The Old Boy Network: The Impact of Professional Networks on Remuneration in Top Executive Jobs

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Abstract

We investigate the impact of social networks on earnings using a dataset of over 20,000 senior executives of European and US firms. The size of an individual's network of influential former colleagues has a large positive association with current remuneration. An individual at the 75th percentile in the distribution of connections could expect to have a salary nearly 20 per cent higher than an otherwise identical individual at the median. We use a placebo technique to show that our estimates reflect the causal impact of connections and not merely unobserved individual characteristics. Networks are more weakly associated with women's remuneration than with men's. This mainly reflects an interaction between unobserved individual characteristics and firm recruitment policies. The kinds of firm that best identify and advance talented women are less likely to give them access to influential networks than are firms that do the same for the most talented men.

JEL codes: A14, J16, J31, J33
Keywords: professional networks, gender wage gap, executive compensation, placebo technique

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1 Introduction

The impact of individuals' networks of personal and professional contacts on the development of their careers has been the subject of academic research for several decades and of intense private speculation for much longer than that. The popular saying "it's not what you know, it's who you know" implies that networks of contacts are causally effective in promoting individuals' professional advancement, but measuring the causal impact of these networks has proved extremely difficult. Until recently the only way to obtain information about networks has been through surveys that, while often informative, raise concerns about representativeness. More fundamentally, even if networks are oberved to be correlated with professional advancement, causality has been almost impossible to determine, for the simple reason that "who you know" is likely to be highly correlated with "what you know". Individuals with talents and other characteristics that contribute to their professional success are also likely to build more extensive networks. Their networks might be symptomatic of their talents, leading to correlation between their networks and their success even if their networks in no way contribute to that success.

Measuring this causal impact is important for many reasons. In particular, even if networks contribute to professional advancement by enabling individuals to communicate more effectively to employers their suitability for certain jobs, they may also serve as a mechanism of exclusion of certain categories of people from positions of social, political and economic power. Women and ethnic minorities, for instance, may be statistically under-represented among the economic and political leaders of the industrialised countries not because (or not only because) of conscious prejudice,

ing the data. Thierry Mayer first gave us the idea of using placebo network measures. We had very valuable discussions with Suzanne Scotchmer a few months before her untimely death. We would like to thank all these people, and also Bina Agarwal, Samuele Centorrino, Anna Dreber, Guido Friebel, Yinghua He, Astrid Hopfensitz, Thibault Laurent, Thierry Magnac, Nicolas Pistolesi, Mirjam van Praag, Marie-Claire Villeval, and seminar audiences in Berkeley, Gothenburg, New Delhi, Oslo, Rome, Santiago and Toulouse for very valuable comments and advice. Support through the ANR - Labex IAST is gratefully acknowledged. The usual disclaimer applies.

but also because the effectiveness of networks in promoting the careers of men and majority ethnic groups may implicitly exclude those who are not integrated into these networks.

In this paper we propose a methodology for measuring the causal impact of networks, which we test on a panel dataset of over 20,000 senior executives of over 5,000 European and US firms, representing over 90 per cent of the firms listed in the Standard and Poors 500, NASDAQ 100 and main European indices. We construct measures of the size of individuals' networks of currently influential former colleagues, and show, first, that individuals with larger networks have substantially greater remuneration. In order to establish how much of this statistical association reflects the causal impact of networks rather than the fact that more talented individuals are likely to have been hired by similar firms, we construct placebo measures of individuals who were employed in the same firm at different times. If our estimates of the impact of networks reflected unobserved talents, these placebo measures should have a similar statistical impact on remuneration. As we explain below, there is no theoretical reason for the placebo measures to have a zero or even a small impact, but in fact their impact is very small. Controlling for them in our main estimations does not substantially reduce, and even tends to increase, our estimates of the causal effect of networks. To our knowledge this is the first study in the literature to establish a substantial causal role for professional networks on executive remuneration.

We then apply this technique to comparing the effect of networks on remuneration between men and women. We show that the statistical association of networks with women's remuneration is significantly weaker than for men. It lies at around a half of the male correlation for salaries, and between two thirds and three quarters for non-salary measures of remuneration. This is not mainly because connections are less valuable to women than to men (contrary to what we ourselves argued in an early version of this paper - Lalanne and Seabright, 2011). Though there is some weak evidence of a difference in the remuneration value of men's and women's networks, the main difference appears to reflect an interaction between unobserved individual characteristics and firm recruitment policies. Those firms that are relatively supportive of the most talented women tend to give them access to substantially smaller networks than are available to both male and female employees of firms with more traditional recruitment policies. We also find evidence of "window-dressing" by firms that appoint women to non-executive directorships without doing anything to support talented women in other ways. These findings suggest the persistence of subtle forms of discrimination at the highest level in executive labor markets.

The remainder of this paper is organized as follows. Section 2 summarizes the literature on impact of networks on professional advancement. Section 3 provides information on the data set and the methodology used. Section 4 presents results. Section 5 concludes.

2 The literature on networks and professional advancement

2.1 Networks and executive remuneration

The elite network structure of the individuals holding top corporate positions has attracted considerable attention from researchers. A person who sits on a company board may sit on several other boards and may be an executive in one (or several) of the corresponding firms (or may have been an executive at a previous time). Each such individual typically also has personal connections to board members and top executives in other companies. Recruitment to board and top executive positions often takes place through an informal process, typically involving the role of both professional headhunters and word of mouth recommendations.

The pioneering work of Granovetter (1973) highlighted the importance of social connections in obtaining both jobs and job-related advantages. Recruitment to board-level and top executive positions seems particularly likely to give value to such informal connections. According to Granovetter, the social connections that are the most valuable when looking for a job are not the closest ones but the more distant ones. Strong ties, such as close friends and relatives, are more likely to have similar information concerning job opportunities, while weak ties, such as acquaintances and coworkers, are more likely to move in different social circles and to have access to different information about job and other opportunities.

The association of such connections for individuals in top corporate positions with career advantages has been confirmed empirically by a number of studies (Horton et al., 2012; Liu, 2010; Liu, 2014; Berardi and Seabright, 2011; Brown et al., 2012; Engelberg et al., 2013)[1]. As far as we are aware ours is the first study to identify causality in the way we do, as well as the first to examine the impact of gender, and (apart from Horton et al., 2012) the first to consider remuneration of senior executives in general rather than just of Chief Executives.

Only recently have researchers begun to take seriously the endogeneity issues. Brown et al. (2012) and Horton et al. (2012) mention that ability might be an omitted variable accounting for the association between networks and remuneration. They control for human capital variables (age and tenure). Brown et al. (2012) also include past firm performance as a proxy for CEO reputation and ability. However, these measures do not address the concern that networks may reflect unobserved individual heterogeneity in talent. The few papers that have considered this possibility have used instrumental variables or fixed effects techniques. Engelberg et al. (2013) use school and industry fixed-effects, but these are not equivalent to individual fixed-effects. Renneboog and Zhao (2011) use random effects estima-

[1]Guedj and Barnea (2009), Hwang and Kim (2009), Renneboog and Zhao (2011), Fracassi and Tate (2012), Kramarz and Thesmar (2013) focus more on the impact of individuals' connections on corporate governance outcomes.

tion, which is unlikely to capture the unobserved talent differences we are concerned with here. They also use IV estimation with board size as an instrument for their measures of centrality in networks, though the validity of the exclusion restriction must be open to doubt.

To the best of our knowledge, three papers have used ingenious attempts to deal with unobserved individual heterogeneity, but they do not investigate, as we do, how individual professional networks impact own remuneration of top executives. Liu (2014), in a paper on CEO turn-over, uses the average connectedness of a CEO's initial peers on the current network as an instrument for the CEO's own connectedness. However, it seems doubtful that the characteristics of a CEO's initial peers are unrelated to the CEO's unobserved talent. Zimmerman (2015) is interested in the question who makes it to the top in Chilean companies and, in particular, whether college and high-school ties help getting a top managerial position. He uses a regression discontinuity design (individuals who were just above and just below the admission threshold) together with a difference-in-difference between individuals in the same cohort and same degree program and individuals in the same cohort but different degree program or individuals in different cohorts but in the same degree program to show that such ties matter for reaching the top. Similarly, based on education networks, Shue (2013) exploits the exogenous every-five-years alumni reunions of top executives who were MBA students at Harvard Business School, as well as their random assignment to sections. She shows evidence of similarity in managerial decision making for those individuals who were in the same section, with an even stronger effect following the alumni reunions. She looks at different corporate policies such as acquisition strategy or executive compensation. But she does not trace back how education networks of top executives affect their own compensation; she finds similarity in the way firms, managed by these former MBA students at Harvard Business School, remunerate their team of executives.

2.2 The gender gap and women's networks

In spite of several decades of substantial increase in women's participation in the labor force in industrialized countries, the representation of women in senior corporate positions remains extremely marginal, and the phenomenon of the "glass ceiling" continues to puzzle researchers and lay commentators alike. Although women represent 51.4% of what the US Bureau of Labor Statistics calls "Management, professional and related occupations", in 2010 they made up only 15.7% of board members, and just 2.4% of chief executive officers, of Fortune 500 companies[2]. Empirical studies have also shown that, even for those who reach the top, substantial gender differences in earnings still exist. Among the explanations, various authors have proposed a gender difference in rank and firm size (Bertrand and Hallock, 2001), in area specialization (Smith et al., 2013), in exit rate (Gayle et al., 2012), in career interruptions (Bertrand et al., 2010) and more generally in the preference for flexibility in employment (Goldin, 2014), in the impact of family (Bell et al., 2008), in the structure of compensation (Albanesi and Olivetti, 2006; Kulich et al., 2011) and in the existence of discrimination (Lee and James, 2007; Selody, 2011).

The question whether men and women differ in the structure of their social networks has been investigated in the sociological and psychological literatures (Booth, 1972; Baumeister and Sommer, 1997; Benenson, 1993; Friebel and Seabright, 2011). However, there is little agreement about the extent of any systematic differences (see Seabright, 2012, chapter 7, for an overview). Scholars have also had difficulty distinguishing between the relative importance of gender differences in preferences, as opposed to difference in opportunities and constraints, for forming and using social connections (Moore, 1990; Fisher and Oliker, 1983).

Nevertheless, there is suggestive evidence that women tend to rely rela-

[2]See Seabright, 2012, chapter 5. The figure of 51.4% is for 2009, the statistics on Fortune 500 companies are for 2010. In 2011 the proportion of women chief executives rose to 3.6%

tively more on small social networks of strong relationships, while men tend to build larger groups with weaker types of relationship. This is consistent with evidence from primatology and evolutionary psychology, based on the hypothesis that coalitions reflect different reproductive strategies in prehistory (Low, 2000, chapter 11), though there may be other, purely cultural explanations for the divergence.

These findings have received some support from the managerial literature. In the workplace, women's connections seem to be built in order to respond strategically to the different constraints they face, such as a legitimacy problem (Burt, 1998) or their underrepresentation in top positions (Ibarra, 1993, 1997). There is also evidence that preferences play a role, such as homophily (a preference for interacting with similar others, such as others of same sex - see McPherson and Smith-Lovin, 2001) or mentoring for example (Brass, 1985; Athey et al., 2000). Homophily may compound the effect of female underrepresentation, leading women's networks to differ from males' ones. However, little is known about how much such differences matter for women's professional advancement. One exception is a laboratory experiment by Mengel (2015), investigating gender differences in the strategic use of networks. She finds that networks do indeed display homophily and that males reward their contacts more than females, thereby explaining gender earnings and promotion gaps. Similarly, in their field experiment in Malawi, Beaman et al. (2015) observe that males systematically refer less females for hiring. Males seem to do so because they have poorer information about women's skills and receive more benefits from men. Despite useful insights, evidence on observational data, in particular on access to the top of hierarchies, is still lacking.

Several studies based on interviews of top corporate individuals reveal that women appear lack the relevant informal connections to access top positions (Linehan and Scullion, 2008; Lyness and Thompson, 2000; Metz and Tharenou, 2001) and reap lower benefits in terms of career outcomes from their social networks (Bu and Roy, 2005; Tattersall and Keogh, 2006; Forret and Dougherty, 2004). However, studies in this literature mainly rely on surveys (and are thus inevitably subjective). The surveys are also of relatively few individuals, most of the time from a single organization, which raises issues of representativeness. Our purpose in this paper is to use a substantially larger sample of individuals than has hitherto been possible.

3 Data and Methodology

3.1 Data description

The analysis is based on an original dataset describing the career history of 22,389 top executives of 5,064 European and US companies between 2005 and 2011 and for whom data on demographics, education, networks (of past and present colleagues), career history and remuneration are available. These individuals are a subset of some 300,000 individuals in a larger database provided to us by BoardEx Ltd, a UK supplier of data to headhunting companies (we refer to the latter hereafter as the ”source” database). They consist of current or past board members or senior executives of European and US companies. For firms to be included in the BoardEx source database requires them to reach a market capitalization above 1 million USD. Once this threshold is reached, analysts at BoardEx start collecting data on the career history of top executives and board members working at such companies from their résumés.

The reasons why our analysis sample differs from the source sample are two fold. On the one hand, we explicitly focus on executives and therefore, non-executive board members are not included in our main analyses[3]. Executives and non-executive directors are two very different populations among the senior employees of a company; they have very different roles within the company and also very different salaries. Non-executives typically work

[3]Executive and board member statuses are not exclusive. An executive can also be a board member; the typical example would be the CEO.

part-time and may often hold several directorships simultaneously. Although non-executive directors of one firm may hold executive positions in another, there is a substantial population (making up over 30% of the source dataset) of individuals who hold only non-executive positions. Executives and non-executives are very different in terms of observables and, in particular, in the representation of females and in the effect of networks on their remuneration. Therefore, any sound analysis looking at some career outcome of senior employees has to be done separately for executives and non-executives. We focus here on executives, who are the individuals actually running the companies[4]. On the other hand, salary information is needed for the purposes of this work. By legal requirements, listed firms in the US have to disclose compensation information on their top five earners. Therefore, for each year we only have salary information for the top five earners, who may be different individuals from one year to another for the same company, even though the individuals in question are still working for the company[5]. In addition we often find zero reported salaries for some years, and have difficulty knowing whether this means that the data are not available or that the individual concerned literally drew no salary in the year in question[6]. Therefore, we perform the majority of analyses on a pooled set of observations of available remuneration for the seven years 2005-2011[7,8], and some on the individual

[4]Supplementary Tables 1 and 2 show the equivalent of our main results on executives for non-executives.

[5]Similarly for the other main countries in our sample (UK, France and Germany), compensation information disclosure is required for executive directors. As soon as these individuals are no longer on the board, the company does not need to disclose their compensation packages.

[6]In the latter case, non-salary compensation will be non-zero, driving up the individual among the top five earners of the company. Analyses on non-salary remuneration will also be performed, therefore using a slightly different sample than the sample for the analyses on salaries.

[7]With available information on salary, we have data on 22,389 individuals working for 5,064 firms; with available data on total annual compensation, we have data on 22,553 individuals working for 5,133 firms; with available information on total wealth, we have data on 21,453 individuals working in 4,997 firms. Definitions of remuneration variables are provided in Table 1.

[8]We include year dummies and cluster standard errors at the individual level because several observations for a same individual can be present if the individual was among the top five earners in different years. We discuss later the case of individuals who always are among the top five earners during the period 2005-2011 and that we call core individuals.

year 2008[9]. In particular, we present descriptive statistics for the year 2008, where we have data on 10,740 executive individuals who between them work in a total of 3,268 firms (see Tables 2 and 3).

Although in principle the limited availability of salary information might raise questions about the representativeness of our sample, the firms in our analysis sample represent the overwhelming majority of the firms in the following indexes: S&P 500, NASDAQ 100, FTSE 100, EUROTOP 100, CAC 40 and DAX. Table 4 shows the number of firms from our analysis sample which belong to these indexes. For 2008, 92% of the firms in these indices are represented, while for the full seven-year period the proportion is 91%.

The main originality of this dataset is that we also have information relevant to individuals' social networks. It's important to clarify the characteristics of this information since they affect the inferences that can be drawn from our results. Ideally, in order to study the impact of top business people's social networks on their career, in terms of remuneration or promotion, we would like to have information on their active social contacts. Unfortunately, this kind of information is extremely difficult to obtain for significant numbers of individuals. Most studies of social networks in a business context (see Linehan and Scullion, 2008; Metz and Tharenou, 2001; Tattersall and Keogh, 2006; Forret and Dougherty, 2004) have conducted interviews and collected detailed information about a relatively small number of individuals and their active networks of contacts; these subjects are often employees of the same firm or users of the same professional network (which raises questions about selection). We do not have such data. Instead we have information, based on matching individuals' résumés, about which other members of the BoardEx source database a given individual has overlapped with in the course of his

As for the distinction between executives and non-executives, core individuals are very different from peripheral individuals, who step down in some years from the top five (see Supplementary Tables 3, 4 and 5 for descriptive statistics and regression results on core and peripheral individuals).

[9] 2008 and 2009 being the years for which we have most observations, but 2009 being more likely to be affected by the recession.

or her career. This is effectively a list of "currently influential people" with whom any given individual has had an opportunity to interact; whether that interaction has been actively pursued is evidently not something we are in a position to observe.

In what follows we use the variable name "Connections" to refer to the number of members of the BoardEx source database with whom an individual in our dataset has worked in the same firm at the same time. Notice that the connections are not necessarily to other individuals in our own smaller dataset, which would arbitrarily restrict our measure of the size of individuals' networks by whether or not we have salary information about the executive members of that network. In addition to measuring connections we also construct a measure that weights connections by how recently they occurred and by how long they lasted and that we call "Weighted Connections". Specifically, we weight each connection by the number of years the individuals in question worked together, as well as by the inverse of one plus the number of years since the connection ended. We test to see whether our results are robust to using this weighted measure.

Our measures of individuals' career outcomes for the purposes of this paper are various indicators of remuneration. Individuals' earnings are represented by three components, all measured in thousand of US dollars: salary (base annual pay), total compensation (sum of salary, bonus, value of shares awarded, value of long term incentive programs awarded and estimated value of options awarded) and total wealth (sum of equity held, estimated value of options held and long term incentive programs held). Because individuals may have several jobs each year, we compute a variable "total salary", corresponding to the sum of salaries of all the jobs for each year for each individual (and similarly for the other components of remuneration). There are important differences between men and women in terms of the proportion of total remuneration provided via salary and other mechanisms, a finding that matches what has been reported previously in the literature (Albanesi and Olivetti, 2006; Kulich et al., 2009). As will be seen below, the elasticity of compensation with respect to measures of network size is even larger when we use non-salary compensation measures.

3.2 Methodology

We want to understand whether social networks have an impact on individuals' career outcomes, and if so whether this impact differs between men and women. We regress our measures of individuals' remuneration on our measures of connections, and in some specifications we interact the connections variable with a dummy variable for gender, in order to test whether there is a significantly different impact of networks on remuneration for women than for men. However, there are a number of statistical difficulties with this procedure. First, there is a risk of simultaneity bias because of reverse causality if we simply regress salaries on connections in the current year. For example, while those individuals with more connections in 2008 might as a result have higher salaries in 2008, it might also be true that individuals changing employment in pursuit of higher salaries in 2008 thereby acquire a larger network of contacts in 2008. Instead of using connections in 2008 as explanatory variables, we include in the regression equation their own lagged values four years earlier[10,11].

A second statistical concern is that there may be unobserved characteristics of individuals that determine both the size of their networks and the size of their salary. Suppose, for instance, that job mobility is related to entrepreneurial dynamism: then individuals who accumulate more connections through more frequent changes of job may also independently have the talent to earn higher salaries. Alternatively, suppose certain types of firms

[10]We chose the lag of four years as a reasonable compromise between the need to keep observations in our sample and the wish to eliminate reverse causality concerns. However, experimenting with different lags has made no significant change to any of our estimations (see Supplementary Tables 6 and 7).

[11]Similarly, because we are interested in how the previous connections affect current salary, we consider in the regressions individuals that previously were executives. In other words, we look at how lagged connections of lagged executives affect their current salary.

attract more talented individuals, who thereby accumulate more connections to other influential people even though it is their talent rather than their connections that is making them successful. In principle, if the unobserved characteristics operate as individual fixed effects, panel data estimation can correct for the problem. However, when there are many missing observations, panel data estimation is possible only at the cost of a drastic reduction in the number of individuals who remain in the panel.

In our case the number of executives who remain in the panel drops to 1366 (compared to the 10740 individuals for whom we have data in 2008). Furthermore, this introduces an additional source of bias as the individuals for whom observations are present over many years, whom we call core individuals, are very different from individuals for whom we have missing observations, whom we call peripheral individuals (they are more likely to be male, for one thing). They also have less need of networks since they are typically those who have already secured stable and remunerative employment (65% of core individuals were in 2011 in the same company as in 2005, while this percentage drops to 30% when we consider peripheral individuals)[12].

To deal with the problem of unobserved individual heterogeneity we have therefore sought to conserve the full panel, and to do so we use an insight from the literature on treatment effects in medicine, where treatments are compared to the placebos which capture the various components of the patient-doctor interaction without the administration of the chemical molecule under investigation. It is important to note that there is no reason to expect placebo effects to be zero: they may be positive or negative, small or large. Placebo effects capture the combined impact of everything involved in the treatment except the fact of consuming the particular chemical molecule under investigation. The impact of the molecule (known as the "treatment effect over placebo") is defined by comparing the outcome for patients who receive the molecule plus everything else involved in the treatment, with those who re-

[12]See Supplementary Tables 3, 4 and 5 for descriptive statistics and regression results on core and peripheral individuals.

ceive everything else but not the molecule.

To apply the technique to our problem here, we construct a measure which we call "Placebo Connections", which capture the various characteristics that individuals share with their contacts through being hired by the same employer, except for the fact of having been employed at the same time. It is the fact of having been employed *at the same time* that is our equivalent of the impact of the chemical molecule under investigation, and we have no prior expectations about the impact of everything else involved in being hired by the same firm, which is what the placebo measures capture. For each individual we count all the people in the source database who worked in the same firms but at different times, without overlapping. If differences in individuals' connections were due just to their changing employer more frequently, or to the fact that more talented individuals were employed by certain firms, then placebo connections should be just as effective at explaining salaries as connections. Indeed, the sole difference between placebo connections and connections is that the latter consists of those people who work at the same place *at the same time*. The difference between the coefficient on placebo connections (which is not necessarily expected to be zero) and the coefficient on connections therefore captures as precisely as we believe possible the effect of proximity rather than selection on the strength of individuals' network connections. In what follows we include both connections and placebo connections in our regressions of remuneration (together with a large number of control variables). We can then test the null hypothesis that the coefficient on connections and the coefficient on placebo connections are the same - if this null hypothesis is rejected we can conclude that network connections have a causal impact on remuneration.[13]

In what follows we sometimes use the term "real connections" to emphasize the distinction between our connections variable and our placebo con-

[13]Both connections and placebo connections should be included together in the regression since we are comparing outcomes for individuals who receive everything involved in the treatment except the particular element under consideration with those who receive everything plus the particular element.

nections variable. "Real connections" and "connections" denote the same variable.

4 Results

4.1 Descriptive Statistics

Full details are given in Tables 2 and 3. On average, executive women in our sample are three years younger than executive men (51.4 years old against 54.2 years old in 2008). Their educational attainments slightly exceed those of men: 23.8% of women have a Bachelors degree, 32.7% have a Masters degree and 17.5% have a PhD; while the percentages for men are respectively 22%, 31.8% and 16.4%. The distribution of men's and women's degrees in business subjects are similar. Overall, the broad human capital of executive men and women does not seem very different among the individuals in our sample. A slight educational difference in favor of women is offset by a difference in favor of men in terms of work experience: men have spent an average of 11.8 years in the organization as compared to 9.5 years for women. This is not more than would be expected, though, given the average difference in age. Women's mobility is around the same as men's (on average for 2008, both had moved 2.6 times) and women seem to work for slightly larger firms (the average board size of firms in which women have worked until 2008 is 9 against 8.6 for men).

Our measure of connections reveal that executive women in 2008 have somewhat more of these on average than executive men - 157.6 as against 118.7 for men (the same is true of the lagged values from 2004 we use in the regressions). This may be related to the fact that women tend to work in slightly larger firms. The weighted connections figures show a similar pattern (on average for 2008, women have 330.7 such weighted connections against 298.8 for men). So executive women are clearly not at any disadvantage in terms of their overall number of connections. Placebo connections, which represent the number of individuals who worked in the same firm but at a different moment in time, are quite different between men and women (the average figures for 2008 are 153.7 and 99, for women and men respectively). This might be the result of women working for larger firms or firms with a higher executive labor force turnover.

Table 5 shows that placebo connections are quite strongly correlated with connections, but more weakly with weighted connections. This makes sense given that weighted connections will give higher relative weight to the connections of individuals who have changed firms less often - it is the frequent changers who tend to accumulate more placebo connections.

There are very striking differences in employment outcomes by gender. Executive women earned on average \$269,000 in 2008, while executive men earned on average \$338,000 (the corresponding median earnings are \$226,000 for women and \$280,000 for men). These earnings differences are even larger for total compensation and total wealth. In common with what has been previously found in the literature, executive women are very unlikely to hold senior positions with large decisional power such as CEO or Chairman of the Board. 9.21% of our executive women (already a small minority of the dataset) hold CEO positions as against 24.29% of the men (the figures for Chairman of the Board are 1.63% and 6.93% for women and men, respectively).

At a descriptive level there is a very striking association between network size and remuneration, an association that (once again, descriptively speaking) appears to be stronger for men than for women. This can be seen in Figure 1 and Figure 2. We have divided the sample of executive individuals first by gender and secondly according to their network size, with "Large Network" referring to those individuals who have weakly more than the median of the distribution of connections of all individuals in 2004, and "Small Network" referring to those who have strictly less than the median. For each group we plot the mean annual salary and the mean total annual compensation for each year from 2000 to 2011. First, for a given size of network, men always have higher remuneration than comparable women. Secondly, it seems that the size of networks may make more difference to the remuneration of men than to those of women. Women with large networks earn more than women with small networks, whereas men with large networks seem to earn much more than men with small networks[14]. Testing this hypothesis rigorously is the task of the next section.

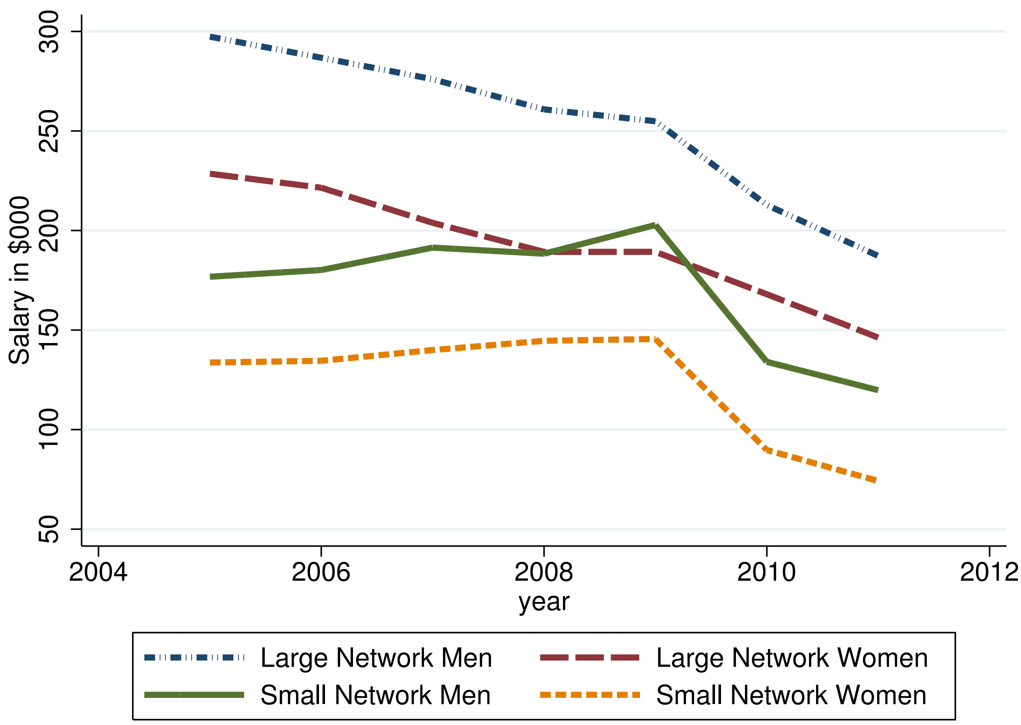

Figure 1: Evolution of salary by network size and gender for executives

[14]In case these average remuneration figures are distorted by the presence of a few very large earners in the sample we have plotted the equivalent of Figure 1 using median earnings for each group. These are available from the authors on request and show qualitatively similar results.

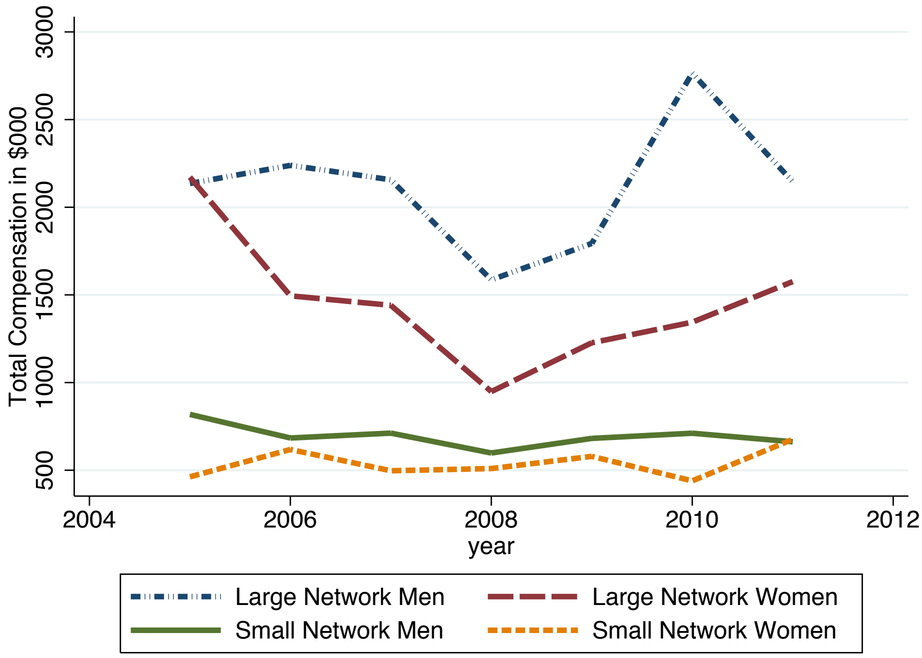

Figure 2: Evolution of total compensation by network size and gender for executives

4.2 Estimating the impact of networks on remuneration for all executives

Table 6 shows how connections and placebo connections compare for a single year (2008). It reports, for individuals who were executives in 2004, regressions of the logarithm of total salary in 2008 on the logarithm of connections in 2004. We use a gender dummy variable and controls for age, age squared, degree level and degree field (in fact, we use dummy variables for bachelors, masters and PhD degrees and for the fields of business, science, social science and finance). We do not at this stage include sectoral or country controls, since these are likely to be to some extent endogenous to individual choices and constitute part of the outcomes that we are seeking to explain. However, as will be seen below the results are qualitatively unchanged when these are included.

Equation I of the table shows that individuals with larger numbers of connections have higher salaries, with an elasticity of a little over 12 per cent. The concern about unobserved individual characteristics can be expressed as follows. Suppose that there is some characteristic (call it "dynamism" to fix ideas) that a) makes those who possess it more likely to be successful professionally and b) though unobservable to the econometrician, can be partly observed by firms, which have a preference for hiring individuals with that characteristic. Some firms will be more successful than others at hiring individuals with dynamism, so that individuals with dynamism will tend to find themselves clustering in firms that have many employees with dynamism. It is natural to conjecture that such firms will give their employees access to larger networks, so that there would be a selection effect on the distribution of connections between individuals. This selection effect would lead to a statistical association between the larger networks and the employees' eventual success even if the networks did not causally contribute in any way to that success. The empirically relevant question is how much of the actual statistical association is due to the selection effect and how much is due to causality.

Equation II appears to indicate that the selection effect is positive but small, of the order 2.5 per cent, and therefore that the majority of the overall statistical association is indeed causal. It shows the result of regressing the logarithm of total salary in 2008 on the logarithm of placebo connections in 2004. However, this specification is inaccurate because it omits real connections, and since we know real connections to be positively correlated with placebo connections it is likely that the coefficient on placebo connections is picking up some of the effect of the omitted variable. Equation III therefore shows the result of including both real and placebo connections as regressors together. The coefficient on placebo connections is now significantly negative, representing an elasticity of around minus 5 per cent. The coefficient on real connections is now substantially higher, at around 20 per cent. The results of the Wald test indicate that we can reject the null hypothesis that the two coefficients are equal at a tiny fraction of one per cent. This is telling us that, far from the statistical association of network size with remuneration being partly due to the selection effect of unobserved characteristics that both increase network size and increase remuneration, the true causal impact is even larger than the apparent impact. How can we interpret this finding?

The explanation seems to be that firms that are relatively successful in employing individuals with positive characteristics (with "dynamism") do not, on average, give their employees access to larger networks. In other words, individuals with success potential tend to work in smaller rather than larger firms, especially early in their careers when they are developing their networks. In contrast, individuals who accumulate larger networks tend, on balance, to be the slightly less dynamic individuals who opt for early employment in larger, and probably less risky firms. The fact that nevertheless their larger networks are associated with substantially greater career success indicates that the causal impact of networks on remuneration is even greater than it initially appeared before correcting for selection. And it is an economically very large effect: individuals can expect to have, other things equal, 2 percent more salary for every 10 percent increase in their number of real connections relative to the mean of the sample.

Table 7 shows our main results for the salary measure of remuneration. Here we pool the observations for all years from 2005 to 2011, and report the parameter estimates for the same specification as Column 3 in Table 6, comparing connections and weighted connections, in each case including placebo connections as well. We add dummy variables for each year and cluster standard errors on individuals. We report results both with an without sectoral and country dummies. The results are clear and very striking. The causal impact of connections has an elasticity of 20 per cent and the causal impact of weighted connections an elasticity of 17 per cent without sectoral and country controls, the coefficients falling slightly to 17 per cent and 15 per cent respectively when the controls are included.

The results for non-salary remuneration are even more striking, as can be seen in Table 8. Total compensation is the sum of salary, bonus, value of shares awarded, value of long term incentive programs awarded and estimated value of options awarded in thousands of USD. Total wealth is the sum of equity held, estimated value of options held and long term incentive programs held in thousands of USD. We consider here again the totals from all jobs held by individuals.

All elasticities are substantially larger than those for salary. The elasticity of total compensation is 51 per cent with respect to connections and 42 per cent with respect to weighted connections. The corresponding elasticities of total wealth are 71 per cent and 61 per cent respectively. The coefficients on placebo connections are either negative or positive but small, indicating that the estimated effects of connections are causal. These are very large numbers indeed.

To give a sense of the economic magnitude of these effects, Figure 3 shows the estimated increase in remuneration that could be expected by individuals at 5 percentile intervals above the median in the distribution of network connections, from the 55th to the 75th percentile. In calculating these effects we have used the coefficients from the pooled regressions for salary, total compensation and total wealth respectively. It can be seen that an individual at the 75th percentile in the distribution of connections could expect to have a salary nearly 20 per cent higher than an otherwise identical individual at the median. The effects on total compensation and total wealth would be substantially higher even than this.

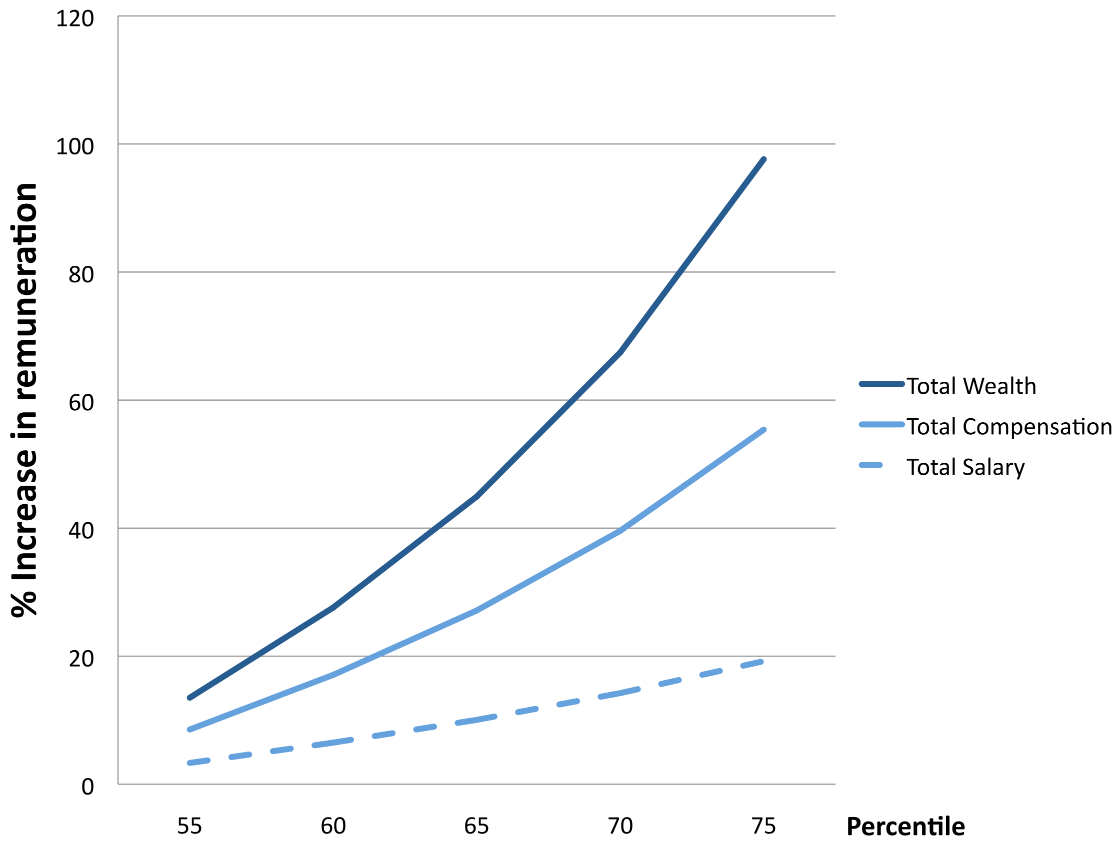

Figure 3: Percentage increase in remuneration implied by percentile increases in connections above the median

A natural question arises whether these network connections enable individuals to be given greater remuneration in their firms, or whether they work mainly by enabling individuals to increase their chances of being hired by high-paying firms. Table 9 answers this question by including as regressors indicators of firm characteristics, notably the number of employees, the firm's market capitalization and its net sales. The effect of these controls is to reduce the coefficient on both connections and placebo connections to small negative values, that are statistically indistinguishable from each other. This clearly indicates that the mechanism by which networks operate to increase remuneration is by increasing the probability that individuals are hired by high-paying firms.[15]

We can conclude, therefore, that the statistical association of network connections with executive remuneration is not due to selection effects, which if anything tend to make the statistical association understate the true causal impact. We turn now to the question whether networks function differently for men and for women.

4.3 Do networks have a different impact on remuneration for women than for men?

In an earlier version of this paper (Lalanne and Seabright 2011) we argued that they do. Equation I of Table 10 shows why this is a tempting conclusion. It shows the statistical association of connections with salary interacted with a dummy variable for women. This interaction term is negative and very highly significant (at well under one per cent), and large in absolute magnitude, implying that network connections are associated with only a little over on third of the increment in salary for women that it brings for men. Furthermore, when the interaction is taken into account the dummy variable for women, which has been consistently negative and very large in absolute terms in our previous estimations without interaction terms (over 35 log points in most specifications) becomes insignificantly different from zero.

[15]Note that this is a purely descriptive regression and cannot be used to infer the net causal impact of networks since the additional controls are all highly endogenous variables.

It is this finding which we earlier interpreted as showing that women's networks were failing to make them as conspicuous as men in an executive labor market which, as has been amply documented, is highly reliant on word of mouth recommendations. Finally, the finding appears robust to the inclusion of placebo connections for all individuals, as shown in Equation II. The inclusion of placebo connections, which has a significantly negative coefficient of a little over 4 per cent, raises the coefficient on connections to around 20 per cent, as in Tables 6 and 7, but has essentially no effect on the interaction of the female dummy with the connections variable.

However, we should not assume that selection effects on unobserved individual characteristics work in the same way for women as for men. After all, if the selection effects are due to choices made by women, those choices may have been made differently from the choices of men. Alternatively, if the selection effects are due to the choices of firm recruiters early in women's careers, those choices may have been made in different ways for female and male job applicants, since gender is observable to recruiters. Equation III therefore shows the effect of entering placebo connections separately for men and for women. Doing so changes the picture substantially. The coefficient on the interaction of the female dummy with placebo connections is significantly negative, indicating that the negative selection effect acts more than twice as strongly for women than it does for men. And there is now no significant difference between the coefficients on the connections and placebo connections terms when interacted with the female dummy, implying that the causal effect of network connections for women is no lower than it is for men. Tables 11 and 12 show essentially the same pattern with respect to our two measures of non-salary remuneration. How can we interpret this finding?

It is clear from our results that selection effects on unobserved characteristics in the determination of the distribution of network connections operate differently for women than they do for men. Three possible types of explanation suggest themselves for this phenomenon:

a) The first type of explanation is a purely statistical bias induced by the existence of a subset of individuals for whom we have no recorded change in firm and who therefore may have few or no placebo connections. The sign of this bias would depend on whether such individuals have higher or lower salaries than others, and even without bias the presence of such individuals would in any case introduce noise into the estimation.

b) The second type of explanation appeals to the preferences of women for the type of firm in which they choose to work early in their careers, and the way in which these preferences are correlated with the unobserved characteristics that determine later professional success ("dynamism"). Suppose that, on average, the more dynamic women are choosing, early in their careers, to work in firms that (perhaps because they are smaller on average, like start-ups) give them access to smaller networks than do those of less dynamic women. One reason might be that less dynamic women are much more risk averse than more dynamic women (and that this difference is larger than the difference between more and less dynamic men), so have strong preferences for working in larger firms. The result would be a stronger negative correlation between dynamism and network size for women than for men. Note that this is entirely compatible with women working in larger firms on average than men: it describes the firm size comparison of more dynamic versus less dynamic women early in their careers, not the comparison between average women and average men.[16]

c) The third type of explanation appeals to the preferences of firms rather than of individuals. Suppose, for example, that some firms are more "female-friendly" than others, and give significantly greater encouragement and op-

[16]Another possibility would be that more dynamic women differ from others not in the type of firm they join but in how often they change jobs. However, even if this were true there is no evidence in our data that it would have any impact on our results. Table 3 shows that men and women executives do not on average have very different numbers of moves in their careers. And Table 16 controls for the number of times individuals move between firms during their careers and the average size of board in the firm in which they work during their career. Doing so has no impact on the qualitative results, which implies that mobility difference cannot be driving our findings.

portunities for talented women to succeed. Suppose, however, that these firms, perhaps because they are younger, or smaller, or less linked to the establishment than others, provide less access to network connections than do less female-friendly firms. Then, independently of women's risk preferences, there would be a tendency for more dynamic women to cluster relatively more than men in female-friendly firms early in their careers, with corresponding implications for the correlation between their later network connections and their unobserved dynamism.

Diagnosing which of these three accounts best matches our data is not a straightforward task (and they are not mutually exclusive). We begin with hypothesis a). Tables 13, 14 and 15 drop from the estimations of Tables 10, 11 and 12 all observations with less than one reported change of firm. In each case the effect of the exclusion of these individuals is to lower somewhat the coefficients on connections and weighted connections. The interaction of the female dummy with weighted connections is now more negative than before, and is statistically significant at 5% for salary and wealth, and at 1% for total compensation. However, the coefficients on unweighted connections remain statistically insignificant at conventional levels, albeit negative. It seems reasonable to conclude that there is some weak evidence for women's networks' being less effective than those of men, but this is not the whole explanation for the weaker statistical association between female networks and remuneration.

Hypothesis b) is very difficult to investigate in the absence of evidence on risk preference or other relevant characteristics . However, we have been able to investigate hypothesis c) in Tables 17 to 19.

Table 17 does three things. First we investigate whether it makes a difference, independently of the firm they currently work in, to what extent women have networks composed of other women. A number of studies have highlighted a positive impact of women in top positions on other women's positions and earnings (Bell, 2005; Cardoso and Winter-Ebmer, 2010; Weber and Zulehner, 2010), though they are not able to determine the mechanism by which such an impact occurs. It may be that women in top positions are mentoring and helping other women in lower positions. The first column of Table 17 therefore reports the same specification as column 1 of Table 7, but with the addition of a variable representing the ratio of women among each individual's connections, as well as the interaction of this variable with the gender dummy.

The inclusion of this variable does not make much difference to the remaining coefficients. Intriguingly, however, executives of either gender benefit from having women among their contacts. Women appear to benefit more than men from this effect, though the difference is not statistically significant. This may be evidence that women are more likely to mentor and help others, including men. It may also be that individuals with more women among their connections have for various reasons tended to work for firms that have a stronger team ethic and whose members are more likely to look after the interests of former colleagues. In the absence of further evidence this can only remain a conjecture.

The two remaining columns of Table 17 explore in more detail the effect of female friendly firms (FFFs) on salary. To answer this question, we use two alternative indicators of a firm's female friendliness: the number of women on the board, and the number of women in the top management team. We include these two new variables in our main specification. Table 18 then shows the impact of connections in enabling individuals to gain access to female-friendly firms as measured by these two variables. The results reveal a paradox: firms with women on their boards help men, and help men more than they help women! More precisely, connections help men to be recruited into firms with more women on their boards, which in turn boost their salary. Equation I of Table 18 shows that connections do not help women to be recruited into such firms, and Equation II of Table 17 shows that the impact of working in such firms on their remuneration, compared to that of working for other employers, is less than half that for men. This seems to corroborate the "window dressing" theory of female non-executive appointments, and suggests that policies designed to increase female board representation may do little by themselves to increase the representation of women in positions of executive power.

Firms with female friendly top management teams are quite different from firms with female friendly boards. Equation III of Table 17 shows that the impact of working in such firms on remuneration, compared to that of working for other employers, is substantial (and highly statistically and economically significant). However, Equation I of Table 18 shows that there is no difference in the effectiveness of men's and women's networks in helping them to be recruited into such firms. And the large negative coefficient on the interaction of the female dummy with placebo connections alerts us to the fact that there is a large negative selection effect on network connections among women recruited into such firms.

This impression is confirmed by Table 19 which provides a correlation matrix of our two measures of female friendliness with other variables including indicators of firm size, and the network connections and placebo connections of those who work for them. The correlation of the number of women in the top management team with these measures is much weaker than the correlation of the number of women on the board.

To see what is going on here we plot kernel densities of the logarithm of network connections for firms for four pairs of comparisons. Figure 4 plots the kernel density of the logarithm of network connections for firms with no women in their top management team on the same graph as the equivalent density for firms with at least one woman in the team, for all the firms in our source database. It can be clearly seen that the density of the latter is substantially left-shifted compared to the density of the former. Figure 5 performs the same comparison for firms with and without at least one woman on their board. There is a striking contrast - having women on the board is associated with access to more connections, while having women in the top management team is associated with having access to fewer connections.

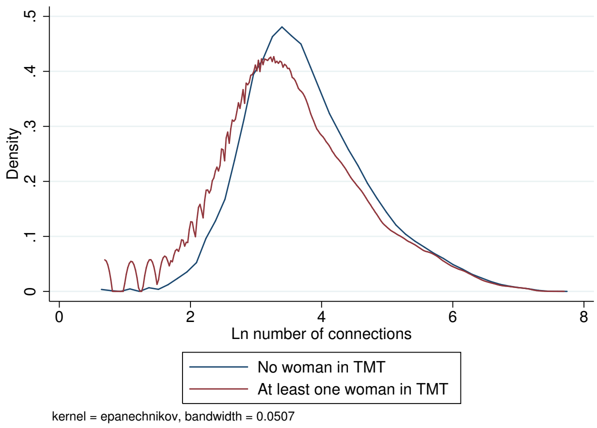

Figure 4: Kernel density estimates of connections for firms with and without women in top management team

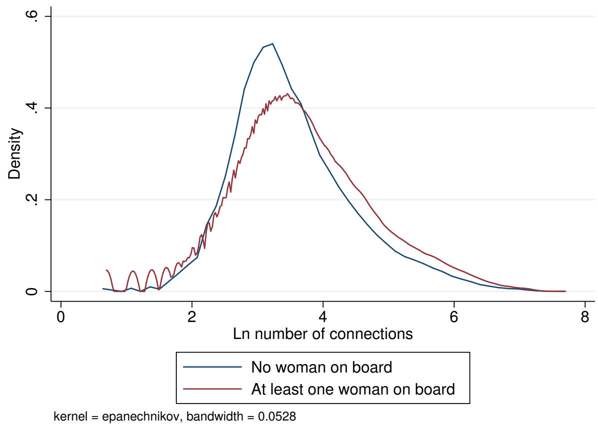

Figure 5: Kernel density estimates of connections for firms with and without women on board

These comparisons are performed for all firms in the source database, while Figures 6 and 7 do so for just the firms in our regressions for which our executives are currently working. Now the density of connections is not left-shifted for firms with women in the top management team compared to all other firms. But the right shift is small - much smaller than the equivalent tight shift in the density for firms with at least one woman on the board - the ones which, paradoxically, help men more than they help women. This confirms the impression of the correlation table that the firms which attract the most dynamic and talented women seem to offer them relatively less access to professional networks than the firms which attract the most dynamic and talented men.

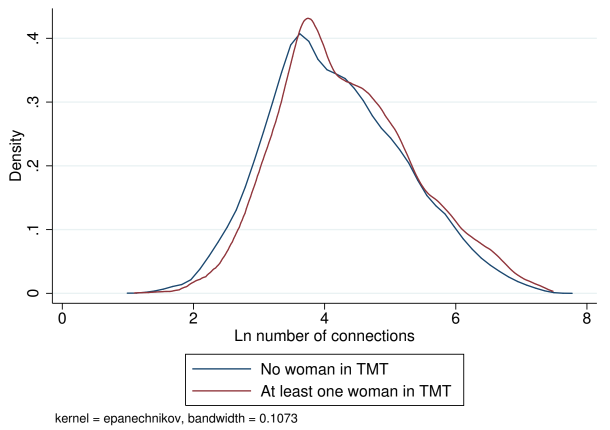

Figure 6: Kernel density estimates of connections for firms with and without women in top management team - Sample of individuals in the main pooled regression

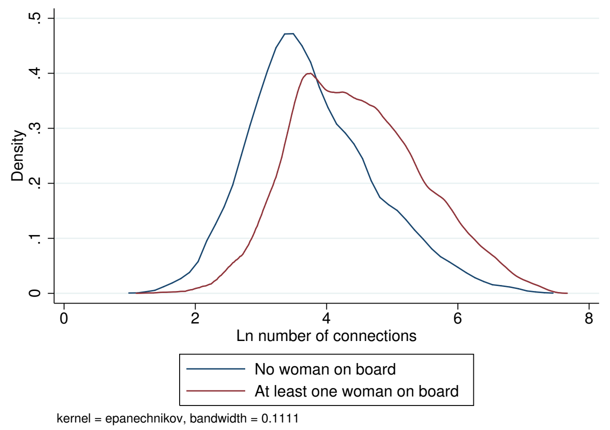

Figure 7: Kernel density estimates of connections for firms with and without women on board - Sample of individuals in the main pooled regression

Overall, the evidence suggests that firms which are successfully supporting talented women are giving them access to fewer connections than those of which support the most talented men. By contrast, firms engaging in window dressing by appointing women purely to board positions continue to favor men's connections in employment, and to pay men more than equivalently talented women. Not surprisingly, talented women tend to be drawn to the former type of firm, and in order to find employment where their talents are rewarded have to make do with lower access to connections that would help them to move on to find better remuneration elsewhere.

5 Discussion and Conclusions

We have found substantial evidence that connections with former colleagues matter for the remuneration of top executives, in the sense that controlling for other factors, individuals who have overlapped professionally with a larger number of currently influential people have higher salaries and non-salary remuneration. Our use of a placebo variable gives us strong reason to believe that our measures of connections are capturing real network proximity between individuals and are not merely proxying for the frequency with which they move and the characteristics of the firms that employ them.

These effects are economically very important: an individual at the 75th percentile in the distribution of connections could expect to have a salary nearly 20 per cent higher than an otherwise identical individual at the median, and the impact on non-salary remuneration is very substantially larger even than this. It is natural therefore to wonder whether an inability of women to mobilize networks to the same extent as men might be responsible for the large differences in executive pay between apparently equally talented men and women. Our findings initially appeared to support this view, because the statistical association between network connections and remuneration is much weaker for women than for men. In fact our placebo technique reveals that the story is more complex. There is indeed some evidence that women's networks are less productive than those of men, but the evidence is relatively weak, and this is clearly not the whole story. A more important phenomenon is the fact that talented women are more likely to be hired by the kinds of firm that are good at finding and nurturing female talent, and these kinds of firm tend to offer women less access to networks of influential individuals than do more traditionally-minded employers.

We have also found evidence for a "window-dressing" policy on the part of some firms, to appoint women to non-executive positions as a substitute for appointing them to executive jobs. If so this suggests that quota policies that fail to distinguish between executive and non-executive positions may have little effect on the distribution of real power within firms. "Window dressing" theory of female non-executive appointments, since firms that have appointed female board members have often done little else to attract, support and reward female talent. This suggests that policies designed to increase female board representation may do little by themselves to increase the representation of women in positions of executive power.

The 2014 AEA Presidential Address by Goldin observes that the gender pay gap is higher within certain occupations, including corporate ones. Her argument is the following: these occupations incur a higher cost for time flexibility and therefore individuals who can afford to work long hours and to take no career breaks are disproportionately rewarded. This raises the intriguing question why the cost of flexibility is so high: is it because working more flexibly reduces individuals' productivity or because individuals who work more flexibly become less visible in the corporate network? Our results provide suggestive evidence in favor of the visibility hypothesis. Working part-time or taking maternity leave may makes such individuals less visible in the corporate network, leading them to obtain less favorable career opportunities, despite the fact that they might be equally talented.

Overall, these results provide very strong support for the hypothesis that network connections have a causal impact on executive remuneration - this is the first study to our knowledge to separate out the causal impact from the effect of selection on unobserved characteristics. Our results also suggest that the use of networks in recruitment enable the persistence of subtle forms of (perhaps unconscious) discrimination in executive labor markets. However, whether the causes of such diminished visibility within corporate networks consist solely of such discriminatory behavior on the part of employers, or also of differential risk aversion on the part of some women, is difficult to judge on the evidence available to us. It may be that the preferences and behaviors of women interact with those of men in complex ways. These suggestions remain conjectural, however, and are an important subject for further research.

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Tables

Table 1: Dependent and Independent Variables

Variables	Description
Total Salary	Sum of salaries of all jobs held in one year
Total Compensation	Sum of salary, bonus, value of shares awarded, value of LTIPs* awarded and estimated value of options awarded for all jobs held in one year
Total Wealth	Sum of equity held, estimated value of options held and LTIPs* held for all jobs held in one year
Connections	Number of individuals in the source database who worked in the same firm in the same year
Placebo Connections	Number of individuals in the source database who worked in the same firm but not in the same year
Weighted Connections	Connections weighted by the number of years of overlap and by the reciprocal of one plus the number of years since the overlapping ended
Sex Ratio	Proportion of connections who are female
Female Friendly Firm Board	Proportion of females on the board
Female Friendly Firm Top Management Team	Proportion of females in the top management team

*Long Term Incentive Programs

Table 2: Human capital and network characteristics by gender for executives in 2008

	Men				Women			
	Median	Mean	Std.Dev.	N	Median	Mean	Std.Dev.	N
Age	54.00	54.17	7.60	10060	51.00	51.36	6.54	680
Number of degrees	2.00	2.00	0.88	8311	2.00	2.09	0.90	571
Degree level: BA (percentage)	-	22.00%	-	10060	-	23.82%	-	680
Degree level: MA (percentage)	-	31.80%	-	10060	-	32.65%	-	680
Degree level: PHD (percentage)	-	16.43%	-	10060	-	17.50%	-	680
Degree field: Science (percentage)	-	1.40%	-	10060	-	0.74%	-	680
Degree field: Social Science (percentage)	-	7.44%	-	10060	-	13.53%	-	680
Degree field: Business (percentage)	-	23.50%	-	10060	-	23.53%	-	680
Degree field: Finance (percentage)	-	9.22%	-	10060	-	6.62%	-	680
Number of connections	62.00	118.7	161.2	10060	72.00	157.6	207.8	680
Mean overlap	2.94	3.17	1.13	10060	2.82	2.99	0.88	680
Mean oldness	4.30	5.00	3.30	10060	4.40	5.04	3.17	680
Weighted connections	207.63	298.8	297.7	10060	218.64	330.7	333.4	680
Placebo connections	25.00	99.0	194.9	10060	37.00	153.7	292.0	680
Sex ratio	0.11	0.11	0.072	10060	0.15	0.15	0.085	680
Number of CEO connections	3.00	5.01	7.27	10060	3.00	6.59	9.09	680

Table 3: Job characteristics by gender for executives in 2008

	Men				Women			
	Median	Mean	Std.Dev.	N	Median	Mean	Std.Dev.	N
Current								
Total salary* (in thousands USD)	279.93	338.39	299.57	10060	225.54	269.47	248.74	680
Total compensation* (in thousands USD)	702.23	1901.60	5582.55	10060	500.68	1294.29	3033.99	679
Total wealth* (in thousands USD)	2031.50	21174.80	459536.47	9355	1136.92	4892.60	14429.46	630
Number of jobs	1.00	1.39	0.78	9960	1.00	1.44	0.77	673
Years in company	9.00	11.83	9.51	10060	7.50	9.47	7.73	680
Years in role	3.70	4.98	4.98	10060	3.20	4.24	3.88	680
Years on board	6.20	8.38	8.07	6624	4.00	5.81	6.21	374
CEO (percentage)	-	24.29%	-	9956	-	9.21%	-	673
CFO (percentage)	-	12.03%	-	9956	-	12.63%	-	673
COO (percentage)	-	5.19%	-	9956	-	2.08%	-	673
Counsel (percentage)	-	3.42%	-	9956	-	8.47%	-	673
President (percentage)	-	3.31%	-	9956	-	3.42%	-	673
Vice President (percentage)	-	15.97%	-	9956	-	24.22%	-	673
Director (percentage)	-	11.23%	-	9956	-	8.77%	-	673
Board Chairman (percentage)	-	6.93%	-	9956	-	1.63%	-	673
Historical**								
Number of moves	2.00	2.55	2.04	10060	2.00	2.59	1.95	680
Average board size	8.00	8.63	3.41	10060	8.60	9.03	2.94	680

*Total compensation measures refer to the sum of compensation measures for all jobs held in 2008. These measures are corrected, when one year compensation measure is missing and the job and firm are the same in the previous and next years, by a linear approximation of previous and next years' compensation measures.

**From beginning of career until 2008.

Table 4: Number of firms from our sample belonging to the main indexes

Year	S&P 500	NASDAQ 100	FTSE 100	EUROTOP 100	CAC 40	DAX
2005	437 (87.4%)	77 (77%)	78 (78%)	75 (75%)	33 (82.5%)	18 (60%)
2006	454 (90.8%)	83 (83%)	84 (84%)	84 (84%)	31 (77.5%)	28 (93.3%)
2007	467 (93.4%)	90 (90%)	88 (88%)	83 (83%)	29 (72.5%)	27 (90%)
2008	466 (93.2%)	93 (93%)	89 (89%)	87 (87%)	34 (85%)	28 (93.3%)
2009	475 (95%)	98 (98%)	91 (91%)	91 (91%)	38 (95%)	28 (93.3%)
2010	493 (98.6%)	100 (100%)	92 (92%)	88 (88%)	36 (90%)	28 (93.3%)
2011	496 (99.2%)	100 (100%)	96 (96%)	86 (86%)	36 (90%)	28 (93.3%)

Before 2009, BoardEx collected remuneration data for every company which disclosed it; after 2009, BoardEx collected remuneration data only for companies listed on the S&P500, NASDAQ 100, FTSE, Eurotop 100, CAC and DAX.

Table 5: Correlation matrix of connections variables for executives in 2008

	Connections	Weighted Connections	Placebo Connections
Connections	1		
Weighted Connections	0.7338	1	
Placebo Connections	0.7551	0.4588	1

Table 6: Determinants of salary in 2008 for executives in 2004

	I	II	III
Ln connections (2004)	0.123***		0.198***
	(0.00958)		(0.0142)
Ln placebo connections (2004)		0.0253***	-0.0578***
		(0.00549)	(0.00806)
Female	-0.424***	-0.407***	-0.419***
	(0.0383)	(0.0385)	(0.0382)
Constant	32.64***	34.79***	32.70***
	(3.299)	(3.318)	(3.291)
Controls	Yes	Yes	Yes
Observations	10737	10737	10737

OLS estimation, standard errors in parentheses

Controls include time in role, time in role squared, age, age squared, degree level, degree field

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Pooled regressions of salary for executives

	I	II	III	IV
Ln lagged connections	0.201***	0.171***		
	(0.00956)	(0.0102)		
Ln lagged weighted connections			0.170***	0.149***
			(0.00672)	(0.00734)
Ln lagged placebo connections	-0.0441***	-0.0348***	0.00329	0.00445
	(0.00535)	(0.00547)	(0.00384)	(0.00384)
Female	-0.362***	-0.376***	-0.369***	-0.381***
	(0.0275)	(0.0272)	(0.0274)	(0.0271)
Constant	78.78***	70.71***	69.60***	64.74***
	(5.656)	(5.777)	(5.628)	(5.719)
Controls	Yes	Yes	Yes	Yes
Country and sectoral dummies	No	Yes	No	Yes
p-value for equality of coefficients	0.000	0.000	0.000	0.000
Observations	66276	66012	66276	66012

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Pooled regressions of non salary remuneration for executives

	Total compensation	Total compensation	Total wealth	Total wealth
Ln lagged connections	0.510***		0.708***	
	(0.0149)		(0.0228)	
Ln lagged weighted connections		0.416***		0.613***
		(0.0103)		(0.0157)
Ln lagged placebo connections	-0.104***	0.0196**	-0.241***	-0.0769***
	(0.00856)	(0.00605)	(0.0130)	(0.00910)
Female	-0.470***	-0.488***	-0.630***	-0.662***
	(0.0407)	(0.0404)	(0.0578)	(0.0570)
Constant	170.5***	147.3***	264.2***	231.5***
	(8.397)	(8.363)	(12.06)	(12.02)
Controls	Yes	Yes	Yes	Yes
p-value for equality of coefficients	0.000	0.000	0.000	0.000
Observations	66991	66991	64093	64093

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Pooled regressions of salary for executives (including firms' number of employees, market capitalization, net sales, sectoral and country dummies)

	I	II	III	IV
Ln lagged connections	-0.0742***	-0.0263*		
	(0.00748)	(0.0103)		
Ln lagged weighted connections			-0.0793***	-0.0584***
			(0.00782)	(0.00786)
Ln lagged placebo connections		-0.0361***		-0.0383***
		(0.00511)		(0.00375)
Female	-0.364***	-0.359***	-0.365***	-0.356***
	(0.0269)	(0.0268)	(0.0269)	(0.0268)
Ln nb of employees	0.0498***	0.0495***	0.0524***	0.0534***
	(0.00649)	(0.00650)	(0.00651)	(0.00653)
Ln market capitalization	0.106***	0.107***	0.105***	0.111***
	(0.00518)	(0.00518)	(0.00512)	(0.00520)
Ln net sales	0.0614***	0.0616***	0.0625***	0.0631***
	(0.00570)	(0.00569)	(0.00573)	(0.00574)
Constant	27.33***	29.54***	30.48***	27.43***
	(5.658)	(5.664)	(5.587)	(5.587)
Controls	Yes	Yes	Yes	Yes
p-value for equality of coefficients		0.491		0.029
Observations	62101	62101	62101	62101

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, sectoral dummies, country dummies, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Pooled regressions of salary for executives

	I	II	III	IV	V	VI
Ln lagged connections	0.152***	0.208***	0.203***			
	(0.00687)	(0.00966)	(0.00984)			
Female*ln lagged connections	-0.0968***	-0.0950***	-0.0284			
	(0.0253)	(0.0253)	(0.0391)			
Ln lagged weighted connections				0.178***	0.175***	0.172***
				(0.00664)	(0.00684)	(0.00686)
Female*ln lagged weighted connections				-0.0853**	-0.0859**	-0.0312
				(0.0262)	(0.0263)	(0.0300)
Ln lagged placebo connections		-0.0439***	-0.0407***		0.00368	0.00731
		(0.00534)	(0.00550)		(0.00384)	(0.00393)
Female*ln lagged placebo connections			-0.0492*			-0.0566***
			(0.0224)			(0.0164)
Female	0.0235	0.0194	-0.0844	0.0617	0.0642	-0.0239
	(0.0992)	(0.0992)	(0.111)	(0.129)	(0.129)	(0.132)
Constant	78.28***	78.80***	78.76***	69.37***	69.65***	69.58***
	(5.665)	(5.654)	(5.654)	(5.614)	(5.627)	(5.626)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
p-value for equality of coefficients		0.000	0.000		0.000	0.000
p-value for equality of female coefficients			0.719			0.530
Observations	66276	66276	66276	66276	66276	66276

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 11: Pooled regressions of total compensation for executives

	I	II	III	IV	V	VI
Ln lagged connections	0.388***	0.519***	0.516***			
	(0.0105)	(0.0150)	(0.0153)			
Female*ln lagged connections	-0.142***	-0.137***	-0.0874			
	(0.0371)	(0.0372)	(0.0601)			
Ln lagged weighted connections				0.439***	0.425***	0.422***
				(0.0100)	(0.0105)	(0.0105)
Female*ln lagged weighted connections				-0.134***	-0.137***	-0.0764
				(0.0380)	(0.0379)	(0.0436)
Ln lagged placebo connections		-0.103***	-0.101***		0.0202***	0.0242***
		(0.00856)	(0.00884)		(0.00605)	(0.00622)
Female*ln lagged placebo connections			-0.0369			-0.0627**
			(0.0342)			(0.0239)
Female	0.0924	0.0818	0.00431	0.191	0.205	0.107
	(0.145)	(0.145)	(0.166)	(0.185)	(0.185)	(0.189)
Constant	169.5***	170.6***	170.5***	145.8***	147.4***	147.3***
	(8.425)	(8.398)	(8.398)	(8.345)	(8.363)	(8.364)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
p-value for equality of coefficients		0.000	0.000		0.000	0.000
p-value for equality of female coefficients			0.572			0.817
Observations	66991	66991	66991	66991	66991	66991

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: Pooled regressions of total wealth for executives

	I	II	III	IV	V	VI
Ln lagged connections	0.416***	0.726***	0.725***			
	(0.0163)	(0.0231)	(0.0236)			
Female*ln lagged connections	-0.142**	-0.129*	-0.113			
	(0.0538)	(0.0539)	(0.0872)			
Ln lagged weighted connections				0.577***	0.631***	0.628***
				(0.0155)	(0.0161)	(0.0162)
Female*ln lagged weighted connections				-0.143*	-0.129*	-0.0797
				(0.0558)	(0.0561)	(0.0642)
Ln lagged placebo connections		-0.244***	-0.244***		-0.0778***	-0.0745***
		(0.0130)	(0.0134)		(0.00912)	(0.00943)
Female*ln lagged placebo connections			-0.0117			-0.0505
			(0.0497)			(0.0343)
Female	-0.0751	-0.112	-0.138	0.0503	-0.00733	-0.0904
	(0.221)	(0.221)	(0.251)	(0.284)	(0.285)	(0.290)
Constant	266.3***	270.0***	270.0***	242.8***	236.8***	236.8***
	(12.18)	(12.09)	(12.09)	(12.02)	(12.02)	(12.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
p-value for equality of coefficients		0.000	0.000		0.000	0.000
p-value for equality of female coefficients			0.437			0.735
Observations	64218	64218	64218	64218	64218	64218

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Pooled regressions of salary for executives (without individuals who never change firm)

	I	II	III	IV	V	VI
Ln lagged connections	0.162***	0.132***	0.129***			
	(0.00946)	(0.0133)	(0.0135)			
Female*ln lagged connections	-0.104**	-0.103**	-0.0572			
	(0.0357)	(0.0356)	(0.0534)			
Ln lagged weighted connections				0.176***	0.142***	0.141***
				(0.00965)	(0.0107)	(0.0107)
Female*ln lagged weighted connections				-0.127***	-0.130***	-0.0995*
				(0.0374)	(0.0370)	(0.0451)
Ln lagged placebo connections		0.0304**	0.0333***		0.0523***	0.0548***
		(0.00971)	(0.00996)		(0.00753)	(0.00770)
Female*ln lagged placebo connections			-0.0483			-0.0429
			(0.0424)			(0.0344)
Female	0.0324	0.0218	0.0320	0.234	0.239	0.268
	(0.161)	(0.161)	(0.161)	(0.196)	(0.195)	(0.196)
Constant	73.98***	72.97***	72.93***	68.31***	68.87***	68.80***
	(8.416)	(8.419)	(8.418)	(8.405)	(8.380)	(8.379)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
p-value for equality of coefficients		0.000	0.000		0.000	0.000
p-value for equality of female coefficients			0.921			0.422
Observations	40145	40145	40145	40145	40145	40145

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14: Pooled regressions of total compensation for executives (without individuals who never change firm)

	I	II	III	IV	V	VI
Ln lagged connections	0.376***	0.352***	0.350***			
	(0.0142)	(0.0203)	(0.0207)			
Female*ln lagged connections	-0.147**	-0.146**	-0.116			
	(0.0534)	(0.0534)	(0.0793)			
Ln lagged weighted connections				0.411***	0.352***	0.350***
				(0.0144)	(0.0161)	(0.0162)
Female*ln lagged weighted connections				-0.189***	-0.195***	-0.171**
				(0.0550)	(0.0544)	(0.0650)
Ln lagged placebo connections		0.0242	0.0261		0.0928***	0.0947***
		(0.0151)	(0.0155)		(0.0115)	(0.0118)
Female*ln lagged placebo connections			-0.0312			-0.0324
			(0.0622)			(0.0497)
Female	0.0906	0.0826	0.0887	0.418	0.430	0.451
	(0.242)	(0.242)	(0.242)	(0.289)	(0.286)	(0.289)
Constant	159.6***	158.8***	158.8***	145.9***	147.2***	147.1***
	(12.20)	(12.20)	(12.20)	(12.18)	(12.13)	(12.13)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
p-value for equality of coefficients		0.000	0.000		0.000	0.000
p-value for equality of female coefficients			0.520			0.170
Observations	40668	40668	40668	40668	40668	40668

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 15: Pooled regressions of total wealth for executives (without individuals who never change firm)

	I	II	III	IV	V	VI
Ln lagged connections	0.405***	0.531***	0.529***			
	(0.0212)	(0.0306)	(0.0312)			
Female*ln lagged connections	-0.137	-0.140	-0.118			
	(0.0764)	(0.0767)	(0.107)			
Ln lagged weighted connections				0.548***	0.570***	0.570***
				(0.0211)	(0.0235)	(0.0237)
Female*ln lagged weighted connections				-0.215**	-0.212**	-0.197*
				(0.0788)	(0.0790)	(0.0895)
Ln lagged placebo connections		-0.126***	-0.124***		-0.0345*	-0.0334
		(0.0222)	(0.0230)		(0.0166)	(0.0172)
Female*ln lagged placebo connections			-0.0222			-0.0194
			(0.0793)			(0.0631)
Female	-0.151	-0.117	-0.115	0.339	0.331	0.341
	(0.351)	(0.352)	(0.353)	(0.423)	(0.423)	(0.426)
Constant	212.7***	217.5***	217.5***	201.2***	200.9***	200.8***
	(16.82)	(16.86)	(16.86)	(16.75)	(16.76)	(16.76)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
p-value for equality of coefficients		0.000	0.000		0.000	0.000
p-value for equality of female coefficients			0.577			0.177
Observations	38245	38245	38245	38245	38245	38245

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level

Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16: Impact of mobility and board size on salary

	I	II	III
Ln lagged connections	0.214***	0.169***	0.189***
	(0.00971)	(0.0101)	(0.0100)
Female*ln lagged connections	-0.0251	-0.0199	-0.0198
	(0.0387)	(0.0390)	(0.0386)
Ln lagged placebo connections	0.0444***	-0.0396***	0.0364***
	(0.00659)	(0.00547)	(0.00660)
Female*ln lagged placebo connections	-0.0518*	-0.0546*	-0.0552*
	(0.0221)	(0.0224)	(0.0222)
Ln nb of moves	-0.484***		-0.435***
	(0.0244)		(0.0250)
Ln avg board size		0.303***	0.203***
		(0.0217)	(0.0224)
Female	-0.0984	-0.105	-0.111
	(0.110)	(0.111)	(0.110)
Constant	68.92***	82.46***	72.40***
	(5.567)	(5.611)	(5.561)
Controls	Yes	Yes	Yes
Observations	66276	66276	66276

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level
Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17: Impact of female friendly firms on salary

	I	II	III
Ln lagged connections	0.193***	0.156***	0.201***
	(0.0101)	(0.0101)	(0.00985)
Female*ln lagged connections	-0.0362	0.0122	-0.0609
	(0.0399)	(0.0394)	(0.0363)
Ln lagged placebo connections	-0.0387***	-0.0355***	-0.0407***
	(0.00551)	(0.00544)	(0.00550)
Female*ln lagged placebo connections	-0.0459*	-0.0571*	0.0104
	(0.0224)	(0.0223)	(0.0217)
Lagged sex ratio	0.495***		
	(0.0845)		
Female*lagged sex ratio	0.537		
	(0.315)		
Nb females on board		0.126***	
		(0.00619)	
Female*nb females on board		-0.0685**	
		(0.0242)	
Nb females in TMT			0.00956
			(0.0120)
Female*nb females in TMT			0.521***
			(0.0427)
Female	-0.157	-0.181	-0.753***
	(0.113)	(0.111)	(0.110)
Constant	79.57***	80.21***	77.40***
	(5.643)	(5.604)	(5.616)
Controls	Yes	Yes	Yes
p-value for equality of coefficients	0.000	0.000	0.000
p-value for equality of female coefficients	0.869	0.232	0.192
Observations	66276	66012	66012

Pooled OLS estimation, standard errors in parentheses, clustered at the individual level
Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18: The role of connections in giving access to female friendly firms

	Nb Females on Board	Nb Females in TMT
Ln lagged connections	0.735***	0.253***
	(0.0195)	(0.0237)
Female*ln lagged connections	-0.513***	-0.0951
	(0.0561)	(0.0642)
Ln lagged placebo connections	-0.106***	-0.0697***
	(0.0112)	(0.0144)
Female*ln lagged placebo connections	0.172***	-0.216***
	(0.0345)	(0.0368)
Female	2.664***	3.663***
	(0.158)	(0.176)
Controls	Yes	Yes
p-value for equality of coefficients	0.000	0.000
p-value for equality of female coefficients	0.000	0.208
Observations	73667	73667

Pooled Ordered Logit estimation, standard errors in parentheses, clustered at the individual level
Controls include time in role, time in role squared, age, age squared, degree level, degree field, year dummies
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 19: Correlation matrix

	Female Friendly Board	Female Friendly TMT	Nb Females on Board	Nb Females in TMT	Ln employees	Ln market capitalization	Ln total assets	Ln net sales	Ln nb connections	Ln placebo connections
Female Friendly Board	1									
Female Friendly TMT	0.2220	1								
Nb Females on Board	0.8886	0.1440	1							
Nb Females in TMT	0.2951	0.7966	0.2540	1						
Ln employees	0.2535	-0.0868	0.3872	0.0411	1					
Ln market capitalization	0.2342	-0.0958	0.3830	0.0459	0.7530	1				
Ln total assets	0.2357	-0.1121	0.4190	0.0267	0.7418	0.8701	1			
Ln net sales	0.2582	-0.0954	0.3899	0.0351	0.8687	0.8029	0.8322	1		
Ln nb connections	0.2297	-0.0766	0.3173	0.0657	0.4778	0.5406	0.5124	0.4903	1	
Ln placebo connections	0.1444	-0.0375	0.1996	0.0460	0.3272	0.3652	0.3374	0.3255	0.7345	1