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Editor Visits and Publication Success

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Abstract

Professionals invest heavily in gaining brief access to gatekeepers, yet we know little about whether such encounters affect career outcomes. We study whether an editor's visit to a university raises junior scholars' chances of publishing in that editor's journal. We link seminar visits at top-100 U.S. finance departments to junior faculty publication histories and editorial rosters for leading economics and finance journals. Because departments select whom to invite and editors choose where to visit, simple comparisons are unlikely to identify causal effects. We use seminar rotation norms: after hosting a speaker, departments typically wait several years before inviting that person again, generating predictable variation in the timing of editor visits. Estimated effects are close to zero across two-, three-, and four-year horizons. We can rule out increases of 50 percent or more relative to the baseline publication rate at every horizon and 30 percent or more after three and four years, though smaller effects remain possible. Point estimates are more positive for women, scholars at lower-ranked schools, visits by editors who travel to fewer schools, and scholar–editor pairs with greater research overlap. These patterns are suggestive of information or access benefits, but the evidence is not conclusive. We find no persistent publication gains outside the editor's journal. The evidence rules out large average publication gains from a single editor visit, while leaving open smaller gains and benefits where access is scarce.

JEL classification

A11, A14, D83, L82, I23

Keywords

professional networks, academic publishing, editorial favoritism, personal connections, causal inference

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1 Introduction

Personal connections shape career outcomes wherever gatekeepers hold discretion, from executive pay set by connected boards (Hwang and Kim, 2009) to firm policies spread through executive networks (Shue, 2013) and promotions obtained through informal contact with managers (Cullen and Perez-Truglia, 2023). Yet this evidence largely concerns established relationships or repeated interactions. We know much less about whether brief contact with an influential decision-maker can shape high-stakes outcomes in settings governed by formal evaluation. Academic publishing provides such a setting. Publishing in elite journals is consequential for academic careers: in economics, a single top-five publication increases the probability of receiving tenure by 80%; two by 230%; three by 310% (Heckman and Moktan, 2020). Can a brief opportunity to meet an editor affect whether a junior scholar subsequently publishes in that editor's journal?

We assemble new data linking seminar visits at top-100 U.S. finance departments to junior faculty publication histories and editorial rosters for leading finance and economics journals. These data allow us to track whether junior scholars publish in an editor's journal after that editor visits their department. The estimates are close to zero across two-, three-, and four-year horizons. Confidence intervals rule out increases equal to 50 percent of the outcome mean at every horizon and 30 percent at the three- and four-year horizons, but smaller effects remain possible. The estimates are more positive in settings where a visit might plausibly matter more: for female assistant professors, for assistant professors at lower-ranked schools, when editors visit fewer schools, and when the scholar and editor work on similar topics. These patterns suggest that editor visits may be more valuable when they provide information or access that would otherwise be difficult to obtain, but the evidence is not conclusive.

Editor visits are endogenous: departments choose whom to invite, and editors choose where to go. We address this using seminar rotation norms. Departments typically wait several years before re-inviting the same speaker, creating predictable variation in whether an editor is likely to visit a given school. Within the same department-year, we compare AP publication outcomes across journals whose editors face different rotation constraints. Some editors are eligible to visit, while others are blacked out because they visited recently. School-by-year fixed effects absorb conditions common to the department that year, while school-by-editor fixed effects absorb persistent differences in each department-editor relationship. The first stage is strong, with an F-statistic of 726. The exclusion restriction is that rotation-induced variation in the likelihood of an editor visit affects junior faculty publication outcomes only through the visit itself. Consistent with this restriction, junior scholars look similar when editors are more versus less likely to visit based on the rotation cycle, including in prior publications, career stage, gender, and research similarity with the editor.

The estimates remain close to zero as we add fixed effects, use rotation-induced visits, vary the definition of the rotation cycle, and aggregate the data to the institution level. These results provide evidence against a large average effect, but they do not rule out smaller effects.

The estimates remain positive at three and four years for assistant professors at lower-ranked schools, for visits by editors who give seminars at fewer schools, and for scholars whose research is closely aligned with the editor's, although the confidence intervals are wide. For female assistant professors, the estimates rise from 1.7 percentage points at two years to 5.8 percentage points at four years. These results leave open the possibility that brief editor access matters more for some groups, but they do not establish persistent gains.

An editor visit could affect an assistant professor beyond publication in the editor's journal by improving a paper published elsewhere, increasing citations, or shaping tenure prospects. We estimate a modest short-run increase in publication outside the top eight journals, but the estimates do not establish a persistent increase. The citation estimates show no clear pattern. We treat the tenure estimates as descriptive because tenure occurs once and the rotation design cannot isolate cumulative editor contact. Meeting more editors is not detectably associated with tenure.

Our estimates for a single visit contrast with evidence that sustained connections to editors are associated with publication gains. Brogaard et al. (2014) exploit editor rotations to show that an editor's university colleagues publish about twice as much in the editor's journal during the editor's term. Colussi (2018) finds that scholars with past institutional ties to editors publish more once those editors take office, and Azoulay et al. (2010) document that collaborators' publication rates fall by 5 to 8% after the unexpected death of a superstar coauthor. These studies examine relationships built through years of proximity. We also recover a positive association between multi-year co-location with an editor and publication in that editor's journal in our own data. For a single seminar visit, the estimates provide evidence against large average gains, although smaller effects remain possible. The difference suggests that the duration and depth of the relationship may matter.

We advance this literature in three ways. First, we construct novel data linking seminar visits to publication outcomes. We combine seminar records from 100 finance departments (2000-2024), publication histories for 651 assistant professors who began tenure-track positions during 2010-2019, and editorial rosters for top economics and finance journals. Second, we exploit quasi-experimental variation from seminar rotation policies. Research departments maintain informal norms discouraging repeat visits by the same speaker within 3-5 years, creating mechanical, calendar-based fluctuations in meeting opportunities plausibly unrelated to contemporaneous research quality. Third, we study brief seminar encounters rather than the sustained institutional affiliations examined in prior work.

Departments devote a disproportionate share of limited seminar slots and budgets to editors, whose invitations rise sharply after editorial appointment (Figure 1). If departments make this investment partly to improve junior faculty publication prospects, our estimates provide evidence against large average gains from a single visit. Visits may still provide feedback, visibility, or smaller publication benefits that our design cannot distinguish. Editor visits also concentrate at higher-ranked schools. Because our design covers departments on active seminar circuits, it cannot determine whether unequal access among institutions that rarely host editors widens publication gaps.

The rotation design captures routine editor visits among departments on active seminar circuits. It does not cover repeated or deliberately cultivated relationships with editors. The estimates are more positive at lower-ranked schools in our sample, but their wide confidence intervals leave open whether visits matter more where editor access is scarce. Our results come from economics and finance and may not extend to fields with different editorial and peer-review processes.

We observe publications and citations, but not desk rejections, referee assignments, or revision guidance. A visit could change those decisions without changing either outcome. We also know whether an editor visited a school, not whether a particular assistant professor met with that editor. We account for overlap between a faculty member's research and the editor's, but that does not tell us who actually met. Our estimates are about editor visits to schools, not face-to-face meetings or specific editorial decisions.

The paper proceeds as follows. Section 2 reviews related literature. Section 3 describes institutional background on seminar rotation norms and editorial discretion. Section 4 presents data sources and sample construction. Section 5 details our identification strategy. Section 6 reports results. Section 7 concludes.

2 Literature Review

Our work contributes to research on the returns to professional connections, which predict outcomes in settings that range from friendship networks and economic mobility (Chetty et al., 2022) to boards and labor markets (Hwang and Kim, 2009; Cullen and Perez-Truglia, 2023). We study these returns in academic publishing, where editors hold discretion over a scarce, career-defining resource.

Most existing work documents correlations between author–editor relationships and publication success and points to distinct channels. One is author identity. Double-blind review reduced acceptance rates for authors from near-top institutions (Blank, 1991), and editors apply different standards to highly published authors (Card and DellaVigna, 2020). A second is personal ties. Papers by authors connected to the editor are cited more, which Laband and Piette (1994) interpret as editors using their networks to find good papers rather than to favor friends. An editor's former graduate students and faculty colleagues publish more articles in the editor's journal while the editor is in charge (Colussi, 2018), and an author's publications in a journal rise when a coauthor joins its editorial board (Ductor and Visser, 2022). Moreover, Carrell et al. (2024) show that authors with PhD, employment, or NBER ties to the handling editor are substantially more likely to avoid desk rejection. These studies face a fundamental identification challenge: personal connections correlate with researcher quality, making it difficult to separate favoritism from legitimate information advantages or quality signaling.

The most directly related work is Brogaard et al. (2014), who use editor rotations to show that an editor's current university colleagues publish about 100% more papers in the editor's journal during the editor's tenure than in years when the editor is not in charge. These inside articles also receive 10–20% more citations, suggesting that editors use their information advantages to identify high-quality work rather than to favor weaker papers. This evidence leaves two questions open. First, because the treatment is multi-year institutional co-location, it cannot isolate the discrete interpersonal interactions (seminars, conferences, informal meetings) that build and activate professional networks (Chai and Freeman, 2019; de Leon and McQuillin, 2020). Second, because editors reside at a small set of elite universities, evidence from their home institutions does not speak to the networking opportunities available to researchers at other schools. These questions matter given that sustained contact with editors is concentrated at the few institutions that employ them, while the form of contact departments across the hierarchy are able to obtain is the brief seminar visit. Whether the returns to sustained contact extend to these encounters is unknown.

We measure face-to-face contact through seminar visits and exploit quasi-random variation from rotation policies as an instrument for editor–author meetings. This design isolates discrete networking events rather than broad institutional affiliation, allowing us to examine dynamic effects and distinguish transitory timing effects from persistent productivity changes. Because editors visit departments across a wider institutional hierarchy, our sample captures effects for faculty at non-elite universities, broadening inference beyond the small set of institutions that dominate editorial boards.

Methodologically, we build on work using institutional features to identify causal effects when randomization is infeasible (Angrist and Krueger, 2001; Roberts and Whited, 2013). Our design belongs to the family of rotation designs, in which an institutional schedule rather than the agents themselves determines who interacts with whom.

3 Institutional Background

Our identification strategy exploits two institutional features: seminar rotation norms and editorial discretion.

3.1 Seminar Rotation Norms

Research departments maintain informal rotation norms discouraging repeat speaker visits within three to five years. Although rarely formalized, seminar organizers enforce these norms through decentralized coordination. The resulting calendar-based constraints on meeting opportunities provide our instrument for editor-faculty meetings.

3.2 Editorial Discretion

Editors-in-chief and associate editors at the eight journals we study wield substantial discretion over publication decisions, though the degree of influence varies by role and journal structure. Editors-in-chief typically screen submissions for desk rejection, select referees, interpret conflicting referee reports, and make final publication judgments. Associate editors often handle subsets of submissions within their areas of expertise, with varying degrees of autonomy depending on journal-specific governance structures.

At each decision point—whether made by editors-in-chief or associate editors—subjective assessments of paper importance and journal fit create scope for personal connections to influence outcomes. Familiarity with an author's work established through a seminar presentation might similarly affect desk rejection decisions, referee selection, or interpretation of borderline reports. However, because we do not observe these editorial choices and only see final publication outcomes, our analysis tests whether seminar-based meetings have any aggregate impact on subsequent publication probabilities in the editor's journal.

Editorial roles are harmonized across journals based on functional decision authority. Table A1 summarizes the resulting classification for the top three finance journals, including the time-varying editorial structure at the *Journal of Financial Economics*.

4 Data and Summary Statistics

4.1 Data Sources and Sample Construction

Our analysis combines three primary data sources to construct a panel dataset linking faculty members, journal editors, departmental seminars, and publication outcomes.

Junior Faculty Data: Our analysis focuses on junior faculty at research-active universities, where publications in top journals play a critical role in tenure and promotion. We begin with the top 100 finance departments based on the *U.S. News* Business School Rankings used in Getmansky Sherman and Tookes (2022) and Gertsberg (2026b), which provide a consistent set of research-intensive institutions. The sample period covers 2010–2019 to avoid COVID-19 disruptions to seminars and research productivity.

Given typical publication lags, we observe outcomes through 2023 for seminars occurring in 2019, ensuring sufficient time for manuscripts to progress through the review process.

Following Brogaard et al. (2014), for a seminar visit in year t, we begin tracking publication outcomes in $t+2$ as papers published $t+1$ are likely already in the review pipeline at the time of the editor meeting, leaving limited scope for treatment effects. We extend the window to $t+3$ and $t+4$ to capture longer review cycles.

We use the Academic Analytics Research Center (AARC) database to identify scholars in the first five years of a tenure-track appointment at these universities during 2010–2019 who are at risk of meeting editors during the treatment period. Focusing on this early-career window captures the stage when seminar exposure and interactions with editors are most likely to influence publication trajectories, given the limited development of professional networks. To maintain a clean and comparable sample of early-career faculty, we exclude individuals whose Ph.D. year precedes their first observed year at a sample institution by more than two years, and we drop scholars in the year they obtain tenure. We additionally exclude scholars who exit before we are able to observe their $t+2$ outcomes, as these individuals cannot contribute to the post-treatment publication window we analyze. Applying these criteria yields 651 assistant professors at 91 universities.

We hand-collect curricula vitae for these scholars and use OpenAI's GPT-4o model (temperature set to zero) to parse CVs and extract: (1) publication histories (paper titles, journals, years) across the top 400 journals in the RePEc ranking, capturing the vast majority of promotion-relevant peer-reviewed outlets in economics and finance; (2) employment histories (affiliations, dates); and (3) Ph.D. credentials. We extract publication data from CVs because they provide more complete and current records than AARC bibliometric data, particularly for recent publications. AARC defines our sampling frame and validates extracted data for earlier years.

In cases where CVs were unavailable (for example, due to early exits from academia), we obtained the information above through manual searches using Google Scholar, former faculty websites, SSRN, and dissertation records. For scholars with no publications, we determine research specializations based on publicly available working papers.

Editorial Data: We focus on individuals who have ever served as editors-in-chief or associate editors at the top-three finance journals: the *Journal of Finance*, *Journal of Financial Economics*, and *Review of Financial Studies*. When these scholars ("finance editors") also serve on the editorial boards of the top-five economics journals (the *American Economic Review*, *Econometrica*, *Journal of Political Economy*, *Quarterly Journal of Economics*, and *Review of Economic Studies*) we treat them as relevant editors for finance APs as well. Editors drawn into top economics journals from finance typically cover areas that are adjacent to or overlapping with finance, making them influential gatekeepers for early-career finance researchers. This approach ensures that we capture the full set of influential editors whose decisions meaningfully shape publication opportunities in finance.

We compile editorial histories for these eight journals from mastheads, editorial announcements, and internet archives, identifying all editors and associate editors, their affiliations, and terms starting in 2000. We collect CVs for all editors using the same process for assistant professors described above, and classify each as editor-in-chief or associate editor.

Overall, our sample period (2010–2019) covers 201 individuals who served as editors-in-chief or associate editors at these journals. Appendix Table A2 reports the number of editors by journal and role.[1]

Seminar Data: We construct a novel, comprehensive dataset of seminar activity at the sample finance departments from 2000 to 2024 using three complementary data-collection strategies. First, we systematically gathered historical seminar schedules from departmental websites and recovered missing years using archived webpages from the Internet Archive's Wayback Machine. Second, we contacted departments directly (seminar coordinators, administrators, and faculty) to obtain schedules for years not publicly available. This process yielded seminar series for 75 universities, with coverage improving markedly after 2010.[2] Third, to fill remaining gaps in coverage, we extracted seminar presentations listed on the

[1] The per-journal counts in Appendix Table A2 sum to 264 editor-journal pairs because editors can serve at multiple journals: of the 201 individuals, 146 serve at one journal, 48 at two, 6 at three, and 1 at four.

[2] See Gertsberg (2026a) for details on data construction and coverage.

personal CVs of editors and faculty, capturing visits that were not recorded in departmental archives.

Combining these sources allowed us to reconstruct seminar schedules for 91 universities; the remaining institutions appear not to host regular seminar series. This triangulated collection strategy produces a very comprehensive long-run record of seminar activity available for top U.S. finance departments and enables precise measurement of editor–department interactions.

For each seminar, we record the speaker's name, date, host department, and topic (when available). In total, our database contains 7,664 seminars given by individuals who ever held associate editor or editor-in-chief positions at the top-3 finance and top-5 economics journals from 2000 to 2024.[3] Of these seminars, 1,834 occurred during our sample period (2010–2019) at times when the finance editors were actively serving in their editorial roles at one of these top-eight journals.

Research Area Alignment: To control for editors preferentially visiting faculty in aligned research areas, we construct a similarity measure for each faculty-editor pair. Using the CVs above, we classify research profiles across fourteen categories: seven finance (Theory Corporate, Empirical Corporate, Asset Pricing, Theoretical Asset Pricing, Financial Intermediation, Household Finance, Other) and seven economics (Labor Economics, Macroeconomics, Development Economics, Industrial Organization, Public Economics, Microeconomic Theory, Other).

We use GPT-4o to analyze the titles of publications and working papers listed on each CV and assign percentage weights across categories summing to 100.

For each faculty-editor pair, we compute the cosine similarity between their research profile vectors. Let $\mathbf{a}_i = (a_{i1}, \ldots, a_{i14})$ denote faculty member i's research profile, where a_{ik} repre-

[3]This count includes seminars by individuals who served as editors at the top-5 economics journals only. However, we exclude these economics-only editors from our analyses because rotation schedules for economists are difficult to accurately establish: they may be invited by multiple departments within the same university (e.g., economics, finance, public policy), which do not necessarily coordinate visits, and we do not observe many of these seminar schedules.

sents the share of research in category k (i.e., $\sum_{k=1}^{14} a_{ik} = 1$). Similarly, let $\mathbf{e}_j = (e_{j1}, \ldots, e_{j14})$ denote editor j's profile. The cosine similarity between faculty member i and editor j is:

$$\text{CosineSimilarity}_{ij} = \frac{\sum_{k=1}^{14} a_{ik} \cdot e_{jk}}{\sqrt{\sum_{k=1}^{14} a_{ik}^2} \times \sqrt{\sum_{k=1}^{14} e_{jk}^2}} \tag{1}$$

This measure ranges from 0 (completely orthogonal research profiles) to 1 (identical research profiles). We include this cosine similarity as a time-varying control in our regressions to account for research area alignment between faculty and editors.

Panel Construction and Sample Restrictions: Starting from the sample of early-career faculty defined above, we construct a school–assistant professor–editor–journal–year panel. For each school–AP pair, we match to every editor–journal–year observation during the first five years of the AP's tenure-track career in which the editor is active. This creates the full set of potential publication opportunities for each AP–editor pair.

We exclude AP–editor pairs with institutional overlap during the AP's final three PhD years. Such overlaps are potential confounders: prior work (e.g., Colussi (2018)) shows that early-career proximity may raise publication probabilities through advisor-type connections. Removing these pairs ensures that any estimated effects reflect interactions occurring through seminar exposure rather than pre-existing institutional relationships.

We merge the school–assistant professor–editor–journal–year panel with seminar data to create treatment indicators (editor visited school in year t) and construct our rotation instrument. For editor-school pairs with at least one observed seminar, we calculate years since the previous visit and define blackout indicators. A blackout period equals one if the editor visited the school in any of the prior three years. We choose a three-year window for our main analysis because reinvitations begin to increase at this horizon (Figure 2), although our results are robust to alternative windows.

Not all editor–school pairs have observed seminar visits during our sample. For pairs where no visit occurs, or for years prior to the first observed visit, we cannot define years since last visit.

The preferred specification retains all observations, including never-visit and pre-visit periods (the “no-drop sample”). For observations with no previous visit, we code the blackout indicator as zero. School-by-editor fixed effects absorb baseline differences in meeting propensities. As a robustness check, we re-estimate the models using only AP–editor pairs with a defined rotation clock (the “drop sample”).

In our main analysis, we pool editors-in-chief and associate editors to preserve statistical power. Because editors-in-chief hold the strongest decision authority, we also estimate the specification using editors-in-chief only.

Treatment equals one if an editor visited the faculty member’s school in year t (during the faculty member’s first five tenure-track years). Outcomes measure publication in the editor’s journal within two, three, or four years of the visit.

4.2 Summary Statistics

Table 1 presents descriptive statistics for our main analysis sample. The table distinguishes between school-editor pairs with defined rotation schedules (where at least one prior seminar has occurred, allowing us to compute the blackout instrument) and pairs with undefined seminar history (where the editor has never visited the school).

[Insert Table 1 About Here.]

Selection into Editor Visits: Editor visits select strongly on institutional prestige. Among school-editor pairs with defined rotation schedules, the average school rank is 14 for observations with an editor visit versus 22 for observations without a visit (lower numbers indicate higher-ranked departments). School-editor pairs without any seminar history have even lower-ranked institutions on average (rank 44). Editors rarely visit less prestigious departments.

Selection does not extend to research alignment. The average cosine similarity between assistant professor and editor research profiles is nearly identical across all groups (0.78).

Publication Outcomes and the Selection Problem: The raw data show similar or slightly higher publication rates when editors visit. Among pairs with defined rotation schedules, the two-year publication rate is 9.9% both with and without an editor visit. At three years, the rate is 20.3% with a visit versus 19.8% without (a 0.5 percentage-point difference). At four years, the corresponding rates are 29.2% and 28.4% (a 0.8 percentage-point difference). These gaps range from zero to approximately 3% relative to the no-visit baselines.

Pairs without any seminar history have much lower publication rates: 7.0%, 13.9%, and 20.3% at two, three, and four years. Editors do not visit these lower-ranked schools, and their assistant professors publish less frequently in top journals. Of course, this pattern likely reflects selection, not any causal effect of visits.

The Rotation Instrument: The blackout period indicator (our instrument) shows variation consistent with rotation norms. Among school-editor pairs with observed visit history, the probability that a school hosts the editor is 3.7% in years within three years of the editor's previous visit and 6.7% once the editor is eligible again — a reduction of roughly 45%. Equivalently, only 24.6% of visits occur within three years of the editor's previous visit, even though 37% of all pair-years with visit history fall in such windows.

The mean time since last visit is approximately 5.5–6.0 years among pairs with defined schedules, with standard deviations around 3.5–3.8 years. Some school-editor pairs go many years between visits; others meet more frequently.

Sample Composition: Our analysis sample includes approximately 281,000 school-assistant professor-editor-journal-year observations. Roughly 47,000 (17%) involve school-editor pairs with defined rotation schedules where we can compute the blackout instrument. The remaining 234,000 (83%) involve school-editor pairs with no prior visit history. Editor visits occur in roughly 5.6% of observations with defined schedules, and the average faculty member is approximately two years post-tenure-track start.

5 Identification Strategy

5.1 The Endogeneity Problem

As the summary statistics reveal, editor visits are highly selective. A naive regression of publication probability on editor visit indicators would suffer from severe positive selection bias. Editors disproportionately visit elite departments (average rank 14 versus 22), which produce higher-quality research independently of editor contact. Departments may strategically time editor invitations around periods when faculty have papers under review, making visits consequences rather than causes of submission activity. Authors strategically target journals where their research fits, and editors preferentially visit departments with faculty working in relevant areas, creating spurious correlations between meetings and publications through research area alignment.

These selection channels make ordinary least squares estimates uninterpretable: they combine the omitted-variable, simultaneity, and self-selection problems. We require variation in editor contact that is plausibly independent of research quality, strategic behavior, and unobserved determinants of publication success.

5.2 Rotation Norms as Quasi-Experimental Variation

We exploit the seminar rotation norms described in Section 3 to generate quasi-experimental variation in editor visits.

Figure 2 illustrates these rotation patterns. The figure plots the probability of hosting a finance editor seminar against years since the editor's last visit to that school. School-editor pairs that met within the past three years (the "blackout period") have a 3.5% probability of hosting the editor again. This probability rises to 5.5% at 4–6 years, peaks at 6.3% at 7–9 years, then declines to 4.7% for pairs that last met 10+ years ago. The initial jump from 3.5% to 5.5% reflects rotation norms discouraging repeat invitations within short windows.

[Insert Figure 2 About Here.]

We identify the effect from changes in each department-editor pair's rotation status over time, relative to other department-editor pairs in the same department-year.

We define Blackout_{asejt} as an indicator equal to one if editor e from journal j visited school s (where faculty member a is employed) in the three years before year t. Our instrumental variables strategy uses blackout status to predict editor visits, then estimates how these predicted visits affect publication outcomes.

The first-stage relationship is:

$$\text{EditorVisit}_{asejt} = \alpha + \beta \cdot \text{Blackout}_{asejt} + \gamma X_{at} + \delta_{se} + \sigma_{st} + \mu_j + \epsilon_{asejt} \tag{2}$$

where $\text{EditorVisit}_{asejt}$ equals one if editor e from journal j visited school s (employing faculty member a) in year t. The vector X_{at} includes time-varying characteristics of faculty member a (years since tenure-track start–i.e., tenure controls–, the natural logarithm of cumulative top-8 publications plus one, and cosine similarity between the research profiles of faculty member a and editor e) as well as AP gender (*Female*). We also include Ph.D. school fixed effects to account for persistent differences in doctoral training and networks; δ_{se} are school-by-editor fixed effects; σ_{st} are school-by-year fixed effects; and μ_j are journal fixed effects.

The two interacted fixed effects define the identifying comparison. School-by-editor fixed effects absorb persistent differences in each school-editor relationship, including research overlap, geographic proximity, and historical ties. School-by-year fixed effects absorb all factors common to a department in a particular year, including hiring, seminar activity, and department-wide publication prospects. We use the remaining variation to test whether APs at the same department in the same year are more likely to publish in the journal of an editor whose rotation eligibility induced a visit.

The second-stage equation estimates the causal effect of editor visits on publication outcomes:

$$\text{Publish}_{asejt,t+k} = \theta + \lambda \cdot \widehat{\text{EditorVisit}}_{asejt} + \eta X_{at} + \delta_{se} + \sigma_{st} + \mu_j + \nu_{asejt} \quad (3)$$

where $\text{Publish}_{asejt,t+k}$ equals one if faculty member a at school s published in journal j within k years after year t (we examine $k \in \{2, 3, 4\}$); and $\widehat{\text{EditorVisit}}_{asejt}$ is the predicted visit probability from equation 2. The second stage asks whether assistant professors are more likely to publish in the journal of an editor whose rotation eligibility induced a visit, relative to journals associated with editors facing different rotation constraints in the same department and year.

Under the exclusion and monotonicity assumptions, λ identifies the local average effect of a rotation-induced editor visit on publication in that editor's journal, for school-editor relationships whose visit timing responds to rotation eligibility. Because identification operates within school-year, the estimate is net of shocks or effects common to the department that year. Section 6.6 and Appendix Table A11 characterize these complier pairs.

To see the identifying variation concretely, hold the department and year fixed. The first stage uses differences in rotation status across department-editor pairs to predict the probability of each editor visit. The second stage asks whether APs at the department subsequently publish in the journal of an editor whose visit was induced by rotation eligibility. School-by-editor fixed effects measure this change relative to each pair's own history.

5.3 An Institution-by-Journal-by-Year Panel

The faculty-school-editor-journal-year panel above requires us to take a stand on which assistant professor could have met each editor. We therefore aggregate the data to the institution-journal-year level. Following Fisman et al. (2017), we ask whether APs at an institution account for a larger share of a journal's publications after one of that journal's editors visits the institution. We estimate the following specification using OLS:

$$\text{PubShare}_{ijt} = \beta_1 \text{Visit}_{ij,t-2} + \beta_2 \text{CosineSim}_{ijt} + \phi_{ij} + \psi_{jt} + \omega_{it} + \epsilon_{ijt} \tag{4}$$

where for institution i, journal j, and year t, PubShare_{ijt} is institution i's share of journal j's publications that year (publications in journal j by institution i's assistant professors, divided by such publications across all institutions in journal j); $\text{Visit}_{ij,t-2}$ is an indicator equal to one if an editor of journal j active in year $t-2$ gave a seminar at institution i in $t-2$; CosineSim_{ijt} is the mean cosine similarity between the research profiles of institution i's assistant professors and the editors of journal j; ϕ_{ij} is an institution-by-journal fixed effect; ψ_{jt} is a journal-by-year fixed effect; and ω_{it} is an institution-by-year fixed effect. We measure the visit at $t-2$ because a seminar can affect a publication only after the lag required to submit, revise, and print a paper.

We test whether APs at the same institution in the same year account for a larger share of publications in journals whose editors recently visited the institution. Institution-by-year fixed effects absorb changes in the institution's overall publication output. Journal-by-year fixed effects absorb changes in each journal's publication slots. Institution-by-journal fixed effects absorb persistent differences in each relationship, including field overlap and the institution's historical publication rate in the journal. We cluster standard errors at the institution level.

We identify β_1 from changes within institution-journal pairs, relative to other journals at the same institution in the same year and other institutions publishing in the same journal-year.

To address the concern that editors time visits to institutions already rising in their journal, we further instrument $\text{Visit}_{ij,t-2}$ with the pair's recent rotation exposure: the share of journal j's editors active in $[t-5, t-3]$ who visited institution i during that window.

Because departments avoid inviting the same editors in close succession, higher recent exposure lowers the probability of a fresh visit at $t-2$. We use this rotation-induced variation in visits to estimate whether APs at the institution account for a larger share of the journal's publications.

5.4 Identifying Assumptions and Threats

Our identification strategy requires two key assumptions.

Instrument Relevance: Blackout periods must strongly predict editor visits. Figure 2 provides visual evidence for this relationship: the probability of hosting an editor jumps sharply once a school-editor pair moves beyond the three-year blackout window.

Table 2 reports the first stage. In Column 1, the specification used in the 2SLS, the dependent variable is an indicator for any visit by editor e at school s in year t, including a pair's first visit. Column 2 restricts the sample to school–editor pairs with at least one prior observed visit, so that the rotation clock is computed from observed history rather than coded as zero; it shows the relationship is not driven by pairs without visit histories. Column 3 restricts to editors-in-chief, the group with final decision authority, and corresponds to the appendix results for that sample. Column 4 uses the return visit recorded in the rotation-clock panel as the dependent variable; first visits cannot enter the clock, so this column isolates the re-invitation margin that the norm governs directly, at a lower baseline rate. Throughout the paper we report the KP rk Wald F, the first-stage F statistic that remains valid when standard errors are clustered, as ours are at the school level; it is interpreted like a conventional first-stage F statistic, with larger values indicating a stronger first stage. In our main specifications, the first-stage F statistics exceed even the stricter thresholds that Lee et al. (2022) show are required for valid inference.

Blackout periods substantially reduce editor visits. Being within the blackout period reduces the probability of a visit by 25.2 percentage points relative to a baseline visit rate of 2.5% (KP rk Wald F $\approx$ 726). The relationship holds in every column, and none of the individual-level controls predicts visits once the fixed effects are absorbed: within a pair, the rotation clock is essentially the only systematic driver of visit timing, consistent with a mechanical norm.

[Insert Table 2 About Here.]

Exclusion Restriction: Being in a blackout period must affect publication outcomes only through reducing the probability of a current visit. Because the exclusion restriction cannot be tested directly, its defense must rest on institutional knowledge of the setting and on indirect evidence (Roberts and Whited, 2013). We assess the plausibility of this assumption in three ways.

First, blackout status must be uncorrelated with other determinants of publication success. The main worry is relationship strength. Schools with close ties to an editor host that editor more often, and therefore spend more years in blackout, while their faculty independently publish more in that editor's journal. The school-by-editor fixed effects absorb these ties, along with all other time-invariant features of the pair, such as editorial preferences for certain departments. What remains is variation in a pair's rotation status over time, evaluated against other editors' status in the same school-year, and within a pair that timing follows mechanically from past visit dates and the rotation norm.

A second potential concern is department-wide dynamics. Schools may invite editors during publication upswings, and because blackout years follow visits by construction, they could inherit the tail of such an upswing. The exclusion restriction is testable on this margin. If blackout status matters only through own visits, it should not predict the publication outcomes of *other* assistant professors matched to *other* editors in the same department-year. Column 1 of Appendix Table A3 shows some evidence of this pattern under school-by-editor fixed effects alone. In our design, the school-by-year fixed effects in the main specification address this concern, driving the placebo to exactly zero (Column 2). Any remaining violation would have to operate within a department-year, differentially across editors, and produce a delayed publication response, which the lagged-visit analysis in Section 6 shows is absent (Appendix Table A4).

Third, we examine whether observable characteristics differ by blackout status. Table 3 presents means and standard deviations of assistant professor characteristics (research productivity, years since tenure-track start, gender composition, and research similarity to the editor) separately for blackout and eligible years, for pairs with an observed rotation clock.

[Insert Table 3 About Here.]

We find no economically meaningful differences in these characteristics as a function of blackout status. This supports the assumption that blackout status is uncorrelated with publication determinants other than through current visits.

6 Results

6.1 Do Editor Visits Increase Publication Probability?

Table 4 tests whether a rotation-induced editor visit increases an assistant professor's probability of publishing in the visiting editor's journal. Each observation pairs an assistant professor with an editor, the editor's journal, the assistant professor's school, and calendar year t. The treatment equals one if the editor visits the school in year t. The dependent variables equal one if the assistant professor publishes in the editor's journal in year $t+2$, during years $t+2$ through $t+3$, or during years $t+2$ through $t+4$, respectively. If seminar contact improves the assistant professor's access to the editor or raises the visibility of the assistant professor's research, the coefficient on the instrumented visit should be positive.

[Insert Table 4 About Here.]

Only the publication window changes across columns. Columns 2 and 3 add one and two years, respectively, to the year-$t+2$ outcome in Column 1. We treat Column 3 as the benchmark because the four-year window gives an editor visit the longest opportunity to affect a publication. Standard errors are clustered by school.

Across the three publication windows, we fail to find evidence of a persistent increase in publication probability. The estimate is 1.7 percentage points for publication in year $t + 2$ and is statistically significant at the 10 percent level. Expanding the window through year $t + 3$ reduces the estimate to 1.1 percentage points, which is not statistically significant. The benchmark four-year estimate is -0.2 percentage points and is also not statistically significant. The positive two-year estimate does not persist as the publication window expands.

Relative to the mean publication rates, the estimates correspond to 23 percent at the two-year horizon, 8 percent at the three-year horizon, and essentially zero at the four-year horizon. The two-year estimate is not economically trivial. However, because the longer windows include every publication counted in the shorter windows, the decline across horizons provides little evidence that the two-year estimate captures a lasting increase in publication probability.

Appendix Table A3 tests whether publication timing can explain the positive two-year estimate. Column 3 estimates a 1.9 percentage-point increase in publication in year $t + 1$, slightly larger than the 1.7 percentage-point estimate in year $t + 2$. An editor visit in year t is unlikely to cause a new paper to appear in print by year $t + 1$ because submission, review, and production take longer. Because the 1.9 percentage-point one-year estimate matches the 1.7 percentage-point two-year estimate, the two-year coefficient may partly capture papers already in review or production when the editor visited. However, the one-year estimate cannot tell us whether the visit helped an assistant professor complete a revise-and-resubmit that was already under review at the editor's journal.

An editor visit could matter more when the visiting editor has final authority over publication decisions. Appendix Table A5 therefore restricts the sample to editors-in-chief. The estimated effects are -0.3, -1.0, and -2.6 percentage points at the two-, three-, and four-year horizons, and the estimates do not differ statistically from zero. Appendix Table A6 addresses a separate concern: school–editor pairs with no prior observed visit lack a defined rotation history. Restricting the sample to pairs with at least one prior visit yields estimates of 2.4, 2.0, and −3.6 percentage points, with confidence intervals that include zero. Both four-year estimates are negative. Across both restrictions, we fail to find evidence of a persistent increase in publication probability.

The control variables behave in familiar ways. Research overlap and prior top-eight publications predict higher publication probability at all three horizons, while the female indicator predicts lower publication probability. Of course, these are conditional correlations, not causal effects.

The main analysis assigns each school-editor visit to every assistant professor at the school because we do not observe individual meetings. The institution-by-journal-by-year analysis avoids this assignment by aggregating publications across assistant professors at the same school. Following Fisman et al. (2017), the analysis tests whether assistant professors at a visited school later account for a larger share of the visiting editor's journal publications.

Table 5 reports an instrumental-variables estimate of 1.9 percentage points for the school's share of the journal's publications. The estimate is 2.6 percentage points when the outcome is the visiting journal's share of the school's top-eight publications. Both confidence intervals include zero. In Figure 3, the institution-level estimates fluctuate around zero before and after a visit. The institution-level results therefore corroborate the assistant-professor-level estimates in Table 4: neither design provides evidence that editor visits increase publication in the visiting editor's journal.

[Insert Figure 3 About Here.]

[Insert Table 5 About Here.]

We next return to the assistant-professor-level design and test whether the estimates depend on the three-year blackout threshold. Table 6 redefines the blackout period as two, four, or five years since the previous seminar. Across the nine alternative specifications, the coefficients range from -0.008 to 0.017. None differs statistically from zero, although the confidence intervals remain consistent with effects in either direction. The alternative definitions retain strong first stages, with KP rk Wald F statistics ranging from 445.7 to 616.4. The estimates therefore fail to indicate that the main pattern is specific to the three-year definition.

[Insert Table 6 About Here.]

The blackout instrument depends on each editor's past visits, so a delayed effect of an earlier visit could make blackout status affect current publications directly. Appendix Table A4 tests this possibility by measuring publication in the editor's journal only in year t and separately instrumenting visits at $t-2$, $t-3$, and $t-4$ with blackout status at the same lag. The final column instead instruments any visit from $t-4$ through $t-2$ with average blackout status over that interval. The estimates are -1.1, -7.2, and -0.2 percentage points for the three individual lags and -3.6 percentage points for any visit during the interval. Standard errors range from 1.7 to 8.7 percentage points, and KP rk Wald F statistics range from 44.8 to 414.5. Precision is lowest at $t-4$, where the sample falls to 9,157 observations. Every confidence interval includes zero. We therefore fail to find evidence of a positive delayed payoff in this test, but the imprecision at longer lags means we cannot rule out effects in either direction.

6.2 Comparison to OLS Estimates

To distinguish the cross-sectional association from the within-pair timing comparison, Table 7 estimates OLS specifications with progressively stricter fixed effects. The first specification for each publication horizon includes journal, year, PhD-school, and tenure-year fixed effects but excludes school-by-editor and school-by-year fixed effects. In these specifications, editor visits predict increases of 1.3 percentage points within two years, 3.3 percentage points within three years, and 4.9 percentage points within four years. These associations equal 17, 22, and 23 percent of the corresponding mean publication rates, and all three coefficients differ statistically from zero. Because these specifications compare outcomes across school–editor relationships, the estimates combine any effect of a visit with selection into which editors visit which schools.

[Insert Table 7 About Here.]

Adding school-by-editor fixed effects changes the estimates to −0.3, −0.1, and 0.3 percentage points at the two-, three-, and four-year horizons. None of these estimates is statistically significant. These specifications compare years when the same editor does and does not visit the same school, removing persistent differences across school–editor relationships. Replacing year fixed effects with school-by-year fixed effects yields estimates of −0.4, −0.2, and 0.1 percentage points. These estimates are also not statistically significant. The attenuation from the cross-sectional associations to the within-pair estimates is consistent with positive selection across school–editor relationships. However, the OLS timing comparison does not identify the causal effect of a visit if schools schedule visits when publication prospects change. The rotation instrument addresses this remaining source of timing endogeneity.

At the three- and four-year horizons, the IV estimates also resemble the OLS estimates with the full set of fixed effects. The three-year IV estimate is 1.1 percentage points, compared with −0.2 percentage points in OLS, and the four-year IV estimate is −0.2 percentage points, compared with 0.1 percentage points in OLS. None of these four estimates is statistically significant. By contrast, OLS without school-by-editor or school-by-year fixed effects yields estimates of 3.3 and 4.9 percentage points at the same horizons. The longer-horizon IV estimates therefore do not exhibit the inflation relative to OLS that Jiang (2017) documents in published finance applications. At the two-year horizon, the IV estimate is 1.7 percentage points and differs statistically from zero at the 10 percent level, while the full-fixed-effects OLS estimate is −0.4 percentage points and is not statistically significant. As discussed above, the similarly sized estimate for publication in year $t + 1$ suggests that the two-year IV estimate may capture papers already in review or production when the editor visited.

6.3 Statistical Power and Effect Size Bounds

Statistical insignificance alone does not establish a zero effect, so Table 8 quantifies the range of effects that the estimates can detect and exclude. Panel A reports minimum detectable effects at 80% power and 95% confidence intervals for the IV estimates, while Panel B reports the same measures for OLS with the full set of fixed effects. The IV minimum detectable effects are 2.8, 4.6, and 5.9 percentage points at the two-, three-, and four-year horizons, equal to 37.6, 30.7, and 27.0 percent of the corresponding mean publication rates. The upper confidence bounds are 3.7, 4.3, and 3.9 percentage points, equal to 49.1, 29.0, and 18.0 percent of the corresponding means. The IV estimates therefore rule out increases of 50 percent of the mean at all three horizons and increases of 30 percent at the three- and four-year horizons, but not at the two-year horizon. The OLS upper bounds are 0.2, 0.5, and 1.0 percentage points, equal to 2.1, 3.2, and 4.6 percent of the corresponding means. These bounds exclude large positive effects, but they do not establish that the effect of an editor visit is zero.

[Insert Table 8 About Here.]

One concern is that our data may fail to capture publication advantages from connections to editors more generally. Appendix Table A7 replicates the co-location specification of Brogaard et al. (2014) over our sample period using assistant professors' school-by-journal-by-year publication counts. The treatment indicates that a colleague served as an editor two years earlier, and the four columns add journal-by-year, school-by-journal, and school-by-year fixed effects sequentially. The estimates range from 0.198 to 0.384 additional publications and are positive in every specification, with wild-cluster bootstrap p-values ranging from 0.002 to 0.097. In the specification with all three sets of fixed effects, the estimate is 0.198 publications, or 23 percent of the 0.867 mean, and is statistically significant at the 10 percent level. The same publication data therefore recover a positive association for sustained co-location with editors, in contrast to the estimates for a single seminar visit.

6.4 Heterogeneity

Our full-sample estimates may conceal larger effects for groups to whom meeting an editor is more valuable. We therefore re-estimate the model within four sample splits.[4]

The first split compares female and male assistant professors. Female economists have fewer collaborators (Ductor et al., 2023). Following #MeToo, junior women in economics and finance also began fewer projects, largely because they formed fewer collaborations with new senior male coauthors (Gertsberg, 2026b). At a large financial institution, informal interactions with male managers gave male employees a promotion advantage that female employees did not share (Cullen and Perez-Truglia, 2023). Formal mentoring, however, improved career outcomes for female assistant professors in economics (Ginther et al., 2020). We therefore expect editor visits to have larger effects for female assistant professors if structured access partly substitutes for informal connections.

The second split compares assistant professors at top- and lower-ranked schools. We split the 91 schools in our sample at the median finance-department rank of 46 and classify ranks of 46 or better as top-ranked. Author identity and prominence can affect peer-review outcomes (Blank, 1991; Huber et al., 2022), and economics editorial boards are concentrated among scholars at elite universities (Baccini and Re, 2025). We therefore expect visits to have larger effects at lower-ranked schools if direct contact gives editors information that institutional affiliation does not convey.

The third split compares editors by seminar activity. For each editor, we count the distinct schools visited in each calendar year and use the highest annual count. The 75th percentile is two schools, so we classify the 26 editors with a peak annual count of three or more as high-activity and the remaining 175 editors as low-activity. By construction, low-activity editors never visit more than two schools in a calendar year, while high-activity editors visit at least three schools in their busiest year. We therefore expect visits to have larger effects for low-activity editors if an editor's other seminars reduce the value of any one visit as an

[4] We use split samples to preserve the strength of the instrument.

opportunity to meet junior faculty.

The fourth split compares assistant professor–editor pairs by research similarity. Of the 85,668 pairs with nonmissing cosine similarity, 21,417 exceed the pair-level 75th-percentile cutoff of 0.913 and enter the top-quartile group. The remaining 64,251 form the comparison group. We observe department visits rather than individual meetings, so we cannot determine which assistant professors interact with the visiting editor. We expect assistant professors to be more likely to interact with visiting editors who share their research areas and to receive more relevant feedback from them. We therefore expect editor visits to have larger effects among top-quartile pairs.

Table 9 reports 24 subgroup-by-horizon estimates. Four estimates are positive and statistically significant: 6.7 percentage points for lower-ranked schools at two years, 2.7 percentage points for low-activity editors at two years, 4.8 percentage points for top-quartile research-similarity pairs at two years, and 5.8 percentage points for female assistant professors at four years. In every split and at every horizon, the point estimate is larger for the subgroup we hypothesized would benefit more. For female assistant professors, the estimates rise from 1.7 percentage points at two years to 4.1 at three years and 5.8 at four years, but only the four-year estimate is statistically significant. The estimates for lower-ranked schools, low-activity editors, and top-quartile similarity pairs remain positive at three and four years but are not statistically significant. This loss of significance does not show that the effects disappear. For example, the implied four-year 95 percent confidence intervals are approximately -6.4 to 11.6 percentage points for lower-ranked schools and -7.2 to 10.4 percentage points for top-quartile similarity pairs. Taken together, the estimates suggest that information and access from editor visits may matter more in the settings we hypothesized would benefit most, but the evidence is not conclusive.

[Insert Table 9 About Here.]

6.5 Alternative Outcomes: Other Journals, Citations, and Tenure

A meeting with an editor could affect publication outcomes beyond the editor's own journal. Comments received during the visit may improve a paper's framing, analysis, or positioning, raising its chances of publication wherever it is submitted. We test this broader feedback channel using an indicator for whether the assistant professor publishes in any journal outside the top-eight set within two, three, or four years of the visit. We focus on journals outside the top eight to separate a general improvement in publication prospects from possible substitution among the leading journals, which compete for many of the same papers. For this analysis, we collapse the journal dimension and estimate the same instrumental-variables specification at the assistant professor–editor–year level. Appendix Table A8 reports positive estimates of 0.9, 0.7, and 0.6 percentage points at two, three, and four years. The two-year estimate is statistically significant at the 10 percent level and equals 3.7 percent of the outcome mean; the longer-horizon estimates are not statistically significant. The pattern is consistent with a modest short-run improvement in broader publication prospects, but it does not establish a persistent effect.

Visibility gains may appear in outcomes other than publication counts. An editor who learns about an assistant professor's work during a visit could draw greater attention to it within the journal's research community, for example through referee suggestions or informal recommendations. We therefore count citations to the assistant professor's work from papers published in the editor's journal during the one, two, three, or four years after the visit. This outcome measures attention from the editor's journal rather than publication in that journal. Appendix Table A9 reports estimates of -0.032, 0.031, -0.011, and -0.148 citations across the one- through four-year windows. None is statistically significant.

Finally, editor visits may affect tenure if editors later write external letters. The rotation instrument is not suitable for this outcome because tenure occurs only once and requires aggregating exposure across the tenure track. Past visits determine subsequent blackout status, so aggregate rotation eligibility is partly determined by prior meetings and provides a weak first stage for cumulative editor contact. We therefore estimate descriptive annual hazards of tenure, following Heckman and Moktan (2020), Sarsons (2017), and Gertsberg (2026b).

Appendix Table A10 reports descriptive annual-hazard estimates for 265 assistant professors from the 2010–2015 cohorts. We measure meetings at the starting school during tenure-track years zero through four. This is the cleanest cohort range because all five meeting years are observed for every scholar in the 2010–2019 seminar data. We follow career outcomes for up to ten years after the tenure-track start. The independent variable is the log of one plus the cumulative number of distinct editors met at the starting school through the previous year. The dependent variables are annual indicators for two tenure events: receiving tenure at any institution, and receiving tenure at the starting school or at a comparable or better-ranked school, defined as one ranked no more than five places lower than the starting school. The second, narrower outcome excludes tenure granted after a substantial move down the rankings, because such moves may follow a tenure denial at the original school (Sarsons, 2017).

Meeting more editors is not significantly associated with either tenure outcome. The coefficient is 0.000 for tenure at any institution and −0.004 for tenure at the original or a comparable school, relative to mean annual hazards of 7.4 and 5.0 percent, respectively.

Taken together, the alternative outcomes show at most a modest short-run increase in publication outside the top eight and no detectable citation response or relationship between cumulative editor contact and tenure.

6.6 Discussion: Brief vs. Sustained Connections

Overall, the results provide limited evidence of broader gains from a routine editor visit and no evidence of persistent gains. This does not establish that the effect is zero. For publication at the editor's journal, the confidence intervals rule out increases equal to 50 percent of the outcome mean at every horizon and 30 percent at three and four years.

The estimates therefore provide evidence against large effects, while smaller effects remain possible. Prior research instead finds publication gains from sustained coauthorship and editorial networks (Azoulay et al., 2010; Colussi, 2018; Carrell et al., 2024; Brogaard et al., 2014). Our data also show a positive association between multi-year co-location with an editor and publication. Routine seminar exposure does not appear to substitute for sustained contact. This comparison does not isolate duration because sustained ties differ in how they form and how intensive they are.

The estimates apply to visits made more likely by the editor–school rotation. Appendix Table A11 shows that the affected editor–school pairs are in active seminar circuits. The schools are better ranked and the editors visit more schools, while research alignment and prior productivity are typical. The instrument does not cover pairs that meet during blackout periods, such as coauthors or close colleagues. The results therefore speak to routine visits among seminar-hosting departments, not to repeated or deliberately cultivated relationships.

The small average effects have several possible explanations. A seminar may add little information beyond the manuscript, and any impression may fade before submission. Peer review may also limit how much one editor can affect the decision. The design cannot distinguish among these explanations.

The causal results cover publication and citation outcomes in economics and finance. They may not extend to fields with different review processes or to outcomes we do not observe, such as desk rejection, referee selection, and revision guidance.

7 Conclusion

Brief encounters with gatekeepers are widely assumed to open doors. We test this assumption in academic publishing, asking whether an editor's seminar visit makes assistant professors more likely to publish in that editor's journal. Estimates are close to zero at two, three, and four years. We rule out increases of 50 percent of the baseline rate at every horizon and 30 percent after three and four years, though smaller effects remain possible. Estimates are more positive for women, faculty at lower-ranked schools, editors visiting fewer schools, and editors and faculty working on similar topics. These patterns may reflect information or access benefits, but the evidence is inconclusive. Publications outside the top eight rise modestly at first but not thereafter. Citations show no clear response.

Prior work links multi-year co-location with editors to much higher publication rates (Brogaard et al., 2014). Our estimates for a single seminar visit are smaller, though modest gains remain possible. The duration and depth of contact may therefore matter more than brief exposure.

Departments devote scarce seminar slots and budgets to editor visits. Higher-ranked departments host more editors and also publish more in leading journals, so the raw pattern can look like privileged access paying off. Our rotation-based estimates instead provide evidence against large publication gains from a routine visit, while leaving smaller gains possible. The raw pattern alone therefore does not establish editorial favoritism. Future research could match specific contacts to confidential submission histories and manuscript versions. Substantive changes in the paper after a visit would point toward useful feedback, while more favorable editorial treatment without corresponding changes in the paper would make favoritism more plausible.

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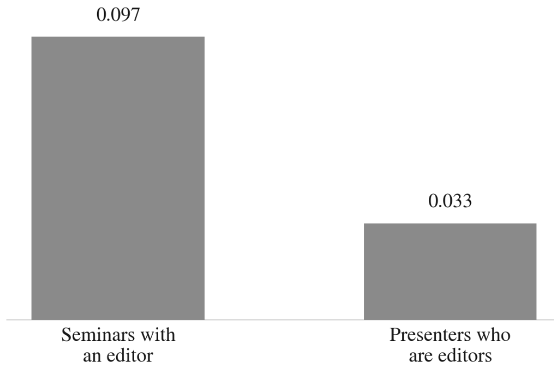
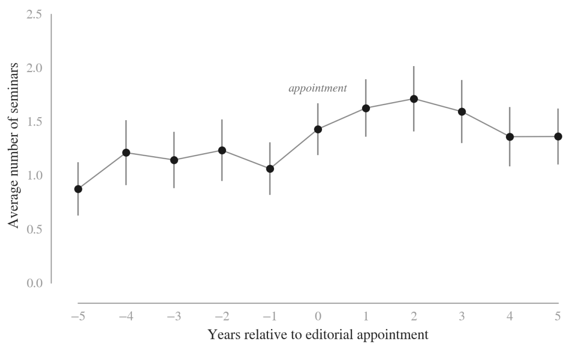
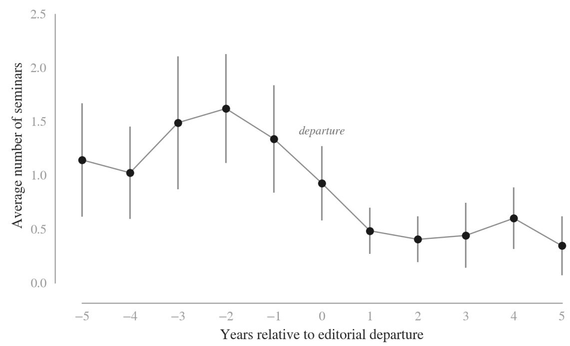


(a) Editors in seminar schedules



(b) Seminars around editorial appointment



(c) Seminars around editorial departure

Figure 1: Editor representation and seminar activity

This figure describes editors' representation in finance seminar schedules and their seminar activity around editorial service. Panel (a) reports editors' shares of seminar presentations and unique presenters at top-100 U.S. finance departments from 2010 through 2024. Panels (b) and (c) plot mean seminars per editor-year from five years before through five years after an editor's first appointment and final year of service, respectively. Year 0 marks the appointment or departure year. Vertical bars report 95% confidence intervals. The sample includes editors-in-chief and associate editors at finance journals; Panel (c) excludes editors whose service continues beyond 2019.

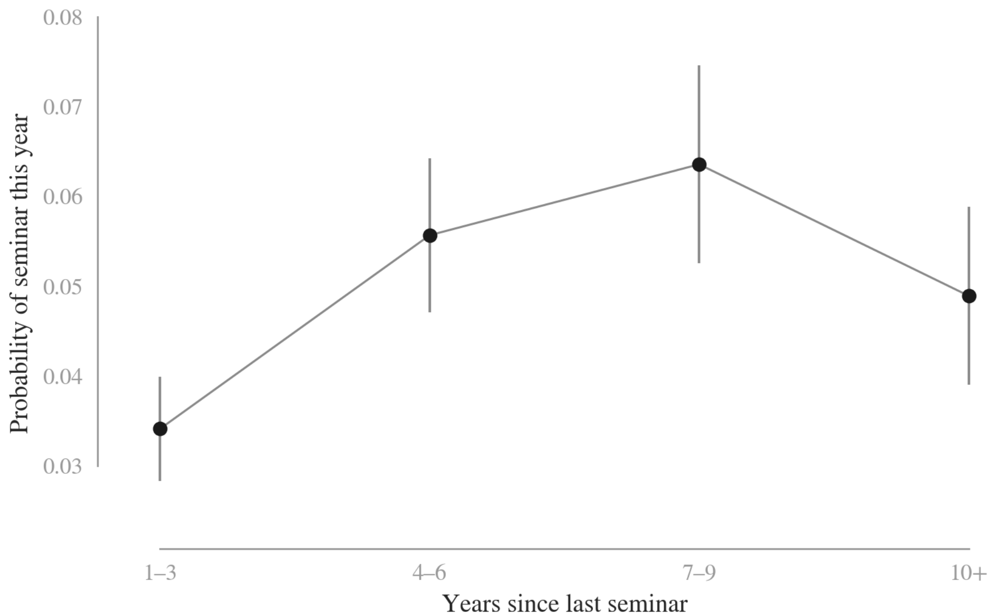


Figure 2: First Stage: Editor Visit Probability by Years Since Last Visit
This figure plots the mean probability that an editor visits a school in the current year against the years since that editor's previous seminar at the school. The horizontal axis groups elapsed time into 1–3, 4–6, 7–9, and 10 or more years. Vertical bars report 95% confidence intervals. The sample covers 2010 through 2019 and includes editors-in-chief and associate editors at top-three finance journals with at least one observed prior seminar at the school.

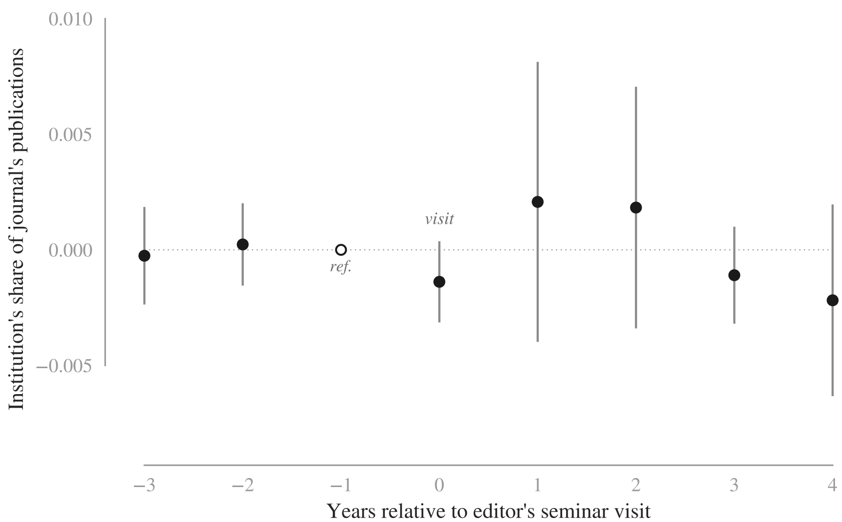


Figure 3: Institution-Level Event Study Around Editor Visits

This figure presents event-study estimates of changes in a school's share of a journal's publications around a visit by one of that journal's editors, with publications matched to visits after two years. The share equals publications by the school's assistant professors divided by publications by all sample assistant professors in the same journal-year. The omitted period is $k = -1$, and vertical bars report 95% confidence intervals based on standard errors clustered by school.

Table 1: Summary Statistics

	Defined Rotation Schedule						Undefined Seminar History		
	Seminar Visit			No Visit					
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Published in Journal (2 years)	2,618	0.099	0.298	44,548	0.099	0.298	233,982	0.070	0.255
Published in Journal (3 years)	2,618	0.203	0.402	44,548	0.198	0.398	233,982	0.139	0.346
Published in Journal (4 years)	2,618	0.292	0.455	44,548	0.284	0.451	233,982	0.203	0.402
Blackout Period (3 years)	2,618	0.246	0.431	44,548	0.381	0.486	233,982	0.000	0.000
Years Since Last Visit	2,618	6.016	3.449	44,548	5.527	3.818	0	.	.
School Rank	2,618	13.631	16.116	44,548	21.806	21.983	233,982	44.064	29.763
Years since tenure track start	2,618	1.926	1.344	44,548	1.902	1.361	233,982	1.862	1.357
Cosine similarity AP vs Editor	2,618	0.768	0.180	44,548	0.771	0.178	233,982	0.778	0.183
Log Top-8 Publications (cum.)	2,618	0.382	0.504	44,548	0.364	0.498	233,982	0.309	0.467
Log cohort size	2,618	1.278	0.512	44,548	1.206	0.473	233,982	1.163	0.460
Female AP	2,618	0.198	0.399	44,548	0.186	0.389	233,982	0.192	0.394
Total Sample									281,148

This table presents summary statistics for the main regression sample. "Seminar Visit" and "No Visit" divide observations for school–editor pairs with at least one observed visit; "No Seminar History" identifies pairs with none. "Published in Journal" equals one if the assistant professor publishes in the editor's journal in the stated window. "Blackout Period" equals one if the pair is within three years of its previous visit. "School Rank" is the finance-department rank, where a lower number indicates a higher rank. "Cosine Similarity" measures research overlap between the assistant professor and editor. "Log Top-8 Publications" measures cumulative publications in the top five economics and top three finance journals. "Log Cohort Size" is descriptive and is not a regression control.

Table 2: First Stage: Effect of Blackout Periods on the Probability of an Editor Seminar

	All editors	Defined clock	EIC only	Return visits
Blackout period (3 years)	-0.252***	-0.159***	-0.294***	-0.081***
	(0.009)	(0.014)	(0.023)	(0.011)
Cosine similarity AP vs Editor	0.002*	-0.001	0.004	-0.000
	(0.001)	(0.004)	(0.002)	(0.001)
Log Top 8 pubs (cum.)	-0.000	0.000	-0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.000)
Female AP	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.039***	0.116***	0.054***	0.014***
	(0.001)	(0.007)	(0.002)	(0.001)
Mean of DV	0.025	0.056	0.032	0.009
KP rk Wald F	726.2	127.6	170.0	53.1
Journal FE	Yes	Yes	Yes	Yes
School × Editor FE	Yes	Yes	Yes	Yes
School × Year FE	Yes	Yes	Yes	Yes
PhD School FE	Yes	Yes	Yes	Yes
Tenure Controls	Yes	Yes	Yes	Yes
Observations	281,148	47,116	48,333	281,148

This table presents first-stage estimates of how a three-year blackout period changes the probability that an editor visits a school in year t. *Blackout Period (3 years)* equals one if the school–editor pair is within three years of its previous seminar. Column 1 includes all editors, Column 2 requires at least one observed prior visit, Column 3 includes editors-in-chief only, and Column 4 examines return visits and therefore excludes first visits. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 3: Balance Test: Assistant Professor Characteristics by Blackout Status

	Eligible		Blackout			
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Difference	T-stat
Log Top 8 pubs (cum.)	0.358	0.495	0.377	0.505	0.019	1.18
Years since TT start	1.906	1.357	1.894	1.365	-0.012	-0.59
Female AP	0.188	0.391	0.189	0.391	0.001	0.07
Cosine similarity AP vs Editor	0.772	0.177	0.771	0.179	-0.001	-0.19
Observations	30,426		18,117			

This table compares assistant professor characteristics between blackout and eligible years for school–editor pairs with at least one observed visit. The difference equals the blackout-year mean minus the eligible-year mean. The t-statistic comes from a regression of each characteristic on the blackout indicator with standard errors clustered by school.

Table 4: Second Stage: Effect of Editor Meeting on Publication

	2yr	3yr	4yr
Editor visit this year	0.017*	0.011	-0.002
	(0.010)	(0.016)	(0.021)
Cosine similarity AP vs Editor	0.109***	0.188***	0.278***
	(0.017)	(0.029)	(0.038)
Log Top 8 pubs (cum.)	0.041***	0.063***	0.064***
	(0.009)	(0.015)	(0.016)
Female AP	-0.017**	-0.030**	-0.037*
	(0.008)	(0.014)	(0.020)
Mean of DV	0.075	0.149	0.216
KP rk Wald F	726.3	726.3	726.3
Journal FE	Yes	Yes	Yes
School $\times$ Editor FE	Yes	Yes	Yes
School $\times$ Year FE	Yes	Yes	Yes
PhD School FE	Yes	Yes	Yes
Tenure Controls	Yes	Yes	Yes
Observations	281,148	281,148	281,148

This table presents instrumental-variables estimates of whether an editor visit changes an assistant professor's probability of publishing in the visiting editor's journal. The dependent variables equal one if the assistant professor publishes in that journal in year $t+2$, from $t+2$ through $t+3$, or from $t+2$ through $t+4$. *Editor Visit This Year* equals one if the editor visits the assistant professor's school in year t and is instrumented with *Blackout Period (3 years)*, which equals one when the school–editor pair is within three years of its previous visit. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 5: Alternative Design: Institution × Journal × Year Panel

	OLS, no FE	OLS, full FE	First stage	IV
Panel A: Institution's share of the journal's publications				
Editorial match $(t-2)$	0.004	-0.001		0.019
	(0.003)	(0.004)		(0.039)
Rotation exposure $[t-5, t-3]$			-0.826***	
			(0.192)	
Mean of DV	0.004	0.004		0.004
KP rk Wald F				18.5
Observations	11,427	11,427	11,427	11,427
Panel B: Journal's share of the institution's own publications				
Editorial match $(t-2)$	0.012**	0.004		0.026
	(0.005)	(0.005)		(0.074)
Rotation exposure $[t-5, t-3]$			-0.827***	
			(0.192)	
Mean of DV	0.008	0.008		0.008
KP rk Wald F				18.5
Observations	11,427	11,427	11,427	11,427

This table presents estimates of whether a journal editor's visit changes the match between the editor's journal and the visited school. Panel A measures the school's share of the journal's publications; Panel B measures the journal's share of the school's top-eight publications. *Editorial Match* equals one if an editor active at journal j in year $t-2$ visited school i in that year. The instrument is the share of journal j's editors who visited school i from $t-5$ through $t-3$. The sample covers eight top journals, 91 schools, and 2000 through 2024, and excludes assistant professor–editor pairs from the same PhD programme. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 6: Robustness: Alternative Blackout Window Definitions

	Pub within 2 years			Pub within 3 years			Pub within 4 years		
	2yr inst.	4yr inst.	5yr inst.	2yr inst.	4yr inst.	5yr inst.	2yr inst.	4yr inst.	5yr inst.
Editor visit this year	0.017	0.008	-0.000	0.010	-0.008	-0.007	0.010	-0.008	0.002
	(0.014)	(0.012)	(0.014)	(0.018)	(0.016)	(0.017)	(0.020)	(0.020)	(0.021)
Mean of DV	0.075	0.075	0.075	0.149	0.149	0.149	0.216	0.216	0.216
KP rk Wald F	566.4	616.4	445.7	566.4	616.4	445.7	566.4	616.4	445.7
Observations	281,148	281,148	281,148	281,148	281,148	281,148	281,148	281,148	281,148

This table presents the main instrumental-variables estimates after defining blackout periods as two, four, or five years since the previous seminar. The three-year blackout period in Table 4 is the benchmark. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 7: OLS: Effect of Editor Visits on Publication

	2yr			3yr			4yr		
	No FE	S×E	Full FE	No FE	S×E	Full FE	No FE	S×E	Full FE
Editor visit this year	0.013**	-0.003	-0.004	0.033***	-0.001	-0.002	0.049***	0.003	0.001
	(0.005)	(0.004)	(0.003)	(0.009)	(0.004)	(0.004)	(0.011)	(0.005)	(0.005)
Cosine similarity AP vs Editor	0.076***	0.109***	0.109***	0.140***	0.191***	0.188***	0.209***	0.284***	0.278***
	(0.011)	(0.015)	(0.017)	(0.019)	(0.026)	(0.029)	(0.028)	(0.035)	(0.038)
Log Top 8 pubs (cum.)	0.052***	0.034***	0.041***	0.087***	0.054***	0.063***	0.099***	0.056***	0.064***
	(0.007)	(0.008)	(0.009)	(0.012)	(0.013)	(0.015)	(0.014)	(0.014)	(0.016)
Female AP	-0.018***	-0.022***	-0.017**	-0.030***	-0.038***	-0.030**	-0.031*	-0.042**	-0.037*
	(0.006)	(0.007)	(0.008)	(0.011)	(0.013)	(0.014)	(0.016)	(0.018)	(0.020)
Mean of DV	0.075	0.075	0.075	0.149	0.149	0.149	0.216	0.216	0.216
Journal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PhD School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenure Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × Editor FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School × Year FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	281,148	281,148	281,148	281,148	281,148	281,148	281,148	281,148	281,148

This table presents OLS estimates of whether an editor visit predicts an assistant professor's subsequent publication in the editor's journal. "No FE" includes journal, year, PhD-school, and tenure-year fixed effects. "S×E" adds school-by-editor fixed effects. "Full FE" replaces year fixed effects with school-by-year fixed effects and matches the fixed effects in Table 4. Standard errors are clustered by school and reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 8: Statistical Power and Confidence-Interval Bounds

	2yr	3yr	4yr
Panel A: IV			
Estimate	0.017	0.011	-0.002
Standard error	(0.010)	(0.016)	(0.021)
95% CI	[-0.003, 0.037]	[-0.021, 0.043]	[-0.043, 0.039]
Mean of DV	0.075	0.149	0.216
MDE	0.028	0.046	0.059
MDE, % of baseline	37.6%	30.7%	27.0%
CI upper bound, % of baseline	49.1%	29.0%	18.0%
Rules out a 30% increase	No	Yes	Yes
Rules out a 50% increase	Yes	Yes	Yes
Panel B: OLS			
Estimate	-0.004	-0.002	0.001
Standard error	(0.003)	(0.004)	(0.005)
95% CI	[-0.010, 0.002]	[-0.009, 0.005]	[-0.009, 0.010]
Mean of DV	0.075	0.149	0.216
MDE	0.008	0.010	0.013
MDE, % of baseline	10.9%	6.7%	6.2%
CI upper bound, % of baseline	2.1%	3.2%	4.6%
Rules out a 30% increase	Yes	Yes	Yes
Rules out a 50% increase	Yes	Yes	Yes

This table compares the instrumental-variables estimates in Panel A with the OLS estimates in Panel B. For each publication window, the table reports the estimate, standard error, 95% confidence interval, outcome mean, and minimum detectable effect at 80% power. The minimum detectable effect equals 2.8 times the standard error. The minimum detectable effects and upper confidence bounds are scaled by the outcome mean. The final rows indicate whether the upper confidence bound lies below 30% or 50% of the outcome mean.

Table 9: Second Stage: Effect of Editor Visits on Publication, by Subgroup

	2yr	3yr	4yr	N (2yr)	N (4yr)	KP rk Wald F (2yr)
Panel A: AP gender						
Male APs	0.012	-0.004	-0.026	227,196	227,196	708.7
	(0.012)	(0.020)	(0.024)			
Female APs	0.017	0.041	0.058**	52,121	52,121	346.6
	(0.016)	(0.027)	(0.027)			
Panel B: School rank						
Lower-ranked schools	0.067***	0.043	0.026	95,699	95,699	226.5
	(0.019)	(0.030)	(0.046)			
Top-ranked schools	0.011	0.009	-0.003	185,449	185,449	581.2
	(0.011)	(0.018)	(0.023)			
Panel C: Editor seminar activity						
Low-activity editors	0.027**	0.022	0.017	223,196	223,196	544.9
	(0.013)	(0.020)	(0.026)			
High-activity editors	0.002	-0.008	-0.031	57,952	57,952	362.2
	(0.017)	(0.022)	(0.024)			
Panel D: AP–editor research similarity						
Top-quartile similarity	0.048**	0.043	0.016	69,390	69,390	417.1
	(0.021)	(0.029)	(0.045)			
Below top-quartile similarity	0.013	0.013	0.002	210,240	210,240	590.0
	(0.011)	(0.018)	(0.020)			

This table presents the main instrumental-variables estimates separately by assistant professor gender, department rank, editor seminar activity, and assistant professor–editor research similarity. Department rank is split at the sample median, editor activity at the 75th percentile of schools visited, and research similarity at the top quartile of pair-level cosine similarity. The last columns report subgroup observations and the first-stage KP rk Wald F for the two-year outcome. Standard errors are clustered by school and reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Editor Visits and Publication Success

Internet Appendix

Table of Contents

This Internet Appendix contains one supplementary figure and eleven supplementary tables, which we organize as follows:

1. **Appendix Figure**
   (a) Figure A1 plots the regression-adjusted probability of an editor visit by years since the editor's previous visit to the school.
2. **Appendix Tables**
   (a) Table A1 describes the editorial roles used to classify editors and associate editors at the top finance journals.
   (b) Table A2 reports counts of editors-in-chief and associate editors by journal.
   (c) Table A3 reports placebo tests for the exclusion restriction and publication timing.
   (d) Table A4 estimates how visits two, three, or four years earlier affect current publications.
   (e) Table A5 reports the main instrumental-variables estimates for editors-in-chief.
   (f) Table A6 reports the main instrumental-variables estimates for school–editor pairs with defined rotation schedules.
   (g) Table A7 replicates Brogaard et al. (2014) using assistant professors' school-by-journal-by-year publication counts.
   (h) Table A8 estimates whether editor visits affect publication outside the top-eight journal set.
   (i) Table A9 estimates whether editor visits affect citations from the editor's journal to an assistant professor's work.
   (j) Table A10 reports descriptive estimates of tenure.
   (k) Table A11 compares the observable characteristics of compliers with unweighted sample means.

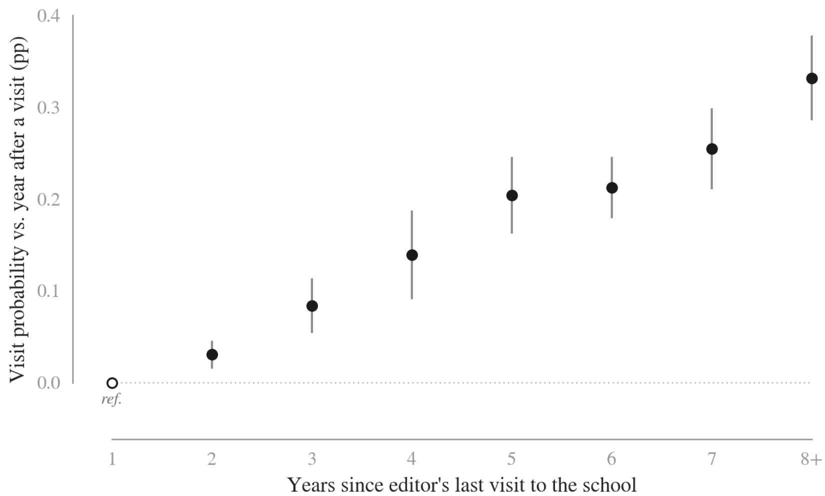


Figure A1: Regression-Adjusted Visit Probability by Years Since Last Visit
This figure plots regression-adjusted differences in current visit probability against the years since an editor's previous visit to the school. The omitted period is one year after the previous visit; the final bin combines eight or more years. The regression absorbs school-by-editor and year fixed effects. Vertical bars report 95% confidence intervals based on standard errors clustered by school. The sample includes school–editor–year observations with at least one observed prior visit.

Table A1: Harmonized Editorial Role Classification for Top Finance Journals

Journal	**Editors**	**Associate Editors**
Journal of Finance	Editor, Co-Editors	Associate Editors
Review of Financial Studies	Executive Editors, Editors	Associate Editors
Journal of Financial Economics	Managing Editor, Editor-in-Chief, Co-Editors	Associate Editors

This table presents the editorial roles used to classify journal personnel. Editors hold primary or delegated authority over publication decisions; associate editors advise on decisions and manage referee reports without final authority. At the *Journal of Financial Economics*, co-editors exercised delegated decision authority under the Managing Editor model from 2000 through 2021; the journal adopted a centralised Editor-in-Chief structure thereafter.

Table A2: Editors-in-Chief and Associate Editors by Journal

Journal	Total	EIC	AE-only
JF	97	9	88
JFE	36	5	31
RFS	104	23	81
AER	7	0	7
Econometrica	3	0	3
JPE	4	0	4
QJE	5	2	3
ReStud	8	6	2

This table presents unique editor-by-journal counts in the estimation samples. Editors-in-chief follow the classification in Table A1. An editor can appear in more than one journal or role during the sample period.

Table A3: Placebo Tests: Exclusion and Timing

	Other pairs' pub within 3yr		Timing
	without S×Y FE	with S×Y FE	Pub within 1yr
Blackout period (3 years)	0.012***	0.000	
	(0.004)	(0.000)	
Editor visit this year			0.019**
			(0.009)
Mean of DV	0.153	0.153	0.064
KP rk Wald F			726.3
School × Year FE	No	Yes	Yes
Observations	263,671	263,671	281,148

This table presents placebo tests for the exclusion restriction and publication timing. Columns 1–2 test whether a pair's blackout status predicts three-year publication outcomes for other assistant professor–editor pairs in the same school-year, without and with school-by-year fixed effects. These columns omit school-years with no leave-out comparison. Column 3 estimates the effect of a visit on publication in year $t+1$. Standard errors are clustered by school and reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A4: Dynamics: Lagged Visits and Current Publications

	Visit t-2	Visit t-3	Visit t-4	Any [t-4,t-2]
Visit at t-2	-0.011			
	(0.017)			
Visit at t-3		-0.072		
		(0.053)		
Visit at t-4			-0.002	
			(0.087)	
Any visit in [t-4,t-2]				-0.036
				(0.027)
Mean of DV	0.066	0.074	0.083	0.066
KP rk Wald F	414.5	157.6	44.8	145.5
Observations	91,152	38,818	9,157	91,697

This table presents instrumental-variables estimates of how visits two, three, or four years earlier affect publication in the editor's journal in year t. Each publication is therefore counted once. *Visit at* $t-k$ is instrumented with blackout status at $t-k$; the final column instruments any visit from $t-4$ through $t-2$ with average blackout status over those years. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A5: Second Stage: Effect of Editor Meeting on Publication, Editors-in-Chief Only

	2yr	3yr	4yr
Editor visit this year	-0.003	-0.010	-0.026
	(0.022)	(0.029)	(0.031)
Cosine similarity AP vs Editor	0.111^{***}	0.184^{***}	0.263^{***}
	(0.021)	(0.036)	(0.046)
Log Top 8 pubs (cum.)	0.034^{***}	0.057^{***}	0.061^{***}
	(0.009)	(0.016)	(0.018)
Female AP	-0.013	-0.027^{*}	-0.036^{*}
	(0.009)	(0.016)	(0.021)
Mean of DV	0.072	0.143	0.207
KP rk Wald F	170.2	170.2	170.2
Journal FE	Yes	Yes	Yes
School $\times$ Editor FE	Yes	Yes	Yes
School $\times$ Year FE	Yes	Yes	Yes
PhD School FE	Yes	Yes	Yes
Tenure Controls	Yes	Yes	Yes
Observations	48,333	48,333	48,333

This table presents the main instrumental-variables estimates for editors-in-chief. The dependent variables measure whether an assistant professor publishes in the editor-in-chief's journal in year $t+2$, from $t+2$ through $t+3$, or from $t+2$ through $t+4$. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A6: Second Stage: Effect of Editor Meeting on Publication, Defined Rotation Schedules Only

	2yr	3yr	4yr
Editor visit this year	0.024	0.020	-0.036
	(0.017)	(0.027)	(0.036)
Cosine similarity AP vs Editor	0.105***	0.179***	0.274***
	(0.023)	(0.042)	(0.054)
Log Top 8 pubs (cum.)	0.045***	0.074***	0.065***
	(0.015)	(0.025)	(0.024)
Female AP	-0.028***	-0.044***	-0.048**
	(0.010)	(0.017)	(0.023)
Mean of DV	0.099	0.198	0.285
KP rk Wald F	127.8	127.8	127.8
Journal FE	Yes	Yes	Yes
School × Editor FE	Yes	Yes	Yes
School × Year FE	Yes	Yes	Yes
PhD School FE	Yes	Yes	Yes
Tenure Controls	Yes	Yes	Yes
Observations	47,116	47,116	47,116

This table presents the main instrumental-variables estimates for school–editor pairs with at least one observed prior visit. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A7: Replication of Brogaard et al. (2014)

	(1)	(2)	(3)	(4)
During editorship (t-2)	0.362***	0.384***	0.307**	0.198*
	(0.102)	(0.124)	(0.145)	(0.115)
Observations	825	825	825	825
Mean of DV	0.867	0.867	0.867	0.867
Wild bootstrap p	0.002	0.007	0.053	0.097
Journal × Year FE	No	Yes	Yes	Yes
School × Journal FE	No	No	Yes	Yes
School × Year FE	No	No	No	Yes

This table presents a replication of Brogaard et al. (2014), Table 3, using assistant professors' school-by-journal-by-year publication counts. *Colleague Is Editor* equals one if a colleague held an editorship two years earlier. The sample includes schools that ever had an editor. Columns 1–4 add journal-by-year, school-by-journal, and school-by-year fixed effects sequentially. Standard errors are clustered by school. Wild-cluster bootstrap *p*-values address the small number of treated schools. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A8: Alternative Outcomes: Publishing in Other Journals

	2yr	3yr	4yr
Editor visit this year	0.009*	0.007	0.006
	(0.004)	(0.004)	(0.004)
Mean of DV	0.242	0.342	0.435
KP rk Wald F	659.4	659.4	659.4
Observations	238,095	238,095	238,095

This table presents instrumental-variables estimates of whether an editor visit changes an assistant professor's probability of publishing in a journal outside the top-eight set. The dependent variables equal one if the assistant professor publishes in such a journal from year $t+1$ through $t+2$, from $t+1$ through $t+3$, or from $t+1$ through $t+4$. Because these outcomes are not specific to an editor's journal, the analysis uses one observation per assistant professor–editor–year; Table A9 instead remains at the journal-specific level. *Editor Visit This Year* equals one if the editor visits the assistant professor's school in year t and is instrumented with *Blackout Period (3 years)*, which equals one when the school–editor pair is within three years of its previous visit. Standard errors are clustered by school and reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A9: Alternative Outcomes: Citations from the Editor's Journal

	1yr window	2yr window	3yr window	4yr window
Editor visit this year	-0.032	0.031	-0.011	-0.148
	(0.046)	(0.082)	(0.107)	(0.138)
Mean of DV	0.550	1.299	2.201	3.247
KP rk Wald F	726.3	726.3	726.3	726.3
Observations	281,148	281,148	281,148	281,148

This table presents instrumental-variables estimates of whether an editor visit changes citations from the editor's journal to the assistant professor's work. In the k-year column, the dependent variable counts OpenAlex citations from papers published in the editor's journal from $t+1$ through $t+k$ to any work by the assistant professor. Pair-years with no citations equal zero, and citation counts are winsorised at the 99th percentile. *Editor Visit This Year* is instrumented with *Blackout Period (3 years)*. Standard errors are clustered by school and reported in parentheses. KP rk Wald F is the Kleibergen–Paap F. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A10: Editor Meetings and Tenure

	Tenure (any)	Tenure orig./up/lat
Log editors met through year t-1 (+1)	0.000	-0.004
	(0.013)	(0.010)
Log top-8 pubs (cum., year t)	0.074***	0.070***
	(0.015)	(0.014)
Female AP	0.012	0.006
	(0.015)	(0.011)
Mean hazard	0.074	0.050
Tenure events	156	106
Scholars	265	265
Person-years	2,109	2,109

This table presents discrete-time hazard estimates of whether meeting more editors predicts tenure. The sample contains 265 assistant professors from the 2010–2015 cohorts, for which the full meeting window is observed in the 2010–2019 seminar data. Editor meetings are measured at the starting school during tenure-track years zero through four. Each assistant professor remains at risk until tenure at any institution, departure from the tenure track, tenure-track year 10, or the end of the career data in 2026. *Tenure (any)* records tenure at any school. *Tenure orig./up/lat* excludes moves to schools ranked more than five positions below the starting school. *Log Editors Met* counts distinct editors met at the starting school through year $t-1$ within the year-zero-to-four meeting window; *Log Top-8 Publications* counts cumulative top-eight publications through year t. All specifications include starting-school, tenure-track-cohort, PhD-school-rank, research-field, and tenure-track-year fixed effects. Standard errors are clustered by starting school and reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table A11: Complier Characteristics

	Defined-clock sample	Compliers
Cosine similarity AP vs Editor	0.771	0.745
Log Top 8 pubs (cum.)	0.365	0.382
Female AP	0.187	0.216
Years since TT start	1.904	2.077
School rank (finance)	21.309	12.403
Editor: no. schools visited	0.872	3.569
Observations	47,116	

This table compares observable characteristics of compliers with unweighted means for school–editor pairs that have an observed rotation clock. The sample is the estimation sample of Table 2, Column 2. Compliers are pairs induced to meet by rotation eligibility, and their means use the kappa weights in Abadie (2003). A logit model estimates eligibility using the listed characteristics, tenure-year indicators, and year indicators. The complier estimates exclude always-takers who meet during blackout periods, including co-authors and close colleagues.