Network Position and Productivity: Evidence from Journal Editor Rotations*

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Abstract: Using detailed publication and citation data for over 50,000 articles from 30 major economics and finance journals, we investigate whether network proximity to an editor influences research productivity. During an editor's tenure, his university colleagues publish *more* papers in the editor's journal, compared to years before and after his appointment. In contrast to editorial nepotism, such "inside" articles have significantly *higher* ex post citation counts, even when same-journal and self-cites are excluded. Our results thus suggest that despite potential conflicts of interests faced by editors, personal associations are used to improve selection decisions.

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I. Introduction

Networks are of first order importance in many economic decisions and outcomes. Whether finding a job (Granovetter, 1974) or surviving in a POW camp (Costa and Kahn, 2007), it is clear that networks funnel resources to benefit their members. Less obvious is whether such benefits arise via efficiency improvements, or whether they simply reflect value transfers from those outside the network. The key hurdle is that in most cases, the econometrician cannot benchmark the choices made by network members (e.g., which people are recommended for jobs) to an objective quality standard (i.e., which people *should* have been recommended). Without this comparison, it is impossible to assess whether networks improve overall welfare.

In this paper, we study a setting largely immune from this criticism: academic publishing. While journal editors may have private incentives to favor members of certain groups (e.g., coauthors or colleagues) by publishing their papers, the ultimate measure of an article's success – citations – is decided by the overall market, and is consequently objective. By comparing the observed choices of editors to the market's *ex post* judgment of article quality, our results speak directly to whether an editor's network is used to pursue private or broader objectives.

Our analysis covers over 50,000 articles published in 30 major economics (e.g., *American Economic Review*) and finance journals (e.g., *Journal of Finance*) since 1955. We begin by first collecting the editor or editor(s) for each journal, and then, using each editor's affiliation, form two "professional networks:" 1) his current university colleagues, and 2) his past coauthors. The goal is to identify a set of authors where a conflict of interest might arise. With these networks in place, we then analyze whether a submitting author's affiliation with the editor influences his chances at publication, and in particular, how any such "connected" articles fare *ex post*.

The construction of our empirical tests is important for appreciating how the effects are identified. For each university *i* in our data set, we aggregate into a single observation the number of articles published in journal *j* at time *t*. As an example, supposing that the economics faculty at Harvard published three articles in *Econometrica* in 1997, this would constitute a single observation. We then form a dummy variable that takes a value of one if, and only if, the editor of journal *j* two years prior (*t-2*) worked at institution *i*. For example, because Harvard did not employ an *Econometrica* editor in 2005, the dummy variable of interest would take a value of zero in this case.

Averaged across all journals and years, we find that colleagues of journal editors (at the estimated time of submission) have markedly higher publication rates, nearly 100% higher compared to the years immediately preceding or following the editor's assignment. Moreover, recalling that our unit of observation is a school-journal-year triple, we can include fixed effects for every pairwise combination of these, i.e., dummy variables for each school*year, journal*year and school*journal pairing. The first of these accounts for time varying school quality (e.g., the ascent of NYU's publication output since 1955), and allays concerns that editors are selected from improving departments. The second controls for size differences across journals (e.g., the *Journal of Labor Economics* publishes far fewer articles per year than the *American Economic Review*). The final interaction is also important, accounting for any persistent school-journal match effects, which might occur if departments specialize in certain fields.

Nevertheless, one remaining concern is the temporal aspect of endogenous editor selection. Although school-year fixed effects account for improvements or impairments at the overall university level, there are school-specific time trends that these will not address. For example, think about a school that gains or loses a prominent econometrician, and can therefore be expected to experience an output shock, potentially in only a few journals. If these events are correlated with the appointment of editors at these journals, our estimates may be biased.

Fortunately, our data are well suited to address such a problem. While the appointment of editors may be based on unobservable measures of expected productivity, this is not a concern when an editor's tenure is completed. Specifically, because in most cases editorships are for fixed terms, the timing of departure is mechanical, and thus, will not be plausibly correlated with omitted variables. Because we find productivity drop-offs upon an editor's departure of almost identical size to the ramp-ups we observe upon his arrival, we conclude that endogeneous selection seems unlikely to explain the results.

We then move to our second set of tests, which, instead of examining publication rates, explores citations counts. As mentioned previously, this is the key result that permits a judgment on network efficiency – for every subpar paper an editor let in, presumably, a higher quality paper is excluded. Moreover, if non-connected authors perceive favoritism, this may feed back into their initial effort decisions, which may further impair overall research quality.

Inspection of the data, however, suggests just the opposite. Across a variety of specifications, we find that connected articles have citation counts between 5 and 25% higher than unconnected articles. This result survives even author fixed effects, so that we are comparing cite counts for articles authored by the same person. With this specification, we find citation counts for connected articles that are about 9% higher among economics journals, and 19% higher among finance journals.

Together, the results suggest two ways that networks likely improve research quality. The first, as indicated by the higher citation counts for connected articles, suggests that editors likely use their personal associations to better evaluate papers, and simply select the best ones. However, that we observe higher output (i.e., number of papers) indicates a second effect. For note that if editors simply see connected papers with a clearer lens, we would expect higher average quality, but not necessarily higher output (as bad papers become more obvious too). The fact that an editor's colleagues are writing not only better papers, but more of them, indicates that journals are likely associated with positive spillovers, part of which accrue to the members of its home institution.

However, all these conclusions depend crucially upon citation counts being an objective standard of quality. But what if editors can use their power to influence citation counts? For example, an editor may place connected articles in the same issues as “star” articles (hoping for citation spillovers), or he may ask future papers to cite connected articles. These and other possibilities draw into question the credibility of citation counts as a relevant benchmark, and this, threaten the interpretation of the result.

We conclude by addressing this possibility, but find no evidence. While being placed in the same issue behind a star article (i.e., one with over 500 eventual citations) increases counts by over 8%, there is no evidence that editors attempt such opportunistic placement for connected articles. Moreover, we also find little evidence that editors funnel citations to connected papers by asking future articles to cite connected papers. When we remove same-journal citations from our main citation measure, we find no change in the positive relationship between citation counts and connectivity.

Overall, our results pertain to the growing literature on networks and economic outcomes, and specifically, on the potential for in-group favoritism to outweigh the benefits of network externalities. See, e.g., Banerjee and Munshi (2004), Jackson and Schneider (2011), and Fisman, Paravasini, and Vig (2011) for recent examples. It also builds upon Laband and Piette (1994), which examines citation counts for an editor’s colleagues in the 1984-1985 cross-section. Relative to the their work, we contribute not only by examining research output, but also by identifying editorial effects from the time-series. As discussed in the text, trends in institutional quality are likely to be correlated with editor selection, and thus can bias the coefficients of interest.

The paper is organized as follows. In the subsequent section, we describe our data and the construction of variables. Section III presents the results of our main specifications relating publication frequency to the presence or absence of a connected editor. Section IV considers whether connected papers have higher ex-post cite counts than unconnected ones, and performs a series of robustness and falsification tests. Section V concludes.

II. Data and Variable Construction

We collect publication, citation, and editorship data for 30 leading economics and finance journals. Our set includes general economics journals – e.g., *Quarterly Journal of Economics (QJE)*, *Journal of Political Economy*, *Econometrica*, *Review of Economic Studies*, and *American Economic Review (AER)*. It also includes top field journals in finance (e.g., *Journal of Finance, Review of Financial Studies, Journal of Financial Economics*), urban economics (e.g., *Journal of Urban Economics*), econometrics (e.g., *Journal of Econometrics*), labor (e.g., *Journal of Labor Economics*), game theory (e.g., *Game and Economic Behavior*), and monetary economics (*Journal of Monetary Economics*). The complete list of journals is presented in Table 1. Also, the first row of Table 2 shows that in the typical case, the journals in our set publish a little more than 47 articles every year, although this varies substantially, with an interquartile range of 29-59.

In order to build a database of historical editorships and their affiliations, we searched the JSTOR and ScienceDirect databases, which contain PDF versions of historical issues for each journal. Typically, the editor and co-editors are named in the first few pages or "front matter" of each journal issue. In a few cases, names were either not legible or were not listed, so we obtained physical copies from local libraries. When these two options failed (only in rare cases), we filled in the blanks from CVs, obituaries, biographies, and in some instances, personal correspondence.

There is some variability across journals in how editors are listed. While some journals list a single editor (e.g., currently William Schwert at the *Journal of Financial Economics*), others have a flatter hierarchy (e.g., the *Quarterly Journal of Economics*, which currently has four editors of equivalent standing). Other arrangements are observed as well, such as the *Journal of Finance*, which in 2011 lists Campbell R. Harvey as "Editor" and John R. Graham as "Co-Editor." Because of this variability, some subjectivity is required, although in most cases the distinction between editors and associate editors (which can number in the dozens) is clear. As shown in the second and third rows of Table 2, journals in our sample have 3-4 editors per year on average, and the typical editor serves for about six years. Also shown (row 4) is the number of people editors have historically co-authored with (average 12.6), that is, in years leading up to their editorships.

Next, we used Web of Science (WOS) to gather detailed publication records. For every economics journals since 1955 (not just the 30 for which we collect editor histories), we download the "full record" from WOS. Using the entire database (224 economics journals) from WOS, rather than the 30 journals on our list, has several advantages. First, we can use all 224 economics journals to observe co-author relationships between editors and authors. This allows us reduce Type II errors (failing to observe true relationships), because we observe a wide spectrum of economics publications and, hence, co-authorships. We can also use the other journals to more accurately measure the publication and citation history of individual authors. This is important for our citation analysis, where historical citation counts are, as we will see, an excellent measure of author quality. Finally, because we observe the cited references of every economics publication, we know precisely which economics articles cite each other. This helps us deal with potential problems stemming from same-journal citations and self-citations.

The list of variables considered include the number of authors (which Table 1, row 5 shows numbers 1.66 on average), school affiliation(s), publication year, journal issue and page numbers, a list of articles the publication cited, and the number of times the publication is cited by other publications. The last variable, *Times Cited Count* is the measure of total citations gathered from its five citation indexes, and has been used in many prior journal bibliometric studies (e.g. Wuchty, Jones and Uzzi, 2007) to measure the performance of an article.[1]

Rows 6-11 present summary statistics for a number of citation metrics for the articles published in our set of 30 journals. The average of *Times Cited Count* indicates that the typical article is cited slightly less than 33 times. Restricting attention to articles published only by co-authors at the top 50 schools (ranked by number of publications), this average increases to 44.5. Moving down the table, we see that *Same-Journal Citations* are relatively infrequent (mean 1.84), as are *Self-Citations*. Finally, the average of *Top 30 Journal Citations* (9.11) indicates that only about 9.11/32.95 = 27% of total citations are attributable to cites in other Top 30 journals.

Ultimately, we are interested in whether authors within an editor's network – specifically his coauthors and university colleagues – experience positive or negative productivity shocks around editor rotations. Were articles published immediately or very soon after submission, it would be easy to identify whether, for example, an editor publishes a paper written by a past co-author. However, with lags between submission and publication, and without data on the actual review process, forming these linkages is more subjective. In our analysis, we use a lag of 2 years when matching editors with publications.[2] That is, for an article published in 2005, we assume that the editor in 2003 handled the review process. Because the typical editor is in place for about six years (Table 1, row 3), this assumption bites less often than one might suppose (i.e., because the editor one year ago and the editor two years ago is often the same person). In any case, mismatching editors and authors is almost certainly idiosyncratic, and consequently, should introduce noise into our estimates.

[1] See http://images.webofknowledge.com/WOKRS51B6/help/WOS/hp_times_cited_count.html for a detailed description of the *Times Cited Count* variable.

[2] See Ellison (2002) for a detailed review of the peer review process in economics journals. He documents a substantial slowdown, from, e.g., 6-12 months on average for general economics journals in the 1970s to 24-30 months in the 1990s. Over the last 20 years, the vast majority of journals had total review times between one and two years.

Throughout the analysis, we will have two ways in which to form connections between authors and editors. A *Colleague Connection* is one in which an editor and author simultaneously work at the same university. For example, Richard Green from Carnegie Mellon edited the *Journal of Finance* from 2000 to 2003. We would therefore assign a *Colleague Connection* to any *Journal of Finance* publication between 2002 and 2005, provided that one or more of its authors was also from Carnegie Mellon. As shown in Table 1, a little more than 7% of our articles have at least one *Colleague Connection*.

The second way that editors and journal authors can be connected is through past co-authorships. Here, we use the WOS to infer authors with whom the editor of interest has published a paper in the past. Continuing with the example, Jonathan Berk was a coauthor of Richard Green before the latter assumed the editorship of the *Journal of Finance*. Consequently, we would assign a *Coauthor Connection* to any of Jonathan Berk's *Journal of Finance* publications between 2002 and 2005. These types of connections are, as shown in Table 1, about half as frequent, occurring about 3.2% of the time. Aggregating both types, 8.8% of articles have a connection of some type.

III. Editor Networks and Publication Rates

We begin our analysis with perhaps the simplest question: upon assuming an editorship, do members of the editor's network have more success publishing in *that particular journal*? If editors obtain private benefits by bestowing favoritism upon members of their network, and if professional sanction or other implicit incentives are insufficient detterents, we might expect editors to publish more papers by colleagues or past co-authors. On the other hand, there are less cynical possibilities. Perhaps the most direct is that being awarded an editorship increases the prestige or visibility of the editor's institution. This might lead, for example, to more interactions with high quality seminar speakers or other interactions. Likewise, members of an editors' network – particularly if they are used disproportionately as referees – might learn about the types of papers being submitted, real-time publication trends, or other information that allows them to write more or higher quality papers.

By the same token, either of these mechanisms might generate *less* (more) output for members of an editor's network upon his arrival (departure). Crowding out is one obvious way: if an editor's current colleagues or coathors are burdened with referee work, this might take away from their own research time or effort. As for nepotism, editors may be particularly wary of appearing corrupt, and might therefore be reluctant (or refuse altogether) to publish papers by those with an association. (At least one editor in our sample, via personal communication, expressed exactly this sentiment.)

To resolve this tension, we aggregate the 146 institutions which, at any point in the sample, have had at least one editor of the 30 journals listed in Table 1. For each school *i*, journal *j* and year *t*, we count the number of publications and call it $Pubs_{i,j,t}$. Next we define a dummy variable $During_{i,j,t}$ which is equal to one if school *i* has an editor at journal *j* during year $t - 2$ (recall from Section II that we assume a two year delay between initial submission and publication). In order to ascertain whether a school's publication rate at a specific journal is higher when an editor is there, we estimate the following linear model (with variable coefficients omitted for brevity):

$$(1)\quad Pubs_{i,j,t} = During_{i,j,t} + \varepsilon_{i,j,t}$$

The results are presented in Column 1, Panel A of Table 3. The positive coefficient of 1.421 ($p < 0.001$) suggests that schools currently "hosting" a journal through having one of its professors serve as editor publish about one-and-a-half more articles per year in that journal. To put this number in context, the mean value of *Pubs* is 0.35, so that the magnitude of the marginal effect is, at least in this simple specification, enormous.

However, it is clear that this benchmark specification is grossly mis-specified, and consequently, does not permit us to infer a causal effect between editorships and the publication rates of their colleagues. One obvious reason is that top schools are both more likely to employ editors, and likewise, are more likely to publish in top journals. Consequently, $During_{i,j,t}$ may mostly be capturing cross-school effects. Likewise, there are a number of journals where *During* is always (or mostly) equal to one -- e.g., with the "House journals" listed in Table 1 such as *Quarterly Journal of Economics* and *Journal of Financial Economics* – making it unclear exactly what *During* is picking up.

Given that our unit of observations is three dimensional (i, j, t), we can admit three pair-wise fixed effects: journal*year fixed effects, school*journal fixed effects and school*year fixed effects. Their inclusion allows us to identify *During* relative to each school's time-varying productivity (school*year dummies), each school's persistence tendency to publish in a given journal (school-journal dummies), and each journal's time-varying output (journal-year dummies). The final three columns of Table 3 Panel A add each of these pairwise fixed effects in succession to the main specification.

Column 2 adds journal*year fixed effects, which controls for the average number of articles a journal publishes in a given year. While this leads to a sizable improvement in Adjusted R-Squared (from less than 4% to over 10%), it leaves the coefficient on $During_{i,j,t}$ nearly unchanged. This is not the case in Column 3, which adds journal*school fixed effects to the specification in Column 2. By adding journal*school fixed effects, we have now controlled for the average publication rate of a given school at a given journal (e.g. UCLA's publication rate at *AER*). This means that the coefficient on $During_{i,j,t}$ is now estimated from variation within journal*school, e.g. comparing publication rates for UCLA in the *AER* during the years when it had an editor at *AER* (1981 – 1986) to the years when it did not (prior to 1981, or after 1986).

Although adding journal*school fixed effects leads to a sizable reduction in the coefficient on $During_{i,j,t}$, -- from 1.427 in Column 2 to .333 in Column 3 – the coefficient remains large, and recalling the mean of the dependent variable, represents an increase of nearly 100% relative to the typical department's productivity.

The final column adds school*year fixed effects so that the specification is now:

$$(2)\ \ Pubs_{i,j,t} = During_{i,j,t} + \sum_{j,t} JY_{j,t} + \sum_{i,j} JS_{i,j} + \sum_{i,t} SY_{i,t} + \varepsilon_{i,j,t},$$

where $\sum_{j,t} JY_{j,t}$, $\sum_{i,j} JS_{i,j}$ and $\sum_{i,t} SY_{i,t}$ represent journal*year, journal*school and school*year fixed effects, respectively. This final model is akin to a triple-difference specification where we have netted out time-varying school quality, any school-journal matching effects, and the total output (time-varying) for each journal. Continuing with the UCLA-*AER* example, *During* compares UCLA's 1983 publication output in the AER (where it had an editor) to UCLA's 1983 publication output in the *QJE* (where it did not), while accounting for the fact that: 1) the *AER* might have published more papers than the *QJE* in 1983 (it did), and 2) that UCLA might *persistently* publish at a higher rate at the *AER* compared to *QJE* (it has).

The results in Column 4 Panel A show but little change in the coefficient of interest. This is important because it suggests whatever productivity improvements that accrue to an editor's colleagues from hosting the journal, they are captured by the editor's journal. That is, if becoming editor made an editor's colleagues more productive across all journals, then the coefficient on $During_{i,j,t}$ should be close to zero once we include school*year fixed effects. The fact that it is not suggests the main *productivity differences occur at the editor's own journal.* Of course, it is not clear whether these additional papers are of higher or lower quality (as we explore in the next section), but at this point, it is sufficient to rule out the two output-reducing alternatives with which began this section.

The bottom panel (B) of Table 3 repeats the same set of tests among the three major finance journals: the *Journal of Finance*, the *Journal of Financial Economics* and the *Review of Financial Studies*. Here, we consider the 30 schools which – at any point in their histories – ever employed an editor one or more of these journals.

We observe similar results to what we observe in the larger set of economics journals. Specifically, the magnitude on the *During* coefficient is slightly bigger than one without controls, but is cut in half once we account for school-journal match quality.[3] Taking the final column as the most indicative of the underlying behavior, we see that having an editor appointed increases the number of a department's "top finance" publications by about 0.6 papers. As with the larger sample of economics journals, this is a substantial (46%) increase against a base rate of nearly 1.3.

Although it is clear that schools publish more articles in a journal when they have an editor at that journal, one concern is that editor assignments are not random. Although a school*journal effect in the aforementioned analysis will control for the average productivity of a specific school at a specific journal, it does not account for time variation in productivity at that journal. It may be that an editor is selected to a journal precisely because his school is becoming more productive at that journal. As an example, the *Journal of Finance* was edited by Rene Stulz (Ohio State) from 1989 to 1999. Although Ohio State faculty have consistently published in finance journals since the beginning of our sample, its productivity increased markedly about the time Stulz became editor.

This selection problem leads us to examine the specific timing of productivity shocks around editorial rotations. Recall that $During_{i,j,t}$ is a dummy variable which turns on during the years that school i has an editor at journal j (subject to the 2-year publication lag). We now create a new variable, $JustBefore_{i,j,t}$ which is a dummy variable that that turns on during the five years before school i has an editor at journal j and $JustAfter_{i,j,t}$ which turns on during the five years after school i has an editor at journal j. Returning to the previous example, we want to compare

[3] While the inclusion of school-journal fixed effects cuts the coefficient on *During* by nearly three-quarters in the sample of economics journals, the reduction is less pronounced (about one-half) in the finance sample. The most likely reason is that the variability of journal quality is much lower among the three "mainstream" finance journals listed. Consequently, while we might still expect large difference between schools (captured by the school-year effects), school-journal match effects for the finance journals (e.g., NYU Stern-*Journal of Finance* and NYU Stern-*Review of Financial Studies*) are almost certainly less informative than the the same matches in the broader sample.

Ohio State faculty productivity in the *Journal of Finance* in the years during, just before and just after the journal was at Ohio State. In Table 4 Column 1, we estimate the following:

$$(3)\ \ Pubs_{i,j,t} = During_{i,j,t} + JustBefore_{i,j,t} + JustAfter_{i,j,t} + \varepsilon_{i,j,t},$$

which is identical to the specification in Table 3 Column 1, except that we have added our two new variables: $JustBefore_{i,j,t}$ and $JustAfter_{i,j,t}$. Although the coefficients on $JustBefore_{i,j,t}$ (.824) and $JustAfter_{i,j,t}$ (.797) have positive coefficients they are substantially smaller than the coefficient on $During_{i,j,t}$ (1.439). Linear restriction tests reject the hypotheses that the coefficient on *During* is the same as the coefficient on *JustBefore* or *JustAfter* ($p<0.01$). Columns 2, 3 and 4 add our journal*year, journal*school and school*year fixed effects in succession so that the final specification (Column 4) is:

$$(4)\ \ Pubs_{i,j,t} = During_{i,j,t} + JustBefore_{i,j,t} + JustAfter_{i,j,t} + \sum_{j,t} JY_{j,t} + \sum_{i,j} JS_{i,j} + \sum_{i,t} SY_{i,t} + \varepsilon_{i,j,t}$$

Again, we find a statistically significant difference between the coefficient on *During* and those on *JustBefore* and *JustAfter*. For example, during an editor's tenure the marginal increase in publication rate is .38; this falls to .17 immediately after the editor leaves (Figure 1). Given the fixed and predictable nature of most editorial terms, it seems implausible that such a drop off could be explained by endogenous selection.

The bottom panel repeats the exercise for the set of finance journals and finds similar results. In the full specification (Panel B Column 4), the coefficient on *During* (.759) is more than double the coefficient on *JustBefore* (.297) and *JustAfter* (.337).

IV. Connected Papers and Ex-Post Performance

In the prior section, we found that an editor's colleagues publish more papers in the editor's journal precisely when the editor begins his tenure at the journal. While these results suggest positive externalities for an editor's network, they do not indicate whether journals and readers benefit from connected publications. On one hand, the boost in publication may represent preferential treatment. Under this nepotistic view, connected papers will be of poorer quality because favorable treatment suggests lower standards for publication. On the other hand, editors may use their network to gather private information about superior papers, or perhaps positive externalities from hosting the journal spill over to an editor's colleagues. Under this view, connected papers will be of greater quality.

In order to sort between these two views, in this section we compare the *ex post* performance of connected and unconnected papers. We use article citations as our measure of ex-post performance because, while the decision to publish is left to the editor, the decision to cite is left to the market.

For each article in the 30 journals listed in Table 1, we collect the number of citations the article has received from Web of Science (*Times Cited Count*). We define a variable $LogCites_{k,j,t}$ which is the natural logarithm of *Times Cited Count* for article k in journal j in year t. Note that *Times Cited Count* includes all citations in the Web of Science database, not just the 30 for which we have editorial information. Later, we vary this definition.

Next, we define three dummy connection variables: $CoauthorConnection_{k,j,t}$, $ColleagueConnection_{k,j,t}$, and $AnyConnection_{k,j,t}$. $CoauthorConnection_{k,j,t}$ equals one if an author of article k has any prior publication with the editor of journal j in year t - 2 (recall we assume a two year lag between submission and publication). $ColleagueConnection_{k,j,t}$ equals one if an author of article k is at the same school as the editor of journal j in year t - 2. $AnyConnection_{k,j,t}$ equals one if either $CoauthorConnection_{k,j,t}$ or $ColleagueConnection_{k,j,t}$ equals one. As with the publiciation rate regressions, we begin with the simplest model, estimating:

$$(5)\quad LogCites_{k,j,t} = Connection_{k,j,t} + \varepsilon_{k,j,t},$$

where $Connection_{k,j,t}$ represents one of the three connection variables defined above.

The results are presented in Columns 1 – 3, Panel A of Table 5. The positive coefficients on the different connection variables vary between 0.350 and 0.425 ($p<0.001$) suggesting that on average, connected articles receive on the order 35 – 43% more citations.

However, the same types of selection concerns that apply to the publication rate regressions (Tables 3 and 4) apply here. Specifically, we already know that connected articles, having been written by academics affiliated with an editor, are not of average quality. Professors from prestigious schools are more likely to be selected as editors, as are academics with impressive publication records (perhaps due in part to well chosen co-authors). Consequently, it is important to account for author quality, which we do in the next three columns.

Perhaps the most straightforward way to control for author quality is his (or, in the case of a group, their) recent performance, as measured by citation counts. With the variable *Times Cited Count: Last 5 Years*, we perform the following exercise: for every paper in our data set published in year *t*, we tabulate the citation counts for each author for the trailing five years, and then take the maximum. To give an example, suppose that three co-authors A, B, and C publish a paper in 1998. Furthermore, suppose that co-author A's papers from 1993-1997 were cited 24 times, co-author B's papers (over the same interval) were cited 15 times, and that co-author C's papers were cited 43 times. In this case, the *Times Cited Count: Last 5 Years* would take a value of 43, corresponding to the recent cite counts of co-author C. This metric pays special attention to the tails of the citation distribution, which we already know (see Table 1) is highly skewed. However, alternatives such as summing the cite counts or averaging them makes little difference.

The fourth column shows, unsurprisingly, that accounting for recent citations is very important. Co-author groups with highly cited papers in the recent past continue to have their papers cited, described as the "Matthew effect" by Merton (1968). It also includes as a control the *Number of Authors* for each paper; the negative coefficient is not particularly meaningful in this context, given that it is highly correlated with *Times Cited Count: Last 5 Years* variable. Nonetheless, we include both to be conservative.

The estimate on *Any-connected article* remains economically and statistically significant at 0.250 ($p<0.001$), indicating that relative to the recent performance of a co-author team, connected articles are of higher quality. Note that this column also controls for journal-year, i.e., with separate dummy variables for *American Economic Review-2004, American Economic Review-2005,* etc.

While *Times Cited Count: Last 5 Years* controls for quality through lags of the dependent variable, it cannot account for young authors who, prior to the year of consideration, have scant publication records. To account for cross-sectional differences in author quality using a non-parametric framework, the last two columns include dummy variables for schools (column 6) and individual authors (7). The number of authors well exceeds the number of articles in our data set, so we include 500 fixed effects for the most prolific authors. Because we are mostly concerned about the right tail of the distribution, the set of 500 fixed effects for top authors are probably sufficient to capture the early successes of eventual stars.

While the inclusion of these controls substantially cuts the point estimates – suggesting the importance of "early" publications in our analysis – connected papers still appear to receive somewhat better citation counts than ones lacking any connection to the editor. The last column, which effectively compares specific line items on a given author's *CV*, indicates that editors use private information to identify an author's "diamonds in the rough." On average, this increases citations by about 9% relative to articles published in that journal, during that year. Among the top finance journals, connected articles have cite counts which are 19% higher when we include author fixed effects (Panel B Column 6).

V. Falsification and Robustness

At first blush, the fact that connected articles are better cited would seem to be strong evidence against editorial nepotism. If editors imposed lower standards on their colleagues and/or coauthors, we would expect connected articles to have lower citation counts, not higher. However, *this is true only insofar as editors cannot use their power to influence a connected article's citations*. For example, an editor may place connected articles at the front of an issue (Oswald, 2008), or he may ask future papers to cite connected articles. In either case, we are left with the possibility that our measure of paper quality is itself contaminated.

We begin with an examination of article placement, i.e., whether an article is placed first, second, third, etc., in a given issue. Clearly editors control placement and, provided that citation counts are *causally* related to placement, this constitutes a tool by which measures of article quality can be manipulated. There are two reasons this might be the case. One is simply limited attention, i.e., that authors are less aware of articles published near the back of an issue. A second possibility is that editors have private information about the quality of a given article, and communicate this information via placement.

On the other hand, the placement of articles might simply reflect editorial convention, whereby higher quality articles are placed earlier in issues. Under this story, there is no causal role for an article's placement, so that randomly shuffling articles within an issue would have no impact on its eventual citation count.

For the moment, we leave this ambiguity unresolved, and simply quantify whether, and if so by how much, article placement matters for cite counts. We again consider the *LogCites* variable (defined above) as well as the dummy variables *First Article*, *Second Article*, and *Third Article*. As their names suggest, *First Article* equals one if the article is the first in the issue, *Second Article* equals one if the article is the second in the issue, and so on. Columns 1-3 of Table 6, Panel A show the results. All else equal, lead articles are cited more than 50% more than other articles published in that journal during that year (note the inclusion of journal-year fixed effects). Articles in the second slot fall to about half the magnitude (26%), and articles placed third about 17% more than the typical article. In summary, we see that article placement is strongly associated with citation counts, but the direction of causation is unclear.

In Panel B, column 1-4, we take the relation between article placement and citations (Panel A) as given, and ask whether connected articles enjoy better placement. To do so, we estimate the following equation:

$$(6)\ LeadArticle_{k,j,t} = Connection_{k,j,t} + PastCites_{k,j,t} + NumAuthors_{k,j,t} + \sum_{j,t} JY_{j,t} + \varepsilon_{k,j,t}$$

where the dependent variable is discrete, taking a value of one if an article is placed in the lead position, and zero otherwise. Regardless of the type – i.e., either a *Colleague connection* or *Coauthor connection* – connected articles are 5-7% more likely to be placed in the lead position. This result is virtually unaffected by including controls for past citation counts (*Times Cited Count: Last 5 Years*), *Number of Authors*, or journal-year fixed effects.

This result, however intriguing, is not a smoking gun. The reason, as mentioned above, is that editors may simply follow a convention of placing higher quality articles earlier in issues. Thus, the evidence in columns 1-3 of Panel A of Table 6, and in columns 1-4 of Panel B of Table 6 may reflect little more than differences in objective quality.

For this reason, we examine a second type of discretionary placement, and importantly, one that does not suffer such an ambiguous interpretation. The idea is as follows: while placement *within* an issue likely reflects an editor's assessment of article quality, placement *across* issues within a year does not. To be concrete, suppose that we are thinking about two generic months in which the *Journal of Finance* regularly publishes: June and August. Although it is likely that the respective lead articles in either month are regularly of higher quality than the non-lead articles published in those respective months, it would be surprising for lead articles in June to systematically differ from lead articles in August. Similar logic applies to articles placed in other slots.

Given this logic, the fourth column shows the results of an interesting exercise. The thought experiment is to compare the citation counts for two non-lead articles (e.g., two articles placed in fourth position) published in the same journal-year, but where only one of them *follows a "star" lead article*, i.e., in the same issue. For example, imagine articles like White (1980) or Jensen and Meckling (1976), incredibly influential papers by any measure. We are interested in whether these star articles confer "citation spillovers" to other articles within the same particular issue. Presumably, authors seeking out the star articles may have stumbled upon articles in the same issue, and consequently, led to increased citation counts.

And indeed, this is exactly what appears to happen. When we define *Star Articles* as the highest cited lead articles in a particular journal-year (e.g., the highest cited of *QJE's* lead articles in 1997), we observe spillovers to other articles in the same issue of about 8% ($p<0.001$).[4] This is similar to the differences between a second and third placed article.

To complete the argument, all we need is for editors to have some idea of which articles are likely to be stars. (It seems difficult to believe otherwise, given that editors have likely observed a number of quality signals by the time placement decisions are made, and that we are restricting attention to lead articles.) Provided that they do, then the question is whether they "stack" connected papers in the same issues as, say, White (1980) in order to reap the 8% incidental citation spillover. The final column of Panel B of Table 6, where we include the full family of controls, suggests not. Here, we see that connected articles are no more (or less) likely to be placed in the same issue as a *Star Article*.

Another, perhaps more direct way editors might influence citations counts is by encouraging other papers to cite connected papers. In fact, this is a specific case of the more

[4] We focus on lead articles because we are interested in situations where the editor has information about an article's eventual success. Articles whose high citation counts surprised even the editor can clearly not be used in the manner hypothesized.

general criticism that perhaps citations should not be treated equally. For example, one might view citations in the connected editor's journal as less objective (for the reason described above), similar to the arguments levied against self-citations as a measure of quality. In Table 7, we conduct a number of robustness exercises dedicated to these and similar concerns.

First, to remove the possibility that citations are disproportionately picking up activity from less prestigious journals, we include only cites from articles published in the top 30 economics journals. Except for this redefined dependent variable, the specification otherwise matches the one whose results are shown in Table 5, Columns 4-6. That is, *PastCites* and *NumAuthors* are included in each specification, the first regression for each dependent variable includes Journal*Year Fixed Effects, the second Journal*Year Fixed Effects and School Fixed Effects, and the third Journal*Year Fixed Effects and Author Fixed Effects. Comparing these results to the full sample however, we observe similar, and even somewhat larger effects. The fact that the best journals are citing connected articles with higher frequency gives credence to the claim that they are, in fact, of higher objective quality.

The next three rows investigate the possibility that the extra citation counts for connected articles stem disproportionately from articles within the same journal. Although this would not necessarily indicate inflated cite counts from editorial pressure, excluding them means that our effects are identified purely from articles *outside* the editor's sphere of influence. Although the magnitudes are slightly reduced, we observe very similar magnitudes for the full sample. Through similar logic, the final column excludes self-citations which, as Table 1 shows, amount to a very small percentage of overall cites. Perhaps unsurprisingly, excluding them makes very little difference.

VI. Conclusion

The long-run quality of academic research (citations) is ultimately judged as most other goods – by a largely annomyous market. However, short-run quality decisions (journal acceptances) are made by a small number of individuals, and thus admits the possibility for conflicts of interest to bias decision making. Because these two are linked – i.e., whether and where an article is published may impact how influential it can become – the credibility of the editorial process is of paramount importance. Also of consequence are career and tenure outcomes, many of which are linked *directly* to publications (perhaps less so to citation counts).

This paper examines whether editors of academic journals admit more of their colleagues' or coauthors' papers to their own journals, and if so, whether these papers deserved to be accepted. Examining over 50,000 articles from 30 top economics and finance journals since 1955, we provide strong affirmative answers to both questions. Although members of an editor's network publish at higher frequencies at the editor's journal, the citation counts for such papers are at least 5% higher, and up to 25% higher. Our specifications are stringent, accounting for time-varying school quality, time-varying journal quality, school-journal match effects, and even author fixed effects.

Whether these results are remarkable or not largely depends on one's view of an editor's incentives. On the one hand, editorial positions are almost always *pro bono*, implying little if any direct pecuniary incentives. On the other hand, the perception of corruption is likely quite costly to editors, let alone intrinsic motivation, the combination of which appears to be capable of reducing agency costs.

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Figure 1: Colleagues' Marginal Publication Rates Around Editor Arrival

The figure graphs the coefficients on "Just Before", "Connected Editor" and "Just After" from Table 4. The coefficients represent the marginal change in publication frequency for school I in journal j in the five years before that school's editor arrives at journal j, during his tenure and in the five years after he editor leaves. Economics Journals are all the 30 journals from Table 1. Finance journals are the Journal of Finance, the Journal of Financial Economics and the Review of Financial Studies.

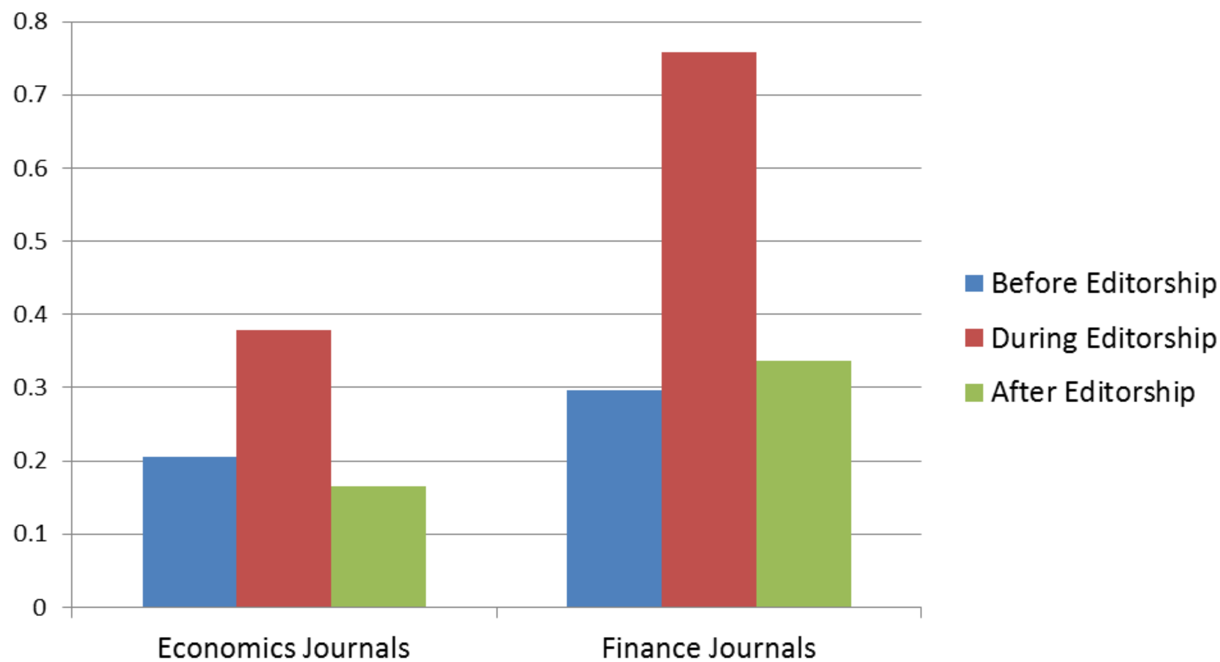

Table 1: Journal List

The table lists 30 major economics journals for which we have detailed editorial histories. House Journal is "Yes" if every year of the editorial history contains at least one editor from the same university (e.g. Harvard and the Quarterly Journal of Economics). First Year in Sample is the first year the journal's publications have full records in the Web of Science database.

	Journal	House Journal	First Year in Sample
1	ECONOMETRICA	No	1955
2	JOURNAL OF ECONOMIC LITERATURE	No	1969
3	JOURNAL OF POLITICAL ECONOMY	Yes	1956
4	JOURNAL OF FINANCIAL ECONOMICS	Yes	1976
5	QUARTERLY JOURNAL OF ECONOMICS	Yes	1956
6	AMERICAN ECONOMIC REVIEW	No	1956
7	JOURNAL OF ECONOMIC PERSPECTIVES	No	1988
8	JOURNAL OF FINANCE	No	1956
9	JOURNAL OF LAW & ECONOMICS	Yes	1958
10	REVIEW OF ECONOMIC STUDIES	No	1956
11	RAND JOURNAL OF ECONOMICS	No	1984
12	JOURNAL OF MONETARY ECONOMICS	Yes	1976
13	REVIEW OF FINANCIAL STUDIES	No	1990
14	JOURNAL OF ECONOMETRICS	No	1980
15	JOURNAL OF LABOR ECONOMICS	Yes	1983
16	JOURNAL OF ECONOMIC GROWTH	Yes	1999
17	REVIEW OF ECONOMICS & STATISTICS	Yes	1956
18	JOURNAL OF ECONOMIC THEORY	No	1969
19	JOURNAL OF BUSINESS & ECONOMIC STATISTICS	No	1985
20	ECONOMIC JOURNAL	No	1956
21	JOURNAL OF HUMAN RESOURCES	Yes	1966
22	JOURNAL OF INTERNATIONAL ECONOMICS	No	1971
23	JOURNAL OF PUBLIC ECONOMICS	No	1976
24	INTERNATIONAL ECONOMIC REVIEW	Yes	1960
25	JOURNAL OF APPLIED ECONOMETRICS	Yes	1987
26	JOURNAL OF INDUSTIAL ECONOMICS	No	1956
27	JOURNAL OF MONEY CREDIT & BANKING	Yes	1976
28	GAMES & ECONOMIC BEHAVIOR	Yes	1991
29	ECONOMIC THEORY	No	1995
30	REVIEW OF ECONOMIC DYNAMICS	No	2001

Table 2: Summary Statistics

Total Articles per Journal Year is the number of articles a journal publishes in a given year. Number of Editors per Journal Year is the number of editors a journal has in a given year. Editor Tenure is the number of years an editor serves at a journal. Number of Editor Coauthors is the number of historical coauthors an editor has while serving as editor. Number of Authors per Article is the number of authors of a given article. Times Cited Count is the Web of Science count of the number of times an article has been cited in the Web of Science database. Same-Journal Citations is the number of times a given article has been cited by a publication in the same journal. Self Citations is the number of times a given article has been cited by a publication with the same author(s). Colleague-Connected Article is a dummy variable which takes the value one if the article has an author at the same institution as the journal's editor. Coauthor-Connected Article is a dummy variable which takes the value one if the article has an author which is a prior coauthor of the editor. Any-Connected Article is the maximum of Colleague-Connected Article and Coauthor-Connected Article.

	Mean	Standard Deviation	5th Percentile	25th Percentile	Median	75th Percentile	95th Percentile
Articles per Journal Year	47.38	26.41	11	29	43	59	96
Number of Editors per Journal Year	3.42	2.37	1	1	3	5	8
Editor Tenure (years)	6.13	5.18	1	3	5	7	16
Number of Editor Coauthors	12.60	10.37	1	5	10	17	34
Number of Authors per Article	1.66	0.78	1	1	2	2	3
Times Cited Count	32.95	105.47	0	3	10	30	125
Times Cited Count (Top 50 Schools)	44.50	127.60	0	4	15	43	167
Same-Journal Citations	1.84	4.63	0	0	0	2	8
Self Citations	1.07	2.04	0	0	0	1	5
Top 30 Journal Citations	9.11	24.19	0	0	2	9	38
Colleague-Connected Article (Dummy)	0.071	0.257	0	0	0	0	1
Coauthor-Connected Article (Dummy)	0.032	0.176	0	0	0	0	0
Any-Connected Article (Dummy)	0.088	0.283	0	0	0	0	1

Table 3: Connectivity and Productivity

Each observation is a school, journal, year triplet (i, j, t) which counts the number of publications school *i* has in journal *j* in year *t*. The top panel considers the 146 schools which have ever had editors at the 30 journals in Table 1. The bottom panel considers the 30 schools which have ever had editors at the 3 finance journals. *During Editorship* is a dummy variable which takes the value of one if school *i* has an editor at journal *j* in year t. Panel B considers the three finance journals: *Journal of Finance*, *Review of Financial Studies* and *Journal of Financial Economics*. Robust standard errors clustered by school are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

PANEL A: ALL JOURNALS

	Dependent Variable: Published Articles			
During Editorship	1.421***	1.427***	0.333***	0.297***
	(0.156)	(0.154)	(0.054)	(0.050)
Journal*Year Fixed Effects	NO	YES	YES	YES
Journal*School Fixed Effects	NO	NO	YES	YES
School*Year Fixed Effects	NO	NO	NO	YES
Observations	163,520	163,520	163,520	163,520
Adjusted R^2	0.0376	0.1014	0.5053	0.5246

PANEL B: FINANCE JOURNALS

	Dependent Variable: Published Articles			
During Editorship	1.178***	1.349***	0.636***	0.609***
	(0.205)	(0.181)	(0.154)	(0.138)
Journal*Year Fixed Effects	NO	YES	YES	YES
Journal*School Fixed Effects	NO	NO	YES	YES
School*Year Fixed Effects	NO	NO	NO	YES
Observations	3,888	3,888	3,888	3,888
Adjusted R^2	0.0224	0.1683	0.4687	0.5084

Table 4: Connectivity, Productivity and Timing

During Editorship is a dummy variable which takes the value of one if school i has an editor at journal j in year t. *Just Before* (*Just After*) is a dummy variable which takes the value of one during the five years before (after) school i has an editor at journal j. Panel B considers the three finance journals: *Journal of Finance*, *Review of Financial Studies* and *Journal of Financial Economics* Robust standard errors clustered by school are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

PANEL A: ALL JOURNALS

	Dependent Variable: Published Articles			
During Editorship	1.439***	1.454***	0.428***	0.378***
	(0.157)	(0.154)	(0.062)	(0.057)
Just Before	0.824***	0.879***	0.248***	0.206***
	(0.079)	(0.073)	(0.052)	(0.048)
Just After	0.797***	0.795***	0.189***	0.165***
	(0.096)	(0.088)	(0.056)	(0.054)
Journal*Year Fixed Effects	NO	YES	YES	YES
Journal*School Fixed Effects	NO	NO	YES	YES
School*Year Fixed Effects	NO	NO	NO	YES
Observations	163,520	163,520	163,520	163,520
Adjusted R^2	0.0536	0.1183	0.5061	0.5252

PANEL B: FINANCE JOURNALS

	Dependent Variable: Published Articles			
During Editorship	1.248***	1.509***	0.820***	0.759***
	(0.226)	(0.199)	(0.153)	(0.158)
Just Before	0.327	0.948***	0.291*	0.297*
	(0.287)	(0.251)	(0.162)	(0.156)
Just After	0.922**	1.023***	0.536***	0.337**
	(0.386)	(0.342)	(0.190)	(0.169)
Journal*Year Fixed Effects	NO	YES	YES	YES
Journal*School Fixed Effects	NO	NO	YES	YES
School*Year Fixed Effects	NO	NO	NO	YES
Observations	3,888	3,888	3,888	3,888
Adjusted R^2	0.0353	0.1909	0.4720	0.5095

Table 5: Editor Connectivity and Article Performance

Times Cited Count is the Web of Science count of the number of times an article has been cited in the Web of Science database. Log(Times Cited Count) is the natural logarithm of one plus Times Cited Count. Times Cited Count: Last 5 Years is the sum total of an author's citations over the prior five years (for articles with multiple authors the maximum is taken). Number of Authors is the number of authors of a given article. Colleague-Connected Article is a dummy variable which takes the value one if the article has an author at the same institution as the journal's editor. Coauthor-Connected Article is a dummy variable which takes the value one if the article has an author which is a prior coauthor of the editor. Any-Connected Article is the maximum of Colleague-Connected Article and Coauthor-Connected Article. Panel A considers all 30 journals in Table 1. Panel B considers the three finance journals: *Journal of Finance*, *Review of Financial Studies* and *Journal of Financial Economics*. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

PANEL A: ALL JOURNALS

	Dependent Variable: Log(Times Cited Count)					
Coauthor-Connected Article	0.350***					
	(0.044)					
Colleague-Connected Article		0.425***				
		(0.029)				
Any-Connected Article			0.405***	0.250***	0.043**	0.089**
			(0.026)	(0.019)	(0.020)	(0.036)
Times Cited Count: Last 5 Years				0.114***	0.097***	0.040***
				(0.003)	(0.003)	(0.011)
Number of Authors				-0.046***	-0.028***	0.046**
				(0.009)	(0.009)	(0.021)
Journal*Year Fixed Effects	NO	NO	NO	YES	YES	YES
School Fixed Effects	NO	NO	NO	NO	YES	NO
Author Fixed Effects	NO	NO	NO	NO	NO	YES
Observations	54,046	54,046	54,046	54,046	49,218	8,908
Adjusted R^2	0.0013	0.0045	0.0052	0.3928	0.4157	0.6290

PANEL B: FINANCE JOURNALS

	Dependent Variable: Log(Times Cited Count)					
Coauthor-Connected Article	0.821***					
	(0.119)					
Colleague-Connected Article		0.532***				
		(0.085)				
Any-Connected Article			0.580***	0.249***	0.079	0.190*
			(0.075)	(0.052)	(0.057)	(0.103)
Times Cited Count: Last 5 Years				0.111***	0.082***	-0.003
				(0.007)	(0.008)	(0.033)
Number of Authors				-0.079***	-0.047**	0.018
				(0.022)	(0.023)	(0.058)
Journal*Year Fixed Effects	NO	NO	NO	YES	YES	YES
School Fixed Effects	NO	NO	NO	NO	YES	NO
Author Fixed Effects	NO	NO	NO	NO	NO	YES
Observations	6,395	6,395	6,395	6,395	5,893	1,167
Adjusted R^2	0.0071	0.0064	0.0099	0.5373	0.5219	0.6548

Table 6: Editor Connectivity, Article Placement and Citations

Times Cited Count is the Web of Science count of the number of times an article has been cited in the Web of Science database. Log(Times Cited Count) is the natural logarithm of one plus Times Cited Count. Times Cited Count: Last 5 Years is the sum total of an author's citations over the prior five years (for articles with multiple authors the maximum is taken). Number of Authors is the number of authors of a given article. Colleague-Connected Article is a dummy variable which takes the value one if the article has an author at the same institution as the journal's editor. Coauthor-Connected Article is a dummy variable which takes the value one if the article has an author which is a prior coauthor of the editor. Any-Connected Article is the maximum of Colleague-Connected Article and Coauthor-Connected Article. Lead Article is a dummy variable which takes the value of one if an article is first in an issue. Second Article and Third Article are similarly defined. Same Issue as Star Article is a dummy variable which takes the value of one if a paper is in the same issue as the a lead article with the mist cite counts during the journal year. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

PANEL A: PLACEMENT AND CITATIONS

	Dependent Variable: Log(Times Cited Count)			
Lead Article	0.536*** (0.018)			
Second Article		0.263*** (0.018)		
Third Article			0.167*** (0.018)	
Same Issue as Star Article				0.079*** (0.014)
Times Cited Count: Last 5 Years	0.113*** (0.003)	0.117*** (0.003)	0.119*** (0.003)	0.116*** (0.003)
Number of Authors	-0.042*** (0.009)	-0.046*** (0.009)	-0.048*** (0.009)	-0.042*** (0.010)
Journal*Year Fixed Effects	YES	YES	YES	YES
Observations	54,046	54,046	54,046	48,638
Adjusted R^2	0.3883	0.3803	0.3788	0.3769

PANEL B: CONNECTIVITY AND PLACEMENT

	Dependent Variable: Lead Article (Dummy)				Dependent Variable: In Star Issue (Dummy)			
Coauthor-Connected Article	0.069***				0.060***			
	(0.010)				(0.015)			
Colleague-Connected Article		0.055***				0.025***		
		(0.006)				(0.009)		
Any-Connected Article			0.057***	0.055***			0.029***	0.010
			(0.006)	(0.006)			(0.008)	(0.007)
Times Cited Count: Last 5 Years				0.009***				0.0004
				(0.001)				(0.001)
Number of Authors				-0.011***				0.002
				(0.002)				(0.003)
Journal*Year Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Observations	54,046	54,046	54,046	54,046	48,638	48,638	48,638	48,638
Adjusted R^2	0.0012	0.0020	0.0028	0.0112	0.0003	0.0001	0.0003	0.2286

Table 7: Robustness

Times Cited Count is the Web of Science count of the number of times an article has been cited in the Web of Science database. Log(Times Cited Count) is the natural logarithm of one plus Times Cited Count. Times Cited Count: Last 5 Years is the sum total of an author's citations over the prior five years (for articles with multiple authors the maximum is taken). Number of Authors is the number of authors of a given article. Colleague-Connected Article is a dummy variable which takes the value one if the article has an author at the same institution as the journal's editor. Coauthor-Connected Article is a dummy variable which takes the value one if the article has an author which is a prior coauthor of the editor. Any-Connected Article is the maximum of Colleague-Connected Article and Coauthor-Connected Article. The first three columns only considers citations received from journals on the list in Table 1. The first three columns exclude from Times Cited Count citations which come from the same journal (e.g. *QJE* articles citing *QJE* articles). The final three columns exclude from Times Cited Count citations which come from the same author(s). Panel A considers all 30 journals in Table 1. Panel B considers the three finance journals: *Journal of Finance*, *Review of Financial Studies* and *Journal of Financial Economics*. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

PANEL A: ALL JOURNALS

Dependent Variable: Log(Times Cited Count)

	ONLY CITATIONS FROM TOP JOURNALS			EXCLUDING SAME JOURNAL CITATIONS			EXCLUDING SELF CITATIONS		
Any-Connected Article	0.250***	0.055***	0.098***	0.227***	0.033*	0.078**	0.243***	0.042**	0.086**
	(0.016)	(0.017)	(0.030)	(0.018)	(0.019)	(0.034)	(0.018)	(0.019)	(0.034)
Times Cited Count: Last 5 Years	0.105***	0.089***	0.027***	0.106***	0.090***	0.039***	0.106***	0.089***	0.042***
	(0.003)	(0.003)	(0.010)	(0.003)	(0.003)	(0.011)	(0.003)	(0.003)	(0.011)
Number of Authors	-0.120***	-0.099***	0.013	-0.038***	-0.022**	0.046**	-0.049***	-0.032***	0.034*
	(0.008)	(0.008)	(0.017)	(0.009)	(0.009)	(0.020)	(0.009)	(0.009)	(0.020)
Journal*Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
School Fixed Effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Author Fixed Effects	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	54,046	49,218	8,908	53,864	49,077	8,891	53,981	49,160	8,898
Adjusted R^2	0.3439	0.3755	0.5328	0.3879	0.4119	0.5583	0.3833	0.4088	0.5542

PANEL B: FINANCE JOURNALS

Dependent Variable: Log(Times Cited Count)

	ONLY CITATIONS FROM TOP JOURNALS			EXCLUDING SAME JOURNAL CITATIONS			EXCLUDING SELF CITATIONS		
Any-Connected Article	0.265***	0.087*	0.176*	0.236***	0.080	0.184*	0.244***	0.081	0.202**
	(0.045)	(0.049)	(0.093)	(0.048)	(0.053)	(0.097)	(0.049)	(0.054)	(0.098)
Times Cited Count: Last 5 Years	0.112***	0.083***	-0.0001	0.0100***	0.075***	-0.001	0.0103***	0.077***	-0.002
	(0.007)	(0.007)	(0.002)	(0.007)	(0.008)	(0.032)	(0.008)	(0.008)	(0.032)
Number of Authors	-0.159***	-0.122***	0.014	-0.066***	-0.039*	0.008	-0.076***	-0.049**	0.001
	(0.020)	(0.020)	(0.044)	(0.021)	(0.021)	(0.055)	(0.021)	(0.021)	(0.055)
Journal*Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
School Fixed Effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Author Fixed Effects	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	6,395	5,893	1,167	6,381	5,879	1,165	6,393	5,891	1,167
Adjusted R^2	0.4647	0.4638	0.5991	0.5396	0.5237	0.6409	0.5347	0.5204	0.6420