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Are elite universities losing their competitive edge? ☆

E. Han Kim [a], Adair Morse [b], Luigi Zingales [c,*]

[a] University of Michigan, USA
[b] University of Chicago, USA
[c] University of Chicago, NBER & CEPR, USA

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ABSTRACT

We study the location-specific component of research productivity for economics and finance faculty over the last three decades. We find that there was a positive effect of being affiliated with a top 25 university in the 1970s; this effect weakened in the 1980s and disappeared in the 1990s. The decline in elite university fixed effect is due to the reduced importance of physical access to productive research colleagues, which in turn seems due to innovations in communication technology. One implication is that knowledge-based organizations should find it more difficult to capture rents vis-à-vis workers. We find that faculty salaries increased the most where the estimated spillover dropped the most. Despite the loss in local spillovers, elite universities still enjoy an edge in *average* productivity because of agglomeration of top researchers in prestigious institutions with high long-term research reputations.

1. Introduction

Do higher performing firms contribute to the productivity of individual employees or do they simply attract more productive individuals? If more productive firms give rise to more productive individuals, how are firms able to sustain this competitive edge over time? Does the edge arise from positive spillovers from more productive coworkers? How does corporate culture affect worker productivity?

Although these are important issues in the theory of the firm, they have not been adequately studied empirically because of the difficulty in measuring individual productivity. For most firms, the observable product is the result of a conglomeration of inputs from many individuals. Such conglomeration makes the task of

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* Corresponding author.
E-mail addresses: ehkim@umich.edu (E.H. Kim), adair.morse@chicagobooth.edu (A. Morse), luigi.zingales@chicagobooth.edu (L. Zingales).

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disentangling individual productivity virtually impossible. One environment, however, in which firm observable output can be assigned to individual members is that of university research, where individual output can be measured as the number of coauthor-adjusted pages published in academic journals.

In this paper we attempt to address these theory-of-the firm issues by examining research productivity in the top North American university economics and finance departments over the last three decades. To identify the university fixed effect as separate from the individual fixed effect, we trace faculty moves across universities by documenting the location of all faculty who have ever been affiliated with the top 25 schools over the last three decades.

The results have implications not only for the higher education industry, but also for other knowledge-based industries in which individual productivity is recognizable and individual reputation is important. Examples of such industries are widespread, including the financial, professional, scientific, and technical services industries. Key players in these industries have a fundamental characteristic in common with academic researchers: Achievement and success in knowledge-based production is often gauged against a professional standing outside the firm. Such a characteristic is readily apparent for principal scientists in company labs, journalists, investment bankers, fund managers, consulting or law firm partners, and even professional athletes. By examining university fixed effects on faculty research productivity, we hope to provide insights into issues such as how much the productivity of, for example, deal makers and traders in the investment banking industry is affected by their location choices, and how the location effect has changed over time.

We find that, in the 1970s, residence in an elite university had a sizable impact on individual productivity. During that time, a random economics faculty member moving from a non-top five university to a top five university would see her productivity increase by 1.68 *American Economic Review* (*AER*) impact-equivalent pages (our measure of quality-adjusted productivity) per year from an average of 2.79 pages. This is a 60% increase. In the 1990s, this effect all but disappeared. And the disappearance is not just a top five phenomenon. Of the top 25 economics departments studied, 17 (five) had a significantly positive (negative) impact on productivity in the 1970s. By the 1990s only two (nine) had a significantly positive (negative) effect. In finance, 16 (three) had a positive (negative) impact in the 1970s, and four (seven) for the 1990s. One might argue that classification of 25 universities as being elite is too broad. As a robustness check, we run all of our estimations based on only top five, top ten, top 15, and top 20 schools defined as elite. The conclusions do not change.

These results do not seem to stem from endogenous selection inherent in location decisions. We carefully consider four selection stories: quasi-retirement, non-promotion, complementarities, and tournaments. The patterns of post-move changes in recent productivity do not support any of these selection stories. Nevertheless, we formally address possible selection bias in faculty moves by estimating a two-stage selection model. We use a logit model to estimate the probability of moving as a function of age and a conditional logit model to estimate the probability of being at each location (given a move) as a function of the desirability of each location for individual faculty. Desirability is captured by the distance to the individual's origin (defined as the location of the undergraduate alma mater) and the relative productivity difference to incumbent faculty. Using the predicted unconditional probability of being at a location as an instrument for the university indicators, the results remain materially the same.

We then attempt to explain the cross sectional differences in university fixed effects by relating them to the quality of colleagues in each department and the differences in organizational culture. The quality of colleagues can generate positive spillovers through the exchange of expertise and feedback among colleagues (Laband and Tollison, 2000), including that from star faculty (Goyal, Van Der Leij, and Moraga, 2006; Azoulay and Zivin, 2006). A strong positive team effect on productivity was evident in the 1970s, where team is measured as the (lagged) average productivity of one's departmental colleagues. The positive team spillover effect remained positive in the 1980s and disappeared in the 1990s. In addition, the presence of editors of a major journal had a positive effect on the faculty productivity in the 1970s, which turned negative by the 1990s.

Organizational culture could likewise be important, but in this realm the influence from colleagues might not always be positive. Our primary measures of culture are the percentage of faculty in a department who have not published in top journals in the recent past and the quality of the Ph.D. program. Non-publishing faculty could set an example for others, helping re-direct journal-targeted research to other activities, which might be important for the department but are not gauged as research production in our definition. The percentage of non-productive colleagues has a strong negative effect on the university fixed effect. The quality of the Ph.D. program does not seem to matter. Although important in explaining the university fixed effects, organizational culture does not explain the decline of the university fixed effects over the last three decades.

We conjecture that the loss of elite university effects is due to advances in communication technology. While collaboration across universities was common even in the 1970s, local interaction was very important. Communication at a distance was costly from a monetary and a technical point of view. We argue that the Internet and the concomitant decline in communication costs have changed the localized nature of research interaction, giving faculty in remote places access to the latest developments in their research area and tools for communicating with distant colleagues for collaboration and feedback. Throughout the period, the costs of long-distance telephone calls and airfares declined, easing the burden of voice and person-to-person communication. Early innovations for exchanging written work included faxes and overnight mail deliveries. The arrival of the Internet in the

1990s, however, initiated a new era of communication and access to others' research. This conclusion is consistent with the Agrawal and Goldfarb (2008) finding that the adoption of the Bitnet (the predecessor to the Internet) significantly increased engineering research collaboration among US universities.

Alternative explanations could exist for the disappearance of the university fixed effects. A simple explanation is that the quality of the faculty at other universities is catching up to the top research universities. Our data tell a different story however. The difference in average individual faculty productivity between the top 25 universities and the others has increased, not decreased, in the last three decades. Elite universities seem to attract and retain the most productive researchers, even though these universities do not make their faculty more productive. We find that top researchers tend to agglomerate in institutions with prestigious undergraduate programs and in departments with high past research reputations. Such agglomeration could be due to the utility and the prestige of co-location with other creative minds. This is analogous to the explanation proffered by Glaeser (2000) and Glaeser and Berry (2005) for why highly educated cities tend to become even more highly educated.

Another possible explanation is that a sudden shift in the production frontier created a first mover advantage in the 1970s that had slowly eroded in the subsequent two decades. While this explanation is plausible for finance, which effectively took off as a separate field in the 1960s, it cannot be true for economics, which was a well-established discipline four decades ago.

A final possible explanation is related to Ellison (2006). He finds a trend among Harvard University faculty toward forgoing the journal publication process that for them carries fewer advantages in terms of visibility because of the availability of Internet distribution. If the same trend exists in other top schools, those universities will show a reduction in our measure of productivity. However, such an effect should apply only to well-known full professors, because non-full professors and lesser known people have to publish in the journals to establish a reputation. Thus, we examine university fixed effects for assistant and associate professors separately from those for full professors. We find declining university effects for both groups. If anything, contrary to the notion that the Ellison effect could add to the declining effect for the full professor group, the declining university effect seems to be sharper for the assistant and associate group.

Because the alternative explanations are inconsistent with the data, we probe deeper into the communication technology based explanation and test whether the evidence is consistent with its natural implications. The most direct implication is that the spillover of having better research colleagues declined over the sample period. This is what we indeed find. Coauthorship at a distance rises steadily during the period, perhaps due to the reduced barriers stemming from innovations in communications technology. Among all articles published in the top 41 journals written by scholars residing at a top 25 school, the percentage of coauthored papers with faculty in a nonelite school nearly doubled, from about 32% in the beginning of the 1970s to 61% by 2004, suggesting that it has become much easier for authors at non-elite universities to access scholars at elite universities.

These findings are consistent with Laband and Tollison (2000), Rosenblat and Mobius (2004), Goyal, Van Der Leij, and Moraga (2006), and Agrawal and Goldfarb (2008), who show that decreasing communication costs have increased distant collaboration in academia and opened vast research networks. Furthermore, Laband and Tollison (2000) show that formal collaboration (coauthoring) is not replacing the growing role of informal intellectual feedback on article preparation from a distance. This finding is consistent with our evidence that proximity is no longer required for spillovers in research production.

The disappearance of university fixed effect is consistent with results from Oyer (2006) for the 1990s as well. He cleverly uses the tightness in labor market conditions to identify the benefit that freshly minted economics Ph.D.s receive from being placed at the beginning of their career in a top department. He finds that a top placement has long-term benefits in terms of career, but he finds no benefit in terms of enhanced productivity, supporting the view that top departments have no productivity spillovers in the 1990s.

The de-localization of the spillover generated by more productive researchers has important implications in the higher education industry. First, it makes the position of leading universities less stable. De-localization of production spillovers renders faculty more mobile, making it easier for a new university to attract the most talented researchers with higher salaries. This is the second important effect. When spillover was localized, universities could more easily appropriate the rents (see Mailath and Postelwaite, 1990). Today, with universal access to knowledge, this might no longer be the case. We find evidence consistent with this prediction in the average salaries at different institutions. Between the 1970s and the 1990s, faculty salaries increased the most at universities where the estimated spillover dropped the most.

These results also have implications for other knowledge-based industries, such as the financial, professional, scientific, and technical services industries. Traditionally, physical access to the firm was important for knowledge-based production. If innovations in communication technology have made low-cost access at a distance possible for production purposes, then firms have lost a powerful instrument to regulate and control the accumulation and utilization of knowledge. Appropriating the return to investment in research and development will become more difficult, and firms' boundaries will become fuzzier. The implications extend far beyond what we show here. A firm's inability to contain spillovers could force a rethinking of the legal boundaries of the firm, changing current views of employment and competition.

The rest of this paper proceeds as follows. Section 2 presents the data. Section 3 reports estimation of the university fixed effects, followed by an examination of potential selection biases influencing the results in Section 4. Section 5 decomposes university fixed effects onto institution-wide characteristics, and Section 6 explores possible causes and consequences of the disappearance of university fixed effects. Section 7 concludes.

2. Data

To study the effect of location on research productivity, we collect data on individual research productivity for a large panel of faculty. Special care is exercised to ensure that we identify the location of faculty during the production of articles, not at the publication date. In addition, we cautiously choose measures of productivity that are comparable over three decades and use alternative measures for robustness checks.

2.1. Faculty sample selection

Because collecting career information for all academic fields is difficult, we restrict our attention to the fields of economics and finance, our areas of research interest. Because location-specific effects are likely to be strongest among top universities, our analysis is focused on top research institutions. We collect data on research productivity for all individuals who ever have been affiliated (tenure track or visiting) with the top 25 universities over years 1970–2001.

To choose the top 25 universities, we average university research rankings provided by 11 previous studies.[1] These studies employ a broad range of methodologies and journals to rank departments over 1970–2001 subperiods. Relying on these studies alleviates some of the subjectivity inherent in using a single ranking methodology. We use the Borda Count method (de Borda, 1781) to average the rankings from the eleven studies: A university ranked first in a study is given 20 points; the second ranked is allocated 19 points, and so on. We then average the rankings, individually for finance and economics, weighting each study's allocated points by the number of years covered by the study. The scores for finance and economics are averaged (with equal weighting) for each university. The average reported in Table 1 shows a natural break point in score magnitude at the 25th university.[2]

Faculty affiliation is based on where individuals self-report their location each year, not the affiliation reported in published articles. Identifying the physical location of individuals during the production process is crucial to estimate a university effect on productivity. Thus we undertake a painstaking three-step procedure to manually compile the appropriate faculty affiliation.

Table 1
Research rankings of universities.
The rankings are based on the Borda Count method (de Borda, 1781) to average university research rankings provided by 11 previous studies, which employ a broad range of methodologies and journals to rank departments over 1970–2001 subperiods.

Rank	University	Borda Count score
1	University of Chicago	17.74
2	University of Pennsylvania	17.19
3	Harvard University	16.94
4	New York University	13.05
5	Massachusetts Institute of Technology	12.98
6	Stanford University	12.89
7	Northwestern University	11.83
8	University of California at Los Angeles	11.70
9	University of Michigan	10.54
10	Columbia University	9.10
11	University of Rochester	7.74
12	University of California at Berkeley	7.59
13	Yale University	6.78
14	Princeton University	6.20
15	Ohio State University	5.60
16	Cornell University	5.36
17	University of Wisconsin	5.32
18	Duke University	4.63
19	University of British Columbia	2.39
20	Purdue University	2.28
21	University of Washington	2.13
22	Indiana University	1.91
23	University of Texas at Austin	1.81
24	Carnegie-Mellon University	1.79
25	University of Southern California	1.71
26	Boston University	1.42
27	University of Illinois	1.36
28	University of California at San Diego	1.33
29	University of Minnesota	1.18
30	University of Maryland	0.98
31	Johns Hopkins	0.63
32	London School of Business	0.63
33	Rutgers University	0.63
34	Boston College	0.51
35	University of Pittsburgh	0.50
36	London School of Economics	0.43
37	University of North Carolina	0.42
38	Louisiana State University	0.27
39	Virginia Polytechnic University	0.22
40	University of Iowa	0.21
41	University of Toronto	0.20
42	Hong Kong Polytechnique University	0.18
43	Brown University	0.17
44	Oxford University	0.13
45	California Institute of Technology	0.10
46	University of Virginia	0.08

First, we extract the curriculum vitaes (cvs) from websites for each member of the finance and economics faculty currently employed by the top 25 universities.[3] Second, we look at each individual's cv for every economics and finance faculty for all universities included in *Business Week*'s top one hundred business schools and the National Research Council's top 100 economics programs for 2002 (a total of 138 additional universities). If an individual in the additional universities ever held a position in any of our top 25 universities, that person is

[1] The 11 studies consist of seven studies on economics (Graves, Marchand, and Thompson, 1982; Davis and Papanek, 1984; National Research Council, 1983; Scott and Mitias, 1996; Dusansky and Vernon, 1998; and Coupe Revealed Performance, http://student.ulb.ac.be/~tcoupe/ranking.html, for 1994–1998 and 1998–2001) and four on finance (Klemkosky and Tuttle, 1977; Niemi, 1987; Borokhovich, Bricker, Brunarski, and Simpkins, 1995; Chan, Chen, and Steiner, 2002).

[2] The average of finance and economics rankings could result in the inclusion of schools that would not be ranked highly in one of the two departments. For example, Princeton University has no finance department but still made the top 25 because of its high ranking in economics.

[3] This work was conducted during the period May 2003 to March 2004.

included in the data set. Third, we capture faculty who moved to other professions, retired, or passed away during our sample period by contacting the archives of each of our 25 universities to request a faculty roster for the economics and finance departments over 5-year intervals starting in 1973; namely, 1973, 1978, 1983, and 1988.[4] These archives and obituary records, supplemented with the *Hasselback-Prentice Hall Guide to the Finance Faculty* for 1993, provide the majority of missing names for the earlier periods.

From faculty cv's, we extract individuals' university affiliations and position ranks over time, as well as doctoral and undergraduate degree institutions and graduation years. When the websites publish biographical sketches that do not contain the full historical detail, we fill in unavailable data following a series of procedures. We contact via e-mail a subset of individuals to request their full vitae. We also use relevant statistics derived from the set of full cv's of other faculty members to fill in estimates of the missing variables.[5] The result of our efforts is a data set of 3,262 faculty members whose careers touch than 800 universities.

2.2. Publication sample selection

Measuring research productivity requires selecting a reasonable number of economics and finance journals. To avoid making choices based on our priors, we use all of the 36 economics journals and the top five of the finance journals used by the ranking studies mentioned above.[6] We exclude all but the top five finance journals in an effort to equalize the minimum quality standard across finance and other economic subfields. This is important because the calculations of productivity are pooled across all economics subfields, including finance. Inclusion of top five finance journals results in a roughly equivalent average (impact-weighted) productivity between finance and economics faculty. Table 2 lists the 41 journals.

We obtain article information for the period 1970–2002 from two sources. Our primary source of data is EBSCO Information Services, a publication data vendor. The EBSCO download consists of 73,039 articles, representing 48,917 author names and 111,150 unique article-author observations.[7] We hand-match the EBSCO author names to our list of 3,262 faculty to capture inconsistencies in naming conventions. Of the 111,150 article-author observations, 25,010 of them correspond to our list of faculty ever affiliated with the top 25 universities. The second source of publication data is the ISI Web of Science/Social Science Citation Index (SSCI) from which we collect citation counts for each article.[8]

To approximate the timing when the actual work was performed, we lag the publication date of articles to account for time spent in research and write-up, the peer review process, and journal publication backlog. The lag adjustment is complicated by journals having different lags in review and publication process. Fortunately, Ellison (2002) calculates the decade average submission-to-acceptance time in months for 20 journals included in our sample. For the other journals, we use the average lag time of the economics or finance journals included in Ellison.[9] Ellison's lag, however, does not include the lag from acceptance to publication and from work to submission.

The lag between acceptance and publication varies depending on the journal backlog. To estimate this lag, we look at two journals [the *Review of Economic Studies* (*RES*) and the *Journal of Financial Economics* (*JFE*)] that report the acceptance date of each paper. For each year in 1970–2002, we randomly select 15 articles from the *RES* and *JFE* and calculate the mean lag time from submission to publication. When we plot these sampled lag times (not reported), Ellison's lag times are smaller because the sampled lag time includes Ellison's submission-to-acceptance estimate plus the publication backlog time. To account for the time spent between acceptance and publication (seven months on average) and the time between the middle-of-writing and submission, we add one year to Ellison's lag.

2.3. Productivity measures

After mapping publications to the year and place of production, we estimate a measure of impact productivity in terms of *AER* equivalent pages custom-made to our needs. The four most commonly used measures of

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[4] A few universities were unable to supply us with the archive reports. For these schools, we searched the university websites for records of department retirements and obituaries from the 1970s and 1980s.

[5] Missing Ph.D. years are replaced with the first year that the person appears as an assistant professor in our database. If the first assistant professor year is 1970, which is the start date for our database, or if the person is never an assistant professor in our database, the Ph.D. year is replaced with the undergraduate graduation year plus six years, the median time between degrees in our sample of full cv's. Likewise, missing undergraduate graduation years are inferred from the Ph.D. year. If we are unsure of promotion years, we infer promotion dates using the university department's decade average time spent in each promotion rank, which is again computed from our sample of full cv's for that university. Finally, if the individual retired, passed away, or otherwise disappeared such that we are unable to locate a website or a cv at all (less than 10% of the cases), we infer institutional attachment via affiliations cited on journal articles.

[6] The list of 36 economics journals comes from the union of all journals covered by the ranking studies except those using the entire Social Science Citation Index or all of EconLit journals. The top five finance journals are based on Arnold, Butler, Crack, and Altintig (2003), which includes *Journal of Business* as a finance journal.

[7] EBSCO's classification scheme allows us to discard comments, notes, book reviews, and other nonarticle publications. We discard articles with fewer than three pages and verify the page count and content for articles with three to four pages or page counts greater than 69.

[8] The Web of Science data have several limitations. Often all but the first author are excluded from the author lists when the number of coauthors is greater than two (Alexander and Mabry, 1994) and the authors' first names are abbreviated. Although matching the abbreviated names to publications with precision is difficult, we circumvent the problem by mapping the journal issue and page range to the EBSCO data.

[9] More precisely, for finance journals not included in Ellison, we use the average of the *Journal of Finance* and the *Journal of Financial Economics* (*JFE*), the two finance journals covered by Ellison, for the 1980s and 1990s. For the 1970s we only use the *Journal of Finance* lag because the *JFE* started in the mid-1970s and had an unrepresentatively short publication process time during the 1970s.

Table 2
Impact factors and decade impact rankings.
This table ranks 41 major economics and finance journals by *impact factor*. *Impact factor* is the number of citations to each journal appearing in the references of all articles in the 41 journals, divided by the number of pages published by the journal being cited and normalized to the *American Economic Review* (*AER*) for each decade. Author self-citations are excluded. The 1990s decade includes 2000 and 2001.

Journal (ordered by 1970 rank)	1970s		1980s		1990s	
	Impact factor	Rank	Impact factor	Rank	Impact factor	Rank
American Economic Review	1.00	1	1.00	2	1.00	1
Journal of Political Economy	0.93	2	0.75	4	0.72	5
Journal of Financial Economics	0.85	3	1.04	1	0.88	3
Review of Economics and Statistics	0.74	4	0.43	11	0.51	7
Econometrica	0.71	5	0.89	3	0.49	8
Review of Economic Studies	0.69	6	0.59	9	0.67	6
Rand Journal of Economics (formerly Bell Journal of Economics)	0.61	7	0.66	6	0.41	9
Journal of Finance	0.60	8	0.60	8	0.96	2
Journal of Monetary Economics	0.58	9	0.75	5	0.37	11
International Economic Review	0.49	10	0.27	22	0.33	16
Quarterly Journal of Economics	0.43	11	0.62	7	0.80	4
Journal of American Statistical Association	0.43	12	0.37	14	0.34	15
Journal of Economic Theory	0.43	13	0.37	15	0.17	29
Journal of Public Economics	0.42	14	0.28	20	0.27	21
Journal of Money, Credit, and Banking	0.40	15	0.39	13	0.32	17
National Tax Journal	0.40	16	0.16	33	0.28	20
Journal of Econometrics	0.35	17	0.29	18	0.24	25
Journal of International Economics	0.33	18	0.43	12	0.35	13
Economic Inquiry	0.32	19	0.27	23	0.15	33
Journal of Business	0.31	20	0.37	16	0.30	19
Industrial and Labor Relations Review	0.30	21	0.23	28	0.31	18
Journal of Human Resources	0.29	22	0.15	35	0.24	26
Journal of Urban Economics	0.28	23	0.17	31	0.13	36
Economica	0.27	24	0.22	30	0.17	31
Journal of Financial and Quantitative Analysis	0.26	25	0.28	21	0.35	14
Journal of Law and Economics	0.21	26	0.26	24	0.16	32
Southern Economic Journal	0.20	27	0.16	34	0.08	38
Economic Journal	0.18	28	0.23	29	0.20	27
Journal of Legal Studies	0.16	29	0.24	26	0.12	37
Journal of Economic Dynamics and Control	0.14	30	0.15	36	0.15	34
Oxford Economic Papers	0.13	31	0.12	40	0.15	35
Journal of Economic History	0.13	32	0.13	37	0.07	39
Journal of Regional Science	0.13	33	0.12	39	0.07	40
European Economic Review	0.12	34	0.17	32	0.26	24
Journal of Development Economics	0.10	35	0.13	38	0.19	28
Economic Development and Cultural Change	0.09	36	0.05	41	0.06	41
Journal of Business and Economic Statistics	—		0.51	10	0.39	10
Review of Financial Studies	—		0.32	17	0.36	12
Journal of Labor Economics	—		0.29	19	0.26	22
Journal of Law, Economics and Organization	—		0.24	27	0.26	23
Journal of International Money and Finance	—		0.26	25	0.17	30

academic productivity are counts of articles written, raw counts of publication pages, citations to published articles, and impact-weighted counts of pages where impact is gauged by the citations to the journal in which the publication occurs.[10] As we describe below, each measure has strengths and weaknesses.[11]

The *count of articles* published is the simplest productivity measure and is calculated by summing the number of articles each individual publishes in the 41 journals each year. The shortcoming of this measure is its inability to distinguish impact and length of articles, in that all of them count the same. The other, easier-to-address problem is that it is a discrete measure.

The second measure of productivity, *raw productivity*, is calculated by summing pages published by individual i in each journal j at time t ($pages_{ijt}$), divided by the number of coauthors in each article ($coauthor_{ijt}$), and adjusted to each journal's *AER* equivalent length:

$$Raw_{it} = \sum_{articles_{it}\text{in all } j} \left[\frac{pages_{ijt}}{coauthors_{ijt}} \cdot (AER\ equivalent\ adjustment)_{jt} \right]. \tag{1}$$

[10] For counts of articles written, see Heck, Cooley, and Hubbard (1986); raw counts of publication pages, see Klemkosky and Tuttle (1977), Graves, Marchand, and Thompson (1982), Niemi (1987), Scott and Mitias (1996); citations to published articles, see Davis and Papanek (1984); Blair, Cottle, and Wallace (1986); and for impact-weighted counts of pages, see Liebowitz and Palmer (1984), Alexander and Mabry (1994), Laband and Piette (1994), Borokhovich, Bricker, Brunarski, and Simkins (1995), and Conroy and Dusansky (1995).

[11] Other measures of individual productivity in economics and finance research include representation on editorial boards (Kaufman, 1984) and references in graduate-level texts (Liner, 2002).

The number of pages and coauthors for each article are from the EBSCO data set.[12] The *AER* equivalent adjustment normalizes each journal to the length of the *AER* to account for different typesetting and conventions in article lengths. We follow Graves, Marchand, and Thompson (1982) in normalizing both the average number of pages per article and words per page to the *AER*.[13] The normalization of *raw* to the *AER* equivalent allows productivity assigned to an individual to be interpreted as the number of *AER* style pages produced in a given year.

Raw page counts are simple to use and easy to understand, but they do not reflect the quality of articles. One way to assess quality is to use the citation method, which weights every article by its number of citations as recorded in the SSCI. Citations are perhaps the most direct measure of an article's influence. However, this method also has several flaws.

First, citation counts increase with the age of the article. For our sample, the mean citation count per faculty in the 1990s (7.8 cites) is less than one-third of those for the two previous decades (27.5 cites, 25.7 cites). To make the citation counts comparable over years, we inflate citations such that all articles have had equal time lag since publication. Citations do not evolve linearly over time. Instead, they tend to increase at an accelerating rate for a couple of years and then grow at a decreasing rate each year. We attempt to capture these nonlinearities using downloaded data from two-time snapshots: a summer 2005 download of citations used for this paper and a summer 2006 download used to compile Kim, Morse, and Zingales (2006b). Using the differences between these two citation counts for each article, we estimate a one year growth rate for citations of each article-age cohort. The objective is to bring all articles to a 30-year aging using the incremental growth from each cohort. In practice, however, the median citation growth is zero after 14 years. Thus, for each article under the age of 14 years, we multiply the observed citations in 2005 by each incremental year factor to bring the aging up to 14 years. When we make this age adjustment, the mean citation count per article for 1970s, 1980s, and 1990s are 39.9, 41.1, and 38.2, respectively, giving us confidence that our age inflation method is reasonable.

Second, simple citations as a measure of productivity are highly skewed. The citation standard deviation of individual faculty for each decade is about four times as large as the mean, whereas none of the standard deviations for other measures of productivity exceeds two times the magnitude of the mean. The skewness for simple citations is approximately 14; for the other measures, the skew ranges between 2 and 3. Even after adjusting for the age factor, citation counts still exhibit a large skew. At the faculty-year observation level, the mean citation count is 24.3, whereas the median number of cites is zero. (Because a large number of faculty have no papers for a given year, the faculty-year mean is almost half of the article mean citation counts.)

In addition, a few papers have thousands of citations (see Kim, Morse, and Zingales, 2006b, for a list of the most highly cited papers). This magnifies possible misidentification of faculty's affiliation at a particular time. If the time lag applied from publication to writing is incorrect for a groundbreaking article, a huge number of citations belonging to one institution would be inappropriately credited to the wrong institution. Groundbreaking articles are more susceptible to misidentification because they are more likely to face longer than average delays in the publication process. Given the disproportionate weights that these articles carry in terms of citations, an error would have an enormous impact on the estimates.

Even without a mistake, the influence factors of high-citation observations would imply that a few observations completely determine the ordinary least squares (OLS) estimates and that the variance of the estimates could tend toward infinity.[14] For these reasons, we upper winsorize citations at the 5% level, bringing the maximum adjusted citation down to 117.1 and the mean citation down to 15.9.

These adjustments do not alleviate other flaws inherent in citations as a productivity measure. SSCI counts citations from hundreds of journals, not just from journals at the forefront of the research in economics and finance. Thus, a citation from an obscure journal article has the same weight as one from an *AER* article. Citations also create a bias in terms of fields and types of articles. For example, of all economics and finance articles published in the 41 major journals since 1970, 11 of the 20 most-cited articles are econometric foundation papers, with White (1980) topping the list (Kim, Morse, and Zingales, 2006b).

The impact-weighted count of pages, *impact productivity*, is a compromise between *raw productivity* and citation counts, incorporating key features of each. We follow the noniterated method of Liebowitz and Palmer (1984) and Laband and Piette (1994), in which publication pages are credited to the individual faculty as in the *raw productivity* calculation and the credit is weighted by the impact factor of the journal for decade *d*. *Impact productivity* is defined as

$$\text{Impact}_{it} = \sum_{articles_{it}\ in\ all\ j} \left[\frac{pages_{ijt}}{coauthors_{ijt}} \cdot AER\ equivalent\ \text{adjustment}_{jt} \cdot \text{ImpactFactor}_{jd} \right]. \quad (2)$$

The impact factor for journal *j* in decade *d* is the number of citations to journal *j* ($cites_{sjd}$) appearing in the references of all articles in the source journals *s*:

[12] Page counts were missing in 3% of the article-author observations. We use the average pages for the appropriate journal and year for these observations. Normalizing by the number of coauthors is consistent with the findings of Sauer (1988) that the salary return to papers coauthored by *n* authors is approximately $1/n$ the return of a solo-authored paper.

[13] In making the page count adjustment, we exclude the *AER Proceedings* issue. To obtain the word-per-page adjustment, we count the number of words for a standard, nonequation page for each of the 41 journals for three decade observations: 1975, 1985, and 1995.

[14] We thank Glenn Ellison for pointing out this.

$s \in \{1,\ldots,41\}$, defined according to the formula

$$\text{ImpactFactor}_{jd} = \frac{\sum_{articles \text{ in } s\in 1,\ldots,41} \frac{cites_{sjd}}{PagesPublished_{jd}}}{\text{ImpactFactor}_{AER,d}}. \quad (3)$$

We divide cites by the number of pages published by the journal being cited ($PagesPublished_{jd}$), reflecting the fact that, if a journal publishes more pages, it has put more content out into the profession that could generate citations. We normalize impact factors to the *AER* for each decade for comparability. The data for creating impact factors come from manually counting citations to journal j from references in each article of the 41 source journals. We do not include author self-citations. In total, we collect reference counts for more than 6,000 articles. We do this count once for each decade, choosing the reference years 1979, 1989, and 1999 to capture the impact factor for articles written in the middle of the decade.

We expend the extra effort to manually create impact factors instead of simply relying on SSCI impact factors, which are based on citations from the universe of all journals. The SSCI impact factors count only articles from the reference list published in the prior two years and normalize by the number of articles published. In contrast, our custom-made impact factors are based on citations from the 41 leading journals, use all articles in the reference list, and normalize to the page length as well as number of articles. This explains some slight differences in the two rankings.[15]

Table 2 presents impact factors and the decade rank of the impact factors for 36 economics journals and five finance journals (with the *Journal of Business* classified as a finance journal) for the three decades. The ranking of journals is generally persistent, with a Kendall's tau-b rank correlation of approximately 0.70 across decades.[16]

Laband and Piette (1994) report impact factors for roughly the same time periods as our 1970s and 1980s calculations. Because we follow their character-based impact factor model, the method of adjusting for article counts and page and font conventions is the same. The methods differ, however, in their use of the larger set of SSCI journals, which give more weight to lower-tier journals. Less substantially, their calculations also differ in that they include comments and notes, and we do not. In spite of these differences, the correlation between our impact factor and theirs is 0.88 in the 1970s and 0.83 in the 1980s.

2.4. Summary statistics

Table 3 reports the summary statistics and correlations of our four measures of productivity. The mean *raw productivity* per faculty (6.3 pages) is approximately double that of *impact* (3.1 pages) over the entire period. The average number of articles is 0.62 per year. Both *raw* and *impact* measures increase from the 1970s to the 1980s, with a slight reversion in the 1990s. *Article counts* and *adjusted citations* show bigger drops in the 1990s. The decline in *adjusted citations* is due both to the drop in *article counts* in the 1990s and to the shorter period to accumulate citations. The medians are zero for all four measures over all three decades. All four measures of productivity are highly correlated with each other.

2.5. Productivity comparability over decades

Because we are interested in productivity over time, we need to be able to compare a unit of productivity output across decades. To check the comparability over time, we first consider the shifts in supply of and demand for manuscripts submitted for publication during the three decades. The ratio of supply (the number of manuscripts submitted) to demand (the number of articles published) in *AER* was 7.2 in the first five years of the 1970s, almost doubling to 12.7 in the last five years leading to 2002.[17] This suggests that a simple *AER* article-count measure would undervalue productivity in 2000 relative to that in the 1970s. Consistent with this conjecture, Panel A in Table 3 presents a steady decline in the average *article counts* over the three decades.

Countering this trend, however, is the Ellison (2002) finding that a 2000 article is twice as long as a 1970

[15] For example, the SSCI 1999 impact factor rankings list the *QJE* as the top economics journal whereas our impact ranking put the *AER* on the top. The difference is due to the fact that the *QJE* has more pages per article than the *AER* and has a larger ratio of recent to older citations.

[16] Liebowitz and Palmer (1984) also develop an iterated impact factor method, which weights the citations to each journal according to the impact factor of the source journal citing the reference. Calculation of the iterated impact factor results in large impact factors for finance journals vis-à-vis general economics or, in particular, other economics subfields. Specifically, the *Journal of Finance* and the *Journal of Financial Economics* rank first and second in the 1990s under this method. Although this is interesting and warrants further study to examine the causes, the large finance impact factor makes it difficult to compare research productivity across economic subfields. If we were to use the iterated impact factors, an economics faculty member publishing in a finance journal would be given more weight relative to her colleagues publishing in other subfields, making the comparison somewhat inequitable. The large finance impact factor is also partially due to insular citing within the finance field because, unlike other economics subfields, finance has a separate department. Finally, we are from finance departments; we thus would rather take the risk of underestimating the impact of finance than of get caught in a controversy over the relative impact of economics versus finance.

[17] These data are from the *Reports of the Editor* published each year. The increase in manuscript submissions could be attributed to three possible sources: more time per faculty member for preparing manuscripts, more faculty per university, and more universities participating in the publication market. Although difficult to quantify precisely, teaching loads have been reduced substantially over the three decades, allowing more time for research production. The growth in faculty per university can be estimated using the archive reports of faculty rosters. We find that the growth in the size of economics (finance) faculty for the top 25 schools is cumulatively 26% (69%) from 1973 to 2001. These figures capture only the intensive margin, ignoring the growth in the number of institutions with faculty submitting to top journals. Heck, Cooley, and Hubbard (1986) find that, whereas 201 institutions are represented in the *JF* during 1966–1975, 270 institutions are represented in the 1975–1985 *Journal of Finance* (*JF*) publications, corresponding to a 34% growth in the extensive margin for finance. Goyal, Van Der Leij, and Moraga (2006) show that the number of authors publishing (not trying to publish) in existing EconLit journals rose from 22,960 in the 1970s to 32,773 in the 1990s, a 43% increase.

Table 3
Summary statistics and correlations for individual productivity measures.

Panel A presents the mean, median, maximum and standard deviation for our four measures of productivity. Panel B and Panel C present the Spearman Rank Correlations and Pearson Correlations among the productivity measures, respectively. *Impact* and *raw* productivities are measured as the count of *American Economic Review* (*AER*) equivalent pages written by each faculty in 41 economics and finance journals. Adjustment to *AER* equivalents normalizes by font, typesetting, and average article length. Publication pages are divided by $1/n$ coauthors. *Impact* productivity multiplies each article by the decade impact factor of the journal published. *Article counts* is the simple sum of articles published by year. *Adjusted citations* are from Web of Science, adjusted for age and upper winsorized at 5%. The 1990s decade includes 2000 and 2001.

Panel A. Summary statistics

Productivity	Decade	Mean	Median	Maximum	Standard deviation
Raw	1970s	5.8	0	105.4	10.1
	1980s	6.7	0	112.7	11.5
	1990s	6.2	0	138.4	11.9
	Overall	6.3	0	138.4	11.5
Impact	1970s	3.2	0	74.6	6.2
	1980s	3.4	0	70.1	6.5
	1990s	3.0	0	103.9	6.3
	Overall	3.1	0	103.9	6.3
Article counts	1970s	0.75	0	9	1.10
	1980s	0.70	0	13	1.05
	1990s	0.53	0	8	0.89
	Overall	0.62	0	13	0.99
Adjusted citations	1970s	17.1	0	117.1	32.8
	1980s	18.0	0	117.1	33.4
	1990s	14.3	0	117.1	30.2
	Overall	15.9	0	117.1	31.7

Panel B. Spearman rank correlation

	Impact	*Raw*	*Article counts*	*Citations*
Impact	1			
Raw	0.981	1		
Article counts	0.969	0.974	1	
Adjusted citations	0.939	0.928	0.937	1

Panel C. Pearson correlation

Impact	1			
Raw	0.871	1		
Article counts	0.783	0.850	1	
Adjusted citations	0.711	0.669	0.732	1

article.[18] The number of pages per article has doubled for the *AER* over the same period, making the ratio of pages submitted to pages published roughly constant over time. Although this might suggest the use of *raw productivity* as the time-comparable measure, this calculation does not factor in the increase in the number of journals. Of the 41 journals in our sample, 17 did not exist in 1970. With the additional pages supplied by the new journals, *raw productivity* overestimates productivity in later years.

Finally, the *adjusted citations* measure suffers both from the drop in article counts in the 1990s and from the shorter time period to accumulate citations, biasing the 1990s downward.

A compromise measure is found in *impact productivity*, which removes the effect of the increase in number of journals. When a new journal arrives, it competes with existing journals for citations, often diminishing the impact factor of other second-tier journals. The *impact productivity* measure also diminishes article inflation because most article inflation occurs in second-tier journals, which are given lower impact weights. Consistent with this intuition, Table 3 presents that individual average *impact productivity* has remained fairly constant over three decades, which is not true for *adjusted citations* or *article counts*. For these reasons, we focus on *impact productivity* as our main measure, and we use *raw productivity*, *article counts*, and *adjusted citations* for robustness checks.[19]

[18] Ellison (2002) finds that the number of pages in the average economics article in top tier journals increases by 70–100% from the mid-1970s through the late 1990s. Consistent with this, the average article length in our data grows from 10.7 pages in 1970 to 21.9 in 2001, doubling over the three decades.

[19] Comparability over decades is less of concern for *adjusted citations* because they are already adjusted for differences in age.

Table 4
Average individual productivities for faculty by university and decade.

Individual *raw productivity* is measured as the count of *American Economic Review* (*AER*) equivalent pages written by each faculty in 41 economics and finance journals. Adjustment to *AER* equivalents requires normalizing by font, typesetting, and average article length to equilibrate words across journals. Publication pages are divided by $1/n$ coauthors. Impact productivity multiplies each article by the decade impact factor of the journal published. *Article counts* is the simple sum of articles published by year. *Adjusted citations* are from Web of Science, adjusted for age and upper winsorized at 5%. The 1990s decade includes 2000 and 2001. The *others* category reports the productivity of individuals who have ever been or will be at top 25 universities but who are at non-top 25 universities in the observation year. The method for determining the top 25 universities is discussed in Section 2 and Table 1. All averages are averages over faculty in the set of universities, not raw averages across universities.

	Impact productivity			Raw productivity			Article counts			Adjusted citations		
	1970s	1980s	1990s	1970s	1980s	1990s	1970s	1980s	1990s	1970s	1980s	1990s
Massachusetts Institute of Technology	5.94	6.58	6.64	10.04	11.80	12.00	1.26	1.13	0.97	36.4	35.3	30.5
Univ. of Chicago	5.65	6.07	5.84	9.03	10.28	10.00	0.99	0.94	0.80	31.9	33.1	28.3
Ohio State Univ.	5.17	3.54	3.66	9.01	7.41	7.81	1.14	0.83	0.75	24.7	23.4	20.4
Harvard Univ.	4.93	4.62	5.16	8.44	8.77	9.10	0.94	0.92	0.80	21.9	24.0	27.7
Carnegie Mellon Univ.	4.51	4.96	2.47	7.35	9.21	5.31	0.93	0.90	0.49	30.5	22.2	11.2
Univ. of Rochester	4.48	4.81	3.23	7.33	8.40	6.33	0.77	0.78	0.52	20.2	24.9	15.1
Univ. of California, Los Angeles	4.27	5.06	4.03	7.51	9.59	8.46	0.96	0.93	0.69	21.0	26.1	16.5
Yale Univ.	4.20	3.92	3.08	7.46	7.99	7.17	0.83	0.79	0.56	19.5	21.4	13.7
Princeton Univ.	3.82	7.34	5.67	6.91	12.80	10.25	0.92	1.27	0.84	20.2	35.2	28.1
Univ. of Pennsylvania	3.66	3.93	4.18	6.67	7.39	8.25	0.82	0.77	0.74	17.6	19.7	21.3
Stanford Univ.	3.59	4.64	4.19	6.35	8.32	7.76	0.86	0.83	0.61	19.0	23.8	18.7
Columbia Univ.	3.01	2.73	2.61	5.06	6.08	5.41	0.69	0.61	0.46	20.3	14.1	11.4
Univ. of British Columbia	2.77	2.64	2.54	5.50	5.69	6.05	0.74	0.60	0.50	23.2	15.3	10.1
Univ. of California, Berkeley	2.57	2.77	2.82	4.39	5.55	6.21	0.52	0.65	0.53	14.9	18.0	14.8
Northwestern Univ.	2.54	4.08	3.45	4.73	7.59	7.38	0.63	0.73	0.61	15.8	21.8	16.8
New York Univ.	2.50	2.34	3.00	4.63	4.30	5.50	0.74	0.53	0.53	12.4	10.0	13.6
Purdue Univ.	2.48	2.15	2.08	5.19	4.43	3.77	0.86	0.55	0.40	9.1	12.1	8.9
Univ. of Michigan	2.22	3.19	2.54	4.08	5.63	5.57	0.61	0.61	0.48	14.1	16.7	10.9
Univ. of Washington	2.09	2.48	1.79	4.27	5.07	4.88	0.48	0.51	0.41	8.9	12.1	9.9
Univ. of Southern California	2.00	2.09	2.64	3.76	5.08	5.95	0.52	0.48	0.50	12.3	10.8	11.7
Univ. of Wisconsin	1.96	2.10	2.70	4.16	4.84	6.40	0.59	0.54	0.53	10.1	13.0	13.6
Cornell Univ.	1.87	2.65	2.18	4.17	6.26	5.46	0.59	0.72	0.47	8.3	14.3	10.7
Univ. of Indiana	1.61	1.63	1.45	3.78	3.44	3.89	0.45	0.38	0.34	8.2	5.7	6.8
Duke Univ.	0.94	2.92	2.59	2.30	6.27	5.34	0.41	0.62	0.43	6.1	12.4	10.3
Univ. of Texas	0.38	1.03	2.10	0.86	2.36	4.73	0.12	0.26	0.36	0.9	3.7	7.3
Top ten	4.71	5.18	4.78	8.04	9.48	8.93	0.97	0.94	0.74	25.1	27.5	22.9
Top 25	3.37	3.86	3.55	6.01	7.34	7.09	0.76	0.75	0.60	18.1	20.2	16.8
Others	2.82	2.48	2.11	5.29	5.51	5.05	0.72	0.61	0.45	14.0	13.1	10.1
All schools	3.23	3.43	3.01	5.83	6.77	6.33	0.75	0.70	0.54	17.1	17.9	14.3
Top ten-Others	1.89	2.70	2.67	2.74	3.97	3.87	0.25	0.34	0.29	11.1	14.4	12.8
Top 25-Others	0.55	1.38	1.44	0.71	1.83	2.04	0.04	0.14	0.15	4.1	7.1	4.5

3. Empirical results

Having defined our measures of productivity we can move to study how this productivity is affected by the location in which a faculty is. We start by looking at the average faculty productivity.

3.1. Average faculty productivity

Table 4 reports average individual productivity by university and decade in terms of *impact*, *raw*, *article counts*, and *adjusted citations* for the top 25 schools and others. All non-top 25 universities are clustered into a 26th university called *other*. At the bottom of the table, we average productivities over all individuals in the top 25 and in the top ten ranked schools for that decade. The numbers indicate that faculty members are on average more productive while at the top ten and 25 universities than while at *other* universities. The difference in average productivity (shown in the bottom two rows) grows larger over time for the first three measures of productivity, while it peaks in the 1980s for *adjusted citations*.

The statistics in Table 4 do not imply, however, that the top universities make individuals more productive. The average productivity levels do not isolate the marginal effect of universities (the university fixed effect) on individuals' productivity above and beyond what the individual brings. All we can conclude from the table is that more productive researchers agglomerate in top universities and that tendency has increased over time. Whether the higher average individual productivity at the top schools is due to a favorable marginal effect of the top universities on their faculty or simply reflects the top universities' ability to attract and retain more productive colleagues is the subject of our investigation.

3.2. University fixed effects methodology

To isolate the university fixed effect, the marginal impact that home institutions have on individual productivity, we remove the individual characteristics affecting productivity. Then we control for common factors influencing individual productivity, primarily, experience and position rank. In essence, we view the university fixed effect as that of a treatment. We would like to remove all other factors influencing productivity such that we can compare treatments across universities.

A fairly general representation of the identification of productivity effects is given in Eq. (4).

$$y_{irfut} = \theta_{rfut} + \alpha_i + X_{irt}\beta + \varepsilon_{irfut}. \quad (4)$$

The subscripts index individuals (i: $i \in 1,\ldots,3262$), position rank (r: $r \in$ *assistant professor*, *associate professor*, *full professor*, *chaired full professor*), fields (f: $f \in$ *economics*, *finance*), universities (u: $u \in$ {top 25 schools}, *others*), and years (t: $t \in 1970,\ldots,2001$). y_{irfut} is the productivity (*impact*, *raw*, *article count*, *or adjusted citations*) of individual i during year t. θ_{rfut} is a general notation for the level effects of rank, field, university, decade, and all of their interactions. In practice, we make some restrictions on the interactions for tractability and describe them in the Appendix of Kim, Morse and Zingales (2006a).

The α_i are the individual fixed effects, which are included to control for differences in individual faculty quality. In specifying θ_{rfut} and α_i as such, we implicitly assume that the individual's fixed effect does not interact with rank, field, or time (or their interactions) in a way that would be systematic to schools.

X_{irt} is the matrix of individual control variables, varying over time and representing common factors across faculty, which could affect individual productivity irrespective of physical location. The control variables include the number of years elapsed since an individual's first academic appointment (*career years*), whether or not the individual is a visiting faculty in that year (*visiting*), and the impact weight of the journal for which the person is currently editing (*editor impact*).

There is no reason to expect *career years* to be related linearly to productivity (Oster and Hamermesh, 1998). Given that our primary interest is not in the structural explanation for *career years*, but in controlling for its relation with productivity, we allow the data to dictate the form of the relation for which we control. A plot of *impact* and *raw productivity* as a function of *career years* is shown in Fig. 1. In a univariate setting, raw productivity increases for the first few years of an individual's career and then declines, eventually at a decelerating rate. *Impact productivity* reaches its peak during the first couple of years and soon thereafter declines monotonically. The figure suggests that productivity is inversely related to age and that the relation warrants a cubic functional form.

To establish a baseline, we first estimate a model of individual productivity that includes individual fixed effects (to capture the quality of each faculty member), career experience years and stages of career (to control for general productivity trends for all faculty); field effects (to eliminate differences in productivity standards between finance and economics), and decade fixed effects (to control for possible trends in average productivity). We then add the university fixed effects at a decade-field level. The empirical model is given by

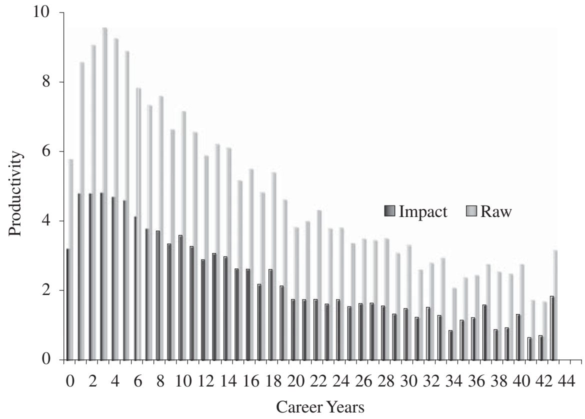

Fig. 1. Average individual annual productivity by career years. An individual's *raw productivity* is measured as the *American Economic Review* (*AER*) equivalent pages for that person for the year in which the article productivity was written, using Ellison (2002) adjustment plus one year to lag from the publication dates of articles to the writing time period. *Impact productivity* multiplies each article by the decade impact for the journal published. *Career years* is the number of years since the faculty member earned a Ph.D. year. If the person has not yet received his Ph.D., *career years* is recorded at zero.

$$y_{irfut} = \theta_r + \theta_f + \theta_d + \theta_{fd} + \theta_u + \theta_{fu} + \theta_{ud} + \theta_{fud} + \alpha_i + X_{irt}\beta + \varepsilon_{irfut}. \quad (5)$$

The θ's refer to fixed effects for rank (θ_r), finance department (θ_f), decade (θ_d), decade-department (θ_{fd}), university (θ_u), university-department (θ_{fu}), university-decade (θ_{ud}), and university-decade-department (θ_{fud}). The α_i are the individual fixed effects, and the X_{irt} are the experience and rank controls. Because the individual fixed effect, adjusted for age, is constant over time, the university-decade fixed effects are identified by faculty moves and by the changes in age-adjusted productivity over different decades of faculty who do not move.

Estimation of Eq. (5) allows us to address three key questions. First, are the university fixed effects significant in any statistical and economic way in explaining variation in productivity? As in Bertrand and Scholar (2003), we perform a simple F-test of joint statistical significance of university fixed effects en masse. In our case, this is only the first, but necessary, step. If the university fixed effects are not significant, then any additional tests would be irrelevant. Even a finding of no fixed effect would be interesting because it would suggest that the research environment has no effect on individual productivity.

If the university fixed effects are significant, what factors drive the university fixed effects? Is it organizational culture, spillovers from colleagues, or perhaps weather and other location factors that drive whether one university is more conducive to research than another? The final question we address is: Do university fixed effects vary over time? The answer should give some insights into the long-term sustainability of comparative advantages.

3.3. University fixed effects results

Table 5 reports the estimates of Model 2 for *impact productivity* (Columns 1 and 2), *raw productivity* (Columns 3 and 4), *article counts* (Columns 5 and 6), and *adjusted citations* (Columns 7 and 8). Article count productivity is estimated with a Poisson model to incorporate the count nature of the data, and thus the coefficient magnitudes cannot be directly compared with the other columns.[20] Columns 2, 4, 6, and 8 include university fixed effects, while columns 1, 3, 5, and 7 do not.

An initial observation is that the effects of career experience, editorship, and visiting status are not very sensitive to the choice of productivity measure. As shown in Fig. 1, productivity is the highest in early career years. When we use the estimates from Columns 1, 3, 5, and 7 to plot productivity over career years, we find that *impact productivity* is highest during the rookie year and *adjusted citations* peaks during the third and fifth years. *Raw* and *article counts* productivities peak between the fourth and sixth year (not coincidentally the time of tenure decision in most places) and drop monotonically afterward. Age has a particularly large negative influence on *impact productivity*. Even beyond the effect of rank, a faculty member produces 1.8 fewer impact pages per year after 20 career years, a drop of 44% from the rookie year (years zero and one). For citations, the effect is similar. Papers in career year 20 generate 30% fewer citations (nearly five citations fewer from a peak of 17.5) than papers in career year four. For *raw productivity*, the inference is a bit smaller. The faculty produces 2.1 raw pages per year less than at the peak, a drop of 15%. The fact that the *impact productivity* peaks at the rookie year and declines thereafter provides a possible answer to the puzzle of why schools devote so much energy and resources to recruit rookies when they can hire seasoned junior faculty with proven track records and more polished teaching abilities. The standard answer used to be that rookies have a higher option value. We provide a much simpler explanation: To hire researchers at their peak, the optimal year is the rookie one.

Faculty with higher rank also seem to be less productive. Taking into account career years and individual differences in talent, the marginal impact of being an associate professor is one-half page less *impact productivity* compared with assistant professors. The marginal impact of being a full professor is almost one page less; for a chaired professor, it is one and one-third pages less. Because we are already controlling for career years and do not have a measure for time devoted to other duties, interpreting these numbers in a causal way is impossible. We are interested in them only as controls and note that all of our results are robust to their omission. Accounting for career years and rank, the *impact productivity* of a chaired professor 20 years after earning a Ph.D. is 75% lower than at the beginning of her career. With this result, we are measuring only academic article productivity. More senior faculty might write more books and cases, distribute articles via other media outlets or journal types, and contribute in other ways, often through greater administrative service and mentoring junior faculty and doctoral students.

Editing a journal is not negatively associated with productivity. One possible explanation is that editors are a select group; only active senior faculty become editors. Hence, the editor variable captures not only the negative effect of the time commitment to editing, but also the positive effect of remaining actively engaged in research despite a relatively senior rank. Alternatively, editors could have expertise in writing papers suited for the publication process and thus can maintain their productivity level in spite of the time commitment required by the editorship.

The coefficients on the decade dummy variables in Columns 1 and 5 show no increase in *impact* or *article counts* productivities for economics faculty from the 1970s to the 1980s or 1990s. In contrast, the decade coefficients for *raw productivity* in Column 3 are significantly positive. This result is due to the publication inflation inherent in raw productivity. The 1990s decade coefficient for *adjusted citations* in Column 7 is significantly negative, primarily reflecting the drop in *article counts* in the 1990s. Thus, we rely on the *impact productivity* hereafter to make cross-decades comparisons. The coefficients on the interaction of decades with finance faculty are significantly negative by a similar magnitude across decades, implying that finance faculty are less productive than those in economics departments, but equally so across decades.

Column 2 adds university fixed effects to the specification. The university fixed effects are estimated at a decade-field level. In other words, we fit 153 fixed effects (26 schools $\times$ 2 departments $\times$ 3 decades $=$ 156$-$3 for the absence of a finance department at Princeton University). The F-test for joint significance of the university fixed effects is 35.28, well above conventional significance thresholds, establishing the relevance of examining university fixed effects. The effects of the control variables on productivity do not materially change with the inclusion of the university fixed effects.

The only coefficients that change markedly are those on the decade dummies. For *impact productivity*, the decade coefficients for the 1980s and 1990s are a significantly positive 0.481 and 1.116, respectively. One must be careful in interpreting the decade coefficients, as they take on a different interpretation when the university fixed effects are included. The offset of the university fixed effects structure is an *other* university in economics in the 1970s.[21] The positive decade coefficients imply that a move from one of the top 25 universities to one of the *other* increases productivity relative to the effect of

[20] Because the distribution of adjusted citations is still highly skewed, citation productivity is estimated using linear procedures instead of Poisson. The results do not materially change in using count process estimation, but the linear model is not subject to over-dispersion concerns.

[21] Although the *other* set of faculty does not represent the universe of faculty at all other universities, the interpretation of the *other* fixed effect remains valid in that the individual fixed effects have been removed.

Table 5

Panel A: University fixed effects and other determinants of faculty productivity.

Observations are at the individual-year level. *Impact* and *raw* productivities are measured as the count of *American Economic Review* (*AER*) equivalent pages written by each faculty in 41 economics and finance journals. Adjustment to AER equivalents normalizes by font, typesetting, and average article length. Publication pages are divided by $1/n$ coauthors. *Impact productivity* multiplies each article by the decade impact factor of the journal published. *Article counts* is the simple sum of articles published by year. Adjusted citations are from Web of Science, adjusted for age and upper winsorized at 5%. The 1990s decade includes 2000 and 2001. *Career years* is the years since earning a Ph.D., estimated in thousands for the cubic. *Associate*, *full chaired*, and *visiting* professor are indicators for the faculty member's position. *Editor impact* is the sum of impact factors for the journals for which the faculty member serves as an editor or coeditor. All columns include individual fixed effects. Columns 2, 4, 6, and 8 add university-field fixed effects. *Article counts* are estimated with Poisson Regression. T-statistics are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. + indicates that the coefficient is statistically different (at the 5% level) from the coefficient of the previous decade. For Panel B, the *impact productivity* estimation is from Panel A, Column 2. For Panel C, the *adjusted citations* estimation is from Panel A, Column 8. Panel D presents reestimates of university fixed effects for the regression in Panel A, Columns 2 and 8 for the pooled top 25 universities by decade. The top portions of the estimation are not shown. They are very similar to Panel A, Columns 2 and 8 for *impact productivity* and *adjusted citations*, respectively.

Panel A. Determinants of faculty productivity

Variables	Impact productivity		Raw productivity		Article counts		Adjusted citations	
	1	2	3	4	5	6	7	8
Career years	−0.010	0.007	0.099	0.123	0.045***	0.046***	0.249	0.320
	(0.28)	(0.21)	(1.16)	(1.45)	(6.01)	(6.04)	(0.99)	(1.35)
Career years^2	−0.006***	−0.007***	−0.015***	−0.018***	−0.005***	−0.005***	−0.036***	−0.045***
	(2.88)	(3.69)	(3.35)	(3.83)	(11.04)	(11.10)	(3.14)	(4.11)
Career year^3	0.101***	0.138***	0.242***	0.297***	0.070***	0.072***	0.622***	0.807***
	(2.99)	(3.84)	(3.19)	(3.69)	(9.00)	(9.16)	(3.32)	(4.35)
Associate professor	−0.487***	−0.455***	−1.131***	−1.022***	−0.138***	−0.128***	−1.747**	−1.674**
	(3.09)	(2.78)	(3.43)	(3.07)	(4.91)	(4.45)	(2.46)	(2.38)
Full professor	−0.895***	−0.876***	−1.946***	−1.841***	−0.237***	−0.224***	−7.581***	−6.527***
	(4.36)	(3.78)	(5.29)	(4.81)	(6.42)	(5.93)	(6.44)	(5.70)
Chaired full	−1.260***	−1.055***	−2.515***	−2.184***	−0.190***	−0.172***	−4.189***	−4.177***
	(5.55)	(4.50)	(6.37)	(5.37)	(4.23)	(3.72)	(4.75)	(4.58)
Editor impact	−0.038	−0.039	−0.142	−0.162	0.045	0.029	−1.573	−1.970
	(0.04)	(0.04)	(0.11)	(0.13)	(0.78)	(0.49)	(0.41)	(0.53)
Visiting	0.028	0.034	0.161	0.163	0.051*	0.037	0.939	1.079*
	(0.33)	(0.42)	(0.91)	(0.96)	(1.79)	(1.26)	(1.49)	(1.73)
Decade 1980s	0.180	0.481**	0.841***	1.528***	0.042	0.108**	−0.266	2.511**
	(1.60)	(2.42)	(3.98)	(4.76)	(1.36)	(2.28)	(0.38)	(2.27)
Decade 1990s	0.237	1.116***	1.282***	2.831***	0.055	0.200***	−2.095**	3.732*
	(1.44)	(2.79)+	(3.49)	(4.08)+	(1.11)	(3.24)	(2.19)+	(1.71)
Finance* 1970s	−1.278**	−0.756	−0.470	0.835	0.319	0.333	−0.257	3.529
	(2.47)	(0.90)	(0.52)	(0.65)	(1.60)	(1.47)	(0.13)	(1.40)
Finance* 1980s	−1.126**	−0.387	−0.939	0.470	0.032	0.027	0.689	6.966***
	(2.29)	(0.49)	(1.06)	(0.39)	(0.17)+	(0.13)+	(0.40)	(2.81)
Finance* 1990s	−0.975*	−0.434	−1.190	−0.023	−0.170	−0.271	0.267	5.587**
	(1.78)	(0.55)	(1.24)	(0.02)	(0.88)+	(1.27)+	(0.15)	(2.32)
Observations	35,917	35,917	35,917	35,917	35,917	35,917	35,993	35,993
F-test (*p*-value) for inclusion of University		35.28		18.55		228.58		545
Fixed effects		(0.000)		(0.000)		(0.000)		(0.000)
Fixed effects	Faculty	Faculty, University	Faculty	Faculty, University	Faculty	Faculty, University	Faculty	Faculty, University

Panel B. University fixed effects estimates for the *impact productivity* across decades and fields

School	Dependent variable: impact productivity					
	Economics departments			Finance departments		
	1970s	1980s	1990s	1970s	1980s	1990s
Carnegie Mellon Univ.	−1.190***	0.432*	−0.109	1.717***	0.550	−1.062*
	(4.89)	(1.90)+	(0.56)	(4.25)	(1.06)	(1.90)
Columbia Univ.	1.077***	−0.044	−0.847*	1.174***	−0.227	−0.347**
	(3.96)	(0.13)	(1.94)	(8.43)	(1.06)	(2.03)
Cornell Univ.	−0.499**	−0.332	−0.255	−0.045	−0.937***	−0.551
	(2.34)	(0.90)	(0.68)	(0.11)	(4.44)	(1.06)
Duke Univ.	−0.401*	0.367	−0.619***	−0.535***	0.233*	−0.656***
	(1.84)	(1.64)	(2.86)	(2.62)	(1.95)	(4.78)
Harvard Univ.	2.127***	0.250	−1.781***	−2.249***	−2.114***	−2.781**
	(11.33)	(1.00)+	(5.53)+	(3.90)	(3.33)	(2.30)
Univ. of Indiana	0.349***	0.819***	0.149	1.846***	−0.883***	0.246**
	(2.65)	(4.73)	(0.87)	(29.39)	(3.39)	(2.36)

Table 5 (*continued*)

Panel B. University fixed effects estimates for the *impact productivity* across decades and fields

School	Dependent variable: impact productivity					
	Economics departments			Finance departments		
	1970s	1980s	1990s	1970s	1980s	1990s
Massachusetts Institute of Technology	1.003***	0.560***	−1.208***	1.533*	0.819	−0.128
	(5.28)	(3.10)	(3.30)+	(1.92)	(0.81)	(0.20)
New York Univ.	−0.307	−0.300	−0.279	1.457***	0.340**	0.991***
	(0.95)	(0.67)	(0.59)	(10.99)	(1.97)	(7.95)
Northwestern Univ.	0.892***	1.377***	0.351	0.427*	0.743***	−0.253
	(5.59)	(6.68)	(1.01)+	(1.75)	(3.47)	(0.75)+
Ohio State Univ.	1.535***	−0.348*	−0.892***	2.365***	−0.181	−0.270
	(7.48)	(1.68)	(3.22)	(11.58)	(0.69)	(0.47)
Princeton Univ.	1.220***	1.530***	−0.439			
	(4.16)	(4.81)	(0.96)+			
Purdue Univ.	−0.276	−0.911***	−0.821***	0.952**	1.396**	0.056
	(0.50)	(3.49)	(2.59)	(2.24)	(2.18)	(0.11)
Stanford Univ.	1.313***	0.332	−1.074***	1.193***	2.719***	1.141**
	(7.62)	(1.25)	(3.29)+	(3.83)	(6.82)	(2.17)+
Univ. of British Columbia	0.564***	0.196	0.415*	3.285***	0.320	0.497
	(2.61)	(0.80)	(1.80)	(3.01)	(1.29)	(1.49)
Univ. of California, Berkeley	−0.475**	−0.147	−0.932***	0.214	−0.866**	−0.319
	(2.32)	(0.56)	(3.29)	(0.64)	(2.26)	(0.94)
Univ. of California, Los Angeles	1.425***	0.635	−0.345	2.025***	0.987**	0.508*
	(3.61)	(1.56)	(1.11)	(11.25)	(2.26)	(1.72)
Univ. of Chicago	1.796***	0.889***	−0.463	2.466***	1.465***	−0.460
	(8.66)	(2.91)	(1.56)+	(12.08)	(6.12)	(1.19)+
Univ. of Michigan	0.892***	1.149***	0.357*	1.259***	1.001***	−0.621**
	(6.10)	(7.56)	(1.74)	(4.19)	(3.11)	(2.17)+
Univ. of Pennsylvania	1.217***	0.868***	−0.167	0.591***	−0.539*	−0.210
	(5.11)	(5.18)	(0.89)+	(3.47)	(1.76)	(0.66)
Univ. of Rochester	1.420***	1.552***	0.166	−0.163	−1.045***	−0.558*
	(5.89)	(5.74)	(0.60)+	(0.29)	(3.38)	(1.65)
Univ. of Southern California	0.293***	−0.437***	0.031	−0.101	1.338***	−0.485
	(2.69)	(4.95)	(0.15)	(0.19)	(3.97)	(1.58)
Univ. of Texas	−0.059	−0.138	−0.171	−0.631**	−0.193	−0.180
	(0.17)	(0.47)	(0.34)	(2.35)	(0.59)	(0.32)
Univ. of Washington	−0.600***	−0.501*	−1.462***	1.094	−0.036	−1.041
	(2.95)	(1.85)	(4.15)	(1.48)	(0.04)	(1.09)
Univ. of Wisconsin	0.316**	−0.227	−0.676	0.687***	0.060	0.497
	(2.51)	(1.09)	(1.49)	(3.32)	(0.30)	(1.63)
Yale Univ.	1.029***	−0.028	−0.100	1.882***	1.036***	−1.660***
	(3.35)	(0.07)	(0.26)	(2.72)	(3.56)	(4.19)+
Significant (+) count	17	9	2	16	10	4
Significant (−) count	5	4	9	3	6	7

Panel C University fixed effects estimates for *adjusted citations* across decades and fields

School	Dependent variable: adjusted citations					
	Economics departments			*Finance departments*		
	1970s	1980s	1990s	1970s	1980s	1990s
Carnegie Mellon Univ.	−5.467***	−4.197***	−2.579*	15.50***	−1.544	−6.013
	(3.77)	(2.99)	(2.04)	(7.71)	(0.42)+	(1.64)
Columbia Univ.	13.34***	−1.988	−3.546**	4.461***	−1.597	−6.095***
	(10.2)	(1.69)+	(2.28)	(3.89)	(1.15)	(3.59)
Cornell Univ.	−4.409***	−5.622**	−5.316**	−6.414	−10.91***	−8.169*
	(3.68)	(2.43)	(2.76)	(1.56)	(4.61)	(1.86)
Duke Univ.	−0.139	−0.924	−4.815***	−0.327	−1.681	−5.514***
	(0.22)	(0.95)	(6.46)	(0.46)	(1.19)	(2.81)
Harvard Univ.	9.641***	0.702	−9.166***	−9.081***	−9.384***	−16.56**
	(8.61)	(0.60)+	(7.15)+	(3.39)	(3.46)	(2.45)
Univ. of Indiana	2.032	−0.004	0.628	17.74***	−6.114**	4.655***
	(1.26)	(0.00)	(0.52)	(8.20)	(2.32)+	(4.13)
Massachusetts Institute of Technology	12.95***	3.717	−6.374***	17.30**	−1.439	−2.160
	(13.7)	(1.62)+	(3.04)+	(2.74)	(0.22)+	(0.56)

Table 5 (*continued*)

Panel C University fixed effects estimates for *adjusted citations* across decades and fields

School	Dependent variable: adjusted citations					
	Economics departments			*Finance departments*		
	1970s	1980s	1990s	1970s	1980s	1990s
New York Univ.	−1.378	−4.038**	−2.052	5.331***	−2.310*	0.533
	(1.32)	(2.50)	(1.41)	(4.17)	(1.87)	(0.43)
Northwestern Univ.	7.823***	4.946***	1.653	1.993*	0.189	−7.267***
	(8.24)	(4.16)	−(0.90)	(1.99)	(0.30)	(5.96)+
Ohio State Univ.	10.74***	5.840**	−0.764	14.99***	6.040***	6.549*
	(4.06)	(2.32)	(0.28)	(5.51)	(5.96)	(1.90)
Princeton Univ.	7.771***	7.024***	−3.863*	—	—	—
	(6.78)	(6.38)	(2.00)+			
Purdue Univ.	−8.560***	−6.151***	−5.527***	7.272**	7.770*	−6.249
	(4.19)	(3.62)	(2.83)	(2.62)	(1.79)	(1.60)+
Stanford Univ.	10.80***	2.812*	−7.859***	1.340	1.912	1.644
	(8.73)	(1.81)+	(4.12)+	(0.77)	(0.56)	(0.39)
Univ. British Columbia	6.550***	2.828**	−1.000	48.98***	3.032***	−1.565
	(4.04)	(2.24)	(1.23)	(3.89)	(3.94)+	(0.83)
Univ. of California, Berkeley	−1.486	1.538	−5.224***	3.466	−8.367*	−6.657
	(1.25)	(1.37)	(−4.39)+	(0.82)	(2.05)+	(1.32)
Univ. of California, Los Angeles	6.662***	2.971*	−1.975	10.90***	−3.313	−8.736***
	(3.74)	(1.87)	(1.53)	(6.84)	(1.13)	(3.63)
Univ. of Chicago	7.713***	1.924	−5.867***	16.64***	5.927***	−6.516***
	(7.79)	(1.54)+	(4.41)+	(11.4)	(4.25)+	(3.49)+
Univ. of Michigan	6.506***	3.612*	−1.911	10.17***	2.842**	−5.531***
	(4.55)	(1.81)	(1.28)	(6.04)	(2.25)	(3.27)+
Univ. of Pennsylvania	8.335***	4.836***	2.925*	0.481	−3.481**	−3.687*
	(8.28)	(5.47)	(2.05)	(0.34)	(2.52)	(2.02)
Univ. of Rochester	0.091	6.172***	1.264	−2.334	−4.698***	−8.465***
	(0.08)	(4.68)	(0.97)	(0.84)	(2.88)	(−3.94)
Univ. of Southern Calif.	4.568***	−0.509	4.809***	3.262	1.889	−9.536***
	(4.08)	(0.29)	(3.07)	(1.62)	(0.99)	(5.62)+
Univ. of Texas	−0.498	0.431	1.151	−1.009	−0.878	3.387
	(0.27)	(0.30)	(0.54)	(0.60)	(0.48)	(1.45)
Univ. of Washington	−0.098	0.967	−1.622	8.954***	2.474	2.072
	(0.08)	(0.86)	(1.11)	(4.55)	(1.20)	(1.23)
Univ. of Wisconsin	3.280***	2.750**	0.099	3.050***	0.044	4.045***
	(3.87)	(2.59)	(0.092)	(4.13)	(0.06)	(4.23)
Yale Univ.	4.849***	2.483	−0.561	12.70***	2.963*	−13.65***
	(3.88)	(1.58)	(0.37)	(3.83)	(1.94)	(6.34)+
Significant (+) count	15	9	2	15	6	2
Significant (−) count	3	4	11	1	7	12

Panel D. University fixed effects across decades and fields, the pooled model of top 25 universities

Decade	Economics	Finance
	Dependent variable: *impact productivity*	
1970s	0.839***	1.030***
	(4.45)	(5.99)
1980	0.490***	0.444**
	(4.41)	(2.34)
1990s	−0.482**	−0.276
	(2.70)	(1.25)
	Dependent variable: *adjusted citations*	
1970s	4.162***	1.045
	(3.77)	(0.59)
1980s	1.454	−2.393
	(1.62)	(1.67)
1990s	−1.587*	−2.693***
	(1.94)	(2.17)

moving to an *other* in the 1970s. That is, the relative fixed effect of *other* schools has increased by almost one-half an impact-weighted *AER* page in the 1980s and by more than one full impact-weighted *AER* page in the 1990s, both these changes are statistically different from zero (Table 5).

The declining effect of elite universities on faculty productivity is robust to the number of schools classified as elite universities. We repeat the regression in Column 2 with only the top five schools defined as elite universities, and the rest as *other*. We also do the same for the top ten, 15, and 20. In all cases, the decade coefficients for the 1980s and 1990s are significantly positive at the 1% level.

This diffusion of positive spillover effects from top universities to *other* over the last two decades can be seen more clearly in Panel B of Table 5, which presents the university fixed effects by decades, separately for economics and finance. These coefficients are interpreted as the influence of each of the 25 universities on faculty publications over and above that of the same faculty member when residing at an *other* university. While the coefficients show that there were 17 and 16 universities with significantly positive effects in the 1970s for economics and finance, respectively, only two and four universities have positive effects by the 1990s. In fact, in the 1990s, nine economics and seven finance departments had negative fixed effects. A similar pattern emerges when university fixed effects are estimated with *adjusted citations* in Panel C.

Some of the coefficients of university fixed effects in Panels B and C have different patterns or have significances varying from the trend. Thus, we estimate the combined pattern for the elite universities as a group by implementing a pooled model of all 25 elite universities. Panel D presents the university fixed effects for the elite universities as a whole by decade, separately for economics and finance.[22] A clear picture emerges for the pooled model. For economics faculty, the all elite universities coefficient is significantly positive in the 1970s and significantly negative in the 1990s, with the magnitude of the 1980s coefficient falling in between, for both *impact productivity* and *adjusted citations*. Finance faculty also show a similar pattern. We conclude that the declining university fixed effects is a general trend that holds for the elite schools as a group.

Caution should be exercised in interpreting these fixed effects results, however. Although it could be surprising to see that some of the most elite universities exhibit negative fixed effects, these effects do not mean that the elite universities have less productive faculty members relative to *other* universities. Because the individual fixed effects are removed, the negative university effects mean that an individual is less productive when residing at that university than if she were at an *other* university.

It is also worth noting that for some schools, our exclusion of book publications and case writing from the productivity measure could contribute to the negative fixed effects. For example, the Harvard finance group exhibits consistently negative effects throughout the three decades, perhaps because case writing is not included in our productivity measure.

To the extent that trade-offs are made in allocating resources to research versus teaching, our estimate of university fixed effects could bias against schools that emphasize teaching. It could be argued that, because *Business Week* began ranking MBA programs in the early 1990s, the top business schools in the US put more emphasis and resources into teaching, thus contributing to the decline in the university effects during the 1990s. Economics departments, however, are not under the same pressure (the *US News and World Report* ranking does not put the same emphasis on teaching as does *Business Week*), and no obvious shift has taken place from research to teaching in economics departments. Thus, our omission of teaching-related activities in measuring productivity is not likely to be responsible for the reduction in the positive university effects during the 1970s.

The negative elite university fixed effects in the 1990s also could be partially due to the trend among some very well known scholars opting for Internet article dissemination in lieu of journal publication (Ellison, 2006). Such an effect should be most prevalent among full professors, because non-full professors have to publish in journals to establish a reputation and obtain tenure. Thus, we examine university fixed effects for assistant and associate professors separately from those for full professors (not reported). These results show declining university effects for both groups, and the decline appears sharper for the assistant and associate group.

4. Treatment selection bias in university effects estimation

Our major concern with these estimates is that they are subject to a possible treatment selection bias. The location of an individual at any time can best be thought of as a two-sided search model, in which the university looks for the best person to fill its needs and the scholar chooses the best university opportunity to maximize her utility.[23] In this section, we consider four stories that might result from selection matching that would be consistent with our results. To assess the viability of each story, we first set up a matrix of changes in productivity around moves.

Our transition matrix tabulates changes in productivity around moves in two dimensions: the direction of the move (up, lateral, or down) and the status of faculty rank (full professor or not). The change in productivity compares average individual–adjusted productivity in the two years after a move to that in the two years before a move. Individual adjusted productivity is defined as the residual of a regression of individual productivity on career years, career years squared, career years cubed, rank, visiting, and editor status. We drop the first year at the new university to account for possible set-up costs in moving. An up move is a move from any university that is not in the top five to a top five institution or from an *others*

[22] For finance, the group has 24 schools because Princeton does not have a finance department.

[23] See MacLeod and Parent (1999) and Ackerberg and Botticini (2002) for examples of endogenous matching applications.

university to a top 25.[24] A down move is any move from a top five to a non-top five or from a top 25 to *others*. A lateral move is a move within the top five, within the top 25 universities, or within the *others* universities. The results are reported in Table 6.

4.1. Quasi-retirement story

If full professors leave elite schools to *other* universities with the intent of going into quasi-retirement, the *other* school fixed effect would be biased downward. The results in Table 6 do not seem consistent with this story. Although all downward moves by full professors show negative signs, none is significant. Furthermore, the changes in productivity following downward moves are indistinguishable from those following other types of moves by full professors.

4.2. Non-promoted moving down story

A second selection story concerns assistant and associate professors who move to less prestigious universities because of a failure to obtain tenure or promotion. For this selection story to explain our declining positive university fixed effect from the 1970s to the 1980s, individuals moving down in the 1970s should exhibit more negative productivity shocks than individuals moving down in 1980s. Because this pattern is not observed in the data, the nonpromoted moving down story cannot explain our results.

4.3. Complementarity story

The third selection concern arises if universities tend to make offers to people whom they consider to have good future prospects, and if individuals tend to accept offers from universities with a research environment complementary to their needs and attributes. This match is more likely to occur for successful individuals moving to higher ranked or lateral schools, irrespective of position rank. The complementarity would lead to higher productivity in the new location, generating positive university fixed effects for the elite schools.[25]

This story finds only isolated support in the transition matrix. Of the 12 relevant combinations of ranks and decades for the lateral and up moves, nine are statistically zero; two are statistically negative; and one is statistically positive. The lone positive impact on productivity occurs for assistant and associate professors moving upward in the 1970s, which becomes insignificant in the 1980s and the 1990s. This pattern is consistent with the pattern in the university fixed effects over the three decades, rendering some credence to the complementarity selection story. However, it still begs the question of why the complementarity effects have disappeared in the 1980s and 1990s.

Table 6
Change in individual adjusted impact productivity following moves.

The transition matrix presents the change in individual adjusted *Impact Productivity* around a move, where *Individual Adjusted Impact Productivity* is the residual of regressing impact productivity on rank, career years, career years squared, career years cubed, visiting, and editor impact. To construct the statistics, we calculate the average of the two years of adjusted *impact productivity* following a move and subtract from it the average of the two years of adjusted productivity prior to the move. We exclude the move year in both pre- and post-measures. A move up is defined to be a move into a top five university by anyone or a move into a top 25 university by those from an *others* school. The top five are chosen as those with the highest decade average individual productivity in the field. A lateral move is moving within the top five, within the top six-25 universities or within *others*. A move down is a move from top five to top six-26 or from top 25 to others. The observation counts for moves in each category are given below the change in productivity. The 1990s decade includes 2000 and 2001. Asterisks *, **, and *** denote significance at a 10%, 5%, and 1% level, respectively.

Move direction	1970s	1980s	1990s
Assistant and associate professors			
Down	0.558	−0.806*	0.337
	54	119	145
Lateral	−0.795	−0.484	0.240
	63	131	124
Up	2.013**	0.454	−0.254
	31	75	60
All moves	0.287	−0.385	0.193
	148	325	329
Full professors			
Down	−1.008	−0.151	−0.692
	37	104	123
Lateral	−1.938**	−0.493	−0.172
	40	111	164
Up	−0.067	1.326	−1.119*
	28	66	95
All moves	−1.112*	0.061	−0.575**
	105	281	382

4.4. Tournament and regression-to-the-mean stories

Finally, our estimates would be biased if there is an implicit tournament. For example, a tenured faculty at a relatively low ranked elite university might continue to work with the goal of making it to the top economics department, where a tenured person might have no higher career goal to motivate him (except for the Nobel prize). A tournament bias would result in a negative university fixed effect for the very top schools, as individuals who reach the top fail to sustain the high level of productivity once they arrive.

[24] The top five universities are defined to be those on a decade level with the highest average *impact productivity* for the faculty members as reported in Table 5 separately for finance and economics.

[25] The flip side of the complementarity story is that university hiring practices have changed. To explain our results with this story requires that the top universities were more effective in hiring in the 1970s than in the 1990s. To test this story, we compare the productivity, adjusted for age and rank, of individuals coming into a school with that of incumbent faculty. Using the average adjusted productivity from the prior two years, we find the same pattern: New hires have higher productivity than incumbents for every decade (0.67 in the 1970s, 1.44 in the 1980s, and 1.00 in the 1990s). The 1970s new hire surplus is smaller than that of the 1990s, invalidating this story as a potential explanation for the disappearance of the fixed effect.

Table 6 reports that up moves by full professors have statistically zero effects on productivity in the 1970s and the 1980s, but the impact turned negative in the 1990s. The negative effect in the 1990s is consistent with the tournament story. However, when we break the full professor up moves into moves of top six to 25 to top five, moves of *others* to top five, and moves of *others* to top six to 25, we find that Table 6 negative coefficient on up moves in the 1990s results from faculty moving from *others* to top six to 25, where the tournament effect, if any, should still be in effect.

4.5. Two-stage selection model

Although Table 6 reveals no convincing evidence in support of the selection bias stories, as a further robust check we estimate a treatment model in which the selection into a particular university treatment is not random. The selection model consists of two pieces: estimating the probabilities both of moving and, given a move, of locating at a particular university.

First, we estimate ϕ_{it}^{move}, the probability that individual i moves in time t, with a logit model based on age: $\phi_{it}^{move} = logit[f(age_{it})]$. Because individuals tend to move more in their youth (when they do not have children) or in their late 40s and early 50s, when their children are in college, age is a good exogenous selection variable. To capture these effects, we fit a polynomial of the fourth order on the individual's age. Higher orders did not materially increase the likelihood ratio test statistic for relevance. These estimates are reported in Table 7, Panel A. The likelihood ratio test has a statistic of 541.2, indicating a high degree of relevance for age in the probability of a move.

Second, we use a conditional logit model (McFadden, 1974) to estimate the location choice of where each individual might prefer to go. The probability of moving to potential location u conditional on moving is a function of exogenous variables Z_{iut} specific to individual i at time t: $\phi_{iut}^{locate|move} = conditional\ logit(Z_{iut}\eta + v_{iut})$.

The key is to find good exogenous variables predicting the preference of location for each individual. We use four variables. The first is peoples' desire to locate close to their origin. Because we cannot obtain city of birth information for our sample, we use the city of their undergraduate alma mater as a proxy. The closeness to one's origin is measured by distance in flight times.[26] To create this instrument, we search more than four thousand combinations of flights between cities on expedia.com for midweek, non-holiday dates. Our measure, *flight times*, is the minimum flight time (expressed in hundreds of minutes) to the city of origin from the prior-year university minus the flight time to the origin from the potential university. If the difference in flight times is a positive number, reflecting a move closer to home, there should be a higher probability of being in that location.

Another evident instrument is *prior location*. Because of transaction costs in moving, the prior year location should have high predictive power for the current location.[27]

Our third instrument is motivated by anecdotal evidence that top schools often attract back their former best students. We create a dummy variable, *Ph.D. school location*, equal to one if the potential university is the Ph.D. alma mater, and the faculty is at one of the top five universities. Universities generally do not hire their own students in the job market, so we set this variable equal to one only if the person has more than two years in career experience.

The fourth instrument, *productivity distance*, is the difference between an individual's productivity and the potential schools' average productivity, both measured in the two years prior to a potential move. In measuring *productivity distance*, we must distinguish untenured faculty from tenured. Only untenured faculty with below-average productivity can be fired, but all types of faculty with above-average productivity can be hired away. Thus, for assistant and associate professors we use the absolute value of the difference in productivity, and for full professors we use the actual difference. To take full benefit from the varying predictions by rank, we also interact *productivity distance* with each position rank.

Table 7, Panel B reports the conditional logit estimates. The probability of being at a location increases significantly when individuals move closer to their origin. The probability of being observed at a prior year location is higher, reflecting the strong role of transaction costs to moving. The most successful individuals are more likely to be observed at their Ph.D. locations. The Ph.D. location effect is even stronger for the location choices that are not the prior year location. When individuals move, they are more likely to move to their Ph.D. alma mater. Finally, faculty tend to be at a university with an average productivity similar to their own, regardless of their rank. Overall, our selection model fits the conditional logit model extremely well. With 816,624 observations, the pseudo R^2 is 0.821 and the Wald test statistic is 42,415.

The goal of the two first stages of the selection model is to obtain $\hat{\rho}_{iut}$: the predicted probability of individual i being at university u at time t, which can be obtained multiplying the probability of moving times the conditional probability of a location given a move: $\hat{\rho}_{iut} = \hat{\phi}_{it}^{move} \cdot \hat{\phi}_{iut}^{locate|move}$.

Having obtained $\hat{\rho}_{iut}$, we can finally estimate the second stage as

$$y_{irfut} = \theta_r + \theta_f + \theta_d + \theta_{fd} + \hat{\rho}_{iut}\theta_{ufd}^* + \alpha_i + X_{irt}\beta + \varepsilon_{irfut}, \quad (6)$$

where θ_{ufd}^* is the decade-field university effects based on the instrumented location of each individual. The standard errors are corrected for the first stage estimation error by bootstrapping the logit five hundred times in the

[26] We collect these data in November 2005 and assume that the relative time for flight travel has not changed substantially over our sample period.

[27] Using prior location as an instrument should not confound the selection of university with productivity. Consider the individual who undertakes a move with the intent either to change her own productivity or to better match her characteristics with a potential school. For such an individual, the prior location instrument has no predictive power on potential location.

Table 7

Determinants of faculty productivity, selection model.

Panel A presents the logit estimation of the probability of moving as in Eq. (6) The dependent variable is an indicator for a move. We estimate a fourth-order polynomial of the individual's age, calculated as the current year minus the undergraduate year plus 22. A likelihood ratio test (LRT) is given for testing joint relevance of the variables in estimating the probability of a move.

Panel B presents the conditional logit selection estimation of the probability of being at a given location as in Eq. (6). The dependent variable is an indicator variable for the faculty being at the potential school for each year. *Flight time* is the difference in flight times (expressed in hundreds of minutes) to the origin home city, defined as the city of the undergraduate institution, from the prior year location and potential location. *Prior location* is an indicator for the potential location being a different university than the university from the prior year. *Non-prior location* = 1–*prior location*, for convenience of interpretation. *Ph.D. School location* is an indicator for whether the potential school is the location of the individual Ph.D. degree. It is allowed to equal one only for individuals whose prior year school is a top five school and who have graduated from a Ph.D. program more than two years prior. *Productivity distance* is the difference between the individual's prior two year average productivity and the potential location's prior two year average productivity. For assistant and associate professors, we take the absolute value of this number, as described in the text. *Associate*, *full*, and *chaired* are rank indicator variables. Robust standard errors are clustered at the school level. A Wald test statistic is provided for testing joint relevance of the variables in the estimation.

Panel C and Panel D present the estimation results from the second stage of the selection model. The only difference in these estimations from those of Table 5 is that the contrast matrix setting up the university fixed effects estimation has been replaced by the predicted probability of each individual being at each location, estimated in the conditional logit in step 1. *Impact productivity* is the dependent variable and is measured as the count of *AER* equivalent pages written by each faculty in 41 economics and finance journals with each article multiplied by the decade impact factor of the journal published. Adjustment to *American Economic Review* (*AER*) equivalents normalizes by font, typesetting, and average article length. Publication pages are divided by $1/n$ coauthors. The 1990s decade includes 2000 and 2001. *Career years* is the years since earning a Ph.D. The model is fit with individual fixed effects. Standard errors are adjusted to account for the first-stage variance by bootstrapping the first stage, taking the variance of the predicted probabilities and adding the variance to the regression variance in the second stage following Petrin and Train (2003). In all panels, *t*-statistics are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A. First stage: selection to move

Independent variable	Dependent variable: indicator for moves
Age	7.29***
	(7.89)
Age^2	−0.23***
	(7.27)
Age^3 (/1,000)	3.23***
	(6.63)
Age^4 (/1,000,000)	−16.37***
	(6.04)
Constant	−84.83***
	(8.72)
Number of observations	35,993
Pseudo R^2	0.037
LRT test statistic for relevance	541.2

Panel B. First stage: selection among potential schools

Independent variable	Dependent variable: indicator for each faculty being at each of 26 locations
Flight time	0.223**
	(2.01)
Prior location	5.375***
	(27.58)
Ph.D. school location	1.122***
	(5.23)
Ph.D. school location * non-prior location	1.614***
	(9.89)
Productivity distance	−0.065***
	(3.16)
Productivity distance * associate	−0.021
	(0.59)
Productivity distance * full	−0.016
	(0.23)
Productivity distance * chaired	−0.118*
	(1.71)
Number of observations	816,624
Pseudo R^2	0.821
Wald test statistic for relevance	42,415
Robust standard errors clustered at school level	

Table 7 *(continued)*

Panel C. Determinants of faculty productivity: selection model

Independent variable	Dependent variable: impact productivity
Career years	−0.222***
	(4.99)
Career years^2	0.004*
	(1.94)
Career years^3	−0.048
	(1.32)
Associate professor	−0.425***
	(2.77)
Full professor	−0.651***
	(3.18)
Chaired full	−0.855***
	(3.55)
Editor impact	−0.348
	(0.94)
Visiting	0.022
	(0.16)
Decade 1980s	0.704***
	(2.96)
Decade 1990s	1.100***
	(3.65)
Finance * Decade1970s	−0.427
	(0.35)
Finance * Decade1980s	0.060
	(0.05)
Finance * Decade1990s	0.204
	(0.19)
Constant	4.969***
	(12.60)
Number of observations	29,754
Individual fixed effects	Yes

Panel D. University fixed effects across decades and field – selection model

	Dependent variable: impact productivity					
School	Economics departments			Finance departments		
	1970s	1980s	1990s	1970s	1980s	1990s
Carnegie Mellon Univ.	−34.81*	15.44	0.02	81.72***	−2.00	−20.78
	(1.74)	(1.46)	0.00	(2.64)	(0.12)	(1.18)
Columbia Univ.	49.82***	−5.59	5.28	36.54	5.50	−0.17
	(2.84)	(0.63)	(0.55)	(1.57)	(0.56)	(0.02)
Cornell Univ.	0.61	−3.88	6.30	−10.62	−32.49	−15.95
	(0.05)	(0.44)	(0.52)	(0.32)	(1.20)	(0.48)
Duke Univ.	−5.57	9.14	−5.29	21.82	27.62	23.29
	(0.40)	(0.86)	(0.32)	(0.56)	(1.39)	(1.27)
Harvard Univ.	81.69***	14.61**	−18.66**	−2.52	−21.89	−25.49
	(6.31)	(2.05)	(2.34)	(0.03)	(1.18)	(1.36)
Indiana Univ.	29.26	11.80	8.52	24.94	−11.25	3.69
	(1.44)	(0.97)	(0.49)	(0.60)	(0.65)	(0.17)
Massachusetts Institute of Technology	26.41***	32.51***	−1.51	19.83	24.42	−3.59
	(2.60)	(3.87)	(0.19)	(0.82)	(1.62)	(0.23)
New York Univ.	−17.31	−23.15**	−25.90**	5.91	2.06	23.25*
	(1.33)	(2.43)	(2.23)	(0.31)	(0.21)	(1.91)
Northwestern Univ.	6.71	14.31*	17.39*	12.07	7.73	−24.06**
	(0.56)	(1.79)	(1.88)	(0.59)	(−0.72)	(2.01)
Ohio State Univ.	50.32**	−15.68	−47.57*	31.27	6.80	−7.19
	(2.18)	(0.99)	(1.86)	(0.87)	(0.43)	(0.28)
Princeton Univ.	21.80**	30.98***	−14.22*	—	—	—
	(1.96)	(3.83)	(1.74)	—	—	—
Purdue Univ.	39.68	−27.50*	−29.12	−9.49	−3.03	−23.52
	(1.26)	(1.72)	(1.45)	(0.33)	(0.16)	(1.03)
Stanford Univ.	16.75	10.78	−10.62	11.68	30.47**	−2.08
	(1.45)	(1.39)	(1.00)	(0.52)	(2.27)	(0.12)
Univ. of British Columbia	15.93	−19.89*	−4.83	43.48	−4.12	−16.88
	(1.12)	(1.79)	(0.36)	(1.03)	(0.28)	(1.01)
Univ. of California, Berkeley	11.39	2.77	−13.31	−19.23	8.85	7.41
	(0.96)	(0.32)	(1.36)	(0.69)	(0.52)	(0.36)

Table 7 (*continued*)

Panel D. University fixed effects across decades and field – selection model

School	Dependent variable: impact productivity					
	Economics departments			Finance departments		
	1970s	1980s	1990s	1970s	1980s	1990s
Univ. of California, Los Angeles	−14.06	9.52	6.48	40.20	18.95	12.01
	(0.78)	(0.71)	(0.50)	(1.25)	(1.26)	(0.66)
Univ. of Chicago	1.98	4.72	−11.82	21.16	19.14**	−15.59
	(0.18)	(0.59)	(1.34)	(1.03)	(2.10)	(1.37)
Univ. of Michigan	4.96	27.73***	26.81**	−1.46	20.68*	3.25
	(0.48)	(2.93)	(2.47)	(0.04)	(1.73)	(0.27)
Univ. of Pennsylvania	25.36**	25.36***	1.90	13.29	−11.77	−1.02
	(2.17)	(3.07)	(0.21)	(0.89)	(1.31)	(0.10)
Univ. of Rochester	21.84	29.65**	−3.57	−29.69	−12.43	−5.16
	(1.27)	(2.42)	(0.29)	(1.30)	(1.03)	(0.34)
Univ. of Southern California	−10.97	−21.11	−17.36	8.64	14.12	−15.06
	(0.55)	(1.38)	(1.11)	(0.25)	(0.63)	(0.80)
Univ. of Texas, Austin	−19.79	−11.67	−16.52	−3.33	−3.34	6.60
	(0.76)	(0.69)	(1.03)	(0.17)	(0.25)	(0.31)
Univ. of Washington	−9.13	−8.44	−22.44	5.58	6.46	−15.09
	(0.57)	(0.57)	(1.25)	(0.27)	(0.42)	(0.63)
Univ. of Wisconsin	7.94	−6.58	−6.71	84.26	4.68	25.17
	(0.70)	(0.77)	(0.62)	(1.20)	(0.19)	(1.27)
Yale Univ.	2.92	−4.66	−12.01	144.10***	38.30**	−24.23
	(0.29)	(0.58)	(1.32)	(3.07)	(2.44)	(1.04)
Significant (+) count	6	7	2	2	4	1
Significant (−) count	1	2	4	0	0	1

first stage and adding the variance in the predicted probabilities to the regression variance in the second stage following Petrin and Train (2003). These estimates are reported in Table 7, Panel C.

As the table reports, the decade effect of *other* universities exhibits the same positive time trend as observed in the nonselection model. This result confirms our previous finding that selection stories cannot explain our results.

Panel D of Table 7, which lists the individual university fixed effects, also shows that the positive elite university fixed effects of the 1970s and 1980s disappeared in the 1990s. The selection model, with the bootstrapped standard error correction, increases the errors in estimation rendering many of the estimates insignificant relative to those in Table 5. However, the pattern of diminishing fixed effects for the elite schools is robust. For both economics and finance we observe a steady decline (increase) in the number of positive (negative) signs over the three decades. We conclude that the dissipation of the university fixed effects in the 1990s is robust to selection concerns.

5. Components of university fixed effects

Having established that university fixed effects existed, we attempt to explain what factors determine them and why they have disappeared. We are particularly interested in the roles of spillovers and cultural norms as determinants of these effects.

The factors driving the university fixed effects can be estimated in two ways. Method 1 takes the university fixed effects at the decade-field level θ_{fud} from Table 5, Panel B and decomposes them into observable determinants of department spillover potential, cultural norms, and university location factors:

$$\hat{\theta}_{fud} = \beta_{0f} + X_{fud}^{dept\ spillovers} \cdot \beta_{1f} + X_{fud}^{dept\ culture} \cdot \beta_{2f} + X_{u}^{location} \cdot \beta_{3f} + v_{fud}. \tag{7}$$

The advantage of this approach is in its ease of interpretability.

Method 2 inserts the same observable determinants into our original productivity estimating Eq. (5) and estimates the regression directly using the whole sample. Method 2 understates the standard errors because the regression is estimated at a person-year level, and most of the regressors change only at a university-decade level. One of the regressors, the spillover effect from colleagues, is an exception in that it varies by person-year. Therefore, Method 2 is more efficient for estimating the impact of the quality of colleagues using the whole sample. The spillover effect changes for each person-year because the average productivity of each person's colleagues is different within the same department and a fortiori over time (e.g., Moretti, 2004). Therefore, to estimate spillover effects, we use Method 2. To estimate the effect of the other observable determinants of the university fixed effect, we use Method 1.

The quality of colleagues can generate positive spillovers through the comments on colleagues' work

(Laband and Tollison, 2000) and spillovers from the expertise of star faculty (Goyal, Van Der Leij, and Moraga, 2006; Azoulay and Zivin, 2006). We examine whether this spillover effect sustains over time. As a measure of colleagues' quality we use team, the two-year moving average of productivity of all productive members of the department in each year.[28] We lag the variable one year to remove simultaneity with the productivity dependent variable. We allow the coefficient on *team* to vary by decade to test whether the spillover changes over time. We use the same method as Eq. (5), with *impact productivity* as the dependent variable and include individual fixed effects, except that, instead of including university fixed effects, we include university random effects. By using university random effects, we can identify a *team* effect, part of whose variation would otherwise be subsumed in the university fixed effects.

The results are presented in Table 8. The quality of colleagues has a positive and significant effect on productivity during the 1970s and the 1980s, but the effect turns negative in the 1990s. A possible explanation for the negative effect is that being surrounded by highly productive teammates could make a faculty member overly cautious and thus less productive. Such an effect might have existed in the 1970s and 1980s but was overwhelmed by positive spillover effects.

One potential problem with this estimation procedure is that no direct measure exists for the quality of *team* at an *others* university. In Table 8 we assign the lowest quality team among the top 25 universities as team for *others*. In additional tests, we interact the *team* of *others* with a dummy to allow its coefficient to differ. Neither of these approaches is fully satisfactory. Hence, we test the robustness of the team effect by reestimating the same regression while restricting the sample to faculty members who have always been at a top 25 university. The results (not reported) are substantially the same.

These results on team effects are consistent with the central results of the paper; namely, the spillover emanating from superior colleagues either disappeared or is diffused beyond the restricted boundaries of the home institution, e.g., to teammates at other universities. If the team effect is solely responsible for the elite university fixed effect, the inclusion of the team variable would eliminate all the positive university fixed effects presented in Table 5, Panel B. Although not reported in a table, we find that, with the *team* variable, 17 (18) of the university fixed effect coefficients are positive and significant for economics (finance) in the 1970s and only two (four) are positive and significant for the 1990s.[29] It is possible that our measure of the colleague spillover is not properly captured by *team*. A more likely explanation, however, is that the effect of highly productive colleagues alone does not fully explain the decrease in the elite university effect.

Table 8

Determinants of faculty productivity and team externality.

Only two differences distinguish the regression from Column 2 of Table 5, Panel A. First, included is the variable *team*, calculated as the prior two-year average productivity of all colleagues in one's department who are productive for the years considered. Second, all columns include individual fixed effects and university random effects. As before, observations are at the individual-year level. *Impact productivity* is measured as the count of *American Economic Review* (*AER*) equivalent pages written by each faculty in 41 economics and finance journals, normalized by font, typesetting and average article length, divided by $1/n$ coauthors and multiplied by the decade impact factor of the journal. *Career years* is the years since earning a Ph.D. *Associate professor*, *full professor*, *chaired*, and *visiting* are indicator variables for the position of the faculty. *Editor impact* is the sum of the impact factors for the journals for which the faculty serves as an editor or coeditor. The 1990s decade includes 2000 and 2001. *t*-Statistics are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Independent variable	Dependent variable: impact productivity
Career years	−0.005
	(0.13)
Career years^2	−0.006***
	(3.27)
Career years^3 (in thousands)	0.117***
	(3.42)
Associate professor	−0.501***
	(3.09)
Full professor	−0.914***
	(4.06)
Chaired full professor	−1.169***
	(4.90)
Editor impact	0.004
	0.00
Visiting	0.043
	(0.56)
Decade 1980s	0.310**
	(2.45)
Decade 1990s	0.993***
	(4.02)
Finance * Decade1970s	−1.024*
	(1.78)
Finance * Decade1980s	−0.883
	(1.53)
Finance * Decade1990s	−0.701
	(1.21)
Team * Decade1970s	0.160**
	(2.76)
Team * Decade1980s	0.119***
	(3.60)
Team * Decade1990s	−0.131***
	(2.94)
Constant	4.342***
	(11.64)
Number of observations	35,917
Individual fixed effects	Yes
University random effects	Yes

To capture the role of alternative variables in the unexplained portion of the university fixed effects, we take the residual university fixed effects after removing the *team* effect and decompose them on the basis of university characteristics using Eq. (7).

The first characteristic is whether a university hosts journal editors or houses a journal. On the one hand, editors could share their know-how on crafting papers for publication with their colleagues, and having an in-house

[28] We define a person who has not published in the current or prior year as nonproductive and exclude these individuals in our measure of *team* because later we capture the culture effect through these nonproductive faculty members.

[29] Although we employ university random effects to identify the team effect, we switch to a fixed effects specification to identify the university fixed effect above and beyond the control for team.

editor could favor the editor's colleagues, whose papers might be accepted more easily (e.g., Laband and Piette, 1994). On the other hand, having in-house editors could have a negative effect if editors delegate more refereeing to colleagues, taking their time away from the production of papers. To explore how these potential impacts play out, we define *editors-in-house* to be the natural log of one plus the average number of editors during each decade for the department. We use the natural log because editorship is clustered in a few key schools and the incremental impact could be decreasing.

Another potential source of spillovers is the average quality of colleagues' training. *Faculty training quality* is measured as the percentage of faculty at a department whose Ph.D. is obtained from a top five university, where the top five designation is made by ranking average productivity separately for finance and economics by decade.

In addition to externalities captured by *team*, *editors-in-house*, and *faculty training quality*, cultural norms could play a role in the university fixed effects. For instance, in elite schools peer pressure to perform might be very strong.[30] Our primary measure of cultural norms is the presence of non-researching colleagues, referred to as *nonresearch/service* faculty (Our definition of research is that which appears in top economics and finance journals only.) Nonresearch faculty could have a negative impact on the department's research output by setting an example or diverting new colleague attention and school resources to nonresearch related activities, or both. We define *nonresearch/service* as the percent of faculty who have no publication for two years in a row. Admittedly, this definition is harsh, because the lumpy nature of the publication process might cause very productive faculty to be classified occasionally as *nonresearch/service*.

One could argue that *nonresearch/service* and team capture the same characteristic of a department, with opposite signs. In fact, the correlation of *nonresearch/service* and *team* at the yearly observation level is relatively low at 0.21. If high team values are driven by a few stars, the relation between *team* and *nonresearch/service* would not be automatic. In additional, *nonresearch/service* captures the negative research environment that is not measured by a low level of *team* (a low average productivity among productive colleagues).

A second measure of cultural norms within the department is measured by the quality of the Ph.D. program. Presumably, a vibrant and successful Ph.D. program reflects the research intensity of the department. However, faculty's research time also could be absorbed by too much interaction with Ph.D. students. We measure *Ph.D. program quality* with the decade average count of students who are hired into in the top 25 schools.

We also control for whether a university is a *state school*. The public or private nature of the institution might affect financial resources and how they are utilized.

Finally, we want to consider the impact of two environmental variables: the weather and the distance to the closest metropolitan city. The former could influence productivity by affecting the opportunity cost of sitting in the office. We measure weather by the average annual *snowfall* from weather.com. *Distance metro* could affect both the consulting and the distraction opportunities, but, at the same time, it could help stimulate intellectual curiosity by exposing faculty to topics of current interest in a timely manner. We measure *distance metro* as the natural log of miles to the closest city with more than three-quarters of a million people as measured on mapquest.com.

We are interested not only in showing cross-sectional associations between the university fixed effects and characteristics of the departments, but also in understanding the negative trend in university fixed effects identified in Panel B of Table 5. We examine whether this negative trend can be explained by changes in university characteristics over time. Furthermore, the relation between the university fixed effects and certain university characteristics could change over time. We test for this possibility by interacting the most important university characteristics with decade dummies. Our focus on the trending aspect of the university fixed effects heightens the concern that an omitted trending factor might bias our coefficients. A Breusch-Pagan test confirms this concern.[31] Ordinarily, we would add decade fixed effects to resolve this issue. But many of our university characteristics vary only a little over time, so it is difficult to identify both decade fixed effects and university characteristics interacted with a decade dummy. Thus, instead of fitting a model with decade fixed effects, Table 9 presents the results with decade random effects. This is without loss of generality. A Hausman test fails to reject the equality between fixed effects and random effects estimates.[32]

Because the nature of publication productivity might not be the same across fields, we split the decomposition of the university fixed effects into economics (Table 9, Columns 1 and 2) and finance (Columns 3 and 4). Columns 1 and 3 in Table 9 report that *nonresearch/service* has a strong and significant negative effect on the university fixed effect. At the bottom of the table, a partial R^2 analysis shows that excluding *nonresearch/service* from the estimating equation lowers the percentage of variation explained by 6% (from 21%) in economics and by 29% (from 42%) in finance. These results indicate that norms play a role in organizational productivity. The magnitude of the coefficients, significance, and partial R^2 are greater in finance than in economics. A possible explanation for this difference is that finance consulting and executive teaching opportunities tend to be more financially rewarding than the policy-related consulting opportunities for most economics faculty, making the *nonresearch/service* effect more contagious in finance.

[30] Sacerdote (2001), for example, finds evidence of peer effects on individual outcomes using student living arrangements and subsequent grade performance at Dartmouth University. Hoxby (2000) and Angrist and Lang (2004) study peer effects within the classroom.

[31] The test rejects no variance across decades with a $\chi^2(1) = 4.72$ corresponding to a p-value of 0.030.

[32] The test fails to reject equivalence of the fixed effects and random effects coefficients with a $\chi^2(8) = 6.74$ corresponding to a p-value of 0.556.

Table 9
Decomposition of university fixed effects.
The dependent variable is the field-decade university fixed effects from the estimation of Table 8 in which the *team* externality effect has been removed. *Nonresearch/service* is the decade average of each year's percentage of faculty who have not published an article in the prior two years. *Editors in-house* is the natural logarithm of the count of editors housed by the department for a year, averaged over the decade. *Faculty training quality* is the decade average percentage of faculty trained in top five schools, where the top five schools have the highest average productivity in the field for the decade. *Ph.D. program quality* is average number of students placed in top 25 universities from that department. *Distance metro* is the natural log of the distance to the closest metropolitan area. State school is an indicator of whether the university is a public institution. *Snowfall* is the average snowfall in January for the university location. The 1990s decade includes 2000 and 2001. *t*-Statistics are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

Independent variable	Dependent variable: University fixed effects estimates			
	Economics		Finance	
Editors in-house	−0.109		−0.273**	
	(1.31)		(2.32)	
Editors in-house * Decade 1970s		0.422***		0.142
		(3.66)		(0.48)
Editors in-house * Decade 1980s		0.193*		−0.079
		(1.84)		(0.49)
Editors in-house * Decade 1990s		−0.106		−0.367**
		(1.03)		(2.49)
Faculty training quality	1.384*	0.925	−0.955	−0.943
	(1.91)	(1.58)	(1.50)	(1.48)
Nonresearch/service	−1.975**		−3.404***	
	(2.26)		(5.66)	
Nonresearch/service * 1970s Decade		−0.776		−2.694***
		(1.01)		(4.05)
Nonresearch/service * 1980s Decade		−1.242		−3.739***
		(1.59)		(4.89)
Nonresearch/service * 1990s Decade		−1.299		−3.499***
		(1.49)		(5.18)
Ph.D. program quality	−0.083	−0.207*	0.195	0.326
	(0.59)	(1.77)	(0.74)	(1.26)
Distance metro	0.052	0.023	0.083	0.082
	(0.95)	(0.52)	(1.20)	(1.21)
State school	−0.079	0.138	−0.179	−0.061
	(0.36)	(0.77)	(0.65)	(0.23)
Snowfall	0.001	0.004	−0.010	−0.012*
	(0.11)	(1.02)	(1.53)	(1.90)
Constant	0.557	−0.008	2.337***	2.196***
	(0.98)	(0.02)	(5.09)	(4.85)
Number of observations	75	75	72	72
Decade random effects	Yes	Yes	Yes	Yes
Partial R^2- (Type III SSE)				
Model (R^2)	0.21	0.53	0.42	0.50
Nonresearch/service	0.06	0.02	0.29	0.27
Editors	0.02	0.16	0.05	0.06
Training of faculty	0.04	0.02	0.02	0.02
Ph.D. program	0.01	0.02	0.00	0.01
Metro distance	0.01	0.00	0.01	0.01
State school	0.00	0.00	0.00	0.00
Snow	0.00	0.00	0.02	0.03
Both non-research and editors	0.08	0.40	0.36	0.44

The existence of in-house editors has a negative sign on the productivity of colleagues in economics and finance but is significant only for finance. The negative aspects of in-house editorship seem to outweigh the positive ones. The net effect is stronger for finance, perhaps because the burden of having an in-house editor is shared by a smaller number of colleagues in finance than in economics.

The percentage of faculty with Ph.D. degrees from top five universities is positively significant for economics and insignificant for finance. The *Ph.D. program quality* seems to impose more burdens than to provide benefits, but only for economics departments. However, our productivity measures do not capture the full benefits associated with a high quality doctoral program, such as enhanced reputation of the department, which helps attract top researchers, and a better research network stemming from placing students in top schools. Furthermore, these variables do not explain much of the variation across departments. Therefore, no definitive inferences can be drawn from these results.

Neither proximity to metropolitan areas nor being a state school seems to have an impact on the university fixed effect. Surprisingly, *snowfall* is also insignificant. We expected a positive coefficient in that good weather might make leisure relatively more attractive. The result does not change when we replace *snowfall* with average January temperatures.

In Columns 2 and 4, we allow the effect of *nonresearch/service* and *editors* to vary over decades. The effect of *nonresearch/service* is stable over time, in both economics and finance. The stability of cultural norms suggests that, unlike the spillover from productive colleagues, the cultural aspects of institutions do not easily dissipate with technological change. In academia, the tenure system could contribute to endemic cultural norms.

The spillover impact of editorship seems to decline over the three decades, as does that of having productive colleagues. In economics departments (Column 2), the effect of *editors-in-house* begins as positive in the 1970s and becomes insignificant in the 1990s. In finance departments (Column 4), *editors-in-house* begins as insignificant in the 1970s and turns negative in the 1990s. Both cases suggest a reduction in the positive spillover.

In an unreported regression we repeat the same exercise using as a left hand side the university fixed effects derived using our citation measure. The results are substantially the same. For economics, the impact of nonproductive colleagues remains negative and significant and stable over time. The effect of an in-house editor is positive and significant only in the 1970s. For finance, the impact of nonproductive colleagues remains negative, large, significant, and stable over time. The in-house editor variable is never significant.

Our final check is whether our results are affected by a shift in the number of elite universities between 1970 and 2000. It is possible that in the 1970s there was a big discontinuity in the quality of the department just below the top 25 universities and that the position of this discontinuity had moved down the ranking ladder in the 1990s. If most of the faculty moving away from the top 25 departments move to departments slightly lower in quality, this catching-up hypothesis could explain the relative improvement in the fixed effects of *others* universities vis-à-vis the top 25.

Several facts are inconsistent with this interpretation, however. First, the catching-up story cannot explain why the spillover effect of better colleagues decreases over time. The reduction in the spillovers persists even when we exclude *others* universities and restrict our analysis only to the top 25 universities.

Second, if it were true that *others* schools have improved dramatically vis-à-vis the top 25, we should observe an increase in the average productivity of *others* relative to the top 25. We find the opposite in Table 4.

Finally, not all the 25 universities could qualify as elite institutions. Thus, we repeat our analyses for the top five and the top ten universities only. The key results do not change, confirming that the choice of the cutoff point at the 25 is not driving the results.

6. Causes and consequences of disappearing university fixed effects

We show that the favorable impact on productivity of working at elite universities diminishes after the 1970s. We also show that the importance of colleague externalities vanishes in the 1990s, while the influence of cultural norms remains persistently strong.

6.1. Diffusion and technology advancements

To what extent is the progress in communication technology responsible for these trends? Technological advancement can facilitate communication and transfer of ideas among coauthors at a distance. It can also facilitate access to knowledge itself, particularly for universities far from the forefront of research. But can we show the magnitude of this effect in practice?

The reduction in the impact of higher quality colleagues is consistent with the reduction of geographical boundaries through better communication technology. Several papers (Laband and Tollison, 2000; Hamermesh and Oster, 2002; Rosenblat and Mobius, 2004; Goyal, Van Der Leij, and Moraga, 2006; Azoulay and Zivin, 2006) show dramatic increases in coauthoring at a distance in the latter part of the 20th century. In particular, Goyal, Van Der Leij and Moraga show that the proportion of the economics profession networked through coauthorship has increased from nearly 20% in the 1970s to 50% in the 1990s. Our additional contribution to this literature is to show that most of the increases in coauthorship took place between elite universities and nonelite ones.

We take the 25,010 author-article observations from the 41 journals with at least one author residing at a top 25 school and weigh each observation with the inverse of the number of authors so that articles have equal weight. The articles are divided into four types: $S=$ solo articles, $X=$ coauthored articles with all coauthors within the same school, $Y=$ coauthored articles with at least one coauthor not from the same school but all from a top 25 school, and $Z=$ coauthored articles with at least one coauthor outside the top 25 schools.

Fig. 2 plots the percentage of the four types of papers in our data set. The results are striking. The percentage of multiple-author papers with authors in both elite and nonelite schools steadily increases from about 32% in the beginning of the 1970s to nearly double that amount (61%) by 2004. Papers coauthored with outside authors within the elite schools also increased from 3% to 9%. These increases contrast sharply with the steady decline in solo authored papers (from 66% to 24%) and essentially no change for inhouse coauthored papers (hovering around 6%).

This evidence is consistent with Internet and communication technology advancements making access at a distance easier, which disproportionately favors nonelite universities. However, earlier advancements in other communication delivery systems, such as overnight mail service and fax machines could explain the gradual increase in coauthorship instead of a sudden increase that might be expected if the Internet were the only technological advancement adopted by everyone at the same time.

To investigate whether the sharp increase in participation by scholars in nonelite schools is due to an increase in

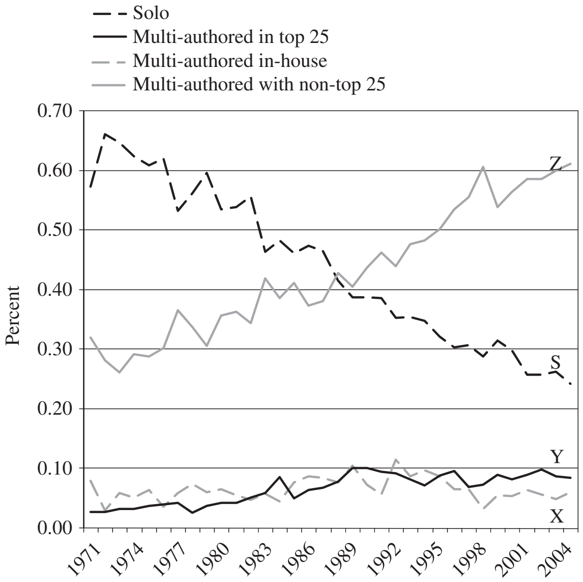

Fig. 2. Percentage of articles by type of authorship, 1971–2004. For all articles published in the top 41 journals with at least one author residing at a top 25 school, presented are the percentages of articles falling into one of four types: S = solo articles (black dotted line), X = coauthored articles with all coauthors within the same school (black solid line), Y = coauthored articles with at least one outside coauthor but within the top 25 schools (gray dotted line), and Z = coauthored articles with at least one outside coauthor outside of the top 25 schools (gray solid line).

the publication market share of *others* schools, we compare the percentage of all articles published in the 41 journals with at least one author affiliated with the top 25 schools with the percentage of articles written exclusively by authors in *others* schools in our data set. No evidence emerges of a change in market share between elite and nonelite schools. The percentage of articles with top school participation oscillates between 19% and 27%, without a discernable trend, and the participation by exclusively nonelite school scholars oscillates between 73% and 81%. A similar lack of pattern holds when we look at market share weighted by the impact of journals in which the articles were published.

In sum, the available evidence on coauthorship suggests that the reduction in the university fixed effect is due to a diffusion of the spillover produced by better colleagues beyond the physical limits of a university.

6.2. Impact on salaries

Why does it matter that physical location affects productivity? The diminishing benefits from physical location have many important implications concerning jobs, outsourcing, migration, regulation, competition policy, education, and so on. In this section, we focus on its implication to wages, specifically, on the ability to appropriate the spillover. If the spillover generated by a better faculty is concentrated within the physical boundaries of a university, the university can capture some of it. If a faculty member's productivity benefits tremendously from being at a top department, she might be willing to accept a lower salary to benefit from this spillover. If that spillover diminishes, so should the discount in salary. Hence, universities with reductions in their fixed effect should experience higher increases in salaries. That is, faculty salary should be negatively correlated with changes in the university fixed effects.

Although we are unable to obtain time series data for economics and finance professors' salaries, the National Center of Education Statistics of the US Department of Education conducts faculty salary surveys for all the faculties in US colleges and universities on a yearly or biennial basis. The data are compiled into the Higher Education General Information Survey (HEGIS) series for 1968–1986 and the Integrated Postsecondary Education Data System (IPEDS) series for 1987–2000. The surveys collect salary information by gender, position, and contract length (nine- or ten-month versus full-year contracts) for faculty in all fields. For comparability across time, we take the average salary across gender for nine- or ten-month contracts of assistant, associate, and full professors.

While we do not expect that all departments are affected in the same way by communication technology changes, such changes are likely to affect the spillovers in the production process of research for a substantial portion of the university faculty (see Agrawal and Goldfarb, 2008 for the effect on engineering departments). Hence, we examine the relation between salaries and the university fixed effects. To this end, we regress the change in salary (by rank) at each of the 25 elite institutions over the three decades on the change in the university fixed effects for economics faculty. We do not include finance because finance faculty enjoyed unusually large salary increases over the past three decades, and business schools often have separate budgets that could lead to distinct salary structures.

To equilibrate the quality of the faculty input, we control for the change in the average individual productivity, proxied by the decade-department average of each individual's fixed effects. We fit the estimation in a seemingly unrelated regression (SUR) framework across rank status to account for outside relations of errors among the ranks.

Table 10 reports, as predicted, that changes in the university fixed effects have a negative and statistically significant effect on salary.[33] This relation is robust across ranks. The results suggest that, for each *AER* impact page decline in the university fixed effects, assistant professors' salaries increase by $1,386; associate professors' by $1,750; and full professors' by $2,917.[34]

In sum, the salary evidence is consistent with leading universities becoming relatively less attractive places to work in the last three decades. This salary effect appears

[33] The observation count of 94 is smaller than the combination of having three ranks, 25 schools, and two changes across time because of missing data for Canada and some missing 1970s observations in HEGIS.

[34] The monotonic decline in R^2s, from 6% for assistant professors to 1% for full professors, is due partly to the much larger variance in salaries for higher ranked faculty.

Table 10
Differences in salaries seemingly unrelated regression estimation.

Salaries are the decade average university nine or ten month salaries as collected in the National Center of Education Statistics (Higher Education General Information Survey and Integrated Postsecondary Education Data System series) yearly survey broken down by assistant, associate, and full professorship. *F.E.Impact* is the estimated university fixed effects for economics departments by decade. *F.E.Individual* is the estimated individual fixed effects for economics departments by decade. Differences are taken at the decade level. The difference estimations are fitted using a seemingly unrelated regression to capture cross-equation correlation in the errors. *Z*-statistics are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. The estimating equation is given by

$$\Delta Salary_t = \beta_0 + \beta_1 \Delta F.E.Impact_{it} + \beta_2 \Delta F.E.Individual_{it} + \varepsilon_{it}.$$

	Coefficient	*Z*-statistic
Assistant professors		
ΔF.E.Impact	−1,385.8**	(2.23)
ΔF.E.Individual	267.4	(0.51)
Constant	18,739.5***	(28.44)
R^2	0.059	
Number of observations	94	
Associate professors		
ΔF.E.Impact	−1,749.5***	(2.45)
ΔF.E.Individual	262.1	(0.44)
Constant	21,201.7***	(28.01)
R^2	0.032	
Number of observations	94	
Full professors		
ΔF.E.Impact	−2,916.5***	(2.66)
ΔF.E.Individual	902.4	(0.98)
Constant	31,750.8***	(27.26)
R^2	0.091	
Number of observations	94	

to be driven, at least in part, by a reduction in the university fixed effect.

6.3. Agglomeration in top universities

With the diminishing university effects, how do the elite universities maintain their ability to attract highly productive faculty as Table 4 shows? More generally, how can we reconcile our evidence with the growing evidence coming from the economic geography literature that service industries tend to become more geographically concentrated (Black and Henderson, 1999) and highly educated cities tend to become even more highly educated because they attract an increasing number of highly educated people (Glaeser and Berry, 2005)?

Looking more closely at who is agglomerating with whom can help. Glaeser (2000) argues that nonmarket interactions are important components in determining location choices. Our personal observation also suggests that faculty's location choices are not based solely on the highest monetary return (current salary plus the present value of future salary increases correlated with future productivity). Nonmarket factors seem to affect the location choice. For example, the prestige of a university, department reputation, and the quality of the students she has to teach could play a role in the decision making.

To test whether these potential reasons for agglomeration helped elite universities sustain their ability to attract the highly productive faculty, we relate the yearly average productivity levels of the 25 elite universities to reputation and prestige features unrelated to the individual production process of research. The prestige factor, the *halo* effect, is proxied with the undergraduate rankings by the *Gourman's Guide to Four Year Colleges*. We define the variable such that the university with the top ranking is assigned a value of 25, the second ranked a value of 24, and so on. *Reputation* is measured by the department's past average impact productivity ten years prior. We take the ten-year lag so that our reputation variable omits the current university fixed effect. The ten-year lag also conveniently takes out the 1970s, the period in which we show significant university fixed effects.

The decade fixed effects regression shows that the average productivity of a department is significantly positively related to both the *halo* effect and the department's *reputation* based on its past research[35]:

$$AveProd_{it} = \underset{[3.30]}{0.043} \cdot Halo + \underset{[9.22]}{0.311} \cdot \text{Reputation} + \text{Decade Fixed Effects}, \quad R^2 = 0.11, \tag{8}$$

where *t*-statistics are reported inside the brackets. Apparently, universities with highly ranked undergraduate programs enjoy a *halo* effect in recruiting and retaining top researchers. Departments with good past research records do, too, sustaining agglomeration of top researchers in elite universities even after spillover effects have dissipated.

7. Conclusion

In this paper we study the nature and the magnitude of production spillovers in research activities at elite North American universities. We find strong positive spillovers emanating from high quality colleagues during the 1970s. This effect disappeared in the 1990s. Advances in communication technology appear to be an important factor behind this change. These advances diminish the importance of cooperation within physical boundaries and greatly facilitate collaboration from a distance, as witnessed by a substantial increase in coauthorship between scholars at elite and nonelite universities.

Our results suggest that elite universities are indeed losing their competitive edge in terms of their ability to boost faculty research productivity. Nevertheless, elite universities still enjoy an edge in average productivity, because top researchers agglomerate in institutions with prestigious undergraduate programs and in departments

[35] The undergraduate program rankings are interpolated for missing data. Although the rankings in *Gourman's* guides began in 1967, they were sparse, with a second appearance in 1977, and thereafter every two to three years until 1993. The final *Gourman* guide was in 1998, three years before the end of our sample period.

with high past research reputations. Such agglomeration could be due to the utility and the prestige of co-location with other creative minds. The perception that affiliation with top ranked departments enhances the visibility and credibility of their research output also could have helped elite universities attract productive researchers.

In the process of documenting these results, we uncover patterns in research activities with implications for university administrators. The percentage of faculty members not producing journal research, a proxy for an organizational culture engendering alternative goals, has a strong and persistent negative impact on the productivity of other faculty members. The influence of nonresearch colleagues is analogous to the effect of bad apples in the education literature (Hoxby, 2000).

We also find that faculty research productivity reaches its peak well before tenure and that age and higher ranks are associated with substantially lower research productivity. Part of the age and rank factor, however, could be due to our inability to capture indirect contributions. Older and higher ranked faculty might produce in other ways such as assuming greater administrative responsibilities, thereby freeing up the time of younger colleagues. They also could devote more time to writing books and cases and mentoring junior faculty and doctoral students, all which are excluded from our measure of research productivity.

The dissipation of university effects also has implications for knowledge-based industries. An increasing fraction of production in developed countries consists of research-like products. If, as our evidence suggests, the physical location for creation of these products is less important today, the nature of these firms and the way they should be managed are fundamentally changed. When physical proximity is required for spillover, firms exercise authority over their employees by controlling access to the physical premises (Rajan and Zingales, 1998, 2001). In this context, our findings suggest that the boundaries of firms are becoming fuzzier in knowledge-based industries. As a consequence, appropriating returns to investment in research and development become more difficult.

The implication of fuzzier firm boundaries goes beyond industries that specialize in research-like products. Increasingly, back-office operations are outsourced overseas. Industries that can provide service over the Internet (e.g., real estate brokerage) are breaking out of traditional modes of operation. Our results suggest that these innovations are only the tip of the iceberg. In the not-so-distant future, many industries could find little need for any location anchors.

However, forces exist that might slow down the pace of this transformation. First, economies of scale make physical assets important. Second, nonmarket interactions sustain agglomeration. As a result, while advances in communication technology threaten the competitive advantage of universities, firms, cities, and countries leading in research and technology, the nonmarket benefits of residing in those locations could help attract and retain the most productive members of society within their physical boundaries, as the most productive faculty continue being located at the most prestigious universities.

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