# The Old Boy Network: Gender Differences in the Impact of Social Networks on Remuneration in Top Executive Jobs

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## Abstract

We investigate gender differences in the impact of social networks on earnings using a dataset of over 22,000 senior executives and non-executive board members of European and US firms. There is a large positive impact on executive men's remuneration of the number of currently influential individuals they have previously worked with. The impact on executive women's remuneration is significantly weaker: on average women gain around half the benefit that men gain from larger networks. We use a placebo technique to show that our measures reflect genuine connections and not merely unobserved characteristics.

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# 1 Introduction

## 1.1 The puzzle: gender gaps in top executive positions

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[1]. 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).

## 1.2 A possible explanation: gender differences in the impact of social networks

**Social networks and job-related benefits** One factor whose differential impact by gender has not been sufficiently studied is the role of the elite network structure of the individuals holding top corporate positions. 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

[1]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%

personal connections to board members in other companies. Recruitment to board 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 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 value of such connections for individuals in top corporate positions has been confirmed empirically by a number of studies (Horton et al., 2009; Hwang and Kim, 2009; Liu, 2010; Renneboog and Zhao, 2011; Berardi and Seabright, 2011; Fracassi and Tate, 2012; Brown et al., 2012; Engelberg et al., 2013; Kramarz and Thesmar, 2013), though as far as we are aware ours is the first study to examine the impact of gender. As a result, if men and women differ in terms of the size or the composition of their networks, or in the way in which they use these networks for professional advancement, this may have a systematic impact on the gender composition of positions for which such networks matter for recruitment.

**Gender differences in social networks** 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 relatively 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.

**Gender differences in social networks within firms** 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.

**Women's connections at the very top** 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.

## 1.3 Methodology, results and outline of the paper

Our work is based on a data set of more than 22,000 individuals working in high positions in almost 4,000 European and US firms over a 13 year period (from 1999 to 2012). This original data set allows us to create social network measures based on past employment overlap of executives and non-executive board members, contrary to the majority of studies on social networks which only rely on directorship links (Horton et al., 2009; Renneboog and Zhao, 2011). We want to understand whether individuals' connections (the number of other individuals with whom they have previously been in contact) affect their career history.

We construct measures of the number of currently influential people (board members and senior executives) each individual has encountered previously in his or her career. There is a large positive impact on executive men's salaries of the number of currently influential individuals they have previously worked with; the findings are even stronger for non-salaried remuneration.

The impact of networks on executive women's remuneration is significantly weaker: on average women gain around half the benefit that men gain from larger networks, this estimate varying between between a third and two thirds depending on the measures of compensation and of network size used. Comparing our results with a placebo measure of individuals who were employed in the same firms at different times, as well as with a modified network measure adjusted for length and date of overlap, we show that our measures reflect genuine connections and not merely unobserved individual characteristics.

In contrast to executives, non-executive board members do not display systematic gender differences in the impact of networks on remuneration. We explore possible mechanisms, and note that firms which integrate women into executive positions rely less on networks for recruitment.

The remainder of this paper is organized as follows. Section 2 provides information on the data set and the methodology used. Section 3 presents results. Section 4 concludes.

# 2 Data and Methodology

## 2.1 Data Description

The analysis is based on an original dataset describing the career history of more than 22,000 executives and members of the non-executive board of European and US companies between 1999 and 2012 and for whom data on education, career history and remuneration are available. We perform some analyses on a pooled set of observations of remuneration for the seven years from 2005 to 2011, and some on the individual year 2008. For the year 2008, we have data on 22,219 individuals who between them work in a total of 3,760 firms; 10737 of these are executives and it is on executives that most of the analysis is performed. 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 "main" 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 main database, they require a market capitalization above 1 million USD .

There are roughly 4,000 firms in our dataset, and for each firm we have information about all board members. For firms with fewer than five board members we have information on all board members plus the most highly salaried executives where salary information exists, up to a total of five individuals. The dataset contains information about individuals' demographic characteristics such as age, nationality and gender, about their employment history such as earnings and position, and about their education characteristics such as degree obtained, field and university.

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 main database a given individual has overlapped with in the course of his 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.

Definitions of the variables are given in Table 1 and summary statistics in Table 2 for the year 2008. In what follows we use the variable name "Connections" to refer to the number of members of the BoardEx main 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 members of that network.

Data on both salaries and connections are frequently missing in the BoardEx main dataset. 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. When we display results for a single year it is for 2008 (2008 and 2009 being the years for which we have most observations, but 2009 being more likely to be affected by the recession). We conduct the single-year analysis on the subset of 10,737 individuals who were executives in 2004, and for whom all salary and employment network data are available in 2004 and 2008 and all salaries are strictly positive in 2008. However, our main reported results pool the observations for 2008 with those for the three previous and three subsequent years; results are given separately for the seven individual years in the Online Appendix.

Table 2 illustrates summary statistics for our 2008 sample compared to all individuals in the main database for whom we have observations for the variable in question. Our sample has a somewhat higher mean number of connections and salary than the rest of the dataset, and a substantially lower proportion of women (9% as opposed to 14% in the main database). While there is evidently a possibility of selection bias, including survivorship bias, we have no idea of its direction, and no reason to expect the bias to be different for men and women.

## 2.2 Independent and Control Variables

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. 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.

As control variables, we use demographic variables (gender, age and age squared) and education variables (highest degree obtained and field of study). We describe below our estimation methods that investigate whether there is also unobserved heterogeneity among individuals.

# 3 Results

## 3.1 Descriptive Statistics

On average, women in our sample are three years younger than men (55 years old against 58 years old in 2008). Their educational attainments slightly exceed those of men: 20% of women have a Bachelors degree, 34% have a Masters degree and 22% have a PhD; while the percentages for men are respectively 22%, 30% and 19%. The distribution of men's and women's degrees between business and science subjects are similar. Overall, the broad human capital of men and women does not seem very different among the individuals in our dataset. 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 just over 10.9 years in the organization as compared to 8.8 years for women. This is not more than would be expected, though, given the average difference in age.

Our measures of connections reveal that women in 2008 have somewhat more of these on average than men - 170 as against 131 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 larger firms than men. So women are clearly not at any disadvantage in terms of their overall number of connections.

However, there are very striking differences in employment outcomes by gender. Women earned on average \$146,000 in 2008, while men earned on average \$212,000 (the corresponding median earnings are \$69,000 for women and \$104,000 for men). Looking at Figure 1 we see that this difference in total salary narrows slightly but remains large over time. These earnings differences are even larger for total compensation and total wealth. In common with what has been previously found in the literature, women are less likely to hold executive positions, and very unlikely to hold senior positions such as CEO or Chairman of the Board. 4% of our women board members (already a small minority of the dataset) hold CEO positions as against 13% of the men.


Figure 1: Total salary evolution by gender

Before we begin to explore the causes of this gender discrepancy, it's important to note that this level of aggregation hides a major difference between two types of individual in our data set: executives and non-executive board members. We now examine this difference in greater detail.

## 3.2 The Importance of Executive Status

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 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 nearly 60% of our dataset in fact) of individuals who hold only non-executive positions. As

Figure 2 reveals, they have much lower salaries on average than executives, and the gender gap looks very different for the two categories. Indeed, over the period 2000-2011 the gender wage gap among non-executives diminishes and more or less disappears, while among executives it is large and remains so over the whole period[2].

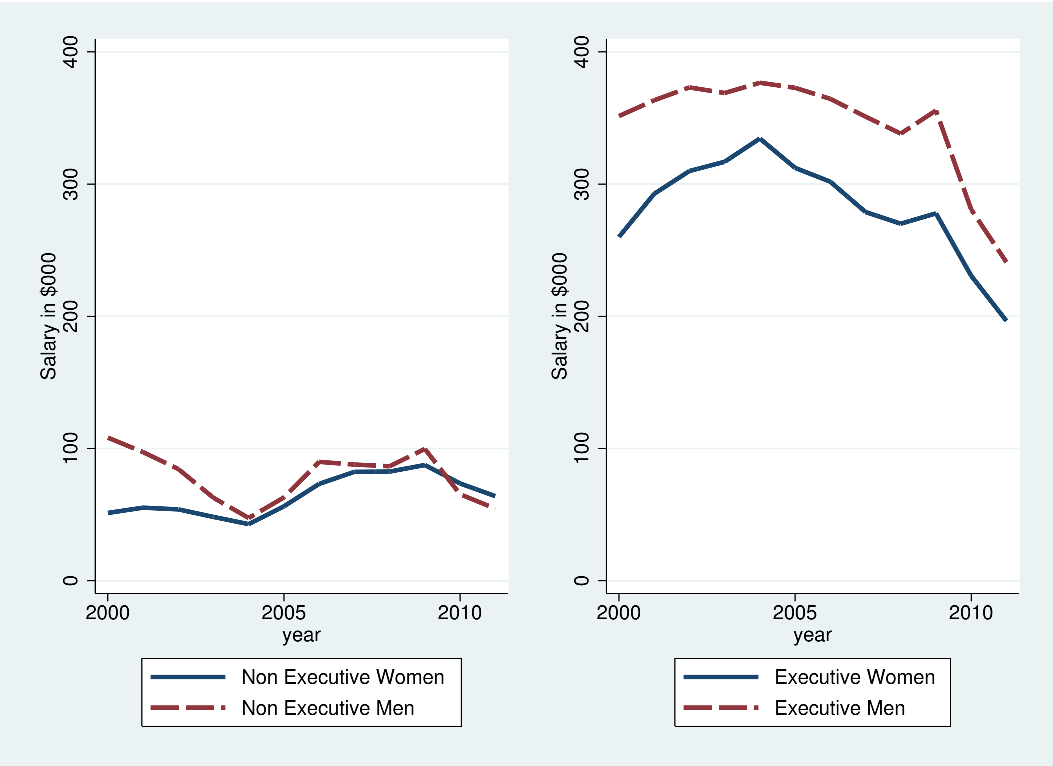


Figure 2: Total salary evolution by gender and executive status

Many more non-executives than executives are women. As Table 3 reveals, only 27% of women in our dataset in 2008 are executives while 43% of men are executives. Since non-executives earn only around one-fifth as much on average as executives, it is essential to take into account the fact that part of the gender gap is a composition effect: women are more likely to be in

[2]It appears in Figure 2 as though the gender gap in earnings is declining over time for both executives and non-executives. It is difficult to test this rigorously since there are different numbers of individuals in different years due to missing observations.

the lower-paid category. We cannot, of course, determine using these data *why* women are more likely to be non-executives. It is possible that different preferences are involved, since non-executive positions typically involve much more flexible working conditions. It is also possible that discrimination is more significant in respect of executive positions, since it is here that real power is exercised in the firm.

The different nature of the gender wage gap for these two groups alerts us to the possibility that the determinants of wages may be different, and in particular that the influence of employment connections on salaries might be very different for executives and non executives. If indeed they play a different role for men and women, it is among executives (where the real gender gap exists) that we should expect to find the evidence.

Figure 3 provides a striking confirmation of this hypothesis. 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 for each year from 2000 to 2011. First, for a given size of network, men always have higher salary than comparable women. Secondly, it seems that the size of networks makes more difference to the salaries 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[3].

Tables 11 to 13 in the Online Appendix provide statistics on human capital, network and job characteristics broken down by both gender and executive status. We now examine whether our conjecture about the differential importance of networks for men and women is corroborated by a more rigorous econometric analysis.

[3]In case these average salary figures are distorted by the presence of a few very large earners in the sample we have plotted the equivalent of Figure 3 (as well as Figure 4 below) using median earnings for each group. These are available from the authors on request and show qualitatively similar results.

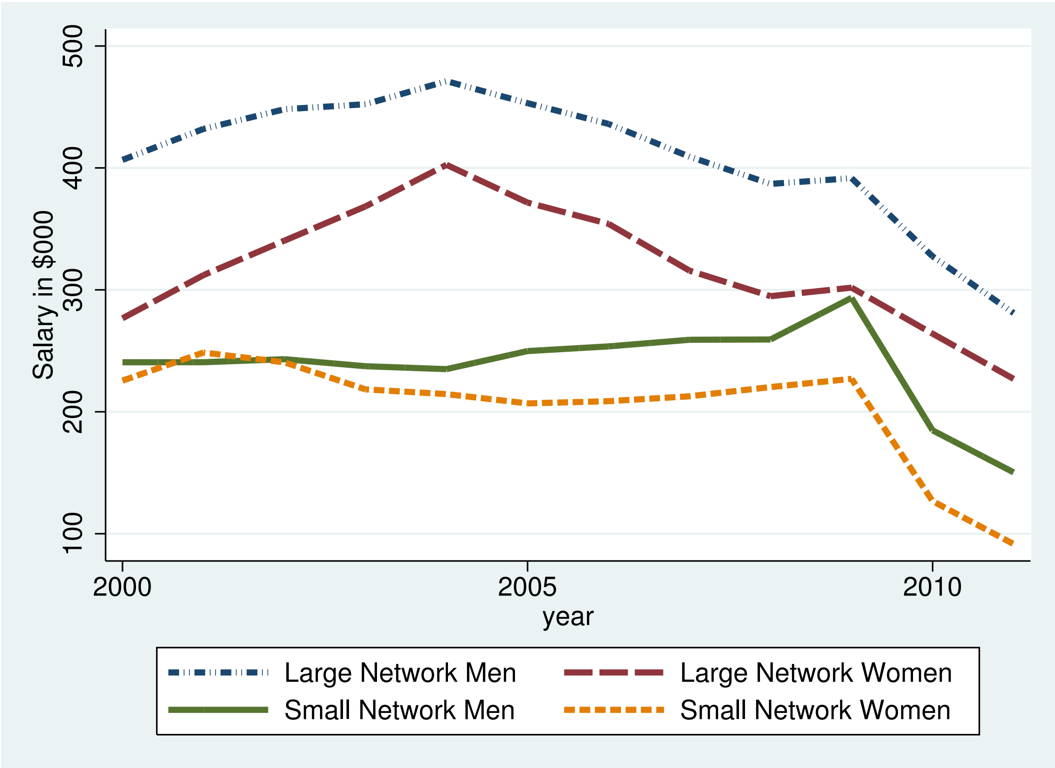


Figure 3: Salary evolution by network size and gender for executives

## 3.3 Impact of networks by gender for executives

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 interact the connections variable with a dummy variable for gender. 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 employment connections in 2008 as explanatory variables, we instrument them with their own lagged values four years earlier[4].

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 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.

There is no perfect way of dealing with this problem, which has not been fully resolved to our knowledge in previous studies of the impact of networks on labor market outcomes[5]. 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 10737 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 are very different from individuals for whom we have missing observations (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. As will be seen below this biases downward (to nearly zero) the parameter estimates for the effect of networks on remuneration when interacted with

[4] 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.

[5] Of the papers that have recognized this difficulty, Hwang and Kim (2009) use past performance as a proxy for ability, a procedure which we ourselves adopted in an earlier version of this paper but which does not remove the bias. Engelberg et al. (2009) use school and industry fixed-effects, but these are not equivalent to individual fixed-effects, and in any case would have little relevance to gender differences. Renneboog and Zhao (2011) use random effects estimation, which is unlikely to capture the unobserved talent differences we are concerned with here.

gender. We opt instead for a different approach, which is diagnostic rather than corrective.

Our approach is inspired by the practice of clinical trials in medicine, where administration of a placebo is compared with administration of the chemical compound in order to separate out the effect of the compound itself from the (possibly therapeutic) effect of participation in a trial, receipt of a pill, discussion with a doctor and so forth. It is not possible to administer the treatment without also including these latter procedures, but the treatment effect with the chemical compound can be compared to the effect of the placebo to infer what additional impact the compound provides.

What we do is, first, to construct a measure which we call "placebo connections". For each individual we count all the people in the main 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.

Secondly, if our measure of connections does indeed measure an impact of proximity over and above the effect of selection, then more prolonged or intense or recent proximity should have a greater effect. Our data do not allow us to measure the intensity of proximity (how much individuals interacted when they worked in the same firm), but they do allow us to measure for how long the individuals worked in the same firm and how recently they did so. We therefore construct a measure which weights 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. If our interpretation of the difference between the coefficient on connections and that on placebo connections is correct, the coefficient on weighted connections should be larger in absolute magnitude than the coefficient on connections.

Table 4 shows how connections and placebo connections compare for a single year (2008), and shows the effect of interacting the connections variable with a gender dummy. It reports, for individuals who were executives in 2004, regressions of total salary in 2008 on connections in 2008 instrumented by their value 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 endogenous to individual choices and constitute part of the outcomes that we are seeking to explain (if, for example, women earn lower salaries because they work for firms in a certain sector we would like to know why they work for in relatively low-paying sectors). However, we compare the results with the addition of sectoral controls in the Online Appendix and they are essentially unchanged.

The model specification is then:

$$\begin{aligned} ln(total\_salary_i) &= \beta_1 + \beta_2 ln(predicted\_connections_i) + \beta_3 female_i \\ &+ \beta_4 female_i * ln(predicted\_connections_i) + \beta_5 age_i \\ &+ \beta_6 age_i^2 + \beta_7 degree_i + \beta_8 degree\_field_i + \epsilon_i \qquad (1) \end{aligned}$$

The first column excludes the interaction of connections with the gender dummy. The second column replaces the connections variable with our placebo connections variable, while the third restores the connections variable and adds the interaction of connections with the gender dummy. Three striking results jump out from the table. First, connections are statistically and economically highly significant determinants of salary, with an elasticity of around 16 per cent. This implies that individuals with 50 per cent more connections - less than half of one standard deviation above the mean

- can expect to have around 8 per cent higher annual salaries. Secondly, placebo connections are utterly insignificant, either economically or statistically. And thirdly, the interaction term between connections and the gender dummy is negative, statistically significant at under one per cent, and large enough in magnitude to offset around three-quarters of the main coefficient. Furthermore, when the interaction term is included the gender dummy itself becomes statistically insignificant.

Table 5 shows our main results for the salary measure of remuneration. Here we pool the observations for all years from 2005 to 2011, and reports the parameter estimates for the same specification as Column 3 in Table 4, comparing connections, weighted connections and placebo connections. We add dummy variables for each year and cluster standard errors on individuals. The results are clear and very striking. First, on average over all years, the elasticity of salary for men is 2.6 per cent with respect to placebo connections, 18.4 per cent with respect to connections and 33.4 per cent with respect to weighted connections. Using the difference between the coefficients and the placebo term as the measure of the true causal impact, this means that individuals with 50 per cent more connections can expect to have 7.9 per cent higher salaries, and those with 50 per cent more weighted connections can expect to have 15.4 per cent higher salaries on average. These are large effects economically, and are statistically significant at a tiny fraction of one per cent.

Secondly, women receive much lower benefits from their connections than men do, the difference being also economically large and statistically significant at a tiny fraction of one per cent. The point estimates of their salary elasticities (given by subtracting the interacted from the uninteracted coefficients) are respectively minus 3.6 per cent with respect to placebo connections, 7.7 per cent with respect to connections and 20.6 per cent with respect to weighted connections. The (small) negative coefficient on placebo connections is not necessarily surprising, and could indicate that more successful women may have tended to work for slightly smaller (and perhaps more entrepreneurial) firms over the course of their careers. These coefficients on connections and weighted connections are very significantly positive but also substantially below the elasticities of men.

Table 6 shows equivalent estimation of pooled regressions for non-salary remuneration. 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 men's total compensation is 10 per cent with respect to placebo connections, 44.4 per cent with respect to connections and 70.7 per cent with respect to weighted connections. The figures for women are 2.9 per cent, 29.4 per cent and 52.3 per cent respectively. The elasticity of men's total wealth with respect to placebo connections is 3.6 per cent, compared to 44.7 per cent with respect to connections and a massive 87.3 per cent with respect to weighted connections. The figures for women are minus 3.6 per cent, 29.2 per cent and 67.2 per cent respectively.

As an illustration, Figure 4 displays the equivalent of Figure 3 for total compensation. It clearly shows that the size of networks makes more difference to the total compensations of men than to those of women. Women with large networks earn more than women with small networks, certainly, but men with large networks earn a lot more than men with small networks.

We report various robustness exercizes in the Online Appendix. These include Tables 14 through 16 which repeat Tables 4 through 6 with the inclusion of sectoral controls; the remain essentially unchanged. Tables 17 through 19 show the salary regressions for each of the individuals years from 2005 through 2011 for the different connection measures; there are some differences between years, but none that cast any doubt on the essential robustness of our main findings. Table 20 further explores robustness by controlling for two other aspects of individual differences that may influence male-female differences: 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. Controling for average board size has essentially no effect at all.

Controling for the number of moves increases the coefficient on connections (rather than decreasing it as might have been expected if individuals who moved more often were both more talented and had larger networks as a result). It has no effect at all on the coefficient on the interaction term. Our findings clearly do not seem to be an artifact of gender differences in respect of either of these two characteristics of individuals.

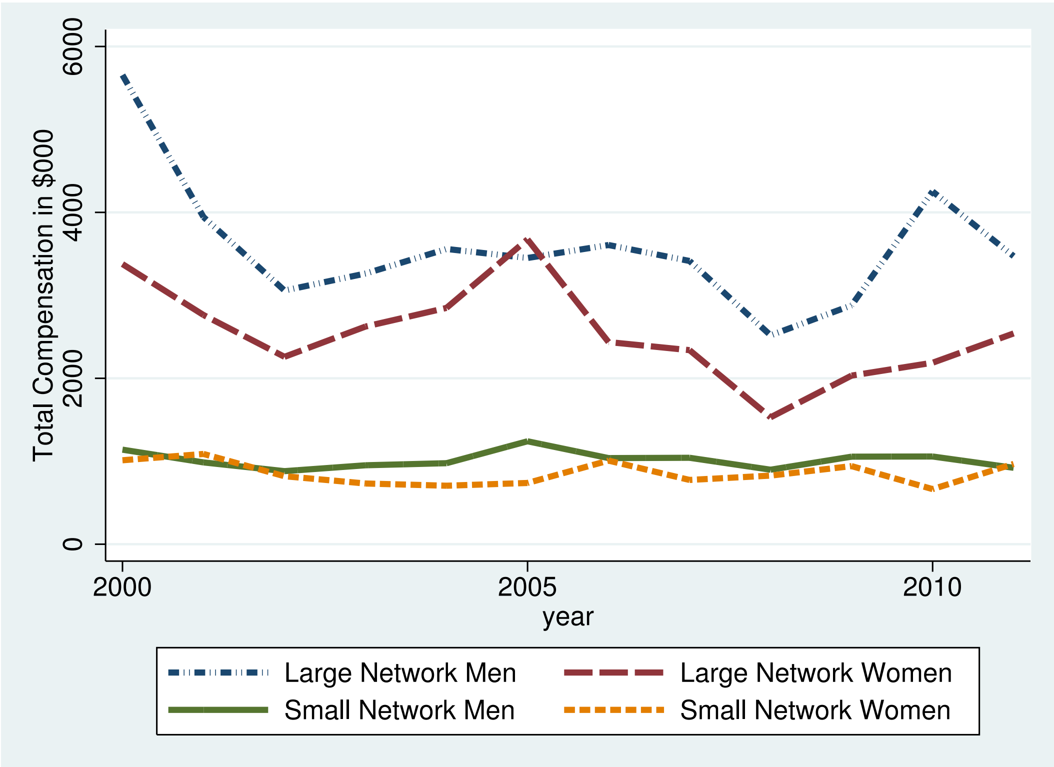


Figure 4: Total compensation evolution by network size and gender for executives

Also in the Online Appendix, Table 21 reports the same specification as the last column of Table 4 but for non-executives, and for all three remuneration measures. Connections are extremely important in determining remuneration, but differential impact of connections by gender can be seen (indeed the coefficient on the interaction term is even positive, though it is offset by a negative coefficient on the dummy variable). Table 13 also reports descriptive statistics from which it can be seen that non-executive women's salaries have a slightly higher median and slightly lower mean than men. Clearly the discrepancy between men and women in respect of salaries is concentrated among executives. Indeed, it seems possible that in the light of public discussion of gender imbalance in the boardroom, a number of firms may be actively recruiting and advancing women to non-executive positions, without doing so to anything like the same extent in respect of executive positions.

How important are networks in explaining the gender imbalance in executive and non-executive remuneration? We use the Oaxaca decomposition technique in order to recover the part of the gap in total salary which is due to gender differences in the magnitudes of the determinants of total salary (such as connections among others), from the part which is due to gender differences in the effects of these determinants on total salary. These two parts are known as the "endowment" and the "coefficient" effects respectively. The main difference with our estimations so far is that the Oaxaca technique estimates separate equations for each group whereas our previous technique estimated a common equation except for the gender dummy and its interactions. Tables 7 and 8 present the results. The gap in total salary seems to be explained mainly by different returns from determinants for executives (such as connections), while for non executives, the (small) gap in total salary seems to be mainly explained by different magnitudes of the determinants themselves (age being the more important). For executive men, returns to connections are positive and significant at the 1% level while for executive women they are about half as great. We do not observe such differential impact of networks for non executives men and women. Again this strongly reinforces the conclusions of our estimations so far.

Overall, therefore, it seems as though the extent of individuals' networks makes a difference to their salaries in a way that is unaffected by gender among non-executives, but among executives is much more beneficial to men. This is consistent with the view that firms are making more efforts at recruitment and advancement of female board members in non-executive than in executive roles; see Daily et al.(1999) and Helfat et al.(2006).

## 3.4 Does the gender composition of networks matter?

One natural question is whether it makes a difference 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 9 therefore reports, for executives, the same specification as column 1 of Table 5, 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 (executive men's network opportunities still appear to benefit them while executive women's do not). 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 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 9 explore in more detail the effect of the sex ratio variable on salary. Is it having more women in your network or is it working for a female friendly firm (FFF) that matters? To answer this question, we build two new variables: a female friendly board variable measuring the percentage of women on the board, and a female friendly top management team variable measuring the percentage of women in the top management team. We include these two new variables in our main specification. Since these are evidently endogenous, we instrument each variable using its own lagged value. Table 22 in the Online Appendix reports the first stage regression results.

The results reveal a paradox: female-friendly firms also help men! More precisely, connections help men to be recruited into so-called female friendly firms, which in turn boost their salary. However, for women, links do not help their recruitment into firms that have female friendly boards, and in any case those firms do not boost women's salaries compared to other employers. Firms with female friendly top management teams do boost women's salaries, but connections do not help women to be recruited into these kinds of firms. This seems to corroborate the "window dressing" theory of female non-executive appointments.

It also suggests that there is a distinction between two broad types of firm: those that use more objective and systematic recruitment procedures, and those that use procedures based more on informal recruitment methods. The former are more likely to recruit on the basis of talent rather than connections, they are likely to pay more, and they are likely to employ more women among their top management teams. It's good for women to be able to join these firms - but their connections are not particularly helpful to them in doing so. Further evidence for this interpretation comes from Reskin and McBrier (2000), who found that more formal recruitment practices lead to a higher share of women holding management jobs, while recruitment through informal networks increases the share of males, and from Bloom et al. (2011), who find that firms with a higher proportion of female managers and more skilled workers, as well as well-managed firms tend to implement more family-friendly workplace practices, which in turn are positively correlated with firm productivity.

Overall, this suggests that, in recruitment, women suffer particularly from a problem of conspicuousness - and their disadvantage is greater the more informal are the recruitment methods used. One last piece of evidence in favor of this interpretation is presented in Table 10. Here we divide individuals into "Core" and "Periphery", the former designating those who are present in every year from 2005 to 2011, while the latter designate those for whom data are missing for at least one year. Although we do not know for certain why the data are missing, it seems likely that individuals who change jobs are more likely to drop out of the dataset. The individuals in the "Periphery" group are therefore more likely to need to make themselves conspicuous to recruiters than are those in the "Core" group.[6]

Intriguingly, it turns out that the disadvantage faced by women in the use of networks is entirely concentrated in the "Periphery" group. Networks are very important in determining salaries for executives in the "Core" group, but they are important for men and women alike. It is in the "periphery" group that women's disadvantage is concentrated. This decomposition of the individuals also shows why a panel regression (which would discard individuals in the periphery group) would bias downward the coefficient on the interaction term to close to zero, as was discussed above.

# 4 Discussion and Conclusions

Using cross-section analysis and several robustness checks we have found substantial evidence that employment connections matter for the remuneration of top executives and non-executive board members, 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 (which has a comparatively negligible effect) 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. This is reinforced by the fact that the coefficient on our weighted connections measure (which gives greater weight to longer and more recent overlap between individuals) is very substantially larger than the coefficient on the unweighted measure.

Two different types of phenomena might explain such results. First, there might exist gender differences in preferences for social contacts or for forms

[6] Milgrom and Oster (1987) develop an invisibility hypothesis, according to which a disadvantaged group (such as women) has less visible skills to employers, and therefore firms have an incentive to under-promote individuals in such group (under the assumption that promotion enhances visibility) to keep them, under-pay them and extract some rents.

of interaction with those social contacts. For instance, as has previously been conjectured, women might be more inclined to build and rely on a few "strong ties", while men might have a preference for a large number of "weak ties". As a result, when considering career evolution, men will be aware of a larger number of job opportunities than comparable women and obtain better labor market outcomes (this is exactly the "strength of weak ties" hypothesis of Granovetter (1973)).

Alternatively, even if the structure of men's and women's networks were the same, women might be less willing to approach their weak ties for help in seeking job opportunities (this would be a variant of the "women don't ask" hypothesis of Babcock and Laschever (2003)). Under either variant of this story, men and women in similar initial position, and given similar numbers of opportunities to meet influential people, might end up with different current employment outcomes due to their different preferences.

The second type of phenomenon might be exclusionary behavior on the part of men, whether consciously through a preference for not admitting women to positions of real power, or unconsciously as a side effect of the greater conspicuousness of other men among the networks of people that predominantly male recruiters turn to when seeking to fill such positions. Either way, old boy networks may exclude women, either through the explicit or implicit preferences of the women or the explicit or implicit preferences of the men.

The recent AEA Presidential Address by Goldin (2014) 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.

Such evidence is not conclusive, however. We cannot conclude from our findings which of these two phenomena - gender differences in preferences or exclusionary behavior - is more important in explaining our results. The truth is likely to involve an interaction of both factors. If the preferences of women were the sole explanation it would be hard to see why they should not apply to non-executive women as well, whereas we can clearly reject the hypothesis that non-executive men and women behave differently. But it does not follow that the preferences of men are therefore the sole cause. It is much more likely that the preferences and behaviors of women interact with those of men, and that men's networks are more likely to exclude women in respect of recruitment to positions of real power in the firm. There appears to be a deliberate "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. These suggestions remain conjectural, however, and are an important subject for further research.

# Tables

Table 1: Variables

| **Variables** | **Description** |
|---|---|
| Connections | Number of individuals in the main database who worked in the same firm in the same year |
| Placebo Connections | Number of individuals in the main 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 |
| 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 |

*Long Term Incentive Programs

Table 2: Sample representativeness for 2008

| Variables | Our sample Mean (Std. Dev.) | Our sample Obs. | Main database Mean (Std. Dev.) | Main database Obs. |
|---|---|---|---|---|
| **Percentage women** | 8.98% | 22 219 | 13.95% | 291 824 |
| **Percentage executives** | 41.38% | 22 219 | 70.72% | 163 041 |
| **Percentage board members*** | 81.19% | 22 026 | 41.97% | 159 220 |
| **Age** | 58.07 (8.89) | 22 219 | 56.72 (4.56) | 321 432 |
| **Connections** | 58.07 (175.17) | 22 219 | 71.27 (113.00) | 231 257 |
| **Weighted connections** | 326.34 (319.34) | 22 219 | 140.29 (200.46) | 231 058 |
| **Total salary** | 205.86 (264.03) | 22 219 | 98.09 (197.73) | 62 797 |
| **Total salary (excluding zero total salary)** | 205.86 (264.03) | 22 219 | 179.83 (238.71) | 34 252 |
| **Total compensation*** | 1 006.66 (3 930.45) | 22 219 | 797.44 (3 246.43) | 35 320 |
| **Total compensation* (excluding zero total (liquid wealth)** | 1 006.66 (3 930.45) | 22 219 | 798.34 (3 248.16) | 35 280 |
| **Total total wealth*** | 12 525.72 (321 102.6) | 19 610 | 10 655.54 (269 432.1) | 31 103 |
| **Total total wealth* (excluding zero total total wealth)** | 12 589.28 (321 915) | 19 511 | 10 762.82 (270 782.8) | 30 793 |

*not included in the main regressions (this is why the number of observations for our sample might differ)

Table 3: Gender by executive status in 2008

| Gender | Non executives | Executives | Total |
|---|---|---|---|
| **Men** | 11 568 (57.20%) | 8 656 (42.80%) | 20 224 (91.02%) |
| **Women** | 1 457 (73.03%) | 538 (26.97%) | 1 995 (8.98%) |
| **Total** | 13 025 (58.62%) | 9 194 (41.38%) | 22 219 (100%) |

Table 4: Determinants of 2008 salary for individuals executives in 2004

| | I | II | III |
|---|---|---|---|
| Ln connections (2008) | 0.156*** | | 0.165*** |
| | (0.0111) | | (0.0114) |
| Ln placebo connections (2008) | | 0.00291 | |
| | | (0.00565) | |
| Female*ln connections (2008) | | | -0.127** |
| | | | (0.0391) |
| Female | -0.373*** | -0.337*** | 0.191 |
| | (0.0409) | (0.0410) | (0.179) |
| Constant | 31.98*** | 35.46*** | 31.68*** |
| | (3.516) | (3.524) | (3.516) |
| Controls | Yes | Yes | Yes |
| Observations | 10737 | 10737 | 10737 |

Standard errors in parentheses

IV estimation with 2004 values of connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 5: Pooled regressions of salary for executives

| | I | II | III |
|---|---|---|---|
| Ln connections | 0.184*** | | |
| | (0.00778) | | |
| Female*ln connections | -0.107*** | | |
| | (0.0289) | | |
| Ln weighted connections | | 0.334*** | |
| | | (0.00947) | |
| Female*ln weighted connections | | -0.128*** | |
| | | (0.0376) | |
| Ln placebo connections | | | 0.0255*** |
| | | | (0.00406) |
| Female*ln placebo connections | | | -0.0612*** |
| | | | (0.0158) |
| Female | 0.133 | 0.351 | -0.0994 |
| | (0.125) | (0.202) | (0.0550) |
| Constant | 99.59*** | 91.76*** | 89.01*** |
| | (5.994) | (5.776) | (5.999) |
| Controls | Yes | Yes | Yes |
| Observations | 66213 | 66212 | 66213 |

Standard errors in parentheses

Pooled IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field, year dummies

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

Table 6: Pooled regressions of non salary remuneration for executives

| | Total compensation | Total compensation | Total compensation | Total wealth | Total wealth | Total wealth |
|---|---|---|---|---|---|---|
| Ln connections | 0.444*** | | | 0.447*** | | |
| | (0.0119) | | | (0.0182) | | |
| Female*ln connections | -0.150*** | | | -0.155** | | |
| | (0.0419) | | | (0.0594) | | |
| Ln weighted connections | | 0.707*** | | | 0.873*** | |
| | | (0.0143) | | | (0.0217) | |
| Female*ln weighted connections | | -0.184*** | | | -0.201** | |
| | | (0.0538) | | | (0.0771) | |
| Ln placebo connections | | | 0.100*** | | | 0.0362*** |
| | | | (0.00652) | | | (0.00970) |
| Female*ln placebo connections | | | -0.0711** | | | -0.0624 |
| | | | (0.0232) | | | (0.0329) |
| Female | 0.194 | 0.527 | -0.157 | 0.0555 | 0.438 | -0.328** |
| | (0.181) | (0.286) | (0.0817) | (0.264) | (0.419) | (0.122) |
| Constant | 190.2*** | 169.3*** | 168.1*** | 278.1*** | 258.3*** | 249.2*** |
| | (8.890) | (8.494) | (9.005) | (12.72) | (12.24) | (12.83) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 67185 | 67184 | 67185 | 64483 | 64482 | 64483 |

Standard errors in parentheses

Pooled IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field, year dummies

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

Table 7: Decomposition Results (pooled data)

| | Executives | Non executives |
|---|---|---|
| Mean prediction (men) | 5.445 | 3.943 |
| Mean prediction (women) | 5.186 | 4.023 |
| Raw differential | 0.258 | -0.080 |
| Due to endowments | -0.082 | -0.080 |
| Due to coefficients | 0.343 | 0.019 |
| Due to interaction | -0.002 | -0.019 |

Table 8: Oaxaca Decomposition (pooled data)

| | Executives | | Non executives | |
|---|---|---|---|---|
| | Men | Women | Men | Women |
| Ln connections (2008) | 0.178*** | 0.098*** | 0.377*** | 0.389*** |
| | (0.004) | (0.018) | (0.004) | (0.009) |
| Controls | Yes | Yes | Yes | Yes |
| Observations | 62053 | 4160 | 60921 | 7671 |

Standard errors in parentheses

IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 9: Impact of gender composition and FFFs on salary (pooled regressions)

| | I | II | III |
|---|---|---|---|
| Ln connections | 0.163*** | 0.151*** | 0.179*** |
| | (0.00453) | (0.00480) | (0.00451) |
| Female*ln connections | -0.110*** | -0.0806*** | -0.0273 |
| | (0.0156) | (0.0156) | (0.0213) |
| Sex ratio | 1.111*** | | |
| | (0.0635) | | |
| Female*sex ratio | 0.354 | | |
| | (0.210) | | |
| Female friendly board | | 1.158*** | |
| | | (0.0769) | |
| Female*female friendly board | | -0.928*** | |
| | | (0.262) | |
| Female friendly TMT | | | 0.953*** |
| | | | (0.175) |
| Female*female friendly TMT | | | 1.010** |
| | | | (0.382) |
| Female | 0.0523 | 0.0917 | -0.408*** |
| | (0.0714) | (0.0785) | (0.122) |
| Constant | 35.08*** | 34.05*** | 35.48*** |
| | (1.398) | (1.404) | (1.409) |
| Controls | Yes | Yes | Yes |
| Observations | 66213 | 65552 | 65517 |

Standard errors in parentheses

IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 10: Pooled regressions of salary for executives (core and periph)

| | All | Core | Periph |
|---|---|---|---|
| Ln connections | 0.184*** | 0.292*** | 0.143*** |
| | (0.00778) | (0.0179) | (0.00826) |
| Female*ln connections | -0.107*** | -0.0140 | -0.107*** |
| | (0.0289) | (0.0654) | (0.0291) |
| Female | 0.132 | -0.108 | 0.154 |
| | (0.125) | (0.313) | (0.127) |
| Constant | 99.57*** | 197.0*** | 96.11*** |
| | (5.993) | (12.73) | (6.662) |
| Controls | Yes | Yes | Yes |
| Observations | 66213 | 9562 | 56651 |

Standard errors in parentheses

Pooled IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field, year dummies

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

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# Online Appendix

Table 11: Human capital characteristics by gender and executive status for 2008

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | Mean | Std.Dev. | N | Mean | Std.Dev. | N |
| **Executives** | | | | | | |
| Age | 53.42 | 7.45 | 8656 | 50.85 | 6.43 | 538 |
| Number of degrees | 1.98 | 0.87 | 7028 | 2.05 | 0.87 | 443 |
| Degree level: BA (percentage) | 22.26% | - | 8656 | 25.65% | - | 538 |
| Degree level: MA (percentage) | 30.34% | - | 8656 | 28.07% | - | 538 |
| Degree level: PhD (percentage) | 15.92% | - | 8656 | 17.84% | - | 538 |
| Degree speciality: Science (percentage) | 1.41% | - | 8656 | 0.56% | - | 538 |
| Degree speciality: Social science (percentage) | 7.69% | - | 8656 | 14.13% | - | 538 |
| Degree speciality: Business (percentage) | 22.37% | - | 8656 | 21.75% | - | 538 |
| Degree speciality: Finance (percentage) | 9.76% | - | 8656 | 7.62% | - | 538 |
| **Non executives** | | | | | | |
| Age | 62.04 | 8.14 | 11568 | 56.90 | 7.86 | 1457 |
| Number of degrees | 2.14 | 1.02 | 9514 | 2.35 | 1.09 | 1205 |
| Degree level: BA (percentage) | 22.28% | - | 11568 | 18.12% | - | 1457 |
| Degree level: MA (percentage) | 30.39% | - | 11568 | 35.90% | - | 1457 |
| Degree level: PhD (percentage) | 21.84% | - | 11568 | 23.61% | - | 1457 |
| Degree speciality: Science (percentage) | 1.77% | - | 11568 | 1.24% | - | 1457 |
| Degree speciality: Social science (percentage) | 8.85% | - | 11568 | 10.71% | - | 1457 |
| Degree speciality: Business (percentage) | 20.69% | - | 11568 | 21.83% | - | 1457 |
| Degree speciality: Finance (percentage) | 5.42% | - | 11568 | 3.16% | - | 1457 |

Table 12: Network characteristics by gender and executive status for 2008

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | Mean | Std.Dev. | N | Mean | Std.Dev. | N |
| **Executives** | | | | | | |
| Number of connections | 104.7 | 148.1 | 8656 | 128.8 | 173.8 | 538 |
| Number of colleagues | 49.6 | 50.9 | 8656 | 53.0 | 54.1 | 538 |
| Mean overlap | 3.21 | 1.18 | 8656 | 3.04 | 0.93 | 538 |
| Mean oldness | 4.88 | 3.25 | 8656 | 4.92 | 3.08 | 538 |
| Weighted connections | 278.4 | 268.7 | 8656 | 299.8 | 304.8 | 538 |
| Sex ratio | 0.11 | 0.074 | 8656 | 0.15 | 0.090 | 538 |
| Closeness* | 0.00034 | 0.000025 | 8656 | 0.00034 | 0.000021 | 538 |
| Betweenness** | 0.000025 | 0.000073 | 8656 | 0.000028 | 0.000071 | 538 |
| Eigenvector*** | 0.0058 | 0.059 | 8656 | 0.0065 | 0.059 | 538 |
| **Non executives** | | | | | | |
| Number of connections | 150.0 | 184.7 | 11568 | 184.7 | 214.2 | 1457 |
| Number of colleagues | 62.7 | 61.3 | 11568 | 77.4 | 69.3 | 1457 |
| Mean overlap | 3.43 | 1.07 | 11568 | 3.31 | 1.01 | 1457 |
| Mean oldness | 4.64 | 3.77 | 11568 | 4.18 | 3.37 | 1457 |
| Weighted connections | 352.3 | 338.7 | 11568 | 414.3 | 393.1 | 1457 |
| Sex ratio | 0.11 | 0.067 | 11568 | 0.14 | 0.069 | 1457 |
| Closeness* | 0.00034 | 0.000017 | 11568 | 0.00034 | 0.000013 | 1457 |
| Betweenness** | 0.000071 | 0.00015 | 11568 | 0.000082 | 0.00017 | 1457 |
| Eigenvector*** | 0.0077 | 0.065 | 11568 | 0.0097 | 0.069 | 1457 |

*Closeness centrality is the inverse of the average distance between and individual and all other individuals in the network.

**Betweenness centrality captures how important an individual is in reducing the distance between all pairs of other individuals.

***Eigenvector centrality is a weighted sum of the direct links an individual has, with the weights being the importance of the individuals in the network.

Table 13: Job characteristics by gender and executive status for 2008

| | Men | | | | Women | | | |
|---|---|---|---|---|---|---|---|---|
| | Median | Mean | Std.Dev. | N | Median | Mean | Std.Dev. | N |
| **Executives** | | | | | | | | |
| Total salary (in thousands USD) | 319.92 | 394.24 | 302.01 | 8656 | 296.73 | 353.19 | 250.83 | 538 |
| Total compensation (in thousands USD) | 942.17 | 2254.96 | 6003.07 | 8656 | 845.39 | 1795.28 | 3443.25 | 538 |
| Total wealth (in thousands USD) | 2627.35 | 21658.66 | 448547.25 | 8265 | 2064.29 | 6461.74 | 16048.70 | 511 |
| Years in company | 10.00 | 12.83 | 9.39 | 8478 | 9.70 | 11.27 | 7.64 | 522 |
| Years in role | 4.10 | 5.31 | 5.17 | 8478 | 3.65 | 4.55 | 4.08 | 522 |
| Years on board | 7.20 | 9.24 | 8.20 | 5114 | 6.70 | 7.76 | 7.14 | 203 |
| Number of moves* | 2.00 | 2.29 | 1.87 | 8656 | 2.00 | 2.25 | 1.73 | 538 |
| **Non executives** | | | | | | | | |
| Total salary (in thousands USD) | 49.59 | 75.18 | 123.57 | 11568 | 51.99 | 69.89 | 62.52 | 1457 |
| Total compensation (in thousands USD) | 83.18 | 145.01 | 349.92 | 11568 | 90.38 | 140.54 | 179.19 | 1457 |
| Total wealth (in thousands USD) | 244.74 | 6031.72 | 191964.98 | 9585 | 213.55 | 4407.20 | 61027.50 | 1249 |
| Years in company | 7.40 | 9.46 | 7.76 | 11476 | 6.00 | 7.88 | 6.41 | 1448 |
| Years in role | 5.50 | 6.99 | 6.08 | 11476 | 5.50 | 6.86 | 5.51 | 1448 |
| Years on board | 7.30 | 9.04 | 7.07 | 11476 | 5.90 | 7.60 | 5.85 | 1448 |
| Number of moves* | 3.00 | 3.65 | 2.82 | 11568 | 3.00 | 3.39 | 2.61 | 1457 |

*from beginning of career until 2008

Table 14: Determinants of executive salary in 2008, with sectoral controls

| | I | II | III |
|---|---|---|---|
| Ln connections (2008) | 0.154*** | | 0.163*** |
| | (0.0113) | | (0.0116) |
| Ln placebo connections (2008) | | 0.00145 | |
| | | (0.00571) | |
| Female*ln connections (2008) | | | -0.122** |
| | | | (0.0392) |
| Female | -0.376*** | -0.341*** | 0.164 |
| | (0.0412) | (0.0412) | (0.179) |
| Constant | 31.47*** | 35.28*** | 32.68*** |
| | (3.521) | (3.528) | (3.521) |
| Controls | Yes | Yes | Yes |
| Observations | 10630 | 10630 | 10630 |

Standard errors in parentheses

IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field, sectors

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

Table 15: Pooled regressions of salary for executives (with sectoral controls)

| | I | II | III |
|---|---|---|---|
| Ln connections | 0.180*** | | |
| | (0.00784) | | |
| Female*ln connections | -0.106*** | | |
| | (0.0290) | | |
| Ln weighted connections | | 0.328*** | |
| | | (0.00959) | |
| Female*ln weighted connections | | -0.128*** | |
| | | (0.0378) | |
| Ln placebo connections | | | 0.0239*** |
| | | | (0.00408) |
| Female*ln placebo connections | | | -0.0626*** |
| | | | (0.0158) |
| Female | 0.120 | 0.337 | -0.106 |
| | (0.126) | (0.203) | (0.0549) |
| Constant | 97.32*** | 90.08*** | 85.65*** |
| | (5.992) | (5.777) | (5.988) |
| Controls | Yes | Yes | Yes |
| Observations | 65967 | 65966 | 65967 |

Standard errors in parentheses

Pooled IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field, sectoral dummies, year dummies

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

Table 16: Pooled regressions of non salary remuneration for executives (with sectoral controls)

| | Total compensation | Total compensation | Total compensation | Total wealth | Total wealth | Total wealth |
|---|---|---|---|---|---|---|
| Ln connections | 0.444*** | | | 0.453*** | | |
| | (0.0120) | | | (0.0183) | | |
| Female*ln connections | -0.150*** | | | -0.159** | | |
| | (0.0421) | | | (0.0594) | | |
| Ln weighted connections | | 0.704*** | | | 0.881*** | |
| | | (0.0145) | | | (0.0219) | |
| Female*ln weighted connections | | -0.186*** | | | -0.215** | |
| | | (0.0541) | | | (0.0769) | |
| Ln placebo connections | | | 0.0989*** | | | 0.0376*** |
| | | | (0.00655) | | | (0.00977) |
| Female*ln placebo connections | | | -0.0734** | | | -0.0622 |
| | | | (0.0233) | | | (0.0328) |
| Female | 0.188 | 0.531 | -0.159 | 0.0520 | 0.491 | -0.354** |
| | (0.182) | (0.288) | (0.0815) | (0.264) | (0.418) | (0.120) |
| Constant | 192.8*** | 172.1*** | 167.5*** | 282.4*** | 264.6*** | 250.1*** |
| | (8.879) | (8.477) | (8.975) | (12.69) | (12.19) | (12.78) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 66936 | 66935 | 66936 | 64230 | 64229 | 64230 |

Standard errors in parentheses

Pooled IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field, sectoral dummies, year dummies

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

Table 17: Determinants of salary in several years for executives

| | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 |
|---|---|---|---|---|---|---|---|
| Ln connections | 0.103*** | 0.129*** | 0.152*** | 0.165*** | 0.176*** | 0.284*** | 0.324*** |
| | (0.0116) | (0.0113) | (0.0113) | (0.0114) | (0.0112) | (0.0127) | (0.0134) |
| Female*ln connections | -0.232*** | -0.0853* | -0.114** | -0.127** | -0.0525 | -0.0795 | -0.101* |
| | (0.0426) | (0.0416) | (0.0402) | (0.0391) | (0.0383) | (0.0417) | (0.0430) |
| Female | 0.632*** | 0.129 | 0.169 | 0.191 | -0.133 | 0.0466 | 0.0155 |
| | (0.184) | (0.181) | (0.179) | (0.179) | (0.173) | (0.195) | (0.204) |
| Constant | 34.99*** | 30.91*** | 29.09*** | 31.68*** | 31.13*** | 47.50*** | 54.59*** |
| | (3.560) | (3.428) | (3.458) | (3.516) | (3.351) | (4.357) | (4.855) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 9925 | 10178 | 10630 | 10737 | 11195 | 7403 | 6145 |

Standard errors in parentheses

IV estimation with 4-year lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 18: Determinants of salary in several years for executives (weighted connections)

| | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 |
|---|---|---|---|---|---|---|---|
| Ln weighted connections | 0.214*** | 0.258*** | 0.302*** | 0.318*** | 0.343*** | 0.450*** | 0.472*** |
| | (0.0155) | (0.0150) | (0.0147) | (0.0149) | (0.0148) | (0.0155) | (0.0159) |
| Female*ln weighted connections | -0.267*** | -0.107 | -0.117* | -0.131* | -0.106* | -0.105* | -0.112* |
| | (0.0580) | (0.0581) | (0.0563) | (0.0551) | (0.0526) | (0.0525) | (0.0541) |
| Female | 1.079*** | 0.330 | 0.294 | 0.335 | 0.206 | 0.270 | 0.173 |
| | (0.314) | (0.316) | (0.307) | (0.304) | (0.289) | (0.291) | (0.303) |
| Constant | 34.54*** | 31.39*** | 30.24*** | 33.37*** | 32.44*** | 49.09*** | 55.56*** |
| | (3.490) | (3.334) | (3.347) | (3.396) | (3.221) | (4.148) | (4.601) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 9925 | 10178 | 10630 | 10737 | 11195 | 7403 | 6144 |

Standard errors in parentheses

IV estimation with lagged values of weighted connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 19: Placebo regressions of salary in several years for executives

| | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 |
|---|---|---|---|---|---|---|---|
| Ln placebo connections | 0.0257*** | 0.0221*** | 0.0109 | 0.00817 | 0.00878 | 0.0586*** | 0.0737*** |
| | (0.00600) | (0.00582) | (0.00572) | (0.00584) | (0.00575) | (0.00732) | (0.00820) |
| Female*ln placebo connections | -0.101*** | -0.0468* | -0.0712*** | -0.0736*** | -0.0420* | -0.0412 | -0.0613* |
| | (0.0225) | (0.0217) | (0.0208) | (0.0204) | (0.0202) | (0.0247) | (0.0271) |
| Female | -0.00872 | -0.0684 | -0.0694 | -0.0869 | -0.193* | -0.114 | -0.154 |
| | (0.0836) | (0.0806) | (0.0794) | (0.0806) | (0.0788) | (0.102) | (0.115) |
| Constant | 35.75*** | 32.14*** | 31.73*** | 36.88*** | 35.96*** | 53.97*** | 63.31*** |
| | (3.564) | (3.434) | (3.463) | (3.523) | (3.365) | (4.445) | (4.992) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 9925 | 10178 | 10630 | 10737 | 11195 | 7403 | 6145 |

Standard errors in parentheses

IV estimation with lagged values of placebo connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 20: Impact of mobility and board size on salary in 2008

| | I | II | III |
|---|---|---|---|
| Ln connections (2008) | 0.262*** | 0.153*** | 0.258*** |
| | (0.0149) | (0.0123) | (0.0166) |
| Female*ln connections (2008) | -0.123** | -0.126** | -0.123** |
| | (0.0384) | (0.0391) | (0.0384) |
| Ln nb of moves (2008) | -0.336*** | | -0.330*** |
| | (0.0335) | | (0.0348) |
| Ln avg board size (2008) | | 0.103** | 0.0155 |
| | | (0.0345) | (0.0352) |
| Female | 0.168 | 0.186 | 0.169 |
| | (0.175) | (0.178) | (0.175) |
| Constant | 27.99*** | 31.99*** | 28.95*** |
| | (3.468) | (3.524) | (3.474) |
| Controls | Yes | Yes | Yes |
| Observations | 10737 | 10737 | 10737 |

Standard errors in parentheses

IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 21: Determinants of compensation in 2008 for non executives

| | Salary | Total Compensation | Total Wealth |
|---|---|---|---|
| Ln connections (2008) | 0.359*** | 0.464*** | 0.383*** |
| | (0.00930) | (0.0110) | (0.0233) |
| Female*ln connections (2008) | 0.0606* | 0.0938** | 0.102 |
| | (0.0260) | (0.0309) | (0.0651) |
| Female | -0.293* | -0.409** | -0.677* |
| | (0.124) | (0.147) | (0.313) |
| Constant | 10.02*** | 7.486* | -45.83*** |
| | (2.487) | (2.982) | (6.914) |
| Controls | Yes | Yes | Yes |
| Observations | 11482 | 11970 | 10365 |

Standard errors in parentheses

IV estimation with lagged values of connections as excluded instruments

Controls include age, age squared, degree, degree field

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

Table 22: Impact of female friendly firms, first stage regressions (pooled regressions)

| | Female friendly board | Female friendly TMT |
|---|---|---|
| Ln connections (lagged) | 0.007*** | -0.001*** |
| | (0.000) | (0.000) |
| Female*ln connections (lagged) | -0.005*** | -0.020*** |
| | (0.001) | (0.001) |
| Female friendly board (lagged) | 0.625*** | |
| | (0.004) | |
| Female*female friendly board (lagged) | -0.055*** | |
| | (0.012) | |
| Female friendly TMT (lagged) | | 0.396*** |
| | | (0.003) |
| Female*female friendly TMT (lagged) | | 0.027** |
| | | (0.008) |
| Constant | 0.852*** | -0.190* |
| | (0.108) | (0.069) |
| Observations | 65552 | 65517 |

Standard errors in parentheses

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