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The chance of influence: A natural experiment on the role of social capital in faculty recruitment☆

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ABSTRACT

The effect of social capital is often overestimated because contacts and centrality can be a consequence of success rather than its cause. Only rare randomized or natural experiments can assess the real causal effect of social capital. This paper relies on data from one such experiment: faculty recruitment at the *École des Hautes Études en Sciences Sociales* (EHESS) between 1960 and 2005, a leading French institution of higher education in the social sciences. It exploits the fact that the electoral commission, a hiring committee which produces a first ranking of applicants, is partly composed of faculty members drawn at random. It shows that when the PhD advisor is randomly drawn, it doubles the chances of an applicant of being shortlisted.

> "What has remained, however, and indeed has considerably increased, is a factor peculiar to the university career. Whether or not an adjunct lecturer, let alone an assistant, ever succeeds in achieving the position of a full professor, let alone of a head of an institute, is a matter of pure chance. Of course, chance is not the only factor, but it is an usually powerful factor."
> Weber (2008, p. 28)

The role played by social networks and personal contacts in getting a job is one of sociology's most famous propositions (Granovetter, 1973, 1974). Indeed, labor surveys have shown repeatedly that an important fraction of the population in developed countries cites contacts as a reason they were hired in their current jobs (Marsden and Gorman, 2001; Ioannides and Loury, 2004). In the United States, half of the workers interviewed in the 1978 wave of the Panel Study of Income Dynamics heard of their current job from a friend or a relative and 40% of the men and one third of the women surveyed thought there was someone who may have helped (Corcoran et al., 1980). Moreover, one fourth of unemployed jobseekers surveyed in a 1992 study indicated that they had checked during the previous four weeks with friends and relatives to find work (Ports, 1993). In France, 20–25 percent of respondents who had been recently hired stated in Labor Force surveys taken between 2005 and 2012 that they "entered their firm" thanks to "family, personal or professional contacts" (Larquié (de) and Rieucau, 2015).

Yet despite the widespread view that personal contacts—and particularly weak ties—often facilitate job finding, the empirical evidence for a clear link between social networks and employment outcomes is limited. Some studies have found that weak ties can affect outcomes, either as a consequence of information gleaned from weak ties about job opportunities (Fernandez and Weinberg, 1997; Yakubovich, 2005) or as a result of the indirect influence that weak ties can have on people in charge of recruitment decisions (Lin et al., 1981). And there is strong evidence for the importance of strong ties, especially in countries like China where labor markets are not very competitive (Bian, 1997; Obukhova, 2012). People in charge of recruitment may therefore have great motivation to use their discretionary power in favor of the closest candidates.

However, studies based on large samples are much less confident about the causal impact of contacts on job opportunities. The first-order correlation between job contacts and professional outcomes disappears once a set of elementary controls is introduced and relationships are tested that extend beyond subsamples of white upper-class males (Bridges and Villemez, 1986). They also go down after the correlation between the characteristics of individuals and the characteristics of their contacts is taken into account (Mouw, 2003). In Mouw's broad survey of the literature on the causal effects of social capital (2006) he argues that there is actually little empirical evidence demonstrating a link between contacts

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and job outcomes. He points to unobserved heterogeneity and reverse causality—two classic sources of bias, that are more likely to occur with network variables—as potentially leading to substantial overestimation of the impact of networks. He forcefully advocates for methods, such as natural experiments and randomized experiment techniques, which can overcome the current statistical limitations. Two previous studies based on such methods do in fact conclude that social capital hardly plays any role in job outcomes (Mouw, 2003; Stinebrickner and Stinebrickner, 2006).

If it is in fact true that social network variables mainly capture confounding variables like skills or successes (either past or anticipated), this finding would be of dramatic importance for network sociology. Indeed, it should lead us to seriously reconsider a very important stream of theoretical and empirical literature in sociology (Granovetter, 1973, 1974; Lin et al., 1981; Burt, 1992, 2005; Fernandez and Weinberg, 1997; Lin, 2001; Yakubovich, 2005; Obukhova, 2012). But while there are strong reason's to support Mouw's general critique of findings based on statistical estimations that neglect the aforementioned biases, at the same time there are reasons to think that Mouw's studies should not lead to a definitive conclusion about the effects of networks. The technique quoted by Mouw (2006), based on random assignment of students in campuses' dormitories, may not be the best natural experiment to assess the pure causal impact of social capital on recruitment. So before throwing out the sociological baby with the methodological bath water, we need to apply a more convincing causal methodology to situations where contacts or positions in the network are more likely to make a difference.

Randomized experiments are expensive and difficult to implement for most real-life situations, including job recruitment. In social sciences, most randomized experiments are run in the fields of public policy research or development economics (Banerjee and Duflo, 2011). Natural experiments that could be used to learn more about the causal impact of networks on recruitment are unfortunately rare.

The only existing natural experiment in the literature is a recent study of recruitment in Spain (Zinovyeva and Bagues, 2015). In order to ameliorate a widespread perception of academic inbreeding (i.e. the tendency for universities to preferentially recruit their former PhD students), the Spanish Education Ministry required from 2002 to 2006 the randomization of the composition of academic hiring committees for the first round of academic recruitment. The presence of such a natural experiment allows Zinovyeva and Bagues to plausibly claim that the presence of personal contacts increases the chance of recruitment.

However, there are still several limits to this study. First, the study is not informed by any clear theory, sociological or otherwise, for why we should expect personal contacts to influence outcomes. Indeed, the study does not engage with forty years of research into the effects of personal ties.

Second, the study does not situate its findings within the particular cultural and political context that produced the natural experiment. Spanish universities are widely perceived as being influenced by a particular form of parochial nepotism unique to the Spanish context, and it cannot be assumed that an effect observed in this particular academic setting would necessarily also be generalizable to a wider array of European universities, and particularly elite institutions where academic leaders claim to be on the cutting edge of social scientific research, and therefore less influenced by parochial ties.

The recruitment of scholars at the École des Hautes Études en Sciences Sociales (EHESS), a leading French institution of higher education in the social sciences, provides a natural experiment that allows us to measure the causal effect of social networks at one of Europe's most elite academic institutions. Assessing recruitment in this setting will allow us to assess the scope of previously observed effects of social capital on academic recruitment. Firmly rooted in the four decade long sociological inquiry into the effects of social ties, this article uses the presence of the natural experiment at EHESS to conduct a theoretically informed estimation of the precise causal effect of social capital on placement outcomes within an elite educational institution.

The EHESS hiring procedure requires that two-thirds of the electoral commission providing the initial rankings for applicants be drawn at random from the institution's faculty. Thanks to the random component built into the selection process, we can apply the classical experimental feature comparing the outcomes of two groups: (a) the treated group, i.e., the applicants whose personal contact has been randomly drawn; and (b) the control group, i.e., the applicants whose personal contact has not been randomly drawn. The difference in the outcome between these two groups will indicate the effect of having a social contact on the committee. I exploit this feature for several types of personal "contacts" that are persons with whom the applicant is likely to have significantly interacted in academia before applying. It includes, for instance, the applicant's PhD advisor, other members of their PhD committee, their coauthors, and other persons who had the same PhD advisor.

As the article shows, when one of the randomly drawn committee members is the PhD advisor for a given candidate, it doubles the odds of that candidate being put forward for recruitment by the electoral commission. The influence of chance here is a chance of influence: the chance to have your contacts in the right place in order to influence an outcome in your favor. In this regard, the status of the university turns out to hardly be a mitigating factor. Academics at elite universities claiming to be at the forefront of scholarship may be just as susceptible to parochialism as any other.

In sum, the article provides strong evidence that social capital matters for academic recruitment. This result may be reassuring for the sociologist who coined the term, as well as the many sociologists who have spent much of their careers researching the effects of social ties. But at the same time it may be discomforting for many academic institutions whose methods of selection may deviate quite substantially from the meritocratic and universalist ideal of the university (Merton, 1973).

The rest of the paper is organized as follows: The first section details the shortcomings of classical estimations of the causal impact of social capital. The second section establishes links between the EHESS study and previous studies of the academic labor market. The third section presents the data and the method. I present the results in the fourth section, and finish with a discussion of their scope and limitations.

1. Natural experiments on social capital

In network sociology, it has been very common since the work of Granovetter (1974) and Burt (1992) to use a basic regression analysis to try to explain an outcome (getting a job or a promotion, level of pay or pay increase) through the use of social capital variables. Social capital variables generally constitute either the "who" type of social capital (who you know, the influence of a specific contact) or the "where" type of social capital (where you are in the network in terms of centrality, structural constraint, etc.).

Mouw (2006) concentrates his criticism on the "who" type of social capital. Building on the research into peer effects conducted by the econometrician Manski (1993), Mouw shows that regressions seeking to evaluate the influence of a specific contact are particularly vulnerable to the "reflection problem." Since homophily is considered to be a universal feature of social relationships (McPherson et al., 2001; Lin, 2001), one can expect the presence of a strong correlation between an individual's characteristics and those of their contact, both on the observable dimensions, which can be controlled for in regressions, and the unobservable dimensions which cannot be addressed. The unobserved heterogeneity, which is often present, may lead researchers to overestimate the impact of having a personal contact.

Let us consider an example. Combes et al. (2008) test how applicant rankings in economics in the *Agrégation du supérieur*, a national competitive exam for university professors in France, are affected by applicants' links to members of the hiring committee, including the presence of a former PhD advisor, or a member of the same department on the committee. The authors find a strong correlation between such links and the probability of an applicant being hired. However, since the French government chooses members of the committee, they presumably are talented in their field. And the homophilous patterns of relationships would suggest that their contacts (especially former PhD candidates) are similarly talented. The authors do control for talent variables, including the number and quality of publications by both applicants and their respective advisors, and the possession of a position or PhD from one of the top six universities for economics in France. Nevertheless, the teaching talent that also strongly contributes to the exam result remains unobserved in the study. If members of the jury are talented teachers and are assortatively matched with contacts equally talented on that dimension, the coefficient of the tie variable could serve more to measure this unobserved talent than to measure the causal effect of having a tie in the jury. The importance of social capital could therefore be overestimated.

It is true that Mouw does not discuss much of the "where" type of social capital, a term that is used in this paper to describe social capital that is approximated by a network aggregate measure such as centrality (Freeman, 1979) or a structural constraint (Burt, 1992).[1] As the characteristics of the contacts and their specific roles are not known, it is difficult to say a priori whether the "reflection problem" plays an equivalent role here. But one must pay attention to the fact that measures such as centrality and structural constraint—although traditionally cited as causes of success—are also a consequence of past success: people want to connect to the most successful people as a way of sharing their status (Gould, 2002; Barabási and Albert, 1999). Moreover, those already in a network of successful people may hear about promising people by word of mouth before they achieve public success (Menger, 2002). This means that promising or successful people may be more likely to have a larger personal network and to appear more central. Regressing success on network centrality or on structural constraint can lead to suspicions of reverse causality because network aggregate measures can be viewed as either gauges of past success or indicators that a person's future success is anticipated.

Mouw suggests several ways to overcome the difficulty of using traditional econometric methods to properly identify the causal impact of social capital. These include individual fixed effects (Mouw, 2003; Yakubovich, 2005; Chen and Volker, 2016), which can control for time constant individual heterogeneity (but not for time changing unobserved covariates), and exogenous instrumental variables, provided that such variables are really exogenous (a characteristic difficult to prove). He therefore strongly advocates for the most reliable research design, natural experiments (or randomized experiments, if possible), in which a random dispatch allows one to compare, as in the classic double-blind experiment of pharmacology, the difference in outcomes for two randomly drawn groups: those receiving the treatment and those receiving a placebo.

For instance, several papers have used the fact that many universities randomly assign students to two-person rooms and dormitories in order to enhance diversity. This random match can also serve as a natural experiment to estimate social capital effects (Sacerdote, 2001; Marmarosa and Sacerdote, 2002; Zimmerman, 2003). For instance, it has been used to compare the fate of students whose roommates were among the top 25 percent of the distribution of a pre-university scholastic test (treatment) to the control group, whose roommates were more ordinary and fell into the two middle quartiles (Sacerdote, 2001). The former group had an undergraduate grade point average 0.047 higher (0.026 standard deviation) than the latter. If roommate assignments were really made at random, this means that the effect was independent of any other observed or unobserved variable and that the estimation avoided the classic unobserved heterogeneity bias.

Based on the rare cases where such methods are possible—usually involving roommate assignments on American college campuses (Sacerdote, 2001; Marmarosa and Sacerdote, 2002)—Mouw (2006) finds that there is little to no effect, concluding his article with the following pessimistic statement:

> If individuals choose friends who are similar to them, then one may reasonably suspect that the effects of many social capital variables are overestimated because of unobserved, individual-level factors that are correlated with friendship choice and the outcome variable of interest. This is not an argument that social capital does not matter, but merely a suspicion that many existing empirical estimates of the effect of social capital are not much of an improvement over our intuition or anecdotal conviction that it does matter. Overall, the evidence reviewed here suggests that when the problem of endogenous friendship choice is taken into account by a method that attempts to deal with it explicitly, the resulting estimates of social capital effects are modest in size, ranging from essentially zero for the majority of the estimates using randomly assigned roommates to the small, but significant, coefficients reported in fixed effects models of peer effects in education or juvenile delinquency.

For the numerous studies that use network variables as exogenous variables, such a conclusion could seem rather severe. Perhaps the strong net correlation is due to endogeneity? Before we accept a conclusion that is so damaging to the established understanding of networks within network sociology, we should recall that the college roommate tie may not be the most appropriate site for studying the impact of a network. First, this type of tie is rather heterogeneous, ranging from very close relationships to distant and even conflicting ones. Second, although the evidence found by the first studies on random roommates assignments was weak (Stinebrickner and Stinebrickner, 2006), the presence of peer effects was more convincingly confirmed using similar methods in very different environments, including French secondary education (Goux and Maurin, 2007) and Indian engineering school (Hasan and Bagde, 2013). Third, roommate relationships have little connection to the professional and work environment, the domain of interest in most of the research on the impact of social capital.

2. The role of mentorship in academic careers

In contrast to some labor markets where network influence in hiring is seen as having a neutral or even positive effect in terms of efficiency, the fact that contacts and networks play a role in academic labor markets is not generally viewed as valuable. Merton (1973) has shown that the scholarly community developed faith in a set of norms that govern or at least should ideally govern the academic world: communalism, disinterestedness, originality, organized skepticism, and universalism. The last of these assumes that scientific claims will not "depend on the personal or social attributes of their protagonists" (p. 270) and "finds further expression in the demand that careers be open to talents" (p. 273). Some

[1] Ron Burt's structural constraint is a measure of direct connection of one's personal ties. It is negatively correlated with the brokerage opportunities offered by structural holes.

studies stress that contacts do have a globally positive role in the development of ideas (Collins, 1998; Wuchty et al., 2007). However, most of them question the extent to which universalism and particularism govern real academic labor markets (Long and Fox, 1995) while studying how personal relations correlate with individual outcomes such as grants, publications, wages, and jobs (Long et al., 1979; Reskin, 1979; Cameron and Blackburn, 1981; Long and McGinnis, 1985; Godechot and Mariot, 2004; Leahy, 2007; Kirchmeyer, 2005; Combes et al., 2008; Zinovyeva and Bagues, 2015; Lütter and Schröder, 2014).[2]

One common finding of quantitative studies on academic careers is that productivity—generally measured by the number of publications—is at best a very partial predictor of academic careers (Hargens and Hagstrom, 1967; Long et al., 1979; Long and McGinnis, 1981; Leahy, 2007). The commencement and advancement of an academic career seems to correlate more with the productivity and prestige of the mentor and that of the doctoral department than with indicators of individual scientific productivity (Long et al., 1979; Reskin, 1979; Long and McGinnis, 1981). Most studies insist on the overwhelming importance of a sponsor or a mentor, and in particular the PhD advisor (Reskin, 1979; Cameron and Blackburn, 1981; Long and McGinnis, 1985). Future productivity is therefore more a consequence of contextual effects than of initial talent (Long and McGinnis, 1981).

Studies on academic careers in the United States generally focus on long-term outcomes such as career advancement or wages among a set of scholars who have generally succeeded in getting at least their first job in the academic system after the PhD (Hargens and Hagstrom, 1967; Long et al., 1979; Long and McGinnis, 1981; Leahy, 2007). But these studies usually fail to investigate properly the role played by social capital at the entrance to the academy. Analyzing the European state competitive exams taken upon entrance to an academic career can help to enrich previous studies by focusing on two elements that are often overlooked: the possibility of comparing PhDs who succeed to those who fail, and the opportunity to delve more deeply into the social capital mechanisms (direct support or indirect prestige) by which a sponsor may help a PhD to get a job. In the French political science field, PhDs benefit from the social capital of their advisor and that of their PhD committee. The number of contacts and the importance of the structural holes of the members of a PhD committee within the network of PhD committees are a predictor of the probability that PhDs will enter an academic career—a result interpreted by the authors as an indicator of greater efficiency in the diffusion of a reputation within a community (Godechot and Mariot, 2004). It is likely, however, that sponsorship becomes effective not only through indirect efforts at promoting the candidate, but also when the applicant has a sponsor on the hiring committee itself. In their study of the *Agrégation du supérieur*, Combes et al. (2008) find that the presence of a person's PhD advisor on the hiring committee has a strong positive impact on the likelihood of that person getting hired, one equivalent to the candidate having written five additional articles. They also find that the presence of colleagues from the applicant's own department has a moderate impact. However, the authors find no significant impact if the hiring committee includes either other faculty from the applicant's doctoral university or coauthors of the applicant's PhD advisor. Zinovyeva and Bagues find very similar results in their study of the first step in academic recruitment of university professors (*catedrático de universidad*) and associate professors (*profesor titular de universidad*) for all disciplines in Spain from 2002 to 2006: the strongest effect, tripling the odds of recruitment, comes from the presence of the PhD advisor on the selection committee. This effect is followed by the presence of an applicant's coauthor, a colleague from the same university, or another member of the PhD committee (Zinovyeva and Bagues, 2015, Table A1).

Although scholars acquainted with an applicant may sometimes adopt rules to limit the influence of personal bias by, for example, remaining silent during an official meeting to discuss the applicant's qualifications (Lamont, 2009), they still usually participate in the final vote. And even when a professor with a personal connection to a particular candidate wants to remain silent, their colleagues on the committee still usually solicit their opinions, since they are likely to have the most information on that applicant. Moreover, abstaining or resigning from a hiring committee when one knows an applicant (a situation very common in academic "small worlds") can often be paralyzing for a committee. In the recruitment exam of the CNRS (Center National de la Recherche Scientifique/National Center for Scientific Research) in France, for example, the members of the hiring committee requested to resign in only a limited number of cases, such as when an applicant is a current or former family member, the object of a strong love or hate relationship, a supervisor, or someone with whom the committee member has a notorious conflict. In fact, it is not unusual for a previous advisee to be among the applicants to a position, a situation that is all the more common in institutions where inbred applicants are allowed to compete (Zinovyeva and Bagues, 2015). The existence of persistent biases in favor of former PhD advisees—biases which have been documented in previous literature—might help explain the high levels of academic inbreeding that have been documented across many countries (Horta, 2013).

Academic inbreeding was very common in the United States until the late 1970s (Eells and Cleveland, 1935a,b; Hargens and Farr, 1973), and has continued to be observed in law schools (Eisenberg and Wells, 2000). Hargens (1969), for instance, found a rate of inbred scholars in the United States of 15 percent at the end of the fifties, a number that is comparable to the one percent that would have prevailed had recruitment been independent from the university of origin. While most departments in the United States have now established formal and informal rules banning the recruitment of scholars who hold doctoral degrees from the same institution (Han, 2003), academic inbreeding remains substantial in many countries in Europe and in Mexico, at least at the beginning of the academic career (Horta, 2013; Horta et al., 2010; Zinovyeva and Bagues, 2015).

Godechot and Louvet (2010a) have shown that in France during the 1980s, inbred PhDs were 17 times more likely to get hired than outbred PhDs. Moreover, most such studies have shown, usually through a university of origin fixed effect, that inbred scholars are less productive scientifically (Horta, 2013; Horta et al., 2010; Eisenberg and Wells, 2000; Eells and Cleveland, 1935a). The classic model of sponsorship by an advisor could therefore have important consequences for patterns of recruitment in the academic labor market because it would contribute to the phenomenon of academic inbreeding. Based on advisor mobility, Godechot and Louvet (2010b) seem to indicate that the presence of advisors on hiring committees could be responsible for one-fourth to one-third of the incidence of academic inbreeding.

Most of these studies indicate that on academic labor markets, contacts count, with the advisor–advisee contact holding a particular significance. Nevertheless, one must not forget Mouw's critique that the role of social capital can be overestimated because of statistical methods that do not properly handle reverse causality or unobserved heterogeneity. The fact that early career success

[2] In his famous book *Getting a Job*, Mark Granovetter (1974: 16) gives a striking example of the importance of contacts in academia that fits much more with the motivation-focused strong-tie framework than the information-focused weak-tie one: "One postdoctoral student in biology received a letter from an institution to which he had applied for a job, saying that there were 'no openings for an individual with your qualifications.' But when his thesis adviser took a position there, the younger man went along as a research associate; he subsequently received an effusive letter expressing the college's delight at his appointment."

is more related to an applicant's doctoral department or advisor's productivity and prestige than any observable differences in merit possessed by the applicant might be explained, for instance, by an improper measure of academic talent. An interesting concept like visibility—the fact that "people know your name, are familiar with your work, and think highly of your intellectual contributions" (Leahy, 2007, p. 537)—has been coined as a form of social capital. But since it is measured through citation counts it can be difficult to identify properly and to distinguish its effect from that of quality. As we have seen with Combes et al. (2008), most studies on the role of contacts rest on classical regressions and do not fully address the endogeneity issue. Godechot and Mariot (2004) deal with this problem by using the usual PhD committee set up by a PhD advisor as an instrument for the PhD committee set up for the observed candidate.

This strategy may account for some, but presumably not all, of the possible endogeneity measurement problems. Zinovyeva and Bagues (2015) developed a very similar estimation at the same time the present paper was being written, based on a similar natural experiment in Spain: from 2002 to 2006, in all disciplines, the first step of the recruitment of university professors and associate professors was evaluated by a jury drawn at random from the members of a given discipline. Strikingly similar results were found from our analysis of the French EHESS between 1961 and 2005. This similarity led us to believe that the phenomenon of social network influence over hiring patterns extends beyond the institutional framework studied, and may be quite present within European Academia.

3. Recruitment at EHESS: electoral procedure, methods, and data

What would become the EHESS was founded in 1948 as the sixth "section" of the École Pratique des Hautes Études (EPHE), a French doctoral school in social sciences. Its chief boosters were Charles Morazé, Lucien Febvre, and Fernand Braudel, historians of the "annals" school, which advocated strongly for interdisciplinary research (Mazon, 1988). Initial faculty at the school came from four main disciplines: history, sociology, anthropology, and economics. The school continued to focus on these four disciplines in subsequent years, even as it expanded into other social science disciplines such as literature, linguistics, geography, psychology, philosophy, law, and area studies. In 1975, the sixth section became independent from the EPHE and Paris University and was renamed the École des Hautes Études en Sciences Sociales (EHESS).[3] This institution rapidly became one of the most famous institutions in the French social sciences, hiring scholars such as Braudel, Legoff, and Furet in history; Bourdieu, Touraine, and Boltanski in sociology; Lévi-Strauss, Héritier, and Descola in anthropology; Barthes and Genette in literature; and Guesnerie, Bourguignon, Tirole and Piketty in economics. EHESS also hired scholars who were both much less famous than the above list of prestigious academics and also much less productive in terms of publication; and some of these lesser known scholars actively supervised numerous PhDs.

3.1. A form of recruitment both specific and general

EHESS promoted new forms of teaching (the research seminar) and new ways of organizing knowledge (notably around area studies), as well as new forms of research that valued interdisciplinary exchange. The school also adopted a special recruitment procedure called "election" that continues to contribute strongly to its identity. Although the election procedure might seem specific, it has features that are common to many other academic institutions.

First, the procedure is interdisciplinary. Apart from a few exceptions, open positions are described by neither discipline nor topic. Rather than being hired by a single-discipline jury, applicants are nominated by a full faculty assembly vote. Consequently, if they are to be successfully recruited, applicants must be convincing beyond their own discipline, a pattern also found by Lamont (2009) in the allocation of postdoc grants by an interdisciplinary committee.

Second, the election process involves neither formal job talks nor auditions; applicants simply submit a research proposal and teaching project. However, in practice, it is still common for applicants to visit—privately, if possible—with the EHESS president, the members of the EHESS governing bureau, and some key members of the faculty. Consequently, if applicants are to be elected, they need faculty members who will campaign actively on their behalf and convince other electors of their merits. Most of this support activity is informal and difficult to observe, but traces of faculty advocacy for particular candidates have been recorded in the archives. The meeting minutes provide fairly systematic evidence that the names of recommenders were mentioned during deliberations, and that some letter writers supported their candidates publicly during faculty assemblies. Scholars are expected to be sufficiently knowledgeable and generalist to evaluate applicants beyond the boundaries of their own respective disciplines.

Third, because the evaluation of applicants is both time-consuming and costly, the EHESS has used an electoral commission to more thoroughly evaluate applicants since the early fifties. The commission consists of 20–32 members of the EHESS faculty and, beginning in 1975, has been assisted by an EHESS reviewer; since 1987, an external reviewer has also been included. Until 1997, members of the EHESS that were not part of the electoral commission were allowed to step in during the meeting to say a few words in favor of one or another applicant. The EHESS president also has a say in which applicants are worth hiring and speaks on behalf of the school's governing bureau, whose associates, by statute, are also members of the electoral commission. At the end of the discussion, the electoral commission will rank the applicants, usually through a one-round vote. This indicative ranking is very influential and is announced at the opening of the faculty assemblies devoted to recruitment. Applicants obtaining an absolute majority from the first round are put forward,[4] followed by other applicants in decreasing order of votes. Unless a faculty member specifically requests it, applicants who did not receive any votes in the electoral commission will not be discussed in the full faculty meeting. The internal reviewer presents only applicants who have some support from the electoral commission. Additional declared supporters then speak in their favor. Multiple rounds of voting follow the discussion. The applicants receiving the highest number of votes are then offered a position.

The electoral commission therefore plays a similar role to that of the hiring or personnel committees at many American universities, which conduct an initial evaluation of applicants before a vote by the full faculty. The commission result constitutes a sort of straw poll, establishing a list of applicants worthy of concentrated support and votes during the assembly. Applicants with majority support from the electoral commission have a very high chance of being elected by the assembly: 87 percent of those who achieved a majority at the first stage were ultimately elected, versus a 5 percent election rate for the rest of the candidate pool. Still, the faculty

[3] The name EHESS will be used throughout this paper for simplicity, although this designation is correct only after 1975.

[4] It must be noted that the combination of one-round votes and absolute majority criteria may sometimes lead the electoral commission to put forward fewer applicants than the number of open positions.

Table 1
Composition of electoral commissions.

	Assistant professors		Professors	
	Composition	Presence	Composition	Presence
Total size (including substitutes)	33.83		28.48	
	(6.05)		(3.24)	
Effective size (excluding substitutes/including present substitutes)	28.00	25.5	24.16	21.61
	(4.30)	(5.38)	(3.06)	(4.26)
Bureau including president	5.43	4.97	5.13	4.61
	(1.19)	(0.82)	(1.42)	(1.15)
Scientific council	6.57	5.56	3.56	3.02
	(4.75)	(4.54)	(2.98)	(2.81)
Randomly drawn members including substitutes	22.67		20.09	
	(4.64)		(3.44)	
Substitutes (randomly drawn/present)	5.83	1.56	4.32	1.58
	(3.21)	(1.58)	(1.46)	(1.45)
Effective numbers of randomly drawn members (excluding substitutes/including present substitutes)	16.83	14.97	15.77	13.84
	(3.59)	(4.91)	(3.49)	(5.34)
Number of competitive exams	30	32	79	99

Note: The average electoral commission for the assistant professor exam has 33.8 members: 5.4 members of the bureau, 6.6 members of the scientific council, 16.8 randomly drawn titular members, and 5.8 randomly drawn substitutes. Standard deviation in parenthesis.

assembly does not automatically validate the electoral commission's choices. One time out of every eight, the assembly contradicts the electoral commission, most generally in the case of applicants who were put forward but did not achieve a strong majority. While 68 percent of the applicants with 50–60 percent of the votes during the electoral commission were ultimately elected, those close to the majority at the first stage (i.e. those receiving 40–50 percent of the votes), still had a 42 percent chance of ultimately being elected.

3.2. *The random dimension of the electoral commission*

Let us now turn to an interesting feature of the electoral commission for testing the causal impact of social capital: its composition. Since 1961, the EHESS has drawn most of the members of its two electoral commissions (one for assistant professors, the other for professors) at random from the faculty assembly. It is therefore possible to compare applicants whose contacts were drawn to those applicants whose contacts were not drawn.

However, before proceeding, we must account for some complexities in what is otherwise a quasi-experimental setting (Table 1). First, one-third of the commission consists of statutory members: the president of the EHESS, the four or five members of his or her bureau, and the EHESS members of the scientific council, who are elected for terms of four to five years. These nonrandom members of the commission may possess some special unobserved characteristics (such as administrative, scientific, and/or political talent) that favored their election as president, bureau member, or scientific council delegate, leading to the fear that applicants in contact with those ex-officio commission members could share their unobserved characteristics and that this relationship could explain their eventual recruitment. Therefore, we must make sure that such contacts do not bias our estimation of the effect of social capital. I add therefore a variable to control for contacts who happen to be ex-officio members of the electoral commission. But I do not interpret this variable causally, as membership in this group is not randomly assigned.

A second complexity is the fact that substitutes are also drawn at random to replace titular drawn members that are not able to attend the electoral commission meeting. Since membership depends on the nonrandom decision of the titular member whether to sit out the electoral commission, the chance any substitute has of sitting on the commission is lower than that of a titular (drawn) member and is not totally random. To add a third complexity, there is a significant difference between the theoretical size of the electoral commission and its effective size. This complication stems from unexpected absences that even the use of substitutes cannot remedy completely. On the one hand, contacts wanting to promote applicants are probably more effective if they are present at the meeting; so social capital might be better measured if we analyze effective presence rather than composition. On the other hand, the decision of attending the meeting is not random, and this may bias the results. In order to avoid those two last biases, then, my regressions are based on the commission composition, which could be viewed as the *intention to treat* effect, rather than meeting presence, which could be viewed as the *treatment on treated* effect.

In a fourth complexity, although the records are of very good quality for a French academic institution overall, there are some holes (Table 2): The results of the electoral commission were not available for one-third of the exams. Of the remaining exams, composition and presence were recorded for two-thirds of the exams, presence for only one-fourth, and composition for only one-tenth.

Table 2
Reconstitution of electoral commissions.

	Number of competitive exams			Number of applications			Number of elected applicants		
Electoral commission records	Asst. prof.	Prof.	Total	Asst. prof.	Prof.	Total	Asst. Prof.	Prof.	Total
Composition and presence	24	70	94	543	796	1339	85	196	281
Composition only	5	7	12	156	154	310	15	32	47
Presence only	8	29	37	274	286	560	25	72	97
Subtotal	37	106	143	973	1236	2209	125	300	425
Composition known, results of EC unknown	15	35	50	336	325	661	85	98	183
Composition unknown	3	10	13	27	69	96	17	16	33
Total	55	151	206	1336	1630	2966	227	414	641

Note: Twenty-four assistant professor exams recorded both composition and presence at the electoral commission. 543 applications were recorded and 85 persons were elected.

Table 3
Types of links investigated.

	Number of links in EHESS	Number of links drawn in EC	Number of links undrawn in EC	Number of applications with links in EHESS	Number of applications with links drawn in EC	Number of applications with links undrawn in EC
EHESS PhD advisor	450	62	357	450	62	357
Other members of the PhD committee	554	62	430	417	61	344
PhD committee invitation link	317	45	236	198	44	159
Coauthor	893	132	667	315	87	274
Same PhD advisor	595	87	473	338	72	297
Same discipline	55,059	6222	45,015	1998	1502	1982
Reference letters for EC	1603	133	1385	774	121	725
Viva voce support in FA	4340	704	3203	798	436	758
Letters or viva voce	5422	806	4127	1165	516	1097
Letters or viva voce in $t-1$	1608	171	1273	413	134	378

Note: 1603 reference letters were written for applicants: 133 from drawn members of the Electoral Commission (EC), 1385 from EHESS faculty undrawn in the electoral commission (EC). There were 774 applications with at least one letter from a member of the faculty, 121 with at least one from EHESS faculty drawn in the electoral commission, and 725 with at least one from EHESS faculty undrawn in the electoral commission.

Sample size could be restricted to the exams for which the most information is available, but to do so could have a negative effect on the statistical power of the study. In order to deal with issue, I therefore check that the magnitude of the effects remains similar when we restrict it to the exam for which we have the most details.

The experimental design is well suited to accurately estimate the effect of having randomly drawn contacts in the electoral commission and to limit this to the population with contacts among the members of the EHESS submitted to the random draw. Not all applicants fall into this case; some do not have contacts or do not have contacts among the EHESS faculty. I must therefore control for those applications outside the experimental framework in order to properly establish the social capital effect. A control variable for contacts' membership to the EHESS will achieve this goal.

3.3. The model

I therefore model the probability of success (for instance winning a majority of votes at the electoral commission) as a function of the number of contacts among the drawn members of the electoral commission (*drawn*), the number of contacts among the ex officio members of the electoral commission (*exofficio*), the number of contacts in the EHESS that do not belong to the electoral commission (*undrawn*), and a fixed effect for each exam ($exam_j$).

$$P(success) = a.drawn + b.exofficio + c.undrawn + exam_j + u \qquad (1)$$

The causal effect of having a contact in the electoral commission is given by (a–c): the difference between drawn contacts (treatment) and undrawn contacts (control). I can reformulate (1) in the following way, so that $a' = a\text{-}c$ is directly estimated:

$$P(success) = a'.drawn + b'.exofficio + c.EHESS + exam_j + u \qquad (2)$$

with $EHESS = drawn + exofficio + undrawn$ referring to all members of the EHESS faculty.

Thus I control for applications outside the de facto experimental setting, such as applicants whose contacts are outside EHESS ($EHESS = 0$) or are nonrandom members of the electoral commission (*exofficio*). I will not interpret these variables, as I cannot correctly identify the underlying effect (effect of the contact or of unobserved heterogeneity), but I use such variables to isolate the causal effect of the random draw.

In all estimations, I add an exam fixed effect because each exam, with its specific degree of competition, is de facto one experiment, where "treated" and "control" applicants compete against one another. To estimate "experimental exams" more accurately, I will restrict some estimates to exams where I find both treated applicants ($\sum(drawn) > 0$) and control applicants ($\sum(undrawn) > 0$).

3.4. Links studied

The following presents some details on the links I can investigate for the 2209 applications for which I know both the members of the electoral commission and the ranking produced during this first step of recruitment (Table 3). I collected the PhD advisor for all applicants.[5] 419 applications out of 2209 had an advisor eligible to the electoral commission and therefore can be included in the experimental design estimating the causal influence of this specific link.[6] I also collected all PhD committees for defenses at the EHESS from 1960 to 2005. I can therefore measure for the applications of EHESS PhDs the impact of having other members of the PhD committee on the committee as titular members. Similarly, the more senior applicants may also have invited some EHESS colleagues to be on the PhD committee of one of their students or have been invited by them for the same reason. I consider this invitation relation to be a link when it occurs during the three years preceding the application. I also study more indirect links based on common characteristics, such as the impact of the number of persons with whom the applicant shares the same PhD advisor or discipline.

A specific feature of the EHESS survey is that its archives contain records of public acts of support, either as reference letters examined during the electoral commission meeting or as viva voce support in the faculty assembly. Unfortunately, reference letters were either uncommon or irregularly recorded in the minutes of the electoral commission before 1980, and viva voce support was not recorded in the minutes of the faculty assembly at all between 1980 and 1993. Moreover, it is likely that these two forms of support are not completely independent from the random composition of the electoral commission. If complete applications are not due until after the electoral commission has been composed,[7] decisions

[5] It was not rare for some persons to apply without a PhD (like Pierre Bourdieu), especially before 1985. Fourteen percent of the applications fell into this case. For 24 percent of the applicants, I could not find any information on either the PhD or their advisor.

[6] Among those 419 applications, 90 percent are "inbred" applications of EHESS PhDs, plus a minority of 10 percent of external applicants whose advisor was hired after their PhD was defended.

[7] Unfortunately I do not always know the precise date the electoral commission was composed, and I generally do not know the date on which complete applications are due. At the end of the period, the composition of the electoral commission could be decided anywhere from three to six months in advance of the electoral commission meeting. Applications are generally due three months in advance of

Table 4
Applications put forward by electoral commission and vote share in the electoral commission.

A. Applications put forward (linear probability models)

Applications whose PhD advisor is:	1	2	3	4	5	6
Randomly drawn member of the EC	0.137** (0.062)	0.129* (0.066)	0.187*** (0.068)	0.220** (0.085)	0.215** (0.091)	0.139 (0.104)
Ex officio member of the EC	0.056 (0.076)	0.019 (0.072)	0.050 (0.081)	−0.002 (0.107)	0.029 (0.089)	0.137 (0.189)
Member of EHESS	0.040 (0.029)	0.051* (0.027)	0.021 (0.030)	0.014 (0.035)	0.015 (0.036)	0.035 (0.055)
Competitive exam fixed effects	No	Yes	Yes	Yes	Yes	Yes
Field	All competitive exams	All competitive exams	All experimental exams	All experimental exams with composition	Asst. prof. experimental exams	Prof. experimental exams
Number of applications [$n1$; $n2$]	2209 [357; 62]	2209 [357; 62]	991 [184; 55]	749 [143; 42]	563 [131; 33]	428 [53; 22]

B. Vote share

Applicants whose PhD advisor is:	1	2	3	4	5	6
Randomly drawn member of the EC	0.059 (0.039)	0.053 (0.039)	0.090** (0.040)	0.098* (0.050)	0.113* (0.057)	0.064 (0.051)
Ex officio member of the EC	0.088 (0.054)	0.05 (0.049)	0.077 (0.06)	0.094 (0.085)	0.017 (0.06)	0.293** (0.108)
Member of the EHESS	0.046** (0.019)	0.053*** (0.016)	0.036* (0.020)	0.041* (0.023)	0.043* (0.024)	0.022 (0.037)
Competitive exam fixed effects	No	Yes	Yes	Yes	Yes	Yes
Field	All competitive exams	All competitive exams	All experimental exams	All experimental exams with composition	Asst. prof. experimental exams	Prof. experimental exams
Number of applications [$n1$; $n2$]	2194 [357; 62]	2194 [357; 62]	991 [184; 55]	749 [143; 42]	563 [131; 33]	428 [53; 22]

Note: OLS estimates. Cluster-robust standard errors (by exams) in parentheses. $n1$ represents the number of applicants whose advisor was eligible but not drawn for the electoral commission, $n2$ represents the number of applicants whose advisor was drawn for the electoral commission. Experimental exams refer to exams where I find both applicants with undrawn contacts ($n1 > 0$) and applicants with drawn contacts ($n2 > 0$).

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

to write or request support letters may be modified by the random composition. Support for someone at the assembly that occurs after the result of the electoral commission may be influenced by what happened during the commission's meeting. Nevertheless, for persons who repeat their application—a common feature, since only half of the applicants are recruited at their first trial—the level of support generated during previous trials is clearly independent from the random composition of the electoral commission.

3.5. Checking the experiment's validity

Before analyzing the results, I will address the classic question of whether experimental conditions can modify behaviors and therefore bias the results of the experiment. The experiment in question here is not double-blind: the members of the electoral commission know that the applicants they support have applied, and applicants may know that their contacts are members of the electoral commission. This knowledge might favor certain strategic decisions, such as whether to apply (if the electoral commission is constituted before application), whether to withdraw an application, and whether to attend the electoral commission meeting. I will analyze this phenomenon with specific attention to the link considered by previous literature as the most effective form of sponsorship: the PhD advisor–advisee link.[8]

Studying the question of whether the random draw modifies applicants' behavior is difficult, because this requires a larger population of potential applicants. I therefore use the larger population of EHESS PhDs and analyze the probability that candidates with PhDs from the EHESS will apply for the assistant professor exam in each of the fifteen years that follow the PhD defense. Table A2 shows that having one's advisor randomly drawn for the electoral commission does not substantially change the results. Having contacts within the EHESS clearly affects whether one applies or not, but the specific fact of having an advisor on or off the electoral commission does not seem to have any impact.

It is easier to determine whether the knowledge of applications influences the probability that an advisor will attend the electoral commission meeting. Table A3 provides such an analysis, and we can see that the experimental conditions are not totally met. The probability that an advisor will attend the electoral commission meeting increases significantly when a former advisee applies. This leads me to privilege here as well the composition of the electoral commission (the *intention to treat* effect) rather than the effective presence (the *treatment on treated* effect).

However, Table A4 shows that the random draw of the electoral commission is independent from the characteristics of the applicants, especially the ones predicting success at the electoral commission stage. Being a native-born French national, holding a prestigious higher degrees such as the École Normale Supérieure or

this event. But reference letters can be sent up to a few days before the electoral commission meeting.

[8] Table A1 in the appendix indeed shows that the PhD advisors are very involved in supporting their former advisees. When advisees apply, 39% of advisors write reference letters, 39% support them publicly in the faculty assembly, and 57% support them in either one way or the other.

Agrégation, and having prior publications or previous applications does not affect the probability of whether an applicant's PhD advisor will be on the committee. This result shows that the random draw is not biased and that I can causally interpret the result without fearing some bias due to unobserved heterogeneity or reverse causality.[9]

4. Results

4.1. The advisor effect on the electoral commission

The descriptive statistics in Table A5 deliver the message of this experiment almost completely. For applicants whose advisors are randomly drawn for the electoral commission, the rate of success is 34 percent, with an average proportion of votes of 28 percent; by contrast, the success rate of the control group with undrawn advisors is 20 percent, with an average proportion of votes of 22 percent. In Table 4 (model 2), exam fixed effects are included in order to take into account the fact that each exam is actually one experiment. "Contact" is defined here as an applicant's PhD advisor being randomly drawn as either a titular or substitute member of the electoral commission. When the composition of the commission is not known (representing one-fourth of the cases), I use the presence of the advisor at the electoral commission meeting. This choice represents a compromise between achieving the purity of a randomized experiment and maintaining the study's statistical power. Furthermore, I will show that the results still hold even if I restrict the experiment more precisely to the random conditions. I privilege linear probability models in order to estimate dichotomous variables such as being put forward by the electoral commission, but I also test these relationships with logistic regression (Table A6), finding very similar results.[10]

The selection of the PhD advisor to the electoral commission increases a former advisee's probability of being put forward by 13 percentage points and increases the vote share by 5 percentage points (not significant). The contrast between these two results may be due to the fact that a PhD advisor will mainly campaign in favor of former advisees when the latter are near the majority threshold.

I restrict the model further (model 4 of Table 4) to just "experimental exams" where applicants with drawn contacts and those with undrawn contacts compete.[11] The advantage of having a contact inside the jury in this case increases the probability of being put forward to 19 percentage points and the share of votes to 9 percentage points. Part of this result could be biased, however, as I also use exams where I only have presence (*treatment on treated*) instead of composition (*intention to treat*). Model 4 shows that the drawn advisor effect remains, and its magnitude even increases when restricted only to exams for which I have the composition. Finally, I estimate the advisor effect within two subpopulations: assistant professors (Maîtres assistants and Maîtres de conférences) and full or joint professor exams (Directeurs d'études and Directeurs d'études cumulants). The advisor effect is much stronger and more significant for assistant professors (+22 percentage points in probability of being put forward, +11 percentage points in share of votes) than for professors (+14 points in probability and +6 percent share of votes), where it is lower and not significant (although not very far from the 10 percent threshold).

Two reasons could explain this difference, and both are very similar to those found by Zinovyeva and Bagues (2015). First, the link to the former PhD advisor may weaken as time passes after completion of the PhD. Second, it might be easier during professor exams to evaluate applicants on the basis of their scientific records and their personal reputation, and voters might rely less on the comments of those who know the applicant best.

In order to more thoroughly evaluate the advisor effect as well as the difference between the *intention to treat* effect and the *treatment on treated* effect, I restrict the sample to experimental exams (as previously defined), for which I know both the original composition of the committee and the presence of its members on the day it meets (Table 5). The advisor effect is somehow higher when I consider titular members initially drawn for the committee (+25 percentage points in probability of being put forward, +13 percentage points in share of votes) or advisors who are ultimately present on the electoral commission. This last *treatment on treated* effect may be biased for the reasons explained above, but we may nevertheless be able to measure the *treatment on treated* effect by opting for instrumental variables. In the first-stage regression, I model the probability that an applicant's PhD advisor who has been drawn for the commission will ultimately be present on the commission, based on two strong instruments: being drawn as a titular member and being drawn as a substitute. In the second-stage regression, instead of using biased presence as the independent variable, I use the prediction of presence based on the two clearly random exogenous instruments described above. The *treatment on treated* effect is even higher, with 29 percentage points higher probability of being put forward and 15 percentage points higher probability in share of votes. This estimation could be an unbiased estimation of the *treatment on treated* effect of the PhD advisor, provided that advisors influence their colleagues only during the final meeting of the electoral commission.[12]

4.2. The advisor effect at various stages

How does the selection of the PhD advisor to the electoral commission influence the recruitment process overall? In Table 6, I estimate the different steps of the recruitment process based on model 4 of Table 4—this paper's favorite estimation combining experimental accuracy and statistical power. I find that randomly drawn PhD advisors seem to have no influence on the president's support (in the name of the bureau) during the electoral commission meeting. Instead, a PhD advisor probably influences other colleagues: they get an average of 9 percent of the electoral commission (equivalent to themselves and one other person) to vote in favor of their former PhD candidates. This may appear to some as a rather limited influence, but we must recall that members of the electoral commission do not always support their former PhD advisees; for some recruitment years, they might influence up to two or three other people, a number that can be decisive for hiring outcomes, especially when the application is near the majority threshold.

The impact of the random composition of the electoral commission continues during the faculty assembly, the final and decisive step of the recruitment process. The selection of the PhD advisor in the electoral commission increases by 0.6 the number of persons speaking in favor of the applicant (nearly significant), adds 6 percentage points to the share of votes (nearly significant), and 13

[9] There is therefore no need to introduce control variables in the following regressions.

[10] There has been recent debate on the respective merits of logistic regression and linear probability models (Mood, 2010; Angrist and Pischke, 2009). Logistic regression provides a better functional form, especially near the 0 or 1 borders, but its constant variance may call into question the comparison of parameters from one regression to another.

[11] Academic inbreeding inflates the parameter for undrawn EHESS contacts in exams where no candidates have had contacts drawn, therefore shrinking the final difference with candidates whose contacts were drawn.

[12] The campaign aspect of the recruitment makes this last hypothesis questionable. Members of the electoral commission who cannot make it to the commission meeting may try to influence their colleagues in advance.

Table 5
Variations of results depending on the definition of the membership of the electoral commission.

Applicants whose PhD advisor is:	1 Put forward	2 Share of votes	3 Put forward	4 Share of votes	5 Put forward	6 Share of votes	7 Put forward	8 Share of votes
Randomly drawn member of the EC	0.219** (0.099)	0.114* (0.059)	0.250** (0.105)	0.130* (0.063)	0.251** (0.117)	0.134* (0.065)	0.286** (0.122)	0.148* (0.074)
Ex officio member of the EC	−0.103** (0.044)	0.094 (0.11)	−0.100** (0.043)	0.095 (0.11)	−0.109** (0.041)	0.092 (0.108)	−0.099** (0.044)	0.096 (0.110)
Member of the EHESS	0.004 (0.044)	0.037 (0.033)	0.002 (0.043)	0.036 (0.032)	0.009 (0.043)	0.039 (0.030)	0.001 (0.044)	0.035 (0.033)
Definition of membership to electoral commission	Drawn as titular or subsidy		Drawn as titular		Presence		Presence predicted with instrumental variables	
Number of applications [$n1$; $n2$]	495 [87; 37]		495 [91; 34]		495 [94; 30]		495 [94; 30]	

Note: OLS estimates. Cluster-robust standard errors (by exams) in parentheses. $n1$ represents the number of applicants whose advisor was eligible but not drawn for the electoral commission, $n2$ represents the number of applicants whose advisor was drawn for the electoral commission. Field: experimental exams for which I know composition and presence with both treated ($n2 > 0$) and control applications ($n1 > 0$).
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 6
The advisor effect on different steps of recruitment.

Explained variable	PhD advisor randomly drawn effect	N [$n1$; $n2$]
1. President's support in electoral commission	−0.018 (0.074)	711 [131; 47]
2. Share of votes in electoral commission	0.091** (0.04)	991 [184; 55]
3. Number of votes in electoral commission	2.06** (0.938)	991 [184; 55]
4. Put forward in electoral commission	0.187*** (0.068)	991 [184; 55]
5. Number of viva voce supports in faculty assembly	0.650 (0.421)	991 [184; 55]
6. Vote share in faculty assembly	0.062 (0.041)	981 [181; 53]
7. Number of votes in faculty assembly	3.908 (3.836)	981 [181; 53]
8. Election in faculty assembly	0.132** (0.06)	991 [184; 55]

Note: Each cell corresponds to a different regression (OLS estimates). The regressions are similar to those used in Table 4. I show only the parameter of interest. Cluster-robust standard errors (by exams) in parentheses. $n1$ represents the number of applicants with an eligible advisor who was not drawn for the electoral commission; $n2$, the number of applicants whose advisor was drawn for the electoral commission. Field: experimental exams with both treated ($n2 > 0$) and control applications ($n1 > 0$).
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

percentage points to the probability of being elected (significant). These effects are essentially due to the better rank achieved during the first step because of the random selection of the PhD advisor to the electoral commission.

4.3. *Effects of other contacts*

PhD advisors—the usual suspects in academic sponsoring—have a clear effect on academic recruitment that cannot be due to some form of unobserved assortative matching between themselves and their former advisees. But are they the only personal contacts with influence over the recruitment process? In Table 7, I analyze the sponsoring effects of other possible contacts, whom I compare to the PhD advisor.

Applicant's coauthors—who sometimes are considered to be a rather strong collaboration tie (Combes et al., 2008; Zinovyeva and Bagues, 2015)—come in second in terms of magnitude. But there is a lack of statistical power here, and the effect is not significant. This might be explained by the fact that within French social science during the period, coauthoring remained rare and socially heterogeneous (with an important fraction of coedited books, which involve less collaboration, among the coauthored publications).

Weaker links, such as other members of PhD committees or members of the same discipline, do not seem to influence the recruitment process. This negative result also tends to show that advisors' involvement is not just a question of thematic or disciplinary similarity with their former advisee. Similarly, PhD committee invitation links for professor exams do not have any measured impact. But if I restrict the pool to external applications, they then have a significant positive impact on the share of votes. At the professor level, one-third of the applications and 44 percent of the applicants put forward are already assistant professors at EHESS. For those applicants who are well integrated into the EHESS institution, multiple channels of influence may exist (such as team and research center memberships). For more external

Table 7
Different types of links and their causal effect on electoral commission decisions.

Effect of contact's presence on electoral commission when contact is:	All			Assistant professor exams			Professor exams		
	Put forward	Share of votes	N [$n1$; $n2$]	Put forward	Share of votes	N [$n1$; $n2$]	Put forward	Share of votes	N [$n1$; $n2$]
PhD advisor	0.187*** (0.068)	0.090** (0.040)	991 [184; 55]	0.215** (0.091)	0.113* (0.057)	563 [131; 33]	0.139 (0.104)	0.064 (0.051)	428 [53; 22]
Other member of PhD committee	−0.012 (0.066)	−0.03 (0.033)	1102 [203; 61]	−0.070 (0.061)	−0.065 (0.038)	597 [128; 30]	0.058 (0.116)	0.009 (0.054)	505 [75; 31]
PhD committee invitation link	−0.041 (0.088)	−0.018 (0.058)	673 [93; 41]	–	–	–	−0.038 (0.091)	−0.016 (0.06)	603 [92; 40]
Coauthor	0.062 (0.052)	0.019 (0.031)	1102 [158; 83]	0.094 (0.161)	0.082 (0.072)	380 [28; 11]	0.046 (0.055)	0.010 (0.033)	722 [130; 72]
Person sharing the same advisor	−0.055 (0.042)	−0.025 (0.028)	1016 [175; 67]	0.010 (0.043)	0.017 (0.029)	544 [98; 32]	−0.159** (0.073)	−0.092** (0.044)	479 [77; 35]
Person in the same discipline	0.005 (0.006)	0.004 (0.004)	2170 [1960; 1493]	−0.009 (0.007)	−0.002 (0.005)	972 [895; 693]	0.013 (0.010)	0.002 (0.007)	1198 [1065; 800]
Reference letter author for electoral commission	0.068 (0.050)	0.014 (0.021)	1178 [549; 118]	0.002 (0.091)	−0.007 (0.037)	572 [203; 42]	0.100* (0.052)	0.028 (0.024)	606 [346; 76]
Viva voce supporter in faculty assembly	0.043* (0.022)	0.029** (0.011)	1335 [744; 434]	0.037 (0.035)	0.039** (0.019)	623 [285; 144]	0.042 (0.031)	0.023 (0.016)	712 [459; 290]
Letter or viva voce supporter	0.043** (0.020)	0.027*** (0.01)	1848 [1023; 511]	0.025 (0.031)	0.032* (0.017)	833 [379; 173]	0.049* (0.027)	0.024* (0.014)	1015 [644; 338]
Letter or viva voce supporter in previous exams	0.086* (0.044)	0.052** (0.023)	1484 [308; 130]	0.155*** (0.052)	0.094** (0.037)	623 [105; 41]	0.071 (0.057)	0.043 (0.029)	861 [203; 89]

Note: Each cell corresponds to a different regression (OLS estimates). The regressions are similar to those used in Table 4. I show only the parameter of interest. Cluster-robust standard errors (by exams) in parentheses. N represents the number of the number of applicants, $n1$ represents the number of applicants who have undrawn eligible contacts in the electoral commission, $n2$ the number of applicants who have contacts drawn in the electoral commissions. Field: experimental exams with both treated ($n2$) and control applications ($n1$).

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

applications, however, PhD links are a way of influencing and entering this institution.

A contact is generally expected to have a positive influence. Although balance theory has modeled for long the combination of positive and negative ties, negative ties in organization and labor markets have been until very recently overlooked (Labianca, 2014). Competitors on small markets are likely to become such negative ties, as their outcome is negatively correlated with ego's outcome. Hence, at the EHESS, I found a type of potential contact which produces negative outcome: when a contact had the same PhD advisor as the applicant. In professor exams, the random selection of this type of contact lowers the probability of being put forward by 16 percentage points and the share of votes collected by 9 percentage points. Zinovyeva and Bagues (2015), by contrast, do not find any impact at all for this type of contact. This unusual result is likely specific to EHESS history and its recruitment procedures; EHESS was a central place for the reconstruction of the social sciences in France during the second half of the twentieth century. It was the locus for many burgeoning and often antagonistic schools of thought, with opposing scholars coming from the same intellectual background who often had the same advisors, in a fractal way (Abbott, 2001). This negative effect may therefore be due to the fact that, as PhD solidarity declines with time, EHESS members may want to be their advisor's only intellectual descendant and may be reluctant to hire other rival descendants.

I also focus on the efficiency of declared acts of support, written letters of reference, or the public viva voce support of applicants during the faculty assembly. As stated above, the electoral commission membership is put together before viva voce support at the faculty assembly can be given, and letters may be written before or afterward, meaning that both of these could plausibly be influenced by the composition of the committee.[13] While I cannot claim to identify the causal effect properly, the results are nevertheless suggestive. A letter supporter only has a significant effect on professor exams. A viva voce supporter helps on all types of exams, and increases the share of votes at assistant professor exams. Either form of support significantly increases the probability of an applicant being put forward (all exams and professor exams) and the share of votes (all exams assistant professor and professor exams). Because the decision to support a candidate is biased by the composition of the commission, I also consider the possibility that past supporters of applicants who repeat their application after a failure are drawn for the commission, and hence those supporters are not influenced by the random composition of the electoral commission.[14] Past supporters have a clear positive and significant causal effect both on the committee proposal (ranging from 9 percentage points for all exams to 16 for assistant exams) and on its share of votes (ranging from 5 percentage points for all exams to 9 for assistant professor exams). This effect remains significant even when I control for the selection of the advisor to the electoral commission. Although advisors are very likely to actively support their applicants through letters or public declarations in the faculty assembly (57 percent of them do so; see Table A1), the supporter effect is not just an advisor effect.

[13] In fact, when I regress the probability for an EHESS scholar to support a given applicant upon being selected for the electoral commission, I find that commission membership significantly decreases the probability of writing a reference letter but significantly increases the probability of publicly supporting an applicant during the faculty assembly.

[14] I successfully verified that there was no significant correlation between these acts of support and being randomly selected for the electoral commission.

5. Concluding comments

Weber (2008) observed a long time ago that entering into academia and progressing further in an academic career is "a matter of pure *chance*". This article presents evidence that the chance of having a supporter on a recruitment committee is a key element of this chance. I identified former PhD advisors as among the strongest potential sponsors, with an effect that can double or triple a candidate's odds of being put forward. This effect is similar to the one found by Zinovyeva and Bagues (2015) in their study of a similar natural experiment in a different institutional setting: a seven-member disciplinary and national recruitment committee in Spain between 2002 and 2006.[15] The striking convergence between the two studies shows that the results can be extrapolated beyond these two cases to European academic labor markets. Moreover, the results found in these quasi-experimental conditions are also strikingly in line with previous non-experimental studies which on their own could not rule out unobserved heterogeneity or reverse causality. Hence, those possible biases finally do not lead here to an overestimation of the effects of social capital in non-experimental studies as dramatic as the one feared by Mouw (2006).

5.1. Possible underlying mechanisms

Although some applicants' ties, such as those to a former PhD advisor, have a purely causal effect on the recruitment committee's decision, the reasons underlying such involvement are still not clear. Several mechanisms may contribute to it. First, advisors and advisees share similar scientific preferences, which are at the basis of their PhD collaboration. Perhaps it is less about advisors supporting persons they know than about supporting scientific approaches they like. A second mechanism in terms of information and evaluation costs could lead to similar results: advisors are already well aware of what is interesting about their advisees' work, and it is less costly to find and promote the key points of these applicants than those of the applicants an advisor does not know. However, the fact that other members of the PhD committee do not have any effect, even when they may share similar scientific preferences and also be well acquainted with applicants' work, would suggest that these two first mechanisms are not that important.[16]

One could also suspect that the strong personal links to former advisees generate either subconscious or conscious judgment biases. In the first case, because advisors know and like their former advisees, they will subconsciously end up valuing their advisees' scientific contribution. The second case could be seen as an example of the classical "motivation" argument (Obukhova, 2012): advisors deliberately support applicants they like, even though they may feel that their advisees are not the best candidates. New research is needed in order to disentangle the respective power of these four mechanisms.

5.2. A touchy issue

As noted previously, the impact of the advisor on academic hiring may contribute to the importance of academic inbreeding, a feature common in many countries and strongly debated. At EHESS, the random selection of applicants' advisors to the electoral commission causes ten more advisees to be put forward than otherwise would have and contributes to one-third of the success differential between inbred (22 percent) and external applicants (16 percent). The probability for inbred applicants to have their advisor on the EHESS recruitment committee is low (1 out of 8), whereas in university departments, especially small ones, it is probably much higher and thus contributes much more to academic inbreeding. The results could also have normative consequences for public policy. Academic systems share (or are supposed to share) the Mertonian ideal of meritocracy and indifference to personal characteristics. Systematic bias in recruitment is prejudicial both to the quality of the academic system and to its equity. On the other hand, academia consists of small communities where people know one another well. Excluding all persons with a potential bias toward an applicant could lead to paralysis. Academic institutions must arbitrage these two risks. One possible way of coping with an important fraction of possible bias without paralyzing recruitment would be to ban the recruitment of inbred candidates, a practice of most American universities since the 1980s (and most mathematics departments in France since the early 2000s).

5.3. On the respective efficiency of strong and weak ties

Finally, this natural experiment furthers the debate on the efficiency of social capital and contacts in getting a job, as well as the respective roles played by strong and weak ties. This article not only shows that social capital clearly does matter (in contrast to Mouw's (2006) statement), but also that strong ties matter more (undermining Granovetter's (1973) and Lin's (2001) predictions). Here, a given strong tie like the PhD advisor is much more effective than a given comparatively weak tie like the other members of the PhD committee. While Lin predicts that stronger ties count more for the success of "expressive action", he maintains a "strength-of-weak-tie" argument for "instrumental action". The rationale for his hypothesis is that the level and the heterogeneity of resources embedded in a tie increases with its weakness. Through weak ties, especially those serving as a bridge in the network, ego can access complementary resources from people higher up in the hierarchy.

Two reasons explain why Lin's proposition fails in our case. The first lies in the mechanisms through which social capital enhance outcomes of action. Although Lin (2001) considers influence to be one of the four mechanisms of social capital (with flow of information, social credentials, and (self-insurance) reinforcement), he does not derive the full conclusion of his finding for tie strength. As influencing is much more involving than informing, it will not be exercised by the tie without strong incentives to do so. Thus tie strength is (along with direct personal interest) one of the bases of this "motivation" (Obukhova, 2012; Chen and Volker, 2016). In our case, we think influence plays the most important role.[17] The second reason has to do with conditionality. In the strength-of-weak tie proposal, it is not the weakness per se that matters but rather some features to which it is tied—such as resource heterogeneity (Lin, 2001) or information novelty (Granovetter, 1973). But once we hold those resources constant, the willingness for a tie to share them with ego does not decrease anymore with the strength of the tie; on the contrary, it begins to increase with it. In the case studied here, comparing the impact of members of a recruitment

[15] They find an advisor effect of 14 percentage points for all exams (18 for assistant professor exams and 10 for professor exams) (Zinovyeva and Bagues, 2015, Table A1). Let us turn their results into an odds ratio for better comparability: In Spain, advisors' selection to the committee multiplies the probability of success by 2.6 (3 for assistants, 2.1 for professors). For EHESS, I find an odds ratio of 2 (2.2 for assistants, 1.8 for professors) and 2.9 (3.5 for assistants, 2.3 for professors) when I restrict to experimental exams (cf. logs given in Table A6).

[16] It is true that members of the PhD committee (chosen in France at the end of the PhD) may have fewer shared scientific preferences with the applicants and less knowledge of their scientific contributions than PhD advisors.

[17] In the type of recruitment studied, it is difficult to distinguish between influencing and credentialing (which is an important way of influencing peer decisions). The absence of job talks gives little space to self-insurance reinforcement. The fact that PhDs don't apply more when their supervisor serves in the electoral committee discards the effect of the information flow from recruiter to candidate.

committee, we hold constant one of their essential resources: speaking and voting rights. This conditionality leads logically to a strength-of-strong-tie result. However, these results are also in line with other studies on sponsoring in academia where tie strength is not as conditioned on the resource provided (Godechot and Louvet, 2010b). Consequently, we can hypothesize that even without conditioning on the resource provided, thanks to the influence mechanism, the efficiency of a given tie for getting a job in academia is positively correlated with its strength.

Before inverting the Granovetterian formula (Krackhardt, 1992), however, we must add some nuances. The strength-of-strong-tie result found in this paper holds for a given tie. However, it does not tell us anything about the aggregate effect of strong ties relatively to weak ties. Recalling that the probability for a given tie to have an effective role in a recruitment committee is low—and that people have more weak ties than strong ties—we must balance (in terms of mathematical expectation) the strong influence of a lower number of strong ties against the small influence of a much higher number of weak ties.[18]

While these results are established for academia (especially in France and Spain), one may wonder whether they hold true also beyond this sector. Indeed as it grants recruitment power to peers who are experts in precisely the type of work of the candidates and who have some chance of having worked with candidates in the past, academia maximizes the opportunities for strong tie influence. One could argue that the role of influence is much weaker in corporate firms, where human resources personnel with little work experience in an applicants' field hold a strong selection power. In such situations the information flow through which one hears about the job should count more than the influence process. However in small firms, there is usually no human resources service. Even in large firms—and especially in industries like finance where hiring is selective (Godechot, 2014)—the supervisors and their direct collaborators often hold the actual recruitment power, while human resource departments serve more as a support function in charge of establishing labor contracts. On the one hand, direct hiring could pave way to influence.[19] On the other hand, incentives to maximize the firm profit might mitigate this phenomenon. Future research efforts should measure the relative importance of these influence mechanisms. It is likely that their magnitude increases not only with the level of disconnection between firms interest and personnel interest (Bian, 1997; Obukhova, 2012; Chen and Volker, 2016) but also with the devolution of recruitment power to the team where the job is offered, as well as with the level of turnover in the sector enabling to resume past coworking ties (Levin et al., 2011; Godechot, 2014).

Appendix A.

Tables A1–A6.

Table A1
Types of contacts and supports.

	Reference letters		Public support in faculty assembly		Either form of support	
	Percentage	Links	Percentage	Links	Percentage	Links
EHESS PhD advisor	39%	404	39%	392	57%	235
Other members of the PhD committee	16%	513	23%	478	31%	337
PhD committee invitation link	16%	313	31%	225	41%	157
Coauthor	11%	1043	23%	604	28%	468
Same PhD advisor link	8.3%	625	16%	472	24%	342
Same discipline	1.6%	55,237	4.3%	48,641	5.1%	35,973
Reference letters	(100%)	1761	52%	1002	(100%)	1002
Viva voce support in assembly	14%	3771	(100%)	4968	(100%)	3771
Letters or viva voce	35%	5010	91%	5448	(100%)	4251
Letters or viva voce in $t-1$	27%	1646	39%	1610	49%	1348
All faculty members	0.46%	383,467	1.44%	344,594	1.78%	239,147

Table A2
Probability for EHESS PhDs to apply depending on the advisor's membership in the electoral commission.

Variables	1 (Logit)	2 (OLS)
Number of years since PhD	0.156** (0.073)	0.0007 (0.0006)
Squared number of years since PhD	−0.022*** (0.006)	−0.0001** (0.00004)
Advisor drawn member of the EC	0.184 (0.183)	0.003 (0.003)
Advisor ex officio member of the EC	−0.012 (0.227)	−0.00008 (0.002)
Advisor member of the EHESS	0.403*** (0.101)	0.004** (0.0009)
Competitive exam fixed effects	Yes	Yes
Field	Asst. prof. exams	Asst. prof. exams
Number of potential applications	41,530	41,530

Note: The probability of applying to an assistant professor competitive exam is modeled with logistic regression (1) and OLS (2). Cluster-robust standard errors (by exams) in parentheses.
* $p<0.1$.
** $p<0.05$
*** $p<0.01$
Field: we selected the population of EHESS PhDs who completed their PhD during the fifteen years preceding the EHESS exam (we excluded those who were already hired at the EHESS).

[18] Limited statistical power prevented me from identifying a significant weak-tie effect in my data (aside from the letters of viva voce supporters, which are more difficult to interpret in terms of tie strength). But relying on Zinovyeva and Bagues (2015), I can make the following comparison: the advisor effect (+14 percentage points) is 4.5 times that of another member of the PhD committee. Since PhD committees have an average of four non-advisor members, the expected influence of the other members could reach that of the advisor.

[19] According to PSID 1978 gross results, 60% of the respondents who thought that someone helped them for getting a job refer to an influence mechanism (either direct, inferred or through recommendation) while 30% refer to an information mechanism. When adding those who transmitted information without being viewed as help, we find that influence and information have the same magnitude. Cf. PSID 1978 Codebook. ftp://ftp.isr.umich.edu/pub/src/psid/codebook/FAM1978_codebook.pdf

Table A3
Probability of attending the electoral commission meeting.

Variables	1 (Logit)	2 (OLS)
Drawn substitute member	−1.405*** (0.146)	−0.306*** (0.031)
Ex officio member	1.06*** (0.154)	0.146*** (0.021)
With at least one PhD advisee applying	0.918** (0.404)	0.133*** (0.048)
Drawn substitute member with at least one PhD advisee applying	0.386 (0.884)	0.053 (0.207)
Ex officio member with at least one PhD advisee applying	1.172** (0.459)	0.093*** (0.023)
Competitive exam fixed effects	Yes	Yes
Field	All competitive exams with composition and presence	All competitive exams with composition and presence
N	2820	2820

Note: The probability of attending the electoral commission is modeled both through a logistic regression (column 1) and an OLS model. Cluster-robust standard errors (by exams) in parentheses. The reference category is drawn titular members.
* $p < 0.1$.
** $p < 0.05$
*** $p < 0.01$

Table A4
Probability of being put forward by the electoral commission and probability of having one's PhD advisor drawn for the electoral commission.

Variables	Put forward by EC		Advisor drawn in EC	
	1 (Logit)	2 (OLS)	3 (Logit)	4 (OLS)
Woman	−0.268 (0.164)	−0.028* (0.016)	−0.119 (0.491)	−0.006 (0.085)
Born outside France	−0.434** (0.201)	−0.042** (0.019)	−0.152 (0.702)	−0.013 (0.109)
École Normale Supérieure alumni	0.574*** (0.222)	0.093*** (0.032)	0.829 (0.925)	0.152 (0.158)
Agrégation (High school professor exam)	0.491** (0.206)	0.052* (0.026)	−0.244 (0.968)	−0.025 (0.159)
Already member of the EHESS	0.93 *** (0.172)	0.124 *** (0.022)	−0.631 (0.7)	−0.149 (0.141)
Age	−0.045*** (0.013)	−0.005 *** (0.001)	−0.0003 (0.045)	0.0002 (0.009)
Anthropology	0.21 (0.222)	0.021 (0.025)	0.463 (0.962)	0.074 (0.137)
History	0.291* (0.165)	0.031 (0.019)	0.522 (0.749)	0.089 (0.117)
Sociology	0.011 (0.221)	−0.009 (0.023)	0.401 (0.916)	0.066 (0.134)
Economics	0.095 (0.272)	0.008 (0.031)	0.663 (1.311)	0.081 (0.24)
Number of previous trials	0.487*** (0.173)	0.058*** (0.018)	−0.525 (0.622)	−0.086 (0.107)
Square number of previous trials	−0.034 (0.026)	−0.004 (0.003)	0.105 (0.109)	0.02 (0.018)
Number of publications	0.018*** (0.004)	0.003*** (0.001)	0.015 (0.033)	0.002 (0.004)
Competitive exam fixed effects	Yes	Yes	Yes	Yes
Field	All competitive exams	All competitive exams	All competitive exams. Applications with advisor at EHESS drawn or undrawn	All competitive exams. Applications with advisor at EHESS drawn or undrawn
N	2171	2171	418	418

Note: The probability of being put forward is modeled both through a logistic regression (column 1) and an OLS regression. Cluster-robust standard errors (by exams) in parentheses. The reference category is Ex officio members (member of the bureau or the scientific council).
* $p < 0.1$.
** $p < 0.05$
*** $p < 0.01$

Table A5
Applications put forward by the electoral commission and average share of votes depending on the advisor's membership in the electoral commission.

Applications whose PhD advisor is	Put forward		Share of votes		Put forward		Share of votes		Put forward		Share of votes	
	%	*N*	Mean (s.d.)	*N*	%	*N*	Mean (s.d.)	*N*	%	*N*	Mean (s.d.)	*N*
Randomly drawn member of the EC	34%	62	28.1% (0.334)	62	37 %	54	31.3% (0.345)	54	38%	48	30% (0.329)	48
Member of the EHESS outside electoral commission	20%	357	22.1% (0.262)	357	20 %	365	21.8% (0.26)	365	20%	371	22.1% (0.265)	371
Member of the EC as a member of scientific council	31%	13	30.4% (0.307)	13	31 %	13	30.4% (0.307)	13	31%	13	30.4% (0.307)	13
Member of the EC as member of the bureau	22%	18	31.2% (0.323)	18	22 %	18	31.2% (0.323)	18	22%	18	31.2% (0.323)	18
Outside EHESS	16%	1759	17.5% (0.269)	1744	16 %	1759	17.5% (0.269)	1744	16%	1759	17.5% (0.269)	1744
All applications	17%	2209	28.1% (0.334)	2194	17 %	2209	31.3% (0.345)	2194	17%	2209	30% (0.329)	2194
Definition of the membership the electoral commission	Drawn as titular member or substitute (if available, presence otherwise)				Drawn as titular member (if available, presence otherwise)				Presence (if available, composition otherwise)			

Table A6
Applications put forward by electoral commission depending on the membership of the electoral commission.

Applications whose PhD advisor is:	1	2	3	4	5	6
Randomly drawn member of the EC	0.707** (0.295)	0.736** (0.359)	1.082*** (0.379)	1.251*** (0.463)	1.253** (0.52)	0.827 (0.546)
Ex officio member of the EC	0.32 (0.405)	0.168 (0.509)	0.42 (0.616)	0.019 (0.899)	0.232 (0.728)	1.222 (1.088)
Member of EHESS	0.272 (0.177)	0.403** (0.193)	0.202 (0.271)	0.133 (0.294)	0.169 (0.376)	0.257 (0.374)
Competitive exam fixed effects	No	Yes	Yes	Yes	Yes	Yes
Field	All competitive exams	All competitive exams	All experimental exams	All experimental exams with composition	Asst. prof. experimental exams	Prof. experimental exams
Number of applications [$n1$; $n2$]	2209 [357; 62]	2209 [357; 62]	991 [184; 55]	749 [143; 42]	563 [131; 33]	428 [53; 22]

Note: Logistic regressions. Cluster-robust standard errors (by exams) in parentheses. $n1$ represents the number of applicants whose advisor was eligible but not drawn in the electoral commission, $n2$, the number of applicants whose advisor was drawn in the electoral commissions. Field: experimental exams with both treated ($n2 > 0$) and control applications ($n1 > 0$).
* $p < 0.1$.
** $p < 0.05$
*** $p < 0.01$

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