whether there may be a potential indirect connection between the employer and the hired candidate, we consider the role of ethnic clustering in hiring decisions. Using an established ethnic name matching algorithm, we assign faculty last names into broad ethnic categories and characterize the ethnic composition of each department at the time of the hiring decisions. If a department falls in the far extreme of the ethnicity distribution, measured in alternative ways, and hires an individual from that ethnic group, we refer to this as an (indirect) ethnic-connected hire. Similar to school connections, the rate of ethnic connections in hiring is approximately 50% greater than would otherwise be expected.

Our evidence on inflated rates of school and ethnic connections in the job matching process parallels some of the findings in the literature on demographic segregation in labor markets (e.g., Hellerstein and Neumark (2008))). The more novel feature of our study is that we offer evidence on the relation between apparent biases in job matching and subsequent on-the-job performance. Our findings on this dimension are illuminating. Considering first school-
connections, we find that school-connected hires publish on the order of 30% more articles in elite finance journals compared to other hires, holding hiring school and doctoral program rankings fixed. This evidence suggests that the observed tendency towards hiring connected individuals may aid employers in securing superior talent.

Our findings on school connections and performance appear robust to a wide variety of model alterations and are substantially stronger for higher-ranked sample schools. The results do weaken somewhat when we include hiring-school fixed effects rather than solely controlling for a measure of hiring-school quality, but the key coefficient of interest remains significant in some of these models when we consider the sample as a whole, and all of these models when we restrict attention to higher-ranked schools. Thus, taken as a whole, the evidence appears fairly convincing of a positive relation between school-connected hiring and subsequent performance for research-intensive schools.

While there are multiple possible explanations of this school-connection evidence, our favored interpretation is that school connections allow the hiring school access to superior information about the candidate (an information channel) that dwarfs any loss of objectivity in using connections in the hiring process (a favoritism channel). There are some other possible interpretations for the evidence, for example, that individuals are more productive when working alongside their connections and/or that observably stronger candidates (based on public information) are more willing to join a department where they have a school connection. While we cannot entirely rule out all of these alternatives, we present several auxiliary tests that suggest that they are unlikely to fully explain the constellation of findings we present.

When we turn to ethnic connections, we find that these hiring outcomes are associated with an approximately 20% lower level of publication success, holding hiring-school quality and other controls constant. Thus, while there appears to be an ethnic factor in hiring, this
performance evidence suggests a possible negative role for this factor in securing research talent. These findings are robust to a wide set of model perturbations, but they are not robust to the inclusion of hiring-school fixed effects. Thus, we are more circumspect in our interpretation of the performance evidence related to ethnic connections, as we cannot dismiss the possibility that the ethnic-connection variable proxies for lack of attractiveness of a department on unobservable (to us) dimensions. We do attempt to add additional controls to account for this possibility, for example, a measure of faculty salaries, but it remains possible that these additional controls are inadequate. While less compelling than the school-connection findings, the ethnic-connection results are at least consistent with the presence of favoritism in which a lack of objectivity arising from some connections can lead to hiring less productive employees.

The rest of the paper is organized as follows. In section 2, we survey the associated literature, develop our hypotheses, and outline our empirical strategy. In section 3, we describe the sample and examine summary statistics. In section 4, we investigate whether the rates of school and ethnic connections in hiring are inflated relative to expectations. Section 5 examines the role of connections in predicting subsequent research success. Concluding thoughts and observations appear in section 6.

2. Personal Networks in Hiring

2.1 Theoretical considerations

Job matching, as modeled by Jovanovic (1979) and others, can be a noisy process. Simon and Warner (1992) consider the role of personal networks in this search process by assuming that networks reduce noise in information flows between the employers and potential employees. This leads to a number of interesting implications regarding job matching and career trajectories that have been explored by several authors (e.g., Dustmann, Glitz, Schoenberg, and
Brücker (2016) and Brown, Setren, and Topa (2016)). In the case of the academic labor market, the value of an individual to a hiring organization is likely much more homogeneous across employers than in other markets, as research production is highly individual and there are strong common elements in how similarly-ranked schools value research output. If a personal network allows a hiring organization access to superior information flows on a candidate's abilities, we would expect this to lead to the hiring of candidates through the network with greater-than-expected talents, holding other factors constant.

Turning to other possible influences of personal networks, a voluminous literature in sociology demonstrates that individuals tend to choose to group together based on similar personal characteristics (McPherson, Smith-Lovin, and Cook (2001)). Consistent with this behavior, several studies of hiring behavior detect evidence of a preference for employers to hire candidates who are similar to individuals in the hiring organization (e.g., Giuliano, Levine, and Leonard (2009)). While in many cases this may reflect the aforementioned information channel, in other cases, it may reflect an independent preference to hire someone with a shared background or experience, leading possibly to a lower hiring threshold and lower ability workers. Evidence along these lines, which we refer to as favoritism, is provided by Beaman and Magruder (2012).

Alternative theories of personal networks in hiring emphasize the role of connections in enhanced productivity via peer effects or personal "fit" related complementarities (e.g., Kugler (2003)). Interestingly, Kim, Morse, and Zingales (2009) find evidence along these lines as indicated by significant school-level fixed effects in explaining economics and finance faculty research productivity in the 1970s and 1980s. However, they report that this relation disappears starting in the 1990s, an outcome they attribute to decreasing barriers to inter-school collaboration arising from technology. Given that our sample starts in the 1990s, these peer or fit
effects may not be an important concern in our sample. However, notwithstanding the Kim, Morse, and Zingales (2009) evidence, we do consider below whether team production/peer effects may drive some of our findings.

2.2 Prior evidence on personal networks in hiring and performance

Topa (2011) provides a general overview of the literature on the role of job referrals in the labor market (see also Hoffman (2017)). With regard to the specific issue of the role of referrals from an employing firm's perspective, Fernandez, Castilla, and Moore (2000) and Castilla (2005) report that call center workers who are hired by referral tend to display relatively higher rates of productivity. Using an employer's subjective measure of productivity, Pinkston (2012) provides evidence that workers hired through prior employment networks tend to display superior performance, but this finding does not extend to workers hired through non-professional networks (i.e., friends and family). The Pinkston (2012) evidence suggests the presence of both positive information effects and negative favoritism effects from referrals that vary with the type of connection.

Two important recent papers exploit rich datasets to closely examine the role of referrals from the perspective of an employer attempting to identify talent. In a study of 9 firms, Burks, Cowgill, Hoffman, and Housman (2015) present evidence that workers hired by referral sometimes outperform others, most notably including patenting activity by high-tech workers. They detect other benefits of hiring by referral, including lower turnover and recruiting costs. In addition, they uncover a strong preference for firms to hire referred applicants over others. In a careful experiment using an online hiring marketplace, Pallais and Sands (2016) present
compelling evidence that referred workers outperform others. This finding appears to be driven both by information effects and team production peer effects.  

Our study is closely related to these recent studies and thus has the potential to complement these authors' findings. The distinguishing feature of our study is that we examine organizations in which identifying talent is likely the most important choice in determining organizational success. Since we have data on a large part of this market, we are able to study a fairly comprehensive set of employees and employers, and thus some general inferences about how a labor market of this type clears may be ascertained. Interestingly, the human-capital-intensive nature of the positions we study and the hierarchical nature of sample employers has many parallels with the market for lawyers, a market for which Oyer and Schaefer (2016) detect a role for both school connections and geography in job matching.

2.3 Academic labor markets

Several prior studies examine the labor market for economists in general and/or financial economists in particular. Collectively, these studies provide an overview of how the market for new economists functions. In addition, they provide context for many of our later modeling choices. The novel feature of our study for this particular literature is that we are the first to consider the role of connections on potentially affecting job-matching decisions.

Oyer (2006, 2008) presents evidence of a relatively efficient allocation of new Ph.D. economists to schools at any point in time, but with some persistent human-capital effects

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1 While referrals may help identify superior candidates, interesting recent evidence by Hoffman, Kahn, and Li (2017) suggests that relying on subjective judgment/discretion in some hiring decisions can lead to poorer hiring outcomes. This suggests that there are limits to relying on soft information in some hiring settings.

2 For a comprehensive overview of the market for economists, see Siegfried and Stock (1999). For an insightful discussion of some new developments in this market intended to enhance the efficiency of matches between schools and candidates, see Coles et al. (2010) and Coles, Kushnir, and Niederle (2013).
depending on exogenous elements of an individual's initial job assignment. Conley and Önder (2014) report that there is substantial noise in identifying research talent at the time the doctoral degree is granted. Chen, Liu, and Billger (2013) identify a growing international element to the U.S. academic labor market for economists. Terviö (2011) reports some clustering of economists together by the research approach/style of their doctoral training.

Turning specifically to studies that examine how new doctoral graduates are allocated in the finance academic labor market, Flagg, Gilley, and Park (2011) and Volkov, Chira, and Premti (2016) examine what factors predict faculty placements into relatively highly-ranked finance departments. Not surprisingly, the ranking of the doctoral program is a very strong predictor of placement success. These studies present a picture of a market in which a candidate's most important objective is to land at a highly ranked school, while a school's main objective is to hire candidates with the most research potential. The evidence indicates that there is some noise in this process, and also that idiosyncratic factors may play a secondary role in market clearing. Our investigation specifically considers the potential role of connections in affecting the information-related and idiosyncratic components of the job match.

If the labor market we study is driven largely by the primary objectives of the prospective employee and employer (i.e., to secure the highest ranked job and most talented researcher respectively), we would expect little to matter in predicting post-hire research success except for the quality of the hiring department. As reported by Smeets, Warzynski, and Coupé (2006) for economists in general, and by Chan, Chen, and Fung (2009) for financial economists, hiring department ranking is the most important predictor of a new faculty member's research productivity. However, both of these studies detect some residual positive role of the quality of the doctoral program in predicting research success (i.e., pedigree matters after controlling for
placement), perhaps reflecting certain market frictions or the presence of substantive secondary objectives in some job matching decisions.

2.4 Empirical Strategy

Our focus on the finance academic labor market is largely driven by the availability of detailed data on the composition of hiring organizations and job candidates for a long time period. The finance market is more homogeneous than the market for economists in general, and thus job matching based on desired areas of research emphasis is a smaller issue. Doctoral programs in finance are also much smaller than in economics, so there is generally more heterogeneity in where doctoral degrees are earned in a typical department. These features may enhance our ability to detect a role for connections in job matching.

While we do not provide a formal theoretical model, we sketch a framework for thinking about how the factors discussed above may become evident in the data. In any given year, the set of available faculty positions and new doctoral degree faculty candidates can be viewed as largely exogenous. Hiring schools search primarily for the candidates with the highest research potential, with some idiosyncratic factors (teaching needs, research tastes, etc.) mattering to a secondary degree. Candidates primarily try to join the highest-ranked department possible, with again some secondary factors potentially playing a role (geographic preferences, match of research interests, etc.). The market clears in a relatively short period of time in the winter of each year, with schools and candidates collecting information by first reading application materials and then conducting conference and on-campus interviews.

If a hiring school has a direct connection with some candidates that provides them with superior information on research talents, they may have the ability to select better than average candidates from this pool. This informational channel suggests (a) an abnormally high
preference to hire school-connected candidates, and (b) better post-hiring research performance for these individuals after they are hired, controlling for other observables. While there are multiple ways to measure connections that may lead to enhanced information flows, a direct overlap between an incumbent faculty member and the candidate during their doctoral studies would appear to be a particularly useful channel to gather reliable information on research potential (creativity, work ethic, technical skills, etc.).

It is possible that common ethnic backgrounds may lead to enhanced information flows in job matching, but this would appear to us to be unlikely given the likely indirect nature of most ethnic connections. On the other hand, favoritism concerns could be present for ethnic connections, as individuals in hiring departments may be willing to sacrifice research potential on the margin to hire someone from a shared ethnic background. Assuming the information channel is small or negligible relative to the favoritism channel for these connections, we would expect to observe (a) an inflated preference to hire ethnic-connected candidates, and (b) poorer post-hire research performance for these candidates after controlling for other observables. We note that favoritism may also play a role in school-connected hiring, so for both types of connections, we effectively can only ascertain the net relation (i.e., the net positive effect relation from information flows/other beneficial channels less any negative favoritism role).

Our discussion above focuses on the role of connections in raising or lowering the expected research potential of acquired new talent at the time of the hiring decision. In our empirical analysis, we will use early-career research productivity as a measure of this research potential. It is possible that connections also play a causal role in affecting whether research potential is fully realized. In particular, connected individuals may be more productive because

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3 It is also possible that candidates have a preference to join departments with which they share connections, thus lowering their acceptance threshold for joining these departments and potentially leading to better-than-expected performance. We consider this additional supply-side possibility in some of our analysis below.
they work well together and/or because connections proxy for a commitment to mentoring new faculty. These considerations would also predict better-than-expected performance by connected individuals. To consider these alternatives, wherever we find a positive role for connections and performance, we investigate whether connections proxy for a supportive research environment by examining the productivity of a department's other young researchers hired around the same time as a connected hire. In addition, we examine whether connected hires are relatively more likely to coauthor with departmental colleagues.

3. Sample Selection and Characteristics

3.1 Identifying departments, faculty, and rookie hires

We identify all U.S.-located finance departments ranked within the top one hundred departments based on publication output in the four elite journals tracked by Arizona State University in their well-known ranking of finance departments. We include a department if it achieves a top 100 rank based on either 1990-1999 or 2000-2009 publication output. This procedure yields a list of 102 schools, a sample that should include most domestic departments that place a heavy emphasis on research output when hiring new faculty. Given our sampling rule, our inferences should be understood to be informative for the subpopulation of the highest ranked organizations within a much larger market.

For the identified set of departments, we identify the composition of the tenure-stream finance-department faculty each year from 1991 to 2006, where a year reference pertains to the start of the academic year in question (i.e., 1991 means September/Fall of 1991). This sample period is dictated by the availability of various Hasselback directories of finance departments which were published on an approximate bi-annual basis from the early 1990s until the late
These directories include a list of the faculty at each department at the start of a given academic year, along with information on the year each individual began their employment at the school (start year) and the school and year where they received their doctoral degree (Ph.D. school, Ph.D. year).

The intermittent nature of the Hasselback directories, coupled with missing starting year information for some faculty, requires an algorithm to interpolate the composition of the faculty in years between directories when an exact start date is unavailable. The algorithm we use is detailed in the appendix, but none of our main results are affected by modifications to this algorithm, as in most cases the composition of the faculty at any point in time is unambiguous. In the limited number of cases in which we do not have a reported start year, an individual’s starting year at a school is the first year that the algorithm assigns the individual to the school.

All tenure-stream faculty who start teaching at a school between two years before and one year after earning their Ph.D. degree are considered rookie hires. This allows for small errors/inconsistencies in reported dates, along with cases in which a person finishes the degree after starting employment or secures a position shortly after the degree is completed. As we report in Table 1, the final sample includes 740 rookie faculty hires from 1991 to 2006 in the 102 highly-ranked departments in our sample.

3.2 Measuring research output

Clearly, a key factor predicting the research output of new faculty will be the research output of the existing faculty in the hiring department. Our goal is to understand whether certain new faculty members publish more or less on the margin after controlling for departmental

\[ \text{Equation} \]

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\[ ^4 \text{The Hasselback directories have been used in prior studies of finance departments. See, for example, Borokhovich, Bricker, Brunarsi, and Simkins (1995) and Chan, Chen, and Steiner (2002).} \]
research profile. To measure a department’s research stature at any point in time, we aggregate the entire history of past publications of all individuals who are members of the department as of that point in time.\(^5\) Our default departmental quality measures are based on the aggregate number of past publications by all departmental faculty members. As a robustness check, we later experiment with using only the recent history of publications for a school's faculty. We do not adjust for number of coauthors, citations, or article length, as these adjustments would be quite cumbersome and would surely result in metrics that are highly correlated with our default choice.

Since it is widely recognized that there are three top finance journals, we create one departmental quality measure that represents the total publication output in these three elite journals, normalized by the number of tenure-stream faculty members.\(^6\) As a close alternative to the top 3 measure, we create an alternative top 5 measure based on a slightly broader set of five elite journals. While most highly-ranked departments emphasize elite publications, certainly other publications carry weight, particularly for the lower-ranked schools in our sample. Thus, we create an all-encompassing measure of total output in a broad set of 21 journals (the top 21 measure). The journals identified for these sets are based on past studies of finance scholarly productivity and are discussed in detail in the appendix. In our modeling of an individual's research success, we also control for the quality of the department of Ph.D.-degree granting school. If an individual graduates from a non-sample department (foreign programs, unranked

\(^5\) Note that this procedure gives departments “credit” for publications of an existing faculty member, even when the individual worked at a different school when the research was published. Given the data available to us, it is not feasible to track moves of all faculty across schools in all years. Moreover, in our observation, departmental research expectations and perceptions of department quality are often based on the past success of members of the faculty, independent of whether they were at the current employer when some of this success occurred.

\(^6\) These journals are the *Journal of Finance*, the *Journal of Financial Economics*, and the *Review of Financial Studies*. These are widely recognized as the most elite influential finance journals and constitute 3 of the 4 journals that enter the Arizona State rankings. These are also the only 3 journals that are included in the well-known UT Dallas business school research database.
departments), we assign the Ph.D. program quality variables a value of 0 and include in the associated models a dummy variable indicating a missing doctoral program quality rating.

Following Conley and Önder (2014), we measure initial publication success based on publications during an individual's first six years after the hire (a window corresponding to the tenure clock at many schools) in the same journals that are used to measure departmental quality. We consider individual output in the top 3 journals, the top 5 journals, and the top 21 journals, and also a weighted measure based on output in the 21 top journals weighted by the relative research ranking of the journals in this set (referred to as the weighted top 21 measure). Details on the weighting procedure are reported in the appendix.

Sample summary statistics for measures of department quality and individual publication success are reported in the latter rows of Table 1. Not surprisingly, individuals tend to graduate from more elite programs than the department that hires them, so the doctoral program quality metrics substantially exceed the metrics for hiring departments. The data also indicate that a large number of new faculty publications in our sample appear in the most elite journals. For example, fully 67% (83%) of published articles by new faculty in their first six years in the broad set of 21 outlets we track appear in the top-3 (top-5) journals. Moreover, when we randomly select 50 individuals and search for all refereed publications in any outlet, we find that 90% appear in the top 21 tracked outlets. All of these figures are greater when we consider the higher-ranked schools within our sample. These statistics lend support to our focus on the more elite journals as a primary measure of research success.

3.3 Identifying school-connected rookie faculty hires

For all rookie hires, we identify faculty members at the hiring department who graduated from the same degree-granting institution. While there may be connections between rookie hires
and individuals who graduated from the same program many years apart (e.g., common advisors, common acquaintances), we expect school connections to be substantially stronger if the individuals overlapped with each other while in the program. Thus, our primary variable for detecting a school-connected hire assumes a value of 1 when there is at least one existing faculty member who graduated from the same school within four years of the rookie hire’s degree year. In our robustness checks, we experiment with measures that consider alternative overlap windows and/or scaled measures of overlap. As we report in the first row of the first column of Table 2, the overall school-connected frequency for sample rookie hires is 13.78%.

3.4 Identifying ethnically-connected rookie faculty hires

We identify likely ethnicities of individuals by using the Ambekar et al. (2009) algorithm that exploits open source data and hidden Markov modeling to match last names to the most likely ethnic origin of the name. This algorithm has been shown to generate aggregate statistics on ethnicities that closely match self-reported census figures, and it has been exploited in prior research by financial economists (e.g., Pool, Stoffman, and Yonker (2015)). The algorithm has some similar features to the Kerr and Lincoln (2010) technique for assigning likely ethnic origin categories to inventors. We rely on the Ambekar et al. (2009) algorithm because it can be easily implemented using an open-source online calculator (see Pool, Stoffman, and Yonker (2015) for details). Related approaches require access to unique commercial marketing databases. While surely there will be noise and errors in the process of assigning likely ethnicities, the algorithm we select provides an efficient and objective way of grouping faculty into broad ethnic-origin groups.

We rely on the most detailed ethnic categorization scheme that is available for all sample last names, resulting in an assignment of every last name to one of eight categories. Since three
of these categories have small representation in the sample (African, Muslim, and Eastern European, all with under 5%), we group these together into a single group which we label “Other.” The remaining five categories, all with substantial sample representation, are British (38.29%), West European (15.09%), Greater East Asian (11.86%), Jewish (15.81%), and Indian Subcontinent (8.55%).

To identifying hiring decisions with a potential ethnic influence, we first determine whether a hiring department appears abnormally tilted towards a certain ethnicity. To identify departmental ethnic tilt, we predict the expected fraction of the faculty in each ethnicity by conducting a sample wide predictive regression for each ethnicity. The dependent variable in these regressions is the percent of faculty in a given school-year of a given ethnicity, and the independent variables include the departmental top-3 research output metric, year dummies, region dummies (dividing the U.S. into six broad geographic regions), a large urban location dummy, and a faculty size measure (additional details in the appendix).

We categorize the department as tilted towards an ethnicity in a given year if the corresponding residual from the ethnic regression (actual rate minus predicted rate) is in the top decile. Importantly, any rookie hire is excluded from the prediction model in determining whether there is an ethnic tilt at the time of the hire. An event is then coded as an ethically-connected hire if the individual is from an ethnicity towards which the department is tilted. In our robustness checks, we consider measures of ethnic tilt that are not based on these predictive regression residuals. As we report in the first row of the second column of Table 2, using our default measure, the overall ethnic-connected frequency for sample rookie hiring is 22.70%.

4. Connections and Hiring Decisions
To investigate whether there is a relation between connections and job matching, we characterize what the distribution of connections would look like if connections were irrelevant for job matching. We then examine where the observed rate of connections falls relative to this distribution. We adopt a procedure that is similar to other recent non-parametric approaches for detecting segregation in labor markets after conditioning on potentially relevant covariates (e.g., Hellerstein and Neumark (2008), Åslund and Skans (2009, 2010)). Similar to these authors, we derive a distribution under the null hypothesis of no selection/discrimination by first sorting into cells based on the relevant covariates, and then randomly assigning individuals to positions within each cell. The informativeness of comparisons of the actual distribution to the derived distribution will depend on the adequacy of the first-stage sort in controlling for other relevant stochastic features of the job matching process. Since we have only a rudimentary understanding of an appropriate initial sort, we experiment with several alternatives.

Before conducting the full Monte Carlo analysis, we first consider five simple reshuffles of the data in which sample hires are deterministically assigned to alternative positions that are "close" in some sense to the actual position they obtain. The first reshuffle assigns every rookie hire to the closest hiring school in ranking in the same year within the five closest ranked degree-granting schools supplying sample rookies that year. The second reshuffle selects the school joined by the rookie from the closest ranked degree program in the same year within the set of rookies going to the five closest ranked hiring schools. The third and fourth reshuffles are the same as the first and second respectively, but only after first imposing the additional requirement that the assigned school for the reshuffling is in the same broad geographic region as the actual
hiring school. The fifth reshuffle simply assigns each rookie hire to the next closest ranked hiring school in the same year.7

The implied rates of school-connected (ethnic-connected) hiring for each of these job reshuffles are reported in column 1 (column 2) of Panel A of Table 2. As these figures indicate, in all cases the implied rates of connected hiring based on these reshuffles are substantially smaller than the corresponding actual connected hiring rate. In most cases, the actual rate is approximately 1.5 times the rate based on the reshuffles, suggesting a bias towards hiring individuals with school or ethnic connections of substantial magnitude (on the order of 50%).

Turning next to simulations, we consider distributions based on 1,000 simulated samples (reshuffles) in which we first pre-sort hiring events into cells based on certain covariates and then randomly assign individuals to jobs within the cell. In all simulated samples, individuals are allowed to be assigned to the actual position they obtain, and every filled position is assigned to exactly one rookie hire from the corresponding annual cohort (and vice versa). In the first sort, we randomly assign each rookie hire to a position within the set of schools that fall in the same tercile of hiring school rankings in the sample year while also hiring from the same tercile of doctoral programs in the sample year (resulting in $3 \times 3 = 9$ cells from the pre-sort).8

As we report in the first row of Panel B of Table 2, the mean rates of school-connected (ethnic-connected) hiring within these 1,000 simulated sample reshufflings of 9.02% (14.18%) is substantially smaller than the corresponding rate observed in the actual data of 13.78% (22.70%), suggesting again an observed rate that is inflated above the predicted rate by more

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7 See the appendix for details on the geographic assignment procedure. In reshuffles 1-5, we allow the possibility that multiple individuals take the same position if the position is the closest match according to the imposed criterion. If we instead use a randomization device to select unique alternative positions, the resulting figures are substantively unchanged.

8 We use rankings based on publications in top-3 outlets in all sorting and randomization procedures.
than 50%. Moreover, the actual rates are larger than the corresponding rates in all 1,000 simulated samples, indicating a high level of statistical significance.\textsuperscript{9}

Given the findings of Terviö (2011) of clustering in research specializations across economics departments, one might be concerned that schools or ethnicities specialize in certain types of finance research, thus leading to an inflated rate of connected hiring that has little do to with our hypotheses. To investigate, we have created 1,000 reshuffles using a procedure in which each hired individual is randomly assigned to a school that hired in their research area (four distinct areas, categorization procedure discussed below) and that falls in the same hiring department-quality tercile. As we report in the second row of Panel B of Table 2, the mean rates of connected hiring (both school and ethnic) using these simulated samples are again substantially smaller than the actual rate. Moreover, the actual rate falls again in the far upper tail of the simulated distribution. A similar conclusion applies if we pre-sort on research area and doctoral program quality tercile (row three of Panel B of Table 2).

To consider alternatives that do not condition on hiring school quality, we consider 1,000 simulated samples in which we (a) randomly assign individuals to positions within their annual doctoral program quintile cohort, and (b) randomly assign individuals to positions within their annual cohort with no sorting on school or individual characteristics. As we report in the final two rows of Table 2, our findings using these alternatives continue to indicate a significantly lower rate of connected hiring under the null than what is observed, both from an economic and statistical significance perspective.\textsuperscript{10}

\textsuperscript{9} We also have used the simulated samples in row 1 of Panel B of Table 2 to investigate whether there is a geographic-proximity element to hiring decisions. The median distance between the doctoral school and the hiring school in the actual data is insignificantly larger than the median of the median for the 1,000 simulated samples, providing no evidence for the notion that geographic closeness is a factor in job matching in this market.

\textsuperscript{10} The Table 2 findings on ethnic connections are similar if we use a simple annual cohort decile to define ethnic-connected hires (i.e., the hire is in a group for which the department is in the (unadjusted) top decile).
5. Connections and Performance

We now turn to the main question of whether connections in hiring decisions are systematically informative in predicting publication success, first considering school connections and later turning to ethnic connections. Our basic approach is to estimate regression models of an individual's publication success as a function of a connection dummy variable and other controls. These controls include hiring department quality, doctoral program quality, and year dummies.

We expect the departmental quality measure to be positive and highly significant, as higher ranked departments should both attract the highest research potential scholars and cultivate/incentivize the research efforts of these new hires to the greatest degree. The sign on the doctoral program quality variable is ambiguous, as it is unclear whether there should be any marginal information content in the quality of the doctoral program. Some prior research suggests a positive residual role for doctoral program quality in predicting research success.

5.1 School connections

In columns 1-4 of Panel A of Table 3, we report regression estimates predicting publication success of new hires as a function of hiring school quality, doctoral program quality, year dummies, and the school connection variable. Standard errors are clustered by hiring school. Each column uses a different measure of research success based on top 3, top 5, top 21, and weighted top 21 publications respectively. Not surprisingly, the coefficients on hiring school quality are positive and significant, indicating that rookie hire publication success is positively and significantly related to the research stature of the hiring department. Interestingly, the coefficient on the doctoral school quality variable is in most cases small, positive, and
significant, indicating marginal positive information content regarding eventual publication success in school-degree quality. This could reflect frictions in the matching process in which observably more promising candidates sometimes under-match to hiring schools.\textsuperscript{11}

Turning to the key variable of interest, school connections, all of the estimates in columns 1-4 of Panel A of Table 3 indicate a positive and significant coefficient of substantive magnitude. For example, the coefficient in column 1 for articles in top 3 journals indicates that school-connected individuals tend to publish .386 articles more on average than what would otherwise be predicted. Measured relative to the mean expected publication rate of slightly over one in these outlets (sample mean = 1.19), the implied relation appears to be economically significant (over 30%). The coefficients in the other columns are of similar economic magnitude. These positive and significant coefficients support the information benefits hypothesis from connected hiring discussed earlier.

To explore the robustness of these initial findings, in columns 5-8 of Panel A of Table 3 we add hiring school fixed effects to the estimated models. Given that many departments only hire sporadically, power may be low in these models as they rely entirely on within-school variation. As we report, the results on the school connection variable in these models are indeed weaker, although all point estimates are positive and have magnitudes that are at least three-quarters of the corresponding earlier (columns 1-4) model estimates. In the case of the broader measures using the unweighted and weighted top 21 measures of publication success, the coefficients are significant at the 10% and 5% levels respectively. These findings provide some support for the hypothesis that school connections are related to rookie publication success,

\textsuperscript{11} There are many frictions that could generate this result including incentive problems at top Ph.D. schools with multiple strong candidates, agency problems at hiring schools with some individuals seeking to avoid stronger candidates, or idiosyncratic preferences by promising scholars that result in them at times choosing schools lower in the academic hierarchy than the highest rank school available to them. Our main findings regarding school/ethnic connections and research success are unaltered if we drop the Ph.D. quality variable from all models.
although they are weaker than the estimates from models with no hiring school fixed effects. Given these differences, we consider in more detail below the potential role of additional hiring school characteristics that may be correlated with research success.

The findings of Oyer and Schaefer (2016) suggest that more prestigious employers are willing to overcome larger frictions to match with top talent. This suggests the possibility that higher ranked employers may be more willing to use school connections in hiring decisions if they provide an informational advantage, while perhaps at the same time being less likely to succumb to favoritism that could lead to hiring below-par candidates. Moreover, higher-ranked schools are more likely to use research output in top outlets as their primary measure of faculty success/productivity. All of these considerations suggest a more positive and significant relation for school connections in performance at higher ranked schools. Thus, in Panel B of Table 3, we restrict attention to hiring schools ranked above the sample median.

As the figures in the table reveal, the estimated relation between school connections and performance for the more highly ranked schools are in all cases larger in magnitude. All eight reported coefficients are significant at the 10% level, and seven of the eight are significant at the 5% level. Thus, when we restrict attention to higher ranked schools, the results become stronger and are robust even to the inclusion of hiring-school fixed effects. These findings provide further evidence that school connections are related to hiring more productive young researchers. We have experimented with using schools in the top three quartiles of hiring school quality, and the results are at least as strong as what we report in Panel B. At the same time, the coefficient on school connections is generally small and insignificant in predicting rookie research success at bottom quartile schools. Thus, consistent with our suspicion, it appears that the overall positive relation between research success and school connections is largely driven by the more research-intensive schools in the sample.
Our default definition of a school connection is based on the assumption that individuals who overlap at any point during their doctoral training are likely to have a close connection that would be particularly conducive to information flows. We have experimented with using a weighted overlap variable that places more weight on a longer tenure in school together by replacing the school connection variable with an alternative that assumes a value of 4/3/2/1 if an incumbent connected faculty graduated 1/2/3/4 years earlier than the rookie higher (the longest overlap is used if there are multiple school connections). When we use this alternative in the 16 models of Table 3, the evidence of a positive relation between school connections/overlap and research productivity is virtually unchanged, except in one model in which the school connection/overlap variable falls from the 5% to 10% significance level.

Considering more distant school-connections, we have also experimented with augmenting the Table 3 models by adding an additional variable that assumes a value of 1 if a rookie hire comes from the same school as an incumbent faculty member who graduated from 5-10 years before the rookie hire. When we augment the Table 3 models with this additional variable, it is in all cases insignificant, and in many cases negative. At the same time, the coefficients on the close school connection variable (i.e., our default measure) are substantively unchanged with these model modifications. Thus, it appears that there is something special about school connections in which direct overlap occurs, which is highly consistent with the presence of useful information transmission only when individuals have direct (rather than indirect) interactions.

5.2 Robustness of school connections findings

To check the robustness of the Table 3 findings, we have performed a battery of robustness checks. To account for possible nonlinearities, we have experimented with including
quadratic terms for both hiring school and Ph.D. school quality in the Table 3 models that do not include hiring school fixed effects. In addition, in all Table 3 models, we have experimented with including Ph.D. program fixed effects and clustering standard errors by Ph.D. program or geographic region (rather than hiring school). None of these alterations has a substantive effect on the magnitudes or significance levels of the school connection coefficients reported in the table. We also have experimented with using publications in the most recent three-year and five-year windows to measure hiring department quality, rather than an unlimited backward-looking window. Again, these alterations have no substantive effect on the magnitudes or statistical significance levels of any of the school connection coefficients reported in Table 3.

It is possible that larger departments tend to have different success rates with new hires, even after controlling for departmental quality. Larger departments also have more potential for a school-connected hire given their larger set of incumbent faculty. To investigate, we have experimented with modifying the Table 3 models that do not include hiring-school fixed effects to include the size of the department (measured as the number of tenure-track faculty). Alternatively, in these same models, we have included the number of new hires at that department in the prior two or three years, as this variable may be more closely related both to school-connected hiring and new faculty productivity. The school connection variable coefficients with these variables added, either one at a time or together, are substantively equivalent to what we report in the table.

It is possible that researchers within certain subareas of finance tend to cluster in certain hiring schools and/or Ph.D. programs, and publication rates may differ for different subareas. To allow for this possibility, we read the dissertation abstract of every new hire in our sample and assign their research area into one of four mutually exclusive bins based on an inspection of this
abstract. The assignment procedure is admittedly subjective, but it was independently coded by at least two individuals familiar with the general language of academic finance (the authors and/or senior doctoral research assistants). When we add fixed effects for each of these research genres to the model, their inclusion has no substantive effect on the estimated school-connection coefficients in Table 3.

One may be concerned about the reported standard errors given non-normality in the error term and skewness in publication rates. In particular, these features may reduce the accuracy of standard errors that are based on asymptotics in a sample of only moderate size. To account for this possibility, for each Table 3 model, we create 1,000 samples with all data unaltered except the school connection indicator variable. This variable is then randomly assigned to assume a value of 1 for the same number of observations as in the actual data. We then estimate the model for each of these 1,000 samples and use the estimated coefficients on the school connection variable to derive a coefficient distribution under the null of no school connection effect. Significance levels derived from these empirical distributions are quite comparable to what we report in the table.

5.3 Alternative explanations for school connection evidence

The preceding findings are consistent with school-connections providing a potential employer with an informational advantage in hiring that exceeds any school-related favoritism concerns associated with the connection. These findings appear particularly convincing for the higher-ranked schools in the sample, as the school coefficient in all models for these schools is

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12 These four areas are empirical corporate finance, theoretical corporate finance, empirical investments, and theoretical investments. Note that financial economics is a subarea of economics, so these four categories represent subgroups of an already somewhat homogenous economics subarea. While there are surely even finer distinctions that can be made, in our experience, the categorization we select is frequently used in finance hiring decisions when there is a goal of hiring in a specific area.
positive and significant, regardless of performance measure employed or the inclusion of hiring-school fixed effects.

Several of the studies surveyed earlier offer alternative explanations for the positive relation between school-connected hiring and individual performance. In particular, connected hires may work better with other parts of the organization (peer effects). Alternatively, connections may proxy for an omitted variable measuring how much the hiring organization supports its new hires, which in turn could result in increased productivity (a department nurturing effect). While it is difficult to completely rule out all of these alternatives, we are able to offer evidence that suggests that the information advantage channel is likely a main part of the explanation for our findings. Before turning to this evidence, we note that Kim, Morse, and Zingales (2009) find little evidence of substantive departmental effects on individual research productivity in finance departments starting in the 1990s, which is when our sample period starts, casting some initial doubt on some peer effects channels in explaining our findings.

We first consider the possibility that school connections lead to more nurturing/support of connected hires. If this is the case, we may expect to observe more same-department coauthoring relationships for the connected hire. We have investigated by replacing the dependent variables in the Table 3 models with corresponding variables representing the number of these papers that are co-authored with a same-department colleague (or weighted number of papers for the weighted measure). In all cases, the coefficient on the school connection variable in these models is negligible in magnitude and statistically insignificant.

Next, we consider the possibility that the school-connection variable proxies for whether a school is particularly committed to the success of all of its younger faculty, thus deferring to them more in hiring decisions while simultaneously mentoring them more in research. To investigate, we create an alternative dependent variable measuring the research success of a
department's portfolio of other young researchers. In particular, we select all other rookies hired in the same department at most 3 years before the rookie hire (excluding the rookie herself). We then create a variable measuring the sum of the research output of this group in the journal sets identified in each Table 3 model over the same 6-year window as the corresponding rookie hire, normalized by the number of members of the group. When we use the resulting junior-faculty research success measure in place of the rookie's success measure in the Table 3 models, the coefficient on the school connection variable is in all cases small in magnitude and statistically insignificant. Thus, it does not appear that school-connected hiring is associated with departments that have a portfolio of other young researchers that outperform expectations given the department's ranking. This evidence based on the productivity of other new faculty and on coauthoring casts doubt on a strong peer effect/nurturing explanation for our school connection evidence.

There are also some possible supply effect explanations for our findings. It may be that schools that hire more school-connected friends are more attractive places for a job candidate to work either because the school is better on a dimension not captured by the school's research measure (e.g., higher salaries), or because a stronger-than-usual candidate is willing to go to a school with which she has a school connection. To evaluate the first possibility, which is only relevant in models that exploit variation across schools, we collect data on the average (university-wide) faculty compensation of assistant professors at each sample school as of the sample-period midpoint. When we add this variable to the column 1-4 models of Table 3, it is

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13 Since we have multiple faculty members grouped together in these models, we exclude a measure of the Ph.D. program quality in these regressions. Our findings regarding school connections are unaltered if we instead include the equally-weighted average of Ph.D. program quality for programs where these faculty earned their degrees. These findings are also similar if we use a 5-year window to identify a department's other young researchers.

14 We use the average total compensation for assistant professors (across all fields) at each institution in the Fall of 1999 as reported by the American Association of University Professors in Bell (2000).
in all cases insignificant, and its inclusion has no substantive effect on the coefficients on the school connection variable.

Turning to the second possibility, if a given school is able to attract better candidates as a result of school connections, we may expect school-connected hires to generally come from stronger-than-expected doctoral programs. Thus, we estimate models analogous to the Table 3 models with a rookie hire's doctoral program quality used as the dependent variable in place of their post-hire research output. The associated school-connection coefficients are generally small in magnitude and vary in sign across models. Moreover, they are never significant at the 5% level or better (one is significantly positive at the 10% level). Thus, there is little evidence indicating that (publicly) observably better candidates deliberately join school connected departments because of individual preferences. This is perhaps not surprising given the strong career incentives and pressures for candidates to join the highest-ranked possible hiring school.

5.4 Ethnic connections

The preceding findings on school-related connections are consistent with the notion that there are substantive information benefits of hiring someone who is directly connected to an existing faculty member via a relatively recent school connection. To the extent that there may also be some agency costs arising from favoritism or loss of objectivity in these types of hiring decisions, the findings above are consistent with the hypothesis that the information benefits from school connections exceed these costs, particularly at the more research-intensive sample schools. We now turn to the role of ethnic connections with the hiring department in predicting

15 These models necessarily drop the Ph.D.-program control variables from the regressions. We cannot estimate these models for specifications corresponding to column 4 and 8 of Table 3 as we do not have a weighted version of the departmental quality metric.
publication success. As discussed earlier, for these hiring events, it is possible that information
benefits may be relatively small while favoritism costs could be substantial.

To investigate, we estimate models that parallel the Table 3 models above, but with the
ethnic-connection indicator variable replacing the school-connection indicator. In addition to the
earlier control variables, we include in these models fixed effects for each of the six ethnic
categories. The estimates for these fixed effects are not tabulated, and the findings on the ethnic-
connection variable are not sensitive to the inclusion of these effects.

We tabulate estimates for the full set of 16 models in Table 4. The estimates in columns
1-4 in both Panel A (all schools) and Panel B (higher ranked schools) reveal negative and
significant coefficients of substantial magnitude on the ethnic connection variable. For example,
the column 1 Panel A estimate indicates that new hires from ethnic categories towards which a
department is tilted tend to publish -.289 fewer articles than would otherwise be predicted in top
3 journals, a greater than 20% reduction measured relative to the sample mean rate of 1.19
articles. The corresponding estimate for higher-ranked schools in Panel B of -.542 is
substantially larger. When we include hiring-school fixed effects in columns 5-8 of both panels
in the table, the coefficient estimates remain negative in all models, but they are smaller in
magnitude and have larger standard errors, leading in all cases to statistical insignificance. Thus,
it appears that the negative relation between ethnic connections and performance is only
significant if we exploit variation across schools, rather than solely within-school variation.

To explore the robustness of these Table 4 ethnic-connection findings, we apply the same
robustness checks as outlined in subsection 5.2 for the school-connection findings.16 None of

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16 We do not consider analogs to the analysis in subsection 5.3, since most of these model extensions either are not
applicable to the ethnic connection variable or concern omitted variable biases that would lead to a positive relation
between connections and performance (e.g., supply effects). We have also examined whether connected-hires tend
to stay at the initial hiring school longer than others (within the 6-year tracked window), and we find no evidence of
any significant difference for either school-connected or ethnic-connected hires versus all others.
these robustness checks or model alterations has a substantive effect on the corresponding Table 4 estimated coefficients for the ethnic-connection variable. We have also experimented with using an alternative ethnic connection variable based on simple annual cohort top deciles of ethnic composition rather than the default regression-based residual top decile approach. The findings on the ethnic-connection variable with this modification are substantively unchanged from what we report in the table. In addition, we have experimented with restricting the ethnic connection variable to assume a value of 1 only when the candidate does not belong to the most common sample ethnic category (both for the regression residual approach and the simple decile alternative). Again, this alteration has no substantive effect on the character of the Table 4 findings regarding the estimated ethnic-connection coefficients.

The negative relation between ethnic connections and subsequent research performance in the column 1-4 models of Table 4 are broadly consistent with the favoritism costs hypothesis discussed earlier in which employers favor candidates with observable personal characteristics towards which a department may have a bias. This evidence appears robust to a variety of model modifications, but it does not survive the inclusion of hiring-school fixed effects. While robustness to the inclusion of these effects may be a high hurdle given the sample sizes, to be cautious, we interpret the ethnic-connection evidence on performance as highly suggestive, but less than conclusive.

6. Conclusion

We present evidence on the role of connections in hiring and post-hire employee productivity by studying the academic labor market for new doctoral graduates in financial economics. We consider direct connections based on whether an existing member of a hiring department recently graduated from the same school as a new hire, and indirect connections
based on ethnic similarities in last names between hired candidates and the hiring department. We expect that connections in this market may benefit the employer by increasing the precision of their information regarding a candidate. At the same time, connections may entail costs if the employer displays favoritism in evaluating connected candidates. Other possible roles for connections in predicting subsequent performance are also considered.

In a sample of 740 newly minted doctoral graduates from the 1990s and 2000s, we detect strong evidence that both types of connections increase the probability that a candidate is hired. The rate of both school and ethnic-connected hiring appears to be about 50% higher than would be predicted given other features of the data. Similar to other labor markets, connections appear to play an important role in academic job-matching decisions.

Turning to the possible consequences of this propensity to hire connected individuals, we find that school-connected hires publish on the order of 30% more in elite publication outlets than would be expected based on other observables. This finding is robust to a large number of robustness checks and model alterations, with the exception of some models that use the entire sample of schools, along with the most selective measures of publication success and inclusion of hiring school fixed effects. The estimated relation between school connections and a new hire's research productivity is stronger and significant in all cases when we consider hiring schools ranked above the sample median, or alternatively in the top three quartiles. Our favored interpretation of this evidence is that direct school connections provide prospective employers with an informational advantage with regard to the talents of certain candidates. We find no direct evidence supporting some other possible explanations. These include the possibility that school connections make a school more attractive to observably better candidates and/or that school connections proxy for a more supportive or attractive research environment or more
opportunities for productive co-authoring opportunities. However, we cannot completely rule out all of these alternatives.

In the case of ethnic-connected hires, we estimate that they publish on the order of 20% less in elite outlets than would otherwise be expected. This evidence is consistent with the presence of favoritism costs associated with these types of hiring events outweighing any compensating benefits (information related or otherwise). This evidence on ethnic connections and performance is quite robust in all models that exploit variation across schools, but it disappears when we restrict attention to within-school variation by including hiring school fixed effects in the model estimation. Since we cannot rule out the possibility that ethnic-connection hires are more likely at departments with inferior research environments on unobservable (to us) dimensions, we more cautiously interpret the ethnic evidence as suggestive of favoritism effects.

Our findings complement recent related evidence reported by Burks, Cowgill, Hoffman, and Housman (2015) and Pallais and Sands (2016) from other specialized hiring settings. Those authors detect some productivity benefits to hiring via connections, and they attribute those effects to information and/or peer benefit effects. Our results are highly consistent with the hypothesis that when team production is relatively unimportant, information benefits from connected hiring can be quite substantial, at least in a market in which individual human capital is a key input into organizational success. However, as our ethnic-connection results suggest, favoritism effects may at times appear in some types of connected hiring, so a blanket recommendation to emphasize connected hiring of all types is far from warranted. Consequently, further research into how organizations balance the benefits from connected hiring with the competing costs across types of firms, positions, and connections would be quite illuminating.
Appendix

Faculty Composition Algorithm

The Hasselback directories were published from the early 1990s until the 2007/2008 issue and include information from surveys sent to departmental administrators/offices in all domestic finance departments. Inspection of the directories reveals that the reported composition of the faculty is for an academic year earlier than the year in the directory title (the actual year of the data is referred to as the directory year). The directories were published every two years or, in one case, three years apart.

If an individual tenure-stream faculty member is listed in two successive directories, we assume they were at the school for the entire intervening window. If an individual moves between schools and the directories list a start year for the new school, we assume the individual was at the new employer as of the start year, and at the prior employer up to the earlier of (a) the last year they are listed with the prior employer plus one, and (b) the start year at the new employer minus one.

The majority of cases are accounted for using the preceding algorithm, and thus in most cases, it is clear when a move was made from one department to another. However, there remain some cases with a slightly higher degree of ambiguity. If an individual moves between schools, as revealed by two successive directories, but the directories do not list a start year at the new school, we assume the individual was at the new employer as of the directory year, and at the prior employer up to the last directory they are listed with the prior employer plus one year. If an individual is listed in a directory and never shows up again in any later directory, we assume the individual was at the employer for one additional year past the directory year listing. If an individual is listed in two non-consecutive directories, we assume they joined the new employer as of the start year (if reported) and otherwise as of the directory year. In these cases, we assume the individual was at the prior employer up to the last directory year they are listed with the prior employer plus one (unless this falls after the start year at the new employer). In cases in which a person is not assigned to a school for a given year according to the above algorithm, we assign their location in that year as missing and do not attribute their human publications/identity to any school.

The algorithm above is our default algorithm for assigning faculty to departments. However, we do experiment with some alternatives including (a) the algorithm outlined above but ignoring all start dates and assuming the directory listing year is always the start date, (b) the algorithm outlined above but always assuming the individual left the prior employer in the last directory listing year rather than sometimes carrying this forward one year, and (c) the algorithm outlined above but assuming both that the directory listing year is always the start year and that the individual left the prior employer in the last directory listing year. As we mention in the text, none of our main findings in the paper are substantively changed using these alternative assignment algorithms.

Identifying Journals and Publications

The three publication outlets (JF, JFE, RFS) selected in our default performance metric are widely recognized as the most influential journals in the finance field. When we consider five journals, we add the Journal of Financial and Quantitative Analysis (JFQA) and the Journal of Business (JB). The JFQA is included as the fourth influential journal in the Arizona State rankings. The JB, which ceased publication at the end of our sample period, was often included in rankings of highly regarded (top 5) finance journals. See, for example, Arnold, Butler, Crack, and Altintig (2003) and Kim, Morse, and Zingales (2009).
To obtain our comprehensive set of 21 journals, we start with the 16 “core journals” listed in Chan, Chen, and Steiner (2002). We drop from this list Financial Analysts Journal and Journal of Portfolio Management, as information for these journals was not available from Research Papers in Economics (RePEc). This leaves us with a set of 14 journals. In addition to this list, we add the top three economics journals as identified by Kim, Morse, and Zingales (2009) (American Economic Review, Quarterly Journal of Economics, Journal of Political Economy), the two other highly ranked finance journals identified by Chen and Huang (2007) (Journal of Corporate Finance, Journal of Financial Markets), and the one other high impact economics journal identified by Arnold, Butler, Crack, and Altintig (2003) (Econometrica). Finally, we add the Review of Finance, a newer journal with an impact factor that is higher than several others on our list. While no list is perfect, the resulting list of 21 journals should include the vast majority of the top and medium level outlets targeted by finance scholars. For each individual, we identify all of their publications and the year of publication using the RePEc listing of publications by author. We exclude short notes, comments, and errata by only recording publications that are at least 10 pages in length.

The top-3, top-5, and top-21 measures in the text are simple measures that give a credit of 1 for a publication in any journal in the indicated set and 0 for publications outside of the set. Given the substantial heterogeneity in journal prestige and impact within the broad set of 21 journals, we also create a weighted top 21 measure of a rookie publication success by awarding "points" for different publications and then summing these point totals. The basic weighting scheme is to assign points using the weightings reported by Chen and Huang (2007). Thus, for example, a publication in one of the most elite journals we consider, the Journal of Finance, receives a score of .803. For the five journals not numerically ranked by Chen and Huang (2007), we use an imputed score by multiplying .803 by the journal's most recent impact factor relative to the Journal of Finance as reported by Kim, Morse, and Zingales (2007), or, in the one case when this is unavailable, as reported by Currie and Pandher (2010).

Identifying Geographic Regions and Urban Areas

For some of the procedures described in the text, we utilize geographic information related to the school's location. In particular, we code a geographic region indicator based on which of six broad geographic regions contains the school's campus. The broad regions are as follows: Northeast (Washington D.C., Massachusetts, New York, Pennsylvania, New Hampshire, Virginia, New Jersey, Connecticut, Delaware, Maryland, Rhode Island), Midwest (Michigan, Missouri, Indiana, Iowa, Illinois, Minnesota, Wisconsin, and Ohio), Southeast (South Carolina, North Carolina, Georgia, Florida, Louisiana, Alabama, Kentucky, and Tennessee), Southwest (Arizona, Oklahoma, and Texas), Mountain (Utah and Colorado), and West Coast (California, Oregon, and Washington). We also code a large urban dummy variable based on whether or not the school is located within 50 miles of a city that is ranked in the Top 25 in population based on 2006 population.
References


Kim, E. Han, Adair Morse, and Luigi Zingales, 2009, "Are Elite Universities Losing Their Competitive Edge?" *Journal of Financial Economics* 93, 353-381.


### Table 1 – Sample Summary Statistics

<table>
<thead>
<tr>
<th>Metric</th>
<th>All Years: 1991-2006</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of sample finance departments</td>
<td>102</td>
</tr>
<tr>
<td>Number of rookie hires</td>
<td>740</td>
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<tr>
<td>Mean department size</td>
<td>15.11</td>
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<td>Median department size</td>
<td>14</td>
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<td>Mean hiring department quality - top 3 measure</td>
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<td>Mean quality of doctoral school - top 3 measure</td>
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<td>Mean success of rookie hire - top 3 measure</td>
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<tr>
<td>Median success of rookie hire - top 3 measure</td>
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<td>Mean hiring department quality - top 5 measure</td>
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<td>Mean success of rookie hire - top 5 measure</td>
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<td>Median success of rookie hire - top 5 measure</td>
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<td>Mean hiring department quality - top 21 measure</td>
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<td>Mean success of rookie hire - weighted top 21 measure</td>
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<td>Percent rookie articles in top 3 outlets</td>
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<td>Percent rookie articles in top 5 outlets</td>
<td>83%</td>
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<tr>
<td>Percent rookie articles in top 21 outlets</td>
<td>90%</td>
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</table>

Note:- The sample includes all U.S. located finance departments ranked in the top 100 based on the Arizona State University Finance department rankings for either 1990-1999 or 2000-2009 with data available in the Hasselback finance faculty directories. We collect data on all departments as of the start of the academic year from 1991 until 2006. Rookie hires are all individuals who join a department no more than one year after or two years before completing their doctoral degree. Department size is the number of tenure-stream faculty members as of the start of the academic year for those schools. The top 3/top 5/top 21 departmental school quality measures are calculated as the sum of all past publications by current faculty members in the 3/5/21 elite journals identified according to the procedure outlined in the appendix as of the start of the academic year normalized by the number of faculty members. Quality of the doctoral school is the corresponding metric for the school that each rookie hire graduated from in the year that they graduate. Doctoral schools that are not ranked have missing values for this variable. The top 3/top 5/top 21 measure of success of each rookie hire is an individual’s aggregate number of publications in the set of 3/5/21 journals during the first six years in the profession after joining the hiring school. The weighted top 21 measure is an analogous measure that uses the broad set of 21 journals and awards differential points for publications based on journal impact as described in the appendix. The percent of articles in the top 3 and top 5 outlets are calculated for all rookies over their first six years as a percent of articles in these more elite outlets within the set of 21 total tracked outlets. The percent of articles in the top 21 outlets is calculated by selecting a random set of 50 rookie sample hires and calculating the percent of refereed articles they publish in their first six years in the 21 tracked outlets relative to publications identified in any outlet. All sample summary statistics are calculated treating each rookie hire and the associated school/individual characteristics as a single observation.
### Table 2 – Rate of Connected Hiring for Rookie Finance Faculty

<table>
<thead>
<tr>
<th>Panel A: Actual sample versus five simple deterministic reshuffles</th>
<th>School-Connected Hiring Rate</th>
<th>Ethnic-Connected Hiring Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rate in Actual Sample</td>
<td>13.78%</td>
<td>22.70%</td>
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<tr>
<td>Rate in sample reshuffle #1</td>
<td>9.88%</td>
<td>15.98%</td>
</tr>
<tr>
<td>Rate in sample reshuffle #2</td>
<td>8.36%</td>
<td>16.85%</td>
</tr>
<tr>
<td>Rate in sample reshuffle #3</td>
<td>11.10%</td>
<td>18.16%</td>
</tr>
<tr>
<td>Rate in sample reshuffle #4</td>
<td>9.15%</td>
<td>16.23%</td>
</tr>
<tr>
<td>Rate in sample reshuffle #5</td>
<td>8.54%</td>
<td>17.90%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Connected hiring rates, simulated samples after pre-sort</th>
<th>School-Connected Hiring Rate</th>
<th>Ethnic-Connected Hiring Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean rate, 1,000 reshuffles: hiring school tercile x doc. prog. tercile</td>
<td>9.02%***</td>
<td>14.14%***</td>
</tr>
<tr>
<td>Mean rate, 1,000 reshuffles: research genre x hiring school tercile</td>
<td>10.88%***</td>
<td>15.60%***</td>
</tr>
<tr>
<td>Mean rate, 1,000 reshuffles: research genre x doc. prog. tercile</td>
<td>9.85%***</td>
<td>16.39%***</td>
</tr>
<tr>
<td>Mean rate, 1,000 reshuffles: doctoral program quintile</td>
<td>10.92%***</td>
<td>16.22%***</td>
</tr>
<tr>
<td>Mean rate, 1,000 reshuffles: completely random</td>
<td>7.41%***</td>
<td>13.86%***</td>
</tr>
</tbody>
</table>

Note.- School-connected hires are all cases in which a rookie hire is assigned to a school with an existing faculty member from the same doctoral program who graduated within 4 years of the rookie hire. Ethnic hires are all cases in which a rookie hire is assigned to a school with a tilt in ethnic composition that places the school in the top decile for the same ethnicity as the rookie hire using residuals from the ethnic prediction model discussed in the text. The actual rates reported in the first row are for the true schools that rookie hires join. Other statistics are based on alternative job assignments that assign individuals to alternative employers. In Panel A, sample reshuffle #1 assigns individuals to the closest hiring school in ranking within the destinations of the five closest-ranked degree-granting schools supplying sample rookies in the observation year. Sample reshuffle #2 assigns individuals to the school joined by the rookie from the closest ranked degree program in the same year within the five closest ranked hiring schools hiring sample rookies in the observation year. Reshuffles #3 and #4 are analogous to the first and second reshuffles with the added requirement that the selected assigned school for the reshuffling is in the same broad geographic region as the actual hiring school. Reshuffle #5 simply assigns each hire to next closest ranked hiring school in the hiring year. In Panel B, we report mean statistics based on 1,000 simulated samples (reshuffles) in which we first conduct a pre-sort of the data based on certain covariates and then randomly assign individuals to jobs within groups after the pre-sort in a one-to-one manner. The reshuffles in the first row of this panel randomly assign each rookie hire to one school (including potentially the one they actually join) within the set of schools that fall in the same tercile of hiring school rankings in the sample year while also hiring from the same tercile of doctoral programs in the sample year. The second row (third row) of Panel B assigns individuals randomly to a school that hired in their research area (out of 4 possible) and that falls in the same hiring department-quality (doctoral program quality) tercile. The final two rows of Panel B consider simulated samples in which individuals are randomly assigned to positions within their annual doctoral program quintile cohort (the penultimate row) or to any position in their annual cohort (the final row). ***Denotes that the actual rate of connected hiring falls in the top 1% of the empirical distribution using the 1,000 simulated samples.
### Table 3 – Models of Rookie Finance Research Productivity and School Connections

<table>
<thead>
<tr>
<th>Panel A: Entire Sample</th>
<th>No Hiring School Fixed effect</th>
<th>Hiring School Fixed Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>School connection with hiring dept.</td>
<td></td>
<td>0.386** (0.181)</td>
</tr>
<tr>
<td>Hiring department quality</td>
<td></td>
<td>0.234*** (0.040)</td>
</tr>
<tr>
<td>Ph.D. school quality</td>
<td></td>
<td>0.085*** (0.029)</td>
</tr>
<tr>
<td>Unranked Ph.D. program dummy</td>
<td></td>
<td>-0.257 (0.161)</td>
</tr>
<tr>
<td>Hiring School fixed effect</td>
<td></td>
<td>No</td>
</tr>
<tr>
<td>Number of Obs.</td>
<td></td>
<td>740</td>
</tr>
<tr>
<td>Adjusted R²</td>
<td></td>
<td>0.152</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Above Median Schools</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
</tr>
<tr>
<td>School connection with hiring dept.</td>
</tr>
<tr>
<td>Hiring department quality</td>
</tr>
<tr>
<td>Ph.D. school quality</td>
</tr>
<tr>
<td>Unranked Ph.D. program dummy</td>
</tr>
<tr>
<td>Hiring School fixed effect</td>
</tr>
<tr>
<td>Number of Obs.</td>
</tr>
</tbody>
</table>

Note: All coefficients are for linear regression models predicting each sample rookie finance hire’s publication output. The school-connection variable is a dummy variable that assumes a value of 1 if the rookie graduated from a school from which an incumbent faculty member also graduated within the prior four year period. Robust standard errors clustered at the hiring school level are reported in parentheses under each coefficient estimate. The research quality measure row indicates the performance metric used to measure the publication output of each new hire during their first six years at a school (details in text and appendix). The corresponding research quality measure (departmental aggregate past faculty research output normalized by faculty size) is used in each model to measure hiring department quality and doctoral department quality, except in the weighted top 21 models where we use the unweighted top 21 measure for departmental quality because of data limitations. For Ph.D. programs without publication data, we assign the Ph.D. quality variable a value of 0 and code the unranked program dummy variable as a 1. The models in columns (1)-(4) ((5)-(8)) do not include (do include) hiring school fixed effects. All models include year effects. The Panel A models are estimated using the entire sample while the Panel B models are restricted to individuals hired at above median ranked schools using the top 3 publication metric. *Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level.
Table 4 – Models of Rookie Finance Research Productivity and Ethnic Connections

<table>
<thead>
<tr>
<th>Panel A: Entire Sample</th>
<th>No Fixed effect</th>
<th>Hiring School Fixed Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
</tr>
<tr>
<td>Ethnic connection with hiring dept.</td>
<td>-0.289** (0.130)</td>
<td>-0.370** (0.145)</td>
</tr>
<tr>
<td></td>
<td>-0.296* (0.177)</td>
<td>-0.220** (0.100)</td>
</tr>
<tr>
<td></td>
<td>-0.102 (0.151)</td>
<td>-0.129 (0.172)</td>
</tr>
<tr>
<td>Hiring department quality</td>
<td>0.253*** (0.039)</td>
<td>0.164*** (0.033)</td>
</tr>
<tr>
<td></td>
<td>0.105*** (0.039)</td>
<td>0.200*** (0.031)</td>
</tr>
<tr>
<td></td>
<td>-0.001 (0.156)</td>
<td>0.026 (0.123)</td>
</tr>
<tr>
<td>Ph.D. school quality</td>
<td>0.080*** (0.029)</td>
<td>0.062** (0.026)</td>
</tr>
<tr>
<td></td>
<td>0.046 (0.030)</td>
<td>0.061*** (0.022)</td>
</tr>
<tr>
<td></td>
<td>0.079** (0.035)</td>
<td>0.055* (0.030)</td>
</tr>
<tr>
<td>Unranked Ph.D. program dummy</td>
<td>-0.319* (0.175)</td>
<td>-0.304 (0.208)</td>
</tr>
<tr>
<td></td>
<td>-0.305 (0.283)</td>
<td>-0.244* (0.137)</td>
</tr>
<tr>
<td></td>
<td>-0.385** (0.194)</td>
<td>-0.372 (0.239)</td>
</tr>
<tr>
<td>School fixed effect</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td></td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of Obs.</td>
<td>740</td>
<td>740</td>
</tr>
<tr>
<td>Which Observations</td>
<td>All</td>
<td>All</td>
</tr>
<tr>
<td>Pseudo R²</td>
<td>0.135</td>
<td>0.091</td>
</tr>
<tr>
<td>Research Quality Measures</td>
<td>Top 3</td>
<td>Top 5</td>
</tr>
<tr>
<td></td>
<td>Top 21</td>
<td>Weighted 21</td>
</tr>
<tr>
<td>Panel B: Above Median Schools</td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
</tr>
<tr>
<td>Ethnic connection with hiring dept.</td>
<td>-0.542*** (0.198)</td>
<td>-0.556** (0.227)</td>
</tr>
<tr>
<td></td>
<td>-0.632** (0.274)</td>
<td>-0.508** (0.196)</td>
</tr>
<tr>
<td></td>
<td>-0.330 (0.325)</td>
<td>-0.410 (0.325)</td>
</tr>
<tr>
<td>Hiring department quality</td>
<td>0.181*** (0.063)</td>
<td>0.098** (0.046)</td>
</tr>
<tr>
<td></td>
<td>0.159*** (0.049)</td>
<td>0.118*** (0.041)</td>
</tr>
<tr>
<td></td>
<td>-0.103 (0.217)</td>
<td>0.013 (0.179)</td>
</tr>
<tr>
<td>Ph.D. school quality</td>
<td>0.075 (0.053)</td>
<td>0.055 (0.049)</td>
</tr>
<tr>
<td></td>
<td>0.073 (0.052)</td>
<td>0.050 (0.041)</td>
</tr>
<tr>
<td></td>
<td>0.090 (0.070)</td>
<td>0.069 (0.059)</td>
</tr>
<tr>
<td>Unranked Ph.D. program dummy</td>
<td>-0.551* (0.326)</td>
<td>-0.600 (0.360)</td>
</tr>
<tr>
<td></td>
<td>-0.558 (0.471)</td>
<td>-0.434 (0.365)</td>
</tr>
<tr>
<td></td>
<td>-0.588 (0.369)</td>
<td>-0.614 (0.399)</td>
</tr>
<tr>
<td>School fixed effect</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td></td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of Obs.</td>
<td>355</td>
<td>355</td>
</tr>
<tr>
<td>Pseudo R²</td>
<td>0.092</td>
<td>0.071</td>
</tr>
<tr>
<td>Research Quality Measures</td>
<td>Top 3</td>
<td>Top 5</td>
</tr>
<tr>
<td></td>
<td>Top 21</td>
<td>Weighted 21</td>
</tr>
</tbody>
</table>

Note.- All coefficients are for linear regression models predicting each sample rookie finance hire’s publication output. The ethnic connection variable is a dummy variable that assumes a value of 1 if the rookie has a last name that the ethnicity assignment algorithm described in the text assigns to an ethnic category towards which the department lies in the top sample decile based on the residual from a regression model predicting a department’s ethnic composition. Robust standard errors clustered at the hiring school level are reported in parentheses under each coefficient estimate. The research quality measure row indicates the performance metric used to measure the publication output of each new hire during their first six years at a school (details in text and appendix). The corresponding research quality measure (departmental aggregate past faculty research output normalized by faculty size) is used in each model to measure hiring department quality and doctoral department quality, except in the weighted top 21 models where we use the unweighted top 21 measure for departmental quality because of data limitations. For Ph.D. programs without publication data, we assign the Ph.D. quality variable a value of 0 and code the unranked program dummy variable as a 1. The models in columns (1)-(4) ((5)-(8)) do not include (do include) hiring school fixed effects. All models include year effects and ethnicity fixed effects for the six ethnic groups. The Panel A models are estimated using the entire sample while the Panel B models are restricted to individuals hired at above median ranked schools using the top 3 publication metric. *Significant at the 10% level, **Significant at the 5% level, ***Significant at the 1% level