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Essays on Executive Performance, Pay, and Gender Inequality

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Abstract

Top managers are key players in modern economies. This thesis contributes to the understanding of top managers' performance and pay. The first chapter advances the literature on optimal CEO characteristics, by studying the impact of newcomer CEOs on firm performance during a major economic crisis. The second and third chapters focus on the managerial power extracted from professional networks, using an original dataset. While the second chapter quantifies the network pay premium for top managers, the last chapter investigates to what extent networks can be an additional source contributing to the gender pay gap at the top of the corporate ladder.

Keywords: top managers, firm performance, executive pay, networks, gender pay gap.

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Contents

Introduction

Top managers play a pivotal role in setting the management practices of a firm. Not only do top managers set out the goals and strategies of the firm, they condition the corporate environment, affecting the firm's culture and values. Corporate leadership conditions firms' outcome and performance, and ultimately a country's growth and productivity. Bloom and Van Reenen (2010) find that around 30% of differences in productivity across countries and, within countries, across firms can be attributed to management practices. A large body of theoretical work has mapped Chief Executive Officers (CEO) heterogeneity and talent into firm performance. In addition, Hambrick (2007) posits the need to take into account the characteristics of top executives to understand firm performance. In line with the increased availability of data, there has been a surge of empirical research highlighting the relevance of managerial characteristics.

Several studies focus on the observable attributes of top managers, including gender, age, schooling and tenure, to investigate the association to firm performance or to specific management decisions, such as corporate risk-taking or investment. A few authors have investigated how firm performance relates to unobservable characteristics, such as personality traits like flexibility, need for achievement or overconfidence. Other studies have proposed novel measures to proxy executive talent, that vary from estimating manager fixed effects, to identifying fast-track career CEOs, or capturing general ability.

This thesis contributes to the existing empirical literature on the impact of top managers' characteristics on firm performance. The first chapter, written in co-authorship with João Amador and José Tavares, unveils the impact of a specific kind of CEO – a newcomer, defined as a recent external hire – on firm performance during a severe economic crisis. Unlike most studies, we do not assume that the value of a CEO is constant to the firm, irrespective of economic conditions. On the contrary, we assess whether the value of newcomer CEOs changes from normal to crisis times. A key concern in this kind of empirical investigation is the endogeneity of the manager selection, *i.e*, the possibility that results are driven by the non-random assignment of managers to firms. We address this problem by focusing on a crisis period, characterized by an unprecedented economic downturn, exogenous to firms. The premise is that firms choose their CEOs for a set of expectations about the future. However, a major shock that completely changes the framework under which firms operate, will certainly change the desired set of CEO characteristics. Therefore, we consider that the unexpected nature of the sovereign debt crisis in Portugal turns it into a perfect laboratory, as most firms were unable to adjust their CEO choice to the new circumstances.

One of the most important questions all corporations face is which CEO to hire, namely the trade-off between hiring a newcomer and an experienced insider. While a newcomer may bring innovative and bold initiatives to the management of the firm, they will start with a knowledge deficit as to workings of the firm. On the other hand, an experienced insider masters 'business as usual', but might find it difficult to abandon settled management practices when challenging new circumstances arise, and may fall prey to the 'experience trap', as coined by Sengupta et al. (2008). The literature has highlighted the inverted U-shaped impact of CEO tenure on firm performance (for example, Hambrick and Fukutomi 1991, Luo et al. 2013): with time, as CEOs become myopic, risk-averse and overly wedded to early formulas, tenure starts to hurt firm performance. Other researchers have analyzed the impact of hiring an outsider, as opposed to an internal promotion, but have not reached any consensus as to the relative impact on firm performance. In fact, thus far, the literature has mostly ignored how the value of newcomers, who are both new to the firm and job, changes in times of severe crisis, as the risks of an experience trap gain relevance. Impacted by a major shock, old management practices and strategies may be insufficient to innovate and generate growth, exactly when taking risks and adopting creative solutions becomes most valuable.

We estimate the impact of newcomer CEOs on firm performance – measured as gross value added (GVA) or total sales – during the economic and financial assistance program in Portugal, using a very rich micro-level merged employer-employee dataset. We consider having a newcomer in office when the crisis hit as an exogenous event, and use a difference-in-differences estimator in order to estimate the relative performance gap of newcomer CEOs during a major crisis as opposed to normal times. We find newcomers display a slightly negative performance gap in the period prior to the crisis, in line with human capital theories. This suggests that unobservable differences between newcomers and other CEOs, such as risk preferences or different incentives, are not key during normal times, when conducting business as usual is sufficient. Yet, when the crisis sets in, we find evidence that newcomers perform significantly better, with a performance gap between 19% and 22% in value added and sales, respectively. Under extraordinary circumstances, when truly put to test, previous redundant unobservable differences are magnified and play a major role in how CEOs handle the negative shock, outweighing the firm specific knowledge of more tenured CEOs. We also find robust evidence that newcomer CEOs increase the firm's productivity and survivability during the crisis.

Our results question traditional definitions of human capital based on seniority and tenure, arguing for differences in value in times of crisis. At the very least, we suggest that firm specific knowledge is not key in periods of economic distress, when compared to a newcomer CEO's willingness to take risks and innovate. Flexibility, adaptability, or risk preferences – characteristics that we cannot directly observe, but which are more likely to be present among newcomers – are more valuable than experience when firms are faced with a crisis.

Different mechanisms might explain our findings. We conduct a preliminary analysis, with no claim to causality, to map the performance gap into different management practices concerning some key variables. We show that these firms have easier access to long-term debt during the crisis, suggesting a 'signaling' value of hiring a newcomer CEO, and a less risk-averse management strategy (Faccio et al., 2016, Serfling, 2014). Newcomer CEOs are also able to maintain higher levels of firm employment during the crisis, pay higher wages, and incur higher total costs, without loss of efficiency, as they do not present higher costs per unit sold. Future research should focus on deepening our analysis of these and other management practices of newcomer CEOs.

A different branch of literature, but closely related, focuses on explaining executive pay. Compensation of top managers and CEOs has long been a matter of controversy, especially given the tremendous growth observed in CEO pay during the last 3 decades. Bertrand (2009) summarizes 3 theories that have been put forward to explain the developments in CEO pay, namely marked-based theories, the principal-agent theory and the rent extraction theory. Market-based theories are based on the premise that the rise in manager compensation is the outcome of efficient markets, where an increasing demand for good management meets scarce managerial talent. On the other hand, the principal-agent view states that the objectives of a manager might not be fully aligned with the firm, and the growth in compensation can be associated to the simultaneous increase in incentive pay. Finally, the rent extraction theory claims that faults in corporate governance structures offer CEOs the ability to set their own pay. More recently, a few studies have investigated the role of networks in explaining executive pay, where well-connected managers can accrue value to their firms in line with marked-based theories, or simply leverage their networks to increase managerial and bargaining power to obtain rents.

Another source of controversy regarding top manager compensation, concerns differences in pay across gender. In a male-dominated environment, the growing presence of female top managers, raises interest in understanding the magnitude, if any, of the gender bias which is faced by the few high-potential females that have progressed beyond the glass ceiling. Extensive evidence points towards considerable gender pay gaps at the top of the corporate hierarchy (Bell, 2005, Geiler and Renneboog, 2015), with a big part being explained by segregation across firms (Bertrand and Hallock, 2001).

The second and third chapters add to the literature on top managers' compensation, focusing on the role of professional networks, across firms and gender. We construct novel network indicators, spanning three decades of past interactions, where the network of a top manager in a given year is composed of past co-workers, who have themselves become managers. Our definition of networks displays several conceptual advantages over existing approaches in the literature, made possible by the richness of the data. Contrary to most studies our metrics include sizable time variation, aside from the usual cross-sectional dimension. It focuses on past professional encounters, avoiding issues of simultaneity, reverse causality and perfectly overlapping groups. Finally, and most importantly, managers experience changes in their networks when other workers they have met in the past become managers, or leave the job market. Our identification strategy exploits these exogenous changes together with high-dimensional fixed effects.

Building on existing theoretical and empirical evidence, we argue that networks can increase a manager's pay through various channels: (i) large networks reflect reputation and experience and can thus be regarded as an indirect measure of quality (Renneboog and Zhao, 2011), in particular, during the hiring process, under incomplete information about the quality of the match; (ii) large networks also add objectively value to managers due to their position in accessing privileged information; and finally (iii) networks also increase the outside options of managers, therefore conferring greater bargaining power. While the second chapter investigates the impact of networks on pay and differentiates between different 'types' of connections, the third chapter analyses whether networks can be one additional source of explanation for the existing gender pay gap amongst top managers.

In the second chapter, written in co-authorship with José Tavares, we augment a conventional Mincer wage equation with network metrics and high dimensional fixed effects to account for firm and manager time-invariant heterogeneity. Our results suggest that networks have a robust positive impact on total pay: increasing with diminishing returns. A one standard deviation increase in the crude network measure that accounts for the number of connections is associated to a 8% higher bonus - the more volatile component of compensation, while the impact on total pay is around 5%.

We also investigate whether the network premium is derived from how powerful connections are in corporate leadership or rather from how close they are to the manager. By resorting to weighted network metrics, we propose two measures that weigh each connection by the ratio of years the managers coincided in the same firm over the size of the firm – labeled as *Depth* – and by the ratio of the average size of the firms managed over the number of managers in those firms – labeled as *Power*. The first weighted measure intends to proxy the 'inner circle' of the manager, valuing more those connections that the manager most likely knows best. The second measure values more those connections who are the most powerful corporate leaders and who have most likely access to valuable information. We find that deeper connections - *i.e.* longer and closer connections - are more valuable to the manager. More specifically, a one standard deviation increase in *Depth*, increases total pay by around 3%, predominantly through the base wage. Connections to powerful managers are associated to smaller increases in total pay (around 1%), with the positive impact stemming mostly from bonus pay. We also consider indirect network measures that take into account the position of a manager in the aggregate network, capturing better the access to information and resources channel of networks. We find that these measures also matter for pay, and do so significantly, suggesting that it is not only who managers know, but also what they know through them that matters. This is an important result, as indirect measures are difficult to observe, especially by firms, suggesting that the impact on pay stems from actual value that is accrued to the firm.

The estimated network premium is pervasive across different kinds of firms, in terms of size, age and foreign capital, and across all industries. To overcome any potential endogeneity concerns, we resort to an instrumental variable approach, using as proxy for a manager's network, the network of his initial peers (Bramoullé et al., 2009), that is, the connections formed in the first year of his career. The instrumented results confirm and validate our previous findings. Finally, a preliminary analysis unveils that networks are not overpaid and effectively translate into firm value. Future research should investigate further how manager networks can accrue value to firms.

The third chapter advances the existing literature on the gender pay gap amongst top managers by investigating an additional source of divergence between male and female manager pay: professional networks. Following the findings of the second chapter on the importance of networks for executive pay, it is only straightforward to question whether this can be an additional source of divergence across gender. We start by quantifying the existing gender pay gap at the top of the corporate hierarchy in Portugal. We find that female top managers earn 25% less than their male counterparts, conditional on age, education and firm tenure. We follow closely Cardoso et al. (2016), and using the decomposition proposed in Gelbach (2016), unveil that around one third of this pay gap can be attributed to firm sorting, *i.e.* male top managers segregating into firms with more generous pay policies.

Then, we augment the standard model with network metrics, considering both the size and the gender composition of each manager's network. We conclude that almost 20% of the previously computed gender gap is due to differences in networks. Additionally, Gelbach's decomposition suggests that the network effect takes place through the firm sorting channel, *i.e.* well-connected managers have access to higher paying firms.

By then focusing on episodes of transitions between firms, and relying on a propensity score matching procedure, we estimate that around 90% of the pay gap comes into effect during the hiring process, and is only slightly aggravated thereafter, due to biased career progression. We find that roughly one third of the gender gap can be attributed to firm sorting, two thirds of which can be associated to differences in networks. We show that the network premium goes beyond the firm sorting channel, and that negotiations for higher pay within a firm also play a role. After taking into account all observables and both firm sorting and networks, we are left with an unexplained pay gap among top managers close to 11%. If we also take into account the manager's pay from the departing firm, to proxy for unobserved individual heterogeneity, a 6% pay gap persists, which we interpret as a lower bound for pure gender discrimination among top managers, not associated to sorting across firms.

After establishing the important role of networks in explaining the gender pay gap among top managers, we further investigate and assess how female managers can best leverage their networks to overcome gender segregation across firms. We investigate to what extent benefits from networks are gender specific and whether the gender composition of networks also matters. We find that both genders can extract large pay premiums from their networks, and thus no evidence on less productive female networks. We find that male top managers benefit more from being connected to other males rather than to females. In light of the existing male over-representation in corporate leadership and the bias towards benefiting their peers, female mutual help can play an important role. Indeed, results suggest that females benefit more from having other females in their 'inner circle' of connections. These results suggest that policies that envisage increased female representation in leadership positions, can have positive spillovers for other women, while otherwise, gender bias might be perpetuated. These results suggest fruitful avenues for future research.

This thesis examines, across different settings, important instances affecting both a top manager's contribution to firm performance - in face of a major economic crisis, and how networks relate to pay - across firms and across gender. The empirical investigation conducted sits at the intersection of firm recruitment policies and a CEO's tenure and network. We exploit extensive new data on the universe of private firms in Portugal to weed out firm and manager characteristics from the causal hypothesis under investigation, alternatively exploiting a major, exogenous, unexpected shock, and all the past employment history of top managers' to build new and encompassing indicators of network size and network gender composition.

Chapter 1

CEO Performance in Severe Crisis: the Role of Newcomers

1.1 Introduction

Top executives impact strategic choices and firm performance in significant ways, maybe especially so during major crises. A significant body of work has investigated which CEO attributes are associated with the highest levels of firm performance, but very few papers analyze the existing trade-offs, *i.e.*, characteristics that might only be beneficial under certain circumstances. A key trade-off is that of selecting a CEO new to the firm and the function – who may require a period of 'learning by doing', versus an experienced insider – who masters 'business as usual' but may fall prey to an 'experience trap' (as coined by Sengupta et al. 2008), becoming incapable of performing in new and challenging circumstances. Firms choose their CEO along the tenure-origin locus to maximize the net value of the CEO's inter-temporal performance, for given expectations about the future. However, if unexpected circumstances materialize, such as a major crisis, the optimal mix of CEO characteristics will likely change. A severe economic crisis implies a strong disruption in demand or financing conditions, and may require newcomer CEOs – that is, recent external hires, able to respond more creatively and effectively to the unexpected circumstances.

This paper estimates the impact on the firm performance of newcomer CEOs – those recently recruited outside the firm – during the European sovereign debt crisis. We use a comprehensive matched firm-employee dataset covering the universe of top managers and business firms in Portugal, one of the countries most affected by the crisis. The sovereign debt crisis and the subsequent economic and financial adjustment program were exogenous to the firms, unexpected and unprecedented in nature, reach, and magnitude, making this event the basis of our identifying strategy.[1]

When firms are hit by a shock, conducting 'business as usual' becomes a less attractive option, as the risk of getting caught in an experience trap becomes more salient. We argue that the two characteristics that make CEOs more likely to avoid the experience trap are having been recently promoted to an executive position, and being hired in the market, *i.e.*, not being an internal promotion. We label CEOs combining these two characteristics as newcomer CEOs. We diverge from the existing literature on external CEOs in that we argue that after an extended period at the firm, external hires become essentially insiders. Thus, newcomers are restricted to recent external hires, who are less committed to the status-quo in their managerial options when compared to high-tenured CEOs. This is in line with Berger et al. (1997), who consider CEOs with long tenure more likely to be entrenched, with Naveen (2006) suggesting a negative association between CEO tenure and R&D spending, and with Coles et al. (2006) using CEO tenure as a proxy for higher risk aversion.[2] It is commonly expected that external hires

[1] Other strategies using exogenous shocks which affect CEO turnover – hires, firings, or separations, were used in the past to identify the causal connection between CEO characteristics and firm performance. This avenue is pursued in Murphy and Zimmerman (1993), Chang et al. (2010) and Fee et al. (2013), for hires, and Bennedsen et al. (2020), who uses CEO hospitalizations.

[2] Hoskisson et al. (2016) conduct a thorough review of the determinants of managerial risk-taking, addressing tenure as a key dimension. The literature suggests long-tenured executives undertake fewer

bring about change and innovation, consistent with Borokhovich et al. (1996) finding that shareholders react positively to outside CEOs when change is needed. Initially, when the position of a newcomer is not yet secured at the new firm and incentives to perform are at the highest, it is reasonable to expect a CEO's willingness to innovate to be reinforced. Conversely, higher-tenured CEOs (internal or external hires), aside from being more imbued in the firms old ways, might not feel the urge to innovate, due to a lower risk of losing their job. Indeed, Buhai et al. (2014) argue that higher seniority is associated with a lower probability of lay-offs, and Chen and Zheng (2013) find that higher tenure is linked to declining career concerns and therefore a lower predisposition to take risks.

It is widely accepted in the literature that CEOs begin their career with a knowledge deficit, but learn rapidly their function. Some papers have estimated the ideal length of time for a CEO to hold office (Luo et al., 2013). Wu et al. (2005) show that, with time, low-tenured CEOs take more risk and undertake bolder initiatives[3], expand their knowledge and widen their repertory of skills, improving firm performance. However, after some point, they become myopic, risk-averse, slow to adapt to changing circumstances and overly wedded to early formulas.[4] Henderson et al. (2006) also show that, after some time, tenure starts to hurt performance. In line with these views, Hambrick and Fukutomi (1991) suggested that the relationship between CEO tenure and firm performance can be described as an inverted U. In this vein, longer-tenured CEOs have been shown to favor the avoidance of losses over the pursuit of new gains, as suggested

changes and fewer risks, and this inclination is more pronounced in the case of strategic changes and more complex industry contexts.

[3]Weisbach (1995) shows that 'at a time of a management change, there is an increased probability of divesting an acquisition at a loss (...)', leading to the conclusion that 'management changes are important events for corporations because they lead to reversals of poor prior decisions.'

[4]Consistent with this accommodation for the CEO to old practices, Hermalin and Weisbach (1998) model endogenous monitoring of CEOs and find that, in public companies, board independence declines over the tenure of a CEO. In other words, CEO's become less accountable and may have more room to slack and not innovate sufficiently.

in Luo et al. (2013).[5]

As outsiders, newcomers are also less likely to commit blindly to obsolete strategies in the face of new challenges, bringing richer, diversified experiences from other organizations. Indeed, the common perception is that CEOs recruited in the market are more likely to make bolder changes than those internally promoted, although research is inconclusive on the causality with better performance.[6] On the one hand, Bidwell (2011) and Zajac (1990) argue that external hires perform worse than internal movers, even if they are paid substantially more. While on the other hand, Huson et al. (2004) find evidence that operating performance improves when an outsider, as opposed to an insider, is appointed.

Our paper contributes to the existing literature in three ways. First, we introduce in the CEO literature a recent and powerful identification strategy based on the European sovereign debt crisis (Buera and Karmakar 2019 and Adelino et al. 2020), by collecting data on observed firm behavior during the 2011-2013 period, in which Portugal experienced a severe economic downturn, unequaled in the OECD except for Greece. Portugal was already accumulating important macroeconomic imbalances during the prior decade, which made it highly vulnerable to the crisis. Nevertheless, the 2011 sudden stop was an unexpected shock, resulting in a severe decrease in employment and a sharp drop in sales for most firms. This represented a drastic change for firms, as immediately before the crisis, credit to non-financial corporations was still growing at a faster pace than in the Eurozone as a whole. Therefore, we consider the crisis an unexpected exogenous large shock, transversal to the economy, which left virtually no

[5] The relationship between CEO tenure and performance is often simplified as a linear function. Simsek (2007) tests a more complex non-linear relationship, whereby increasing CEO tenure first increases and then decreases the risk-taking propensity of the management team, which influences performance via the firm's entrepreneurial orientation.

[6] Conventional wisdom suggests that an external solution for leadership is a common option for those firms that have stagnated or are in deep financial distress. This is also corroborated in the literature, for example in Parrino (1997). Notice, however, that in our case, since we will consider a systemic crisis, it may not be as immediate to make the current CEO responsible for low performance.

firm unharmed. The depth and unexpected nature of the crisis precluded preemptive actions by firms, including anticipatory CEO hires. Therefore, the CEO in place when the economic crisis started, had been, in most cases, previously appointed and was expected to operate under different circumstances. This allows us to identify the association between CEO type and firm performance. We do not suggest that newcomers always outperform other CEOs. Instead, we present robust evidence that they become more valuable in the event of a major crisis. In other words, the mix of characteristics of the CEO in place when the crisis occurs becomes central to the firm, the more so as the crisis makes it difficult for companies to re-optimize and change the CEO.

Second, the availability of uniquely detailed CEO and firm-level data is a necessary condition to pursue this type of research, and we put together a unique set of information by merging two large micro-level databases covering the universe of firms and CEOs in Portugal: *Quadros de Pessoal*, which collects extensive information on both firms and workers; *Informação Empresarial Simplificada*, which collects detailed information on balance sheet and income statements for the universe of Portuguese firms.

Third, we use data for the period before and during the crisis, allowing us to detect, evaluate and assess how newcomer CEOs change their contribution to firm performance from normal to crisis times. We perform a difference-in-differences estimation and validate our results through several matching procedures and a latent instrumental variable approach.

Our results show that firms run by newcomer CEOs significantly outperform those run by other CEOs during the crisis, both in terms of value added (GVA) and sales. Notably, this difference in performance is observed during the crisis, but not before. Our results are broadly consistent with Henderson et al. (2006), who show that industry uncertainty magnifies the negative impact of longer-tenured CEOs. The performance gap, in terms of GVA, is approximately 19 per cent during the crisis years, and slightly negative during non-crisis years. This result is robust for newcomers, irrespective of whether they are transitioning from executive or non-executive positions in the origin firm. Firms run by newcomer CEOs are also less likely to fail and go out of business during the crisis. In addition, firms managed by newcomers reach higher values of productivity – in terms of sales and value added per worker. Moreover, we differentiate between possible mechanisms and find that that these firms are also associated with easier access to long-term debt, suggesting a 'signaling' value of hiring a newcomer CEO. Newcomer CEOs are able to sustain higher numbers of workers during the crisis, pay higher wages and incur higher total costs, in an efficient manner, without increasing costs per unit sold. Overall, our results suggest that newcomer CEOs are able to amass resources and make decisions that not only lead to better performance, but also higher productivity. Our findings are robust to the inclusion of CEO and firm controls, including CEO and firm fixed effects, and are present irrespective of whether the firms are managed by one or more CEOs, and whether or not those are also owners.

Taken together, our results strongly support the hypothesis that newcomers deliver better firm performance during crises when compared to high-tenured and/or insider peers. This is in line with the idea that, in times of distress, the existing management paradigm is no longer suitable, and challenging 'business as usual' and adapting to new circumstances becomes vital to escaping the 'experience trap'.

There are however important risks associated to CEO turnover that have to be considered in the decision process, such as the likelihood of hiring a poor-performing CEO whose decisions may be costly to the firm, or the loss of value related to firm-specific information disclosure that a CEO might reveal to a rival firm. Deng et al. (2019) find that CEO departures can be associated to other costs, including worse financing conditions. On the other hand, there are also specific costs concerning newcomers. For instance, the search costs in finding a good match increase since it is more difficult to observe the abilities of an outsider (Parrino 1997), in addition to the initial knowledge deficit. Furthermore, there is a higher cost when transferring firm-specific information and expertise to an outsider (Naveen 2006). These ideas are consistent with the results, as our evidence actually points to a disadvantage associated to newcomers in 'normal times'. We suggest, however, that this picture is reverted in times of crisis. While many authors argue that newcomers become more attractive when change is needed, in this article we test it formally in a major crisis setting. Thus, despite all trade-offs and costs, if the probability of severe shocks is taken into consideration in the selection process, then, ceteris paribus, newcomers have an advantage.

This paper is organized as follows: Section 1.2 details the data used and provides some descriptive statistics; Section 1.3 presents the main results on newcomer CEOs, unveiling in subsection 1.3.3 whether low tenure or the outsider nature of the newcomer CEO drive the results, and includes several robustness tests in subsection 1.3.4; the impacts on firm survival and productivity are analyzed in Sections 1.4 and 1.5; finally, Section 1.6 sheds some light on the mechanisms behind our main results; and we conclude in Section 1.7.

1.2 Sample and data description

Our data combines information drawn from two different panel datasets: *Quadros de Pessoal (QP)* and *Informação Empresarial Simplificada (IES)*. *QP* is a micro longitudinal dataset, collected by the Portuguese authorities, with matched employer-employee data that include all private firms and workers operating in Portugal. The survey is mandatory for all establishments with wage-earners and contains information regarding the establishment, the firm (such as size and total sales) and each of its workers (including gender, age, educational attainment, compensation and hours worked). *QP* includes a personal identification number that enables tracing individuals across time, which allows us to use information from the past (since 1986) to observe a workers promotion within or from outside a firm to the CEO post. In addition, we draw on yearly data from *IES* for information regarding the firm's balance sheet and income statements. *IES* is the mandatory yearly declaration through which firms report accounting information to the tax administration and the statistical authorities. Data is available from 2005 onwards, covering virtually the universe of Portuguese non-financial corporations. Our final sample covers the period from 2008 to 2013.

The sample is restricted to firms for which we can identify a CEO. There is no direct way to do so in the database, however each worker is classified according to the National Classification of Occupations and assigned a professional grade level in QP. Professional grade levels are defined by law, and each firm is obliged to classify each job accordingly. We define CEOs as workers classified simultaneously as chief executives in the National Classification of Occupations and in the highest hierarchical grade level, which corresponds to top managers (following Lima and Centeno (2003)). The sample is restricted to firms reporting no CEO turnover between 2008 and 2013.[7] It is important to stress that results only change marginally without this restriction.[8] Finally, the sample is limited to firms already operating in at least one of the pre-crisis years (2008-2010), a requisite to be able to measure the impact of the crisis on the firm's performance. Our final sample consists of 225 629 CEO-firm-years, representing around 50 718 firms and 63 683 CEOs.

[7] In the sample of firms managed by a non-owner CEO, approximately 16 per cent of the firms observe CEO turnover sometime between 2008 and 2013. This figure drops to approximately half, if we consider only CEO hires during the crisis period.

[8] In the body of the paper we restrict ourselves to firms not undertaking CEO changes, such that we can consider having a newcomer in office at the time of the crisis as exogenous to the firm. However, the results regarding newcomers presented in the paper, hold across both groups.

1.2.1 Data harmonization issues

In QP, in the period between 1986 and 2013 the national classification of occupations was revised several times and the last change occurred in 2010. From this year onwards, we use the latest classification of occupations and are able to identify accordingly the workers classified as CEOs. However, in order to identify which firms maintained their CEO before and through the crises, it is necessary to compare the classifications prevailing before and after 2010. We proceeded by using the official table of harmonization published by Statistics Portugal, taking also into consideration that there was a lag in the adoption of the new classifications by some firms.

We also encounter a problem in IES, since in 2010 there was a change in the official firm accounting system from *Plano Oficial de Contas* (Official Accounting Plan) to *Sistema de Normalização Contabilística* (Normalized Accounting System), which was driven by the need to comply with EU regulations. This leads to small differences in the definition of some of the variables which, however, have a negligible influence on the series under consideration.

1.2.2 Newcomer definition

We define newcomers as external hires (outsiders), as opposed to internal promotions, with a tenure at a top management position at the current firm lower than the yearly sample median (see Table 1.1). More specifically, we identify outsiders as those CEOs, who in their first year at a firm already hold a top management position and define tenure as the number of years at the firm, occupying a top management post.

Table 1.1: Definition of newcomer CEO

		Tenure at a top management position at the current firm: Below sample median	Tenure at a top management position at the current firm: Above sample median
Prior job	Another firm: outsider	Newcomer✓	✗
Prior job	Same firm: insider	✗	✗

1.2.3 CEO and firm characteristics

Table 1.2 presents summary statistics for the two main measures of firm performance used in this paper, GVA and sales, as well as CEO and firm characteristics that are used as covariates in the analysis. Appendix A.1 provides variable definitions and data sources.

Table 1.3 shows the average characteristics for firms with newcomer and 'non-newcomer' CEOs – those who have been internally promoted or/and have a higher than median executive tenure, and tests for the difference.

As expected, newcomers have a significantly lower tenure at a management position, tend to be younger and hold higher degrees of education. In addition, the share of female CEOs is higher amongst newcomer CEOs. As to firm's characteristics, those with newcomer CEOs tend to be smaller (both in terms of number of workers and production scale) and younger.

Table 1.2: Summary statistics

	Mean	Median	Std. Dev.	Min	Max
CEO characteristics:					
Outside CEOs (dummy)	0.835	1	0.371	0	1
CEO tenure (years)	7.185	7	4.267	1	23
CEO age (years)	45.863	46	9.619	19	75
CEO gender (dummy)	0.253	0	0.435	0	1
Education (categorical)[1]	5.406	5	2.200	2	10
Firm characteristics:					
GVA (log)	11.537	11.322	1.811	1.790	20.447
Total sales (log)	12.751	12.431	1.862	2.303	21.188
Firm age (years)	12.916	10	12.340	0	112
No. of workers	20.517	4	115.189	1	4853
No. of establishments	1.347	1	2.377	1	158

This table presents the mean, median, standard deviation, minimum and maximum of each variable used in the baseline regressions, for the sample of firms managed by a non-owner CEO (22,534 observations). Variable definitions are provided in Appendix A.1. (1) Education is a categorical variable where: 1 - less than primary education; 2 - first cycle of primary education; 3 - 2nd cycle of primary education; 4 - lower secondary education; 5 - upper secondary education; 6 - post-secondary non-tertiary education; 7 - bachelors degree; 8 - undergraduate degree; 9 - masters degree; 10 - PhD.

Table 1.3: Newcomer statistics: CEO and firm characteristics

	Newcomer	Non-newcomer	Difference	
CEO characteristics:				
CEO tenure (years)	3.710	10.102	-6.392	***
CEO age (years)	41.596	47.439	-5.844	***
CEO gender (dummy)	0.294	0.264	0.030	***
Education (categorical)	5.220	4.842	0.378	***
Firm characteristics:				
GVA	10.719	11.257	-0.539	***
Total sales	11.984	12.536	-0.552	***
Firm age (years)	6.283	16.542	-10.259	***
No. of workers	6.528	11.092	-4.564	***
No. of establishments	1.157	1.247	-0.090	***
Sample size:	100,414	125,215		

This table presents the mean of CEO and firm characteristics for the sample of newcomer and non-newcomer CEOs and the associated difference. Variable definitions are provided in Appendix A.1. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

1.2.4 CEOs and firm owners

Throughout our analysis, we will split our sample along two dimensions: the number of CEOs at the firm, and whether at least one of those CEOs is, simultaneously, the employer/firm owner. This distinction is important, because a higher number of CEOs may 'dilute' the connection between CEO characteristics and firm performance, while ownership may also affect incentives in an important way. In Table 1.4 we present the average CEO and firm characteristics for those with a single non-owner CEO or a single owner CEO.

Owners tend to be slightly younger, are more often females and hold lower levels of education. Average tenure of CEOs at a management position is around 7 years, irrespective of being the owner or not. Firms run by owners tend to be considerably smaller, both in terms of number of workers and establishments as well as in terms of GVA and sales.

Table 1.4: CEOs and firm owner statistics

	1 CEO: not the owner	1 CEO: the owner	Difference	
CEO characteristics:				
CEO tenure (years)	7.185	7.031	0.153	***
CEO age (years)	45.863	44.246	1.617	***
CEO gender (dummy)	0.253	0.267	-0.015	***
Education (categorical)	5.406	5.061	0.345	***
Firm characteristics:				
GVA (log)	11.537	10.763	0.774	***
Total sales (log)	12.916	10.381	2.535	***
Firm age (years)	12.916	10.381	2.535	***
No. of workers	20.517	5.978	14.539	***
No. of establishments	1.347	1.155	0.191	***
Sample size:	22,534	116,890		

This table presents the mean of CEO and firm characteristics for the sample of owner CEOs and non-owner CEOs and the associated difference. Variable definitions are provided in Appendix A.1. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

1.3 Do newcomers deliver better firm performance in times of crisis?

Uncovering the causal association between CEO characteristics and firm performance is not an easy task. As the assignment of CEOs to firms is not random, separating the impact of CEO attributes from firm characteristics is a challenging endeavor, so that a statistically significant coefficient associated to a CEO type may be due to unobserved firm characteristics which may be, in turn, correlated with the CEO. We propose an innovative route to overcome this endogeneity problem. Our identification strategy is based on the argument that the context in which firms operated changed drastically and unexpectedly during the sovereign debt economic crisis.

While the Portuguese economy was already accumulating important macroeconomic imbalances before 2011, there was a large deterioration in 2011-13, which was transversal to all firms and unexpected in magnitude and reach.[9] Portugal was at the core of the sovereign debt crisis in the euro area, and as pointed out by Barbosa (2017) through the sovereign-bank linkage there were severe negative consequences on the banking system. This represented a huge shock to the Portuguese firms, which are mostly micro and small firms with high leverage ratios.[10]

Given that the crisis affected virtually all firms, each firm was constrained and possibly unable to re-optimize their choice of CEO in wake of the economic shock. Therefore, in great part, firms were stuck with a CEO hired before the crisis, and suitable for a context that was no longer valid. In sum, the unexpected and severe nature

[9] Adelino et al. (2020) also rely on the unexpected nature of the sovereign crisis for their identification strategy.

[10] Some other figures that help illustrate the magnitude of the shock: (i) immediately before the crisis, credit to non-financial corporations was still growing at a faster pace in Portugal than in the euro area as a whole, while between 2011-13 it recorded negative rates, much lower than the euro area; (ii) the unemployment rate reached in 2012-13 a historic record and therefore unprecedented uncertainty and instability for firms; (iii) output gap estimates (from several official institutions) suggest that during that period Portugal observed the largest negative gap of the last 3 decades.

of the crisis created a laboratory to conduct our analysis and investigate causality.

1.3.1 Baseline regressions

We start with a difference-in-differences analysis, where the treatment is defined as a firm being 'hit' by the crisis while being led by a newcomer CEO, hired a few years before in the market. Our control group consists of firms with CEOs with either high tenure and/or internally promoted. The fact that a firm enters the crisis with or without a newcomer CEO is considered exogenous to the firm's characteristics. Being in the treatment or control group is considered unrelated to other unobserved characteristics affecting the dependent variable. In the difference-in-differences procedure we consider the treatment period as the crisis years (2011 to 2013), and the baseline period 2008 to 2010, the years just before the crisis. Our benchmark specification is:

$$\begin{aligned} Ln(Y)_{it} = c + \beta_1 Crisis_t + \beta_2 Newcomer_{it} + \\ \beta_3 Crisis_t * Newcomer_{it} + \beta_4 X_{it} + \gamma_i + e_{it} \end{aligned} \tag{1.1}$$

where the outcome variable Y_{it} is either GVA or total sales in firm i at year t. The constant term captures the mean outcome for the control group – before the crisis. The treatment dummy takes the value of 1 for firms with newcomer CEO, capturing any ex-ante differences between the treatment and control groups. The coefficient on the crisis dummy captures the common impact of the economic downturn on all firms. Finally, and most relevant to our exercise, the interaction term between the crisis and newcomer dummies – a dummy variable taking the value one for firms with newcomer CEOs during the crisis period – tests whether these firms display a significant difference in performance during the crisis. X_{it} is a vector of CEO and firm controls, γ_i are the dummies for each firm and e_{it} is the error term.

In our specifications, all available observable covariates are added as controls, minimizing omitted variable bias and the possibility that the results are driven by firm or CEO characteristics. By adding firm fixed effects, we also address the issue of time-invariant unobservables, which may lie behind the CEO-firm match so that no time-invariant firm-specific omitted variables drive our results. In this identification procedure, we also control for innate CEO attributes. This is important as most of the firms in the database have a single CEO and display no CEO turnover in the period of the crisis, making firm and CEO dummies equivalent. In other words, we are in fact controlling for any time invariant firm or CEO characteristics, while not able to disentangle the two, at least for the majority of CEOs who are in charge of only one firm. We also add CEO fixed effects to confirm that results don't change significantly.[11]

Difference-in-differences models require the fulfillment of a parallel trend assumption.[12] This assumption requires that the average change in the control group is the counterfactual of the treatment group, in the absence of treatment. In the current case, we need to assess whether average changes in GVA across firms were the same during the crisis after controlling for all observable factors and time-invariant unobservables, assuming no difference in CEO type. There is no direct way of testing the identifying assumption. Instead, we follow a method proposed by Baltrunaite et al. (2014) and examine the residuals from the regression of the GVA on the vector of controls X_{it} and the vector of firm fixed effects for the treatment and control groups before the crisis. Comparing the averages of the residuals for the two groups, before the crisis, we find that they are not statistically different. In sum, the unpredicted part of the GVA before

[11] Results are available upon request.

[12] Another pre-condition for the validity of this type of analysis is that the 'program' is not implemented based on pre-existing differences in outcome. In our case, this is not a problem since the sample is restricted to firms with no CEO turnover during the crisis, nor immediately before. Adding this feature to the unpredictability of the crisis, it is possible to conclude that having a newcomer in office when the crisis set in is an exogenous event, not the result of a voluntary choice.

the crisis is essentially the same in treated and control firms, which further supports our identification strategy. Additionally, limiting the sample to the period before the crisis, we run the same regressions, including the same set of controls, and interact year dummies with the newcomer identifier, confirming that there are no significant differences between pre-crisis years. Thus, we conclude, that there is no evidence supporting the violation of the parallel-trend assumption (see Appendix A.2).

Table 1.5 presents the results of the difference-in-differences estimation. Columns 1 and 2 report estimates considering GVA as the dependent variable, first those from a simple OLS regression, and then firm fixed effects are added. In columns 3 and 4 we present the same set of results for total sales. Throughout, we control for CEO attributes – gender, age and education, and firm characteristics – number of workers, establishments, and firm age. Standard errors are clustered at the firm level, to adjust for within firm correlation.

The crisis dummy has, as expected, a consistent negative and significant coefficient, capturing the common downturn in performance for all firms. Our variable of interest, the interaction term between the crisis and treatment dummies, has a consistently positive and significant coefficient across all specifications.[13] Relative to similar firms, firms under the direction of newcomers during the crisis, deliver better performance that corresponds to a 19.2 per cent (17.6 log points) GVA gap and 22.2 per cent (20.1 log points) in terms of sales.[14]

It is important to notice that the coefficient of the newcomer dummy *per se* points to a disadvantage prior to the crisis, although very small in size and not consistently significant, indicating that the positive performance gap does not exist prior to the crisis. This result is consistent with our hypothesis that newcomers become valuable in

[13] If instead of considering the *Crisis* dummy, we interact the *Newcomer* variable with time dummies, we confirm that our findings are consistent across the crisis period and not driven by a particular year.

[14] The performance gap associated to newcomers is consistent across all big industries. There is no evidence that the result is driven by any particular sector of activity.

times of crisis, but do not necessarily foster better firm performance in ‘normal times’, when the gains from learning by doing may outweigh the risk of getting caught in an experience trap.

Table 1.5: Newcomers and firm performance: difference-in-differences

	Firm Performance			
	Gross Value Added		Sales	
	OLS (1)	Firm fixed effects (2)	OLS (3)	Firm fixed effects (4)
Crisis	-0.146*** (0.010)	-0.128*** (0.016)	-0.108*** (0.011)	-0.103*** (0.012)
Newcomers	-0.029** (0.014)	-0.082*** (0.023)	-0.000 (0.023)	-0.042** (0.019)
Newcomers × crisis	0.172*** (0.019)	0.176*** (0.024)	0.170*** (0.022)	0.201*** (0.020)
CEO age	0.001 (0.001)	0.004 (0.006)	0.000 (0.001)	-0.009 (0.006)
CEO gender	-0.005 (0.011)		-0.099*** (0.023)	
Education	0.021*** (0.003)	0.011 (0.010)	0.033*** (0.006)	0.005 (0.006)
Firm age	-0.003*** (0.000)	-0.009** (0.004)	-0.001 (0.001)	0.001 (0.004)
No. of workers	0.267*** (0.017)	0.673*** (0.026)	0.357*** (0.026)	0.623*** (0.025)
No. of establishments	-0.005*** (0.001)	0.002 (0.003)	-0.007* (0.004)	0.002 (0.002)
Average performance	0.797*** (0.017)		0.679*** (0.024)	
Observations	22,324	22,534	23,194	23,783

This table presents difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of gross value added (GVA) and total sales on newcomer CEOs. The treatment variable is defined as being a newcomer in a top management position at the firm. Newcomers are defined as having CEO tenure below the yearly sample median and as having been hired externally (outsiders). The treatment period is the period of crisis, namely 2011-2013. The OLS regressions also include district and industry dummies. The sample consists of all firms, managed by 1 CEO (with no CEO turnover during or immediately before the crisis) and for which CEO data are available. Robust standard errors adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

1.3.2 The experience trap

In this subsection we propose to empirically validate the existence of an experience trap. Namely, we propose to show that the higher the tenure of the CEO at the firm, the lower will be firm performance during the crisis period.

So far we have defined a newcomer CEO as an external hire with tenure lower than the yearly median (years employed at the current firm at a management post). We re-estimate specifications 2 and 4 from Table 1.5 using different upper thresholds for the tenure of a newcomer CEO, maintaining the condition that they are external hires. Figure 1.1 plots both the coefficients of interest, *crisis*×*treatment*, for the GVA and Sales specifications for different tenure thresholds, and the corresponding interval of confidence. We find evidence that as the threshold is less stringent, that is, a low-tenured CEO is defined in a less demanding way, the result weakens, and the performance gap (irrespective of considering GVA or total sales) continuously approaches zero. This is a very important result. The fact that the coefficient of the interaction term is positive throughout and the size of the coefficient monotonously decreases as higher tenured CEOs are considered, suggests a clear relationship between tenure at a management position and firm performance during the crisis, empirically validating the existence of an 'experience trap'.

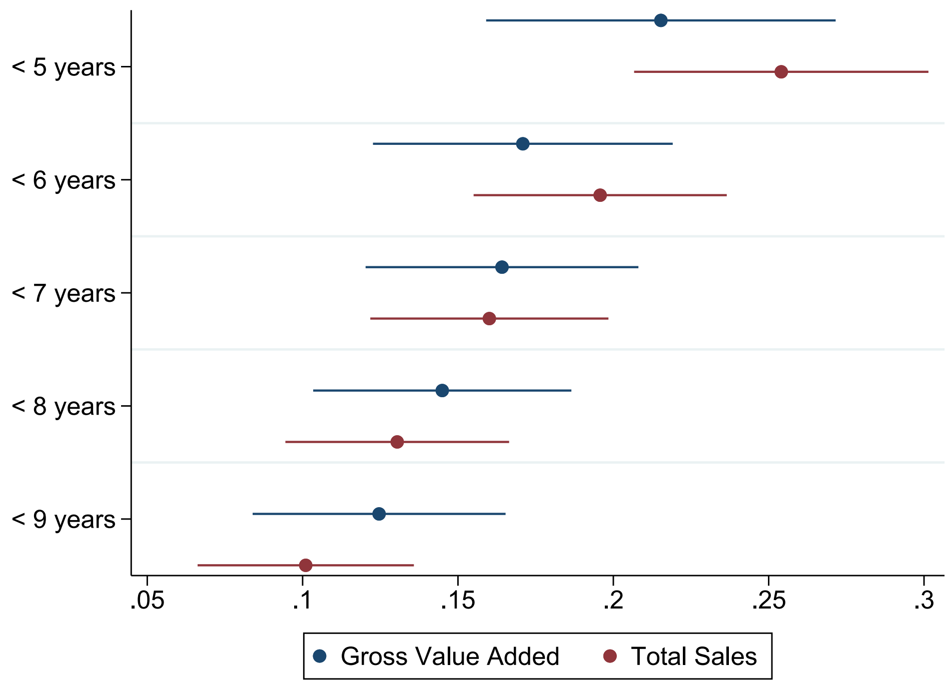


Figure 1.1: Tenure at a top management position and the performance premium
The dots stand for the estimated coefficients from a fixed effects difference-in-differences model, and the lines represent the 95 per cent confidence interval.

1.3.3 Newcomers: is CEO tenure or external origin what matters?

In this paper we define newcomers as recent external hires (refer to Table 1.1). This raises the question of whether our results are driven by just one, or both these attributes: whether it is just being an outsider – externally hired, or being new at the job – a low tenure in a management position at the firm, that drives our findings. Remember outsiders are external hires, but not necessarily recent ones.

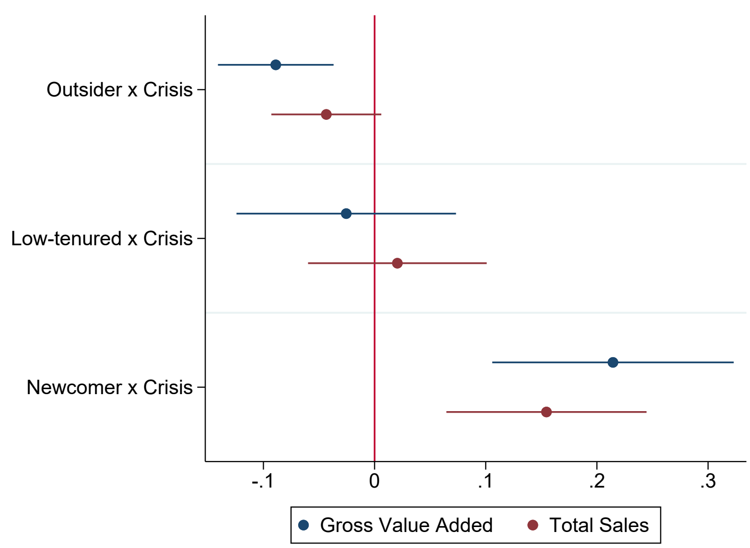
To disentangle the result on newcomers from the performance of outside CEOs and low-tenure CEOs, we perform a difference-in-difference-in-differences (DDD) estimation:

$$Ln(Y)_{it} = \beta_1 Crisis_t + \beta_2 LowTenure_{it} + \beta_3 Outsider_i + \beta_4 Newcomer_{it}$$
$$+\beta_5 Crisis_t Outsider_i + \beta_6 Crisis_t LowTenure_{it} + \beta_7 Crisis_t Newcomer_{it} \quad (1.2)$$
$$+\beta_8 X_{it} + \gamma_i + e_{it}$$

where Y represents either the logarithm of gross value added or total sales.

The DDD estimator starts with the average impact of the crisis on firms run by newcomers and then explicitly nets out the change in means for low-tenured inside CEOs and the change in means for high-tenured outside CEOs. Figure 1.2 depicts the coefficients of interest: β_5, β_6 and β_7, confirming the robustness of the estimated newcomer performance gap during the crisis. Indeed, the margin that matters more for firm performance is a combination of being low-tenured and an external hire, as

Figure 1.2: Difference-in-difference-in-differences
The dots stand for the estimated coefficients and the lines represent the 95 per cent confidence interval. Detailed results are available in Table A.2 in Appendix A.3.



opposed to one or the other.

These results show that there is no evidence that externally recruited CEOs perform better than those internally promoted in times of crises, which suggests that high-tenured external hires become as imbued in the firm's business practices as internally promoted CEOs. Similarly, low tenure is not enough to succeed during the crisis. As CEO tenure is defined as the number of years at firm with a top management position, not accounting for the periods spent at other positions in the firm, low-tenure as a CEO may not be sufficient to avoid being caught in the experience trap, as familiarity with business practices may come from earlier on.

As an additional robustness test we re-estimate the regressions specified in Table 1.5, defining the treatment variable as being an outsider or new at the job – having a lower than yearly sample median tenure in a top management position – instead of simultaneously both. As expected, we find that the coefficients are not statistically significant for outsiders.[15] Results do suggest, however, that low-tenured CEOs help firms perform better in crises times. We further investigate whether this result is homogeneous across insider and outsider CEOs. We find that, for insiders, there is no significant performance difference between low and high-tenured insiders.[16] These results corroborate our main hypothesis and results presented above: the performance gap is associated to newcomers, those CEOs who are simultaneously outsiders and recent at the firm.

Finally, in Table 1.6, we examine how a CEOs experience in the firm they transited from affects their impact on business performance during the crisis. We split the sample of outside CEOs between those that transited from executive and from non-executive positions, to find that low-tenured outside CEOs deliver better firm performance during crises irrespective of the position they transit from. This is an important result, as it

[15]Results can be found in Table A.3 in Appendix A.3.
[16]See results in Table A.4 in Appendix A.3.

sheds some light on whether previous experience as a manager of another firm may also act as a trap in times of crisis. We find that previous experience doesn't alter our results, meaning that being new to the current firm is still beneficial, despite the position held at the previous job.[17]

Table 1.6: Outside executives and non-executives

Panel A	**Outisde CEOs: transitting from non-executive position**			
	Gross Value Added		Sales	
	OLS	Firm fixed effects	OLS	Firm fixed effects
	(1)	(2)	(3)	(4)
Crisis	-0.163***	-0.162***	-0.129***	-0.128***
	(0.014)	(0.021)	(0.014)	(0.017)
Low tenured CEO	-0.050***	-0.072**	-0.054*	-0.061***
	(0.018)	(0.029)	(0.029)	(0.024)
Low tenured CEO×crisis	0.179***	0.201***	0.184***	0.221***
	(0.023)	(0.031)	(0.026)	(0.025)
Observations	13,002	13,154	13,556	13,992
Panel B	**Outside CEOs: transitting from executive position**			
	Gross Value Added		Sales	
	OLS	Firm fixed effects	OLS	Firm fixed effects
	(1)	(2)	(3)	(4)
Crisis	-0.128***	-0.122***	-0.073***	-0.097***
	(0.024)	(0.037)	(0.026)	(0.028)
Low tenured CEO	-0.011	-0.124***	0.036	-0.016
	(0.026)	(0.042)	(0.044)	(0.036)
Low tenured CEO×crisis	0.170***	0.166***	0.155***	0.194***
	(0.036)	(0.043)	(0.044)	(0.040)
Observations	5,397	5,440	5,603	5,717

This table presents difference-in-differences estimates of OLS and firm fixed-effects panel regressions of the logarithm of GVA and total sales on low-tenured CEOs. In Panel A and B the sample of outsiders is further decomposed into outside executives (CEOs who were already top-managers at another firm when they were recruited to the current firm) and non-executives (CEOs who didn't hold an executive job at the previous firm). The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The OLS regressions also include district and industry dummies. The sample consists of all firms, managed by 1 external CEO (with no CEO turnover during or immediately before the crisis). Robust standard errors adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

[17] Additionally, we find that the previous industry of activity does also not alter our results. Our results are driven by the newcomer nature of the CEO, irrespective of previous experience.

1.3.4 Robustness tests

Newcomers: CEOs and firm owners

So far we have restricted our analysis to the sample of firms managed by a unique CEO, who is not simultaneously owner of the firm. In this sub-section, we examine whether the impact of newcomers is robust across different samples, considering two dimensions: the number of CEOs, unique or multiple CEOs, and the number of firm owners. For firms with multiple CEOs, the newcomer dummy takes the value 1 if at least one of the CEOs in charge is a newcomer.

In Table 1.7, the first two columns consider the entire sample of firms in the database – including multiple CEOs and CEO-owner firms, which increases substantially the number of observations. We confirm previous results from our baseline regressions. Firms run by newcomers obtained, during the crisis, approximately a 24.6 per cent (22 log points) higher GVA relative to similar firms.

Figure 1.3 compares the coefficients for the variable of interest – the interaction term between the crisis and treatment dummies — from the fixed effect regressions in Table 1.7.[18] We find a consistently positive and significant interaction coefficient for all sub-samples, confirming that newcomers outperform higher-tenured and/or inside CEOs, irrespective of whether these CEOs manage the firm alone, and whether or not they are also firm owners. Moreover, coefficients aren't significantly different across samples. One important result is that the impact seems to be highest for owner CEOs. This makes a lot of sense, given that owners have more managerial discretion, corroborating the idea that newcomers outperform their peers during the crisis period, because they are able to detach from the status-quo and innovate.

[18]Results regarding the samples with multiple CEOs can be found in Table A.5 in the appendix.

Table 1.7: Newcomers and firm performance (GVA): CEOs and firm owners

	All firms		Firm's managed by one CEO: 1 CEO: not the owner		Firm's managed by one CEO: 1 CEO: the owner	
	OLS (1)	Firm fixed effects (2)	OLS (3)	Firm fixed effects (4)	OLS (5)	Firm fixed effects (6)
Crisis	-0.178***	-0.156***	-0.146***	-0.128***	-0.181***	-0.176***
	(0.003)	(0.005)	(0.010)	(0.016)	(0.005)	(0.008)
Newcomers	-0.038***	-0.065***	-0.029**	-0.082***	-0.045***	-0.067***
	(0.004)	(0.007)	(0.014)	(0.023)	(0.006)	(0.011)
Newcomers×crisis	0.204***	0.220***	0.172***	0.176***	0.206***	0.248***
	(0.006)	(0.008)	(0.019)	(0.024)	(0.009)	(0.011)
Observations	223,797	225,629	22,324	22,534	115,743	116,890

This table presents difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of gross value added on newcomer CEOs, across different samples. In these regressions the treatment variable is defined as being a newcomer (low tenure and externally recruited). The treatment period is the period of crisis, namely 2011-2013. Regressions (1) and (2) from table 1 were estimated for different samples, depending on whether CEOs own the firm or not. The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The OLS regressions also include district and industry dummies. Robust standard error adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

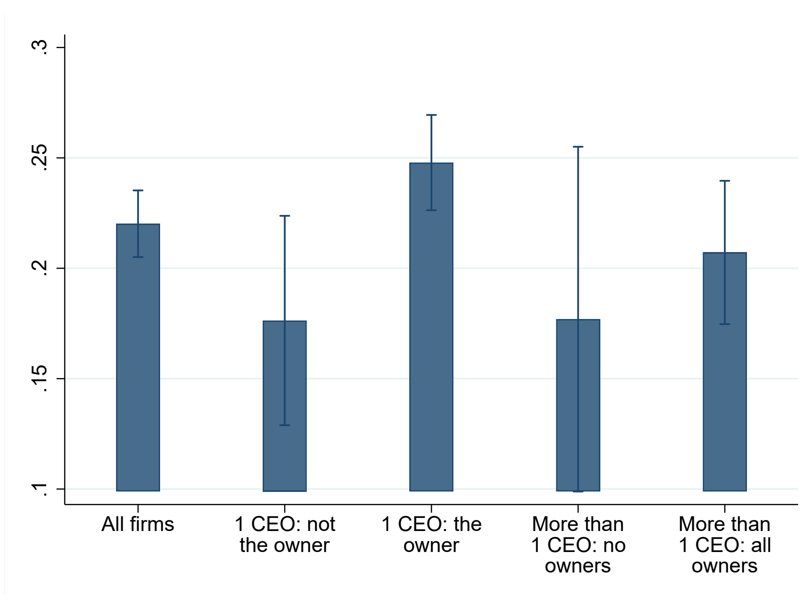


Figure 1.3: Newcomers across different types of samples and the performance premium The columns stand for the estimated coefficients and the lines represent the 95 per cent confidence interval.

Using total sales as an alternative measure of performance, for the different samples specified above, confirms that results are consistent across all firms. The magnitude of the impact of newcomers on sales is quantitatively similar to the impact on GVA.[19]

Placebo Regressions

We also carry out some placebo exercises to show that the effects documented are indeed a feature of this particular period and treatment group and are not confounded by other factors.

We have run two falsification tests: (i) we have drawn a random placebo treatment group from the control group and (ii) defined the placebo crisis period as the year 2007. We have found no significant results, vindicating our baseline approach (see Figure A.1 in Appendix A.2).

Addressing endogeneity

To address any remaining selection bias concerns, driven by the non-random match of CEOs to firms, we resort to a nearest-neighbor matching estimator (Abadie and Imbens, 2011). If possible, we would compare firm performance during the crisis if a newcomer were in office, with the same firm's performance under the alternative of having a non-newcomer in charge. However, for any firm, only one of the alternatives is observable, thus the counter-factual needs to be constructed. We estimate a first-stage probit regression of the probability that a firm is run by a newcomer using observable pre-crisis characteristics of both the firm (firm size and GVA) and the CEO (gender, age and education). We then obtain a propensity score based on the predicted probabilities. Finally, we use the outcome of the nearest neighbor (with replacement) to compute the unobserved counter-factual.

[19] Table A.6 in the appendix presents results for total sales.

In addition, we perform balancing tests to confirm whether the necessary assumptions for the validity of the matching procedure hold. These are, first, the overlap assumption requiring that the estimated propensity scores display no mass around zero and one. Figure A.2 in the Appendix confirms this, showing that the estimated propensity score for all observations used in the matching procedure reflect a positive probability of receiving treatment. To determine the region of common support more precisely, we follow the strategy in the literature that consists in excluding all observations for which the propensity score lies below the minimum or above the maximum of the opposite group. This approach eliminates a very low number of observations, and has only a marginal impact on our results. We then assess the quality of the matching by checking for significant differences in covariate means for both groups, computing the standardized bias measure and the pseudo-R^2 suggested by Rosenbaum and Rubin (1985) and Sianesi (2004), respectively.

Table A.7 in the Appendix, shows that the clear ex-ante differences between the control and treatment groups are reduced to statistical insignificance after the matching procedure. This is confirmed by the decrease in the standardized bias and the low pseudo-R^2.[20] Overall, we conclude that the matched sample increases the similarity between the observables of the treatment and the control groups.[21]

Table 1.8 presents the impact of newcomers on firm performance, measured by GVA, following a matching procedure. The average performance gap between matched newcomer CEOs and the control group is around 17 per cent (15.8 log points, column 1), and is statistically significant. Notably, the estimated size of the effect is remarkably close to the results above, using DiD estimates. Results are pretty stable across samples, for firms managed by owner and non-owners, and unique or multiple CEO firms. These

[20] More specifically, the very low pseudo-R^2 suggests that, after matching, there are no systematic differences in the distribution of covariates between the two groups.

[21] As these balancing tests suggest that matching was successful, we did not impose any stricter distance (at the cost of losing observations) to define the nearest neighbor in the presented results.

results minimize endogeneity concerns, suggesting that our primary findings are not driven by CEO selection. Table A.8 in the Appendix repeats this procedure to compute the average difference in sales. Results are similar, though of a lower magnitude.

Table 1.8: Newcomers and firm performance: propensity score match

		Firm's managed by 1 CEO		Firm's managed by more than 1 CEO	
	All firms	1 CEO: not the owner	1 CEO: the owner	More than 1 CEO: no owners	More than 1 CEO: all owners
	(1)	(2)	(3)	(4)	(5)
Newcomers	0.158***	0.094***	0.190***	0.182**	0.150***
	(0.010)	(0.032)	(0.013)	(0.074)	(0.028)
Observations	107,294	10,843	56,113	3,406	22,535

This table presents estimates of the average difference between a firm's average GVA during the crisis period run by a newcomer CEO and the alternative of not being in charge a newcomer CEO. These estimates were obtained using a propensity score match method, matching firms based on the firm's and CEO's pre-crisis characteristics (gender, age, education, firm size, average performance). Regressions were estimated for different samples, depending on the number of CEOs and owners of the firm. Robust standard errors are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Next, in Table 1.9, we rely on the weights of the previous matching procedure to repeat our baseline difference-in-differences regression (equivalent to Table 1.5) on a matched sample. The insignificance of the newcomer dummy across all specifications confirms the success of our matching procedure and the significant and economically relevant coefficient on the interaction of the newcomer and crisis dummies validates our previous results, suggesting only a small reduction in the size of the performance gap.[22] [23]

[22] As an alternative matching procedure, weights were also computed following coarsened exact matching. Results remain fairly unchanged.

[23] Results for the complete sample and the sample of owner CEOs are summarized in Figure A.3 in Appendix A.3.

Table 1.9: Matched Difference-in-differences

	Firm Performance			
	Gross Value Added		Sales	
	OLS	Firm fixed effects	OLS	Firm fixed effects
	(1)	(2)	(3)	(4)
Crisis	-0.169***	-0.170***	-0.122***	-0.127***
	(0.047)	(0.047)	(0.022)	(0.022)
Newcomers	0.014	0.008	-0.025	-0.027
	(0.041)	(0.041)	(0.025)	(0.026)
Newcomers×crisis	0.103**	0.104**	0.147***	0.152***
	(0.042)	(0.042)	(0.029)	(0.029)
Observations	11,334	11,334	11,527	11,527

This table presents difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of GVA and total sales on newcomer CEOs. The treatment variable is defined as being a newcomer in a top management position at the firm. The treatment period is the period of crisis, namely 2011-2013. The OLS regressions also include district and industry dummies. The sample consists of a matched sample (based on pre-crisis covariates) managed by 1 CEO (with no CEO turnover during or immediately before the crisis) and for which CEO data are available. Robust standard errors adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Finally, to address any remaining endogeneity concerns associated to the strategic selection of the CEO, we adopt a latent instrumental variable approach. We proceed by constructing instruments as simple functions of the model's data, using Lewbel's (2012) method. The Sargan–Hansen test of overidentifying restrictions suggests that the constructed instruments are valid and the results for two regressions on GVA and total sales (summarized in Figure A.4 of Appendix A.3) reaffirm our main conclusions.

1.4 Newcomers and firm survival

In this section we examine the impact of newcomer CEOs on the probability of firm survival. The relevance of this analysis is twofold. On the one hand we use the probability of firm survival for robustness, as an additional measure of performance. If newcomer CEOs increase the probability of a firm staying in business, this validates our argument that they cope best with unstable and demanding economic contexts, such as severe crises. On the other hand, this analysis may help confirm that our primary results are not driven by firm selection, whereby firms run by talented newcomers survive and remain in the sample, whereas firms run by less talented newcomers don't survive and drop out.

Table 1.10 presents the results of probit regressions where the dependent variable equals 1 in time t if the firm doesn't fail that year. We control for the same set of firm and CEO characteristics as in Table 1.5, including industry fixed effects, but coefficients are not shown for reasons of parsimony. The coefficient on the interaction term now captures the percentage increase in the probability that a firm run by a newcomer survives during the crisis, as opposed to other firms.

We find that, during the crisis, the probability of survival decreased for almost all firms. As for firms run by newcomers, while they have an 7 per cent lower probability of survival in the normal period, this reverts during the crisis, so that the probability of survival is almost 10 percentage points higher for firms run by newcomer CEOs (once we correct for the base disadvantage). In columns 2-5 we also estimate the probability of firm survival for the samples run by one or more CEOs, owners and not owners. Our results are confirmed for the one CEO subsamples, owner or not owner. Results are not there for the sample of firms run by multiple CEOs. Note that, in this case, for the firm to be classified in the treatment group it is sufficient that only one of the CEOs in office is a newcomer. Moreover, given that decision power is rarely equally shared and we can't control for who is actually in charge, it is harder to draw clear conclusions, as the existence of a single newcomer CEO may not be enough to impact the firm's survival during the crisis.

Table 1.10: Newcomers and firm survival

		Firm's managed by 1 CEO		Firm's managed by more than 1 CEO	
	All firms	1 CEO: not the owner	1 CEO: the owner	More than 1 CEO: no owners	More than 1 CEO: all owners
	(1)	(2)	(3)	(4)	(5)
Crisis	-0.249***	-0.350***	-0.354***	0.089	-0.182***
	(0.016)	(0.049)	(0.022)	(0.082)	(0.038)
Newcomers	-0.070***	-0.080**	-0.085***	-0.262***	-0.166***
	(0.014)	(0.038)	(0.020)	(0.057)	(0.034)
Newcomers×crisis	0.093***	0.143**	0.124***	-0.138	0.091
	(0.021)	(0.064)	(0.028)	(0.124)	(0.056)
Observations	337,314	50,162	174,819	15,575	65,689

This table presents difference-in-differences estimates of probit panel regressions of firm survival on newcomer CEOs. In these regressions the treatment variable is defined as being a newcomer (low tenure and externally recruited). The treatment period is the period of crisis, namely 2011-2013. The regressions were estimated for different samples, depending on the number of CEOs and owners of the firm. The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The regressions also include industry dummies. The sample includes all firms for which exists available data for at least one CEO (with no CEO turnover during or immediately before the crisis). Robust standard error adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

1.5 Newcomers and firm productivity

So far we have uncovered the impact of newcomer CEOs on a firm's performance during a crisis in terms of scale – firms run by newcomers register higher total sales and in terms of value added – newcomers generate more value for the firm during the crisis. We now examine another economically meaningful perspective, namely whether better performance as far as sales and value added are associated with an increase in productivity levels.

Management practices are relevant for productivity, particularly so in times of uncertainty and crisis. Several authors, including (Bloom et al., 2016, Bloom and Van Reenen, 2010), found a robust link between management practices and firm productivity.

In this section, we study three straightforward measures of productivity, namely: apparent labor productivity, measured as GVA or Sales per worker, and GVA per unit sold.

Figure 1.4 summarizes the main results, from difference-in-differences estimations including the same set of covariates as the baseline regressions in Table 1.5. Overall, we document higher productivity levels in firms managed by newcomer CEOs, during the crisis. Specifically, firms which are apparently equal ex-ante experience 25 per cent (22 log points) higher GVA per worker during the crisis if directed by a newcomer CEO, who doesn't own the firm. This figure is almost unchanged (24 per cent), when productivity is measured in terms of sales per worker. These figures result in a positive impact on th eratio of GVA to sales which is only significant at 15%. However, if we don't restrict the sample to single non-owner CEOs, we also find that firms managed by newcomers generated more value per unit sold during the crisis, than otherwise similar firms in the control group.[24]

These figures are both statistically and economically very significant. Consider Bloom et al. (2016), who estimate that differences in management practices account for about 30% of productivity differences between countries and, within countries, across firms. In times of crisis, we argue that at least one third of these differences can be explained solely by the type of CEO hired.

[24] Results regarding the other samples of firms are presented in Table A.10. Results on the matching procedure that gave rise to the matched sample are also presented in the Appendix in Table A.9.

Figure 1.4: Newcomer and firm productivity: a difference-in-differences approach
The dots stand for the estimated coefficients and the lines represent the 95 per cent confidence interval. Detailed results can be found in Table A.10.

1.6 What newcomers do differently

In this section, we examine whether the presence of newcomer CEOs translates into different management practices. We want to identify the extent to which newcomer CEOs act differently. We estimate Equation 1.1, replacing the dependent variable by several variables that reflect the cost policies, employee decisions, financing and investment decisions and the firm's engagement in foreign markets.

We performed a matched difference-in-difference estimation as before for firms run by a non-owner CEO. Results are summarized in Figure 1.5. The matching was very successful, as it eliminated any significant differences in the pre-crisis period. The weights used in the regression are based on the propensity score match procedure as in Subsection 1.8, presented in Table A.11 in Appendix A.3. Results are in line with the takeaways below.

Newcomer CEOs spend more during the crisis period, but do so more efficiently, without presenting higher costs per unit sold and decreasing the share of employee costs in total costs. Regarding employee decisions we find that, during the crisis, the number of workers and employee related expenditure increases in firms run by newcomer CEOs. In addition, costs per worker also increase. While the direction of causality is not straightforward, it is plausible that under newcomer CEOs, workers were better managed and faced incentives to perform better.

Newcomer CEOs are associated to more leveraged firms during the crisis in terms of long-term debt, contrary to the general tendency of higher short-term debt in times of crisis.[25] This suggests newcomers are better able to access credit during the crisis, particularly long-term debt, which requires higher trust on the part of lenders, and credibility on the part of firms. The difference is quantitatively very significant, and possibly enough to foster performance and survivability of firms. This is also consistent with newcomer CEOs being less risk-averse and more motivated to take risks and make changes, even in times of crisis. Berger et al. (1997) find significantly lower leverage in firms whose CEOs have several characteristics of entrenchment, including high tenure. Along the same lines Serfling (2014) finds a lower operating leverage associated to older CEOs. Finally, we find no significant evidence for differentiated behavior in terms of international trade.[26]

In sum, the results regarding cost policies and leverage suggest indeed a less risk-averse management strategy associated to newcomer CEOs (Faccio et al. 2016 and Serfling 2014 use leverage as a measure of risk-taking), as hypothesized.

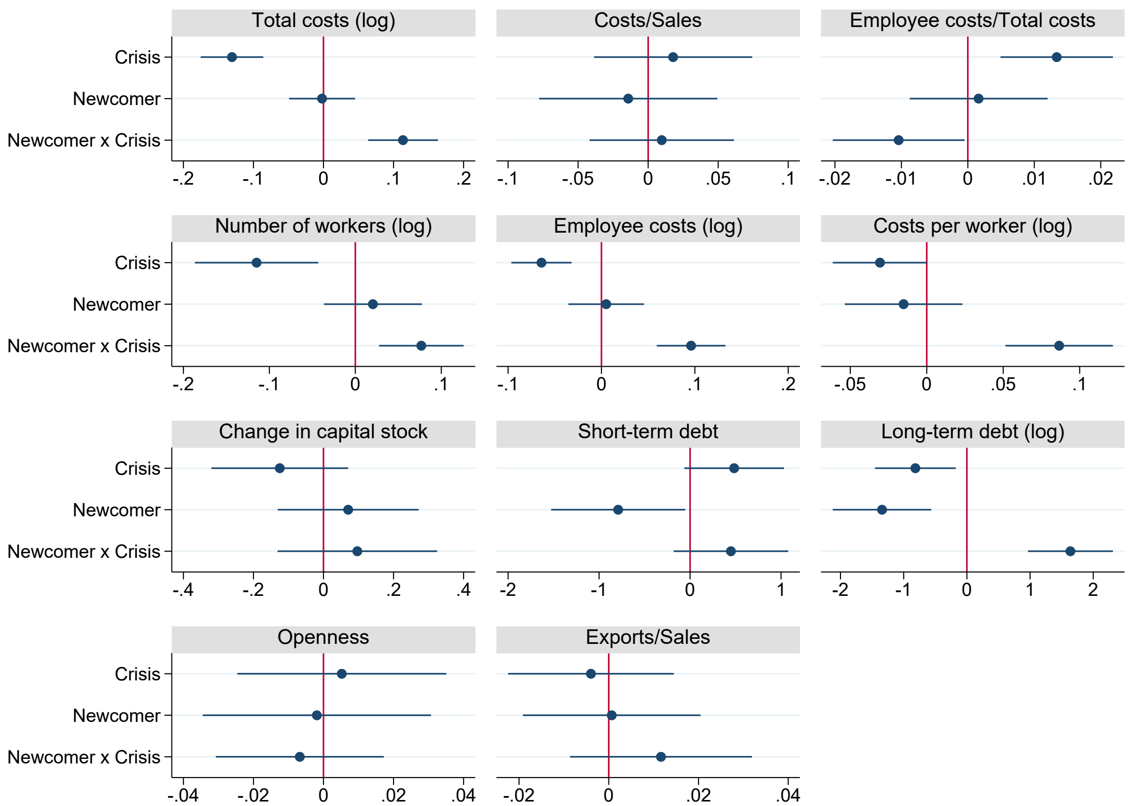
It should be noted that this preliminary analysis is suggestive, and not intended to uncover exactly what lies behind newcomers' success in times of severe crisis. In

[25] According to Lewellyn and Muller-Kahle (2012), young executives are more inclined to embark in risky subprime mortgage lending.

[26] Figure A.5 in Appendix A.3 summarizes the results for the complete sample and the sample of owner CEOs.

particular, the direction of causality between management decisions and firm performance is not established. Nonetheless, our results strongly suggest that differences in performance and survival map into different management practices, worth investigating.

Figure 1.5: Key policy instruments: matched difference-in-differences
Note: The dots stand for the estimated coefficients and the lines represent the 95 per cent confidence interval.



1.7 Concluding remarks

This paper examines how the presence of newcomer CEOs favors firm performance and increases its survivability during a major exogenous crisis, where newcomers are CEOs hired externally and new to the top management position. By focusing on a period of unprecedented and unexpected downturn, the economic crisis of 2011 to 2013, and examining the behavior of firms in Portugal, we mitigate endogeneity issues related to firm-CEO matching. We find clear and robust evidence that newcomer CEOs deliver better firm performance, and increase firm survivability. We test different specifications, notably the inclusion of CEO and firm controls, firm and CEO fixed effects, and alternative measures of performance. Our results are robust to subsamples of one and multiple CEOs, owners or not. Firms led by newcomers don't necessarily perform better under normal times; it is during crisis that such CEO characteristics become an asset.

As acknowledged in the literature, new CEO hirings are a major challenge. Choosing the wrong management team can jeopardize the future of the firm, while a good hire can bolster performance. The choice of CEO needs to balance different trade-offs, while considering expected underlying business conditions. In this regard, a significant economic shock complicates the choice of CEO and can turn an ex-ante good match into a poor match ex-post, or vice-versa. In this paper, we show how the value of hiring a newcomer increases under a severe shock or change in the economic context. In sum, newcomers perform better in uncertain and dire times and, if the possibility of unexpected severe shocks is duly taken into consideration in the selection process then, ceteris paribus, external hires should be given more consideration.

We find that firms which enter the crisis managed by newcomer CEOs deliver about 19 per cent higher value added than equivalent firms managed by higher tenured or internally promoted CEOs. The probability of a firm surviving the crisis also increases for firms with newcomer CEOs. We show that the margin that matter for firm performance is a combination of being new at the job, but also at the firm. Our results question traditional definitions of human capital based on seniority and tenure, especially its robustness to crisis times, suggesting, at the very least, that accumulated experience in the firm is not key in periods of economic distress. In dire times, flexibility, adaptability and risk taking preferences, characteristics we cannot directly observe, but which are more likely to be present among newcomers, become more valuable than experience. Indeed, during the crisis, newcomer CEOs make management choices that are different from those of other managers, and consistent with the view that newcomers are less risk-averse, as they borrow more, and follow less restrictive spending strategies.

Different mechanisms may explain this result. First, newcomers, less familiar with current business practices, are more likely to draw on their experience in other firms, and quickly adjust to challenging circumstances, when business as usual and past practices are ineffective. The incentives that a newcomer CEO faces are also different. A better performance by a newcomer CEO may associate with a higher net benefit in case of a good performance in his initial years, when uncertainty over ability and suitability for the job are higher. The incentives line up for a recently-hired CEO to risk more in face of new circumstances. In addition, good performance delivers benefits for a longer period, as a new manager in a given firm faces a longer expected horizon at the job.

Future research should focus on deepening our analysis of the different management practices of newcomer CEOs, and the possible facilitation of transitions in top management positions when aggregate economic crises arise, or, more generally, the creation of appropriate incentives to make CEOs follow the best management practices in all times.

Chapter 2

Networks and Manager Pay: Evidence from Time-Varying Exogenous Metrics

2.1 Introduction

High executive pay has gained increasing salience in the media and in academia. In the public mind, abnormally high and rising levels of executive pay stem mostly from abusing managerial power to secure excess rents, not rising productivity. Indeed, high levels of pay may compensate for a unique set of capabilities and responsibilities, in a stressful environment, or arise just from a combination of personal power and lack of scrutiny, which create room for rent extraction (Bebchuk et al. 2002).

This paper provides an in-depth analysis of a specific indicator of managerial power — the professional network. Strong professional networks may increase both managers' productivity, as well as their ability to extract rents from the firm, in the form of high pay.

Jackson (2014) argues that the ability to accurately model human behavior requires recognizing the social nature of humans, which in turn implies understanding the interaction patterns shaping behavior and decisions. While networks have long been the subject of study in sociology, only more recently has their analysis gained momentum in the finance and economics literature. Network analysis aims to identify the structure of interactions, and consider how observations are connected, not isolated. Several studies have analyzed the social ties of top executives, including authors such as F. Hallock (1997), Core et al. (1999) and Liu (2014), who have explored observable, formal business settings, such as boards of directors. Other authors explored informal settings, such as connections through a common background, including attendance of educational institutions (Engelberg et al., 2013), region of origin (Hwang and Kim, 2009), experience in civil service (Kramarz and Thesmar, 2013). Brown et al. (2012) has proposed a broader concept of social network that includes all acquaintances from employment, education and social activities. Gomez-Mejia et al. (2001) analyze closer and deeper bonds, such as family ties.

We build a new indicator of a manager's network as the number of all past professional interactions, within the same firm, with co-workers who later become top managers. At the time of the interaction as co-workers, one or both workers may have not yet become top managers, but we interpret their later investiture as top manager as revealing a valuable connection. In other words, in our paper, a top manager's current network is larger, the more often they coincided in the past with other workers who have also become managers.

Our study benefits from a vast country-wide micro-level dataset that collects the full set of information on a worker's employment history. We are able to verify whether any two current managers have worked in the same firm in the past, during the same year. If that is the case, we consider that there is a tie between the two current top managers. Our definition has clear conceptual advantages over existing definitions in the literature. First, and most important, our network metrics evolve over time for reasons exogenous to the manager, avoiding endogeneity problems associated to self-selection. This is because a specific manager experiences changes in her network when other workers she met in the past become managers, or leave the job market. Second, the comprehensive and large sample of firms and years allows us to capture variations across a long time horizon, in addition to the cross-sectional variation most studies rely on. Third, it avoids issues of simultaneity and reverse causality, as it focuses on past professional encounters, at a time when the future potential of each worker was yet to be fulfilled. Other studies are vulnerable to reverse causality, as it is hard to establish whether it is the large network that allows the manager to extract higher wages, or if managers that are well-paid naturally build stronger networks. The very rich and comprehensive dataset also allows us to define networks in a manner that avoids circular/perfectly overlapping groups, and as explained in Bramoullé et al. (2009) that allows us to overcome the 'reflection problem' identified by Manski (1993).[1] In sum, our measure is comprehensive, varies over time and across top managers, and addresses the issue of endogeneity by using past events and capturing changes in a manager's network that are exogenous to the manager, his ability, and choices.

It is important to notice that in any kind of setting, people cannot count on all the members of their network for help or exchange of information. Not only because some of the elements of the network may not be willing to help, but also because, in this particular case, there is no guarantee that having worked with someone at the same firm at the same year implies having met that person. We propose, as a novelty in the literature, to weigh these past encounters by the time future top managers have worked in the same firm, in the same year, as well as inversely by the firm's size. This allows us to understand how working together for longer in smaller firms, thus likely establishing

[1] If all members of a group are linked, and the pay of one manager depends on the average pay of the group, then an identification problem arises, making it impossible to disentangle the network effect.

longer and deeper links may, or not, affect the results. In addition, we argue that it is also important to consider those weaker links, defined as few years in common in large firms. While there is a chance, that any two workers who have worked at a large firm, didn't actually meet, there is a virtual link between them that can be activated at any given moment. We advocate that it is much easier to approach someone to ask for information or to create a business connection if there exists some vague link that allows to break the initial ice and unlock the channel of information.

We hypothesize that a higher number of ties provides an effective channel for the exchange of information or transmission of ideas, which empowers the manager and increases her bargaining power (Fracassi, 2016), leading consequently to a higher pay. According to Engelberg et al. (2013) one of the assumptions needed for a top manager's network to influence their wage is that it must accrue value to the firm. We argue that a network mustn't necessarily add value to the firm, but the firm has to believe it does. In accordance, we organize the potential benefits of a larger network into three types, which add objective and subjective value to the manager.

A larger network provides information on business opportunities, objectively increasing the value of the manager to the firm.[2] A well-positioned manager in a network can act as bridge between the firm and their connections, and therefore the cost of losing this manager gets amplified by the number of other potentially valuable connections that will be lost.

While access to a larger information set objectively increases the competence and productivity of the manager, the increase in bargaining power of the manager may include a subjective attribution of value. As firms cannot unequivocally connect results to manager competence, they may infer the latter from the manager's network. Manager

[2]In addition to present benefits, the firm might benefit in the future from these connections provided certain conditions materialize - for instance, when planning mergers, acquisitions or expanding into new markets.

wages can thus increase with the size of the network for objective or subjective reasons. This view is congruent with the idea that a large network reflects reputation and experience and can thus be regarded as an indirect measure of quality (Renneboog and Zhao, 2011). This is specially relevant during the recruitment process, where despite all the efforts put into hiring the most capable top managers, the firm faces a problem of incomplete information regarding the quality of the manager-firm match.

Finally, there is also the sole bargaining power granted by networks. Since networks reduce job search frictions, highly connected managers are better informed about their outside options and can leverage this information in their favor to negotiate higher pay. In sum, regardless of the specific mechanism, theory predicts a wage-premium for managers who are able to leverage their connections to potentially benefit the firm.

While the literature is mostly consensual regarding the positive impact of networks on wages, this paper advances several key contributions. Firstly, we address endogeneity concerns thoroughly, through the inclusion of high-dimensional fixed effects, including firm and manager fixed effects and an instrumental variable approach, aside from the network definition *per se* that explores exogenous changes in the networks. The use of a unique micro-level dataset allows the use of a richer, time-varying definition of networks, encompassing professional connections that are not restricted to present ties, like most studies thus far. Moreover, we assess the impact of connections with potentially distinct values to the manager, a question not addressed in the literature. Relying on different measures of network theory, we analyze whether it is the quantity or quality of connections that prevails, where quality is defined in terms of either the depth or the power status of the connection. In addition, we look into whether it is the number of direct ties that plays the main role, or the manager's global position in the aggregate network. This is an important angle of analysis, given that the latter is a better proxy for the information value of networks, allowing us to shed some light on whether firms can actually benefit from the manager's network or not. We also unveil the type of firms that value more their manager's networks.

Our empirical analysis generates three main insights. First, results suggest a consistent impact of managers' networks on their total pay. This result is robust across different specifications. A one standard deviation increase in the crude network measure that accounts for the number of connections is associated to a 8% higher bonus - the more volatile component of compensation. Part of the network premium is closely linked to being better informed about job opportunities and thus having better outside options that allow to match to higher paying firms. While this is a direct benefit derived from the network, it will be absorbed by the firm fixed effect. The individual and firm fixed effects control for any innate manager characteristics and time-invariant firm characteristics, at the cost of clearly sub-estimating the network premium. Nonetheless, even including these high-dimensional fixed effects, we still find a statistically significant 8% network premium for the compensation bonus and 5% for total pay. Second, we find that quality prevails over quantity. In particular, deeper connections - i.e. longer and closer connections - are more valuable to the manager. Third, indirect measures that reflect better access to valuable information are also associated to higher pay, suggesting that it is not only who they know, but also what they know through them that matters. Our results are not firm-specific, and hold across different kinds of companies, however evidence suggests that those who pay most for well connected managers are large young firms in the services sector. Finally, a preliminary analysis suggests that networks are not overpaid and translate into higher firm productivity.

The remainder of this paper proceeds as follows. In Section 2.2, we carefully define the manager's network and explain the metrics underlying its topology that are used in the analysis. In Section 2.3, we describe the database. Section 2.4 presents the empirical results relating manager networks to a pay premium and several robustness checks. Concluding remarks are provided in Section 2.5.

2.2 Networks

2.2.1 Network definition

Our network is a set of managers (referred to as 'nodes' in network theory) and the connections between them ('edges', formally).To construct a manager's yearly network we take various sequential steps: first, we identify all the firms where top managers have worked at in the past; we then list all the employees who worked at the firms identified in the first step, at the same time as our managers; finally, we construct yearly network measures for the manager in question, considering as connections only past co-workers who are currently managers as well (excluding same-firm managers to avoid simultaneity problems). We assume that a connection is activated - or becomes 'valuable', only when both parties are managers and it remains a connection until one or both parties cease to be a manager - by being demoted or leaving the job market entirely. This procedure allows us to define all the managers that form part of a network and, once they are identified, characterize each connection by the depth of the link itself and the power of the link (as defined below). Note that, for a connection to be established between two managers, they need not have been managers when they met, but they are so at the moment they count as part of each other's network (see Figure 2.1).

Notice that the procedure to construct the network is computationally very intensive and extremely time consuming. The vector containing all interactions (every two managers who worked in a given same firm at the same year) is composed of approximately 563 million observations. All network metrics were computed using the R package igraph (Csardi and Nepusz, 2006).

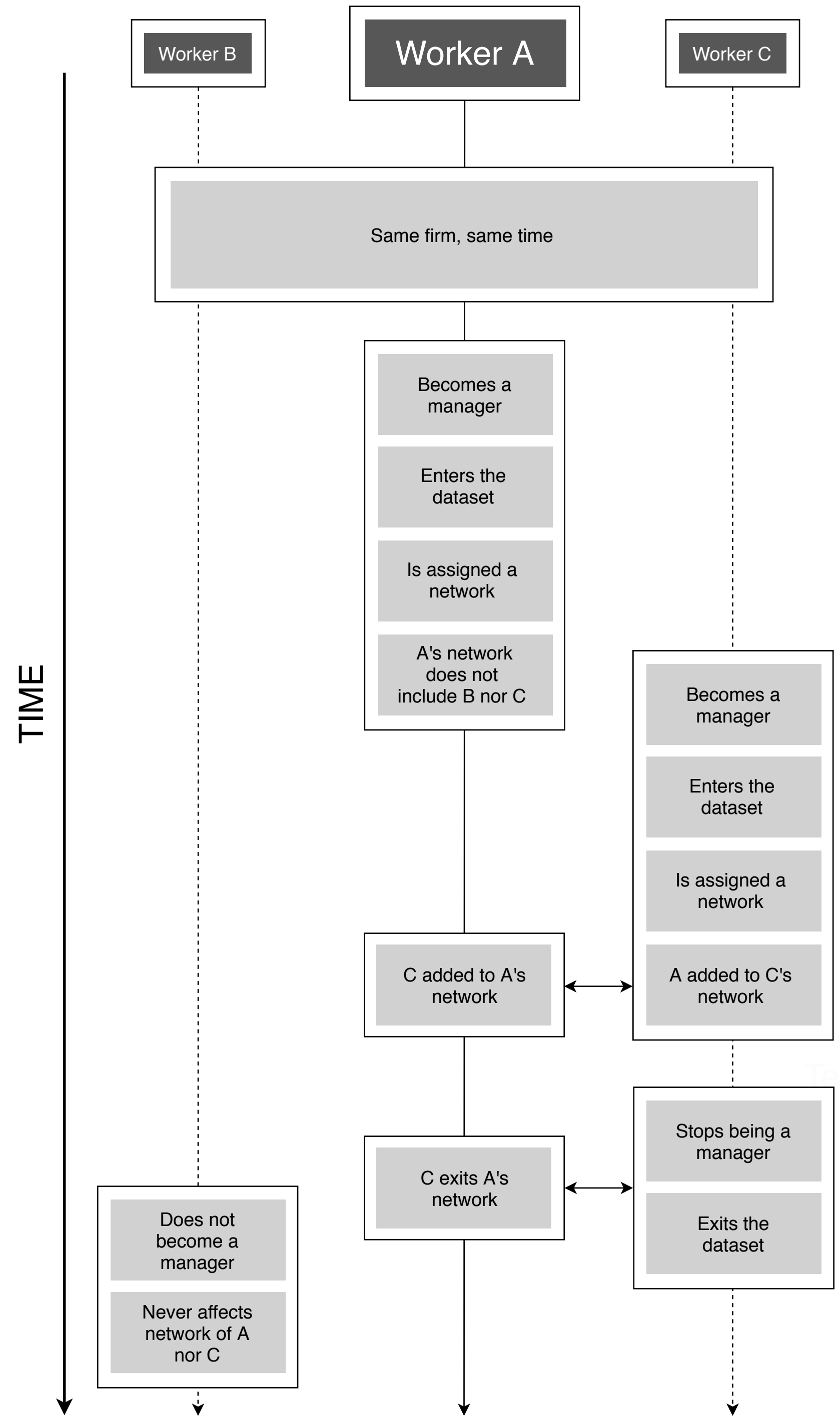


Figure 2.1: Network definition

2.2.2 Network metrics

Figure 2.2 depicts the network of a random manager in our sample in 2017. In the figure, each manager is denoted by a node, and a connection between two nodes - signaling the two managers have worked together in the same firm, in the past, is a line in the picture. The particular manager represented has 34 connections, *i.e.* 34 past co-workers who also occupy a management position in 2017.

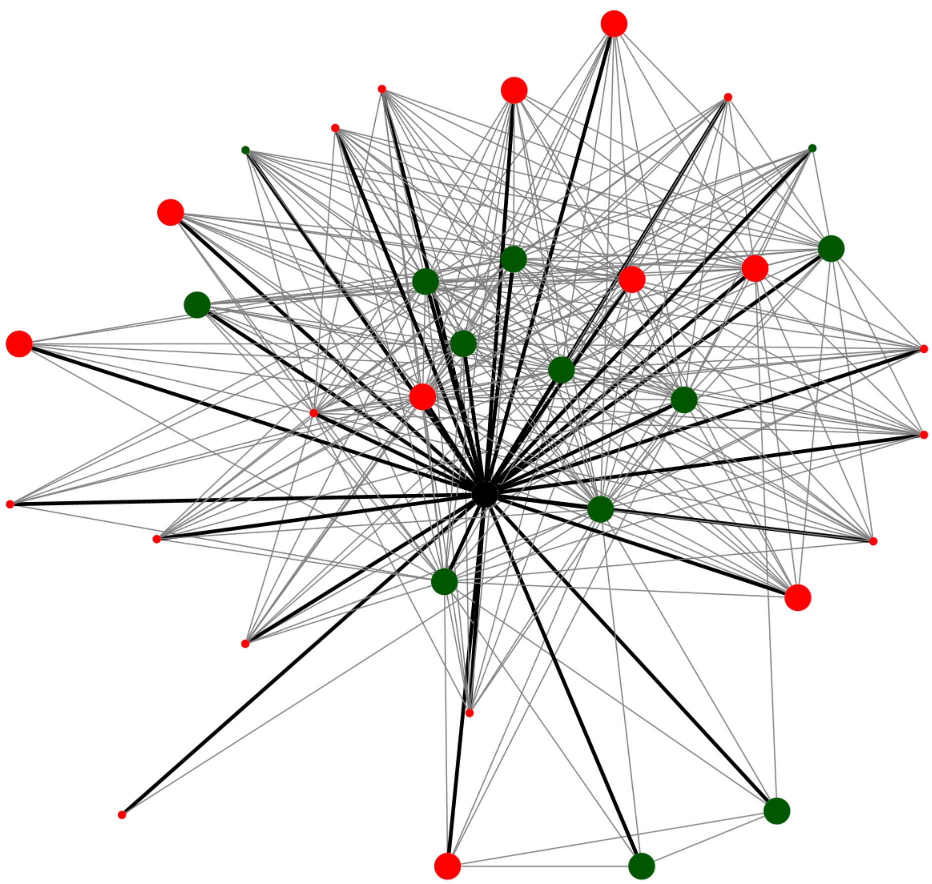


Figure 2.2: A fragment of the network of a top manager
The black node represents a randomly selected manager in the top quartile of the pay distribution. The black lines correspond to his direct connections. Green/red nodes are managers who earn more/less than the black node in 2017. Large/small nodes have a higher/lower level of *Degree* in 2017 than the black node. Grey lines represent connections amongst the direct connections of the black node.

The *Degree* centrality of a node is composed of the number of connections:

$$\mathrm{D(i)} = \sum_{j \neq i} x_{ij}, \tag{2.1}$$

where x_{ij} is 1 for the presence of a link between i and j.

A node's *Depth* is computed as the sum of weights on all direct links, where each weight is defined as the number of years any two managers coincided in the same firm, divided by the firm's size. This weighted network measure attributes a higher value to connections who are likely to be stronger, either because the two managers worked together for a longer period of time, or because they did so in a smaller firm, facilitating more frequent and closer interactions between co-workers.

$$\mathrm{S(i)} = \sum_{j \neq i} w_{ij} x_{ij}, \tag{2.2}$$

where w_{ij} is greater than 0 for the presence of a link between i and j and the value represents the weight of the tie defined as:

$$w_{ij} = \frac{Years_{ij}}{FirmSize_{ij}}, \tag{2.3}$$

Finally, a node's *Power* is derived from how powerful the connections of a manager are. It is computed as the sum of weights on all direct connections, where each weight is the average size of the firm managed, divided by the average number of managers at those firms. This weighted network measure attributes higher values to connections likely to be more valuable as implied by the average size of the firm managed by that connection. In sum, whereas a node's strength is related to the intensity of the past connections between managers, power is a proxy for the importance in corporate leadership of the nodes that form part of a manager's network.

$$\mathrm{P(i)} = \sum_{j \neq i} w_{ij} x_{ij}, \tag{2.4}$$

where w_{ij} is greater than 0 for the presence of a link between i and j and the value represents the weight of the tie defined as:

$$w_{ij} = \frac{\sum_j FirmSize_j}{\sum_j NumberManagers_j}, \tag{2.5}$$

where $FirmSize_j$ represents the size of all firms managed by manager j and $NumberManagers_j$ is the number of managers of these same firms, to account for the number of managers' amongst which the decision power is divided.

As a more global network measure, which takes into account indirect ties, we consider *Betweenness centrality*. The *Betweenness* of node i is defined as the sum of betweenness ratios, *i.e.* the number of geodesic paths - shortest path, which is not necessarily unique, between any two nodes passing through node i, divided by the total number of geodesic paths between those two nodes. This is:

$$B_i = \sum_{j \neq i \neq k} \frac{g_{jik}}{g_{jk}}, \tag{2.6}$$

where g_{jik} are the number of geodesic paths between j and k that pass through i. Given the dimension of our graph, we considered only paths of lengths smaller than 3. *Betweenness centrality* is commonly used as a measure of control over the network, as a higher *Betweenness* implies that more information will pass through that node.

PageRank is the algorithm behind the Google search engine and can be easily extended to a social network setting as in Eckbo et al. (2016). The *PageRank* centrality PR_i of node i is given by:

$$PR_i = \alpha \sum_j \frac{x_{ji}}{D_j} PR_j + (1 - \alpha) \tag{2.7}$$

where α is a constant (the damping factor), x_{ij} is 1 for the presence of a link between i and j, D_j is the *Degree* of node j if such *Degree* is positive, or $D_j = 1$ if the *Degree* of j is null. *PageRank* measures the quantity and quality of a node's connections.

2.3 Sample and data description

Our data draws on information from *Quadros de Pessoal* (henceforth *QP*), an official micro longitudinal dataset, with matched employer-employee data that include all private firms and workers operating in Portugal. The survey is mandatory for all establishments with wage-earners, and contains information regarding the firm - including size, and each of its workers - including gender, age, education, total compensation and hours worked. *QP* includes a personal identification number that enables tracing individuals across time, allowing the use of the entire professional history of a worker since the dataset's inception in 1986, that is, for a total of more than 30 years.

Each worker is classified according to the National Classification of Occupations and assigned a professional grade level in *QP*. Professional grade levels are defined by law, and each firm is mandated to classify each job in accordance. We restrict the sample to all managers between 1995 and 2017. We define a top manager as a top decision makers or as top management, as inferred from the available 6-digit occupational classification system that identifies all types of managerial occupations.[3] For the classification of top managers, we restrict the sample to workers classified in the highest hierarchical

[3] In *QP*, in the period between 1986 and 2017 the national classification of occupations was revised several times and the last change occurred in 2010. From this year onwards, we use the latest classification of occupations and are able to identify accordingly the workers classified as managers. Before that, we proceeded by using the official table of harmonization published by Statistics Portugal, to minimize entry and exit of managers attributable to this change.

grade level. While self-employed managers, i.e. firm owners, are included in the global network of each manager, they cannot be included in our regression analysis given the lack of data regarding wages, stemming from the specific compensation mechanism in place in owner-managed firms.[4] Our final sample consists of 1 077 233 manager-firm-years, representing around 135 424 firms and 2 776 44 top managers.

Table 2.1 and 2.2 present the descriptive statistics of firm and top managers characteristics, including the network variables. The average firm has around 1035 workers, of which roughly 15% have top management positions. However, half of the sample consists of small firms with less than 37 workers and 3 top managers. As shown in Table 2.1, the average age of top managers in the sample is 45 years old and the average tenure is 10 years. Around 28% of the sample are women, and 40% hold a college degree. The base salary accounts, on average, for 80% of a top manager's total compensation. While bonus pay plays a smaller role in total compensation, it displays a wider variability and is a significant fraction of total pay for part of the sample.

As for network measures, a top manager benefits on average from 66 connections to other managers, but there is significant variation in the number of connections across the sample. Indeed, the standard deviation of direct connections is 137, and around half of the sample has 9 or fewer connections. Table 2.2 presents the correlation between top managers' network measures in the sample. *Degree*, *Power*, and *PageRank* have correlations in excess of 0.60, while *Depth* displays very low correlations with the remaining measures.

Figure 2.3 depicts developments in the average network measures for each additional year of experience at a top management job. It shows that *Degree* and *Betweenness* centrality are the metrics that present larger increases during the first years at the job. While the latter continues growing steadily, *Degree* seems to increase on average less

[4] In other words, in our exercise, firm owners add value to other managers' networks, but we cannot analyze how their networks impact their wages.

after the first 8 years of experience. *PageRank* presents the lowest growth rates.

Table 2.1: Summary statistics

	Mean	Median	Std. Dev.
Firm Characteristics			
Number of workers	1035.26	37	3005.09
Number of managers	151.50	3	573.53
Number of establishments	43.36	1	142.22
Manager Characteristics			
Gender	0.28	0	0.45
Age	44.61	44	10.30
Education	4.10	4	1.06
Tenure (years)	10.29	8	9.09
Hourly total compensation	21.10	16.05	30.20
Hourly base wage	16.82	13.51	16.13
Hourly bonus	4.61	1.16	23.67
Degree	65.96	9	137.35
Depth	0.17	0.09	0.25
Power	1123.63	112.67	2780.73
Betweenness	96334.29	937.11	365166.40
PageRank (x10^6)	6.30	4.41	6.71

This table presents the summary statistics of firm and manager characteristics for 916,511 firm–year observations between 1995 and 2017. *Degree*, *Depth*, *Power*, *Betweenness* and *PageRank* are network metrics defined in Section 2.2.2. Education is a categorical variable where: 1 - less than primary education; 2 - 1st and 2nd cycle of primary education; 3 - lower secondary education; 4 - upper secondary education; 5 - tertiary education. See Appendix B.1 for definitions of the other variables.

Table 2.2: Correlation matrix of network variables

	Degree	Depth	Power	Betweenness	PageRank
Degree	1.0000				
Depth	0.1715	1.0000			
Power	0.8093	0.0969	1.0000		
Betweenness	0.4171	0.2842	0.4161	1.0000	
PageRank	0.6038	0.3393	0.6378	0.5505	1.0000

The network metrics are defined in Section 2.2.2.

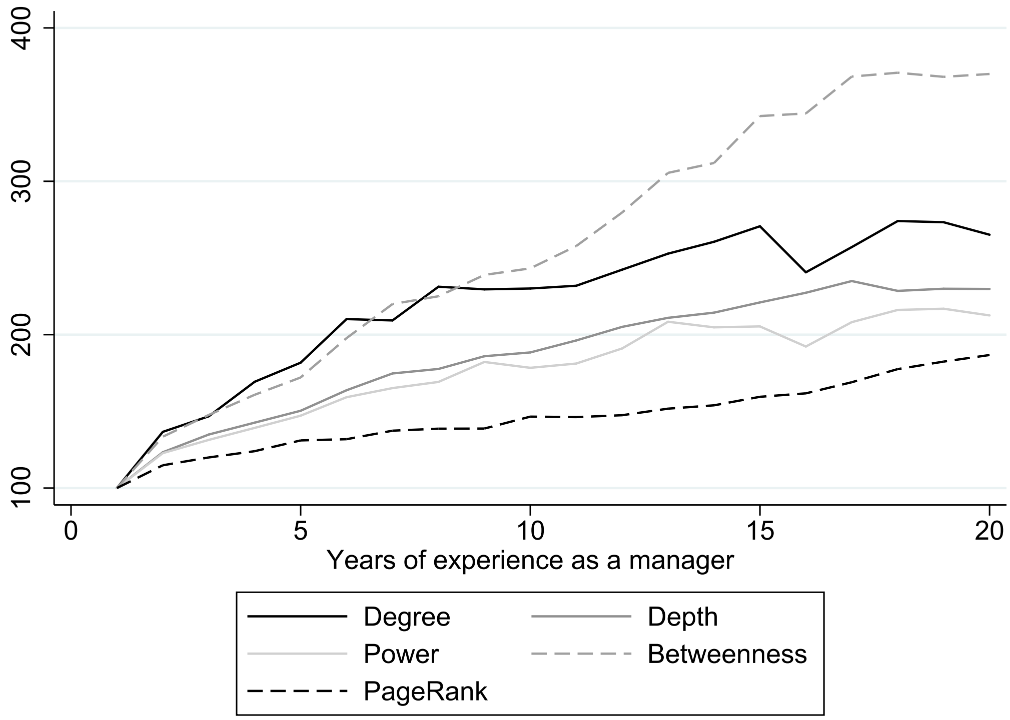


Figure 2.3: Average network metrics by years of experience: index 100

Descriptive statistics comparing managers with high centrality to managers with low centrality, according to each of three network metrics: *Degree*, *Depth*, and *Power*, are presented in Table 2.3. Managers of larger firms are, on average, better connected, and better connected managers are more likely older, educated, males, with longer tenures. Better connected managers are also paid more, about twice as much per hour as poorly connected managers. Also, note that filtering a manager's network into high and low according to *Depth*, leads to a reversion of the split of managers into large and small firms, with higher network managers associated with small firms, where interactions are more frequent and, possibly, intense.

Table 2.3: Highly connected managers: statistics

	Degree centrality			Depth centrality			Power centrality		
	High	Low	Difference	High	Low	Difference	High	Low	Difference
Firm charact.:									
No. of workers	2108.22	118.61	1989.61***	733.23	1316.70	-583.49***	2069.60	109.48	1960.13***
No. of managers	314.93	11.88	303.05***	74.54	223.24	-148.70***	304.77	14.32	290.45***
No. of establishm.	88.99	4.39	84.60***	42.10	44.54	-2.44***	87.63	3.75	83.88***
Manager charact.:									
Gender	0.23	0.32	-0.08***	0.24	0.31	-0.07***	0.24	0.31	-0.07***
Age	45.05	44.23	0.82***	44.88	44.36	0.52***	44.92	44.33	0.59***
Tenure (anos)	11.15	9.55	1.60***	9.24	11.26	-2.03***	11.02	9.63	1.39***
Education	4.49	3.76	0.73***	4.27	3.94	0.33***	4.49	3.75	0.73***
Hourly total pay	29.86	13.62	16.24***	25.51	16.99	8.52***	29.44	13.64	15.81***
Hourly base wage	22.95	11.57	11.38***	20.32	13.54	6.77***	22.65	11.59	11.06***
Hourly bonus	7.11	2.46	4.65***	5.48	3.80	1.68***	7.00	2.47	4.53***

This table presents the mean of manager and firm characteristics for the sample of high and low centrality managers and the associated difference. High-centrality is defined as having a centrality level higher than the sample median. Variable definitions are provided in Appendix B.1. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

2.4 Is there a wage premium associated to manager networks?

Figure 2.4 shows the empirical distributions of the log hourly wages of managers in the economy for different *Degree* centrality quartiles. As expected, raw wages for managers with larger networks, *i.e.* with a higher number of connections, are displaced to the right. Managers with a *Degree* level in the top (4th) quartile present additionally less dispersed wages. Given that the statistical evidence corroborates the existence of a network effect, we next propose to quantify more precisely this effect.

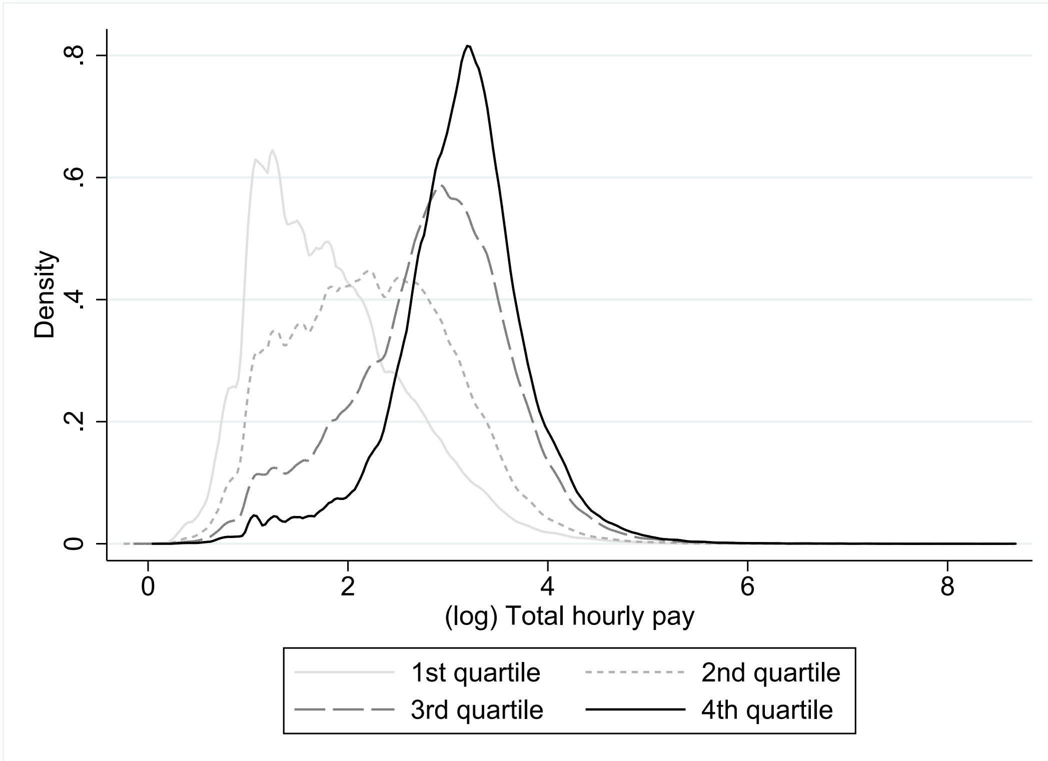


Figure 2.4: Distribution of (log) wages by *Degree* quartile
This figure reports kernel densities of log hourly total wages of managers for different *Degree* quartiles.

2.4.1 Baseline regressions

We now proceed to examine the effect of a manager's network centrality on her compensation, using a standard Mincerian wage equation to which we add high-dimensional firm and manager fixed effects, following Torres et al. (2018), as well as the network measures of interest:

$$\begin{aligned} Ln(w)_{ijt} = \beta_1 Network\, Measures_{ijt} + \beta_2 Manager\, Characteristics_{ijt} \\ + \beta_3 Firm\, Characteristics_{jt} + \eta_t + \gamma_j + \alpha_i + e_{ijt} \end{aligned} \tag{2.8}$$

In the above equation the outcome variable is the natural logarithm of the real hourly wage of manager i, in firm j, at year t.[5] *Network Measures* stand for the different network metrics presented in the previous section. *Manager characteristics* is a vector of the manager's observed attributes, present in the standard wage regressions in the literature, including a gender dummy that takes the value of 1 for females, a categorical education variable that increases with the level of education - as defined in Appendix A, tenure at the current firm, tenure squared and age squared. Notice that the linear term age has to be dropped as it will be absorbed when manager fixed effects are added in a model that includes both manager and year fixed effects. *Firm characteristics* include the number of establishments, and the logarithm of the number of workers in the firm. It is key to control for firm size, as larger firms tend to be more complex and require more skilled and hence more highly paid managers (Gabaix and Landier, 2008). η_t are year dummies, $gamma_j$ are firm fixed effects - capturing observed and unobserved firm constant heterogeneity, and $alpha_i$ are manager fixed effects - capturing observed and unobserved manager constant heterogeneity. e_{ijt} is the error term, assumed to follow the conventional assumptions. T-statistics use robust clustered standard errors, thus adjusting for heteroskedasticity and within-manager correlation.[6] Estimation of Equation 2.8 by ordinary least squares (OLS) is complicated due to the inclusion of two high-dimensional fixed effects. However, by using an algorithm proposed by Guimarães and Portugal (2010) that consists of an iterative procedure (that alternates between the estimation of the fixed effect and of the coefficients, taking as given the last estimates of the coefficients or the fixed effects, respectively) estimates converge to the true OLS solution.

In Table 2.4, we regress managers' total hourly compensation on their number of

[5]Wages were deflated using the consumer price index (base 2017), but this correction is inconsequential for the regression analysis, since we always include year dummies.

[6]Clustering the standard errors at the firm level has no impact on the significance of the main results.

connections - *Degree*, following Equation 2.8. In specification 1 we only control for the observable covariates, as specified above. In specification 2, we add firm fixed effects, such that additionally within-firm changes in compensation are taken into account and in specification 3 we add manager fixed effects. In specification 4, aside from all standard firm and manager characteristics, we include both firm and manager fixed effects to address the issue of time-invariant unobservables, which may drive the manager-firm match. Our preferred specification 4 minimizes the possibility that firm-specific omitted variables, as well as innate manager characteristics, such as talent, are driving our results.

Our results show that *Degree centrality* displays a positive and statistically significant association with a manager's hourly wage throughout all specifications. The quantitative impact is of 1.9% for an increase of one standard deviation in the *Degree* measure. When we add firm fixed effects alone, that coefficient decreases to 1.1%, but it is 2.6% when we add manager fixed effects alone. More relevant, the introduction of both firm and manager fixed effects leads to an estimate of a 2.4% increase in a manager's total hourly wage. In the last column of the table, we add the squared network measure to the specification, as suggested in Engelberg et al. (2013). In this way, we account for diminishing returns, *i.e.* while a certain connection may yield valuable benefits, additional connections will deliver marginally less valuable benefits. Our estimate of the negative squared term, confirms decreasing returns to connectivity in manager wage regressions. Once we take this into account, a one standard deviation increase in a manager's *Degree* will lead to an approximately 5% increase in pay, reaching the maximum at 766 connections.

Our estimates compare well with the literature, which finds an effect of 9%, as in Renneboog and Zhao (2011) and Engelberg et al. (2013). Note that existing papers analyze more restricted samples, made up of UK and US listed firms only and mostly

Table 2.4: Managers' total pay and the network premium

	Manager hourly pay				
	OLS	Firm fixed effects	Manager fixed effects	Manager & firm fixed effects	
	(1)	(2)	(3)	(4)	(5)
Degree	0.0001***	0.0001***	0.0002***	0.0002***	0.0004***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Degree squared					-0.0000***
					(0.0000)
Gender	-0.2106***	-0.1400***			
	(0.0032)	(0.0025)			
Age	0.0642***	0.0589***			
	(0.0010)	(0.0009)			
Age squared	-0.0005***	-0.0005***	-0.0005***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure	0.0002***	0.0003***	0.0010***	0.0005***	0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure squared	0.0000***	-0.0000**	-0.0000**	-0.0000***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Education	0.3119***	0.1010***	0.0229***	0.0080***	0.0081***
	(0.0015)	(0.0016)	(0.0031)	(0.0022)	(0.0022)
No. of workers (ln)	0.1846***	0.0523***	0.0906***	0.0804***	0.0805***
	(0.0008)	(0.0023)	(0.0018)	(0.0020)	(0.0020)
No. of establishments	-0.0009***	-0.0004***	-0.0003***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Year dummies	✓	✓	✓	✓	✓
Manager fixed effect			✓	✓	✓
Firm fixed effect		✓		✓	✓
Observations	1,077,233	1,077,233	1,077,233	961,029	961,029

This table presents estimates of ordinary least squares (OLS) and firm and manager fixed effects panel regressions of the logarithm of manager total hourly pay on the network measure *Degree* and other manager- and firm-level control variables. The sample consists of all firms, for which data on their top managers is available in the 1995-2017 period. Variable definitions are provided in Appendix B.1. Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

relying on cross-sectionl-variation, without firm and manager fixed-effects.[7] Thus, for

[7] Renneboog and Zhao (2011) consider more specific links, namely sitting on the same director board, and finds that a one standard deviation increase in the *Degree* measure leads to a 9% increase in the total compensation of CEOs of listed firms in the UK. Similarly, using a sample of US firms, Engelberg et al. (2013) find a 9% network wage premium associated to a one standard deviation increase in the number of connections, including past professional, university and social connections.

a very wide sample of firms in a country characterized by smaller, mostly not listed firms, we confirm the existence of a quantitatively significant wage premium associated to a manager's time varying network, controlling for firm and manager time-invariant unobservables.

A number of papers have emphasized the role of fixed effects when it comes to management compensation. Graham et al. (2012) find that firm and, especially, manager fixed effects explain most of the heterogeneity in executive pay. Our results, using the time variation in network variables, as opposed to cross-section variability, are thus new and important.

Notice that the firm fixed effects capture firm heterogeneity in terms of pay standards. A large positive firm fixed effect corresponds to high-wage firms, *i.e.* firms that reward their managers above what would be expected taking into account the time-invariant heterogeneity of managers, as captured by the individual fixed effects, and all observable time-varying manager and firm characteristics. Figure 2.5 displays the distribution of the firm fixed effects, revealing considerable heterogeneity across firms, where well-connected managers are systematically overrepresented in firms with more generous pay policies. For the least well connected managers the distributions are smoother and very dispersed, reflecting the existence of a wide range of wage policies.

Given this evidence that networks grant access to better paying firms, it is also important to keep in mind that, by including fixed effects, we are clearly under-estimating the network premium, as part of the benefits derived from a network are associated to the time invariant firm heterogeneity. These include the outside option channel which allows better connected managers to match to higher paying firms. Notice that our network measure, when controlling for the fixed effects, only varies in response to circumstances completely exogenous to the manager, as other managers enter and exit

Both studies focus on cross-sectional variation, with little time-series variation, and do not include firm nor manager fixed effects.

manager positions or the labor market. In conclusion, we find that a manager's *Degree*, through time, in the same firm, unequivocally impacts her hourly pay, and our estimates can be read as a lower bound for the network effect.

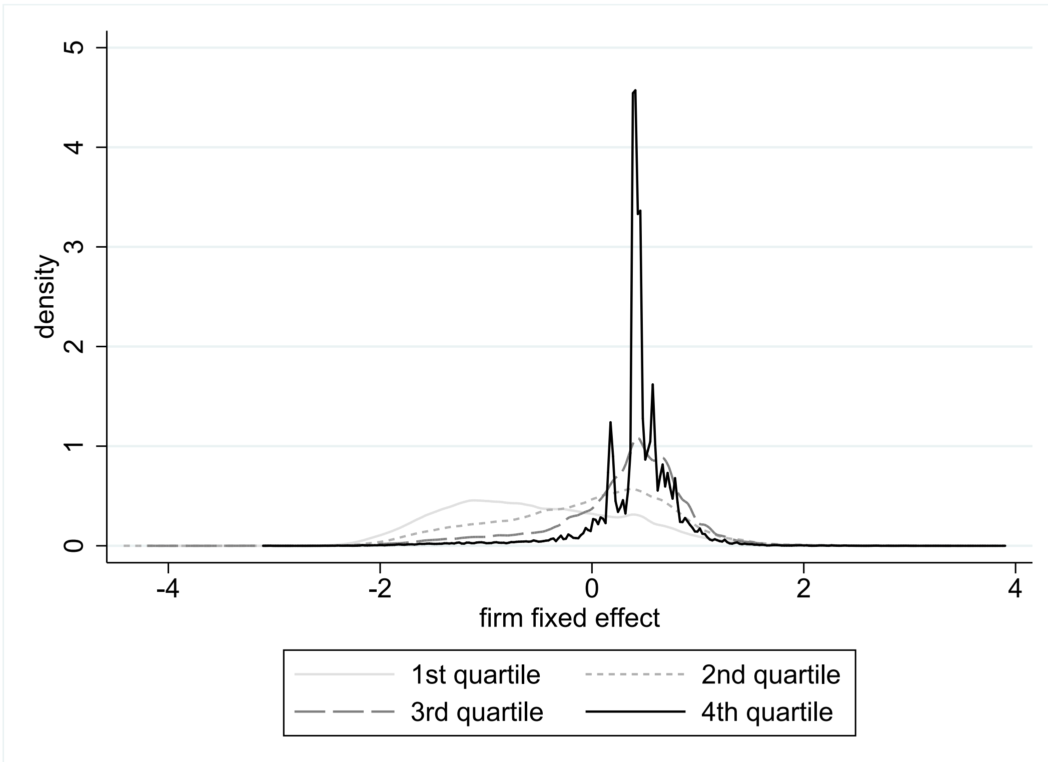


Figure 2.5: Distribution of firm fixed effects by *Degree* quartile
Reports kernel densities of the firm fixed effects for managers for different *Degree* quartiles. These figures follow from the estimation reported in Table 2.4.

Finally, it is also worth noting that wages are relatively rigid in the Portuguese labor market (see Marques et al., 2012), making our results particularly relevant as evidence of the importance of networks to manager pay. In order to explore the impact of networks on the more variable component of pay, we now decompose total compensation into the base salary and any extras – such as seniority compensation, other benefits, and extra-hour pay – which we label for simplicity as bonus. Table 2.5 shows that, while the network premium is present for both components of pay, it is more substantial in the case of the bonus, which displays much higher within firm and manager variability compared to base salary. The coefficient in specification (1), with no fixed effects, stands at 6.4%, and is more directly comparable to the above cited studies. When both firm and manager fixed effects are taken into account, the estimate of the impact for a one standard deviation increase in *Degree* stands at 8.2%.

Table 2.5: Total compensation components and the network premium

Panel A	Manager hourly bonus			
	(1)	(2)	(3)	(4)
Degree	0.0005***	0.0002***	0.0007***	0.0006***
	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Observations	1,077,233	1,077,233	1,077,233	961,029
Panel B	**Manager hourly wage**			
	(5)	(6)	(7)	(8)
Degree	0.0001***	0.0000***	0.0001***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	1,077,233	1,077,233	1,077,233	961,029
Year dummies	✓	✓	✓	✓
Firm and Manager controls	✓	✓	✓	✓
Manager fixed effect			✓	✓
Firm fixed effect		✓		✓

This table presents estimates of ordinary least squares (OLS) and firm and manager fixed effects panel regressions of the logarithm of manager hourly wage (Panel A) and hourly bonus (Panel B) on the network measure *Degree*. Manager- and firm-level control variables are included. The sample consists of all firms, for which data on their top managers is available in the 1995-2017 period. Variable definitions are provided in Appendix B.1. Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

2.4.2 Power or depth: is it who you know or how well you know them?

In the previous section, we have unveiled that the number of connections managers form throughout their careers matters for the compensation they receive at each moment in time. The question that arises is which are the most important names in one's network, who convey the most relevant information or prestige, and thus the highest wage premium? In this section we create two new indicators, weighing the *Degree* indicator previously computed along two dimensions: the depth of the connection and the power of those to whom the manager is connected to. The new two network measures are weighted *Degree* metrics - commonly referred to as a tie's strength in network theory. The variable *Depth* attributes a higher weight to the connections with whom the manager is likely to have contacted more intensively, whereas the variable *Power* assigns a higher weight to ties with managers' who have gained a greater influence in corporate leadership.

To define the *Depth* of a connection we consider the number of years the two managers worked simultaneously in the same firm, and the corresponding firm size. Having worked more years together, in smaller firms, translates in a higher *Depth* indicator. While the number of years increases the probability of actually having met someone and created a strong bond, the size of the firm reduces the odds of having worked side by side. The *Power* of a connection is defined also on the basis of two dimensions: in the firms where the connection works, how many other managers are present, and how large that firm is. The size of the firm is a measure of its power in industry and by also considering the number of managers we assess how powerful the connection might be in the decision making process of the firm. Therefore, working in large firms with fewer managers is assumed to translate into higher *Power*.

Figure 2.6 depicts the impact on pay given a one standard deviation increase in the weighted network measures, *Depth* and *Power*, derived from the model as specified in Equation 2.8.[8] For ease of comparison the results regarding the crude unweighted *Degree* measure, discussed in the previous section, are also depicted. We find that both weighted network measures have consistent positive and statistically significant impacts on pay, whether this means total pay or its components, wage and bonus.

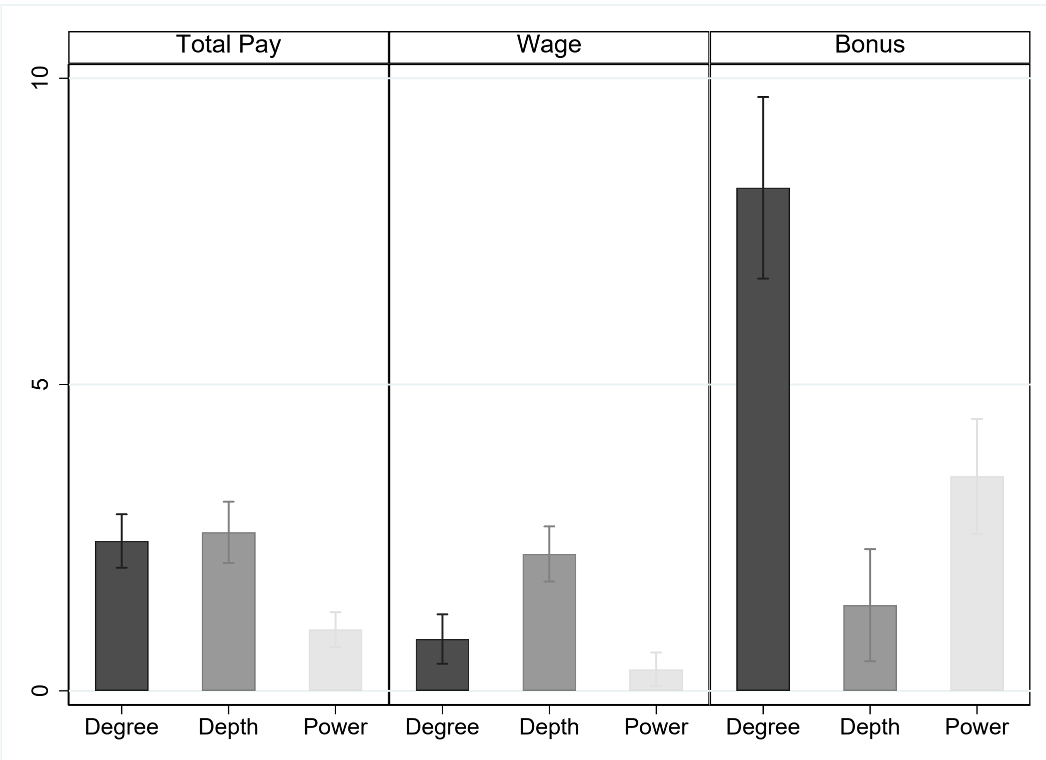


Figure 2.6: Network measures
The columns stand for the estimated coefficients for a one standard deviation change in the variable and the lines represent the 95 per cent confidence interval. Detailed estimation results can be found in Table B.1 in the Appendix.

More: *Depth* has a quantitatively larger impact on total compensation than *Power*, suggesting that deeper connections increase a network's value by more than powerful

[8]The table with the detailed results is presented in Appendix 2.

connections. This result is even more relevant as *Depth* displays low variability throughout a manager's career. Indeed, considering only firm fixed effects, not manager fixed effects, nearly doubles the coefficient on *Depth*.[9] Second, deeper connections have a similar impact on the base salary – the wage, and the more variable component- the bonus, whereas *Power* has a low impact on the base salary, but impacts the bonus more than does *Depth*.

Networks impact a manager's pay, as previously mentioned, through different channels, which can be grouped into 2 theories: (1) being more connected in a network implies higher productivity and (2) more connections facilitate rent extraction. We can speculate that the rent extraction portion of the network effect is more likely associated to the bonus, while the productivity portion of the network effect can impact more easily the base wage, given the variability of the former and the rigid nature of the latter. Indeed, a simple exercise allows us to verify this hypothesis. By measuring the contribution of the manager fixed effect to base wage and bonus variability, we can confirm which compensation component is more closely linked to a manager's ability or time-invariant productivity. Table B.2 in Appendix B.2 suggests that indeed, the role of manager heterogeneity, including ability, is more prominent for base wage, than for bonus payments, despite being also very important for the determination of the latter. Thus, results suggest that perhaps *Depth* is more closely related to a manager's actual productivity, for example given the ease of exchanging valuable information with closer links. Regressing the estimated manager fixed effects on the network metrics together with the set of controls confirms that, indeed, the network metric more closely related to a manager's ability is *Depth* (Figure B.1 in Appendix B.1).

To further validate our results on powerful ties, we pursued two strategies. First, we use an alternative measure of network power, regarding just the last firm managed

[9] When we interact *Depth* and *Power*, we confirm that deep links to powerful managers are more valuable than deep links to less powerful managers. results are available upon request.

by the connection, not the average firm managed. Our results are unchanged. Second, we restricted the scope of firms defining network power to firms with 250 workers or more, i.e. a weight of 0 is given to ties with firms smaller than 250 workers. Results do not suggest that ties to large firms are more valuable. Thus, we find no evidence that firms pay a higher premium to managers with connections to large firms, as suggested by Engelberg et al. (2013).

To this point, we have concluded that connections to other managers (*Degree*) are valuable, and the more acquainted those connections are (*Depth*) the higher the benefit. It is intuitive to consider that those deeper links are the ones more likely to transmit high-quality relevant information or favors. Although we do not report these results, we have analyzed whether the value of these deep connections increases even further when both managers share the same industry. Industry connections are much more rare (the median manager has 11 connections, of which only 2 are from the same industry), but do not seem to increase more the network premium compared to other connections.

In addition, we have posed the question of whether ties to the financial sector are specifically more valuable due to the financing need of firms, particularly for smaller firms with higher credit constraints. We find no evidence to support this hypothesis. On the contrary, larger firms seem to be willing to pay more for connections to the financial sector.

2.4.3 The value of indirect ties

So far we have restricted our analysis to the direct ties that managers form throughout their careers. It is only natural to question how valuable the indirect ties are. Indirect ties proxy for a fuller range of connections the manager has access to, with just one degree of separation. While in the literature direct measures are used as a proxy for managerial influence, indirect connections capture the information collection ability.

Therefore, indirect ties may be more related to actual added value for the firm. Also, given that it is more difficult for firms to observe these indirect metrics, any impact on pay will be less related to a subjective attribution of value to managers, and more to objective results for the firm.

In this section we analyze two centrality measures of manager networks. They are known in the literature as *Betweenness* and *PageRank*. A manager with high *Betweenness* or *PageRank* has a higher probability of receiving information that is circulating in the network. These measures capture not only the volume, but the variety of information that is accessible. *Betweenness centrality* is frequently used in the literature as the main proxy for information collection efficiency, as suggested in (Renneboog and Zhao, 2011), and defined as the total number of shortest paths between any two other managers that pass through a given manager, normalized by the total number of shortest paths between them. It assesses how central a certain individual is in the network. Individuals with a higher *Betweenness* measure not only have access to richer and more diverse information (Intintoli et al., 2018), but may wield considerable influence in the network by virtue of their position as a go-between for others.

Considering that access to information is one of the channels through which a firm can benefit from a manager's network, then the most relevant names in a manager's *Degree* are those that have access to high-quality information and are likely to transmit that information. The indicator *PageRank* takes both these dimensions into account. More specifically, there are three distinct factors that determine a manager's *PageRank*: her number of connections, that is, her *Degree*; the centrality (*PageRank*) of her connections; and their *Degree*. Finally, a manager's *PageRank* also takes into account that, the more widely connected is a given connection, the less likely it is that it passes valuable information only to the manager in question. Thus, *PageRank* depends not just on the quantity of links, but also on their qualities, and a high *PageRank* can be the result of a few highly ranked connections.

Figure 2.7 depicts the impact on pay of a standard deviation increase in the *Betweenness* and *PageRank* measures. Both are positively correlated to total compensation, and significantly so, though the impact of *PageRank* is considerably higher.[10] The impact on total pay is driven mainly by the impact on base wage, as with the *Depth* measure, in the previous section. Thus, indirect centrality measures seem to play an important role in wage setting. Given that these indirect measures are not observable, even less so by firms, rent extraction is not likely to be behind their impact on wages, rather, these results suggest that there is indeed a positive productivity effect stemming from a more valuable network. Finally, we proceed with a simple exercise presented in Appendix B.2.3 to validate this hypothesis. We propose to estimate a productivity-wage gap as in Kampelmann et al. (2018) to gauge whether the network effect on the firm's wage bill is in line with firm-level productivity gains, or whether instead manager's with large networks are simply overpaid. Results suggest that there is a statistically significant productivity-wage gap, *i.e.* firm rents increase when small-network managers are substituted by large-network managers.[11] Indeed, the gap is largest for the indirect network metrics (in particular *PageRank*) and for the closer direct connections (*Depth*), while it is only marginally significant for the crude number of connections (*Degree*).

2.4.4 Additional robustness checks

To reach our goal to test whether a highly connected manager is paid a premium over a more isolated manager with otherwise similar characteristics at a similar firm, we have used 5 different network measures. Next, we create an index of the global

[10] The impact of *PageRank* is robust to controlling simultaneously for network size (using any of the direct metrics), while the coefficient for the *Betweenness* metric becomes statistically insignificant.

[11] This is in line with Horton et al. (2012) who don't find evidence on rent extraction for a sample of listed UK firms, but rather argue that firms compensate their directors for the resources these better connections provide.

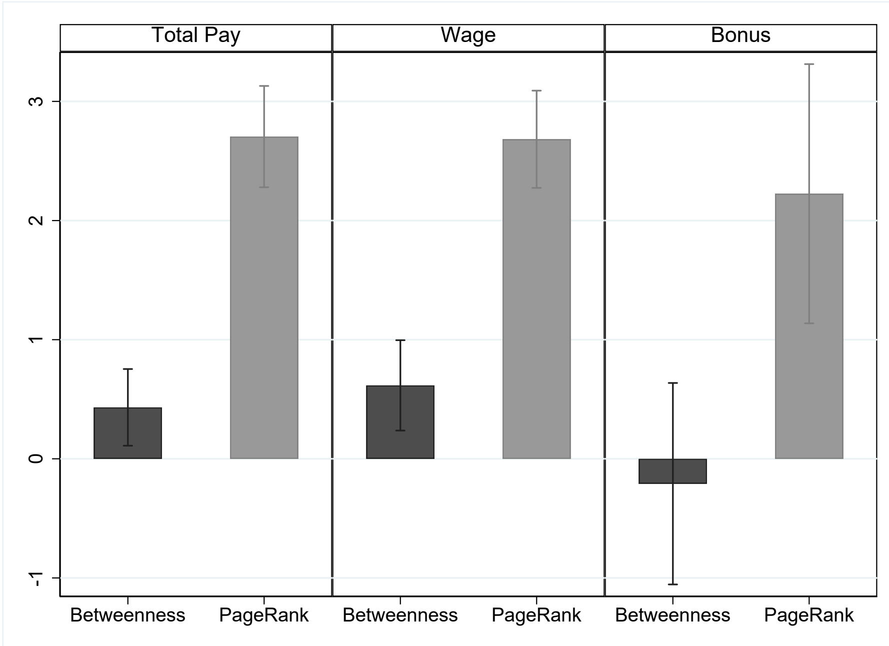


Figure 2.7: Indirect network measures: Betweenness and PageRank
The columns stand for the estimated coefficients for a one standard deviation change in the variable and the lines represent the 95 per cent confidence interval. Detailed estimation results can be found in Table B.3 of the Appendix.

network position of the manager, based on these 5 proxies: *Degree*, *Depth*, *Power*, *Betweenness* and *PageRank*, to maximize the information content of our index. To reduce these variables into a unique informative index, we extract common components, using principal component analysis. Using this method, we obtain only one component with an eigenvalue higher than one (2,83), indicating higher explanatory power than the original input variables. We find that this broad network index is positively and significantly associated with manager total compensation. A one standard deviation increase in this index is associated with an additional 4% in hourly total pay, in line with our previous results (Table B.5 in Appendix B.2.4).

Finally, as an additional robustness test, we restrict the network to connections formed during the last 5 years. Liu (2014) argues that including all past connections can be problematic, because: (i) a gap of many years can weaken the connection and (ii) a cumulative network measure can create a spurious relation with pay. The latter argument is not directly an issue in our setting, because a given connection is only activated in our yearly network, if they are a manager at the time, thus our network measures can decrease over time, depending on the connections career choices. Notwithstanding, we do test whether the rolling window approach suggested by Liu (2014) changes results and conclude that our estimates remain qualitatively robust when a 5-year window is examined. We do not find any evidence that these more recent connections are more valuable for the network effect on pay (Table B.2 in Appendix B.2.4).

Next we investigate whether the previously estimated network premium is a horizontal result, or whether it is heterogeneous across different types of firms. Table 2.6 presents a set of results, where we have run specification 2.8 for groups of firms, according to their size, age and capital.

Columns 1 and 2 display results for the sample split into firms with a number of workers below or above the median, labeled as small and large, respectively. We find that larger firms pay a higher network premium, which is in line with more talented managers being matched to larger firms (Gabaix and Landier, 2008). This finding is also consistent with the idea that larger firms have more need for external links, given the scope and complexity of their operations (Coles et al., 2008). Columns 3 and 4 present results for firms with and without foreign capital. A positive relation between pay and networks exists in both groups. In terms of the size of the network premium, we find no consistent evidence that any of the two groups of firms are willing to pay more or less for highly-connected managers. Finally, in columns 5 and 6 we examine how the firm's experience (age) affects the manager pay - network relation. We rank firms into young or old firms according to the median of the sample. Results point towards a higher network premium in less experienced firms. This result seems plausible, as younger firms are less well established in the market and thus more in need of a central position in the global network to access high quality information. Overall, the coefficient of interest is positive and significant in most specifications suggesting that our results are not firm-specific, but rather consistent across different firm types.

Table 2.6: Firm type and the network premium

	Firm size		Foreign capital		Firm age	
	Small	Large	No	Yes	Young	Old
	(1)	(2)	(3)	(4)	(5)	(6)
Degree	-0.0000	0.0001***	0.0001***	0.0001***	0.0001***	0.0001***
	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
	(7)	(8)	(9)	(10)	(11)	(12)
Depth	0.0569***	0.1439***	0.0829***	0.1352***	0.0792***	0.0763***
	(0.0108)	(0.0114)	(0.0089)	(0.0194)	(0.0099)	(0.0126)
	(13)	(14)	(15)	(16)	(17)	(18)
Power	0.0729**	0.0206***	0.0127**	0.0859***	0.0285***	0.0179*
	(0.0299)	(0.0048)	(0.0064)	(0.0075)	(0.0056)	(0.0100)
	(19)	(20)	(21)	(22)	(23)	(24)
Betweenness	0.0007***	-0.0000	0.0002***	-0.0001	-0.0000	0.0002**
	(0.0002)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
	(25)	(26)	(27)	(28)	(29)	(30)
PageRank	1.4833**	2.3834***	4.4770***	-0.2212	4.4891***	3.0808***
	(0.7341)	(0.3522)	(0.3923)	(0.5203)	(0.4234)	(0.4892)
Observations	447,865	498,067	760,438	189,906	469,487	470,235
Year dummies	✓	✓	✓	✓	✓	✓
Firm and Manager controls	✓	✓	✓	✓	✓	✓
Manager fixed effect	✓	✓	✓	✓	✓	✓
Firm fixed effect	✓	✓	✓	✓	✓	✓

This table presents estimates of firm and manager fixed effects panel regressions of the logarithm of manager total hourly pay on the network measures and other manager- and firm-level control variables, as specified in Table 2.4. The sample is split according to firms characteristics, where small and large, young and old firms consist of those that are below or above the sample median. Foreign capital refers to all firms with any percentage of foreign capital above nil. All firms, for which data on their top managers is available in the 1995-2017 period, are included. Robust standard errors adjusted for worker-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

In addition, in Table 2.7, we analyze whether the industry in which the firm operates plays a role. We find that the network premium is driven mostly by the tertiary sector, where results are consistently positive across several industries. On the contrary, there is no evidence for a premium in the primary nor secondary sectors of activity, with the exception of the construction industry.[12]

Table 2.7: Firm industry and the network premium

Industry	Primary	Secondary	Tertiary
	(1)	(2)	(3)
Degree	-0.0012	0.0001	0.0002***
	(0.0013)	(0.0001)	(0.0000)
	(4)	(5)	(6)
Depth	-0.0212	0.0744***	0.1126***
	(0.1057)	(0.0177)	(0.0097)
	(7)	(8)	(9)
Power	-0.0875	0.0081	0.0422***
	(0.2028)	(0.0245)	(0.0050)
	(10)	(11)	(12)
Betweenness	0.0002	0.0002*	0.0001*
	(0.0005)	(0.0001)	(0.0000)
	(13)	(14)	(15)
PageRank	3.7176	3.8684***	4.1475***
	(5.7053)	(0.6279)	(0.3580)
Observations	7,619	264,133	681,724
Year dummies	✓	✓	✓
Firm and Manager controls	✓	✓	✓
Manager fixed effect	✓	✓	✓
Firm fixed effect	✓	✓	✓

This table presents estimates of firm and manager fixed effects panel regressions of the logarithm of manager total hourly pay on the network measures and other manager- and firm-level control variables, as specified in Table 2.4. The sample is split according to the industry the firms operate in: primary, secondary and tertiary. Robust standard errors adjusted for worker-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

[12] Result available upon request.

2.4.5 Alternative method: an instrumental variable approach

A source of endogeneity in the network literature is that common factors affect both a manager's network and pay. In other words, an unobservable variable, such as the manager's ability, has an impact on the type of firms the manager works for, determining simultaneously his network and his pay. In our study this endogeneity problem is minimized given how the network measure is defined: though influenced by a manager's career choices, yearly changes are ultimately exogenous to the manager since they are mostly driven by how past colleagues are promoted to management positions, demoted, or leave the labor market. Given the inclusion of manager and firm fixed effects, it is exactly these yearly changes that are most relevant and drive our results.

To overcome any remaining source of potential endogeneity generated by common correlated group effects, we also resort to an instrumental variable (IV) approach. A good instrument for our network measures should affect the manager's pay only by influencing their network. We follow a similar approach to Liu (2014), using the network measure of the manager's 'initial peers' as the instrument. This strategy is also in line with the approach suggested by Bramoullé et al. (2009) for the identification of peer effects. Specifically: we define the initial peers as all the co-workers – those working at the same time in the same firm that later become managers, who coincided with our manager in the first year of her career[13]; we compute the average network measures for these peers; we predict the manager's network measures using the network measure for those peers; finally, we use the predicted network measures from the first stage with pay as the dependent variable. Notice that to instrument *PageRank* of managers, we chose to use the average *Degree* of their initial peers, instead of their *PageRank* as the latter depends also on their own *PageRank*, causing a simultaneity problem.

[13]We also use as an alternative definition to define the instrument, the initial three years of a manager's career. Results remain unchanged and are available upon request.

As the second stage relies on the predicted network measures, which depend on the estimates of the fixed effects, we have to guarantee that the estimated coefficients of the fixed effects are comparable across groups. Following Abowd et al. (2002a), we have restricted our analysis to the largest connected set, *i.e.*, a mobility group that includes all managers and firms that are connected.

Our strategy relies on the assumption that the initial peers' current network measures are likely to be correlated with the manager's own network centrality, but not due to her own efforts, hence with no other connection to pay. First, given that these managers worked previously at the same firm, part of their network will overlap. Second, assuming some sorting into firms and also the reverse impact of firms shaping their workers' careers, one can assume some similarity in their professional choices thereafter, creating another link between their networks.

Table 2.8 presents the results from two stage IV regressions. In the first stage regression, in the first column, we find that the average network measures of the initial peers have a positive coefficient that is statistically significant at the 99% confidence level, validating their correlation with the manager network measures after netting out the effects of all other control variables. The associated t-statistic for the excluded instruments suggests that the chosen IVs do not suffer from the weak instrument problem. Results confirm that there exists a positive network premium on total pay, thus the previously found results persist, mitigating any endogeneity concerns. Only *Betweenness* becomes statistically insignificant.

Table 2.8: The network effect: an IV approach

	First stage	Second stage
		Manager total pay
	(1)	(2)
Degree initial peers	0.5463***	
	(0.0102)	
Degree		0.0001**
		(0.0000)
T-stat	53.44	
Observations	665,150	665,150
	(3)	(4)
Depth initial peers	0.1270***	
	(0.0305)	
Depth		0.1444***
		(0.0337)
T-stat	4.16	
Observations	665,150	665,150
	(5)	(6)
Power initial peers	0.6078***	
	(0.0105)	
Power		0.0536***
		(0.0084)
T-stat	57.70	
Observations	665,150	665,150
	(7)	(8)
Betweenness initial peers	0.0279***	
	(0.0062)	
Betweenness		0.0033
		(0.0036)
T-stat	4.49	
Observations	665,150	665,150
	(9)	(10)
Degree initial peers	0.0033***	
	(0.0000)	
Pagerank		12.4444***
		(5.4102)
T-stat	11.84	
Observations	665,150	665,150

This table presents IV regressions of the logarithm of manager hourly pay on different network measures. The first stage regression is presented in the first column (the coefficient for *degree initial peers* in specification 9 was scaled by 10^6). Variables betweenness and PageRank have also been rescaled ($\times 1/10^3$ and $\times 10^4$, respectively). All specifications include all manager- and firm-level control variables from Table 2.4, as well as year, firm and manager fixed effects. The sample consists of the largest connected set of the database. Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

In Figure 2.8, we replicate the impact on total pay of a one standard deviation increase in the size of the network measures based on the main baseline results, together with the IV estimates, turning the estimates more comparable. As seen, each network measure, except *Betweenness*, has a positive significant impact on total pay. The measure *Depth*, together with the broader centrality measure *PageRank* are more valuable than unqualified direct ties or ties to powerful managers (*Degree* and *Power*). These results are in line with our previous results, also in size, and suggest that the quality of a manager's connections prevails over quantity.

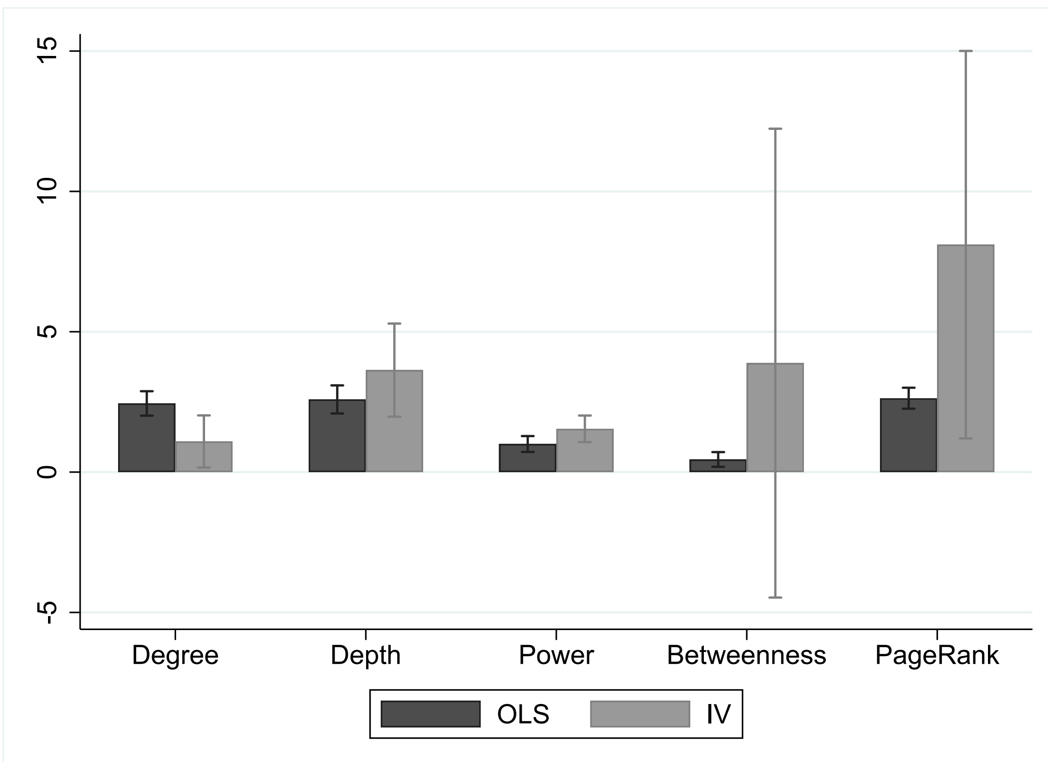


Figure 2.8: The network effect: OLS and IV estimations
The columns stand for the estimated coefficients for a one standard deviation change in the variable and the lines represent the 95 per cent confidence interval.

2.5 Concluding remarks

In this paper we add to the small but emerging body of literature that explores professional networks. Managers who are well connected enjoy better access to private information that can benefit the firm, their career options and their bargaining power. In this context, we propose to gauge the network effect on top manager's pay.

We rely on comprehensive data on the entire career of top managers, which allowed us to go beyond exploring the value of direct present connections. We consider all connections formed throughout a manager's career, unveiling which names are most valuable in a network. The analysis is based on five different network metrics, which take into account the number, *Depth* and *Power* of direct professional connections to other managers, as well as the extent and relevance of indirect connections.

The estimation of network effects is plagued by several econometric problems and data limitations. We address these concerns thoroughly. We diverge from the existing literature, by employing these network metrics in a novel way that includes substantial exogenous variation. Indeed, these measures are time-varying, increasing and decreasing over time depending on whether manager's connections become themselves managers, change firms, or exit the job market, that is, reasons exogenous to the manager. We have addressed remaining endogeneity concerns, by employing high-dimensional firm and manager fixed effects and an instrumental variable approach.

Overall, we have found consistent evidence that supports the hypothesis that networks are associated with higher base wage and bonus pay. Our findings suggest that the quality of connections prevails over quantity, where quality refers to the depth of the connections. Indirect measures that proxy better the information value of networks are also valuable. From the firm's perspective, results suggest productivity gains associated to large-network managers that go beyond the pay premium. Future research should explore further the channels through which networks create value for firms.

Chapter 3

The Gender Gap at the Top: How Network Size and Composition Impact Pay

3.1 Introduction

Research on executive pay has uncovered that men are more likely to attain top executive positions than women, at the same time as extensive evidence on the gender pay bias. Examples are Geiler and Renneboog (2015), who find that female top managers of listed UK firms earn 23% less than their male counterparts; Bell (2005) who documents a bias between 8% and 25% conditional on the gender of the Chief Executive and Corporate Board Chair for US listed firms; and Bertrand and Hallock (2001) who find a 45% gap in US firms, which is reduced to below 5% after accounting for all observable differences, where gender segregation by firm size plays a crucial role.[1] However, the literature has neglected the possible role of professional networks in determining gender

[1] Elkinawy and Stater (2011) find a similar figure as Bertrand and Hallock (2001) with a more recent sample of US firms.

differences in executive pay. A few authors have shown that female presence in leadership positions can lead to positive spillovers in terms of the progress of other women and the general within-firm pay gaps (Magda and Cukrowska-Torzewska, 2018, Matsa and Amalia, 2011). Other authors have acknowledged the role of networks for the gender gap, using very particular network measures such as co-authorships, e-mail exchanges, or same high-school attendance as a firm's chair (Keloharju et al., 2016, Lindenlaub and Prummer, 2020). However, the exact role of broader professional networks in explaining gender pay differences amongst top managers has not yet been addressed. This is an important omission to overcome, as empirical and theoretical work have shown how critical networks are for the professional advancement of managers (Engelberg et al., 2013, Hwang and Kim, 2009, Renneboog and Zhao, 2011).

A related key issue is whether networks differ only in size, or whether the benefits drawn from networks are gender specific, particularly whether the gender composition of a manager's own network is important. Understanding these issues opens the possibility for female managers leveraging their networks' size and composition to reduce the gender pay gap. Matsa and Amalia (2011) show that top managers are primarily male, which may derive from, and lead to tacit discrimination against female managers. The authors stress that the historic absence of women in leadership positions may lead to hysteresis, with the predominantly male composition of existing networks and male-dominated firms further preventing advancement in overcoming the gender pay gap at the top. In sum, networks may play a key role as a tool for female managers to overcome existing barriers.

In this study, we compute the gender pay gap at the top of the corporate ladder for the sample of Portuguese firms. In 2017 female top managers in Portugal earned on average around 80 cents for every euro earned by their male counterparts. This difference in pay is made more salient as we consider the rising trend in females' education levels and labor market experience, so that when we adjust the gender pay gap by age, tenure and schooling, it reaches 25%. Cardoso et al. (2016) and Card et al. (2015) have shown that firm sorting plays a large role in explaining the overall gender gap in the Portuguese labor market. We will advance a similar analysis as Cardoso et al. (2016), extending it to top managers, and precisely quantifying the importance of the firm sorting channel. More specifically, we will estimate a conventional wage equation for top managers and then compute the contribution of sorting into firms with heterogeneous pay policies, combining the estimates of high-dimensional fixed effects regression models with the omitted variable bias decomposition, as suggested in Gelbach (2016). We will contribute to the existing literature by isolating the network effect among managers, discussing how the gender pay gap is impacted.

We rely on two characteristics of networks: size and gender composition. The former is given by the number of all past professional interactions, within the same firm, with co-workers who later became top managers. In other words, a top manager's current network size depends on how often she or he was present in the same firm with workers who later became managers. Our network metric benefits from a key conceptual advantage over the definitions in the existing literature: it changes over time for reasons exogenous to the manager herself, and her choices, as past co-workers become managers – or leave the job market. Additionally, our comprehensive sample of firms and the considerable time interval analyzed allows us to register variations in network size across time, and estimate network effects while controlling for manager and firm fixed effects.

The network size allows us to assess the role of the number of connections in explaining pay differences, but we go one step further and investigate whether the gender composition of the network matters. We see this as unveiling the relative strength of factors such as same-gender empathy and gender diversity in determining the value of a network. In our case, gender composition is defined as the ratio of male connections in a manager's network. In a male-dominated job, can a female manager's advancement depend not only on how many people she is connected to, but also whether she is well-connected to male or female top managers?

We find that, of the 25% (22 log points) of the pay gap amongst top managers in Portugal, approximately 30% can be attributed to male top managers sorting into firms with more generous pay policies. This firm sorting channel can result both from gender discrimination and stereotyping, as well as other corporate preferences, for instance, related to a specific clientele (Matsa and Amalia, 2011). The manager pay gap drops to 20% (18 log points), when we account for differences in network metrics across genders. The contribution of firm sorting to the pay gap more than halves. These results suggest that networks are important: they are used to access better paying firms, in line with the generally accepted notion that being part of a large network translates into greater opportunities for materializing rewarding job changes (Bartlett and Miller, 1985). We explore further the link between networks and firm sorting by focusing on the transitions of top managers between firms, using a nearest neighbor matching procedure. The bulk of the gender gap is created precisely during the hiring process when managers transition between firms, and is only slightly increased by the biased career progression within the firm. This gender bias for new hires is present even when we compare top managers with the same years of schooling, similar education, age and networks, and coming from and moving to firms that are similar in size and wage policies. By comparing managers with the same pay prior to the transition, we are able to control in part for unobservable characteristics – to the researcher and the hiring firm – including ability and accumulated human capital. Our view is that the gender gap that persists after this careful comparison can be legitimately equated to gender discrimination.

Our evidence suggests the existence of a large premium associated with network size, manifested primarily as managers transition across firms. Female and male top managers with more connections than the sample median obtain a 22% and 17% average higher pay, respectively, than otherwise similar managers with thinner networks. We find no penalty as to how females benefit from networks. Controlling for firm characteristics, we confirm that most of the network premium is related to gaining access to firms with more generous pay policies.

Finally, we discuss how the gender composition of networks impacts managers' pay. Despite the existing bias in favor of male managers in top corporate positions, our results suggest that female top managers benefit more from having females rather than males in their 'inner circle'. This suggests gender empathy may trump gender diversity in network value, and suggests an increased presence of females in top management jobs will further facilitate overcoming gender bias at the top. This virtuous cycle stemming directly from the gender composition of networks is here empirically tested for the first time, suggesting fruitful avenues for future research.

This paper is structured as follows: Section 3.2 carefully describes the construction of the network, as well as the underlying data; Section 3.3 computes the gender pay gap amongst top managers and documents results from Gelbach's decomposition; the role of networks during firm transitions is explored in Section 3.4, and Section 3.5 concludes.

3.2 Data and network description

3.2.1 Network definition

We follow the same approach as in Chapter 2, and define manager i's network in year t as all past co-workers, who are themselves top managers in year t. In other words, we backtrack all the firms a manager has worked at in the past, and identify all the employees that coincided at those firms. Next, we exclude from the yearly network all acquaintances who have not reached a management position by year t or are in the same firm as manager i at time t. Yearly variations in a manager's network are driven by firm transitions, that result in the managers of the prior firm being added to the manager's network and by past co-workers being promoted to or ceasing management positions. The latter is a source of exogenous variation as it is completely independent from the manager's choices.

The network size of a manager is given by the total number of connections in a given year – denoted as *Degree* centrality in network theory:

$$\mathrm{D(i)} = \sum_{j \neq i} x_{ij}, \tag{3.1}$$

where $\mathrm{x_{ij}}$ is 1 for the presence of a link between i and j.

For the gender composition of networks, we will consider the share of male connections:

$$\mathrm{G(i)} = \frac{\sum_{j \neq i} x_{ij}}{D(i)}, \tag{3.2}$$

where $\mathrm{x_{ij}}$ is 1 for the presence of a link between i and any male manager j.

3.2.2 Sample

Our data draws on information from *Quadros de Pessoal* (henceforth denoted *QP*), a micro longitudinal dataset that contains mandatory information collected by the Portuguese authorities. The matched employer-employee dataset covers all private firms and each of its wage-earners operating in Portugal between 1986 and 2017, containing information regarding the firm and each of its workers. *QP* includes a personal identification number that enables tracing individuals across time, which allows us to observe the entire professional history of a worker (since 1986).

Each worker is classified according to the National Classification of Occupations and assigned a professional grade level in *QP*. Professional grade levels are defined by law, and each firm is obliged to classify each worker accordingly. We restrict our sample to all managers between 1995 and 2013. We define managers as top decision makers or as top management. The available 6-digit occupational classification system identifies all types of managerial occupations. Additionally we restrict the sample to those workers who are classified in the highest hierarchical grade level, that is, top managers. While we include in the global network self-employed managers, *i.e.* firm owners, they are excluded from our final sample used for our regression analysis due to the lack of data regarding their wages, and the different wage-setting mechanism in place. Put differently, firm owners add value to other managers' networks, but we cannot analyze the impact of their network on pay. Our final sample consists of 665,150 manager-firm-years, representing around 29,785 firms and 102,989 top managers.

In *QP*, for the period between 1986 and 2017 the national classification of occupations was revised several times, the last change having occurred in 2010. From this year onwards, we use the latest classification of occupations and are able to identify the workers who are managers using unchanged criteria. For the prior period, we used the official table of harmonization published by Statistics Portugal, to ensure that we minimize manager movements in and out of the network due to these changes.

3.2.3 Statistics

Table 3.1 summarizes key statistics on top manager and firm characteristics, by gender. Several facts emerge from the table. First, there is a clear under-representation of females in corporate leadership. Indeed, female top managers represent only 25% of our sample. Second, female top managers earn approximately 80% of their male counterpart's pay.

The median female manager is 42 years old – 3 years younger than her male counterpart, holds a college degree and has been at the same firm for 8 years. While the majority of top managers have tertiary education, female top managers have on average more years of schooling. On the other hand, the networks of female top managers have on average fewer connections, and a lower share of male connections. Notwithstanding, networks of both genders are dominated by males, in line with male over-representation in management positions.

In terms of firm heterogeneity across gender, females work on average for larger firms, though dispersion in firm size is also larger amongst females. Finally, the share of female managers per firm suggests sorting of gender into different kinds of firms.

Table 3.1: Summary statistics

	Female Top Managers			Male Top Managers		
	Obs.	Mean	Median	Obs.	Mean	Median
Manager characteristics						
Age	168 638	42.5	42.0	496 512	45.5	45.0
Education (categorical 1-5)	168 638	4.6	5.0	496 512	4.4	5.0
Tenure	168 638	10.5	8.0	496 512	11.0	8.0
Network size	168 638	90	21	496 512	102	30
Share of male connections	168 638	0.62	0.69	496 512	0.72	0.78
Total pay (euros)	168 638	3249	2754	496 512	4195	3373
Firm characteristics						
Firm size (no of workers)	168 638	1790	110	496 512	1575	126
% of female top managers	168 638	0.52	0.43	496 512	0.16	0.14

This table presents the summary statistics of firm and manager characteristics for 665,150 firm–year observations between 1995 and 2017. Education is a categorical variable where: 1 - less than primary education; 2 - 1st and 2nd cycle of primary education; 3 - lower secondary education; 4 - upper secondary education; 5 - tertiary education. See Appendix C.1 for definitions of the other variables.

3.3 Is there a gender gap in top management compensation?

As shown in Figure 3.1, in 1995 total pay of female top managers stood slightly above two thirds of the pay of their male counterparts. Interestingly, the raw gender pay gap between top managers followed a similar trend to the overall gap for all the workers in the economy, narrowing by more than 10 p.p. in the period under study, such that female pay represented almost four fifths of male wages by 2017. One should note that the gender pay gap amongst top managers was always around 3 p.p. above the overall gap, or about 10% higher.

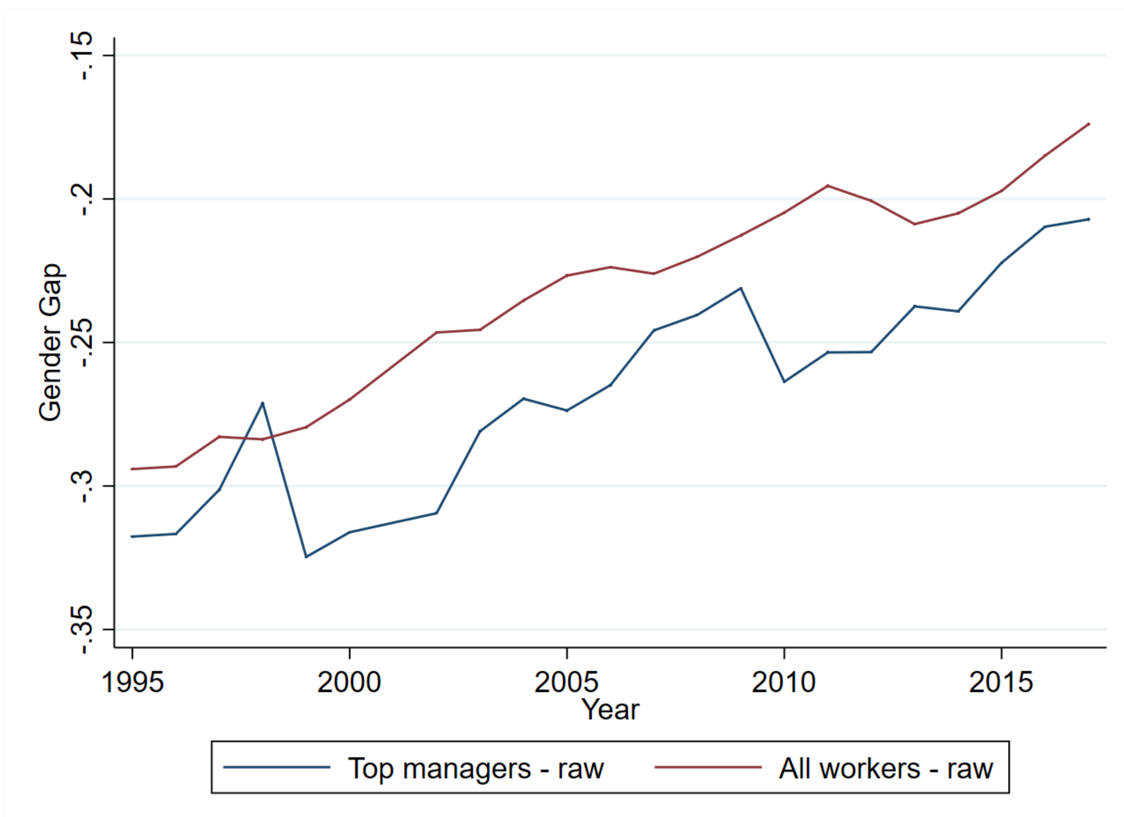


Figure 3.1: The gender pay gap

The story changes, however, if we take observable characteristics into account, and compute an 'adjusted' gender pay gap, as depicted in Figure 3.2. While there is an indication of a minor decrease in the overall gender gap over time, that is not the case amongst top managers, quite the contrary. The wage gain achieved by female top managers over this period is due to the catching up of their skills, not due to a reduction in the unexplained component of the wage difference, equated to gender discrimination (Cardoso et al., 2016).

Figure 3.2: The unexplained gender pay gap

So far, to compute the 'adjusted' gap we have controlled for characteristics usually included in wage equations – age, tenure and education, following Cardoso et al. (2016). Next, we propose to augment the analysis, taking into account professional networks, which have been identified as crucial drivers in the wage setting process at the top of the corporate ladder (Engelberg et al., 2013, Renneboog and Zhao, 2011).

In Figure 3.3 we adjust the gender pay gap for the size and the gender composition of a manager's network. We find that networks play an extremely important role, with the number and composition of network connections associated with about a 8 p.p. lower gap in 2017.

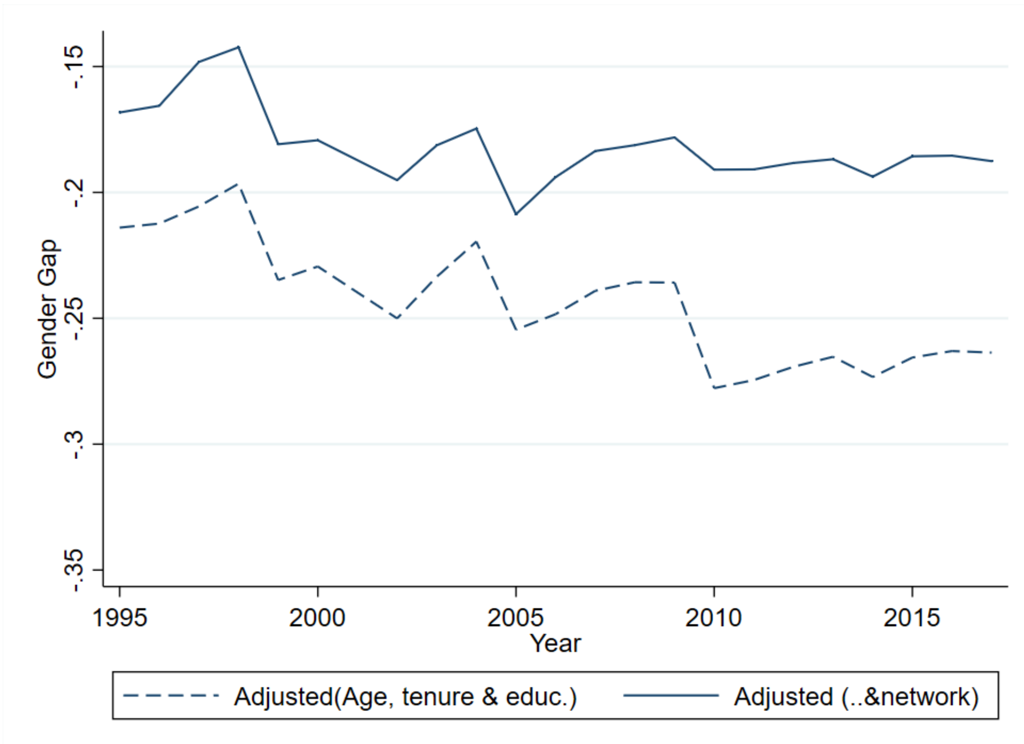


Figure 3.3: The unexplained gender pay gap: the role of networks

3.3.1 The role of firm heterogeneity

The adjusted gender gap in Figures 3.2 and 3.3 above corresponds to the average differential between the wages of two otherwise observably identical managers. However, another potential source of divergence might lie in unobserved heterogeneity. We propose to decompose the gender wage gap into the contributions of each of two sources of unobserved heterogeneity: firm and manager fixed effects. The former captures the time-invariant wage policy of the firm, *i.e* positive firm fixed effects will be generated for firms with more generous executive pay policies, while negative firm fixed effects will be attributed to low-wage firms. The latter is a proxy for the ability/productivity of the manager herself or himself, or simply reflects discrimination not associated with gender sorting across firms, as pointed out by Cardoso et al. (2016).

In this section, we follow closely the methodology presented in Cardoso et al. (2016) based on the Gelbach decomposition proposed in Gelbach (2016) to understand the contribution the allocation of managers across firms has on the observed gender pay differential.[2] We start out with the estimation of a conventional wage regression, augmented by high dimensional manager and firm fixed effects:

$$\ln Y_{ift} = \mathbf{X}_{ift}\beta + \phi_i + \alpha_f + \tau_t + \epsilon_{ift}, \tag{3.3}$$

where $\ln Y_{ift}$ is the natural logarithm of the real hourly wage of individual i (i=1,...,N), working at firm f (f=1,...,F), at year t (t=1,...,T). The vector $\mathbf{X}_{ift}$ contains observed time-varying characteristics of individual i, such as, age, education, tenure and squared terms on age and tenure. The terms ϕ_i and α_f stand for the manager and firm fixed effects, respectively, and are meant to capture time-invariant observed and unobserved manager and firm heterogeneity. The term τ_t refers to year dummies and the error term component ϵ_{ift} is assumed to follow the conventional assumptions.

For ease of presentation, this can be expressed in matrix notation as:

$$\mathbf{Y} = \mathbf{X}\beta + \mathbf{D}_i\phi + \mathbf{D}_f\lambda + \epsilon. \tag{3.4}$$

where $\mathbf{D}_i$ and $\mathbf{D}_f$ are the design matrices for the manager and firm fixed effects, respectively. We will restrict our estimations to the largest connected set, *i.e.*, the analysis is restricted to the set of firms that are connected by manager mobility to ensure compa-

[2] The main difference to Cardoso et al. (2016) is that our analysis focuses solely on top managers and excludes job title fixed effects.

rability of the fixed effect estimates (Abowd et al., 2002b).[3]

Gelbach's decomposition relies on the omitted variable bias formula, and requires the estimation of a benchmark regression excluding the high dimensional manager and firm fixed effects. Such that:

$$\mathbf{Y} = \mathbf{X}\beta + \gamma\mathbf{G} + \epsilon. \tag{3.5}$$

where $\mathbf{G}$ refers to a gender dummy which was dropped in the *Full Model* in Equation 3.4 as it is fully absorbed by the manager fixed effect, and γ measures the pay gap conditional on the other observable factors $\mathbf{X}$ in the model.

Then, an estimate for γ can be obtained through a two-step regression. First, we regress $\mathbf{Y}$ on $\mathbf{X}$ and $\mathbf{G}$ on $\mathbf{X}$ to obtain the residuals of both regressions ($\mathbf{M_X Y}$ and $\mathbf{M_X G}$, respectively, where $\mathbf{M_X}$ is the residual-maker matrix). Next, we regress $\mathbf{M_X Y}$ on $\mathbf{M_X G}$ and obtain the OLS estimate for γ in our benchmark model. In other words, γ is estimated through a simple regression of $\mathbf{Y}$ on $\mathbf{G}$, after removing the impact of $\mathbf{X}$ from both variables.

Such that:

$$\widehat{\gamma} = \mathbf{P_X Y} \tag{3.6}$$

where $\mathbf{P_X} = (\mathbf{G}'\mathbf{M_X G})^{-1}\mathbf{G}'\mathbf{M_X}$ and $\mathbf{M_X}$ is the residual-maker matrix.

Then, if we replace the coefficients and error terms of the *Full Model* 3.4 by their OLS estimates, and multiply this fitted model by $\mathbf{P_X}$ we obtain:

[3]Notice that the estimated coefficients of fixed effects are not comparable across different mobility groups. Since in our work, in addition to controlling for individual and firm heterogeneity, we are interested in using the estimated fixed effects in further analysis, a widely used solution is to restrict the sample to the largest connected set. We confirm that this restriction does not change qualitatively our results to ensure that these are not driven by a sample selection bias.

$$\mathbf{P_X Y} = \mathbf{P_X X}\widehat{\beta} + \mathbf{P_X D}_i\widehat{\phi} + \mathbf{P_X D}_f\widehat{\lambda} + \mathbf{P_X}\widehat{\epsilon}, \tag{3.7}$$

where $\mathbf{D}_i\widehat{\phi}$ and $\mathbf{D}_f\widehat{\lambda}$ are column vectors containing the least-squared estimates of the fixed effects for managers and firms. We have obtained on the left hand side the OLS estimate for γ and $\mathbf{P_X D}_i\widehat{\phi}$ and $\mathbf{P_X D}_f\widehat{\lambda}$ are the coefficients of the regressions of the manager and firm fixed effects on the gender dummy, conditional on the set of variables $\mathbf{X}$. Notice that $\mathbf{P_X X}\widehat{\beta} = 0$ and $\mathbf{P_X}\widehat{\epsilon} = 0$. We can then rewrite Equation 3.7 more succinctly, using Equation 3.6:

$$\widehat{\gamma} = \widehat{\delta}_\phi + \widehat{\delta}_\lambda. \tag{3.8}$$

Above the gender pay gap is partitioned into the contribution of individual and firm fixed effects, conditional on all $\mathbf{X}$ covariates.

In our benchmark regression, we find, in accordance with the literature, that manager pay increases with age and tenure at a decreasing rate.[4] Higher levels of education are also associated to higher pay. Regarding our key variable of interest – the dummy on females – we estimate a wage penalty of 22 log points for female top managers, conditional on age, tenure and schooling.[5]

Gelbach's decomposition suggests that a significant fraction of the gender pay gap is explained by the heterogeneity of the firms' compensation policies (see Table 3.2). The sorting of managers into firms is responsible for 7.3 out of 22 log points of the gender pay gap. Put differently, female managers are disproportionately allocated to firms with less generous wage policies. If managers were randomly assigned to firms, the gender pay gap would be reduced by one third.

[4]See Table C.1 in Appendix C.2.

[5]Cardoso et al. (2016) find a similar pay gap (23 log points) for the entire labor market and a less recent sample (1986-2008).

The unobserved permanent characteristics of the managers explain the remaining two thirds of the gender pay gap. In line with Cardoso et al. (2016) these unobserved – from the researcher's viewpoint – manager characteristics can be equated both with unobserved skills as well as forms of gender discrimination not associated with sorting of managers across firms.

Table 3.2: Conditional decomposition of the top manager gender pay gap

Pay Gap	Firm FE	Manager FE
-0.2196***	-0.0733***	-0.1463***
(0.0046)	(0.0050)	(0.0043)

Decompositions based on Gelbach (2016). The corresponding regressions are presented in Table C.1 in Appendix C.2. The benchmark model includes a gender dummy variable and controls for age, tenure, education, a squared term on age and tenure, and time fixed effects. The *Full Model* further includes fixed effects for the manager and the firm. The number of fixed effects are as follows: 29,785 for the firm and 102,989 for the manager. Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

3.3.2 The role of networks in explaining the Gender Gap

In what follows, we propose to explore the role of professional networks for the gender pay gap. Figure 3.4 displays the distribution of the firm fixed effects previously estimated. It is clear that larger networks give access to firms with more generous pay policies and that the dispersion amongst top managers with smaller networks is much higher. Another important takeaway is that amongst the less connected top managers – that is, those with smaller networks – male top managers seem to be over-represented in higher paying firms. Gender selection is less evident between well connected top managers. With regard to the distribution of manager fixed effects depicted in Figure 3.5, the gender penalty is very striking and there is no evidence of a different distribution for well connected managers.

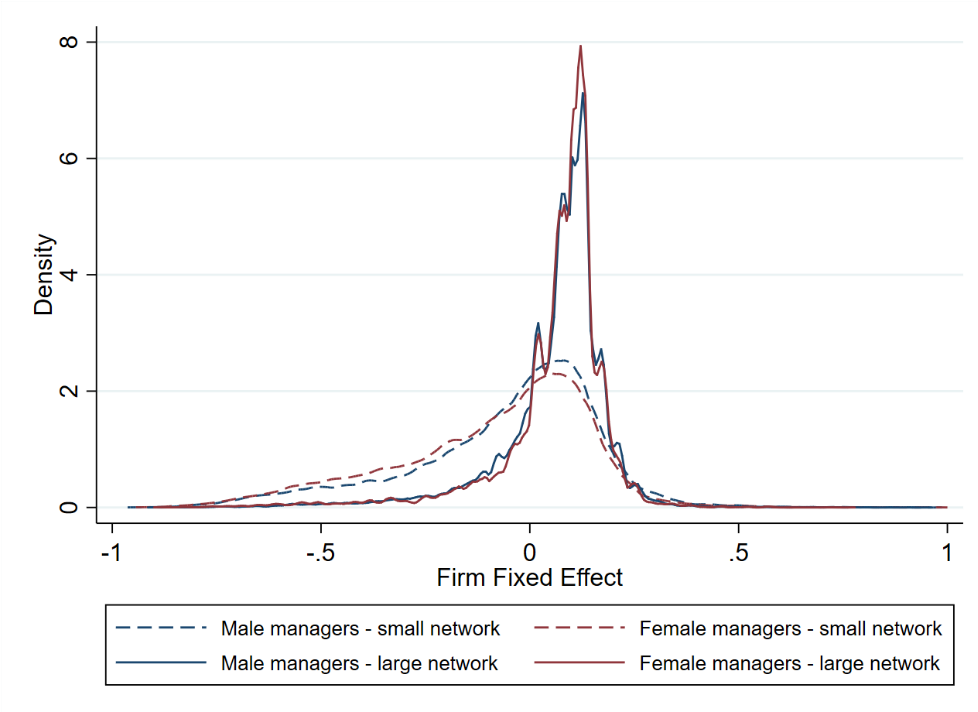


Figure 3.4: The distribution of firm fixed effects

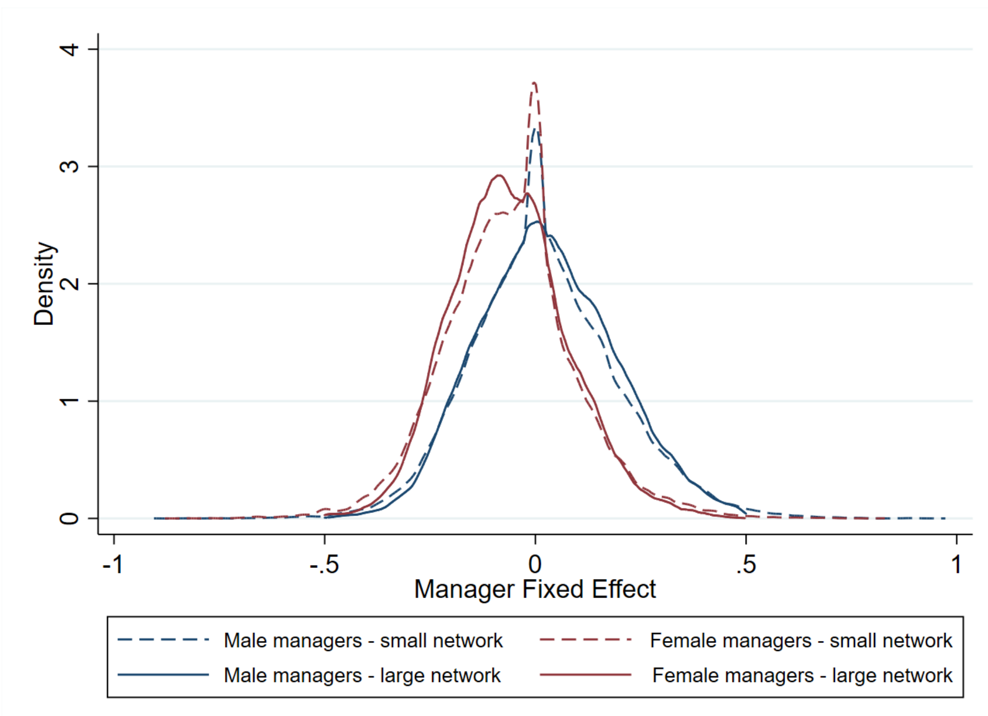


Figure 3.5: The distribution of manager fixed effects

Building on the evidence drawn from Figure 3.4, we augment the conventional wage equation with network metrics, henceforth augmented benchmark. Departing from the benchmark model and controlling additionally for the network size – *Degree* and *Degree* squared[6] – and network composition – share of male connections – reduces the estimated pay gap by almost 4 log points.[7] Relying again on Gelbach's decomposition, we find that 25% of the reduction is driven by the network size, while the remaining 75% stem from the gender composition of the network (Table 3.3).

Table 3.3: Conditional decomposition of the top manager gender pay gap: the role of networks

	Benchmark	Augmented benchmark	Full model
	γ_0	γ_1	γ_2
Gender Gap	-0.2196***	-0.1821***	0.0000
	(0.0046)	(0.0044)	
Observations	665,150	665,150	665,150
Adjusted R^2	0.2772	0.3193	0.8649
Gelbach decomposition			
$\gamma_0 - \gamma_1$		-0.0375	
Network size		-0.0095	
Network composition		-0.0281	
$\gamma_1 - \gamma_2$			-0.1821
Firm FE			-0.0356
Manager FE			-0.1465

Decompositions based on Gelbach (2016). The regressions are presented in Table C.1 in Appendix C.2. The 'benchmark model' includes a gender dummy and controls for age, tenure, education, a squared term on age and tenure and year dummies. The 'augmented benchmark' further includes the network metrics, and the *Full Model* also includes fixed effects for managers and firms. Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Finally, adding manager and firm fixed effects to the augmented benchmark model allows us to conclude that only 20% of the pay gap is due to an uneven distribution

[6] As suggested in Engelberg et al. (2013), and confirmed in Chapter 2, network size impacts positively pay at a decreasing rate.

[7] See Table C.1 in Appendix C.2.

of managers across firms, as shown in the specification denoted *Full Model* in Table 3.3. More importantly, notice that the contribution from the individual fixed effect, which represents, at least partly, discrimination, did not change. The entire network effect took place through the firm contribution. In other words, networks contribute to the matching of male top managers to higher paying firms. Therefore, by including the network metrics in our model, the contribution of the firm fixed effect was almost halved.

In sum, controlling for network metrics, we estimate a gender pay gap of approximately 20% (18 log points) between female and male top managers. Gelbach's decomposition suggests that a random assignment of managers across firms would reduce the gap to 16% (15 log points), which represents a 20% reduction. This is a very striking result, as the role of sorting into firms is substantially reduced when controlling for networks. This highlights the importance of networks as an instrument to allow female top managers to overcome part of the bias they face.

3.4 Manager mobility: top managers transitioning between firms

So far results highlighted the role of networks in matching male top managers to firms with more generous pay policies. In what follows, we propose to focus solely on manager mobility to examine further the role of networks during transitions between firms, and investigate how female top managers can overcome some of he existing biases, leveraging on their networks.

More specifically, we start by estimating the gender pay gap during firm transitions. The purpose of this analysis is threefold. First, we verify whether the gender pay gap widens or shrinks as a result of career progression inside a firm. Second, we provide further evidence, confirming the robustness of our previous finding, as regards the role of networks in sorting into higher paying firms. And finally, we acknowledge that other important factors unobservable to the researcher might play a role, and we estimate a lower bound for how much of the gap can be attributed to discrimination.

Then, in the remainder of this section (Subsection 3.4.2), we turn our focus to networks and estimate a gender specific network premium. In other words, we will answer how much being connected to other top managers impacts total pay at a new firm, and to what extent the extracted benefits might be gender specific. Finally, an important question remains, namely whether both genders benefit equally from networks in terms of gender composition.

In the analysis that follows, we will rely on matching procedures to obtain credible estimates for the unobserved counterfactuals: for example, how a female manager's transition to a new firm compares in terms of pay to a transition were she not a female? Or what would have been her pay under the alternative of having a different kind of network? Without credible answers to these questions, we are unable to determine whether the differences in pay would have occurred irrespectively. The solution followed here is the estimation of the counterfactual outcome relying on a comparison group, which needs to be statistically identical to our treatment group, *i.e.*, we use a nearest-neighbor matching estimator approach.[8] As pointed out by Custódio et al. (2013), top manager transitions are a natural application for a matching procedure as the hiring process relies mostly on observable information.[9] Since the number of covariates is large, we define the comparison groups based on exact matching on the categorical variables and the probability of being in the treatment group in the case of the continuous covariates (the propensity score). After performing balancing tests on our matched sample, we tightened the matching procedure by forcing exact matches by age quartile,

[8]Based on (Abadie and Imbens, 2011).

[9]Custódio et al. (2013) refers, in particular, to the case of CEO selection.

to ensure that there are no statistically significant differences between the treatment and control groups.[10]

This kind of matching procedure faces the risk of identifying a closest neighbor that is actually not that similar. We minimize this problem by imposing a maximum propensity score distance of 0.05 (caliper), avoiding bad matches. We confirm that this is enough to balance the distribution of the relevant covariates and that more stringent levels for the caliper do not alter significantly our results. To determine the region of common support we exclude all observations for which the propensity score lies below the minimum or above the maximum of the opposite group. Our approach eliminates a very low number of observations, having only a marginal impact on our results.

Table C.2, C.3 and C.4 in Appendix C.2 focus on the balancing properties of the matching procedure. These tables present the mean for the treatment and control groups for the unmatched and matched samples and a t-test on the differences, together with the standardized bias measure suggested by Rosenbaum and Rubin (1985). While the matching is not perfect for all variables, the clear ex-ante difference in averages between the treatment and control groups is reduced to statistical insignificance for all variables and the standardized bias is below 5%. Finally, following Sianesi (2004), we also confirm that the pseudo-R^2 of the propensity score in the matched sample is very low, suggesting that, after the match, there are no systematic differences in the distribution of covariates between the two groups. Overall, we conclude that the matching procedure was successful, as it increased substantially the similarity between the observables of the treatment and the control groups.

[10]We have checked with stricter matching rules for other variables, including exact matching within quartiles for the continuous variables. Results are available upon request.

3.4.1 Manager mobility and the gender pay gap

We start by computing the existing gender pay gap amongst top managers, restricting the sample to episodes of transition between firms. As we cannot compare the pay of a female manager when she changes firms with her pay under the alternative were she a male top manager, we compute the counterfactual. More specifically, we estimate a first-stage logit regression of the likelihood that the appointed manager is a female, using observable manager and firm characteristics. We then obtain a propensity score based on the predicted probabilities. Finally, we impute the missing hypothetical counterfactual for each subject by using the outcome of the nearest neighbor from the control group – made up of male top managers, relying on the propensity score for continuous variables and exact matches for categorical variables. The average pay gap is calculated as the average of the difference between the observed and imputed hypothetical wages for each manager.

Table 3.4 summarizes the results. We sequentially add variables to the matching procedure to disentangle the impact of different factors. We start by the basic manager characteristics – age and education, then analyze the network effect, and finally take into account the heterogeneous nature of firms. We control for the size of the previous firm in all specifications as it proxies for experience, in the sense that the human capital accumulated from managing a firm is related to the nature of the firm, including it's size. Additionally, the manager's previous firm also has an impact on the network metrics, as a manager's network increases by the number of managers from the firm it is departing. Comparing managers from similarly sized firms allows us to disentangle the network effect from the experience effect. The drawback is underestimating both the importance of networks and firm sorting, as both also relate to the choice of working at the previous firm.

In column (1) we compute the equivalent to the pay gap from the benchmark model in the previous section, but restricting the sample to transition episodes. The estimated gender gap is around 22% (19.6 log points), close to the figure from our previous benchmark model. Considering that this figure is restricted to transition episodes, while the benchmark model included the entire executive career of a manager, we can conclude that around 90% of the gender gap originates in the hiring process, and is then slightly aggravated by biased career progressions.

Table 3.4: Manager mobility and the gender pay gap: propensity score matching

	Pay Gap	Pay Gap adjusting for		Unexplained Pay Gap[1]
		networks	firm sorting	
	(1)	(2)	(3)	(4)
Gender Pay Gap	-0.1956***	-0.1309***	-0.1271***	-0.1083***
	(0.0106)	(0.0104)	(0.0135)	(0.0125)
Observations	43,185	40,549	23,040	20,462
Matching based on:				
Year, age, educ. & prev. firm size	✓	✓	✓	✓
Network size and composition		✓		✓
New firm size&FE; Prev. firm FE			✓	✓

[1]The unexplained Pay Gap is adjusted for both networks and firm sorting.
This table presents the second stage estimates of a propensity score model of the logarithm of manager total hourly pay at the new firm on the gender dummy, which takes the value of 1 for female top managers. The matching procedure was based on a caliper of 0.05. The matching on year, education, age and *Degree* quartile is exact. Firm FE refer to the fixed effects estimated in 'Full Model' in Table C.1. First-stage results are presented in Table C.5. The sample consists of all top managers who transitioned between firms in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

We then add our network metrics to our matching procedure in column (2), where network size is measured by total number of connections and gender composition by the share of male connections. The gender gap is reduced by 6.5 log points, when we compare managers with similar networks, both in terms of size and composition (column (2)-(1)). Notice that this reduction encompasses two different mechanisms through which networks impact wages: (i) the increased outside options that lead a manager to higher paying firms, *i.e* the firm sorting channel and (ii) the bargaining power derived from one's network that allows a manager to negotiate a higher pay within a firm. Due to the former, the estimated network effect is larger during transition years, as one would expect. We propose to control for this sorting into higher paying firms by controlling additionally for the previous and new firm size and pay policies – proxied by the previously estimated firm fixed effects from the *Full Model* in Subsection 3.3.2. If we do not control for network size and composition, in column (3), we find that the gap falls to below 13 log points taking into account the type of firm the manager transitions to, suggesting that around 7 log points of the base gap is explained by sorting into firms (column (3)-(1)). Notice, however, that matching on the firm' characteristics, in addition to the network metrics, reduces the gap only by 2 log points (column (4)-(2)), suggesting that around two thirds (4.6 log points) of the bias driven by firm sorting is due to differences in networking. This allows us to decompose the network effect (6.5 log points) into 70% associated to accessing higher paying firms and the remaining 30% to bargaining higher wages within a firm. Comparing, on the other hand, the results in column (4) – where we take into account all characteristics – with column (1), we find that the pay gap was reduced by 8.8 log points, which can be roughly decomposed into 75% associated to networks and 25% to sorting into firms, conditional on the network.

The unexplained pay gap in column (4), after taking into account all observables, networks and firm sorting, is close to 11% during episodes of manager mobility. This figure compares well with the almost 15% gap found in the *Full Model*, after parceling out the impact from the firm fixed effects, suggesting again that the bulk of the bias is formed during the hiring process. We perform one last exercise in Table 3.5, where we add to the matching procedure performed in column (4) of Table 3.4 the total pay received from the prior firm. We find that, even when we compare a female top manager to a male top manager with similar pay at the previous job, a 6% (5.8 log points) gap persists at the new job. By controlling for the previous wage, we are not only taking into account individual heterogeneity, such as differences in ability, accumulated experience, or any other unobservable that we are neglecting, but also the best observable indicator that the hiring firm has on the manager's skills. Therefore this remaining gap can be easily equated to gender discrimination beyond any bias associated to sorting across firms. More precisely, this should be considered a lower bound for discrimination as here we are assuming that differences in prior wages are not a result of discrimination.

Table 3.5: Top manager gender pay gap: considering unobservables

	Discrimination
Gender Pay Gap	-0.0577***
	(0.0119)
Observations	19,405
Matching based on:	
Year, age, education and prev. firm size	✓
Network size and composition	✓
New firm size and prev. and new firm FE	✓
Previous pay	✓

This table presents the second stage estimates of a propensity score model of the logarithm of manager total hourly pay at the new firm on the gender dummy, which takes the value of 1 for female top managers. The matching procedure was based on a caliper of 0.05. The matching on year, education, age and *Degree* quartile is exact. Firm FE refer to the fixed effects estimated in 'Full Model' in Table C.1. First-stage results are presented in Table C.5. The sample consists of all top managers who transitioned between firms in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

3.4.2 Manager mobility and the role of gender specific networks

In this subsection we quantify the network effect on a manager's compensation when transitioning to a new firm. This network effect, as previously discussed includes both: (i) the matching to higher paying firms, as networks reduce search frictions, while increasing outside employment options and (ii) the added bargaining power that allows a manager to negotiate a higher pay at the new job. We present separate estimates for male and female top managers to assess whether there exists a gap in the benefits drawn from networks.

Similarly to before, we follow a matching procedure, pairing well connected to poorly connected managers with similar observable characteristics, as well as, similar unobservables as captured by previous pay. We define a well connected manager as someone with a higher than sample median number of connections.

We find that well connected top managers' compensation is between 16 and 20 log points higher than their poorly connected peers, with similar characteristics in terms of experience and education (see Table 3.6, column (1) and (2)). This estimate of the effect of networks on pay is both statistically significant and economically relevant, specially as we are comparing top managers with similar compensation levels at their prior jobs. Notice that the network effect is larger for females than males, suggesting that the network contribution to the gender gap is not related to intrinsic differences in the capacity of women to benefit from networks, but rather to actual differences in the networks.

Table 3.6 also reports results for the impact of networks on pay, beyond the firm sorting channel. In the last two columns of the table, the matching procedure also takes into account the size of the new firm, as well as the firm fixed effects of both firms from the *Full Model* in Table C.1. The size of the firm is relevant as managing larger firms is likely to involve higher responsibilities and therefore higher pay, while the firm fixed effect is intended to capture the heterogeneity of the time unvarying wage policies of firms. The network effect is significantly reduced after accounting for the characteristics of the firm the manager is transitioning to, suggesting that a large part of the network premium is associated with the match to higher paying firms. Notice, however, that the network effect remains relatively larger for females, confirming that women's networks are not per se less productive.

Table 3.6: The network size effect: propensity score matching

	Network size effect		Network effect adjust. for firm sorting	
	Women	Men	Women	Men
Total Hourly Pay	0.1985***	0.1601***	0.1118***	0.0604***
	(0.0223)	(0.0156)	(0.0258)	(0.0166)
Observations	4,654	23,512	2,234	13,249
Matching based on:				
Year, age, educ. & prev. firm size	✓	✓	✓	✓
Network gender composition	✓	✓	✓	✓
Previous pay	✓	✓	✓	✓
New firm size, prev. & new firm FE			✓	✓

This table presents the second stage estimates of a propensity score model of the logarithm of manager total hourly pay at the new firm on the network dummy, which takes the value of 1 for a *Degree* higher than the sample median. The matching procedure was based on a caliper of 0.05. The matching on year, education and age quartile is exact. Firm FE refer to the fixed effects estimated in 'Full Model' in Table C.1. First-stage results are presented in Table C.6. The sample consists of all top managers who transitioned between firms in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Gender composition of networks

We have established that networks play an important role in explaining the gender pay gap at the top, and that accessing higher paying firms is a big part of the story. We have also uncovered that there is no penalty in the way female top managers benefit from networks. Now we turn to a different aspect of networks, which has not been addressed thoroughly in the literature yet, namely their gender composition. Is there any evidence of gender empathy, such that females benefit more from being connected to other female top managers? Does the persistent over-representation of males in corporate leadership and their bias towards females, create room for women serving at management positions to help advance other females? Or on the contrary, does the over-representation of men in powerful positions turn them into a more valuable asset in a female manager's network?

We proceed as before, only now our treatment group is defined as having a higher than median share of male connections, controlling for the total network size. We also take into account age, education and previous firm size and pay, and focusing only on gender composition, we find that both genders benefit more from male connections than female connections (see Table 3.7). We also find that the gender composition of networks only matters for firm sorting, *i.e*, after controlling for the type of firm the manager transitions to, there is no significant difference associated to male-dominated networks.

Now, we question whether this result stems from male manager's larger networks and the associated access to information, *i.e.*, just a direct effect of male's predominance

Table 3.7: Network gender composition: propensity score matching

	Male vs. Female connections		Composition effect adjust. for firm sorting	
	Women	Men	Women	Men
Total Hourly Pay	0.0328**	0.0429***	0.0189	0.0245
	-0.0186	(0.0121)	(0.0295)	(0.0178)
Observations	7,318	27,945	4,372	17,126
Matching based on:				
Year, age, educ. & prev. firm size	✓	✓	✓	✓
Network size	✓	✓	✓	✓
Previous pay	✓	✓	✓	✓
New firm size, prev. & new firm FE			✓	✓

This table presents the second stage estimates of a propensity score model of the logarithm of manager total hourly pay at the new firm on the network dummy, which takes the value of 1 for a male share higher than the sample median. The matching procedure was based on a caliper of 0.05. The matching on year and education is exact. Firm FE refer to the fixed effects estimated in 'Full Model' in Table C.1. First-stage results are presented in Table C.7. The sample consists of all top managers who transitioned between firms in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

in top management, or whether gender per se is relevant. We propose to investigate this further, by considering a weighted network metric –labeled as *Power*– that attributes a higher weight to connections who manager larger firms. More specifically, a manager's *Power* derives from how powerful their connections are, and is computed as the sum of weights on all direct connections, where each weight is defined as the average size of the firm's managed divided by the average number of managers at those firms. That is:

$$\mathrm{P(i)} = \sum_{j \neq i} w_{ij} x_{ij}, \tag{3.9}$$

where $\mathrm{w_{ij}}$ is greater than 0 for the presence of a link between i and j and the value represents the weight of the tie defined as:

$$w_{ij} = \frac{\sum_j FirmSize_j}{\sum_j NumberManagers_j}, \tag{3.10}$$

where $FirmSize_j$ represents the size of all firms managed by manager j and $NumberManagers_j$ the number of managers of these same firms, to account for the number of managers' amongst which decision making power is divided.

Table 3.8 reports the results considering this alternative weighted network metric. As suspected, we find that gender composition becomes irrelevant for females, suggesting that females only benefit from having more male connections than female ones given the status quo that has more males as powerful top managers, and not because male connections, per se, are more valuable. Male top managers do, however, continue to benefit more from same gender connections. While this results needs to be deepened further in future research, it strongly suggests that, in the male-dominated management environment, male top-managers tend to benefit other males more than females, perpetuating gender bias.[11]

Finally, we complement the previous analysis with an alternative weighted metric, which instead of weighing more heavily powerful connections, takes into account the proximity to connections. This measure, which we label *Depth*, has weights increase as the years two top managers coincided in a firm are higher, scaled by the firm's size. This weighted network measure allows us to attribute a higher value to connections who are more likely stronger, either because the two managers worked together for a longer period of time, or because the firm was small, or both, thus suggesting deeper interactions between co-workers. Such that:

$$w_{ij} = \frac{Years}{FirmSize}, \tag{3.11}$$

Comparing as before female top managers who have a share of high *Depth* connections to male managers above the sample median with comparable females, as far as observables, including the generic *Depth* level and previous wage, we find that both genders benefit more from same gender connections. Male top managers benefit similarly from connections to powerful male managers as to managers who they have interacted with extensively. While the result for females contrasts with our previous result, as now females benefit more from longer past interactions with other female managers, in line with the hypothesis that female representation in corporate leadership has positive spillovers for other women (Kunze and Miller, 2017).

[11] Results are statistically insignificant after we adjust for firm sorting, taking into account firm characteristics.

Table 3.8: Network gender composition: *Power* and *Depth*

	Male vs. Female Connections	
	Women	Men
Power	-0.0191	0.0318**
	(0.0202)	(0.0127)
Observations	7,249	28,005
Depth	-0.0361*	0.0377***
	(0.0216)	(0.0116)
Observations	7,305	28,019
Matching based on:		
Year, age, educ. & prev. firm size	✓	✓
Network size	✓	✓
Previous pay	✓	✓

This table presents the second stage estimates of a propensity score model of the logarithm of manager total hourly pay at the new firm on the network dummy, which takes the value of 1 for a male share higher than the sample median. The matching procedure was based on a caliper of 0.05. The matching on year and education is exact. First-stage results are presented in Table C.8. The sample consists of all top managers who transitioned between firms in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

3.5 Concluding remarks

We advance the existing literature on the gender pay gap amongst top managers by investigating an additional source of divergence between male and female manager pay: professional networks. We rely on new indicators of network size and network gender composition, making use of information on the entire history of past interactions between each manager and former co-workers who have themselves also become managers.

We estimate a standard wage equation to find that female top managers in Portugal are paid, on average, 25% less than their male counterparts, conditional on age, education and tenure at the firm. This figure is especially alarming as one would expect the few high-potential females who break the firm promotion glass ceiling to be especially skilled and talented, as argued in (Bertrand and Hallock, 2001). Next, we augment the standard model with network metrics, considering both the size and the gender composition of each manager's network. We find that networks account for 20% of the pay gap. After adding high-dimensional manager and firm fixed effects to the model, we estimate networks to have most of its differentiated impact on pay through firm sorting, i.e, networks give a manager access to firms that follow more generous executive compensation policies.

By then focusing on episodes of transitions between firms, and using a propensity score matching procedure, we estimate a pay gap around 22%, validating that the bulk of the manager gender gap arises during the hiring process and is only slightly aggravated thereafter, due to biased career progression. We also estimate one third of the gap to be associated to firm sorting, two thirds os which are explained by differences in networks. Put differently, the benefits of networks are access to higher paying firms and bargaining power to negotiate within the new firm a higher pay. Differences in networks between male and female managers explain almost 5 p.p. of the pay gap through firm sorting and another 2 p.p. through negotiations for higher pay. After taking into account all observables and both firm sorting and networks, we are left with an unexplained pay gap among top managers close to 11%. If we take into account previous compensation levels to proxy for unobserved individual heterogeneity, a 6% pay gap persists. We interpret this figure as a lower bound for pure gender discrimination among top managers, not associated to sorting across firms. Notice that by considering previous pay, we are not only accounting for unobservable characteristics to the researcher, but we are also taking into account a key observable indicator that the hiring firm may use as proxy for the manager's skills.

After establishing the important role of networks in explaining the gender pay gap among top managers, we further investigate how female managers can best leverage their networks to overcome gender segregation across firms. We find no evidence that females benefit differently from network size than males, considering otherwise comparable managers, also in terms of network gender composition. However, we do find an important new result suggesting the importance of the gender composition of a manager's network. While both genders seem to benefit more from male connections, once we take into account the importance of the connections, female top managers benefit equally from male or female dominated networks, whereas male managers continue to benefit more from same gender connections. Moreover, when the depth of the connections is considered, proxying for the 'inner circle' of a given manager, we find that both female and male managers benefit most from connections to managers of their own gender. Our results demonstrate that, in a male-dominated corporate world, gender bias can be perpetuated. On the other hand, however, with appropriate policies in place that envisage higher female presence in leadership positions, there is evidence for positive spillovers as women become especially supportive of the advancement of their close female connections. Our line of research, data, and methodology, suggests several fruitful avenues for future research.

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Appendix A

Chapter 1

A.1 Variables

Firm Characteristics	
Sales	Log of annual sales (IES)
GVA	Gross value added is defined as log of sales minus intermediary consumption (IES)
Number of establishments	Number of establishments that the firms lists each year (QP)
Number of workers	Log of number of workers registered as working at the firm at October each year (QP)
Firm age	Current year minus year of construction (QP)
Total expenses	Log of total expenses including: costs of goods sold and material consumed, supplies and external services, employee expenses, impairment losses, changes in fair value and other expenses and losses in financial investments and financial instruments (IES)
Employee expenses	Log of total employee expenses, including remunerations and pensions (IES)
Capital stock	Log of fixed tangible assets and intangible assets (IES)
Short-term debt	Log of current liabilities (IES)
Long-term debt	Log of non-current liabilities (IES)
Trade openness	Sum of total exports and imports divided by total sales (IES)
CEO Characteristics	
Newcomer	Dummy variable that takes the value of 1 for CEOs with a lower than sample median tenure, who are simultaneously outsiders and 0 otherwise (QP)
Outsider	Dummy variable that takes the value of 1 if a CEO was hired from another firm directly to a top management position - not necessarily to a CEO position - and 0 if he was internally promoted (QP)
Crisis	Dummy variable that takes the value of 1 during the years 2011, 2012 and 2013 and 0 otherwise
Gender	Dummy variable that takes the value of 1 for females and 0 otherwise (QP)
Age	Current year minus birth year (QP)
Tenure	Defined as number of observations (one per year) at the current firm in a top management position according to the national Classification of Occupations (QP)
Education	Categorical variable where: 1 - less than primary education; 2 - first cycle of primary education; 3 - 2nd cycle of primary education; 4 - lower secondary education; 5 - upper secondary education; 6 - post-secondary non-tertiary education; 7 - bachelors degree; 8 - undergraduate degree; 9 - masters degree; 10 - PhD (QP)

A.2 Difference-in-differences: parallel trend assumption and falsification testes

Table A.1: Parallel-trend: pre-crisis period

	Firm Performance	
	GVA	Sales
	(1)	(2)
2009	-0.039**	-0.059***
	(0.016)	(0.017)
2010	-0.028	-0.051**
	(0.019)	(0.023)
Newcomer	-0.191***	-0.172***
	(0.033)	(0.042)
2009 × Newcomer	0.054	-0.041
	(0.033)	(0.036)
2010 × Newcomer	0.006	-0.058
	(0.038)	(0.044)
Observations	11,544	12,061

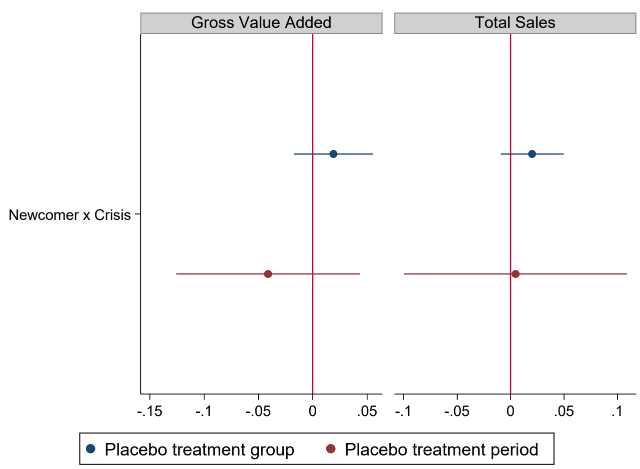


Figure A.1: Falsification tests
This figure depicts the coefficient of the interaction term between the newcomer dummy and the crisis period and the respective confidence intervals for 4 specifications. In blue the baseline specification of Equation 1.1 is run on GVA and sales considering a randomly assigned newcomer group amongst the control group. The red coefficients refer to the same specifications, where a placebo crisis period has been defined (year 2007).

A.3 Additional results

Table A.2: Difference-in-difference-in-differences: outsider, low-tenured CEOs and newcomers

	Firm Performance			
	Gross Value Added		Sales	
	OLS	Firm fixed effects	OLS	Firm fixed effects
	(1)	(2)	(3)	(4)
Crisis	-0.127***	-0.076***	-0.121***	-0.071***
	(0.020)	(0.024)	(0.025)	(0.023)
Low-tenured	0.023	0.022	-0.051	0.012
	(0.024)	(0.040)	(0.052)	(0.044)
Outsider	0.007		-0.017	
	(0.016)		(0.038)	
Newcomer	-0.022	-0.077*	0.094*	0.002
	(0.027)	(0.045)	(0.057)	(0.048)
Low-tenured × crisis	0.035	-0.025	0.073	0.021
	(0.041)	(0.050)	(0.060)	(0.041)
Outsider × crisis	-0.038	-0.089***	0.013	-0.043*
	(0.024)	(0.027)	(0.029)	(0.025)
Newcomer × crisis	0.137***	0.214***	0.067	0.155***
	(0.045)	(0.055)	(0.064)	(0.046)
Observations	22,324	22,534	23,194	23,783

This table presents difference-in-difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of gross value added (GVA) and total sales on low-tenured CEOs, outsiders and newcomers. The treatment period is the period of crisis, namely 2011-2013. The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The OLS regressions also include district and industry dummies. The sample consists of all firms, managed by 1 CEO (with no CEO turnover during or immediately before the crisis) and for which CEO data are available. Robust standard error adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table A.3: Outsider and low-tenured CEOs and firm performance

	Firm Performance			
	Gross Value Added		Sales	
	OLS	Firm fixed effects	OLS	Firm fixed effects
Panel A	(1)	(2)	(3)	(4)
Crisis	-0.116***	-0.071***	-0.085***	-0.040*
	(0.018)	(0.023)	(0.023)	(0.021)
Outsider	-0.019		0.015	
	(0.014)		(0.032)	
Outsider×Crisis	0.025	0.007	0.029	0.019
	(0.021)	(0.024)	(0.026)	(0.022)
Observations	22,324	22,534	23,194	23,783
Panel B	(5)	(6)	(7)	(8)
Crisis	-0.145***	-0.118***	-0.111***	-0.093***
	(0.010)	(0.015)	(0.010)	(0.014)
Low tenured CEO	-0.021*	-0.071***	-0.009	-0.052***
	(0.012)	(0.020)	(0.021)	(0.019)
Low tenured CEO×Crisis	0.155***	0.147***	0.161***	0.182***
	(0.017)	(0.022)	(0.021)	(0.018)
Observations	24,610	24,835	25,526	26,155

This table presents difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of gross value added (GVA) and total sales on low-tenured and outsider CEOs. In Panel A the treatment variable is a dummy that takes the value of 1 for CEOs with low tenure (below the yearly sample median). In Panel B the treatment variable is a dummy that takes the value of 1 for externally recruited CEOs (outsiders), instead of internally promoted. The treatment period is the period of crisis, namely 2011-2013. The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The OLS regressions also include district and industry dummies. The sample consists of all firms, managed by 1 CEO (with no CEO turnover during or immediately before the crisis) and for which CEO data are available. Robust standard error adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table A.4: Low-tenured CEOs and firm performance: insiders and outsiders

Panel A	Inside CEOs			
	Gross Value Added		Sales	
	OLS	Firm fixed effects	OLS	Firm fixed effects
	(1)	(2)	(3)	(4)
Crisis	-0.140***	-0.033	-0.129***	-0.059**
	(0.020)	(0.031)	(0.024)	(0.026)
Low tenured CEO	0.018	-0.005	0.020	-0.069
	(0.029)	(0.053)	(0.058)	(0.065)
Low tenured CEO×crisis	0.062	-0.038	0.060	0.038
	(0.046)	(0.055)	(0.060)	(0.043)
Observations	3,707	3,722	3,808	3,844
Panel B	**Outside CEOs**			
	Gross Value Added		Sales	
	OLS	Firm fixed effects	OLS	Firm fixed effects
	(1)	(2)	(3)	(4)
Crisis	-0.153***	-0.152***	-0.114***	-0.119***
	(0.012)	(0.018)	(0.013)	(0.014)
Low tenured CEO	-0.035**	-0.091***	-0.018	-0.052***
	(0.015)	(0.024)	(0.024)	(0.020)
Low tenured CEO×crisis	0.178***	0.193***	0.176***	0.215***
	(0.019)	(0.025)	(0.023)	(0.021)
Observations	18,617	18,812	19,386	19,939

This table presents difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of gross value added (GVA) and total sales on low-tenured CEOs. The treatment variable is defined as having low tenure in a top management position at the firm. The treatment period is the period of crisis, namely 2011-2013. In Panel A and B the sample is divided into inside and outside CEOs, respectively. The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The OLS regressions also include district and industry dummies. The sample consists of all firms, managed by 1 CEO (with no CEO turnover during or immediately before the crisis) and for which CEO data are available. Robust standard error adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table A.5: Newcomers and firm performance: CEOs and firm owners

	Firm's managed by more than one CEO			
	More than 1 CEO: no owners		More than 1 CEO: all owners	
	OLS (1)	Firm fixed effects (2)	OLS (3)	Firm fixed effects (4)
Crisis	-0.151***	-0.089***	-0.168***	-0.133***
	(0.014)	(0.019)	(0.006)	(0.009)
Newcomers	-0.024	-0.072**	-0.050***	-0.061***
	(0.021)	(0.033)	(0.009)	(0.015)
Newcomers×crisis	0.162***	0.177***	0.206***	0.207***
	(0.032)	(0.040)	(0.013)	(0.017)
Observations	6,685	6,712	44,554	44,712

This table presents difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of gross value added (GVA) on newcomer CEOs. In these regressions the treatment variable is defined as being a newcomer (low tenure and externally recruited). The treatment period is the period of crisis, namely 2011-2013. Regressions (1) and (2) from table 1 were estimated for different samples, depending on whether CEOs own the firm or not. The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The OLS regressions also include district and industry dummies. The sample includes all firms for which exists available data for more than one CEO (with no CEO turnover during or immediately before the crisis). Robust standard error adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table A.6: Newcomers and firm performance: sales

	All firms		Firm's managed by one CEO				Firm's managed by more than one CEO			
			Not the owner		The owner		No owners		All owners	
	OLS	Firm FE	OLS	Firm FE	OLS	Firm FE	OLS	Firm FE	OLS	Firm FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Crisis	-0.132***	-0.124***	-0.108***	-0.103***	-0.131***	-0.142***	-0.109***	-0.069***	-0.121***	-0.103***
	(0.003)	(0.004)	(0.011)	(0.012)	(0.004)	(0.006)	(0.017)	(0.014)	(0.006)	(0.007)
Newcomers	-0.044***	-0.056***	-0.000	-0.042**	-0.071***	-0.057***	-0.086**	-0.036	-0.037**	-0.071***
	(0.007)	(0.006)	(0.023)	(0.019)	(0.009)	(0.008)	(0.039)	(0.024)	(0.015)	(0.013)
Newcomers×crisis	0.203***	0.236***	0.170***	0.201***	0.213***	0.271***	0.171***	0.140***	0.187***	0.228***
	(0.007)	(0.006)	(0.022)	(0.020)	(0.009)	(0.009)	(0.042)	(0.036)	(0.016)	(0.014)
Observations	233,352	238,878	23,194	23,783	121,362	124,890	6,844	6,929	45,745	46,206

This table presents difference-in-differences estimates of ordinary least squares (OLS) and firm fixed effects panel regressions of the logarithm of total sales on newcomer CEOs. In these regressions the treatment variable is defined as being a newcomer. The treatment period is the period of crisis, namely 2011-2013. Regressions (3) and (4) from Table 1.5 were estimated for different samples, depending on the number of CEOs and owners of the firm. The regressions include CEO and firm controls (gender, age, education, firm age, firm size, average performance). The OLS regressions also include district and industry dummies. The sample includes all firms for which exists available data for at least one CEO (with no CEO turnover during or immediately before the crisis). Robust standard error adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

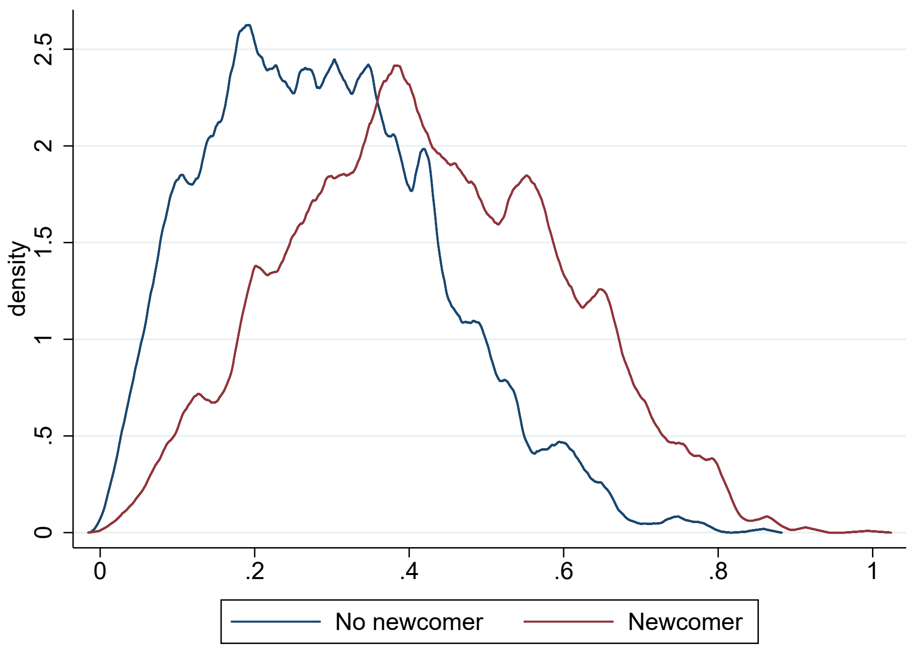


Figure A.2: Estimated propensity scores from probit regressions

Table A.7: Balancing properties of the propensity score matching
Note: *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Variable	Sample	Mean Treated	Mean Control	T-test	%bias	Bias reduction
CEO characteristics:						
CEO age	Unmatched	41.091	46.119	***		
	Matched	41.130	41.287		-1.7	96.9
CEO gender	Unmatched	0.280	0.234	***		
	Matched	0.280	0.263		4.0	62.4
Education	Unmatched	5.502	5.405	**		
	Matched	5.501	5.551		-2.3	48.3
Firm characteristics:						
Average performance (GVA)	Unmatched	10.932	11.962	***		
	Matched	10.950	10.939		0.7	98.9
No. of workers	Unmatched	1.312	1.983	***		
	Matched	1.316	1.326		-0.8	98.5
No. of establishments	Unmatched	1.194	1.375	***		
	Matched	1.195	1.175		1.0	89.0

Table A.8: Newcomers and firm total sales: propensity score match

		Firm's managed by one CEO		Firm's managed by more than one CEO	
	All firms	1 CEO: not the owner	1 CEO: the owner	More than 1 CEO: no owners	More than 1 CEO: all owners
	(1)	(2)	(3)	(4)	(5)
Newcomers	0.090***	0.061*	0.121***	0.051	0.121***
	(0.010)	(0.033)	(0.012)	(0.074)	(0.022)
Observations	113,923	11,488	60,032	3,526	23,402

This table presents estimates of the average difference between a firm's average sales during the crisis period run by a newcomer CEO. These estimates were obtained using a propensity score match method, matching firms based on the firm's and CEO's pre-crisis characteristics (gender, age, education, firm age, firm size, average performance). Regressions were estimated for different samples, depending on the number of CEOs and owners of the firm. The sample includes all firms for which exists available data for at least one CEO (with no CEO turnover during or immediately before the crisis). *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Figure A.3: Matched difference-in-differences
The dots stand for the estimated coefficients from fixed effects regressions and the lines represent the 95 per cent confidence interval.

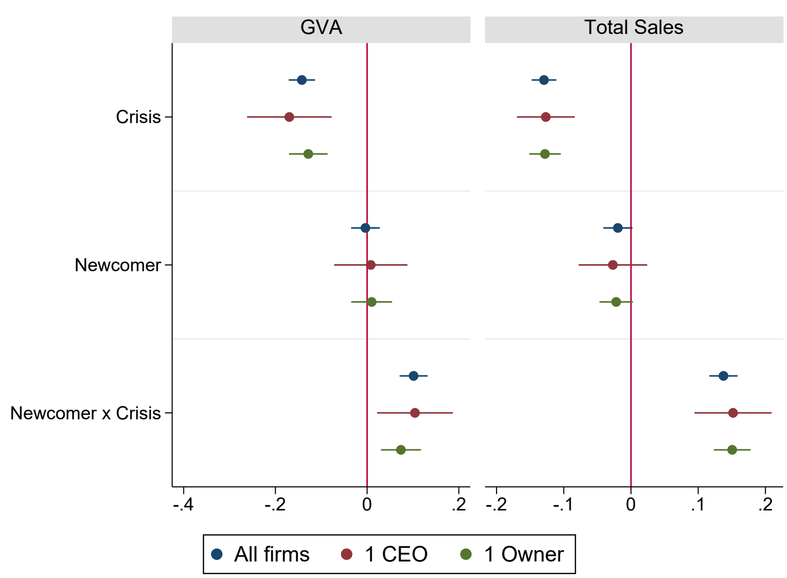


Figure A.4: Latent Instrumental Variable
The dots stand for the estimated coefficients and the lines represent the 95 per cent confidence interval.

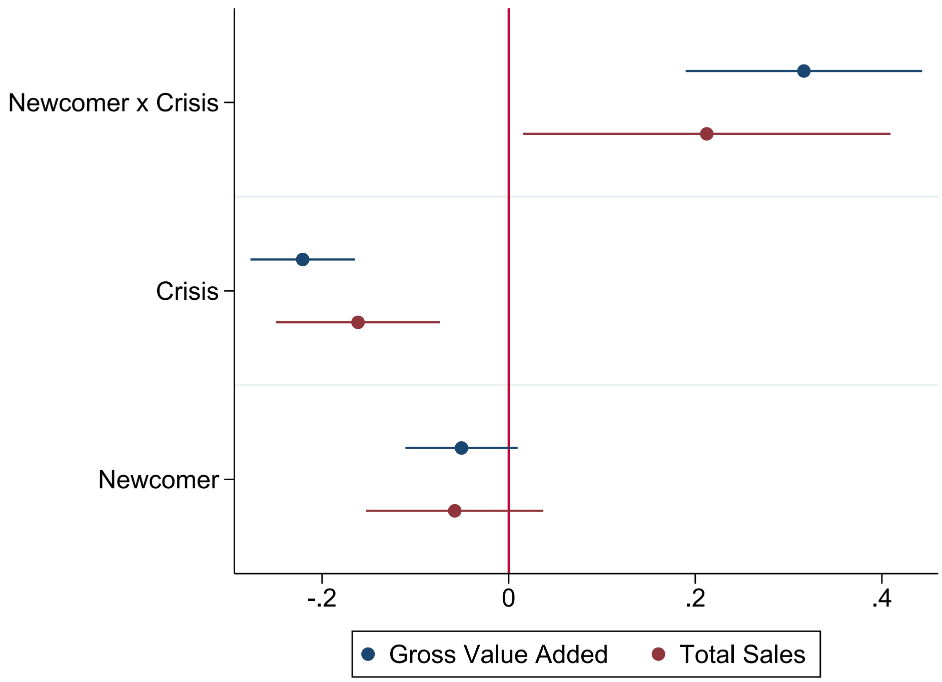


Table A.9: Firm productivity: propensity score matching

	Firm Productivity		
	All	1 CEO	1 Owner
	(1)	(2)	(3)
Panel A: GVA per worker	0.074***	0.057**	0.102***
Newcomer	(0.007)	(0.024)	(0.010)
Observations	107,220	10,788	56,113
Panel B: Sales per worker	0.089***	0.076***	0.139***
Newcomer	(0.007)	(0.024)	(0.009)
Observations	116,502	11,645	61,689
Panel C: GVA per unit sold	0.073***	0.039*	0.069***
Newcomer	(0.007)	(0.023)	(0.010)
Observations	106,932	10,787	55,934

This table presents estimates of the average difference between a firm's productivity during the crisis period run by a newcomer CEO and the alternative of not being run by a newcomer. These estimates were obtained using a propensity score match method, matching firms based on the firm's and CEO's pre-crisis characteristics (gender, age, education, firm size, average performance). Regressions were estimated for different samples, depending on the number of CEOs and owners of the firm. Robust standard errors are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table A.10: Matched difference-in-differences: firm productivity

	Firm Productivity								
	GVA per worker			Sales per worker			GVA per unit sold		
	All	1 CEO	1 Owner	All	1 CEO	1 Owner	All	1 CEO	1 Owner
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Crisis	-0.190***	-0.170***	-0.205***	-0.149***	-0.141***	-0.161***	-0.072***	-0.098***	-0.070***
	(0.012)	(0.047)	(0.019)	(0.009)	(0.026)	(0.012)	(0.010)	(0.034)	(0.012)
Newcomers	-0.051***	-0.123***	-0.080***	-0.053***	-0.029	-0.049***	-0.001	-0.038	-0.006
	(0.013)	(0.041)	(0.020)	(0.010)	(0.031)	(0.014)	(0.010)	(0.037)	(0.014)
Newcomers*crisis	0.214***	0.220***	0.243***	0.210***	0.215***	0.245***	0.024**	0.046	0.018
	(0.014)	(0.038)	(0.019)	(0.011)	(0.032)	(0.015)	(0.011)	(0.031)	(0.015)
Observations	118,525	11,698	63,075	129,547	12,712	68,584	118,438	11,626	62,768

This table presents matched difference-in-differences estimates of firm fixed effects panel regressions of three productivity measures on newcomer CEOs. Matching weights were based on the propensity scores procedure presented in Table A.9. Robust standard errors adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table A.11: Newcomers and key policy instruments

Panel A	Costs								
	Total costs (log)			Costs/Sales			Employee costs/ Total costs		
	All firms	1CEO	1Owner	All firms	1CEO	1Owner	All firms	1CEO	1Owner
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Newcomers	0.167***	0.144***	0.200***	-0.040***	-0.044***	-0.049***	-0.009***	-0.012***	-0.005***
	(0.008)	(0.028)	(0.010)	(0.003)	(0.010)	(0.004)	(0.002)	(0.004)	(0.002)
Observations	113,433	11,370	59,985	111,411	11,069	58,926	113,444	11,375	59,988

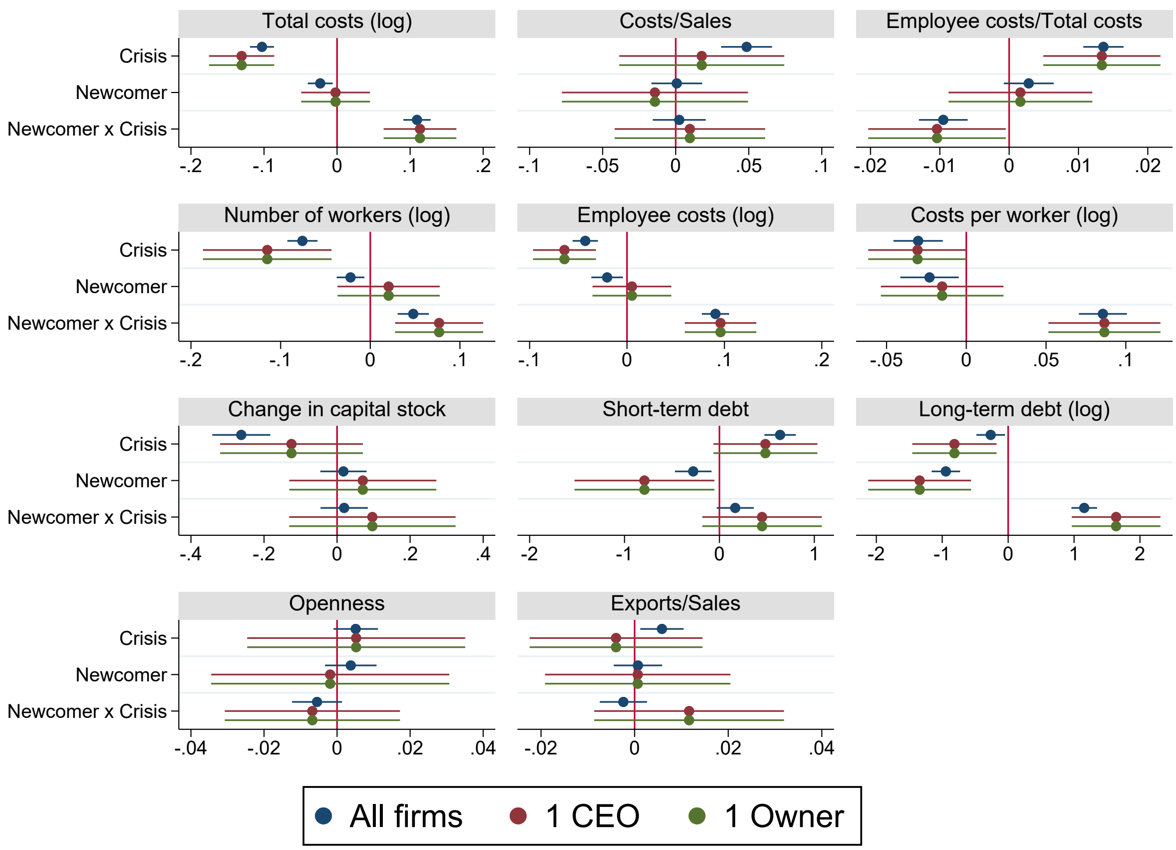
Panel B	Workers								
	Number of workers (log)			Employee costs (log)			Costs per worker (log)		
	All firms	1CEO	1Owner	All firms	1CEO	1Owner	All firms	1CEO	1Owner
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Newcomers	0.072***	0.127***	0.083***	0.121***	0.136***	0.150***	0.061***	0.060***	0.078***
	(0.006)	(0.022)	(0.007)	(0.007)	(0.025)	(0.010)	(0.004)	(0.012)	(0.005)
Observations	117,939	11,905	62,206	113,349	11,367	59,950	113,263	11,312	59,950

Panel C	Investment and Financing								
	Change in capital stock			Short term debt (log)			Long term debt (log)		
	All firms	1CEO	1Owner	All firms	1CEO	1Owner	All firms	1CEO	1Owner
	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)
Newcomers	0.302***	0.232***	0.320***	0.180***	0.266**	0.323***	0.274***	0.502***	0.333***
	(0.014)	(0.045)	(0.019)	(0.037)	(0.118)	(0.049)	(0.040)	(0.129)	(0.053)
Observations	98,442	9,958	51,529	118,044	11,977	62,206	118,044	11,977	62,206

Panel D	Trade					
	Openness			Exports/Sales		
	All firms	1CEO	1Owner	All firms	1CEO	1Owner
	(28)	(29)	(30)	(31)	(32)	(33)
Newcomers	0.007***	0.017**	0.012***	0.015***	0.021***	0.018***
	(0.002)	(0.008)	(0.002)	(0.001)	(0.006)	(0.002)
Observations	113,139	11,201	59,715	113,626	11,261	59,942

This table presents estimates of the average difference between the treated (firms run by a newcomer CEO) and the control group, during the crisis period. These estimates were obtained using a propensity score match method, matching firms based on the firm's and CEO's pre-crisis characteristics. Regressions were estimated for different samples, depending on whether the CEO in office is also the owner or not. The sample consists of all firms (with no CEO turnover during or immediately before the crisis) and for which CEO data are available. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Figure A.5: Key policy instruments: matched difference-in-differences
The dots stand for the estimated coefficients from fixed effects regressions and the lines represent the 95 per cent confidence interval.



Appendix B

Chapter 2

B.1 Variable definitions

Variable	Description
Firm Characteristics	
Sales	Log of annual sales.
Number of establishments	Number of establishments that the firm lists each year.
Number of workers	Log of number of workers registered as working at the firm in October of each year.
Manager Characteristics	
Gender	Dummy variable: equals 1 for females and 0 otherwise.
Age	Current year minus birth year.
Tenure	Defined as the difference between the current year and the year of admission in the current firm.
Years of experience	Defined as the difference between the current year and the earliest year of admission found in the dataset.
Education	Categorical variable where: 1 - less than primary education; 2 - 1st and 2nd cycle of primary education; 3 - lower secondary education; 4 - upper secondary education; 5 - tertiary education.
CEO	Dummy variable that takes the value of 1 for managers who are classified as CEOs, according to the National Classification of Occupations.
Network Measures	
Degree	The sum of total connections the manager has on the annual executive network.
Depth	The weighted sum of connections the manager has on the annual executive network, where the weight equals the number of years worked together over the number of workers at the same firm.
Power	The weighted sum of connections the manager has on the annual executive network, where the weight equals the average number of workers at the managed firms over the average number of managers.
Betweenness	The number of geodesics (shortest paths) going through a manager.
PageRank	PageRank is a way of measuring the importance of each manager in the network by counting not only the quantity, but also the quality of each connection. It is computed through an iterative process, where the PageRank of each manager is the sum of the PageRank of their connections normalized by their degree.

B.2 Additional results

B.2.1 Direct network metrics

Table B.1: The value of direct ties

	Total hourly compensation								
	Total pay			Wage			Bonus		
	Degree (1)	Depth (2)	Power (3)	Degree (4)	Depth (5)	Power (6)	Degree (7)	Depth (8)	Power (9)
Network metric	0.0001***	0.1129***	0.0399***	0.0001***	0.0938***	0.0224***	0.0002***	0.0739***	0.0942***
	(0.0000)	(0.0080)	(0.0065)	(0.0000)	(0.0073)	(0.0061)	(0.0000)	(0.0186)	(0.0196)
Age	0.0440***	0.0434***	0.0446***	0.0391***	0.0384***	0.0395***	0.0494***	0.0504***	0.0504***
	(0.0020)	(0.0020)	(0.0020)	(0.0018)	(0.0018)	(0.0018)	(0.0034)	(0.0034)	(0.0034)
Age squared	-0.0005***	-0.0005***	-0.0005***	-0.0004***	-0.0004***	-0.0004***	-0.0005***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure	0.0054***	0.0055***	0.0053***	0.0037***	0.0038***	0.0036***	0.0015	0.0013	0.0013
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0011)	(0.0011)	(0.0011)
Tenure squared	-0.0000**	-0.0000**	-0.0000**	0.0000	0.0000	0.0000	0.0001**	0.0001**	0.0001**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Education	0.0072***	0.0075***	0.0072***	0.0083***	0.0086***	0.0083***	-0.0060	-0.0060	-0.0062
	(0.0025)	(0.0025)	(0.0025)	(0.0023)	(0.0023)	(0.0023)	(0.0053)	(0.0053)	(0.0053)
CEO dummy	0.0341***	0.0337***	0.0340***	0.0364***	0.0361***	0.0363***	0.0106**	0.0102*	0.0103**
	(0.0022)	(0.0022)	(0.0022)	(0.0020)	(0.0020)	(0.0020)	(0.0052)	(0.0052)	(0.0052)
No. of workers (ln)	0.0767***	0.0742***	0.0747***	0.0689***	0.0674***	0.0677***	0.0368***	0.0320***	0.0331***
	(0.0022)	(0.0022)	(0.0022)	(0.0020)	(0.0020)	(0.0020)	(0.0045)	(0.0045)	(0.0045)
No. establishments	-0.0002***	-0.0002***	-0.0002***	-0.0001***	-0.0001***	-0.0001***	-0.0005***	-0.0004***	-0.0004***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Year dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓
Manager fixed effect	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm fixed effect	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	806,619	806,619	806,619	806,619	806,619	806,619	806,619	806,619	806,619

This table presents estimates of firm and manager fixed effects panel regressions of the logarithm of manager hourly pay on the network metrics *Degree*, *Depth* and *Power* and other manager- and firm-level control variables. The sample consists of all firms for which data on their top managers is available in the 1995-2017 period. Variable definitions are provided in Appendix B.1. Variable *Power* has been rescaled ($\times 10^4$). Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively.

Table B.2: Contribution of manager heterogeneity to compensation variation

	Degree		Depth		Power	
	Base Wage	Bonus	Base Wage	Bonus	Base Wage	Bonus
Manager fixed effect	18.1%	14.1%	17.9%	15.6%	18.1%	15.2%

This table presents the contribution of manager fixed effects to base wage and bonus pay variability ($cov(\ln w_{ijt}, \alpha_i)/Var(\ln w_{ijt})$), based on the estimates from specifications (4) to (9) from Table B.1.

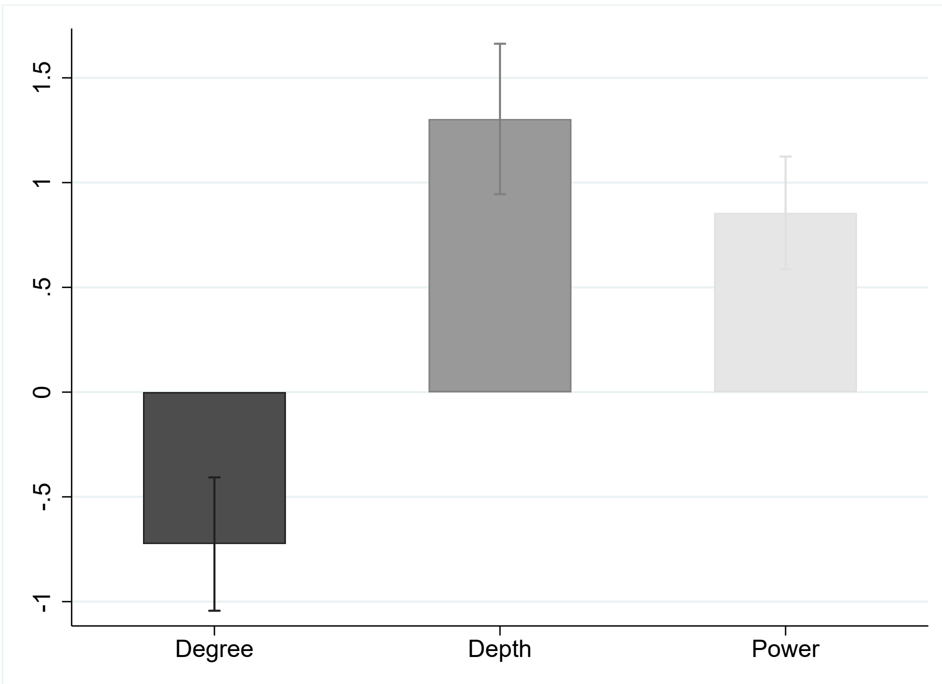


Figure B.1: Manager fixed effects and network metrics
The columns stand for the estimated coefficients for the impact of a one standard deviation change in the network metric on the estimated manager fixed effects from specifications 1 to 3 of Table B.1. The lines represent the 95 per cent confidence interval.

B.2.2 Indirect network metrics

Table B.3: The value of indirect ties

	Total pay		Total hourly compensation Wage		Bonus	
	Betweenness (1)	PageRank (2)	Betweenness (3)	PageRank (4)	Betweenness (5)	PageRank (5)
Network centrality	0.0001***	5.2314***	0.0001***	4.8567***	0.0000	3.1941***
	(0.0000)	(0.2876)	(0.0000)	(0.2697)	(0.0001)	(0.7593)
Age	0.0450***	0.0429***	0.0397***	0.0378***	0.0515***	0.0502***
	(0.0020)	(0.0020)	(0.0018)	(0.0018)	(0.0034)	(0.0034)
Age squared	-0.0005***	-0.0005***	-0.0004***	-0.0004***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure	0.0052***	0.0059***	0.0036***	0.0042***	0.0011	0.0015
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0011)	(0.0011)
Tenure squared	-0.0000**	-0.0000***	0.0000	-0.0000	0.0001**	0.0001**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Education	0.0072***	0.0072***	0.0083***	0.0083***	-0.0062	-0.0062
	(0.0025)	(0.0025)	(0.0023)	(0.0023)	(0.0053)	(0.0053)
CEO dummy	0.0340***	0.0333***	0.0363***	0.0356***	0.0104**	0.0100*
	(0.0022)	(0.0022)	(0.0020)	(0.0020)	(0.0052)	(0.0052)
No. of workers (ln)	0.0743***	0.0763***	0.0675***	0.0693***	0.0320***	0.0333***
	(0.0022)	(0.0022)	(0.0020)	(0.0020)	(0.0045)	(0.0045)
No. of establishments	-0.0002***	-0.0002***	-0.0001***	-0.0001***	-0.0004***	-0.0004***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Year dummies	✓	✓	✓	✓	✓	✓
Manager fixed effect	✓	✓	✓	✓	✓	✓
Firm fixed effect	✓	✓	✓	✓	✓	✓
Observations	806,619	806,619	806,619	806,619	806,619	806,619

This table presents estimates of firm and manager fixed effects panel regressions of the logarithm of manager total hourly pay on the indirect centrality measures betweenness and PageRank and other manager- and firm-level control variables. The sample consists of all firms, for which data on their top managers is available in the 1995-2017 period. Variable definitions are provided in Appendix B.1. Variables betweenness and PageRank have been rescaled ($\times 1/10^3$ and $\times 10^4$, respectively). Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively

B.2.3 Productivity-wage gap

A simple method to compute productivity-wage gaps is to estimate a firm-level equation, following Kampelmann et al. (2018):

$$\begin{aligned} &[Ln(Sales/Hours)_{jt} - Ln(Wagecost/Hours)_{jt}] \\ &= \beta_1 Network_{jt} + \beta_2 X_{jt} + \gamma_j + e_{jt} \end{aligned} \tag{B.1}$$

in which the dependent variable – the gap between firm j's log hourly sales (firm sales are used as a proxy for production) and log hourly wage bill – is regressed on the shares of hours worked by large-network managers (defined as having a network metric above the yearly median) and on a set of controls X_{jt}. The latter includes firm size, the share of managers in each age and tenure quartile, the share of female managers and the share of managers by educational level. These shares account for the total number of worked hours that is accounted for by each type of manager considered. We also include firm fixed effects (γ_j) to account for heterogeneity at the firm level. Although usually in the literature a dynamic element is added to the equation, we have found no evidence for strong persistence.

Estimating Equation B.2 is equivalent to estimating separately a firm-level wage equation and productivity equation (which can be derived from a standard Cobb-Douglas production function augmented by firm-specific characteristics). The coefficient of interest β_1 determines whether marginal wage costs of large networks (the network premium) are in line with the corresponding output elasticicities. A negative coefficient indicates that large-network managers are overpaid, while a positive coefficient suggests firm rents.

Table B.4: Sale-wage cost gap

	Sales - Wage cost gap				
	(1)	(2)	(3)	(4)	(5)
Network metrics: share of managers					
Degree top 50%	0.0233*				
	(0.0123)				
Depth top 50%		0.0714***			
		(0.0085)			
Power top 50%			0.0517***		
			(0.0118)		
Betweenness top 50%				0.0652***	
				(0.0109)	
PageRank top 50%					0.0914***
					(0.0083)
No. of workers (log)	0.2282***	0.2289***	0.2276***	0.2274***	0.2284***
	(0.0112)	(0.0112)	(0.0112)	(0.0112)	(0.0112)
Age: share of managers					
Quartile 2	-0.0609***	-0.0663***	-0.0626***	-0.0659***	-0.0710***
	(0.0124)	(0.0124)	(0.0124)	(0.0124)	(0.0124)
Quartile 3	-0.1363***	-0.1418***	-0.1378***	-0.1421***	-0.1484***
	(0.0145)	(0.0145)	(0.0145)	(0.0145)	(0.0145)
Quartile 4	-0.2237***	-0.2288***	-0.2244***	-0.2286***	-0.2354***
	(0.0162)	(0.0162)	(0.0162)	(0.0162)	(0.0162)
Tenure: share of managers					
Quartile 2	0.0385***	0.0370***	0.0392***	0.0388***	0.0395***
	(0.0078)	(0.0078)	(0.0078)	(0.0078)	(0.0078)
Quartile 3	0.0168	0.0147	0.0189	0.0184	0.0196*
	(0.0120)	(0.0119)	(0.0119)	(0.0120)	(0.0119)
Quartile 4	-0.0708***	-0.0717***	-0.0670***	-0.0678***	-0.0670***
	(0.0175)	(0.0174)	(0.0175)	(0.0175)	(0.0174)
Share of female managers	0.0964***	0.0987***	0.0977***	0.0990***	0.1004***
	(0.0139)	(0.0139)	(0.0139)	(0.0139)	(0.0139)
Schooling: share of managers					
1st & 2nd cycle of primary education	-0.2126	-0.2269	-0.2112	-0.2108	-0.2129
	(0.1649)	(0.1628)	(0.1648)	(0.1648)	(0.1630)
Lower secondary education	-0.2174	-0.2323	-0.2164	-0.2167	-0.2194
	(0.1652)	(0.1631)	(0.1652)	(0.1651)	(0.1633)
Upper secondary education	-0.2373	-0.2502	-0.2368	-0.2373	-0.2387
	(0.1656)	(0.1635)	(0.1656)	(0.1655)	(0.1637)
Tertiary education	-0.2511	-0.2628	-0.2511	-0.2510	-0.2513
	(0.1660)	(0.1639)	(0.1660)	(0.1659)	(0.1641)
Observations	186,433	186,433	186,433	186,433	186,433
R^2	0.7999	0.8000	0.8000	0.8000	0.8002
Year dummies	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓

The sample consists of all firms, for which data on their top managers is available in the 1995-2008 period. The sample is restricted to the period prior to 2009, due to a break in the sales series in the database. Robust standard errors adjusted for firm-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively

B.2.4 Additional robustness checks

Table B.5: Managers' total pay and the network premium: principal component index

	Manager total hourly pay				
	OLS	Firm fixed effects	Manager fixed effects	Manager & firm fixed effects	
	(1)	(2)	(3)	(4)	(5)
Network index	0.0510***	0.0158***	0.0263***	0.0176***	0.0250***
	(0.0012)	(0.0009)	(0.0014)	(0.0014)	(0.0021)
Network index squared					-0.0007***
					(0.0002)
Gender	-0.2034***	-0.1372***			
	(0.0035)	(0.0026)			
Age	0.0603***	0.0570***			
	(0.0011)	(0.0009)			
Age squared	-0.0005***	-0.0005***	-0.0004***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure	0.0002***	0.0002***	0.0010***	0.0005***	0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure squared	0.0000***	-0.0000	-0.0000**	-0.0000***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Education	0.3055***	0.0965***	0.0204***	0.0081***	0.0082***
	(0.0017)	(0.0017)	(0.0027)	(0.0022)	(0.0022)
No. of workers (ln)	0.1682***	0.0520***	0.0855***	0.0805***	0.0806***
	(0.0009)	(0.0024)	(0.0016)	(0.0020)	(0.0020)
No. of establishments	-0.0009***	-0.0004***	-0.0003***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Year dummies	✓	✓	✓	✓	✓
Manager fixed effect			✓	✓	✓
Firm fixed effect		✓		✓	✓
Observations	961,029	961,029	961,029	961,029	961,029
R squared	0.5145	0.8308	0.9012	0.9285	0.9286

This table presents estimates of ordinary least squares (OLS) and firm and manager fixed effects panel regressions of the logarithm of manager total hourly pay on the aggregate network index (the common component of all five network metrics) and other manager- and firm-level control variables. The sample consists of all firms, for which data on their top managers is available in the 1995-2017 period. Variable definitions are provided in Appendix B.1. Robust standard errors adjusted for manager-level clustering are reported in brackets. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

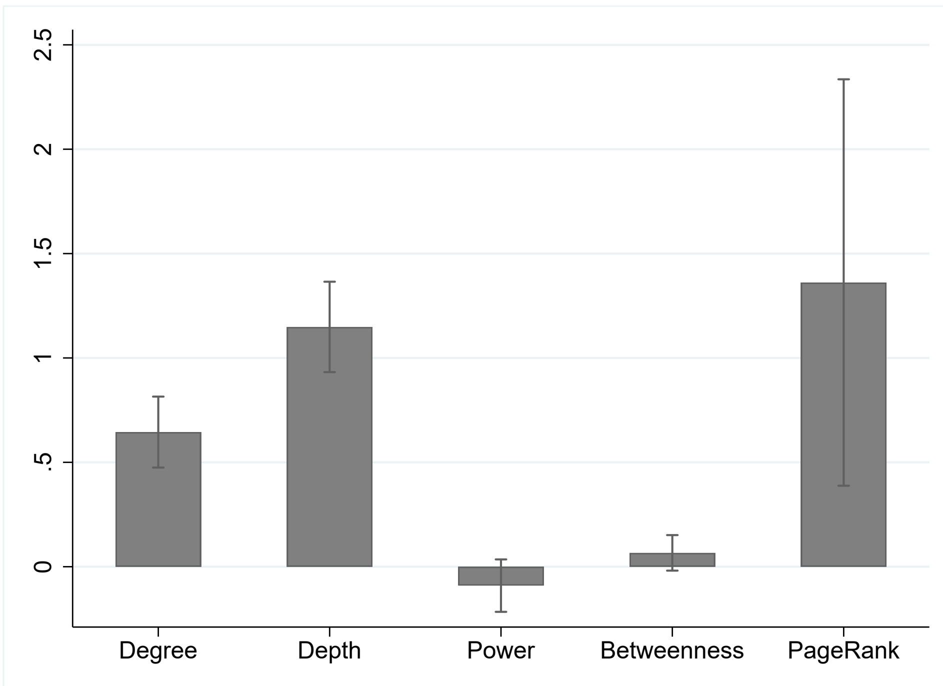


Figure B.2: Network effect: 5 year rolling window
The columns stand for the estimated coefficients for the impact of a one standard deviation change in the network metric on total pay. The lines represent the 95 per cent confidence interval.

Appendix C

Chapter 3

C.1 Variable definitions

Variable	Description
Firm Characteristics	
Number of workers	Log of number of workers registered as working at the firm in October of each year.
Total hourly pay	The ratio of the sum of base wages, regular benefits (including seniority benefits), and overtime pay over total hours (normal and overtime hours worked).
Manager Characteristics	
Gender	Dummy variable: equals 1 for females and 0 otherwise.
Age	Current year minus birth year.
Tenure	Defined as the difference between the current year and the year of admission in the current firm.
Education	Categorical variable where: 1 - less than primary education; 2 - 1st and 2nd cycle of primary educ.; 3 - lower secondary educ.; 4 - upper secondary educ.; 5 - tertiary educ.;
Network Measures	
Network size	The sum of total connections the manager has on the annual executive network.
Network gender composition	Share of total male connections in a manager's annual network
Depth	The weighted sum of connections the manager has on the annual network, where the weight equals the number of years worked together over the number of workers at the same firm.
Power	The weighted sum of connections the manager has on the annual network, where the weight equals the average number of workers at the managed firms over the average number of managers.

C.2 Additional results

C.2.1 Gender Pay Gap: benchmark and full model results

Table C.1: Gender Pay Gap: benchmark and full model

	Total Hourly Pay			
	(1)	(2)	(3)	(4)
Female	-0.2196***		-0.1821***	
	(0.0046)		(0.0044)	
Age	0.0863***		0.0662***	
	(0.0017)		(0.0016)	
Age squared	-0.0008***	-0.0005***	-0.0006***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure	0.0011***	0.0005***	0.0008***	0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure squared	-0.0000	-0.0000***	0.0000	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Schooling	0.3484***	0.0078***	0.3013***	0.0079***
	(0.0027)	(0.0028)	(0.0026)	(0.0028)
Network size			0.0018***	0.0003***
			(0.0000)	(0.0000)
Network size squared			-0.0000***	-0.0000***
			(0.0000)	(0.0000)
Network comp.			0.3469***	0.0206***
			(0.0068)	(0.0036)
Observations	665,150	665,150	665,150	665,150
R^2	0.2772	0.8918	0.3193	0.8918
Year fixed effects	✓	✓	✓	✓
Manager and firm fixed effects		✓		✓

This table presents in column (1) the OLS estimates of a benchmark specification of the logarithm of manager hourly pay on gender, age, age squared, tenure, tenure squared and schooling. Column (2) shows the full specification including manager and firm fixed effects. Gender and age are absorbed by the manager fixed effects (the latter due to the combination of manager and year fixed effects). Columns (3) and (4) augment the previous specifications with network metrics and column (4). Standard errors are clustered at the manager level. The sample consists of all top managers (excluding firm owners) in the 1995-2017 period. Variable definitions are provided in Appendix A. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

C.2.2 Balancing properties of the matching procedure

Table C.2: Balancing properties of the propensity score matching: gender

Variable	Sample	Mean Treated	Mean Control	%bias	Bias reducation	t-test	p-value
Age	Unmatched	41.229	44.186	-34.6		-20.74	0.000
	Matched	41.198	41.103	1.1	96.8	0.57	0.570
Education	Unmatched	4.707	4.564	21.8		12.77	0.000
	Matched	4.744	4.744	0.0	100.0	0.00	1.000
Network size	Unmatched	159.760	147.330	6.3		3.88	0.000
	Matched	166.080	168.660	-1.3	79.3	-0.60	0.546
Network composition	Unmatched	0.690	0.752	-33.8		-21.58	0.000
	Matched	0.704	0.710	-3.0	91.2	-1.51	0.130
Firm size	Unmatched	5.466	5.054	15.9		9.99	0.000
	Matched	5.532	5.638	-4.1	74.3	-1.90	0.058
Prev. firm size	Unmatched	5.423	5.146	11.2		7.06	0.000
	Matched	5.502	5.534	-1.3	88.7	-0.58	0.559
Firm fixed effect	Unmatched	0.120	0.112	1.5		0.94	0.347
	Matched	0.143	0.138	1.0	37.6	0.49	0.626
Prev. firm fixed effect	Unmatched	0.169	0.156	2.8		1.72	0.085
	Matched	0.188	0.177	2.2	23.6	1.09	0.278
Prev. Pay	Unmatched	2.849	3.008	-23.2		-13.89	0.000
	Matched	2.880	2.893	-1.9	91.8	-0.98	0.326

This table presents the mean for the treatment (females) and control (males) groups for the unmatched and matched samples and the corresponding standardized bias measure as suggested by Rosenbaum and Rubin (1985) reported together with the achieved reduction.

Table C.3: Balancing properties of the sample of female managers: network size

Variable	Sample	Mean Treated	Mean Control	%bias	Bias reducation	t-test	p-value
Age	Unmatched	41.510	40.675	9.9		3.37	0.001
	Matched	40.669	40.988	-3.8	61.8	-1.07	0.287
Education	Unmatched	4.768	4.587	28.9		10.05	0.000
	Matched	4.886	4.886	0.0	100.0	0.00	1.000
Network composition	Unmatched	0.708	0.653	24.2		9.24	0.000
	Matched	0.716	0.716	0.2	99.0	0.08	0.937
Firm size	Unmatched	6.313	3.794	110.9		34.31	0.000
	Matched	4.775	4.855	-3.5	96.8	-1.01	0.314
Prev. firm size	Unmatched	6.342	3.610	131.9		40.10	0.000
	Matched	4.623	4.525	4.7	96.4	1.63	0.104
Firm fixed effect	Unmatched	0.252	-0.140	71.1		25.64	0.000
	Matched	0.182	0.159	4.3	94.0	1.30	0.192
Prev. firm fixed effect	Unmatched	0.296	-0.081	77.7		27.99	0.000
	Matched	0.218	0.208	2.2	97.2	0.64	0.520
Prev. Pay	Unmatched	2.992	2.568	65.9		22.26	0.000
	Matched	3.014	3.021	-1.1	98.3	-0.29	0.771

This table presents the mean for the treatment (females with higher than median number of connections) and control (females with lower than median number of connections) groups for the unmatched and matched samples and the corresponding standardized bias measure as suggested by Rosenbaum and Rubin (1985) reported together with the achieved reduction.

Table C.4: Balancing properties of the sample of female managers: network size

Variable	Sample	Mean Treated	Mean Control	%bias	Bias reducation	t-test	p-value
Age	Unmatched	44.564	43.525	11.4		7.51	0.000
	Matched	43.007	43.235	-2.5	78.0	-0.99	0.322
Education	Unmatched	4.635	4.438	27.0		17.99	0.000
	Matched	4.839	4.839	0.0	100.0	0.00	1.000
Network composition	Unmatched	0.760	0.738	11.6		8.33	0.000
	Matched	0.774	0.778	-1.7	85.5	-0.84	0.401
Firm size	Unmatched	5.759	3.821	87.6		54.02	0.000
	Matched	4.459	4.580	-5.4	93.8	-2.36	0.018
Prev. firm size	Unmatched	5.973	3.698	114.4		69.77	0.000
	Matched	4.556	4.472	4.2	96.3	2.10	0.035
Firm fixed effect	Unmatched	0.221	-0.079	53.6		37.13	0.000
	Matched	0.164	0.173	-1.6	96.9	-0.74	0.459
Prev. firm fixed effect	Unmatched	0.266	-0.038	60.1		41.83	0.000
	Matched	0.207	0.186	4.2	93.0	1.87	0.061
Prev. Pay	Unmatched	3.141	2.776	51.3		33.91	0.000
	Matched	3.145	3.174	-4.0	92.2	-1.58	0.114

This table presents the mean for the treatment (males with higher than median number of connections) and control (males with lower than median number of connections) groups for the unmatched and matched samples and the corresponding standardized bias measure as suggested by Rosenbaum and Rubin (1985) reported together with the achieved reduction.

C.2.3 First-stage results from matching procedure

Table C.5: First-stage results: gender pay gap

	Gender				
	(1)	(2)	(3)	(4)	(5)
Age	-0.0353***	-0.0335***	-0.0404***	-0.0372***	-0.0313***
	(0.0014)	(0.0014)	(0.0020)	(0.0020)	(0.0021)
Previous firm size	0.0036	0.0320***	0.0087	0.0093	-0.0002
	(0.0048)	(0.0061)	(0.0095)	(0.0106)	(0.0109)
New firm size			-0.0874**	-0.0349	-0.0569
			(0.0386)	(0.0392)	(0.0407)
Previous firm FE			0.0639***	0.0553***	0.0516***
			(0.0093)	(0.0095)	(0.0097)
New firm FE			-0.0075	0.0358	0.2450***
			(0.0425)	(0.0430)	(0.0492)
Network size		-0.0000		-0.0000	0.0000
		(0.0001)		(0.0001)	(0.0001)
Male share		-1.0754***		-1.5870***	-1.5430***
		(0.0418)		(0.0847)	(0.0864)
Previous Wage					-0.2629***
					(0.0296)
Observations	43,281	43,281	23,562	23,562	22,815

This table presents logit regressions of the gender dummy on the variables above. The sample consists of all top managers transitions in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table C.6: First-stage results: the network effect

	Network Size			
	Females		Males	
	(1)	(2)	(3)	(4)
Age	0.9201***	-0.0004	-0.0005	0.0083***
	(0.0441)	(0.0049)	(0.0016)	(0.0021)
Previous firm size	0.5055***	0.4170***	0.4759***	0.4289***
	(0.0151)	(0.0244)	(0.0076)	(0.0117)
Male share	1.4571***	1.0966***	1.1521***	0.7481***
	(0.1223)	(0.1940)	(0.0715)	(0.1087)
New firm size		0.1495***		0.1259***
		(0.0218)		(0.0105)
Previous firm FE		0.2893***		0.1151**
		(0.1114)		(0.0494)
New firm FE		0.4388***		0.3624***
		(0.0911)		(0.0407)
Previous Wage	0.9201***	0.7574***	0.6625***	0.5534***
	(0.0441)	(0.0737)	(0.0190)	(0.0311)
Observations	8,111	4,795	29,436	18,020

This table presents logit regressions of the network dummy, that takes the value of 1 for managers with a higher than median number of connections, on the variables above. The sample consists of all top managers transitions in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table C.7: First-stage results: network composition effect

	Network Composition			
	Females		Males	
	(1)	(2)	(3)	(4)
Age	0.0149***	0.0215***	0.0223***	0.0273***
	(0.0030)	(0.0041)	(0.0014)	(0.0018)
Previous firm size	-0.1432***	-0.1432***	-0.1172***	-0.1327***
	(0.0201)	(0.0201)	(0.0064)	(0.0100)
Network size	-0.0020***	-0.0008***	-0.0004***	-0.0001
	(0.0001)	(0.0002)	(0.0001)	(0.0001)
New firm size		-0.0431**		-0.0835***
		(0.0176)		(0.0088)
Previous firm FE		-0.0437		0.1445***
		(0.0950)		(0.0440)
New firm FE		0.2203***		0.0641*
		(0.0773)		(0.0362)
Previous Wage	0.1158***	0.2014***	-0.0572***	-0.1293***
	(0.0327)	(0.0577)	(0.0157)	(0.0259)
Observations	7,494	4,677	28,125	17,732

This table presents logit regressions of the network dummy, that takes the value of 1 for managers with a higher than median share of male connections, on the variables above. The sample consists of all top managers transitions in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.

Table C.8: First-stage results: network composition effect of *Power* and *Depth*

	Network Composition			
	Females		Males	
	Depth	Power	Depth	Power
	(1)	(2)	(3)	(4)
Age	0.0189***	0.0113***	0.0194***	0.0176***
	(0.0030)	(0.0030)	(0.0014)	(0.0014)
Previous firm size	-0.1743***	-0.1713***	-0.1600***	-0.1271***
	(0.0108)	(0.0107)	(0.0059)	(0.0057)
Network size	0.0000	-0.4958***	0.0000***	-0.3443***
	(0.0000)	(0.0805)	(0.0000)	(0.0383)
Previous Wage	0.0346	0.1242***	-0.1247***	-0.0484***
	(0.0340)	(0.0357)	(0.0159)	(0.0163)
Observations	7,492	7,492	28,125	28,125

This table presents logit regressions of the network dummy, that takes the value of 1 for managers with a higher than median share of male connections, on the variables above. The sample consists of all top managers transitions in the 1995-2017 period. *, ** and *** indicates significance at the 10%, 5% and 1% level, respectively.