Peer Effects in Academic Research: Senders and Receivers

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Discussion Paper

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Abstract

Using an instrument based on a national contest in France determining researchers' location, we find evidence of peer effects in academia, when focusing on precise groups of senders (producing the spillovers) and receivers (benefiting from the spillovers), defined based on field of specialisation, gender and age. These peer effects are shown to exist even outside formal co-authorship relationships. Furthermore, the match between the characteristics of senders and receivers plays a critical role. In particular, men benefit a lot from peer effects provided by men, while all other types of gender combinations produce spillovers twice as small.

JEL codes: I23, J16, J24

Keywords: economics of science, peer effects, research productivity, gender publication gap

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1 Introduction

The production of academic knowledge seems to be organized so as to exploit peer effects: Researchers are spatially clustered in academic departments and interact in conferences and seminars. In this process, peers may play a direct role as co-authors but they can also provide indirect benefits, by helping in the production/publication process, or by acting as role models. The design of academic institutions requires, however, a precise knowledge of both the size and nature of such peer effects but also the extent to which they could be heterogeneous across groups.

In this paper, exploiting a natural experiment that quasi-randomly allocates new peers to departments, we first provide causal evidence of the existence of peer effects. We show that peers provide indirect benefits over and above joint production through co-authorship. Furthermore, we show that these peer effects critically depend on the characteristics of both senders (those who produce the peer effects) and receivers (those who benefit from them). We show that senders are peers working in the same specific field of research (JEL code in our application) as receivers. Moreover, the match between the characteristics of senders and receivers plays a critical role, in particular their gender and age. Women benefit much less from peer effects provided by men, and senior researchers mostly benefit junior colleagues.

Our identification strategy uses the particular (and peculiar) promotion system from assistant to full professorship for economists in the public university system in France.[1] The recruitment procedure under study consists of a centralized contest to fill a number of positions opened in different universities. Candidates are ranked after a long examination spanning over a six-month period. Successful candidates then sequentially make their choice according to their ranking. Universities cannot at this stage refuse a candidate. We observe the full choice set of positions offered together with the chosen one by candidates. Our empirical analysis show that the only significant factor determining choices of candidates is

[1] This system, restricted to a set of disciplines historically related to law and political science, was essentially abandoned in 2015 (after the end of our data) for economics.

the geographic distance from the university of origin. In particular, the average quality of the university in the field of specialisation of the individual does not seem to play any role. As a consequence, reverse causality due to endogenous location choices does not appear to be a major concern. We also mitigate the issue of non-random spatial sorting by the use of individual and department fixed effects. The particular organization of this allocation procedure allows us to go further and design an even more stringent identification strategy. We can restrict attention to the arrival in an university of professors ranked among the last ten in the contest. Those have a very much restricted choice set (the last ten universities that have not been chosen by a better ranked candidate). The idea is that the specialisation of the new professor who lands in the department, and hence her productivity in the field, creates a variation in peers' productivity which can be considered as good as random. Those arrivals are thus used as an instrument for peers' productivity in the field of the university in which they are allocated. In a robustness check, we further restrict the instruments to those new professors ranked among the last ten and moving further away than 250 km from their previous position place which limits their possibility to interact with their past university.

Using this identification strategy, we find that peer effects remain elusive, when defining the relevant peer group as the entire department. However, we show that *in a given JEL code and year*, one more publication by other members of the department increases one's productivity by a substantial range of 0.3 to 0.6 publications. The set of relevant senders are thus peers in the same field of study. Furthermore we show that the characteristics of the receivers matter as well. We show that women receivers benefit less from spillovers and so do older researchers.

As highlighted above, peer effects could encompass both direct spillovers from peers co-authoring papers, or indirect spillovers (for instance scientific or administrative help, role models). We thus distinguish the effect on papers without any peers as co-authors from papers either single-authored or co-authored with at least one peer. The largest share of the peer effects is driven by an increase in co-authored publications without peers. This demonstrates that peers matter, not only as co-authors, but also as providers of "indirect" spillovers.

Our paper also provides novel evidence on the key role played by the characteristics of the senders and receivers of peer effects. We show that senior researchers provide larger spillovers, and those peer effects benefit mostly junior researchers, highlighting the importance of specific matches between senders and receivers. The important role of gender is maybe more surprising. Men benefit a lot from peer effects provided by men, while all other types of matches produces smaller spillovers. If male peers increase on average their level of publication by 1 paper, men receivers increase their number of single-author publications by 0.289, while the effect is about twice as small if the receiver is a woman. This is the main driver for the result that women on average receive lower peer effects.

Borjas and Doran (2015) differentiate three dimensions along which peers can create spillovers, namely ideas, geographic and collaboration space. The authors illustrate these concepts in the empirical application they consider: When some Soviet mathematicians left for the US, those who remained lost peers "who were close to them in idea space (i.e., working on the same topics), other mathematicians lost peers who were close to them in geographic space (i.e., worked in the same university department), and still others lost peers who were close to them in collaboration space (i.e., they had been co-authors prior to the collapse)."[2]

Most of the literature in the economics of science has shown the existence of spillovers in the collaboration space. In particular, several papers (Azoulay et al., 2010; Oettl, 2012; Jaravel et al., 2018) exploit the unexpected deaths of scientists to estimate the causal effect on the productivity of their co-authors or collaborators. Azoulay et al. (2010) find a strong effect following the death of star scientists, while Oettl (2012) qualifies this result by showing that the effect is restricted to helpful scientists (i.e. those acknowledged in several papers per year). Jaravel et al. (2018), using patent data, show that the effect is not restricted to the stars. Borjas and Doran (2015), after introducing the terminology, show evidence

[2]Using a natural experiment driving the location of labs on the Jussieu campus of Paris, Catalini (2018) highlights that being in the same geographic space (labs co-location) increases the likelihood of collaboration.

of spillovers in the collaboration space, but no evidence of peer effects in the geographical space, i.e. peers located in the same university and not directly collaborating.

The literature generally finds much weaker evidence for spillovers in the geographical space. Waldinger (2012) shows that the scientists whose departments suffered losses during the period from 1925 to 1938 did not publish less or worse compared to other scientists. Similarly, Borjas and Doran (2012) show a negative effect of the influx of Soviet Union mathematicians on the productivity of American mathematicians, due to competition for scarce resources, but no effect on overall productivity.[3] Similarly, Jaravel et al. (2018), using the death of scientists, show no effect on co-workers who are not collaborators and no effect on second degree connections.[4]

In our paper we show that, in order to find peer effects in the geographical space, one needs to restrict the set of relevant peers, in particular in terms of field of specialisation, age and gender. This effect is present even when we exclude collaborations (co-authored papers), and is thus not fully driven by spillovers in the collaboration space. We note several important differences in our setting, compared to most of the papers mentioned above. First, rather than observing the breaking-up of relations between peers (deaths or departures), we measure the effect of the arrival of new peers. Most importantly, a key focus of our paper is on the heterogeneity of spillovers based on characteristics of senders and receivers. To the best of our knowledge, heterogeneity in peer effects in academia has received little attention, except for heterogeneity in terms of field of research.

Gender-specific peer effects have been studied extensively in a connected literature on education. Results are somewhat mixed. Ficano (2012) shows, for college academic outcomes, that the peer effects are characterized by a strong own-gender pattern. In particular,

[3]Without the use of natural experiments, a prior literature finds weak evidence of peer effects (Dubois et al., 2014; Kim et al., 2009). Waldinger (2010), using the same identification strategy based on the dismissal of Jewish professors in pre-war Germany, finds a negative effect on the career path of their PhD students.

[4]There is also a literature focusing on the role of the specific network structure. Ductor et al. (2014) show that incorporating detailed information on the co-author network improves the accuracy of predictions of future productivity. Head et al. (2019) show that ties such as having done the PhD in the same institution or sharing advisors matter for knowledge flows.

male peers influence male students while females are unresponsive to either male or female average academic results, which echoes our results.[5] Regarding academic research, there is an extensive literature studying the productivity gap between men and women (West et al., 2013; Lariviere et al., 2018), but few contributions on peer effects by gender. Bostwick and Weinberg (2018) show that women that enter a PhD program in a year with more women are more likely to finish their PhD in time. In cohorts with particularly low fractions of female peers, women are substantially less likely to complete their PhD within 6 years than their male counterparts.

The remainder of the paper is structured as follows. We present our data on researchers' productivity and the institutional setup driving our supply shock of the spatial allocation of newly promoted economists in section 2. We present and discuss our identification strategy based on the national contest in section 3. Results are presented in section 4.

2 Data and institutional setting

2.1 Institutional setting

In the French public university system, which represents the vast majority of higher education, the hiring and promotion of professors follows a very codified and centralised process. Recruitments at the assistant professor level (called maître de conférences) are decided by each university.[6] Maître de conférences is a civil servant position, hence tenured, but most academics aspire to promotion to the rank of Professeur des universités, equivalent to full professor, which involves a different salary path and increased recognition.

For a number of disciplines, including economics, the promotion to become Professeur des

[5] Foster (2006) on the contrary finds little evidence of peer effects even when separated by gender. Hoxby (2000) finds some evidence of gender-based peer effects. In particular both males and females are found to perform better in classrooms with more females. In the same vein, Lavy and Schlosser (2011) find that an increase in the proportion of girls improves boys and girls cognitive outcomes.

[6] Apart from rare exceptions, the characteristics of the position, in terms of teaching and administrative load as well as in terms of salary, are set centrally.

universités is done by means of a national contest, called concours national d'agrégation.[7] Over our sample period, the agrégation was biannual and entailed four steps over, approximately, a 6-month period, including a research seminar and three oral examinations. The jury then established a ranking of a number of candidates corresponding to the number of positions opened. At the end of the contest, candidates chose sequentially their university according to their final ranking. Importantly, the university chosen by candidates could not turn them down. Candidates lower in the ranking could only choose university position not chosen yet by a better-ranked candidate. When promoted, individuals were required to stay at least three years in the university they chose. After three years, professors were allowed to move to another university wishing to recruit them.

Several features of this system are useful for the rest of the study. First, it implies that we observe exactly the choice set of individuals and their chosen option, which allows us to study the determinants of their choices. Second, the conditions attached to each position, in particular teaching load or wages are centrally determined according to a well-defined grid. Some universities may be more accommodating in how to organize teaching, but the deviations from the standard conditions are small. This implies that we can control for most of the characteristics of the choices. Finally, the organization of the contest implies that candidates ending up low in the ranking have a restricted choice set. This is a useful feature, which limits the possibility to sort on characteristics linked to productivity and that we exploit for identification.

2.2 Data and descriptive statistics

Our data uses the entire population of French academic economists provided by the French Ministry of Higher Education and Research and by the Centre National de la Recherche

[7] Bosquet et al. (2019) provide a complete description of the system. It was abandoned for economics in 2015. Since then, candidates are simply "qualified" by a national committee, which means their name is put on a list for four years, from which universities can recruit.

Scientifique (CNRS)[8] for the years 1990-2007. It includes information about the age, occupation and department membership at the individual/year level. Only individuals that are in departments larger than 4 full-time equivalent academics are kept, in order to restrict analysis to actual economics departments. We run regressions at the department level even when referring to a 'university' by slight abuse of terminology.[9] The Ministry dataset is completed by data on the outcome of nine agrégation contests taking place over our sample period, including the final ranking established by the jury.

We merge this data with the publications recorded in EconLit for years 1991 to 2008, which includes the JEL codes of each publication. We measure the publication output of academics in field f at date t as the number of their publications in field f over a period τ. In our benchmark regressions, τ corresponds to years $t+1$, $t+2$, and $t+3$. Calculating output as a moving average over three years is intended to account for the fact that scientific production is delayed by the publication process. Our measure follows recent work by Ductor et al. (2014), and assumes that knowledge produced in t will only be visible as published articles in the next three years (Waldinger, 2012, uses a one year lag because of shorter delays in the fields of chemistry, physics and maths that he studies). We present robustness checks for our main results varying the definition of τ in Appendix A.3.2. In line with common practice in the literature, each publication is weighted by the inverse of its number of authors. When measured at the field level, as in most of our estimations, $1/J$ of each publication is attributed to each of the J JEL codes (aggregated in 18 different categories at the letter level) mentioned in the publication.

Our final data includes 4,209 researchers working in 83 different departments.[10] Over

[8] Not all academic economics hold a university position in French. There is a separate system involving full-time researchers. Most prominent among those are the researchers employed by the Centre National de la Recherche Scientifique (CNRS). CNRS researchers, who have their own hiring and promotion rules (also national), are hosted by academic departments within university and will thus be part of our sample. In particular these researchers can also benefit from the arrival of a university professor in the department.

[9] A few academics are affiliated to more than one department, in which case their output is split across their various departments, and one individual observation for each department of theirs is considered in the estimations.

[10] The number of departments has been growing over our sample period, either because of creation of new departments or because departments grew larger than our minimal criterion of 4 researchers. The sample of

our sample period, we use 7 contests to construct the instrument (see section 3), with the number of open positions per contest ranging from 15 to 33. Overall, 193 participants were promoted to the rank of full professor through these contests.

In Appendix Table A1, we compare the publication records of the different subgroups of interest for the rest of our analysis. Panel A first compares women and men. Women are less likely to publish than men and are less productive for all publication types except for co-authored publications involving at least one woman peer. The publication gap is large, with men publishing nearly twice as much as women. Senior researchers (above the median age of 45) publish less than junior researchers, the difference being particularly striking for co-authored publications.

Panel B of Table A1 compares the successful candidates in the contest to the rest of the population (left part of the panel). Successful candidates publish close to three times more than the rest of researchers we observe. Regardless of the type of publication under consideration (single authored or co-authored papers), successful candidates are more productive. Moreover, the right part of Panel B shows that those ranked among the last ten in the contest (column 5) are significantly less productive than those ranked above them (column 6), a reassuring feature for the quality of these promotion campaigns.

3 Identification

A standard specification to measure the effect of the number of peers and average peer quality on productivity is the following:[11]

$$y_{it} = \mu N_{u(i,t)t} + \beta \, Y_{u(i,t)t} + \theta_i + \gamma_{u(i,t)} + \alpha_t + \epsilon_{it}, \tag{1}$$

researchers is not balanced, in part due to these inclusions over time.

[11] See for example equation (1) in Waldinger (2012).

where y_{it} is the output (the productivity defined as the three year moving average of publications described above but aggregated over all JEL codes) of individual i working in university $u(i,t)$ at date t; θ_i, $\gamma_{u(i,t)}$ and α_t are individual, university and time fixed effects, respectively. $N_{u(i,t)t}$ is the number of peers at date t (i.e., the department size minus one). Finally $Y_{u(i,t)t}$ is the average output of the peers present in department $u(i,t)$ at date t using their average number of publications per year over the whole observation period, formally:

$$Y_{u(i,t)t} = \frac{1}{N_{u(i,t)t}} \sum_{\substack{j \in u(i,t) \\ j \neq i}} \frac{1}{T(j)} \sum_{t'=\underline{t}(j)}^{\bar{t}(j)} y_{jt'}, \tag{2}$$

where $\underline{t}(j)$ is the minimum between the first year when the individual appears in our panel and the date of their first publication minus three years (i.e. our best guess of the beginning of their career) and $\bar{t}(j)$ is the maximum between the last date they appear in our panel and their last publication minus three years (to assess the end of their career). $T(j) = \bar{t}(j) - \underline{t}(j) + 1$ is the career length. The purpose of considering the whole production of an individual is to restrict the variation of the peer effect variable over time, to be entirely driven by the composition of the department as in Waldinger (2012). We examine in section 4.2 the robustness of our main results when we define production of the peer as the total production up to $t-1$.

Controlling for individual and university fixed effects rules out a number of endogeneity issues that relate to the sorting of researchers according to their permanent characteristics, research skills in particular. We show in Appendix Table A3 that our results are even robust to including match-specific (department times researcher) fixed effects, which should further address this concern. However, the OLS estimation of equation (1) might be subject to an extra bias due to a time-varying endogenous sorting of researchers taking place at the date they move, not only on average over their life. In particular, productive scientists may choose currently strong departments, in their fields of interest when estimations are conducted at the JEL code level, leading us to overestimate peer effects when estimating equation (1).

We therefore build an identification strategy that exploits the national contest described in section 2.1. We instrument the productivity of the peers at date t by the productivity of the successful candidates who join the department at date t through the contest and who were ranked among the last, to limit the set of choices available to them.

Specifically, for department $u(i,t)$ and year t, we instrument the average peers' productivity $Y_{u(i,t)t}$ by:

$$Y_{a(i)t} = \frac{1}{N^a_{u(i,t)t}} \sum_{j \in \mathcal{A}_{u(i,t)t}} \frac{1}{T(j)} \sum_{t'=\underline{t}(j)}^{\bar{t}(j)} y_{jt'} \tag{3}$$

where $\mathcal{A}_{u(i,t)t}$ is the set of successful candidates who were ranked among the last ten in the contest and who arrived in university $u(i,t)$ at date t (if t even) or $t-1$ (if t odd) (the contest occurs every other year). $N^a_{u(i,t)t}$ is the cardinality of $\mathcal{A}_{u(i,t)t}$.

A key identifying assumption is that the candidates do not take into account the projected trend in productivity of the department (specific to their JEL codes when estimations are performed at the JEL code level) when choosing their location. While we cannot formally prove that this assumption is satisfied in the data, we show evidence consistent with this assumption. First, as described above, we restrict ourselves when constructing the instrument to the candidates ranked among the last ten in the contest. These candidates face a largely restricted choice set, and can be considered to arrive in the university quasi-randomly.[12]

Second, we show that among the factors that determine the location choice of researchers, the main driver is distance to the university where the researcher held her previous position, and not at all the scientific quality of the university under consideration. In Table 1 we estimate a conditional logit model where the choice set for each participant in the contest is the actual choice set she faced given her ranking and the choice of those ranked above her.[13] In column (1) we explain the location choice by the number of academics in the

[12] We provide robustness checks in section 4.2 using all successful candidates except those ranked among the first 10.

[13] For the columns with only the last ten ranked (3) and (4), building on those 9 contests, there are therefore $9 \times 54 = 486$ potential observations. In reality, some universities offer several jobs, and the actual number of observations drops to 406.

university and their average productivity, where the productivity in each field is weighted by the candidate's share of publication in that field. Candidates appear to prefer marginally universities with more peers and of higher average productivity. However, when we control in column (2) for the distance of the university under consideration with the university where the mover had her previous position, the scientific characteristics of the university no longer matter. Distance also clearly has the largest explanatory power. The results of Table 1 thus show that the productivity of members of the department in the main fields of interest of the candidate is not a key determinant of her choice. While it does not prove that future trends are not taken into account, it is reassuring evidence.

Table 1: Location choices

	All successful candidates		last ten only			
			All last ten		Move > 250 km	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Number of academics	0.157	0.020	1.652	3.343	7.070	6.989
	(0.819)	(0.869)	(3.045)	(3.475)	(5.563)	(5.590)
Av. academics' productivity	6.514^{a}	3.211	10.416^{c}	-2.749	-2.194	-2.339
	(2.158)	(2.496)	(5.766)	(5.656)	(6.941)	(6.981)
University where ass. prof.		-2.509^{a}		-4.544		
		(0.957)		(3.221)		
Log Distance previous pos.		-1.161^{a}		-1.904^{a}		-0.194
		(0.158)		(0.537)		(0.994)
Pseudo-R^2	0.16	0.35	0.38	0.57	0.59	0.59
Observations	2814	2814	406	406	233	233
Log-likelihood	-440	-343	-76	-53	-30	-30

Notes: Conditional logit estimated. All regressions include department fixed effects. Standard errors between brackets. ^{a}, ^{b}, ^{c} indicate significance at the 1%, 5% and 10% level, respectively. Av. academics' productivity: Average sum of publications of academics (discounted by publications' age with a logistic function), weighted by the JEL specialisation of successful candidates: $\sum_{f=1}^{18} \frac{\tilde{Y}_{uft}}{N_{ut}} \frac{\tilde{y}_{ift}}{\tilde{y}_{it}}$ with $\tilde{y}_{it} = \sum_{t'} \frac{1-exp(-10/(t'+1)^{1.8})}{1+exp(-20/(t'+1)^{1.8})} y_{it'}$.

The special role of distance, also present for candidates ranked among the last ten as shown in columns (3) and (4) of Table 1, raises a second identification concern, related to the specific structure of the contest. There is informal evidence that successful candidates, though assigned in the contest a position in a new university, might in effect spend part of their time in the university where they held their previous appointment, and commute solely to complete their teaching duties. This is consistent with the fact that distance to the previous university plays a large role. Thus the effective time spent in the new position might be endogenous in particular to the scientific characteristics of the host university, which might bias our estimates. We address this concern by estimating a more demanding specification where we construct the instrument using only the candidates among the last ten appointed in a university more than 250 kilometers away from the university where they were maître de conférence. Such a large distance makes commuting an unrealistic strategy, consistent with the fact that when we consider the choice of these individuals in columns (5) and (6) of Table 1, distance no longer plays a role. The consistent results obtained when using this more demanding strategy are presented in the robustness section 4.2.

The last concern is also related to the specific structure of the contest. Since universities do not open a position at each contest, there might be an incentive for a particular candidate and a given university to agree on the timing of application and opening. This appears unlikely, since the strategy is risky: the candidates do not control their ranking or the choices of those ranked above them, so it might be difficult to collude effectively. This is particularly the case for the low ranked candidates and thus our strategy of restricting ourselves to the last ten candidates should alleviate this concern. We nevertheless present in Appendix A.2 a logistic regression of the choice of contest depending on average characteristics of the universities opening a position in that year.[14] We find that none of the characteristics of the contest, except for the number of positions opened in that particular year, can explain why a candidate applied in that year. In particular, neither the distance nor the quality of the universities appear to play a role, strongly suggesting that the strategic choice of when to apply is not an issue.

Most of our estimations are actually conducted at the JEL code level using the procedure we have just described but where any publication is first equally split between all its JEL codes. This similarly leads to an individual i lifetime output at date t, but at the JEL code

[14] Specifically, for an individual applying in contest t we explain the choice between contest t and contests $t-1$ and $t+1$ based on average characteristics of each contest.

level now, which is denoted y_{ift}. The peer effect variable can be computed at the JEL code level, which is denoted $Y_{u(i,t)ft}$. This yields the following specification:

$$y_{ift} = \mu N_{u(i,t)t} + \beta\ Y_{u(i,t)ft} + \theta_i + \gamma_{u(i,t)} + \alpha_{ft} + \epsilon_{ift}, \tag{4}$$

where $\underline{t}(j)$ is the date of the first publication in the JEL code. In that case, the instrument is also similarly computed at the JEL code level.

4 Results

4.1 Peer effects in academia

To provide initial evidence, we start by estimating equation (1). From results presented in Table 2 column (1), we observe that, when individual i's output is aggregated over all JEL codes and the peer group is defined as the entire pool of economists in the department, there is no evidence of peer effects.[15]

This absence of any effect holds in column (2) where the dependent variable is measured at the researcher-JEL code-year level (one observation for each triplet) but keeping the peers' productivity variable at the aggregate level.

In column (3) we estimate equation (4) now with both the productivity of the individual y_{it} and of her peers $Y_{u(i,t)t}$ measured at the JEL code level. In other words, we constrain the pool of senders and receivers to be those in the same JEL code. We find that if other members of the department increase on average their productivity in a JEL code by 1 publication, a member of the department would publish 0.688 additional publications in that JEL code. By contrast, the total number of peers, which could capture economies of scale at the department level, has no significant impact. The average peers' productivity in the JEL code is the key external factor influencing productivity.

[15] The number of peers variable is normalized by the average number of peers in the sample.

In the appendix, we provide a number of robustness investigations. First, when running the analysis at the JEL code level, the dependent variable often takes zero values. Second, the degree of specialisation of the university in the JEL code, that could imply a specific effort to promote the field, and not its peers' productivity in the JEL code could be the relevant channel of spillovers. Table A4 in Appendix A.3.1 shows that our results are robust to the removal of individuals who never publish in general, or never publish in a particular JEL code and to the inclusion of the share of peers publishing in the JEL code.[16] If anything, those robustness tests increase the magnitude of the peer effect.

Table 2: Peer effects, OLS estimations

	(1)	(2)	(3)	(4)	(5)	(6)
	Tot.	JEL	JEL	JEL	JEL	JEL
Number of peers	0.092^c	0.000	0.000	0.000	0.001	0.001^b
	(0.047)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Peers' tot. productivity	0.055	-0.001				
	(0.265)	(0.002)				
Peers' JEL productivity			0.688^a	0.767^a	0.672^a	0.773^a
			(0.019)	(0.020)	(0.017)	(0.019)
– × Woman				-0.360^a		-0.469^a
				(0.024)		(0.025)
– × Age					-0.024^a	-0.028^a
					(0.002)	(0.002)
Year dummies	X					
Year × JEL FE		X	X	X	X	X
R^2	0.41	0.09	0.11	0.12	0.12	0.12
Observations	42,521	771,498	771,498	771,498	771,498	771,498

Notes: All regressions include age, individual and department fixed effects. Standard errors clustered by university-year between brackets. ^{a}, ^{b}, ^{c} indicate significance at the 1%, 5% and 10% level, respectively. The number of peers is divided by its sample mean (67.6). Peers' productivity as defined in equation(2). Column (1) ('Tot.'): Data aggregated over all JEL codes. Columns (2) to (6) ('JEL'): Estimations at the JEL level that include year×JEL fixed-effects. Age is centred with respect to the sample mean when interacted.

Columns (1) to (3) in Table 2 show that the relevant group of senders is the set of peers working primarily in the same JEL code. We then examine the role played by the characteristics of the receivers, paving the way for our detailed analysis of this question in

[16] Bosquet and Combes (2017) do not find any impact of department size on publications either but a role of the share of peers publishing in the JEL code. However they control neither for reverse causality nor peer effects.

section 4.3. In column (4) we show that women receivers benefit less from per effects than men and column (5) shows that older researchers benefit less than younger ones. Comparing a women to a men or a researcher to a researcher 15 year older divides the magnitude of the peer effect by around two.

As explained in section 3, to address the endogeneity of the productivity of the peers, we use an IV strategy based on the instrument defined in equation (3). The results are presented in Table 3. For each specification, we also present the OLS results when restricting the sample to the observations used in the IV regression.[17] The instrument is not weak with a Kleibergen-Paap statistics well above the threshold values for a maximal size of 10% provided by Stock and Yogo (2005) (Table 5.2), equal to 16.38 for column (2) and to 7.03 for columns (4) and (6). Column (2) confirms the result that average productivity of the peers in a JEL code increases the productivity in that field. The IV coefficient is smaller than the OLS one (around 25% smaller), confirming a positive bias in OLS estimates. IV estimates suggest that if members of the department increase on average their JEL code productivity by 1 publication, a member of the department would publish approximatively 0.3 additional publications in that JEL code.

In columns (3) to (6) we confirm that gender and age affect the capacity to benefit from spillovers. The interaction between age or gender and average peers' productivity is instrumented by the interaction of age or gender and the instrument for peers' productivity. As regards the role of gender, the results suggest that in fact women do not benefit at all from peer effects. This is confirmed in Appendix Table A7, where we estimate peer effects separately for men and women. While men significantly benefit from peers, the coefficient for women is not significantly different from zero when instrumenting average peers' productivity. The impact of age is similar to the one obtained with OLS.

As discussed in the introduction, Borjas and Doran (2015) differentiates peer effects

[17] Note that the OLS results on this restricted sample suggest lower peer effects than in Table 2 and therefore stronger peer effects for better universities. Indeed our IV strategy tend to unbalance the panel towards universities of lower quality as better ones either do not open positions at the agrégation, since they cannot fully chose the candidates, or if they do, receive better ranked candidates.

Table 3: Peer effects, IV estimations

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.003^a	0.002^c	0.003^a	0.002^b	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Peers' productivity	0.412^a	0.304^b	0.446^a	0.428^a	0.362^a	0.320^a
	(0.025)	(0.121)	(0.028)	(0.134)	(0.025)	(0.120)
– × Woman			-0.144^a	-0.594^a		
			(0.049)	(0.155)		
– × Age					-0.033^a	-0.029^a
					(0.003)	(0.009)
R^2	0.10		0.10		0.10	
Observations	171,450	171,450	171,450	171,450	171,450	171,450
Kleibergen-Paap		29.7		14.8		15.5

Notes: All regressions at the JEL code level. See Table 2. Instrumented variables are the average peers' productivity in column (2) and also of its interaction with women or age in columns (4) and (6). The instrument is the average successful candidates' productivity defined in equation (3) in column (2) and its interaction with women and age in columns (4) and (6). First stages are reported in Appendix Table A13.

along the geographical and the collaborative spaces. In the collaborative space, a researcher may increase productivity of her peers through co-authorship. In the geographical space, peers can provide indirect benefits, even in the absence of formal co-authorship, by assisting in the production/publication process. These benefits range from commenting the paper, suggesting the correct venues to present, putting researchers in contact with the relevant people in the profession[18] or acting as role models (setting the example in terms of research practices and create an environment that increases productivity).

In our setting a newly arrived peer can create spillovers in both spaces. We now attempt to distinguish these two types of spillovers, by differentiating papers co-authored with peers from other papers. Specifically, for researcher i in university u ar date t, we define publications co-authored with peers as those written with at least one co-author who was affiliated to u, 1, 2 or 3 years prior to publication.

Results in Table 4 suggest that direct co-authorship is not the central channel for spillovers. Column (3) shows that there is an effect of the average productivity of peers on the number of

[18] There is also evidence that peers can help in their position as editors (see Colussi, 2018).

publications co-authored with peers. However this result is not robust to instrumentation as displayed in column (4). On the contrary, average productivity of peers affects co-authored papers without peers both in OLS (column 5) and in IV (column 6). According to the IV results, if peers publish an additional paper in the JEL code on average, this increases the number of co-authored papers without peers by 0.153. Thus, we find evidence of peer effects in the geographical space. When instrumenting, we do not find any significant effect of peers on single-authored papers, suggesting that these indirect spillovers are likely not driven by peers acting as role models.

Table 4: Splitting production in three categories

	Single-author publications		Co-authored publications			
			With peers		Without peers	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.002^{a}	0.001^{c}	0.001^{b}	0.001	-0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Peers' productivity	0.173^{a}	0.044	0.153^{a}	0.107	0.087^{a}	0.153^{b}
	(0.017)	(0.061)	(0.019)	(0.071)	(0.010)	(0.070)
R^2	0.07		0.08		0.09	
Observations	171,450	171,450	171,450	171,450	171,450	171,450
Kleibergen-Paap		29.7		29.7		29.7

Notes: See Table 3.

4.2 Further robustness checks

As explained in section 3, one concern is that some newly appointed professors might choose to commute back and forth to the university where there were previously appointed. To address this, we consider in Appendix Table A8 the results when we use as an instrument the productivity of those ranked among the last ten and in addition appointed in a university more than 250 km away from their prior appointment.[19] As we see the magnitude of the effects is not affected when using this more restrictive definition of the IV, thus addressing

[19]For this set of individuals, we have shown in Table 1, that which their choice of position is not driven by distance.

the concern raised in section 3. We also conduct the opposite exercise which is to show that our results are not driven by the fact that the instrument considers only the last ten candidates ranked. In Appendix Table A9, and Table A10, we construct the instrument using all the successful candidates, except the first 10. Our conclusions are unchanged.

We also consider a robustness check related to the way the productivity of peers is measured. In the previous section, all results were presented using the sum of papers produced by the peers over the sample period. We made this choice to measure peer's productivity more accurately, since the candidates in the contest are still early in their career and their publication profile does not necessarily reflect their quality. Moreover this makes the peer's quality constant over time and the changes in the peer variable only arising from the arrival and departure to the department. This choice however creates a potential issue since, if peers themselves benefit from spillovers, a reverse causality bias occurs. We therefore present in Appendix Table A11 the equivalent of Table 3, when the productivity of peers and the instrument variables are calculated using only publications up to $t-1$. We see that even though the instrument is weaker, consistent with the idea that productivity is measured more imprecisely, the coefficients are of the same magnitude.

4.3 The appropriate match between senders and receivers

Having established the existence of peer effects, we now examine in more depth the heterogeneity depending on the characteristics of both senders and receivers. In the previous sections we have shown that peer effects are present when focusing on particular groups of senders, those publishing in the same field as the recipient, and for certain groups of receivers, such as young and male academics. In this section we explore whether the particular match between the sender and the receiver matters, focusing in particular on age and gender.[20] For instance, are men more likely to provide peer effects to men?

To answer this type of questions, we return to using OLS with individual, university

[20] There is a large literature in management on absorptive capacity (Cohen and Levinthal, 1990), i.e. the capacity to benefit from incoming spillovers, which varies substantially across firms.

and time fixed effects, since our identification strategy does not allow us to appropriately instrument for specific matches between senders and receivers. This is particularly true when studying the role of gender given that few women enter the contest.

Column (1) of Table 5 shows that male peers provide higher spillovers on average than female peers. If male peers' average number of publications in a JEL code is increased by 1 article, the production of any researcher increases by 0.562 in that JEL code. However this average effect hides differential impacts of the match. Column (2) interacts the variables with the gender of the individual receiving the peer effect. Men and women benefit in the same way from women peers but women benefit significantly less from male peers than men do. Overall, we find that peer effects are similar across all types of matches, except when men are matched with men, a match that produces significantly higher spillovers.

Table 5: Gender mechanisms, OLS

Publications	All		Single author	Co-auth. w/o. peers	Co-authored with peers		
					All	1+ wom.	only men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of peers	-0.000	-0.000	-0.001	0.000	0.000	0.000^b	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Peer % women	0.004^b	0.004^b	0.002	0.001	0.001^b	0.001^a	0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Male peers' prod.	0.562^a	0.631^a	0.289^a	0.225^a	0.117^a	0.010^a	0.107^a
	(0.017)	(0.020)	(0.014)	(0.009)	(0.006)	(0.001)	(0.006)
– × Woman		-0.305^a	-0.159^a	-0.096^a	-0.050^a	-0.004	-0.046^a
		(0.024)	(0.017)	(0.012)	(0.007)	(0.003)	(0.007)
Fem. peers' prod.	0.209^a	0.222^a	0.066^a	0.090^a	0.065^a	0.035^a	0.030^a
	(0.014)	(0.016)	(0.010)	(0.007)	(0.008)	(0.004)	(0.005)
– × Woman		-0.038	-0.019	-0.025^b	0.006	0.002	0.004
		(0.025)	(0.016)	(0.011)	(0.008)	(0.005)	(0.007)
R^2	0.11	0.11	0.07	0.11	0.08	0.06	0.07
Observations	771,498	771,498	771,498	771,498	771,498	771,498	771,498

Notes: See Table 3. Male and fem. peers' prod.: Average number of publications per year of male and female peers, respectively (in the field, among all departments' members, calculated over lifetime of each individual). The variation in average peers' productivity hence comes from the changes of the compositions of the departments. Column (6) computes individual productivity based on publications co-authored with peers among whom there is at least a woman and column (7) when all peer co-authors are men.

We then break down the effect according to the type of publication, in the spirit of the previous section. Regardless of publication type, the match between male senders and male receivers stands out as the most productive. For instance, column (3) shows that if male peers increase their average level of publication by 1 paper, men receivers increase their number of single-author publications by 0.289, while the effect is about twice as small if the receiver is a woman. The only exception regards publications co-authored with at least one woman peer (column 6). In this case, the identity of the receiver does not matter. On the contrary, if all peers are male on a publication involving only men co-authors, men benefit again twice more than women from peer effects (column 7).

Table 2, in addition to the role of the gender of the recipient, had also highlighted the role of age. We now explore whether match specificities also matter in the case of age. Distinguishing between junior researchers and senior researchers (above and below the median age at 45), we show in Appendix Table A12 column (1) that senior researchers provide higher levels of peer effects than younger ones. However, senior researchers benefit less from spillovers, and this is particularly true when the spillovers are provided by junior researchers, a rather intuitive result. Once again, this results highlight the particular importance of the characteristics of the match between senders and receivers.

5 Conclusion

This article shows that peer effects in academia are present and large within precisely defined fields and for some groups of researchers, defined based on gender and age. We find that another important component of peer effects is the match between receivers and senders. Moreover, these spillovers are not purely driven by co-authorship, but indirect spillovers also seem to matter. An important finding is that women benefit much less from positive spillovers brought by the arrival of new male researchers in their department. Conversely, men and women benefit equally from peer effects generated by female economists, which are, however, less strong.

Our results have policy implications for the organisation of academia. First, they highlight the value of specialisation and the importance of gender and age composition of the department, as channels to facilitate spillovers. Second, they speak to the important publication and promotion gaps between women and men observed in academia (see Bosquet et al., 2019). The fact that women benefit less from peer effects produced by men, can explain part of this publication gap between men and women (see West et al., 2013; Lariviere et al., 2018), that is also visible in our data (Table A1 shows that men publish nearly twice as much as women). What is the source of these gender-specific effects? Is it that male peers are less available to comment on female colleagues' work or help them advance their career, or even prejudiced to do that? Or is it that women researchers are more reluctant to approach male colleagues to benefit from incoming spillovers? Unfortunately, our data does not allow us to distinguish between these alternative mechanisms. But we view those questions as important themes for future research, in order to setup policies to correct for the publication and promotion gender gaps.

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A Appendix

A.1 Descriptive statistics

Table A1: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A	All	Women	Men	Diff.	Junior	Senior	Diff.
Observations	42,790	10,696	32,094		21,564	21,226	
Woman	0.250				0.326	0.172	-0.154^a
	(0.433)				(0.469)	(0.378)	(0.004)
Age	45.2	41.7	46.3	4.6^a	37.0	53.5	16.4^a
	(9.6)	(9.5)	(9.4)	(0.1)	(4.9)	(5.2)	(0.0)
Prob. to publish	0.337	0.300	0.353	0.053^a	0.429	0.248	-0.181^a
	(0.473)	(0.458)	(0.478)	(0.005)	(0.495)	(0.432)	(0.004)
Quantity	0.283	0.188	0.326	0.138^a	0.359	0.223	-0.136^a
	(1.024)	(0.619)	(1.205)	(0.012)	(1.019)	(1.154)	(0.011)
solo-authored	0.184	0.116	0.216	0.100^a	0.227	0.154	-0.073^a
	(0.902)	(0.546)	(1.068)	(0.011)	(0.887)	(1.037)	(0.009)
coauthored	0.098	0.072	0.110	0.038^a	0.132	0.069	-0.063^a
	(0.260)	(0.175)	(0.291)	(0.003)	(0.285)	(0.245)	(0.003)
without peers	0.031	0.022	0.034	0.012^a	0.040	0.022	-0.018^a
	(0.336)	(0.189)	(0.404)	(0.004)	(0.368)	(0.359)	(0.004)
with peers	0.068	0.051	0.077	0.026^a	0.093	0.047	-0.046^a
	(0.352)	(0.213)	(0.426)	(0.004)	(0.397)	(0.371)	(0.004)
at least 1 woman	0.016	0.017	0.016	−0.001	0.022	0.010	-0.012^a
	(0.154)	(0.120)	(0.165)	(0.002)	(0.184)	(0.120)	(0.002)
only male peers	0.052	0.034	0.060	0.026^a	0.070	0.037	-0.034^a
	(0.316)	(0.178)	(0.391)	(0.004)	(0.352)	(0.350)	(0.003)
Panel B	All	Succ. cand.	Other	Diff.	last ten	Other s.c.	Diff.
Observations	42,790	2,718	40,072		919	1,799	
Woman	0.250	0.266	0.248	-0.018^b	0.322	0.238	-0.084^a
	(0.433)	(0.442)	(0.432)	(0.009)	(0.468)	(0.426)	(0.018)
Age	45.2	36.2	45.8	9.5^a	37.1	35.8	-1.3^a
	(9.6)	(5.8)	(9.6)	(0.2)	(6.4)	(5.4)	(0.2)
Prob. to publish	0.337	0.728	0.314	-0.415^a	0.665	0.761	0.096^a
	(0.473)	(0.445)	(0.464)	(0.009)	(0.472)	(0.427)	(0.018)
Quantity	0.283	0.712	0.264	-0.448^a	0.512	0.813	0.301^a
	(1.024)	(1.314)	(1.067)	(0.021)	(0.957)	(1.453)	(0.053)
solo-authored	0.184	0.458	0.173	-0.286^a	0.334	0.522	0.188^a
	(0.902)	(1.163)	(0.948)	(0.019)	(0.873)	(1.282)	(0.047)
coauthored	0.098	0.253	0.091	-0.163^a	0.178	0.291	0.113^a
	(0.260)	(0.356)	(0.257)	(0.005)	(0.234)	(0.399)	(0.014)
without peers	0.031	0.084	0.027	-0.056^a	0.082	0.085	0.003
	(0.336)	(0.461)	(0.355)	(0.007)	(0.256)	(0.536)	(0.019)
with peers	0.068	0.170	0.063	-0.106^a	0.097	0.207	0.110^a
	(0.352)	(0.521)	(0.372)	(0.008)	(0.262)	(0.609)	(0.021)
at least 1 woman	0.016	0.055	0.014	-0.041^a	0.021	0.072	0.052^a
	(0.154)	(0.346)	(0.132)	(0.003)	(0.116)	(0.416)	(0.014)
only male peers	0.052	0.115	0.050	-0.065^a	0.076	0.135	0.059^a
	(0.316)	(0.390)	(0.347)	(0.007)	(0.235)	(0.448)	(0.016)

Notes: Difference between women and men (column 2 to column 4 of panel A), between junior and senior (column 5 to column 7 of panel A), successful candidates and other researchers (column 2 to column 4 of panel B) and between those ranked last ten and other successful candidates (column 5 to column 7 of Panel B). Succ. cand.: successful candidates in the contest divided between those received 'last ten' in the ranking and other successful candidates (s.c.). Standard errors in columns (4) and (7) and standard deviations in other columns in brackets. ^{a}, ^{b}, ^{c} significant at the 1%, 5% and 10% level, respectively.

A.2 Contest choice

Table A2: Contest choice of successful candidates

	(1)	(2)	(3)	(4)	(5)
Number of jobs	0.069^{a}				0.110^{a}
	(0.023)				(0.035)
Average dept. size		-0.011			0.019
		(0.010)			(0.014)
Av. Av. academics' prod.			3.298		13.257
			(8.830)		(9.946)
Average distance				0.001	-0.001
				(0.001)	(0.001)
Pseudo-R^2	0.03	0.00	0.00	0.01	0.05
Observations	423	423	423	423	423
Log-likelihood	-150	-154	-155	-154	-148

Notes: Conditional logit estimated. Observations are individuals $\times$ contest, where for an individual recruited in contest t we use contest t and contests $t-1$ and $t+1$. The dependant variable takes the value 1 if the candidate was recruited in contest t. All explanatory variables are average characteristics of the universities opening positions in that contest. Standard errors between brackets. ^{a}, ^{b}, ^{c} indicate significance at the 1%, 5% and 10% level, respectively. Av. academics' prod.: Average sum of publications of academics (discounted by publications' age with a logistic function), weighted by the JEL specialisation of successful candidates: $\sum_{f=1}^{18} \frac{\tilde{Y}_{uft}}{N_{ut}} \frac{\tilde{y}_{ift}}{\tilde{y}_{it}}$ with $\tilde{y}_{it} = \sum_{t'} \frac{1-exp(-10/(t'+1)^{1.8})}{1+exp(-20/(t'+1)^{1.8})} y_{it'}$.

A.3 Robustness

A.3.1 Robustness of Table 2

Table A3: Table 2 with interacted individual×department fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Tot.	JEL	JEL	JEL	JEL	JEL
Number of peers	0.140[a]	0.000	0.000	0.000	0.001	0.001[c]
	(0.044)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Peers' tot. productivity	0.064	-0.000				
	(0.252)	(0.003)				
Peers' JEL productivity			0.690[a]	0.772[a]	0.674[a]	0.779[a]
			(0.019)	(0.020)	(0.017)	(0.019)
– × Woman				-0.376[a]		-0.489[a]
				(0.025)		(0.025)
– × Age					-0.025[a]	-0.028[a]
					(0.002)	(0.002)
Year dummies	X					
Year × JEL FE		X	X	X	X	X
R^2	0.50	0.10	0.12	0.12	0.12	0.12
Observations	41,731	771,498	771,498	771,498	771,498	771,498

Notes: See Table 2. All regressions include individual × department fixed effects.

Table A4: Table 2, controlling for the share of peers in JEL code and removing zeros

	Share of peers in JEL code			Removing zeros		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of peers	0.000	0.001	0.000	0.000	-0.000	-0.036[a]
	(0.000)	(0.000)	(0.001)	(0.001)	(0.004)	(0.014)
% peers in JEL		0.224[a]	0.078[b]			
		(0.033)	(0.032)			
Peers' JEL productivity			0.682[a]	0.892[a]	1.062[a]	0.740[a]
			(0.019)	(0.023)	(0.038)	(0.072)
R^2	0.09	0.10	0.11	0.11	0.17	0.48
Observations	771,498	771,498	771,498	427,698	98,947	38,929

Notes: See Table 2. In column (4) observations corresponding to researchers who have never published in any JEL code are removed. In column (5) observations such that a researcher has never published in the JEL code are removed. If a researcher does not publish in a JEL code a given year only, the observation is kept. In column (6) all zero observations are removed.

A.3.2 Robustness of Table 3

Table A5: Table 3 with $\tau = t + 1$

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.004^b	0.003^c	0.004^b	0.003^c	0.002^c	0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Peers' productivity	0.491^a	0.469^c	0.526^a	0.666^b	0.438^a	0.460^c
	(0.043)	(0.241)	(0.046)	(0.283)	(0.041)	(0.245)
– × Woman			-0.143	-0.907^a		
			(0.087)	(0.259)		
– × Age					-0.034^a	-0.025
					(0.005)	(0.018)
R^2	0.05		0.05		0.05	
Observations	171,450	171,450	171,450	171,450	171,450	171,450
Kleibergen-Paap		21.8		10.7		11.1

Notes: See Table 3. The period to compute the peer variable is the following year instead of the average over the next three years.

Table A6: Table 3 $\tau = t + 1, t + 2$

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.003^b	0.003^c	0.003^b	0.003^b	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Peers' productivity	0.448^a	0.415^b	0.482^a	0.590^a	0.394^a	0.409^b
	(0.032)	(0.167)	(0.035)	(0.185)	(0.031)	(0.174)
– × Woman			-0.142^b	-0.813^a		
			(0.062)	(0.193)		
– × Age					-0.035^a	-0.030^b
					(0.004)	(0.013)
R^2	0.08		0.08		0.08	
Observations	171,450	171,450	171,450	171,450	171,450	171,450
Kleibergen-Paap		22.7		11.2		11.8

Notes: See Table 3. The period to compute the peer variable is the next two year instead of the average over the next three years.

Table A7: OLS and IV separate regressions for men and women

	OLS		IV	
	(1)	(2)	(3)	(4)
	men	women	men	women
Number of peers	0.002^c	0.005^a	0.002^c	0.003^c
	(0.001)	(0.002)	(0.001)	(0.002)
Peers' productivity	0.425^a	0.381^a	0.439^a	-0.187
	(0.028)	(0.047)	(0.147)	(0.180)
R^2	0.10	0.09		
Observations	136,044	35,406	136,044	35,406
Kleibergen-Paap			28.2	35.2

Notes: See Table 3.

A.3.3 Robustness with different IV

Table A8: Table 3 restricting the instrument to last ten successful candidates moving more than 250 km

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.003[a]	0.003[a]	0.003[a]	0.003[a]	0.002[b]	0.002[b]
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Peers' productivity	0.409[a]	0.346[a]	0.450[a]	0.465[a]	0.363[a]	0.374[a]
	(0.034)	(0.130)	(0.037)	(0.144)	(0.034)	(0.131)
– × Woman			-0.176[a]	-0.611[a]		
			(0.065)	(0.175)		
– × Age					-0.035[a]	-0.025[a]
					(0.004)	(0.009)
R^2	0.09		0.09		0.10	
Observations	129,636	129,636	129,636	129,636	129,636	129,636
Kleibergen-Paap		35.6		17.9		17.9

Notes: See Table 3.

Table A9: Table 3 keeping in the instrument all successful candidates except the first ten

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.002[a]	0.002[b]	0.002[a]	0.002[b]	0.001[b]	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Peers' productivity	0.395[a]	0.318[a]	0.445[a]	0.457[a]	0.374[a]	0.306[a]
	(0.022)	(0.117)	(0.025)	(0.136)	(0.022)	(0.111)
– × Woman			-0.203[a]	-0.556[a]		
			(0.036)	(0.099)		
– × Age					-0.028[a]	-0.035[a]
					(0.002)	(0.007)
R^2	0.08		0.08		0.09	
Observations	298,782	298,782	298,782	298,782	298,782	298,782
Kleibergen-Paap		45.5		23.3		22.9

Notes: See Table 3.

Table A10: Table 4 keeping in the instrument all successful candidates except the first ten

	Single-author publications		Co-authored publications			
			With peers		Without peers	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.001^b	0.001^c	0.000	0.000	0.000	0.001^c
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Peers' productivity	0.165^a	0.066	0.134^a	0.079^b	0.095^a	0.173^b
	(0.014)	(0.051)	(0.014)	(0.038)	(0.009)	(0.072)
R^2	0.06		0.07		0.08	
Observations	298,782	298,782	298,782	298,782	298,782	298,782
Kleibergen-Paap		45.5		45.5		45.5

Notes: Table 4.

Table A11: Table 3 with productivity of peers defined using past publications only

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Number of peers	0.002^b	0.002^c	0.002^b	0.002^c	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Peers' productivity	0.233^a	0.175^b	0.246^a	0.254^a	0.189^a	0.164^b
	(0.017)	(0.074)	(0.020)	(0.082)	(0.017)	(0.075)
– × Woman			-0.057^c	-0.378^a		
			(0.032)	(0.093)		
– × Age					-0.019^a	-0.018^a
					(0.002)	(0.006)
R^2	0.09		0.09		0.10	
Observations	171,450	171,450	171,450	171,450	171,450	171,450
Kleibergen-Paap		21.9		11.0		11.4

Notes: See Table 3.

A.4 Receivers and senders match based on age

Table A12: Peer effects and age, OLS

Publications	All		Single author	Co-auth. w/o. peers	Co-authored with peers All	Co-authored with peers 1+ wom.	Co-authored with peers only men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of peers	0.001	0.001[c]	-0.000	0.000[b]	0.001[a]	0.000[a]	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Peer % senior	0.002[b]	0.001[c]	0.002[a]	-0.001[c]	0.000	-0.000	-0.000[c]
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Junior peers' prod.	0.323[a]	0.474[a]	0.186[a]	0.121[a]	0.166[a]	0.023[a]	0.098[a]
	(0.017)	(0.023)	(0.015)	(0.008)	(0.010)	(0.003)	(0.006)
− × Senior		-0.276[a]	-0.104[a]	-0.081[a]	-0.090[a]	-0.014[a]	-0.068[a]
		(0.023)	(0.017)	(0.007)	(0.010)	(0.003)	(0.006)
Senior peers' prod.	0.423[a]	0.494[a]	0.213[a]	0.092[a]	0.189[a]	0.016[a]	0.076[a]
	(0.018)	(0.031)	(0.020)	(0.009)	(0.013)	(0.003)	(0.008)
− × Senior		-0.164[a]	-0.031	-0.056[a]	-0.077[a]	-0.012[a]	-0.044[a]
		(0.039)	(0.024)	(0.009)	(0.016)	(0.003)	(0.008)
R^2	0.11	0.12	0.07	0.08	0.11	0.05	0.07
Observations	771,498	771,498	771,498	771,498	771,498	771,498	771,498

Notes: See Table 5.

A.5 First stages of the 2SLS

Table A13: First stages of Table 3

Dep. var.: peers' prod.	–	–	× Woman	–	× Age
	(1)	(2)	(3)	(4)	(5)
Number of peers	-0.004[a]	-0.004[a]	-0.001[a]	-0.004[a]	-0.034[a]
	(0.001)	(0.001)	(0.000)	(0.001)	(0.007)
Sucessful cand.' prod.	0.042[a]	0.040[a]	-0.003[b]	0.042[a]	-0.002
	(0.008)	(0.008)	(0.001)	(0.008)	(0.017)
– × Woman		0.008[a]	0.063[a]		
		(0.002)	(0.012)		
– × Age				-0.000	0.062[a]
				(0.000)	(0.010)
R^2	0.41	0.41	0.41	0.41	0.43
F	29.8	21.1	18.1	20.6	20.0
Observations	171,450	171,450	171,450	171,450	171,450

Notes: See Table 3. Column (1) is the first stage corresponding to column (2) of Table 3. Columns (2) and (3) are the first stages corresponding to column (4) of Table 3. Columns (4) and (5) are the first stages corresponding to column (6) of Table 3.