Universitat Autònoma de Barcelona

Essays in Labor and Organizational Economics

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Doctoral Thesis

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A dissertation submitted to the *Departament d'Economia i d'Història Econòmica* in fulfillment of the requirements for the degree of Doctor of Philosophy by the International Doctorate in Economic Analysis (IDEA).

May 4, 2018

Ao meu Pai, que está sempre comigo.

Acknowledgements

The years I have dedicated to the production of this dissertation contained some of the most important and arduous learning experiences of my life. The small, tightly woven, invisible decisions that have led me to the completion of this degree are indelibly tied to a growing number of people. The work portrayed in these pages has benefited from their crucial contribution to my cognitive development, mental well-being and therapeutic laughter. This page is a small symbol of my deep appreciation for those who are co-responsible for this day having arrived.

To Caterina Calsamiglia, whose invaluable guidance, support and encouragement have been pillars throughout my Ph.D.. I am forever grateful for all she has taught me, her insightful feedback, pragmatism and positive outlook, specially in moments when I doubted myself the most.

To Imran Rasul, whose welcome at University College London propelled me to broaden my research interests. His sharp advice and rigorous questions helped shape my job market paper and motivated me to find better answers.

To Pedro Portugal, for supporting my research visits to the Bank of Portugal. To Amélia Colaço Andrade, Fátima Teodoro, Hugo Reis, Isabel Horta Correia, Luca Opromolla, Lucena Vieira, Pedro Próspero and Soledade Sartoris for making these visits possible, granting me access to crucial data and providing me with most useful feedback.

To a long list of professors and colleagues whom I talked to, presented to, asked questions, shared conferences or office hours. My work has benefited immensely and increasingly from their input. I highlight Alejandra Ramos, Ana Rute Cardoso, Anna Houštecká, Annalisa Loviglio, Charles Noussair, Chris Bush, David Pérez-Castrillo, Edgardo Lara, Francisco Queiró, Joan Llull, Johannes Gierlinger, Hannes Mueller, Inés Macho-Stadler, Lavinia Piemontese, Marcos Vera, Martin Weidner, Matt Delventhal, Matthew Ellman, Miguel Ballester, Nezih Guner, Pau Milán, Paul Rodríguez Lesmes, Pedro Pita Barros, Richard Blundell, Sanghmitra Gautam, Thibaut Lamadon and Tomás Rodríguez Barraquer. A special thank you to Àngels Lopéz García and Mercè Vicente for helping me navigate all the necessary administrative procedures.

To my mother and brother, whose unwavering support gave me strength and resolve when it seemed I would not reach this day. To my friends Aurora, Dirina, Joana, Raquel and Sara, for giving perspective to my days and inspiration to my goals.

To my husband Alejandro - my dearest companion through countless battles, endless nights of typing, deep frustrations turned into tiny breakthroughs. Your calm and vehement faith in me have made a lonely job feel like teamwork, and the darkest moments blinded by cracking windows.

There is something almost nostalgic, yet profoundly freeing, in closing this chapter of my life. Thank you to all who made it possible.

Contents

Introduction

In this dissertation, I explore the role of unobserved labor-related inputs in determining organizational productivity. It is well-established in the economics literature that there are significant productivity differentials across organizations which are not attributable to traditional production factors such as capital or labor force. I study the connection between organizational productivity and management quality and intangible labor inputs, such as motivation and information, in private and nonprofit sectors. I use a combination of panel data analysis and experimental methodologies.

In Chapter 1, I use a matched employer-employee longitudinal data set to analyze the relationship between CEO quality and firm productivity in the private sector. In this chapter, I build on existing literature which uses CEO job switches across different firms to measure their quality. This implies the assumption that CEO and firm productivity contributions are perfectly separable, and that there is a universal ranking of CEOs. In other words, this approach precludes systematic CEO-firm match complementarities in revenue productivity. The novelty of my approach is that I use the time period before the CEO has assumed the lead managerial role so as to avoid the potentially confounding effects arising from selection in CEOs mobility. I find that a one standard deviation increase in CEO quality translates into a 5% increase in firm productivity. Importantly, the econometric findings point to the importance of further studying the CEO-firm match process and its productivity impact.

In Chapter 2, I take the findings in the previous chapter to motivate a matching model in which CEO and firm are part of a non-separable joint production function. This model allows for (i) endogenous CEO mobility based on match revenue realizations and (ii) unrestricted CEO-firm match complementarities to impact firm's outcomes. I use a finite mixture model with discrete firm classes and latent CEO types. The match between a CEO type and firm class produces a different set of revenue realizations, which embed the match complementarities. I find there is a significant impact of CEO-firm complementarities in production. In a counterfactual exercise, in which I randomly assign CEOs to firms, I find that complementarities between CEO and firm play an important role in determining firm productivity. On average, 2-3% of the firm productivity is accounted by those complementarities.

In Chapter 3, I study the impact of introducing non-financial incentives on prosocial behavior in the nonprofit sector. This project includes a finished lab experiment and a projected field experiment with a large international NGO. In the lab experiment, which I describe in detail in this dissertation, I randomize the introduction of six non-financial incentives in a real effort task. Volunteers know that the proceeds of this task revert to a well-known nonprofit. Each non-financial incentive intends to match a specific type of intrinsic motivation, as described in Bénabou & Tirole (2006). After knowing which non-financial incentive was assigned to them, subjects decide (i) whether to participate as a volunteer and (ii) how much time to donate. I find that the likelihood to contribute as a volunteer is greater when the subject is assigned to the incentive she most prefers (exact matching). Moreover, time donated is also greater under exact matching between incentives and motivations. Results suggest that incentives targeting motivation related to identification with the cause are the most effective in increasing productivity, whereas those concerning perceived tangible (monetary) rewards are less effective.

Chapter 1

The Impact of CEO on Firm Performance: a Fixed-Effects Approach

1.1 Introduction

There is extensive documentation in the economics literature suggesting substantial productivity differentials within countries (Foster et al., 2008) and industries (Titman & Wessels, 1988; Smith & Watts, 1992; Bradley et al., 1984; Syverson, 2011), which are not attributable to production factors. These productivity differentials may be partly explained by managerial ability. Managers are responsible for overseeing production, public service and project delivery across for-profit, non-profit and public sectors. Understanding the role of top-management[1] is therefore an important step in analyzing productivity gaps in the economy.

The clear selection of CEOs into firms make identifying a causal effect of CEOs' ability on firms' outcomes a very demanding task. This challenge is at the root of the somewhat sparse evidence in the literature regarding the role of individual CEOs in firms' outcomes. In particular, existing studies rely on the two restrictive assumptions that (i) CEO's ability enters the firm's production function additively and (ii) the same CEO's ability is translated into production equally at any firm they work for, i.e., there are no persistent effects of CEO-firm match in production.

This chapter evaluates the impact of CEOs' ability in firm productivity. I provide an empirical test of the assumptions described and a novel methodology to measure CEO ability, which accounts for selection in CEO-firm pairs. There have been significant recent advances in the literature documenting a relation between managers' individual ability or firm-wide managerial practices and firm outcomes. In the absence of a natural experiment that randomizes CEO-firm assignment, some studies[2] rely on job-to-job transitions to estimate CEO ability. Based on the pioneer work of Abowd et al. (1999), using job transitions provides a quasi-natural experiment that allows for the separate identification

[1] I use the term "CEO" throughout the remainder of the chapter. However, in the data analysis portion of this chapter I under the CEO label I include any and all head-manager of a firm with more than 20 employees.

[2] Bertrand & Schoar (2003); Bender et al. (2016).

of the contributions of the CEO and other firm's resources to its outcomes. The main shortcoming of these studies is the non-random nature of CEOs job transitions across firms, which results in a biased estimation of the importance of the CEO in determining firm productivity. In fact, I show evidence that the impact of CEOs ability in firms' outcomes is overestimated when resorting to job-to-job transitions, since what is measured as CEO ability may in fact stem from a match-quality surplus that is assumed away.To overcome this challenge, I take advantage of a rich data set to develop a novel measure of CEO quality by looking at their performance in the labor market in their early career years, before becoming a CEO. This allows me to significantly mitigate the impact of non-random assignment between CEO and firm.

I develop a simple firm production model to guide the empirical analysis. In a closed economy, firms produce revenue with the traditional inputs -labor and capital- and must hire a CEO to oversee production. Both firm and CEO are endowed with a certain technology type (firm) and quality type (CEO), exogenously determined, which accompanies them throughout the time period under analysis. The CEO's role is mediated in the firm through a span-of-control technology (Lucas, 1978) and CEO's unobserved quality is a labor augmenting input in the production function. I derive testable implications regarding the impact of CEO and CEO-firm complementarities in firm production, which I study empirically.

The empirical analysis is divided into four parts and hinges crucially on the quality and breadth of the data I use. The Portuguese *Quadros de Pessoal* matched employer-employee survey captures a significantly large spell of each CEO's tenure in the labor market, thus allowing me to separate their non-CEO from CEO years. First, I show empirical evidence of non-random assignment of CEOs to firms. CEO mobility appears to be motivated by the search for a better CEO-firm match, after controlling for CEO and firm types. However, the pattern of mobility of employees before becoming CEOs is as good as random vis à vis the employee-firm match, in line with relevant literature (Card et al., 2013, 2015; Sorkin, 2017)[3].

Second, I proceed to build a proxy measure of CEO ability that significantly reduces the bias introduced by non-random CEO assignment, by exploiting the full labor market spell of the CEO. I use the non-CEO (employee) years to estimate the (then) employee's ability as a fixed effect, in the same spirit of Abowd et al. (1999, 2002) by estimating a two-way fixed-effects regression on log-wages. This strategy draws on the fact that mobility pattern in non-CEO years appears as good as random. I evaluate the explanatory power of wages by employee and firm types using a variance decomposition exercise and accounting for finite sample bias (Kane & Staiger, 2008; Gaure, 2014; Best et al., 2017)[4]. I find similar variance decomposition estimates as in the relevant literature, reassuring that the use of this part of the sample does not significantly alter the relative weight of heterogeneity sources.

Third, I evaluate the role of the CEO's ability in firm productivity. I take the standardized estimated fixed effect (estimated in the previous step) as a measure of CEO

[3]The mobility of employees can be used as a quasi-natural experiment to the extent that there is plausible evidence that mobility is orthogonal to specific employee-firm wage realizations; that is, that mobility is orthogonal to the employee-firm match outcome after controlling for employee and firm types.

[4]See Andrews et al. (2008, 2012) for detailed descriptions of finite sample bias (also known as incidental parameter bias) in the context of fixed effects regressions.

quality[5][6]. I evaluate the impact of the measure CEO ability in firm productivity by estimating a firm production function in which the estimated CEO ability is an additional input.

Fourth, I conduct heterogeneity analyses of the coefficient estimates for CEO ability, along firm characteristics (size, economic sector, ownership structure, profits, labor productivity and innovation expenditure) and CEO characteristics (schooling, tenure, experience as CEO, age). I also perform robustness checks for estimated CEO ability and production function specification.

I find that a one standard deviation increase in CEO's ability can be translated into an increase in 5% revenue productivity, after controlling for experience, schooling and other CEO observables. This result is in line with findings in the literature that uses natural experiments[7]. Importantly, I find evidence of overestimation of the individual role of CEO in firms' outcomes in previous literature literature[8]. In particular, I empirically show that the match-driven selection of CEO to firm seems to drive a significant portion of the variation in firm output that has thus far been attributed to the individual CEO ability, in an additive and fully separable model[9].

I present heterogeneity analysis of CEO quality according to observable characteristics of firm and CEO. CEO's quality is more important in the services industry, smaller to medium sized firms, and firms where there is high average worker mobility. Simultaneously, CEO's quality is positively related with observables such as schooling and tenure in the labor market, and negatively correlated with family firm ownership.

This chapter contributes to the literature of organization economics and corporate governance, which has made significant progress in documenting a relationship between top-management[10], in corporate outcomes[11], either through their characteristics (Bertrand & Schoar, 2003; Bennedsen et al., 2012; Queiró, 2016), their time use (Bandiera et al., 2013), or firm ownership structure (Bennedsen et al., 2007; Pérez-González, 2006). Alongside, there have been studies documenting the relation between managerial practices and firm (Bloom & Van Reenen, 2007; Mion et al., 2016) or public bureaucracy (Rasul & Rogger, 2018; Best et al., 2017) performance. In this chapter, I build on on these groups of studies by (i) addressing the challenge of non-random assignment of managers to firms when analyzing panel data sets by proposing forth a proxy measure of CEO quality that attenuates the endogenous CEO selection, (ii) providing evidence that a purely additive model of CEO quality in firm output does not explain the full variation in firm productivity attributable to the CEO and (iii) generalizing the scope of the analysis of the role of CEOs in the firm, which is typically placed on very large and/or publicly traded firms,

[5] Assuming one-dimensional ability and that the selection of a candidate for a position is based on the candidate's performance in their current role (Peter et al., 1969), rather than abilities required for the intended role.

[6] From now on, the mention of "CEO ability" refers to the proxy measure of ability derived from the fixed effects estimation and is a concept used in the limited scope of this chapter.

[7] Pérez-González (2006); Bennedsen et al. (2007).

[8] Bertrand & Schoar (2003); Bender et al. (2016).

[9] By "additive and fully separable model" I mean a framework in which the contributions of CEO ability and firm productivity can be perfectly separated if all information is known. Moreover, CEO ability and firm productivity interact in an additively linear manner.

[10] Chief Executive Officers (CEO), Chief Financial Officers (CFO) and Chief Operating Officers (COO).

[11] Profits, ROA, ROI, M&A decisions among others.

to include all medium and large firms in the Portuguese private sector.

This chapter also fits in the broader category of studies that use panel data to separately identify sources of invididividual-level heterogeneity in labor market outcomes. This literature corresponds to the influential and widespread two-way fixed-effects approach, put forth by the seminal works of Abowd et al. (1999, 2002)[12]. Their work presents a tractable model that employs worker and firm fixed-effects to account for the relative importance of worker and firm heterogeneity in wage dispersion, under some (potentially) stringent assumptions[13].

The remainder of theis chapter is organized as follows. Section 1.2 details the theoretical model which establishes a framework for the empirical analysis. Section 1.3 describes the data and provides relevant information regarding its institutional context. Section 1.4 documents the estimation of CEO quality and its contribution to firm productivity and section 1.5 presents a battery of robustness analyses. Section 1.6 concludes and sets avenues for future research.

1.2 Conceptual Framework

In this section I present a model of firm production where CEO quality is a TFP-augmenting input in production. This stylized model presents a conceptual framework for the empirical analysis developed in section 1.4. The model is composed of two parts, which differ on the assumptions made regarding CEO job mobility. First, I assume CEOs move based on their ability type and firms' types. On a second part, I assume CEOs decision to move also takes into account the CEO-firm match specific realizations and that CEO-firm complementarities are a relevant determinant of overall firm production.

1.2.1 CEO Quality and Firm Productivity with Random Assignment

Consider a closed economy with a homogenous labor force of size L and K units of homogenous capital, both supplied inelastically to the market. The two factors can be combined to achieve production that is sold in a homogenous-good, price-taking market. In order for the firm to operate, it must hire a head-manager/CEO[14] to lead the company and oversee production. Moreover, the quality of the CEO is also an input in the production[15]. Therefore, besides the two traditional inputs (L and K), firm's production is also affected by the quality of the CEO[16]. In the context of this model, CEO "quality" can be

[12] Abowd, Kramarz and Margolis' work has been a cornerstone in the study of wage inequality (Card et al., 2013; Song et al., 2015), gender gap in wages (Cardoso et al., 2016), bargaining and sorting (Card et al., 2015). Their method has also been adopted by other economics fields, specifically in identifying teacher versus school value-added in student performance (Jackson, 2013), or to document sources of variation in health care utilization in the U.S. (Finkelstein et al., 2016).

[13] Some recent papers adopt instrumental variable approaches to disentangle between worker and firm heterogeneity in wage setting (Jäger, 2016).

[14] Throughout the rest of the chapter, I use the label "CEO" to define the concept of head-manager of any firm, regardless of size.

[15] (Guner et al., 2015) also use managerial skill as a production function input.

[16] For simplicity, I borrow Lucas (1978) assumption that workers are a readily available factor of production to the CEO.

thought as an interaction between innate ability and human capital. In this chapter, I assume "quality" is given by a fixed-effect which the CEO brings to any firm (s)he works for[17].

Let the described economy consist of J firms, $j \in \{1, ..., J\}$, at each time-period $t \in \{1, ..., T\}$. Firms are endowed with a firm-specific total factor productivity (TFP) type (A_j) when they are active. Firm technology type is represented by a fixed distribution $\Lambda : \mathbb{R}^+ \to [\underline{A}, \overline{A}]$. Moreover, firms can hire from a pool of CEOs, $i \in \{1, ..., N\}$. As in Lucas (1978) span of control model, I assume CEO's quality is exogenously determined and there is a continuum of identities that are fully represented by a fixed distribution $\Gamma : \mathbb{R}^+ \to [\underline{\alpha}, \overline{\alpha}]$ at each time t. CEO quality is assumed to be permanent and unidimensional (Becker, 1973). I also assume that CEOs are hired according to their success as employees was (Peter et al., 1969). Managerial ability (α_i) enters the production function in two ways. First, as an input (Bender et al., 2016). Second, through a decreasing returns to scale (DRS) transformation of the production function, reflecting the limited span of control of the CEO[18].

At every period, the CEO can move to a new firm. At this stage of the model, let us assume that CEO job mobility is only driven by α_i and A_j, and not by the CEO-firm match specific wage realizations. A standard Cobb-Douglas constant returns to scale (CRS) function represents the firm's production function if span of control were unlimited:

$$Y_{j,t} = A_j \alpha_i^{\mu} L_{j,t}^{\delta} K_{j,t}^{1-\delta-\mu} \tag{1.1}$$

where α_i and A_j correspond to the CEO quality type and firm technology, respectively. Given the limited span of control of the CEO, production oversight and monitoring is a DRS transformation of equation (1.1). I assume this transformations takes the form of a natural logarithmic function $g(Y_{j,t}) = ln(Y_{j,t})$, such that:

$$ln(Y_{j,t}) = ln(A_j) + \mu ln(\alpha_i) + \delta ln(L_{j,t}) + (1 - \delta - \mu) ln(K_{j,t}) \tag{1.2}$$

An allocation of resources is described by $L_j(\alpha_i)$ and $K_j(\alpha_i)$, which correspond to the labor and capital allocations of firm j managed by a CEO with quality α_i. Labor and capital can be hired at equilibrium prices w and r, respectively[19].

For the remainder of the chapter, I focus on gross revenue as the object of the firm's maximization problem[20]. That is,

$$PY_{j,t} = (P * A_j) \alpha_i^{\mu} L_{j,t}^{\delta} K_{j,t}^{1-\delta-\mu} \tag{1.3}$$

[17]An illustrative example would be to think of this quality as an identity type, something that is particular to the CEO.

[18]The intuition being that the CEOs' management and supervisory abilities are inversely proportional to the quantity of L and K units under their control, *ceteris paribus*.

[19]I assume firm size is small enough not to change equilibrium prices.

[20]Several papers in the growth and productivity literature (Hsieh & Klenow, 2009, 2014) and in the organizational literature (Bloom & Van Reenen, 2007; Bertrand & Schoar, 2003; Bandiera et al., 2017) use revenues as outcomes.

where $P * A_j$ is TFPR (or revenue TFP[21]) and P is unique in the final homogenous goods market, reflecting the price-taking behavior of firms. The following proposition illustrates the hypothesized relationship between CEO ability and firm's productivity, measured in gross revenue. This hypothesis will be tested in the empirical analysis, in section 1.4.

Proposition 1 *A higher level of CEO quality results in higher firm gross revenues.*

$$\frac{dPY_{j,t}}{d\alpha_i} > 0 \tag{1.4}$$

Proof: see Appendix A.1.

1.3 Data & Context

I provide a brief account of relevant features regarding the context of the data used in this chapter. While my analysis is based on Portuguese data, the main labor market and productive sector characteristics in Portugal indicate that results may be generalized to other EU or OECD countries. Figure 1.1 presents the evolution of labor market participation rates. The Portuguese participation rate has been fairly constant over the past 10 years, at approximately 74.1% of the whole population. This figure is similar the 72% estimated for EU average in 2016) and OECD average (71.7% in 2016)[22]. The ratio of manager to non-manager employee population is estimated at 6.7% for the Portuguese labor market, a figure close to the OECD average of 6.4%[23]. Despite the similarities in labor participation, labor productivity in Portugal is significantly lower than that of the EU[24]. This gap in productivity makes a stronger case for the role of non-input related productivity differentials in general, and CEO quality in particular.

Alongside labor market features, Portuguese economic activity can be representative of other EU countries. Portugal has experienced, as most southern European countries, a severe economic downturn in the aftermath of the Great Recession followed by a slow recovery that has placed GDP growth at no more that 1-2% a year[25]. Small and medium sized firms represent 99% (95% OECD average) of the total number of firms in Portugal and have accounted for between one half and two thirds of its total value creation over the past decade[26]. Most (68.2%) of these firms' employment is dedicated to services, comparable to a 72% in the EU[27]).

I combine two data sets to generate a matched employer-employee panel. Employee information comes from *Quadros de Pessoal*, a proprietary data set collected and administered by the Portuguese Ministry of Employment, drawing on a compulsory annual employment census of firms[28] that have at least one employee on payroll during the

[21] Hsieh & Klenow (2009, 2014).

[22] Source: OECD (2017), Labour force participation rate (indicator). doi: 10.1787/8a801325-en. The US participation rate is similar (73%).

[23] Source: OECD (2014), Share of employed who are managers.

[24] Source: PORDATA and Eurostat (2016).

[25] Source: Bank of Portugal.

[26] Source: PORDATA (2017), *Empresas, Pequenas e Médias Empresas.*

[27] OECD (2015), Employment in the services sector.

[28] Public administration and informal market services are excluded. Includes private, nonprofit and public firms.

survey reference week[29]. Firm level data is obtained from *Informação Empresarial Simplificada (IES)*, a mandatory annual survey on firm financial information. The two data sets are merged by a common firm identifier. The QP data set has been used in numerous fields of labor economics, namely in the study of gender wage gap[30] and bargaining and unions[31]. The mandatory character of both *Quadros de Pessoal* and , together with reporting based on tax-authority valid profiles[32], lends particular credibility to the data set at hand. Moreover, the QP encompasses the entirety of the Portuguese economic private sector, making its breadth reassuring in providing a safe ground on which to run meaningful empirical analyses.

1.3.1 Employee and CEO Data

Quadros de Pessoal is a longitudinal data set on private sector employees, spanning from 1986 to 2013[33]. As of 2013, the survey collected information on approximately 450,000 firms and 3 million employees. Reported data cover each firm (location, economic activity, employment, sales, and legal status) and each of its workers (gender, age, education, skill, occupation, tenure, managerial versus non-managerial position, hours worked, overtime, and earnings[34]). Firms and workers entering the database are assigned a unique, time-invariant identifier that allows to tracking of firms and worker pairs over time. The data covers information on all personnel working for any firms with at least one employee on payroll.

Importantly, the variable "occupation" allows me to identify the managing director or CEO[35]. For the purpose of the empirical analysis, I focus on single job holders and full-time jobs held by men and women aged between 18 and 68 years old. I perform a 98%[36] winsorization of wage outliers[37].

Table 1.1 presents means and standard deviations[38] for two samples: non-CEO and CEO employees. Column 1 presents the results for CEO employees. Female CEOs make up about 30% of the sample; the average CEO is around 45 years old; about 35% of managers hold a higher degree and have been in that management position for over 5 years. There are considerable differences between CEOs and other employees, both at the earnings level and demographics aspect. Importantly, CEOs present higher job mobility, both across firms and across positions and achieve considerably higher earnings

[29] One week of October of each year.

[30] Cardoso et al. (2016).

[31] Card et al. (2015); Addison et al. (2017).

[32] All employee salaries and employer sales revenues are reported as they are declared to the Portuguese Tax Authority (*Direcção Geral dos Impostos*) and Social Security.

[33] The survey has waves after 2013; however, these are the ones available at the Bank of Portugal.

[34] The information on earnings includes the base wage (gross pay for normal hours of work), seniority-indexed components of pay, other regularly paid components, overtime work pay, and irregularly paid components.

[35] Appendix A.2 elaborates on the methodology used to identify the firm CEO.

[36] I set all variables below the 1% percentile to the 1% percentile of the distribution; the same with 99% percentile.

[37] Appendix A.2 provides further details on sample selection criteria.

[38] Summary statistics for Table 1.1 are constructed using the average of cross-section estimates.

levels. Table 1.2 presents the same statistics for the largest connected set of firms[39]. In comparison, mostly all variables exhibit similar descriptive patterns within the largest connected set and the whole sample. This will become important in section 1.4.2.

The data set also allows me to track employee job to job transitions. In fact, this source of variation is crucial for the identification of person versus firm effects on production. Table 1.3 displays executive transitions, that is, manager switches between positions and firms. In the interest of completeness, I present all switches between any combination of two out of the four management positions: CEO [40], Financial Manager, other high-level managers and mid-level managers. Other high-level managers consist of operative mangers and others who report directly to the CEO. Employee transitions encompass the job mobility of non-managerial employees. In the remainder of this chapter, I focus on the CEO-CEO and employee-employee job transitions. Importantly, Tables 1.1,1.2 and 1.3 point to the fact that the amount of job transitions declines as the employee becomes a CEO. This illustrates part of the differential mobility patterns between CEO and employees, developed further in the next section. The large amount of job transitions, particularly within-groups, is encouraging as it provides valuable job mobility that will be exploited in the identification strategy.

1.3.2 Firm data

The *IES* dataset spans from 2005 to 2015 and includes financial information of the firm. The survey reports data on balance sheet and profit and loss statements. This includes data on capital, raw materials and other consumables, services used in production, salaries and employment, added value, sales, profit or loss. These data are merged with employee-level data via a common firm identifier.

Table 1.4 presents summary statistics on firm characteristics[41]. A share of approximately 28% of all firms is located in Lisbon, whereas 19% are in Porto. Approximately 37% of the firms operate in the manufacturing sector, 14% in construction and 49% in the service sector. The average firm has 18 employees. For the purpose of the upcoming empirical analysis, I focus on non-agriculture sector firms, exclude non-profit and banking related organizations. For more details on sample selection, see Appendix A.2.

1.4 CEO Quality and Firm Productivity

I use a reduced-form approach to the estimate of the production function model developed in the conceptual framework section. In this section, the baseline econometric model unfolds in two stages. First, I measure CEO quality as a person fixed effect in a wage regression, in the tradition of Abowd et al. (1999). Second, the estimated CEO quality is used as an input in the production function. This amounts to testing the validity of Proposition 1 of the conceptual framework.

[39] The largest connected set is, within the groups of firms that are linked together by employee mobility, the one that encompasses more observations within the sample. See section 1.4.2 for a more detailed description of the largest connected set.

[40] Highest management level within the firm organization.

[41] Summary statistics for Table 1.4 are made up of averages of annual cross-sections, except in the case of firm longevity.

1.4.1 Measuring CEO Quality

In the first stage of the reduced form approach, I estimate a model that separates the components of wage variation attributable to employee-specific and firm-specific heterogeneity. I use the two-way fixed-effects model first introduced by Abowd et al. (1999). The economy consists of $i = 1, ..., N$ employees and $j = 1, ..., J$ firms. I model the logarithm of wages as a function of employee observables, firm and employee fixed effects:

$$y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it} \tag{1.5}$$

Consider *j(i,t)* as the indicator for the firm j where employee i works at time $t.y_{it}$ stands for deflated log of wages, $A_{j(i,t)}$ represents firm j fixed effect, which captures constant firm specific heterogeneity, α_i are the employee fixed effects and X_{it} represents a vector of time-varying employee-level control variables[42] and year fixed effects. The parameter of interest is α_i, which I interpret as an innate ability that is valued in the labor market in the same way. I use α_i as the estimated employee quality[43]. I recover the estimated employee qualities for the firm CEOs that are later used as an input in the production function.

1.4.2 Identification and Connected Sets

Ideally, for the purpose of this study, employees would be assigned to firms randomly in an experimental setting and moved randomly throughout the period under observation. This would allow for straightforward separate identification of employee and firm contributions (i.e. heterogeneity types). In a non-experimental setting such as the one in this chapter, separate identification of employee and firm fixed effects can only be achieved if we observe employees working for more than one firm and firms employing more than one employee over the time-series. In other words, we need firms to be linked to one another through employees who move between them in a connected set. The stronger the link, i.e. the more frequent the employee moves, between these two firms, the more accurate the separation of the influence of the employee from that of the firm type on wage setting over time. As shown in Abowd et al. (2002), connectedness is a sufficient condition for identification. As a result, identification can be achieved within a connected set of firms[44].

I follow previous work[45] by focusing on the largest connected set of firm, linked together by non-CEO employee mobilty. This approach seems reasonable in this particular setting, since approximately 96% of the employee-firm pairs are captured within the largest connected set. Moreover, I find similar summary statistics between the largest connected set and the full sample[46] (Table 1.2). Abowd et al. (1999, 2002) prove that,

[42]Quadratic terms in age fully interacted with schooling levels. More details on variables and coding are presented in Appendix A.2.

[43]A definition of "quality" in the context of this chapter is further described in Appendix A.2 of this chapter.

[44]As a counter example, in the case of an employee who stays in the same firm throughout the whole panel time-span, the employee and firm fixed effects cannot be disentangled.

[45]Card et al. (2013); Cardoso et al. (2016).

[46]See Appendix A.3 for a more nuanced analysis of the differences in the largest connected set.

within each connected set, the employee and firm effects are identified only in relation to each other. Therefore, and also in the spirit of previous literature, I take a random firm as reference, normalizing that firm effect to zero and estimating unconditional variances in section 1.4.4.

1.4.3 Job Mobility and Causality

The quasi-experimental nature of the empirical model presented in equation (1.5) derives from the job mobility[47] of employees across firms. This mobility can provide a causal interpretation of employee and firm fixed effects on wage setting to the extent that employee job transitions are orthogonal to the error term. I write the error term associated with equation (1.5) in three parts, as Card et al. (2013), to highlight potential cases where the stated orthogonality condition does not hold. The error term ε_{it} is composed by a random employee-firm match effect, $(\lambda_{j(i,t)})$, a unit-root process that reflects increments in employee quality (ω_i) and a transitory shock component $(\upsilon_{j(i,t)})$ as described in equation (1.6):

$$\varepsilon_{it} = \lambda_{i,j(i,t)} + \omega_{it} + \upsilon_{it} \tag{1.6}$$

The match effect component $(\lambda_{i,j(i,t)})$ represents wage premiums or discounts that employee i faces when matched with firm j that go beyond the channels of firm heterogeneity or employee quality. Match effects could arise if specific employees are especially suited (or unsuitable) for specific firms. Match-specific wage components are present in the search-and-match literature which models an idiosyncratic component of output associated with each possible job match[48]. I apply the same logic in the context of this model, where match effects are reflected in wages. The unit root component ω_{it} reflects potential drift in employee quality that has lasting effects. This component encompasses a wide array of shocks with permanent effects to the employee's ability, such as health shocks or unobserved human capital accumulation. For the time being, I assume that ω_{it} has mean zero for each employee in their observed time period. Finally, there is a transitory term (υ_{it}) that represents any other temporary shock that affects the outcome, which is also assumed to have mean zero for every employee in their observed time period.

To achieve causal identification, OLS assumptions regarding the previously described error terms must hold. Let $\boldsymbol{y}$ denote the stacked $NT \times 1$ vector of year-sorted employee wages (where $NT = N$), $\boldsymbol{E}$ denote the $NT \times N$ design matrix of employee indicators, $\boldsymbol{F}$ is the $NT \times J$ design matrix of firm indicators and $\boldsymbol{X}$ is a $NT \times J$ matrix of time-varying employee covariates and ε denotes the error term. Equation (1.5) can be written in matrix notation as:

$$\boldsymbol{y} = \boldsymbol{C\alpha} + \boldsymbol{F\psi} + \boldsymbol{X\beta} + \boldsymbol{\varepsilon} \tag{1.7}$$

[47] I label a job mobility "event" by identifying employees whose associated firm identifier changes from one period (year) to the next.

[48] Mortensen & Pissarides (1994); Shimer & Smith (2000); Postel-Vinay & Robin (2002); Eeckhout & Kircher (2017); Hagedorn et al. (2017).

Consistent estimation of equation (1.7) via OLS implies the following assumptions regarding the interaction of the error terms with explanatory variables:

$$E[e^{i\prime}\varepsilon] = 0, \forall i \qquad E[f^{j\prime}\varepsilon] = 0, \forall j \qquad E[x^{k\prime}\varepsilon] = 0, \forall k \tag{1.8}$$

Equation (1.8) describes OLS orthogonality conditions between the error term ε and each regressor. Whereas the assumption on the vector of regressors x^k is standard, the same cannot be said about the identifying assumption regarding the employee and firm indicators, e^i and f^j. These assumptions allow for an array of sorting patterns between firm and employee[49]. However, the same assumptions preclude the existence of sorting on match-specific effects, i.e. sorting on premia/discounts earned from a specific individual employee-firm pair[50]. If this type of endogenous mobility is present in the CEO data, causal identification of equation (1.7) is threatened since it would imply a positive correlation between a component ($\lambda_{i,j(i,t)}$) of the error term and f^j. In fact, given the assumptions made on the error term components, causal identification of equation (1.7) boils down to the verification of the assumption $E[f^{j\prime}\varepsilon] = 0, \forall j$[51]. Appendix A.3 provides intuition for a simple example with two CEOs, two firms and two time periods. I now discuss three cases that would lead to biased estimates of fixed effects.

Consider an employee who moves from firm 1 to firm 2, from *t-1* to *t*. The individual's expected change in wages can be summarized by:

$$\begin{aligned} E[y_{it} - y_{it-1}|j(i,t) = 2, j(i,t-1) = 1] &= \psi_2 - \psi_1 + E[\varepsilon_{it}|j(i,t) = 2] - E[\varepsilon_{it}|j(i,t-1) = 1] \\ &= \psi_2 - \psi_1 + E[\lambda_{i2} - \lambda_{i1}|j(i,t) = 2, j(i,t-1) = 1] \\ +E[\omega_{it}|j(i,t) = 2] - E[\omega_{it}|j(i,t-1) = 1] &+ E[\upsilon_{it}|j(i,t) = 2] - E[\upsilon_{it}|j(i,t-1) = 1] \end{aligned} \tag{1.9}$$

In the absence of bias, the expected wage differential for employee i is $\psi_2 - \psi_1$. The presence of sorting based on the employee-firm match component of wages, i.e. endogenous mobility, will result in biased OLS estimates. If this type of sorting occurs, we should observe $E[\lambda_{i2}] \neq E[\lambda_{i1}]$. That is, on average we should observe that the wage premium for an employee who moves from firm 1 to firm 2 is significantly different from the premium faced by an employee who moves in the opposite direction. In order to assess the possibility of endogenous mobility I use event studies as used by Card et al. (2013)[52]. I define an event as any job transition of an employee from one firm to another in consecutive time periods *t-1* and *t*, provided that the employee stays with both firms for at least two years. I classify jobs at origin and destination firms[53] according to the respective quartile of coworker wage distributions. I assign each job transition event to

[49]There can be systematic sorting of effective firms with effective employees (up to a pre-determined measure of effectiveness) that does not break the assumptions of equation (1.8).

[50]Intuitively, the phenomenon of (match-specific) endogenous mobility can be thought of as the pursuit of the perfect match between a CEO and a firm or employee and firm.

[51]Proof: Appendix A.1.

[52]The event study methodology is also used in Card et al. (2015); Finkelstein et al. (2016); Best et al. (2017).

[53]Define firm where employee works in *t-1* as the "origin" firm. Define firm where employee works in to *t* as the "destination" firm.

one of 16 cells of origin and destination quartiles of coworker wages. I calculate mean wages in the years before and after the event for each quartile cell.

Panel A of Figure 1.2 exhibits the employee event study graph. The plot depicts the job transition event timeline against average log wages for each trajectory of quartile mean coworker wages. Log wages are residualized of year fixed effects. We can observe that wage differentials between switching employee trajectories appear symmetric. Note, in particular, the trajectories from a first quartile to fourth quartile firm change and vice versa: the average wage change has opposite sign but a similar order of magnitude. Indeed, the ratio between the average wage gain in the first trajectory (first to fourth quartile) and the average wage loss in the second trajectory (fourth to first quartile) is approximately 1, reinforcing the observed symmetry[54]. This conclusion goes in line with similar findings in the labor literature[55]. Moreover, the ratio between average estimated gains and losses (the ratio between slopes of symmetric movements) is very close to 1.

Panel B of Figure 1.2 shows a different mobility pattern for CEOs. The same event study exercise applied to the pool of CEOs instead of employees, yields an asymmetric plot. Focusing on the switching trajectories between first and fourth quartiles[56], the increase in wages resulting from the movement from the first to fourth quartile is significantly greater than the other way around. CEOs appear to move to a new job because systematically due to specific gains in wages from that CEO-firm pair. Moreover, the ratio between average estimated gains and losses is statistically different from 1.

The unit root component presents another source of bias if we observe $E[\omega_{it}|j = 2 \neq E[\omega_{it}|j = 1]$ in equation (1.9). In that case, a positive (or negative) drift in employees' quality[57] would result in systematic changes towards better (or worse) firms. This would be translated into an observable time trend in mean wages in Figure 1.2. This pattern is not found for employees (Panel A). However, Panel B shows that CEOs display a slightly increasing wage trend, possibly indicating that cumulative experience is increasingly valued in a CEOs career.

It is possible that a transitory wage shock is correlated with employee job mobility (e.g. plant closures). This would lead us to overstate the difference in employee effects since $E[\upsilon_{it}|j = 2] \neq E[\upsilon_{it}|j = 1]$ in equation (1.9) and would translate into an Ashenfelter's dip[58] in wages before a job transition. No such dip in wages is observed in wither panels of Figure 1.2.

1.4.4 Estimation and Variance Decomposition

The event studies indicate there are two different patterns of employee job-to-job transition. During the non-management years, it appears that the Portuguese data validates the literature's result that points to a match-specific exogenous mobility pattern. Later, in the years as a CEO, job transitions seem to be more oriented towards incorporating CEO-firm match gains. The differential mobility patterns throughout a CEO's career provide a case for the use of the non-managerial labor market spell of the CEO to esti-

[54]The ratio is calculated between the two expected wage differentials from *t-1* to *t+1*.

[55]Card et al. (2013, 2015).

[56]The same patterns can be found for other trajectories; see Appendix A.4.

[57]e.g. Increases in human capital accumulation.

[58]Ashenfelter (1978).

mate a proxy measure of her quality as CEO.

Focusing on the first stage of the CEOs career provides three important advantages. First, I avoid the endogenous mobility bias arising from job transitions because of CEO-firm specific wage realizations. Second, given the time separation between the years as an employee and years as a CEO, I ensure that the proxy CEO quality measure is, by construction, exogenous with respect to firm productivity in the CEO years. Third, this approach is compatible section 1.2.1 model's assumptions that CEO quality is unidimensional (Becker, 1973) and that selection of a candidate for the position of CEO is based on the candidate's performance in their previous job position (Peter et al., 1969).

I estimate equation (1.5) on the largest connected set of non-managerial spells for all CEOs for which a large enough employee spell is available in the data set. To ensure employee spells are comparable and not confounded by possible endogeneity in timing of ascent to managerial positions, I define the employee labor spell up until a maximum age[59]. I cluster standard errors at the employee-firm level, accounting for the two-way fixed effects nature of the regression in equation (1.5). I decompose the variance and covariance components log wages as:

$$
\begin{aligned}
Var(y_{it}) = Var(\alpha_i) + Var(\psi_{j(i,t)}) + Var(X\prime_{it}\beta) + 2 * Cov(\alpha_i, \psi_{j(i,t)}) \\
+2 * Cov(\psi_{j(i,t)} X\prime_{it}\beta) + 2 * Cov(\alpha_i, x\prime_{it}\beta) + Var(\varepsilon_{it})
\end{aligned}
\tag{1.10}
$$

where $\frac{Var(\alpha_i)}{Var(y_{it})}$ and $\frac{Var(\psi_{j(i,t)})}{Var(y_{it})}$ represent the percentage of wage variation that is explained by employee quality and firm heterogeneity, respectively. Results can be found in Table 1.4. The baseline variance computation is presented in column 1 and 2. Columns 3 and 4. CEO proxy measure of quality, the employee fixed effect, accounts for approximately 60% of employee wage variation, whereas firm heterogeneity accounts for about 20%[60]. I present "shrinkage" estimators of variance components as in Kane & Staiger (2008), to account for overestimation of employee and firm fixed effects resulting from finite sample bias[61].

The first stage thus results in estimation of proxy measures of CEO quality by focusing on the employee fixed effects before she became a CEO.

1.4.5 Estimating Firm Productivity

In the second stage of the reduced-form estimation I use the CEO quality measure obtained in the first stage to evaluate the role of CEO in firm productivity. Consider the following baseline Cobb-Douglas production function specification[62] for firm j at time t:

$$
q_{jt} = \delta + \phi CEO_{i(j,t)} + \boldsymbol{W_{jt}\beta} + \boldsymbol{Z_{jt}\gamma} + \epsilon_{jt} \tag{1.11}
$$

[59] Maximum age is defined as the 75th percentile of age when the employee first became a CEO, 38 years old. Robustness checks find no significant difference in using 90th percentile.

[60] These results go in line with the labor economics literature: Card et al. (2013); Bonhomme et al. (2017b).

[61] Finite sample bias is also referred in the literature as incidental parameter or limited mobility bias. See Andrews et al. (2008) for a detailed description of this type of bias, commonly associated to panel data estimation. I present a detailed description of the variance shrinkage method used in this section in Appendix A.3.

[62] Bloom & Van Reenen (2007).

where q_{jt} is the log of deflated sales, $\mathrm{W_{jt}}$ is a vector of variable inputs and $\mathrm{X_{jt}}$ is a vector of state variables, all in logarithm form. The proxy measure for CEO quality ($CEO_{i(j,t)}$ for CEO i who works for firm j at time t) also enters the production function as a state variable[63], as suggested in section 1.2.1. The sequence A_{jt} is unobserved firm productivity. The error term ϵ_{jt} has the following structure:

$$\epsilon_{jt} = A_{jt} + \eta_{jt} \tag{1.12}$$

where A_{jt} is a transmitted firm productivity parameter (persistent in time) and η_{jt} is an iid transitory shock.

I use the production function estimation method proposed by Wooldridge (2009). I expand the production function to include CEO quality as a state variable. This method combines Olley & Pakes (1996) (OP) and Levinsohn & Petrin (2003) (LP) approaches[64] in their treatment of simultaneity bias[65]. Both works resort to proxy variables (investment and materials, respectively) to measure firm productivity in two step estimation approaches[66].

Following recent literature, I use a GMM approach rather than two step procedure to jointly estimate both firm productivity and input coefficient[67] in equation (1.11). This approach assumes that productivity A_{jt} is described by a function $g(x_{jt}, m_{jt})$ of state variables and a set of instruments m_{jt}. I use real value of intermediate materials and services used as proxy variables for non-observed firm productivity, as Petrin & Sivadasan (2013), to avoid the problem of lumpy investment associated with the OP investment proxy[68]. The novelty in this section is that I include the CEO quality as a new input parameter of firm productivity as described in section 1.2. I measure CEO quality as the standardized person fixed effect estimated in the first stage of the estimation for all CEOs, i.e. $\hat{\alpha_i}$[69]. I focus on the years of CEO activity for all CEOs for whom the first stage $\hat{\alpha_i}$ was estimated. Given the rigid nature of most labor contracts in Portugal, I consider labor units and real capital stock as state variables. I consider intermediate materials and services used

[63] I include CEO quality as a state variable since it works as a stock of human capital that influences the productivity process. I borrow this insight from the human capital accumulation literature (Black & Lynch, 1996; Galor & Moav, 2004).

[64] Wooldridge (2009) methodology also takes into account the critiques in Blundell & Bond (2000) and Ackerberg et al. (2006).

[65] Since the pioneer work of Marschak & Andrews (1944), economists have discussed potential correlation between input levels and the unobserved firm-specific productivity shocks (e.g. new technology) in the estimation of production function parameters. The intuition behind this problem is that firms that have a large positive productivity shock may respond using more or better inputs. If this concern is verified, using OLS to estimate production functions would yield biased parameter estimates.

[66] In a first step, authors employ semi-parametric methods to estimate the coefficients on the variable inputs. In a second step, the parameters on capital inputs can be identified under assumptions on the dynamics of the productivity process.

[67] Wooldridge (2009) justifies using GMM for three reasons: (i) avoid the potential problem with identification of variable inputs of the parameters in the LP first stage estimation, (ii) efficiently use the moment conditions implied by the OP and LP assumptions in one step and (iii) directly estimate robust standard errors.

[68] LP show that investment has considerable adjustment costs and therefore is not immediately responsive to productivity shocks. In fact, they argue that in most data sets, a lot of firms will exhibit zero investment in many years for this reason.

[69] The standardized person fixed effects are given by $\frac{\hat{\alpha_i} - \mu_\alpha}{\sigma_\alpha}$ and $\mu_\alpha = \frac{\sum_{CEO} \hat{\alpha}}{n_{CEO}}$.

as variable inputs[70].

GMM model is estimated by imposing two moment conditions on the data. The function $g(x_{jt-1}, m_{jt-1})$, which approximates firm productivity A_{jt}, is estimated non-parametrically by approximating a third-degree polinomial on both x_{jt-1} and m_{jt-1}. See Appendix A.3 for further details regarding the GMM estimation.

Results can be found in Table 1.5. Estimation accounts for sector heterogeneity: services and manufacturing. Standard errors are clustered at the firm level. Results indicate that a one standard deviation increase in CEO quality translates into an approximately 5% increase in firm productivity in the services sector and 4% in manufacturing. I conduct the Sargan-Hansen[71] overidentification test to assess the joint validity of productivity proxy measures. The p-values of the overidentification test are reported in columns 3 and 6 of Table 1.5. In none of the cases can the joint validity of the instruments be rejected at the 1% level. CEO quality, estimated in section 1.4.1, is therefore an important parameter in firm productivity while controlling for other inputs and simultaneity bias.

1.4.6 CEO Quality and Observables

Having established the importance of CEO quality in firm productivity, I turn to answer a second question: do higher-quality CEOs are/behave differently? In other words, are there observable characteristics at the CEO and firm that are positively correlated with CEO quality? I use CEO quality, estimated as employee fixed effects, to compute correlations with CEO and firm observables. I run two types of correlation tests: pairwise regressions of CEO quality on each of the observables of CEO and firm separately, and a post-LASSO regularization regression which performs variable selection and coefficient regularization. The regularization parameter is set to minimize the cross-validation (Tibshirani, 1996).

Figure 1.3 presents the results. Panel A exhibits the pairwise coefficients of a regression where each variable presented is the only regressor and CEO quality estimate is the outcome variable. All variables are standardized to have unit standard deviation. Panel B presents the results of the Least Absolute Shrinkage and Selection Operator (LASSO) regularization procedure to enhance the accuracy. This procedure allows for the selection of the covariates to include in the regression according to a penalty mechanism on the sum of squared errors in the OLS minimization problem[72].

We can conclude from Figure 1.3 that CEO quality is closely associated with several observable variables. First, better firm performance indicators, such as profits, operating revenue and employee value added are associated with higher quality CEOs. These results go in line with the production function estimates in the previous section. Second, higher quality CEOs show a strongly positive correlation with investment in innovation, measured as Research and Development expenditures. This result is consistent with management literature that suggests that more experienced, confident and better able

[70] In Appendix A.2, I further detail how each input is measured.

[71] Sargan (1958) and Hansen (1982).

[72] The LASSO regularization procedure yields the coefficients from the multivariate regression of $\hat{\alpha}_i$ derived from equation (1.5) that are selected according to a penalization of extra covariates on the sum of squared errors. The penalization parameter is estimated with by iteratations set to minimize cross-validated error.

CEOs are better innovators (Barker III & Mueller, 2002; Hirshleifer et al., 2012; Custódio et al., 2017). Third, CEO innate quality is positively associated with higher education, age and experience, both in the firm and as a CEO. These results are also in line with management literature, as well as Bertrand & Schoar (2003). Fourth, family owned and managed firms are less likely to employ a higher quality CEO, which is consistent with the literature of family firms[73]. More graphs of the relationship between CEO quality and observables are shown in the Appendix.

1.5 Robustness Analysis

In this section I present the results of two sets of robustness checks for the reduced form analysis presented in section 1.4. First, I develop a battery of checks to ensure that employee fixed effects before becoming a CEO is a plausible measurement CEO quality. Second, I run alternative production function specifications to validate the results obtained on the role of CEO quality in firm productivity.

1.5.1 CEO Quality Measurement

In the previous section I use non-managerial employee fixed effects to proxy CEO quality. I then use the estimated CEO quality as a productivity augmenting parameter in the production function estimation and find that a one standard deviation increase in CEO quality translates into an increase in sales revenues of approximately 5% for the service sector.

One possible concern with this finding is that it may be capturing variation in ability of other firm employees rather than the CEO. Keeping the first stage estimation equal, I run a placebo regression which randomly picks a firm employee to replace the CEO parameter in the production function, the second stage of the estimation. I use this randomly chosen employee fixed effect. Table 1.7 presents the results. The random employee quality measure is not statistically significant as a productivity input in the firm.

I run a separate estimation in which, rather than focusing on the years before the employee becomes a CEO, I use the whole labor market trajectory of the CEO to estimate their individual fixed effects measure of quality in the first stage of the estimation. According to the findings provided by the event study graphs in Figure 1.2, this means including a significant portion of the CEO's trajectory which appears to present endogenous job mobility. If that is the case, CEO fixed effects should be overestimated[74] and, consequently, so will the coefficient of CEO quality in the second stage regression. Results can be found in Table 1.8. We can observe, as expected, that the estimated role of CEO quality on production is considerably higher than when using a proxy measure. Results indicate that a one standard deviation increase in CEO quality translates into 11%, compared to 5% when using a proxy measure that has been significantly clean of endogeneity.

My findings go in line with the results in two separate strands of literature. When

[73] Pérez-González (2006); Bennedsen et al. (2007); Bandiera et al. (2013).

[74] In the presence of CEO job mobility based on CEO-firm match, part of the CEO-firm specific effects on wage realizations are attributed to the CEO.

compared with papers that use variation coming from a natural experiment, my CEO quality proxy measure estimation yields very similar results both in size and magnitude. As an example, Bennedsen et al. (2007) find a 6% increase in productivity due to a high-quality CEO. This is comparable to the 5% result I get when using a proxy measure of CEO quality.

Another set of papers (Bender et al., 2016) use fixed effects models to estimate CEO quality, using the whole spell of CEOs in the labor market. They estimate that around 13% of the revenue variation can be attributed to the CEO, a figure comparable to the results in Table 1.8.

1.5.2 Production Functions

I estimate alternative specifications for the production function[75]. I use an OLS estimation of equation (1.11) with firm×year fixed effects. On a separate estimation, I relax two important Cobb-Douglas assumptions. The second order translog specification allows for output elasticities to change over time and for input substitutability to be different from 1:

$$q_{jt} = \sum_{k=1}^{5} \beta_k X_{jt}^k + \beta_{kk} X_{jt}^{k^2} + \sum_{l \neq k} \sum_{k} \beta_{lk} X_{jt}^k X_{jt}^l + \epsilon_{jt} \tag{1.13}$$

where q_{jt} represents deflated log of sales for firm j in year t, X_{jt} stands for one of the five input variables[76]. As in the OLS specification, I use the same with firm×year fixed effects.

In Table 1.10, I present the results of both specifications. The Wooldridge (2009) and translog methods generate similar predictions regarding the role of CEO quality in firm productivity. OLS performs a relatively worse in estimating input elasticities.

1.6 Conclusions

In this chapter I present evidence that CEO heterogeneity, or "quality", is important both for the overall determination of variation in firm productivity across firms and for the within firm type productivity variation, given by the CEO-firm complementarity in production. I find that one standard deviation increase in manager ability results in an average 5% in the firm's gross revenue productivity. The relevance of quality of CEOs goes beyond the observable human capital, but is connected to the variables that contribute to human capital, such as schooling and labor market experience. Alongside, higher quality CEOs are more likely to invest in innovation and less likely to work in a family firm.

Despite showing that individual CEOs are relevant for firm productivity, I propose to measure this individual CEO's "quality" in a novel manner to incorporate the following observed facts. I find empirical evidence that the isolated ability of the CEO does not seem to tell the full story as to why CEOs are important. I uncover a different pattern of job-to-job mobility across firms for CEOs as compared to non-managerial employees.

[75] Petrin & Sivadasan (2013).

[76] Labor, capital, materials, services and CEO quality.

Whereas employees' mobility does not seem to be systematically driven by expectations of match-specific quality -a finding this chapter shares with relevant literature-, when analysing job-to-job mobility of CEOs, I find the opposite pattern. As a result of this observation, it is natural that the approach used in the literature[77], which relies on CEO job-to-job mobility to separate the fixed effect of the CEO's ability from that of the firm in revenue, will overestimate the role of the individual CEOs, given that what is attributed solely to them will most likely include an effect of the joint CEO-firm pair - a match-specific effect.

The novel measure I present reduces the overestimation bias that occurs when using CEO job mobility to measure the CEO's effect in firm revenue. I propose using the early years of the CEOs experience as an employee to measure the individual's quality. This method reduces the overestimation bias because it decouples the measurement of quality from the decision to switch firms as a CEO later on in the career. The results I find using this measure corroborate the hypothesis that the impact of CEO's quality in firm revenue productivity is overestimated due to confounding CEO-firm match specific effects.

Given the seemingly important role of the CEO-firm match value in shaping these estimations, I argue that the fixed effects model is not the ideal environment in which to study this question. First, because the fixed effects model does not allow for the consistent estimation of the role of the match. Second, due to the imposition of an additively separable revenue production function. I propose a setting where there is no perfect separability between the CEO and the firm; rather, there is a joint production function. In the following chapter I explore this avenue.

[77] Bertrand & Schoar (2003).

Chapter 2

Better Together? CEO-Firm Match Productivity

2.1 Introduction

The documented dramatic rise in CEO pay (Gabaix & Landier, 2008) in recent years has sparked intense public debate in the literature and media as to how determinant CEOs are for firms' outcomes. There is a significant body of theoretical literature (Rosen, 1981, 1982; Terviö, 2008) postulating that top-managers'[1] skills impact different firms differently and that effective CEO-firm matches can increase firm productivity beyond individual CEO ability. Evaluating the empirical relevance of match-specific firm productivity is therefore a key step to uncover the firm production function and establish the mechanism behind the CEO labor market.

Most of the empirical evidence documenting the impact of CEOs' characteristics on firm's outcomes is at odds with the mentioned theoretical assertions. In particular, existing studies such as Bertrand & Schoar (2003) or Bender et al. (2016) rely two stringent assumptions. First, that the impact of CEO's ability is not match-specific and therefore, CEO's quality is perfectly separable from firm's productivity. Second, that it is possible to establish a universal ranking order of CEOs according to a measurable unidimensional ability. This apparent contradiction may be explained by a more prominent role of the match between the CEO and the firm in the firm's production function. In fact, the literature has thus far paid less attention to the role of complementarities between CEO and firm type in explaining firm productivity[2].

This chapter evaluates the importance of CEO-firm match complementarities in firm productivity, in the presence of non-separable CEO-firm production function. On the first chapter of this dissertation, I use a matched employer-employee data set for the Portuguese labor market to find empirical evidence that contradicts both assumptions that have been regularly used to empirically test the impact of CEO's quality within the firm. Having shown that a unidimensional and fully separable approach to CEO ability is not consistent with the mobility patters found in the data, I now propose to depart

[1] I use the term "CEO" throughout the remainder of the chapter. However, under the CEO label I include any and all head-manager of a firm with more than 20 employees.

[2] There is a large body of literature studying employer-employee complementarities, Best et al. (2017); Eeckhout & Kircher (2016); Gulyas (2016) to name a few of the most recent.

from that setting by means of a non-separable production function where a universal and unidimensional raking of CEOs does not exist. Instead, CEOs are no longer assumed to be equally good at any firm but to match specific firms better than others such that the match output is greater that in other matches. Importantly, assuming a firm revenue production function that is non-separable in CEO and firm contributions adds significant complexity to the exercise of empirically determining the contribution of CEO-firm matches to productivity[3].

There are two challenges to overcome when estimating the value of the CEO-firm match in a panel data setting. First, a simple extension of the previous chapter of using a three-fold fixed effects estimation (with separate effects of CEO, firm and match) would violate the non-separability. Therefore, the natural step of augmenting the Abowd et al. (1999) model with a fixed effect on the CEO-firm match is not consistent and we need a structural framework to model the match formation between the CEO and the firm. Second, to quantitatively identify CEO-firm complementarities in a structural framework, we need to establish a data generating process for the match that is consistent with the non-separable and multidimensional match framework. This will allow for pinning down the assumptions about the CEO-firm match that allow for the identification the match-specific productivity. Third, the low number of CEO job-to-job switches means that there is a limited amount of counterfactual matches across the panel for each firm or each CEO. To address these challenges, I develop a dynamic model to explain CEO-firm match productivity based on the framework put forth in Bonhomme et al. (2017a) and Bonhomme et al. (2017b).

The novelty in this chapter is that I apply their model to a setting of revenue productivity estimation based on CEO-firm matches that assumes the CEO chooses which firm to work for, based on the *ex-ante* perceived match-surplus. There is a finite number of CEO and firm types. Importantly, CEO types are multidimensional and unobserved (latent), but inferred from the mobility choices that are observed for each CEO. To ensure the findings of chapter 1 are taken into consideration, I use a dynamic model that includes a one-period Markov process for job mobility and revenue path dependence, which impose a strategic/forward-looking behavior on the part of CEOs. Moreover, he model is non-parametric in the CEO-firm match, meaning that no specific matching rule is imposed. In turn, match complementarities are left unrestricted and are measured by counterfactual experiments. The advantage of this semi-parametric framework is that we use patters of the observed data to infer about the value CEO-firm match complementarities, rather than impose an *ex-ante* match value to describe the observed matches. In comparison with the fixed effects approach used in chapter 1, this framework presents three improvements. Mobility and revenue are no longer independent from their past realizations, making CEO-firm matches today determinant for mobility choice and revenue tomorrow. Moreover, CEO-firm match surplus is taken to be non-separable and therefore no longer assumed as an idiosyncratic, iid error term.

The analysis of CEO-firm pair in this chapter unfolds in four parts. First, I lay out the building blocks and timing of the dynamic model of CEO-firm joint production. The framework depicts a simple economy where firms need CEOs for production and the CEO chooses which firm to work for. I use a finite mixture of Gaussian distributions to illus-

[3] Pan (2015).

trate the non-separable and multidimensional nature of CEO's contribution to the firm's outputs. Each pair of CEO latent type and firm type yields a different distribution of firm revenue productivity. This setting extends the universal ranking approach setting formerly used in the literature to allow for a non-separable firm revenue production function. As in Bonhomme et al. (2017b), I use a four period framework in which the CEO switches firms from period 2 to 3. I discuss the identification of this model using CEO job mobility.

Second, I start the estimation of this model by assuming that firm types are fixed and observable. I reduce firm heterogeneity to finite number of firm types using the *kmeans* clustering algorithm[4] as proposed in Bonhomme et al. (2017b).

Third, I proceed to estimate CEO types. Given that the identifying assumption relies on CEO job mobility across firm types, the most important challenge lies on the finite mixture model estimation. I use the EM-algorithm[5] to estimate the latent CEO types derived from the mixture model that maximize the likelihood of observing (i) the existing CEO job-to-job switches and (ii) the CEO-firm revenue realizations. After this step, there are three sets of estimated parameters that can characterize, in a simplified manner, the revenue distribution derived from each CEO-firm type match.

Fourth, I implement counterfactual experiments using the estimated parameters to analyze the importance of the CEO-firm match in determining firm revenue production. As a result of this experiment, I find significant CEO-firm complementarities in production, which are stronger for higher ability CEOs. Complementarities account for approximately 2% of average revenues, with a stronger effect (3%) in the top 10^{th} percentile of the revenue distribution.

Understanding the role of top-management is therefore an important step in analyzing productivity gaps in the economy. Moreover, firms dedicate considerable amounts of time and resources to hiring and training their managers[6]. In fact, there is a debate on whether the high wage inequality observed between CEOs and the rest of employees and the recent rise in the CEO pay slice can be attributed to a more prominent role of CEO ability or to rise firm productivity/growth. The latter explanation has gained more traction in the literature[7]; however, it is possible that the match between CEO and firm, seen as complementary inputs, explains part of the rise in firm productivity.

This chapter contributes to the literature of organization economics and corporate governance (Bertrand & Schoar, 2003; Bennedsen et al., 2012; Queiró, 2016). In this chapter, I build on on these groups of studies by documenting evidence regarding the importance of CEO-firm complementarities in production[8].

An active literature in personnel economics, both theoretical and empirical, has focused on establishing a source of observed increasing trajectory in CEO wages. Several papers (Terviö, 2008; Gabaix & Landier, 2008; Chade & Eeckhout, 2013; Gayle et al., 2015) point to a greater importance of firm size growh, as opposed to CEO ability, in pay increases. Simultaneously, other papers (Lazear et al., 2015; Bandiera et al., 2017)

[4] Steinley (2006).

[5] Dempster et al. (1977).

[6] A survey of 610 CEOs by Harvard Business School estimates that typical mid-level managers require 6.2 months to reach their break-even point, and higher for a top-level manager.

[7] Gabaix & Landier (2008); Terviö (2008); Malmendier & Tate (2009); Bebchuk et al. (2011).

[8] This issue is addressed also in a recent paper by Bandiera et al. (2017) who develop structural CEO-firm match model.

document the relevance of mid-level and top-level managers in firm productivity, and still others(Custódio et al., 2013) find that higher managerial ability is linked to higher CEO pay. I contribute to this discussion by explicitly analyzing the role of the CEO-firm match complementarities in the firm's productivity, beyond their isolated contributions. I find evidence that a non-negligible part firm revenue productivity can be attributed to the complementarities between the CEO and the firm. This conclusion can be seen as a bridge between the two mentioned findings in this literature.

This chapter also fits in the broader literature in search and match within labor economics. This literature deals with the challenge of analyzing wage dispersion using structural models that underpin the matching process of worker and firm (Postel-Vinay & Robin, 2002; Bagger et al., 2014; Hagedorn et al., 2017). The distributional method I use, put forth by Bonhomme et al. (2017a,b), attempts to bridge the gap between structural and reduced-form approaches by estimating the role of the worker-firm pair in and importance match-specific complementarities in wage setting. I use their environment to evaluate the role CEO-firm complementarities, under endogenous CEO mobility, in firm productivity.

The remainder of this chapter is organized as follows. Section 2.2 details the dynamic CEO-firm framework for the empirical analysis. Section 2.3 briefly describes the data used. Section 2.4 describes the identification strategy. Section 2.5 documents the estimation of firm classes and the finite mixture model and performs counterfactual experiments on the estimated parameters. Section 2.6 concludes and sets avenues for future research.

2.2 Dynamic Framework

In this chapter, as described in section 2.1, I depart from the additively separable model tested in chapter 1. In so doing, I take into account the empirical findings of said chapter to propose a framework in which CEO-firm matches are non-separable within the firm production function. Figures 2.1 and 2.2 portray the change in paradigm from chapter 1 to chapter 2.

I use an adapted version of the framework proposed by Bonhomme et al. (2017b) (henceforth, BLM). Consider an economy with N CEOs and J firms; j_{it} is the identifier of the firm j where CEO i works at time t. Job mobility is denoted by the identifier m_{it}, which is equal to 1 if the CEO switches firms from time t to time $t+1$ and 0 otherwise. Firm heterogeneity, instead of the individual fixed effects format as before, is now characterized by firm class k. The support of firm class is discrete and finite, $k_{it} = k(j_{it})$ and $k_{it} \in \{1, ...K\}$. CEO heterogeneity is also discrete and finite: α_i represents the latent type of the CEO which will be represented as a random effect. There is a stream of firm revenue realizations, Y_{it} from $t = (0, ..., T)$ that is realized for each CEO-firm pair, and a stream of inputs represented by a vector $\boldsymbol{X_{it}}$.

I focus on a dynamic model[9] in which both job mobility and employee's earnings

[9]BLM discuss a static and a dynamic version of the model. Given that the static model is equivalent to the fixed effects approach used in chapter 1 in terms of its assumptions, I focus on BLM's dynamic model as it provides a relaxation of the assumptions of no endogeneity in mobility.

exhibit a specific type of serial correlation. For simplicity, the dynamic model has 4 periods[10]. The CEO moves from firm k to *k'* between *t=2* and *t=3*. The employee stays in the same k firm class between *t=1* and *t=2*, and then again in firm class *k'* between *t=3* and *t=4*. The timing of the model is illustrated in Figure 2.3.

Assumptions *(dynamic model)*

1. Job mobility depends on CEO type α and firm class k and *k'*, but also on match specific revenue realizations Y_{i2}. However, it cannot depend on Y_{i1}.

2. Revenues $Y_{i,t+1}$ depend on the former period revenues realization, Y_{it} but not on $Y_{i,t-1}$.

The assumption detailed above[11] represents two first-order Markov conditions on job mobility and wages. To illustrate the dynamic model in an interactive setting that is comparable with the model used in section 1.4, consider:

$$y_{it} = \rho_t y_{i,t-1} + a_{1t}(k_{i,t-1}) + a_{2t}(k_{it}) + b_t(k_{it})\alpha_i + \boldsymbol{X_{kt}c_t} + \varepsilon_{jt} \tag{2.1}$$

where ρ_t is the persistence parameter on one-period revenues resulting from the Markov process assumption, $b_t(k_{it})$ is the complementarity between CEO and firm. Although equation (2.1) is not estimated in this chapter, it illustrates the linear equivalent of the non-parametric framework.

I assume the revenue productivity process observed in the economy is described by a Gaussian mixture model. As a result of the dynamic model assumptions, we get following the bivariate cumulative distribution function of log-revenues[12] for periods 1 and 4 (Y_{i1} and Y_{i4}), when the CEO moves from firm k to *k'* between periods 2 and 3:

$$\begin{aligned} Pr[Y_{i1} \leq y_1, Y_{i4} \leq y_4 | Y_{i2} = y_2, Y_{i3} = y_3, k_{i1} = k_{i2} = k, k_{i3} = k_{i4} = k', m_{i1} = m_{i3} = 0, m_{i2} = 1] \\ = \int_\alpha G_{y_2,k,\alpha}(y_1) G_{y_3,k',\alpha}(y_4) p_{y_2 y_3, kk'}(\alpha) d\alpha \end{aligned} \tag{2.2}$$

where α is the set of (finite) parameters that account for L CEO types. Y_{i1} is the revenue realization for CEO i in firm k in period 1, which is independent from the revenue realization in Y_{i3} and Y_{i4}, as well as future mobility, conditional on Y_{i2} and k. Similarly, Y_{i4} is independent from past mobility and revenue realizations, conditional on Y_{i3} and k'.

Equation (2.2) is made up of three terms. First, $G_{y_2,k,\alpha}(y_1)$ is the cumulative distribution function of log-revenues in period 1, in firm class k, for CEO of type α who does not change firm between periods 1 and 2 and realizes y_2 in period 2. Second, $G_{y_3,k',\alpha}(y_4)$ represents the cumulative distribution function of log-revenues in period 4, in firm class

[10]The 4 periods are the minimum necessary for the Markov processes for job mobility and firm revenues described in Appendix B.1.

[11]A formal representation of these assumptions is included in Appendix B.1.

[12]I use deflated log of firm revenues.

k', for CEO of type α who does not switch firms between periods 3 and 4 and realizes y_4 in period 4. Finally, $p_{y_2 y_3, kk'}(\alpha)$ is the probability distribution of CEO types who move from k to k' between periods 2 and 3[13].

Under suitable identification conditions[14] and for known k and k'[15], equation (2.2) allows for the consistent estimation of two sets of parameters, CEO types α and job transition probabilities $p_{kk'}(\alpha)$, from the population of CEOs who move from (k, y_2) to (k', y_3).

To characterize the cross-period revenue distribution, the only missing set of parameters is the initial distribution of types. The proportion $q_k(\alpha)$ of each type α_l in the first period can be estimated through equation (2.3):

$$Pr[Y_{i1} \leq y_1, Y_{i2} \leq y_2 | k_{i1} = k_{i2} = k, m_{i1} = 0] = \int_\alpha G_{y_2,k,\alpha}(y_1) F_{k,\alpha}(y_2) q_k(\alpha) d\alpha \tag{2.3}$$

where $G_{y_2,k,\alpha}(y_1)$ is the cumulative distribution function of log-revenues in period 1, in firm class k, for CEO of type α who does not change firm between periods 1 and 2 and realizes log-revenues y_2 in period 2, $F_{k,\alpha}(y_2)$ is the cumulative distribution function of log-revenues in period 2, for firm k and CEO α and $q_k(\alpha)$ is the probability distribution of α_l for CEOs working in firm class k[16].

Note that equation (2.3) is identified by both CEO job movers and stayers between periods 2 and 3. That is, the estimation of initial CEO type proportions within each firm class k is independent from CEO mobility in later periods.

2.3 Data

As in chapter 1, I use two data sets to generate a matched employer-employee panel. Employee information comes from *Quadros de Pessoal*, a proprietary data set collected and administered by the Portuguese Ministry of Employment, drawing on a compulsory annual employment census of firms[17] that have at least one employee on payroll during the survey reference week[18]. Firm level data is obtained from *Informação Empresarial Simplificada (IES)*, a mandatory annual survey on firm financial information. The two data sets are merged by a common firm identifier. The QP data set has been used in numerous fields of labor economics, namely in the study of gender wage gap[19] and bargaining and unions[20]. The mandatory character of both *Quadros de Pessoal* and ,

[13]Note that this implies that $p_{y_2 y_3, kk'}(\alpha_l)$ is the probability that CEO type is α_l when we observe revenue realizations of y_2 in period 2, y_3 in period 3 and mobility from k to k' between those two periods.

[14]I discuss identification in section 2.4.

[15]Section 2.5.1 explains how k is estimated.

[16]In other words, $q_k(\alpha)$ is the proportion of each CEO type within each firm class at the start of the 4 period dynamic model.

[17]Public administration and informal market services are excluded. Includes private, nonprofit and public firms.

[18]One week of October of each year.

[19]Cardoso et al. (2016).

[20]Card et al. (2015); Addison et al. (2017).

together with reporting based on tax-authority valid profiles[21], lends particular credibility to the data set at hand. Moreover, the QP encompasses the entirety of the Portuguese economic private sector, making its breadth reassuring in providing a safe ground on which to run meaningful empirical analyses. The resulting data set is the same as the one used in chapter 1, section 1.3[22].

The dynamic model described in section 2.2 contains 4 periods, during which time the CEO switches from firm k to k' from period 2 to 3. There are two options of data selection to implement this 4-period design. Given that the unit of time is years, one way is to focus on a specific calendar time window of 4 years and use all mid-term switchers to identify equation (2.2). Alternatively, it is possible to stack all job switches observed throughout the panel data so that the switch coincides with the transition from the model's period 2 to period 3[23]. I choose the latter option because it significantly increases the number of available CEO job switches, necessary to identify equation (2.2).

2.4 Identification

Identification in this model relies, as in Bonhomme et al. (2017b), on job mobility. They show that the key condition for identification of the model described in 2.2 is to fully exploit revenue information before and after a job move. That is, comparing differences in log-revenues between two different types of CEOs that move from k to k' is informative about the effects of CEO heterogeneity in the two firm classes.

In a dynamic setting with CEO-firm complementarities, graph connectedness as described in chapter 1, section 1.4 is a necessary but not sufficient to ensure identification. Similarly to the previous chapter, we need the sample used for estimating equations (2.2) and (2.3) to belong to a connected set of firm classes k linked together by CEOs' job switches across k. Further, we also need an extra condition of sufficient variation in the latent types of CEO switchers between different firm classes[24]. In other words, we need every firm class k to contain CEO job switchers of all types. As mentioned in section 2.2, equation (2.2) is identified from the group of CEO movers, whereas equation (2.3) is identified using both movers and stayers.

Identification follows the same steps as the estimation. First, a dimension reduction *kmeans* algorithm is used to classify firms into a finite number of clusters according to firm distribution of log of revenues. A formal discussion of identification of grouped fixed effects is presented in Bonhomme & Manresa (2015). Second, in the dynamic model, 4 periods are needed for identification, in which only one movement is contemplated (between *t=2* and *t=3*). Maximum likelihood estimation is used to estimate density of log

[21] All employee salaries and employer sales revenues are reported as they are declared to the Portuguese Tax Authority (*Direcção Geral dos Impostos*) and Social Security.

[22] Please refer to this section for more details on sample selection, variables included and descriptive statistics.

[23] In practice, that for instance CEO i who moves from firm k to k' from 2008 to 2009. In that case, I would keep the time span 2007 to 2010 and call those period 1 and period 4. Importantly, I make sure that there are the CEO does not switch more than once in consecutive periods. Note that this option is consistent with all arrumptions of the model, given that CEOs are forward-looking in a simple one-period Markov process.

[24] In particular, complementarities would not be allowed if there were random assignment of CEOs to firms.

of revenues distribution, latent CEO types α_i with $i = \{1, ..., L\}$ and transition probabilities $p_{y_2 y_3 k k'}(\alpha)$ for job movers. After having estimated those parameters, job stayers and movers are used to estimate the type proportions within each firm class, $q_k(\alpha)$. Identification of this model is fully discussed in Bonhomme et al. (2017a) and Bonhomme et al. (2017b). An illustrative example is developed in Appendix B.1.3.

2.5 Estimation

There are two-steps in the estimation of the dynamic model described in section 2.2. In the first step, I estimate firm classes k using a *kmeans* algorithm that clusters firms from 1 to J into k discrete classes according to log-revenues per employee. This step allows me to reduce the dimensionality of firm heterogeneity in a manner consistent with the assumption that CEOs are the ones who choose to move across firms, while firms remain static and waiting for the CEO to pick where to work. In the second step, I use maximum likelihood methods to estimate the CEO latent types α and the probabilities of transition between firm classes for each CEO type, $p_{kk'}(\alpha)$. The two steps of this estimation assume that there is a finite support in both firm and CEO heterogeneity, and that the finite support is known to all agents.

2.5.1 Classification

Throughout this model, unobserved firm heterogeneity is assumed to have a finite and known support. Grouping firm heterogeneity into clusters can be accomplished through a machine learning classification problem. Bonhomme et al. (2017b) propose clustering the J firms in the sample into classes of log earnings distribution by solving the a weighted *kmeans*. The *kmeans* algorithm belongs to a class of unsupervised learning algorithms. Unsupervised learning is indicated when the econometrician does not have prior knowledge on which classes to attribute each observation. Unsupervised learning poses the added challenge of estimation the number of points in the support, or number of clusters. There is a large literature attempting the complicated task of estimating the number K. I abstract from this estimation and, as BLM, assume this number is known to all agents. I use the Euclidean distance *kmeans* algorithm as there is evidence that it performs best (Singh et al., 2013).

I adapt this problem to the setting of firm revenues:

$$\min_{k(1),...,k(J);H_1,...,H_k} \quad \sum_{j=1}^{J} n_j \int (\hat{F_j}(y) - H_{k(j)}(y))^2 \tag{2.4}$$

where n_j is the number of CEO[25] in firm j, $\hat{F}_j(y)$ is the empirical cdf of log of revenues for firm j and $H_{k(j)}(y)$ is the cdf of log of revenues of each partition k. The minimization problem is carried out with respect to all possible partitions of the firm data into K classes. I keep classes fixed across the 4 estimation periods. For the *kmeans* algorithm, I use all observations for each CEO-firm pair to establish the class of the firm, regardless

[25] I stack CEO-firm spells on top of one another so that time period $t = 0$ is the event year for all changes; time dimension is taken out by detrending revenues.

of time period. The *kmeans* algorithm procedure is explained in Appendix B.1.

Table 2.1 presents the descriptive statistics at the firm class level. As in Bonhomme et al. (2017b), I use *K=10*, although in robustness checks I expand the number of clusters to 15 and 20. Notice that average salary and average gross revenue are increasing in firm class. Other firm-level observables, such as size and sector composition, also display significant differences across firm classes.

2.5.2 CEO Latent Type Estimation

In the first stage of the estimation, I reduced firm heterogeneity dimensionality to $K = 10$ comparable firm classes. This step allows for a consistent estimation of latent firm heterogeneity while ensuring a higher mobility rate of employees across firm classes k that will be essential for identification in the second stage.

While Bonhomme et al. (2017b) develop estimations for both the linear and finite mixture models, I restrict attention to the latter. This model provides a non-parametric approach to the maximum likelihood construction thus generalizing the approach to a wide array of specifications. A finite mixture model is a convex combination of finite number probability distributions, used for representing the presence of subpopulations within an overall population. In the context of this model, a finite mixture represents a probability distribution of an CEO belonging to any of the L latent types.

Given the assumptions described in section 2.2, I estimate densities of log-revenues and transition probabilities using CEO job movers in the following log-likelihood function (see section 2.2):

$$\sum_{i=1}^{N^m}\sum_{k=1}^{K}\sum_{k'=1}^{K}\mathbf{1}\{\hat{k}_{i2}=k\}\mathbf{1}\{\hat{k}_{i3}=k\}\times ln(\sum_{\alpha=1}^{L}p_{kk'}(\alpha)f^f_{y_2,k\alpha}(Y_{i1})f^m_{kk'\alpha}(Y_{i2},Y_{i3})f^f_{y_3,k'\alpha}(Y_{i4})) \tag{2.5}$$

The log-likelihood derives from the Markov process assumptions. Regarding f^f and f^b, we know that Y_{i4} is independent of past mobility and firm classes conditional on log-revenues[26] $Y_{i3}, k_{i4} = k_{i3} = k', m_{i3} = 0$. Y_{i1} is independent of future mobility and firm classes conditional on $Y_{i2}, k_{i1} = k_{i2} = k, m_{i1} = 0$. As for f^m, we know that firm revenues for job movers depend on the first lag of log of revenues, therefore the densities for movers are bivariate.

There are two objects of interest to extract from equation (2.5): the latent CEO types and the transition probabilities of each CEO type, from each k to k' trajectory. These two objects of interest shape the parameters (means and covariances) of the distributions $p_{kk'}(\alpha)$, $f^f_{y_2,k\alpha}(Y_{i1})$, $f^m_{kk'\alpha}(Y_{i2},Y_{i3})$ and $f^f_{y_3,k'\alpha}(Y_{i4})$. In keeping with the dynamic model described in section 2.2, I assume that each CEO latent type is a Gaussian generative model, i.e. belongs to a different Gaussian distribution, but the parameters (means and covariances) of the Gaussians are unknown.

I estimate this equation using the Expectation-Maximization (EM) algorithm (Dempster et al., 1977) to estimate. The EM-algorithm is an iterative methodology that allows

[26]For the purposes of the estimation in this chapter, I use log-revenues net of input expenditure (capital, labor and intermediate goods).

for the alternative guessing-optimizing between the two sets of parameters. I assume a set number of Gaussians L, which in the context of my dynamic model is equivalent to saying that the finite support of CEO latent types L is known. With this information, I start by placing L number of Gaussians around random means and variances. This will be the starting point for the iterative algorithm. Two steps are then necessary to estimate the EM-algorithm. First, for each observation y_i, the EM computes the probability of it belonging to either of the randomly placed Gaussians. In mathematical terms, that amounts to computing $Pr[l|y_i]$ using the Bayes' rule for each i. The result for step 1 is that there will be a probability mass point for each CEO type l of belonging to that specific Gaussian distribution. Second, the EM-algorithm readjusts the position (means and covariances) of the Gaussian distributions to maximize the likelihood of the probabilities observed in the former step. I repeat these steps until the algorithm converges.

Estimation of equation (2.5) recovers log-revenues densities for each CEO type α. Moreover, I can pin down the transition probabilities between k and *k'*. These two sets of parameters allow me to characterize the revenues distribution at each period and the CEO-type distribution of k to *k'* for job movers. Figure 2.4 displays the revenue trajectories of the CEO-firm pair for each of the CEO latent types. Each line of the graph represents a different CEO latent type. The x-axis represents the 10 firm classes computed in the previous step and the y-axis has log-revenues (net of capital, labor and intermediate input expenditure). If the match-complementarities were not relevant for revenue productivity, we should observe somewhat flat lines for each CEO-type across the different matches. However, there are distinct peeks and dips, indicating an important role for match-complementarities.

The missing parameters to get a full picture of revenues dynamics are the type distributions for job stayers at the origin; that is, before the job move is realized (*t=2*):

$$\sum_{k=1}^{K}\sum_{k'=1}^{K}\mathbf{1}\{\hat{k}_{i2}=k\}\times ln(\sum_{\alpha=1}^{L}q_k(\alpha)f^f_{y_2,k\alpha}(Y_{i1})f^s_{kk'\alpha}(Y_{i2},Y_{i3})f^f_{y_3,k'\alpha}(Y_{i4})) \tag{2.6}$$

Both equations (2.5) and (2.6) are single-agent correlated random-effects log-likelihood functions. Though identification of k and α is non-parametric in this model, estimation of densities needs a distributional assumption.

Results of the estimation of equation (2.6) can be viewed in Figure 2.5. In this figure we can observe the initial distribution of each CEO latent type (y-axis) within each firm class k (x-axis).

2.5.3 Counterfactual Exercises

As explained in section 2.1, I chose not parameterize the model on the CEO-firm match value. Instead, I use the CEO's mobility to infer the underlying value of the match, therefore not imposing a match rule. Given the absence of empirical evidence in the literature on this topic, this chapter aims to be a first approach to the measurement of the CEO-firm match value. Therefore, avoiding to impose a matching criterion seems more fitting.

After having estimated the underlying parameters of the revenue distribution in the previous section, the last step is a quantitative measurement of the value of the CEO-firm match in log-revenues. The estimation of equations (2.4), (2.5) and (2.6) yields the structural parameters of the dynamic model described in section 2.2: k, α, $p_{kk'}(\alpha)$. Is is then possible to execute some counterfactual exercises.

I run a counterfactual experiment to explore the role of CEO-firm complementarities in revenue productivity. In this counterfactual experiment, my goal is to artificially break complementarities and analyze what would have happened to revenues. First, I randomly reassign CEOs to firms. I then simulate the distribution of firm production assuming that the log-revenues distribution conditional on CEO type and firm class are not affected by the reassignment[27]. Note that, if CEO and firm allocation is random, then the term $b_t(k_{it})\alpha_i$ in equation (2.1) is zero (no complementarities). In essence, CEO and firm random assignment is an artificial way to set complementarities to zero and therefore evaluate the role of complementarities by computing the difference in mean productivity and other moments:

$$E[Y_i] - E^{cf}[Y_i] = E[b(k_i)\alpha_i] = cov(b(k_i), E[\alpha|k_i]) \tag{2.7}$$

where $E^{cf}[Y_i]$ stands for the expected log-revenues in the counterfactual environment. If complementarities $b(k_i)$ are correlated with the type distribution with firm classes, equation (2.7) is positive and therefore there is a relationship between CEO type and complementarities that will not be negligible in the data simulations.

The expected change in average productivity is -2%; that is, on average, complementarities increase productivity in about 2%. However, the difference in the top 10th percentile of the distribution (90th percentile) suffers a larger change in productivity on account of artificially eliminating the complementarities: around 3% of productivity is attributable to CEO-firm complementarities. Results can be found in Table 2.2. Standard errors are estimated via bootstrapping 1,000,000 times.

2.5.4 Discussion

I use Bonhomme et al. (2017a,b) framework to analyze CEO-firm complementarities in firm production[28]. I find the BLM model presents a very innovative approach for the estimation of two-sided heterogeneity models and offers significant advantages in my data setting. First, it is a flexible model as it allows for a non-parametric estimation of heterogeneity types as well as unrestricted complementarities between CEO and firm. Second, it provides an easily generalizable model to a variety of settings. Third, it fits the matched employer-employee/CEO setting very well and is thus replicable in other matched panels, which are becoming increasingly available to econometricians. Fourth, it takes advantage of the whole information on revenue realizations and CEO mobility without the need to rely on large panel data sets.

[27] This counterfactual exercise abstracts from equilibrium conditions.

[28] Other important models, such as Arellano & Bond (1991) provide a generalization of the reduced-form approach to include path dependence in wages. The more recent Hagedorn et al. (2017) or Abowd et al. (2017) use structural (former) or bayesian (latter) approaches to explain CEO-firm match complementarities and sorting. Further discussion on the connection between reduced-form and structural approaches in the context of two-sided heterogeneity models can be found in Appendix B.1.

This model has some limitations. While a purely fixed effects model is not parsimonious and opens estimation to a number of challenges, not least incidental parameter bias, the use of random effects introduces a potential error of specification by imposing restrictions on heterogeneity. In the case of this model, random effects leaves way for unrestricted CEO-firm complementarities, but restricts heterogeneity to a small finite support. This may On a related issue, the number of points in the support of both firm and CEO heterogeneity is a difficult issue that has received much attention in the literature (Kasahara & Shimotsu, 2014), but for which there is no consensus of easy solution. Finally, while the clustering algorithm allows for an ingenious treatment of heterogeneity through dimension reduction, it relies on a potentially strong assumption that one can perfectly separate firm observations. Rather, one can think that it is plausible that firm heterogeneity classes are more fluid and behave as probabilistic distributions over types, as assumed for the CEOs.

Overall, I believe this model has significant traction in the data setting at hand and points to plausible conclusions regarding CEO-firm complementarities in production, maintaining a fair amount of degrees of freedom in the model specification and parsimony. The two-step approach significantly reduces computation burden while providing consistent estimates for CEO and firm heterogeneity.

2.6 Conclusions

In this chapter I present a dynamic structural model that attempts to describe, in a simplified manner, the process by which CEOs and firms produce revenues and the value of the match in that production. I show evidence that match-specific complementarities are significant in determining CEO job-to-job mobility and firm productivity. In fact, a counterfactual experiment estimates that complementarities explain between 2% and 3% of productivity differentials. Strikingly, if we take the estimation of approximately 5% of CEO impact on firm productivity uncovered in the chapter 1, match complementarities would amount to about half of the impact of the CEO on firm productivity.

These findings add meaningful implications both to the literature in organizational, personnel economics and corporate governance literature. First, the results point to a sizeable magnitude for the role of the CEO-firm match in firm productivity. These findings go in line with the results of chapter 1 that suggest that not addressing CEO-firm match complementarities may hide the full picture the impact of an individual CEO ability in firm performance. This means that improving establishment productivity must take into account, apart from the the role of the CEO and the observable variables that are connected CEO quality, to aim for the correct of CEO-firm match.

Better knowledge of the impact of CEO quality for firm performance has important policy implications. First, the findings in this chapter suggest that firms stand to gain from setting out clear profile guidelines when recruiting top-managers that prioritize the fostering of match complementarities between the CEO and the firm[29]. Second, my find-

[29] An example could be including, as part of the recruitment requirements, explicit soft skill analysis of complementarity between the managerial style and abilities and the firm managerial policies. In fact, one can observe this a tend in recent years, in particular for very large firms, such as Google, Amazon or the like. The results in this chapter suggest that smaller firms also stand to benefit from this practice.

ings can potentially contribute to shed more light on the debate regarding the size and recent increase in wage inequality[30] and the size and increase in CEO pay[31]. It seems that, while there is ample evidence in the literature that the rise in firm size has contributed disproportionately for the rise in CEO wages, I find evidence that a non-negligible part firm revenue productivity can be attributed to the complementarities between the CEO and the firm.

Importantly, the findings of this chapter set important avenues for future research. The evidence regarding the importance of CEO-firm complementarities motivates further research as to what these complementarities entail and the mechanisms behind the formation of the match. In particular, a natural extension of the framework presented in this chapter is to extend the strategic behavior to the realm of the firm. That is, rather than have CEOs choice be the only driver, allow for firms to also be forward looking and choose different CEO types depending on their own timing. Another relevant topic that would require further research is the degree of diffusion of CEOs knowledge, experience or ability after they move to another firm.

[30]See Card et al. (2013); Song et al. (2015).

[31]See Murphy & Zabojnik (2004); Gabaix & Landier (2008); Terviö (2008); Frydman & Saks (2010).

Chapter 3

Matching non-financial incentives with Intrinsic Motivations: Experimental Evidence from the Lab

3.1 Introduction

Understanding the motivations underlying agents' prosocial behavior has been the object of extensive study in several fields of social science. In particular, there is a significant body of theoretical research in which authors have explained the nature of observed prosocial behavior through intrinsic motivations and their interaction with financial rewards (Andreoni, 1990; Bénabou & Tirole, 2003, 2006). However, few studies focus on the design of incentive-compatible mechanisms with the goal of aligning agents' interests within an organization of the private, public or non-profit sectors. Experimental applications in the field of both financial and non-financial incentive design have mostly focused on (i) understanding the role of financial rewards on employee effort elicitation (Bandiera et al., 2007, 2011) and (ii) measuring the impact of incentive design in charitable donations. Although there is little evidence about the impact of incentives on other types of prosocial behavior aside from charitable donation, two important examples should be highlighted. The research conducted by Ashraf et al. (2014) explores the role of financial and non-financial rewards in public service provision and the experiment carried out by Ariely et al. (2009) evaluates elicitation of prosocial behavioral in response to image incentives in the laboratory and the field.

This chapter provides evidence on the role of non-financial incentives in nonprofit organizations through volunteer behavior. The nonprofit organization setting is chosen under the assertion that a volunteer in this type of entity is, by definition, motivated by non-financial rewards derived from her work. To the best of my knowledge, this is the first paper to experimentally test incentive mechanisms in settings in which no financial compensation is possible. Despite the choice of this setting, the results observed are generalizable to any work environment.

I implement a lab experiment that tests the impact of using non-financial incentives to differentially affect volunteers' performance. Moreover, I also design an incentive compatible mechanism to extract participants preferences over the set of incentives offered, which allows me to compare the effectiveness of non-financial incentives according to the initial preferences of the incentives or initial motivations for volunteering. I include a broad list of six motivations to volunteer, which are not mutually exclusive. That is, an individual may care about more than one of these groups of motives when making the decision to volunteer for a nonprofit and how much effort to put in. However, I assume throughout the chapter that there is one most important motivation (preferred motivation) for each individual. The six mentioned motivations were deemed relevant in the context of prosociality by the economic literature (reputation, self-image and altruism), and psychological research on the topic (career function, skills and personal growth, social function). I further divide them[1] into three large groups: image concerns (Reputation and Warm-glow motivations), tangible concerns (Career and Enjoyment motivations) and identity concerns (Identification with cause and Peer Effects). They are as follows. The Reputation Motivation stands for the will to volunteer because of its effect on building a positive reputation towards others. Apart from the theoretical model of Bénabou & Tirole (2006) mentioned above, this particular motivation has been tested experimentally in the lab by Ariely et al. (2009), who find that time donations depend critically on the visibility of the subject. The Warm-glow Motivation encompasses the need to feel positively about oneself, that is, by this motivation, volunteers derive utility from helping others. This motivation was put forth by Andreoni (1990) and tested experimentally by Tonin & Vlassopoulos (2013), who also find a significant effect of self-image on giving. Cappellari et al. (2011) resort to econometric analysis on a dataset of volunteers in Italy to investigate the importance of "warm glow" and reputation concerns on donations of both time/effort and money. They find that the two motives are significant in predicting both types of donations and there is a positive correlation between the influences of each motive on each donation type. Lastly, Carpenter & Myers (2010) study motivations for volunteer contribution. They use a dataset on volunteer firefighters which include information on altruism. The authors find that altruism is key for choosing to join the force and also for doing it in an efficient way (i.e. going to trainings, committing to the cause). The Career Motivation means that the decision to volunteer derives from a particular benefit that can be reaped and which is valuable for the volunteer (e.g. work experience). Bromnick et al. (2012) resort to questionnaire analysis to identify two classes of motives, the self-focused and the other-focused[2]. The Enjoyment Motivation implies the activity or environment in which the volunteering task takes place is the fuel motive for volunteering. Mojza et al. (2011) find that volunteering is considered by some volunteers as valuable leisure time. The Identification Motivation implies volunteering because of feeling identified with the cause, that is, the matching between own values and goals of the institution. Clary & Snyder (1999) use a survey to identify six different functions of volunteering[3], namely the "Values function" as that whereby subjects identify with the core mission of the nonprofit, and find that volunteers who receive benefits according to their motivations were happier with the experience. Finally, the Peer Pressure Motivation captures the effect of other people's decision to volunteer influences on the

[1]The names given to each category of motivation to volunteer are not standard, but a choice for the present study.

[2]Self-focused: personal rewards, employability, skills and personal growth; Other focused: belonging, helping, generativity and feeling valued.

[3]Values function, Understanding function, Career function, Social function, Protective function and Esteem function.

individual decision to do the same. DellaVigna et al. (2012) conduct a field experiment (door-to-door fund-raiser) and find that social pressure is a determinant of charitable giving. Meer (2011) uses an econometric approach to analyze the effect of peer pressure on charitable giving, which appears to be positive and fairly strong. Furthermore, Linardi & McConnell (2011) deal with the effect of social pressure on volunteering in a field experimental setting. They find that subjects work more when paired with peers and are also more likely to keep volunteering if the others do. This suggests that social pressure is an important factor in volunteer decisions.

I test the effect of providing conditions, which aim to match each of the motivations described) to incentivize volunteers by comparing the productivity and volunteering effort of subjects while working under those different motivations/conditions. The comparison is enabled by a random mechanism which ensures that some subjects are assigned a condition which corresponds to their preferred motivation, while others are assigned a condition which is their second preferred one, and so forth. The experiment unfolds in two different rounds, each comprised of three shots of the same real effort task. The rounds are different in that the condition (or non-financial incentive) given to the subject is different and, more importantly, assigned in a different manner. In the first round, subjects are randomized into one condition; whereas in the second round subjects rank a list of motivations according to their preference, from most preferred to least preferred, then a lottery randomly assigns a non-financial condition to each subject, which may or may not correspond to the motivation she has declared as his preferred one (that is, the one on the top position on the individual ranking). However, the subject is informed that the lottery is biased towards his preferred condition. By allowing subjects the choice of their ranking and having it influence the lottery, subjects are provided with incentives to truthfully reveal their main preference versus the others, while still allowing for different motives to be important for them. This is another contribution of this chapter, in the sense that it establishes a more realistic setting where volunteers care about more than one motivation. Further, my approach considers actual choices of conditions as a means to unveil subjects' main motivations, contrarily assuming the existence of one motivation.

Apart from evaluating the productivity of each subject in the different shots of the task, I also ask the subjects, once under each of the conditions, what percentage of the effort of one task they wish to donate to a nonprofit. The assignment of different conditions generates different treatments: even though the subject's ranking matters for the lottery outcome, in truth the assignment of conditions is not directly chosen by the subject. The lottery is therefore a source of randomization, whereby some subjects are assigned their preferred condition, while others are not. Thus, it is possible to assess variation in outcomes across these two different groups. That is, how donation and productivity of subjects differ if they are incentivized in their preferred motivation, as opposed to one which was ranked in any other position.

Results indicate that, *per se*, incentives that are connected with identity concerns are relatively more effective not only at fostering donations (both in amount and likelihood to donate), but also at raising productivity of the volunteer. On the other hand, incentives related to tangible concerns (namely the career motivation) seem to deter productivity as well as the likelihood to donate. Moreover, I find that the likelihood to contribute with time/effort to a nonprofit, is greater when the subject is assigned to the condition which corresponds to her motivation (exact matching).

The remainder of this chapter is organized as follows. Section 3.2 briefly discusses some figures about the nonprofit sector. Section 3.3 describes the experimental design. Section 3.4 analyzes the results of the experiment. Section 3.5 concludes and sets avenues for future research.

3.2 Background: Nonprofit Sector

Volunteer work is a crucial foundation of the non-profit sector. There has been a continuous increase in the number and economic worth of organizations in the nonprofit sector over the last decade. The nonprofit sector's estimated worth in the US was $906 Billion as of 2015, which represented around 5.4% of the country's GDP[4]. At the same time, charitable giving is fixed around 2% of the GDP[5] in the US for the past 10 years, despite the distinctive growth in the number of charities and fundraisers and attempts to encourage greater giving. Moreover, in the USA, people donated less of their time to help charities (i.e. volunteering) in 2014 as compared to 2013 (1.1% less[6]). The volunteer rate in 2014 was the lowest it has been since 2002. Nonetheless, in the context of nonprofit organizations, volunteers are an especially relevant workforce, accounting for around 45% of the full-time equivalent workforce in nonprofit sector, averaging 36 countries[7].

3.3 Experimental Design

For this experiment[8], university students were hired through e-mail announcements to perform a real effort task at the experimental lab (LeeX) at Universitat Pompeu Fabra (UPF)[9]. To better mimic a real-life setting of volunteering, I recruited subjects while letting them know they will participate in an experiment in which they can donate some of the proceeds to a nonprofit. The purpose of this message was to target subjects who are naturally more prone to giving/volunteering to a nonprofit.

The experiment consisted of a real effort task, programmed and conducted with the experiment software z-Tree (Fischbacher, 2007). In this task, participants could donate part of the proceeds of the task to a predetermined nonprofit. The noonprofit is *Médecins Sans Frontiers* (MSF)[10]. The task was the so-called "slider task", based on the code developed by Gill & Prowse (2012), whereby the subjects were required to correctly place the pointer of each slider in the demanded position (number on the left). Thus, both numbers on the left and right of the slider must match in order for the slider to be complete. Figure 3.1 illustrates an example of the slider task. The choice of the slider task for this experiment was based on the fulfillment of a set of desirable properties: it

[4]National Center for Charitable Service (The Urban Institute), USA.

[5]Science of Philanthropy Initiative, USA.

[6]U.S. Bureau of Labor Statistics.

[7]Johns Hopkins Comparative Nonprofit Sector Project, USA.

[8]I acknowledge funding for this experiment from professor Pedro Rey-Biel.

[9]I thank the manager of LeeX, Pablo López Aguilar, for his help in logistics and implementation of this experiment.

[10]The nonprofit MSF was chosen because it is widely known by most of the population. Moreover, it benefits from a sterling reputation and its cause gathers a unanimous sympathy among people.

entails a costly effort (tedious task); it translates into a clear productivity measure (effort=output); there are no significant productivity differences among subjects (repetitive task, not skill related); finally, the task is easy to communicate and understand.

The rationale behind this experiment is to test the impact of different non-financial incentives on volunteering, measured in two outcome variables: productivity of the slider task and time donated to a nonprofit. As described above, I measure productivity of the volunteer directly through her outcome in the slider task, while the percentage donated attempts at assessing retention (through letting participants elicit the level of time donation). These two variables were be tested in six different shots of the slider task, carried out within the same session of the experiment. The non-financial incentives will henceforth be referred to as "experimental conditions", and they attempt to match the motivations for volunteering as described in the previous section. Nine experimental sessions were run, of which two were control sessions and the other seven were treatment sessions, distinguished among each other by a unique and randomized experimental condition.

Subjects performed the slider task six times (shots), in two separate rounds, whereby each round comprises three shots of the task. Each shot of the task lasted for 6 minutes, which translates into four screens, each filled with 30 sliders for the subjects to try to complete in at most 90 seconds. The two rounds differ in the way subjects are assigned into a condition. In the first round, subjects were randomly assigned into either one of seven treatments (each corresponding to an experimental condition) or the control group (no experimental conditions given). On the other hand, in the second round, subjects were asked to rank the five conditions according to their personal preference. Taking this ranking into account, a lottery assigned a new experimental condition to the subject, which replaced the randomized condition of the first round.

At the end of the experiment, a lottery picked only two shots, both belonging to either one of the two rounds with equal likelihood. The goal of this equal probability lottery is to allow us to gather information on productivity and time donation for all subjects, while at the same time making it incentive compatible for subjects to donate part of their earnings in the second shot of each of the two rounds, as they know it will not be a second donation. Each subject was paid a show-up fee of €2 and earned a piece-rate per completed slider of €0.05. The piece-rate reward reverted either to the subjects themselves, or partly to a nonprofit, depending upon which form of payment the final lottery picks up. After the experiment is concluded, participants filled out a questionnaire which included questions on demographics and volunteer activities (see Appendix C.2).

3.3.1 Timeline and treatments

Figure 3.2 illustrates the timeline of the lab experiment. Subjects were randomly assigned with equal likelihood to one (and only one) condition throughout the experiment, which was their incentive/reward for volunteering their effort to a nonprofit. This condition will be binding for payment only if the final lottery picks this round. The condition can be one of the following 7. Moreover, there was also a control group which received no condition in the first round:

1. **Reputation Condition:** the participant received a pen as a token for his participation/ contribution for the nonprofit Médecins Sans Frontiers (MSF). This token

is an external signal which is not easily known in the day-to-day life. This mimics the concern for reputation in the decision to volunteer;

2. **Warm Glow Condition:** at the end of the experiment, the individual knows how his/her contribution helped to a specific project within the nonprofit, by giving him/her a report on MSF's activities as well as a box of candy from the famous campaign "Pills against other people's pain". These two items ensure that subjects who choose this condition do so because they are concerned with their image toward themselves a they want to feel the warm glow of their actions. The box of candy has no external value, it is quickly perishable and can easily be obtained outside of the lab. Thus, it is simply a symbolic way to convey the message that the subject has volunteered to a specific cause. The report gives more detailed information about the difference the subject made in MSF's projects;

3. **Career Condition:** the individual under this condition was given a lump sum amount of money if she contributes to the nonprofit, the same amount as the participation fee. This money was to be spent at any university service, and thus non-transferable to the nonprofit;

4. **Enjoyment Condition:** under this condition the subject was allowed to perform the task while listening to music[11]. The nature of the task does not change, thus it is still comparable with the other conditions, but music makes the task less tedious/ more enjoyable;

5. **Identification Condition:** with this condition, the subject can choose which nonprofit he/she wants to donate to, instead of default one (MSF). By choosing this condition the subject can state his identification with a specific cause;

6. **Peer Pressure Condition (high):** if assigned to this condition, subjects were given information (on the screen) of how much 5 highest levels of donation by previous participants in the experiment. Information will be given on how many sliders they got right, as well as how much they decided to donate in percentage terms. All subjects will be given the same information, before they carry out the task;

7. **Peer Pressure Condition (low):** if assigned to this condition, subjects will be given information (on the screen) of the 5 lowest levels of donation by previous participants in the experiment. Information will be given on how many sliders they got right, as well as how much they decided to donate in percentage terms. All subjects will be given the same information, before they carry out the task.

After knowing their condition, subjects were informed that the first shot is 100% for themselves. They then performed the first shot of the slider task. Once the first shot is over, subjects were asked to choose the percentage of the second shot of the task they wished to donate to the nonprofit (MSF or another one, depending on whether they were previously assigned to condition 5 -identification- or not). They indicated a number between 0 and 100 percent, which went to charity, and the rest accrued to themselves.

[11]Subjects were asked in the e-mail announcement to bring their MP3 players to the lab.

Then, subjects played the second shot. After the second shot, subjects played the task once again (third shot), this time knowing that the whole proceeds of the task went to the nonprofit in question.

Once the third shot is over, the second round begins. Between the first and the second round, participants will be given a new condition, different from the one they were randomly assigned to, which will enter into effect for the second round of the experiment (that is, the fourth, fifth and sixth shots of the task). In order to determine this extra condition, subjects will be asked to establish a personal preference ranking of the five first conditions (from the ones previously described), on a scale from 1 (most preferred) to 5 (least preferred). Note that the Peer Pressure condition is not included in this round, as it is unrealistic for subjects to choose being pressured – it is a collateral, not direct, effect on volunteering.

Once the participant determined his/her own ranking, a biased lottery randomly set the new condition to be assigned the subject. Every subject faced the following probability distribution over their own ranking of conditions (take x to be the assigned condition - which ranges from the one in the first position (1) to the one in the last position (5)), as shown in Table 3.1. The logic of the lottery is to provide a clear incentive for subjects to state their true preferences regarding the condition to be attributed to them, which is why the lottery has a decreasing probability of picking each position of the ranking[12]. The final objective of asking for this ranking is to have information that allows us to compare productivity and percentage donated between individuals who were randomized into the condition they state as their highest preference, and those who were not. This variation is essential to establish the tradeoff in effectiveness between incentivizing the different levels of motivations of potential volunteers.

After the lottery assigned a condition, subjects were again informed that the fourth shot (or first, within the second round) of the task is 100% for themselves. Once the fourth shot is over, subjects were asked to choose the percentage of the fifth shot of the task they wish to donate to the nonprofit (MSF or another one, depending on whether they were now assigned to condition 5 -identification- or not). They indicated a number between 0 and 100 percent, which went to charity, and the rest accrued to themselves. Then, subjects played the fifth shot. After the fifth shot, subjects played the task once last time (six shot), this time knowing that the whole proceeds of the task went to the nonprofit in question.

As a result of the structure of the experiment, round 1 will create 7 treatments, each of them corresponding to the randomized assignment to one of the 7 conditions, plus a control group (no condition). Table 3.2 summarizes the treatment groups.

I was able to gather information on productivity, time elicitation and personal preference for conditions, by using the six shots of the slider task. Note that, for all treatments of the first round, sub-groups of participants were automatically generated for the second round, according to how they were randomly matched to their incentives. In other words, the ranking of incentives given by each subject between the first and second rounds gives us information on whether e subject was randomly assigned to her preferred condition in

[12]It is noteworthy to emphasize that 98% of the subjects were able to describe the gist of the lottery in the control questionnaire after the experiment was finalized; this provides me with a certain confidence level for accepting the subjects' ranking as valid portrayals of their true incentive preferences.

the beginning of the experiment.

The leading behavioral hypothesis that I put forth is that giving individuals a non-financial incentive that matches their motivation to volunteer increases both productivity and time donations. Moreover, I also test whether there is one particular non-financial incentive (or more than one) that is relatively more effective in augmenting the extensive and/or intensive margin of volunteering. In order to test these hypothesis, I use two outcome variables: productivity and time donations. Productivity is directly measurable from the real effort task, and it aims at mimicking the real life productivity of a volunteer. Time donations are the percentage of time of the real effort task that the subject decides to donate to a nonprofit. I use this outcome to simulate the decision to volunteer and for how long (aka retention).

Hypothesis 1 *Different non-financial incentives affect productivity differently.*

I tested this hypothesis by comparing the productivity of the real effort task (in the first shot) between the control group (where no condition is given) and the treatment groups (where there is a condition), as well as the treatment groups between each other.

Hypothesis 2 *The closer the match between motivation and non-financial incentive, the higher the productivity of the subject.*

This hypothesis can be tested by comparing the productivity of the real effort task between the sub-groups on in the first shot (all for the subject, random condition) and the third shot (all for the subject, lottery assignment taking preferences into account), I can test hypothesis 3 that being given a nonfinancial reward that matches the subject's motivation leads to different productivity.

Hypothesis 3 *Different non-financial incentives affect time donations differently.*

This hypothesis can be tested by comparing the choice of how much time to donate to a nonprofit in the second shot between the control group (where no condition is given) and the treatment groups (where there is a condition), as well as the treatment groups between each other.

Hypothesis 4 *The closer the match between motivation and non-financial incentive, the higher the time donation of the subject.*

I tested this hypothesis by comparing the choice of how much time to donate to a nonprofit the second and fifth shots of the real effort task between the sub-groups mentioned, that is, participants who were randomly assigned their preferred condition as opposed to those who were not, I can test hypothesis 4 of whether different time donations are elicited while being matched to a preferred condition.

3.4 Analysis and Results

The descriptive statistics of the data can be found in Table 3.3. I used the comparison of two outcomes across treatments and conditions to derive conclusions regarding the hypothesis to be tested. The outcomes are: average productivity measured as number of correct sliders[13] and average percentage donation.

Table 3.3 displays six sections with descriptive information gathered from the experimental sessions. The first column pertains to the control group (in which no incentive was offered). The columns from the second to the ninth pertain to an average of all treatments and the breakdown per treatment. The last column is an average over all observations. The first two sections of the table describe the averages of productivity, donation in percentage and donation in percentage only for subjects who were matched, in that round, to the condition they stated to prefer. The third section of the table includes demographics, such as percentage of females, volunteers, religious subjects and an average of age.

In the whole sample, the average productivity is in the first round (that is, tasks 1, 2 and 3) is 168 correct sliders; whereas in the second round it is 191 (tasks 4, 5 and 6). There is a 33% of the subjects who are volunteers in their day-to-day life, and 30% of the subjects claim to be religious. Of all subjects who participated in the experiment, 60% are female. Further, the average donations in the first and second rounds are approximately 13% and 10%, respectively.

From analyzing the experimental data, I find three results regarding the hypotheses formalized in the previous section of this study, "Experimental Design".

Result 1 *Subjects are more productive if they can choose which nonprofit to volunteer for. Moreover, productivity decreases with non-conditional financial reward.*

In what concerns the hypothesis 1, I want to test whether different non-financial conditions can induce different productivity levels (as opposed to no change in productivity across conditions). Here, I will focus on the first round, because I want to analyze the behavior of productivity when a random condition is assigned, to see the different impact of incentives *per se*. By taking a look at the raw descriptive data in Table 3.3 I can get a sense that the average productivity varies, in particular for some treatments. Simply by comparing the average productivity in round 1 across the different treatments, I can see that is goes from 160 (Treatment 3, Career Condition) to 189 (Treatment 5, Identification Condition). In order to test his hypothesis, I perform both regression analysis and statistical testing. I started by running an OLS regression of productivity on treatment status, as follows:

$$\begin{aligned} prod_i = \beta_0 + \beta_1 female_i + \beta_2 age_i + \beta_3 religious_i + \beta_4 volunteer_i + \\ \beta_5 match_i + \sum_{t=1}^{7} (\beta_{6c} treatment_{it}) + \varepsilon_i \end{aligned} \tag{3.1}$$

Subscript i stands for individual, whereas t stands for treatment assigned in the first round. *Treatment* stands for treatment status, and it goes from treatment 1 to 7. That is, the control group is the reference category in this regression. I also control for a

[13]Number of correct (or complete) sliders was chosen as measure of productivity versus number of attempted sliders, since the number does not differ for the majority of subjects. Given that each screen was timed, on average only one slider per screen was attempted without completion, which corresponded to the slider which was being completed when the screen changed.

dummy variable, *match*, which takes value 1 if the subject has been randomly matched in the first round to the condition she states as his preferred one in the second round. Lastly, I control for some demographic variables: *female*, if the subject is female; *age* for the age of the subject; *volunteer* if the subject is a volunteer at a nonprofit in their day-to-day life and *religious* if the subject considers himself religious.

Results are shown in Table 3.4, Model 1. I can see that, one analyzing the bulk of the effects against the control group, the only statistically significant different arises in treatment 5, where the average productivity is higher than the control group at a 5% significance level. It seems that, taking the control group as a reference, the only condition that improves the productivity of volunteers is the one where they can choose which nonprofit to donate their time to. Moreover, being a female and being one year older has a negative and statistically significant relationship with productivity, also with a 5% significance.

I also ran two statistical tests to further explore the hypothesis of different levels of productivity. I chose the Kolmogorov-Smirnov test[14], which is a nonparametric test that compares the cumulative distributions of two data sets (e.g. productivity under treatment and productivity under control groups). The second test is the t-test for unpaired data, which analyzes the null hypothesis that the means of two different data sets are equal. These tests allow us to contrast the treatments against each other while getting two different perspectives (distribution and moments).

Through the t-test, all hypotheses of equality of means of the treatments among themselves cannot be rejected, except for, once again, the case of treatment 5 (Identification Condition) against the other treatments. Nonetheless, when using the Kolmogorov-Smirnov test, I reject at a 5% significance level the equality of distributions between treatment 3 (Career Condition) and the rest of the treatments, as the distribution of productivity in treatment 3 is lower than the other treatments. This might hint to the fact that an extra financial incentive (a personal reward) detaches the volunteer and does not incentivize his highest productivity. The effect of higher productivity distribution for treatment 5 is still maintained.

In what concerns hypothesis 2, I want to test whether matching subjects randomly to their preferred condition influences productivity. From the results of the regression specified in equation (3.1), in Table 3.4, I can see that being randomly matched in the first round to the preferred condition does not affect productivity of the first round. Using the same statistical tests as mentioned before, I cannot reject the hypothesis of equal productivity among subjects who are matched to their preferred condition in the first round, and those who are not.

Result 2 *Subjects are more likely to volunteer if they can choose the nonprofit to volunteer for. Non-conditional financial rewards are correlated with reduced donations, both in terms of likelihood of donating and donated time.*

Regarding hypothesis 3, I test whether incentivizing subjects with non-financial conditions can significantly change effort donation behavior. I resort to regression analysis and statistical testing.

Before entering into the detail of the econometric analysis, I provide a brief description of the donation data. On average, subjects donated more in round 1 than in round

[14] Wilcox (2005).

2. However, by performing a *t-test* on the means, it is evident in treatment 5 (Identification Condition) is this difference statistically significant. Furthermore, it seems that the conditions that imply higher donations on average are the Identification, the Career and Warm Glow Conditions, with average donation (in round 1) per treatment of 14.2%, 12.7% and 12.2% respectively, an average donation in round 2 of 11.07%, 13.25% and 10.53%.

I use two regression specifications: the first, for the likelihood of donation (Probit model) and the second, for the (Tobit model). The equations are as follows:

$$Prob[Donate1_i = 1] = \beta_0 + \beta_1 female_i + \beta_2 age_i + \beta_3 religious_i + \beta_4 volunteer_i + \sum_{t=1}^{7}(\beta_{5c} treatment_{it}) + \varepsilon_i \tag{3.2}$$

$$donperc1_i = \beta_1 female_i + \beta_2 age_i + \beta_3 religious_i + \beta_4 volunteer_i + \sum_{t=1}^{7}(\beta_{5c} treatment_{it}) + \varepsilon_i \tag{3.3}$$

As before, subscript i stands for individual, and t indicates treatment (from 1 to 7). Once again, the reference category will be the control group. *Prob[Donate1=1]* is a dichotomous response variable, which computes the likelihood that the subject makes a donation in the first round, controlling for the previously described regressors. Alongside, *Donperc1* stands for the donation, in percentage points, made by the subject i. Results are displayed in Table 3.4, in columns (2) and (3).

Concerning the first specification, I use a Probit model, in column (2). Again, it seems that only two treatments are statistically significant in changing the likelihood of donation. Those are treatment 3 (Career Condition), which has a negative effect on the probability to donate; and treatment 5 (Identification Condition), which has a positive effect. Moreover, a subject who is a volunteer in his day-to-day life is less likely to donate, whereas being women have a slightly higher (but statistically significant) likelihood of donating.

Regarding the amount donated in percentage points, I chose to use a Tobit model. The Tobit model allows us to account for the left-censoring of the data on percentages donated, which derives from the cluster of zeros. The censoring does not permit us to distinguish the different “types” of zeros in the data (those whose utility from donating is just tied with not donating, and those whose utility from donating is lower from not donating), thus biasing the estimates upwards. The Tobit corrects for this problem. Results for this model can be found in Table 3.4, in column (3). Most treatments have a negative impact on the amount donated, despite a positive impact in the likelihood to donate.

Lastly, I used statistical testing to analyze whether donations in different treatments are significantly different. The Kolmogorov-Smirnov test and the t-test both allows to reject, with a 5% significance level, the hypothesis that donations are the same for treatment 3 as compared to other treatments. Therefore, I cannot reject hypothesis 3 that different incentives affect amounts donated differently.

Result 3 *Subjects are more likely to donate and donated more if matched to their preferred conditions for volunteering. Moreover, this correlation is determinant for volunteer retention (through higher likelihood of volunteering).*

Finally, I test hypothesis 4. I analyze donations made in the second round, as in that case subjects are given a new condition, which may or may not match their stated preference. From analyzing the descriptive data, one can notice that, on average, subjects donate more in the second round when they are matched to their preferred condition (11.76% versus 10.41%). I use two regression specifications: the first, for the likelihood of donation (Probit model) and the second, for the amount donated (Tobit model), as I had before. The equations are as follows:

$$\begin{aligned} Prob[Donate2_i = 1] = \beta_0 + \beta_1 female_i + \beta_2 age_i + \beta_3 religious_i + \beta_4 volunteer_i + \\ \sum_{t=1}^{7}(\beta_{5c} treatment_{it}) + \sum_{c=1}^{4}(\beta_{6c} pref_{ic}) + \varepsilon_i \end{aligned} \tag{3.4}$$

$$\begin{aligned} donperc2_i = \beta_1 female_i + \beta_2 age_i + \beta_3 religious_i + \beta_4 volunteer_i + \\ \sum_{t=1}^{7}(\beta_{5c} treatment_{it}) + \sum_{c=1}^{4}(\beta_{6c} pref_{ic}) + \varepsilon_i \end{aligned} \tag{3.5}$$

Donperc2 stands for the donation, in percentage points, made by the subject *i*. *Prob[Donate2=1]* is a dichotomous response variable, which computes the likelihood that the subject makes a donation in the second round. Also, the variables *pref*, from *c=1-4*, are dummy variables which take value 1 if the subject is assigned by the lottery to the condition she ranks in position *i*. That is, if $pref_i$*=1*, then it means the subject was matched with his most preferred condition (the one she ranked in the first position). Results are displayed in Table 3.4, in columns (4) and (5).

The probit results, displayed in column (4), seem to reinforce the idea that treatments 3 and 5 work negatively and positively (respectively) on the likelihood of donating. Being a female still increases the likelihood of donating, and one can observe see a decreasing trend in the impact of being assigned to the preferred condition, versus the second and the third. In fact, by estimating the Probit model using as regressors only the levels of preference with which subjects were matched, significance increases and the effect on likelihood is decreasing all the way, from matching with first to fifth level of preference. On the other hand, the Tobit model results demonstrates the tradeoff in amount donated between providing incentives for each level of motivation does not present a clear downward trend as in the case of the Probit. The Kolmogorov-Smirnov and t-tests state that I can reject the hypothesis (10% significance level) that donations in the second round are the same if the subject is matched to his preferred condition versus the other conditions.

3.5 Conclusions

Motivating volunteers towards donating their effort and time and doing so in a productive way is not straightforward. The extrinsic reward institutions can offer is not financial and often out of their direct control, as it is perceived differently by each volunteer. With the growing importance of the nonprofit sector and its volunteer workforce in the economy, it becomes paramount for institutions to gather a broader and better workforce. In order to do so it is of most relevance to learn more about specific intrinsic motivations so as to meet them with non-financial rewards.

This chapter contributes to this fairly new avenue of research in economics, by finding evidence for a positive answer the question: is it possible to increase volunteer contributions by giving volunteers conditions which match their intrinsic motivation? I present a lab experiment whose goal is to test whether matching the volunteer's intrinsic motivation with extrinsic non-financial rewards (conditions) offered in return for their work can yield a better outcome for nonprofits. That is, if this matching is powerful enough to increase volunteers' productivity and time/effort donations. The lab environment was chosen because of its potential to have subjects perform a real effort task while controlling for the environment they are working on (i.e. controlling for the extrinsic non-financial reward under which they work) and also to provide a clear outcome measure for different treatments. The chosen outcomes were productivity and percentage of the benefits of effort which the subject chose to donate to the nonprofit. The identification strategy relies on the randomization into treatment, and was based on assigning different conditions for the experiment to different subjects. These conditions may or may not match their intrinsic motivation, thus providing us with a basis for comparison of outcome variables.

Donation amounts respond positively to being assigned to the preferred condition, as does the likelihood to contribute a part of the effort. As such, I conclude that matching volunteers' motivations with extrinsic non-financial payoffs plays an important role in gathering contributors. However, the analysis also indicates that, even if subjects are not matched to their preferred condition, some conditions/incentives (namely identification conditions) work better *per se* than others (namely career conditions), not only on increasing donations (extensive margin) and the likelihood to donate but also on the productivity of the work (intensive margin).

An important limitation of this experiment is that subjects are told which condition they were assigned to, and then decide on how much to donate and afterwards perform the task. In reality, often volunteers are in the dark about how their motivations will be fulfilled. In many cases, fulfilment is not even controlled by the institution.

As possible policy implications of this study (which need further investigation), I venture the following. Institutions can benefit from getting to know their volunteers and what motivates them, if they use this information to provide extrinsic rewards which match their profile. That it itself has the potential to increase involvement, and these rewards can be effective if they are objective and known to the volunteer *a priori*. Moreover, incentives that are linked more closely to identification concerns are more effective in gathering and retaining volunteers.

There is room for a more profound analysis regarding the broader topic of the role of nonfinancial incentives in the workplace. Nonetheless, this chapter contributes to the assertion that adjusting incentives directed at the decision to volunteer seems to be powerful enough to significantly change effort elicitation. As such, it is relevant to continue this path and adopt a research agenda which seeks to answer the questions still open as of now.

These conclusions have some limitations, the most important of which being the lack of dynamics in the decision-making process, since the real effort task presented is a one-shot event. Most of the limitations can be addressed in a field experiment. I design a randomized control trial which assigns volunteers working at OXFAM Spain's fair trade shops to four alternative groups, each corresponding to a different incentive scheme. The warm glow incentive group participant is given detailed information about her specific contribution to OXFAM's mission. The pecuniary incentive group is given a prize in the form of a bundle of OXFAM shop's high-end products. Lastly, the social image group is rewarded with a public thank you message on social networks, visible to the participants' relevant social network.

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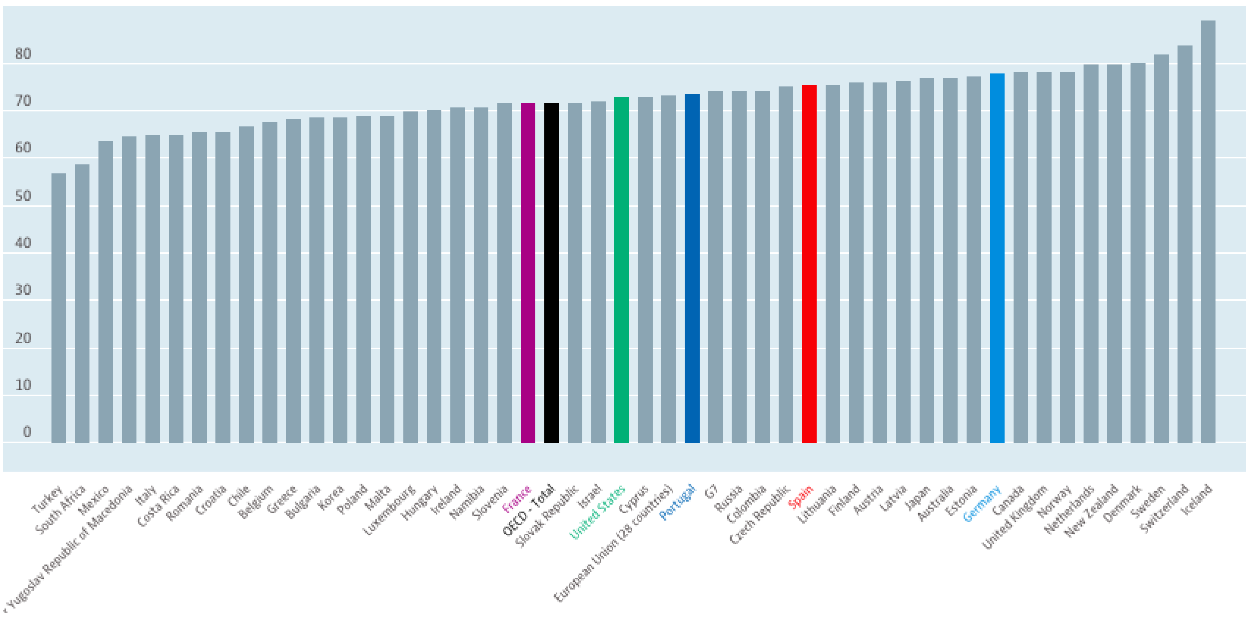
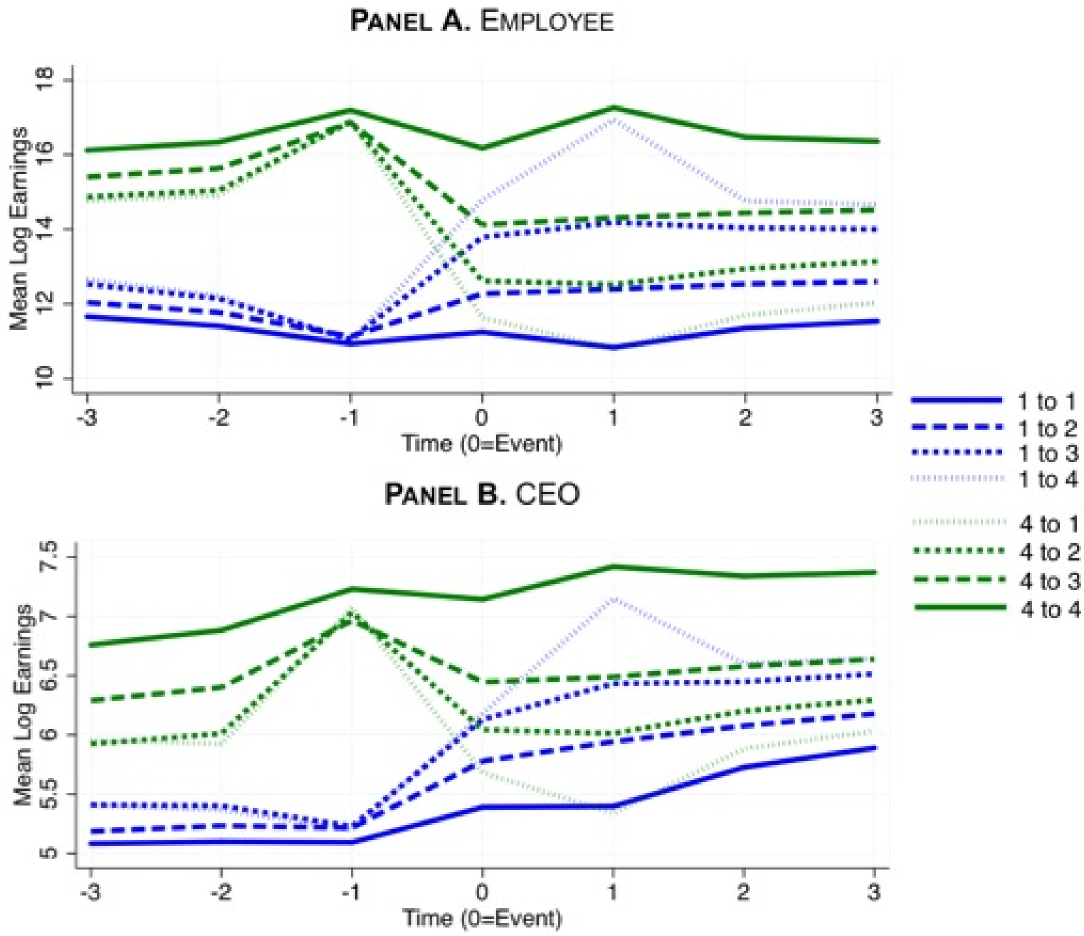
Tables and Figures: Chapter 1

Figure 1.1: Labor Market Participation Rates, OECD.



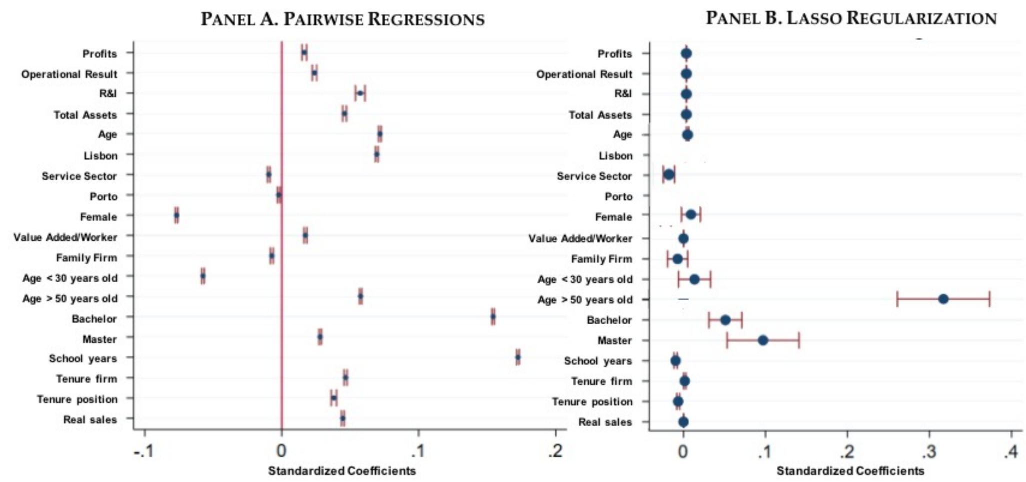
Notes: Figure 1.1 displays the Labor Force Participation (LFP) rates in OECD countries in 2016. The OECD average is presented in black and the Portuguese LFP rate is displayed in navy blue. Portuguese data point is comparable to the EU average and similar to the US participation rate.

Figure 1.2: Event Studies. Employee and CEO Job Transitions.



Notes: Figure 1.2 depicts the average log wages earned before and after a job transition. Panel A illustrates the event study graph for employees switching firms. Panel B depicts CEOs switching firms. Both graphs are plotted under the same procedure. Time *t=0* represents an event; the time corresponding to the first period after an employee (CEO) changes firms. The x-axis represents time periods in relation to the event date. For all events, the firm at which the employee (CEO) works before and after the event is classified into quartiles of coworker wage distributions. As an example, the green solid line represents the average residualized log-earnings of employees (CEO) who move from a firm that belongs to the upper quartile of the coworker earnings distribution to another firm that belongs to the same quartile. When switching to consecutive quartiles, for instance, form 3 to 4 or 1 to 2, the estimated gain/loss of symmetric movements should also be symmetric (on average) in the absence of significantly match-driven mobility (mobility motivated by specific match wage realizations), as argued by Card et al. (2013).

Figure 1.3: Correlations of CEO Quality with Observables.



Notes: Figure 1.3 exhibits two panels. Panel A presents the bivariate regression coefficient of estimated CEO fixed effects, estimated from the regression $y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it}$, on each of the presented variables. Coefficients are standardized and 95% confidence intervals are displayed in red. Panel B shows the result of a Least Absolute Shrinkage and Selection Operator (LASSO) regularization procedure applied over a regression of $\hat{\alpha_i}$ on all covariates presented in the y-axis of Panel A. The LASSO procedure implements a penalty λ for extra covariates in the traditional OLS minimization problem: $\min_\beta \quad \sum_i (y_i - X_i\beta)^2 + \lambda|\beta|$. This results may be that optimal coefficient for some covariates to be zero. The result includes the selection of covariates to include in the model as regularization penalty that minimizes the mean squared error in K-fold cross-validation. Tibshirani (1996) introduces the LASSO method and discuses its properties.

Table 1.1: Descriptive Statistics. CEO and Employees.

	CEO			Employees		
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.
Demographics						
Female (=1)	30.57%	0.46	2,976,326	44.44%	0.48	12,553,863
Age	42.50	11.68	2,976,326	46.78	12.24	12,553,863
Below 30 y.o. (=1)	18.23%	0.38	2,976,326	18.65%	0.39	12,553,863
Between 30 and 50 y.o(=1)	50.99%	0.51	2,976,326	39.03%	0.49	12,553,863
Above 50 y.o. (=1)	30.78%	0.46	2,976,326	42.31%	0.49	12,553,863
Education						
Bachelors degree (=1)	29.78%	0.398	2,976,326	9.51%	0.27	12,553,863
Masters degree (=1)	3.04%	0.101	2,976,326	0.27%	0.05	12,553,863
Tenure, Wages and Job Mobility						
Tenure position	6.17	6.22	2,465,925	5.98	7.79	10,665,774
Log-wages	10.24	0.67	2,180,803	7.58	0.59	7,829,870
Job Mobility	2.15	1.58	2,976,326	3.45	1.49	12,553,863

Notes: Table 1.1 reports summary statistics for two samples. The first three columns correspond to the sample of CEOs (or head managers) of all firms in the *Quadros de Pessoal* data set, detailed in section 1.3.1. The last three columns correspond to the full sample of non-CEO employees, either those who never make it to CEO/head manager or those in the years before becoming a CEO. I present data on demographics, education, salaries and tenure. A detailed account of the construction of each variable can be found in Appendix A.2.

Table 1.2: Descriptive Statistics. Largest Connected Set.

	CEO			Employees		
	(1) Mean	(2) St. Dev.	(3) Obs.	(4) Mean	(5) St. Dev.	(6) Obs.
Demographics						
Female (=1)	25.57%	0.43	402,726	42.47%	0.49	12,302,786
Age	40.33	6.50	402,726	32.29	7.53	12,302,786
Below 30 y.o. (=1)	38.43%	0.38	402,726	23.41%	0.34	12,302,786
Between 30 and 50 y.o(=1)	40.79%	0.51	402,726	56.60%	0.49	12,302,786
Above 50 y.o. (=1)	20.78%	0.46	402,726	20.20%	0.50	12,302,786
Education						
Bachelors degree (=1)	33.78%	0.398	402,726	10.01%	0.27	12,302,786
Masters degree (=1)	7.69%	0.101	402,726	0.51%	0.50	12,302,786
Tenure, Wages and Job Mobility						
Tenure position	5.18	4.55	246,407	5.06	4.81	10,402,459
Log-wages	11.56	0.67	270,884	8.01	0.59	7,673,272
Job Mobility	2.49	1.84	402,726	3.52	1.54	12,302,786

Notes: Table 1.2 reports the same summary statistics as in Table 1.1 for two samples. The first three columns correspond to the largest connected set of the samples considered in Table 1.1. Section 1.4.2 contains an explanation of the largest connected set. A detailed account of the construction of each variable can be found in Appendix A.2.

Table 1.3: Descriptive Statistics. Job to Job Transitions.

	General Managers		**Operational Managers**		**Other High-level**		**Mid-level**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
	#	%	#	%	#	%	#	%	#	%
General Manager	23,981	60.60%	384	1.6%	5,310	2.60%	10,304	0.5%		
Operational Manager	482	1.2%	16,601	70.10%	787	0.4%	6,020	0.3%		
Other High-level Managers	6,483	16.4%	1,664	7.00%	172,246	83.00%	57,534	3%		
Mid-level Managers	8,653	21.9%	5,018	21.20%	29,064	14.00%	1,826,488	96,10%		

Notes: Table 1.3 reports the number of job-to-job transitions at every managerial level across positions in the employee-firm matched panel data set *Quadros de Pessoal*, detailed in section 1.3.1. Each cell reports the number of transitions from the row position to the column position. All transitions are across firms. A transition is identified when an employee changes firm from one survey period to the next.

Table 1.4: Descriptive Statistics - Firm.

	Firm		**Largest Connected Set**	
	(1)	(2)	(3)	(4)
	Mean	St. Dev.	Mean	St. Dev.
Demographics				
Lisbon (=1)	28.90%	0.453	29.70%	0.457
Porto (=1)	21.96%	0.414	24.09%	0.428
Manufacturing (=1)	36.64%	0.482	44.15%	0.496
Construction Sector (=1)	14.38%	0.351	13.16%	0.338
Services (=1)	48.98%	0.500	42.68 %	0.495
Financials				
Log-sales	13.69	2.421	15.75	2.407
Value-Added/Worker	105,897.58	1,049,176.33	135,139.12	832,781.62
Firm size (# employees)	157.36	915.26	158.92	1,015.75

Notes: Table 1.4 presents summary statistics for two samples. The first two columns correspond to the sample of the firms contained in the data set *IES*, detailed in section 1.3.2. The last two columns correspond to the largest connected set of the analysis sample in columns (1) and (2). Section 1.4.2 contains an explanation of the largest connected set. A detailed account of the construction of each variable can be found in Appendix A.2.

Table 1.5: Variance Decomposition. Wage Variation on Worker and Firm Heterogeneity.

	1986-2013		**2005-2013**	
	(1)	(2)	(3)	(4)
	Var	Share	Var	Share
Log-wages	0.397	100.00%	0.524	100.00%
Employee FE	0.241	60.48%	0.289	55.19%
Firm FE	0.079	20.08%	0.135	25.91%

Notes: Table 1.5 displays the results of a variance decomposition exercise conducted as per equation 1.10. The majority -between 55 and 60%- of the wage variation is explained by employee unobserved heterogeneity, while firm heterogeneity represents between 20 and 26% of the variation in wages. Note that firm heterogeneity has gained weight in the latest years.

Table 1.6: Variance Decomposition. Finite Sample Bias Adjusted.

	1986-2013	**2005-2013**
	(1)	(2)
Log-wages	100.00%	100%
Employee FE	53.10%	51.19%
Firm FE	18.68%	18.91%

Notes: Table 1.6 displays the results of a variance decomposition exercise corrected with the use of a variance shrinkage method, detailed in Appendix A.3. This method uses bootstrapping of standard errors in order to estimate sampling error associated to finite sample panel data sets.

Table 1.7: Production Function Estimates. Main Specification with CEO Quality.

	Manufacturing Sector		**Services Sector**	
	Elasticities	**Overidentification**	**Elasticities**	**Overidentification**
	(1)	(2)	(3)	(4)
Sum	1.01	0.00***	0.98	0.00***
CEO Proxy	**0.049***** (0.005)	-	**0.046***** (0.003)	-
Employees	0.21 *** (0.010)	-	0.22 *** (0.005)	-
Capital	0.10 *** (0.011)	-	0.06 *** (0.005)	-
Materials	0.35 *** (0.008)	-	0.33 *** (0.007)	-
Services	0.36 *** (0.005)	-	0.32 *** (0.003)	-
# Observations	1,220,771		1,660,193	

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** (p¡0.001.

Notes: Table 1.7 presents the results of the production function estimations as in Wooldridge (2009). Materials and services are used as instruments for firm TFP and variable inputs. State variables are CEO quality, labor and capital stock. Results are presented both for the manufacturing and services sector. Columns 3 and 6 present the Sargan-Hansen overidentification test p-values. More details on the production function estimations can be found in section 1.4.5.

Table 1.8: Placebo Production Function Estimates. Random Employee.

	Manufacturing Sector		**Services Sector**	
	Elasticities	**Overidentification**	**Elasticities**	**Overidentification**
	(1)	(2)	(3)	(4)
Sum	1.02	0.00	1.01	0.00
Random Employee	0.00 *** (0.007)	-	0.00 *** (0.004)	-
Employees	0.24 *** (0.013)	-	0.26 *** (0.005)	-
Capital	0.05 *** (0.012)	-	0.05 *** (0.014)	-
Materials	0.37 *** (0.025)	-	0.37 *** (0.022)	-
Services	0.36 *** (0.006)	-	0.33 *** (0.004)	-

Standard errors in parentheses.

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

Notes: Table 1.8 presents the results of the production function estimations as in Wooldridge (2009). Materials and services are used as instruments for firm TFP and variable inputs. State variables are the random employee estimated quality, labor and capital stock. Results are presented both for the manufacturing and services sector. Columns 3 and 6 present the Sargan-Hansen overidentification test p-values. Further explanation of this alternative specification is offered in 1.5.1.

Table 1.9: Placebo Production Function Estimates. CEO Full Labor Market Trajectory.

	Manufacturing Sector		Services Sector	
	Elasticities	**Overidentification**	**Elasticities**	**Overidentification**
	(1)	(2)	(3)	(4)
Sum	0.99	0.00	1.03	0.00
CEO FE	**0.09*** *** (0.007)	-	**0.10*** *** (0.006)	-
Employees	0.16 *** (0.018)	-	0.18 *** (0.011)	-
Capital	0.06 *** (0.013)	-	0.05 *** (0.005)	-
Materials	0.36 *** (0.028)	-	0.33 *** (0.023)	-
Services	0.32 *** (0.006)	-	0.37 *** (0.004)	-

Standard errors in parentheses.

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

Notes: Table 1.9 presents the results of the production function estimations as in Wooldridge (2009). Materials and services are used as instruments for firm TFP and variable inputs. State variables are the CEO estimated quality using the whole labor market tenure (including the CEO years) of each CEO, labor and capital stock. Results are presented both for the manufacturing and services sector. Columns 3 and 6 present the Sargan-Hansen overidentification test p-values. Further explanation of this alternative specification is offered in 1.5.1.

Table 1.10: Alternative Production Function Specifications.

	Manufacturing Sector		**Services Sector**	
	OLS FE	**Translog**	**OLS FE**	**Translog**
	(1)	(2)	(3)	(4)
Overidentification P-value	0.00***	0.00***	0.00***	0.00***
CEO Proxy	**0.02*****	**0.05*****	**0.03*****	**0.04*****
	(0.011)	(0.009)	(0.007)	(0.005)

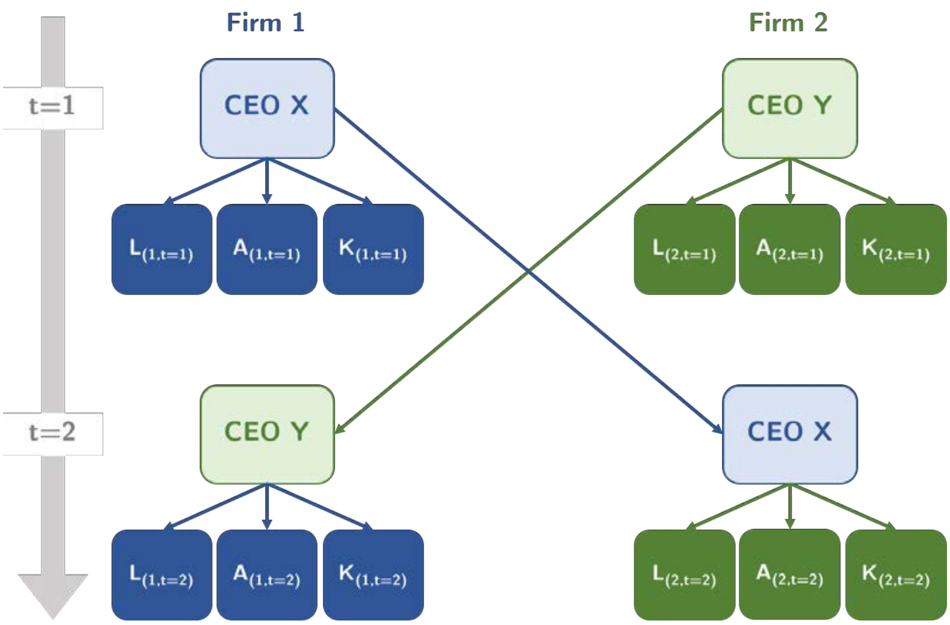
Standard errors in parentheses.

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

Notes: Table 1.10 presents the results of the production function estimations as in Wooldridge (2009). Materials and services are used as instruments for firm TFP and variable inputs. State variables are the random employee estimated quality, labor and capital stock. Results are presented both for the manufacturing and services sector.

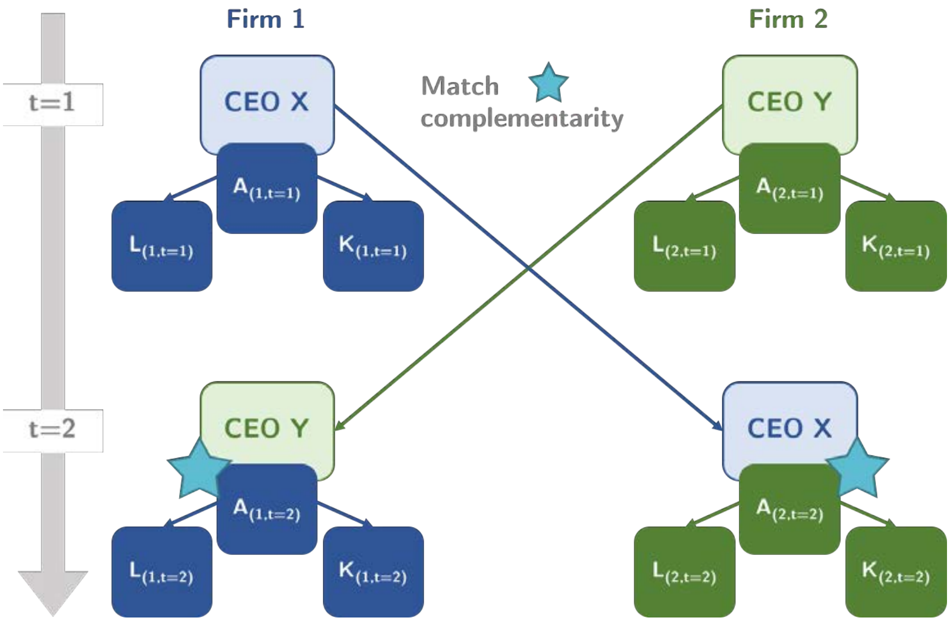
Tables and Figures: Chapter 2

Figure 2.1: CEO and Firm. Additive Separability.



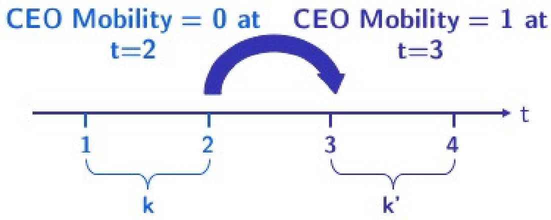
Notes: Figure 2.1 illustrates the framework for CEO and firm interactions used in the fixed effects literature and in chapter 1 of this thesis. In this framework, CEO ability and firm productivity are assumed to be additively separable. Consider a simplified model with two time periods, two firms and two CEOs. This framework would imply that, *ceteris paribus*, whenever a CEO switches firms, whatever happens to the firm's outcomes can be attributed to the change in CEO.

Figure 2.2: CEO and Firm. Match Complementarities.



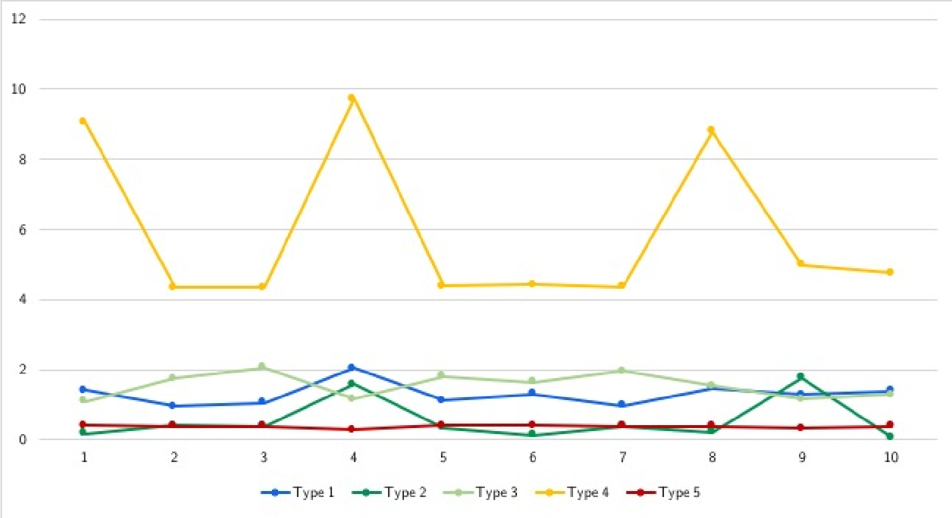
Notes: Figure 2.2 illustrates a new framework for CEO and firm interactions. In the model I explore in section 2.2, I propose that CEO and firm are not fully separable, but rather a joint production function with a surplus for suitable CEO-firm matches.

Figure 2.3: CEO and Firm. Dynamic Model Timeline.



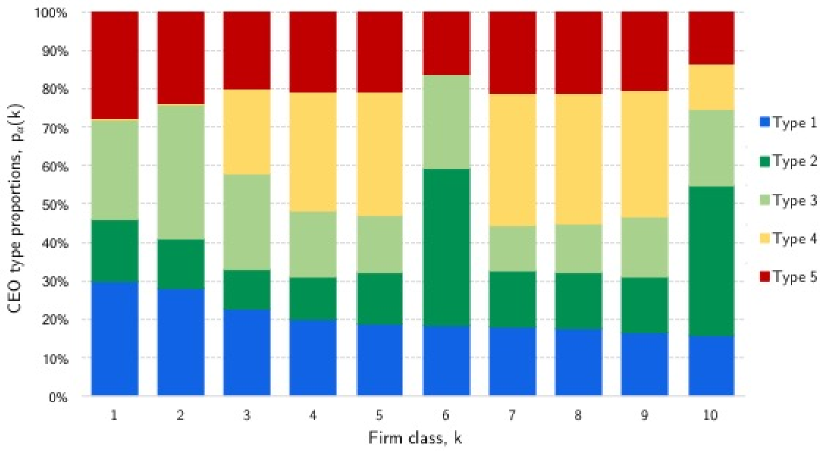
Notes: Figure 2.3 shows the timeline of the dynamic model described in section 2.2. k and k' stand for two different firm discrete heterogeneity classes.

Figure 2.4: CEO Latent Type. Trajectories across Firm Classes.



Notes: Figure 2.4 displays the log-revenue trajectories of the CEO-firm pair for each of the CEO latent types across different firm classes. Each line of the graph represents a different CEO latent type. The x-axis represents the 10 firm classes computed using the *kmeans* algorithm and the y-axis has firm log-revenues net of capital, labor and intermediate input expenditure.

Figure 2.5: CEO Latent Type. Initial Distribution for each Firm Class.



Notes: Figure 2.5 displays the probability distribution, for each firm class k, of the $L = 5$ different CEO latent types.

Table 2.1: Descriptive Statistics. Firm Class Analysis Results.

Class	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**All**
Avg. Age	30.97	43.88	44.20	44.25	44.21	42.79	44.06	43.68	43.30	43.10	**44.09**
Avg. Tenure	7.02	6.28	7.32	7.51	7.27	5.21	6.85	6.47	6.47	5.96	**7.13**
Avg. Salary (eur)	918.87	1,006.02	1,044.28	1,212.20	1,398.17	1,515.50	1,693.19	2,002.25	2,068.67	2,320.53	**1,365.27**
% BA	10.88%	14.47%	10.68%	12.19%	14.79%	22.14%	19.06%	20.99%	24.73%	27.70%	**24.65%**
% MA	0.71%	0.96%	0.78%	0.86%	1.00%	1.90%	1.34%	1.41%	1.92%	1.98%	**1.05%**
Avg. Revenues	376k	390k	546k	1,028k	1,187k	1,326k	2,301k	3,585k	5,406k	16,420k	**1,531k**
# Employees	84,213	28,897	156,017	199,917	182,429	4,575	128,530	68,344	28,670	6,524	**888,116**
% Manufacturing	20.87%	19.93%	23.16%	21.52%	17.09%	19.23%	15.26%	13.15%	30.12%	9.49%	**18.77%**
% Construction	20.23%	21.32%	20.47%	18.35 %	14.63%	23.04%	13.04%	14.78%	38.35%	22.07%	**17.23%**
% Services	58.89%	58.75%	56.37%	60.12%	68.72%	57.73%	71.69%	72.06%	44.90%	68.44%	**64.01%**

Notes: Table 2.1 presents the descriptive statistics for each estimated firm class, according to the *kmeans* clustering algorithm presented in section 2.5.1.

Table 2.2: Counterfactual Experiment. CEO-Firm Complementarities.

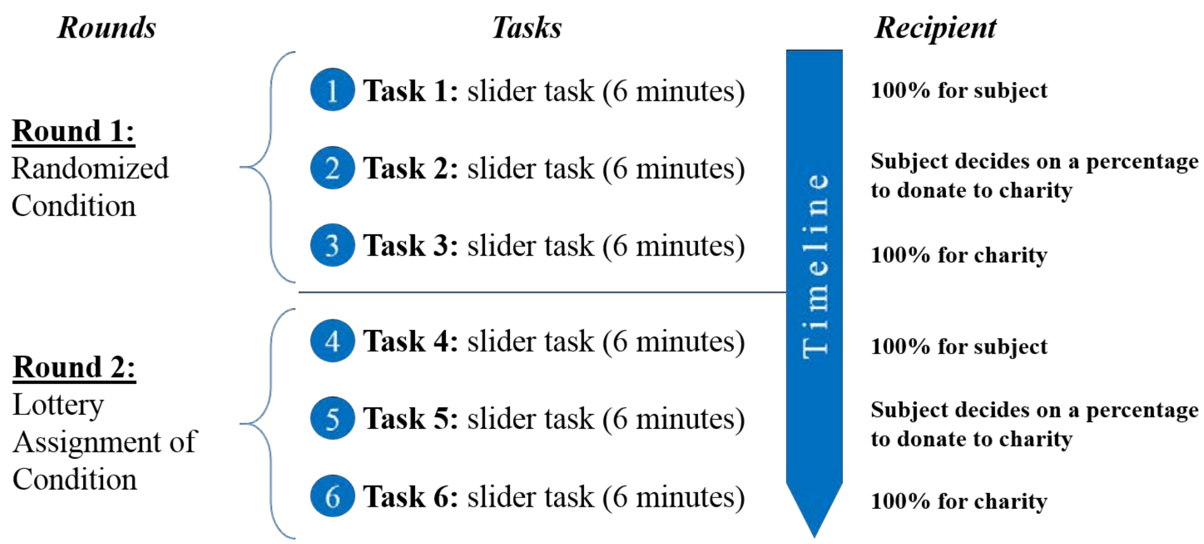
Firm Size	**Mean**	**75th-Percentile**	**90th-Percentile**
Estimate	1.98%	2.12%	3.11%
	(.05)	(.09)	(.10)

Tables and Figures: Chapter 3

Figure 3.1: Lab Experiment. Slider Task.

Notes: Figure 3.1 illustrates the task that participants performed in the lab experiment. The goal of this task is to place the slider in the number displayed on the left hand side.

Figure 3.2: Lab Experiment. Timeline.



Notes: Figure 3.2 summarizes the timeline of the experiment, from the time the participant sits in the lab until the end of the experiment.

Table 3.1: Lottery Probability Distribution

Prob(x=1)	30%
Prob(x=2)	25%
Prob(x=3)	20%
Prob(x=4)	15%
Prob(x=5)	10%

Table 3.2: Description of Treatments

Treatment	**Condition**	**Nr. Subjects**	**Nr. Sessions**
Treatment 1	Reputation	17	1
Treatment 2	Warm Glow	15	1
Treatment 3	Career	20	1
Treatment 4	Enjoyment	19	1
Treatment 5	Identification	15	1
Treatment 6	Peer Pressure (high)	19	1
Treatment 7	Peer Pressure (low)	19	1
Control	*No Condition*	33	2

Table 3.3: Descriptive Statistics

	Control	**Treatments**	**T1: Reputation**	**T2: Warm Glow**	**T3: Career**	**T4: Enjoyment**	**T5: Identification**	**T6: Peer high**	**T7: Peer low**	**Overall**
Round 1										
Productivity	168	166	161	171	160	165	189	165	171	168
	(37.19)	(27.75)	(57.34)	(51.52)	(32.23)	(24.91)	(32.42)	(31.64)	(20.89)	(35.31)
Donation (%)	18.91	11.14	6.29	12.20	12.70	10.26	14.20	11.84	10.78	12.82
	(29.92)	(18.63)	(8.86)	(25.02)	(26.61)	(9.93)	(17.07)	(13.66)	(22.79)	(21.68)
Donation (%) if assigned the preferred condition	-	9.85	7.50	11.66	7.64	-	50.00	-	-	9.85
	(-)	(23.83)	(10.61)	(10.40)	(26.65)	(-)	(0.00)	(-)	(-)	(23.83)
Round 2										
Productivity	185	193	181	194	188	193	218	186	194	191
	(32.98)	(43.56)	(62.57)	(62.11)	(31.42)	(28.07)	(52.61)	(34.15)	(24.44)	(41.55)
Donation (%)	12.35	9.87	5.52	10.53	13.25	9.74	11.07	8.68	10.05	10.41
	(24.90)	(18.06)	(8.80)	(22.71)	(27.21)	(9.35)	(14.58)	(13.00)	(22.85)	(19.67)
Donation (%) if assigned the preferred condition	17.44	10.48	9.00	25.75	1.25	8.89	14.60	9.38	5.50	11.76
	(32.77)	(17.23)	(11.06)	(43.04)	(2.50)	(9.93)	(14.52)	(17.00)	(6.36)	(20.68)
Demographics										
Female	52.94%	61.29%	70.59%	60.00%	50.00%	63.16%	66.67%	84.21%	36.84%	59.49%
Volunteers	50.00%	29.03%	29.41%	13.33%	10.00%	26.32%	53.33%	36.84%	36.84%	33.54%
Religious	11.76%	14.52%	17.65%	20.00%	20.00%	0.00%	0.07%	21.05%	15.79%	13.92%
Age	22	22	22	24	23	21	22	21	22	22

Table 3.4: Econometric Analysis

	OLS		**Probit 1**		**Tobit 1**		**Probit 2**		**Tobit 2**	
	(1)		(2)		(3)		(4)		(5)	
	Productivity$_1$		*Donate*$_1$=1		*Donation*$_1$		*Donate*$_2$=1		*Donation*$_2$	
	β / SE	ME	β / SE	ME	β / SE	ME	β / SE	ME	β / SE	ME
Demographics										
Female (=1)	-11.94**	**	0.62**	0.23**	3.63		0.39	0.15	2.80	
	(6.02)		(0.24)		(5.78)		(0.27)		(5.25)	
Age	-2.31**	**	0.06	0.02	0.28		0.02	0.01	-0.01	
	(1.11)		(0.05)		(0.89)		(0.05)		(0.84)	
Religious (=1)	-0.64		0.08	0.03	2.29		0.49	0.19	4.57	
	(8.30)		(0.46)		(8.43)		(0.38)		(6.53)	
Volunteer (=1)	-1.18		-0.39	-0.14	-8.63		-0.31	-0.12	-6.10	
	(6.35)		(0.31)		(5.52)		(0.33)		(5.70)	
Assigned Condition (1)										
Matching (Round 1)	-4.29									
	(11.00)									
Treatments										
Treatment 1	-1.90		-0.45	-0.17	-19.95**	**	-0.44	-0.17	-17.08*	*
	(10.62)		(0.38)		(9.26)		(0.46)		(9.27)	
Treatment 2	11.89		0.00	0.00	-8.78		-0.10	-0.04	-5.63	
	(11.62)		(0.48)		(10.99)		(0.49)		(10.87)	
Treatment 3	-1.38		-0.91**	-0.34**	-18.42		-0.61	-0.24	-5.66	
	(12.81)		(0.45)		(12.39)		(0.54)		(11.96)	
Treatment 4	-0.16		0.05	0.02	-10.11		0.47	0.18	-0.44	
	(10.17)		(0.41)		(8.55)		(0.50)		(7.89)	
Treatment 5	27.02**	**	0.81**	0.30**	-0.14		0.81**	0.31**	4.86	
	(10.91)		(0.39)		(8.54)		(0.40)		(8.02)	
Treatment 6	0.72		-0.18	-0.07	-9.77		-0.39	-0.15	-11.33	
	(10.27)		(0.41)		(9.35)		(0.36)		(9.84)	
Treatment 7	4.46		0.37	0.14	-6.73		0.78	0.30	3.30	
	(10.09)		(0.33)		(9.45)		(0.57)		(9.13)	
Assigned Condition (2)										
Preferred 1							0.56	0.22	19.82**	**
							(0.50)		(8.38)	
Preferred 2							0.32	0.12	10.42	
							(0.48)		(7.63)	
Preferred 3							0.27	0.10	13.77	
							(0.49)		(8.87)	
Preferred 4							0.53	0.21	22.78**	**
							(0.64)		(10.34)	
Constant	222.59***	***	-1.11		7.07		-0.85		-10.72	
	(25.31)		(1.16)		(22.54)		(1.07)		(22.27)	
sigma					29.51***	***			28.00***	***
					(3.52)				(3.69)	
Observations	158		158		158		158		158	
Pseudo R^2			0.11		0.01		0.11		0.01	
LR chi2			22.00				25.51			
Prob > chi2	0.23		0.02		0.46		0.04		0.49	
Baseline predicted probability			0.64				0.58			

Appendix: Chapter 1

A.1 Appendix: Proofs

A.1.1 CEO Quality and Firm Productivity with Random Assignment

Proof of Proposition 1:

$$\frac{dPY_{j,t}}{d\alpha_i} = \frac{d(P * A_j \alpha_i^{\mu} L_{j,t}^{\delta} K_{j,t}^{1-\delta-\mu})}{d\alpha_i} = \mu > 0, QED \tag{A.1}$$

where the last step derives from the fact that input elasticities are assumed to present $0 < \mu < 1$.

A.1.2 Causal Identification

In section 1.4.3 I claim that consistent estimation of equation (1.7) via OLS implies the following assumptions regarding the interaction of the error terms with explanatory variables:

$$E[e^{i\prime}\varepsilon] = 0, \forall i \qquad E[f^{j\prime}\varepsilon] = 0, \forall j \qquad E[x^{k\prime}\varepsilon] = 0, \forall k \tag{A.2}$$

Furthermore, given the assumptions made on the error term components, causal identification of equation (1.7) boils down to the verification of the assumption $E[f^{j\prime}\varepsilon] = 0, \forall j$, because this equation is a direct result from the assumptions on the error terms.

A sufficient condition for this equation to hold is that the assignment of employees to firms is strictly exogenous with respect to ε :

$$P(J(i,t) = j|\varepsilon) = P(J(i,t) = j) = G_{jt}(\alpha_i; \psi_1, ..., \psi_J) \forall i, t \tag{A.3}$$

where the employment probability functions G_{jt} sum up to 1 at every period for every employee i. This does not preclude sorting among α and ψ.

A.2 Appendix: Data & Sample Selection

A.2.1 CEO Quality

In the context of this chapter, CEO quality or ability is defined as the unobserved heterogeneity component that influences the firm's production function. This unobserved heterogeneity is considered a "black box" that has been studied in the management and personnel economics literature; however, no unique definition can be conveyed that encompasses the whole dimensionality of quality-related characteristics. Since the goal of this chapter is to present a tractable model for understanding productivity differences by relating it to unobserved heterogeneity, I do not aim to present a definition of manager quality from a psychological perspective of skill multiplicity; rather, I consider quality as an identity factor that the CEOs bring with them throughout their career, before an after becoming CEOs.

A.2.2 Identifying General Managers or CEO

The definition of "manager" or managerial position within a firm, albeit seemingly intuitive in the business world, is not straightforward in a scientific context. An accurate definition of the manager is a key step in the analysis of their quality and/or impact. As such, careful consideration should be granted to pinning the precise notion of "manager" to be used. In this chapter, I focus on the analysis of the impact of the so-called general manager, director, or CEO, in firm performance. For conciseness, I broadly define this highest level manager as "CEO" throughout the chapter. I consider a practical definition postulated by the ILO[1]. whereby I consider the code 112 (Managing Directors and Chief Executives).

The data set *Quadros de Pessoal* contains a variable called *job title*[2] that corresponds to a 6-digit occupational classification system which was implemented as of 1995 (CNP 94). This occupation code divides top-managers into two classes: 12 corresponds to medium and large sized firm Directors and 13 corresponds to small firm Directors, where small firms are defined as having fewer than 10 workers. The class of Firm Directors (12) is further detailed into different categories: General Managers, Operations Managers, and Other Managers[3]. Moreover, these data contain another important variable for hierarchical classification, *qualification*[4]. This is a categorical variable which takes value=1 if the employee ID belongs to the highest hierarchical class within the firm. This variable is connected to the legal form of contract associated to the employee. Finally, the the *Quadros de Pessoal* data set also contains a variable entitled *professional status*[5], a categorical variable that takes value 1 if the observation (employee ID) is also the owner of the firm.

I identify the General Manager (or CEO) of each firm by resorting to a classification

[1] I consider the latest International Standard Classification of Occupations (ISCO-08) by the International Labor Organization (ILO): http://www.ilo.org/public/english/bureau/stat/isco/docs/groupdefn08.pdf.

[2] The variable *job title* is called "Profissão" in the *Quadros de Pessoal* data set.

[3] Administrative, Financial and Sales Managers.

[4] The variable *qualification* is called "qualif" in the *Quadros de Pessoal* data set.

[5] The variable *professional status* is called "sitpro" in the *Quadros de Pessoal* data set.

procedure[6] in which different criteria are used at each step:

General Manager. For each firm-year pair, I use the variable *job title* attribute the "CEO" label to the employee ID who is identified as General Manager. If there is a tie[7], I pick the manager with the highest salary among the ones classified as General Manager. This step identifies a total of 12.83% of the firm-year observations.
Operational Manager. Using the same variable, *job title*, to identify the Operational Manager in the absence of an identified General Manager. I attribute the "CEO" label to the identified Operational Manager in this case. If there is a tie[8], I pick the manager with the highest salary among the ones classified as Operational Manager. With this additional step, I can identify a total of 30.37% of the firm-year observations.
Other Manager. Using the same variable, *job title*, to identify Other Managers in the absence of an identified General or Operational Manager. I attribute the "CEO" label to the identified Other Manager in this case. If there is a tie[9], I pick the manager with the highest salary among the ones classified as Other Manager. With this additional step, I can identify a total of 41.49% of the firm-year observations.
Owner. After fully exploiting the *job title* variable, I turn to ownership status to identify the head of the firm for the remaining firm-year pairs with unidentified General Manager or CEO. I use the variable *professional status* described above and label as "CEO" the employee ID for whom this variable takes value 1 (corresponding to the owner). There are no unsolved ties using this criterion. With this additional step, I can identify a total of 51.65% of the firm-year observations.
Top Hierarchical Class. I now use the variable *qualification* described above to identify remaining firm-year pairs. I attribute the label "CEO" to the employee ID associated with the highest hierarchical class within *qualification* (qualification=1). If there is a tie[10], I pick the manager with the highest salary among the ones classified as Operational Manager. With this additional step, I can identify a total of 61.21% of the firm-year observations.
Previous Manager. For remaining unidentified firm-year pairs, I attribute the label CEO to an employee ID which was classified as a General Manager/CEO (according to any of the criteria above) in the period before and is still employed at the same firm[11]. There are no unsolved ties using this criterion. With this additional step, I can identify a total of 67.17% of the firm-year observations.
Next Manager. Similarly, for remaining unidentified firm-year pairs, I attribute the label CEO to an employee ID which was classified as a General Manager/CEO (according to any of the criteria above) in the period after in the same firm[12]. There are no unsolved ties using this criterion. With this additional step, I can identify a total of 71.26% of the firm-year observations.
Maximum Salary. Finally, for any remaining firm-year pair with unidentified CEO, I pick the employee which displays the highest salary. There are no unsolved ties using

[6] This classification procedure as well as sample selection is inspired by those used in Queiró (2016).
[7] Ties happen for 0.54% of the data.
[8] Ties happen for 0.59% of the data.
[9] Ties happen for 0.06% of the data.
[10] Ties happen for 3.76% of the data.
[11] This indicates some type of error/gap in the survey completion.
[12] This indicates some type of error/gap in the survey completion.

this criterion. With this additional step, I can identify a total of 93.09% of the firm-year observations.

The remaining firm-year pairs with no identified General Manager/CEO pertain to those that do not disclose personnel salaries.

A.2.3 Sample Selection

Quadros de Pessoal is a mandatory annual survey that contains personnel information on any private sector firm that employs at least one individual by October of each year. It is an anonymized database with identifiers for both firm and employee and spans from 1982 to 2013, with two gaps (1999 and 2001), totalling 29 years. With an initial data set composed of a total 6,140,063 firm-year pairs and 62,661,660 employee level observations, after eliminating missing identifiers, I proceed to restrict the selection of the final sample to be used.

First, I identify public enterprises or partially state-owned organizations, according to two criteria. I label as *public firms* those whose percentage of public capital exceeds 50%. I attribute that classification to firms that are identified as *public administration* in *legal status* variable of this data set. I discard firms who are labelled as *public* at any point in time since hierarchical structures in the public sector are very different from the private sector, with little cross-sector or within-firm mobility.

Second, and based on the step 1, I identify firms who have at least 50 of the same employees as a firm formerly identified as public and that no longer appears in the data. This amounts to identifying privatized firms which often times maintain their public-style hierarchical structures.

Third, I winzorize the data on both salaries and firm revenues at the 99^{th} percentile. I do not use firms with less than 20 employees.

Lastly, I eliminate two classes of sectors, in line with other matched employer-employee studies in the literature: Agriculture and Fishery, and the Banking Sector. I therefore focus on the sectors pertaining to Manufacturing Industry, Construction and Services.

The *Informação Empresarial Simplificada* (IES) is also a mandatory annual survey that reports on firm-level Balance Sheet and Profit & Loss statements. It includes information on firm's assets and liabilities, inputs, revenues, operating and financial profits, value added, sector, size (number of employees) and location. The survey is anonymized and contains a firm identifier that allows for the matching with the *Quadros de Pessoa* data set.

A.2.4 Variables and Coding

In section 1.4.1 I use education fully interacted with a second-degree polynomial on age as time-varying covariates in the two-way fixed-effects model. **Education** is the number of completed schooling years and **age** calculated using the birthdates available in the data. In cases where different education attainment is recorded for the same employee, I take the mode of the education years reported for each employee. If there is more than one mode, I take the lowest. The outcome variable in this regression is monthly **salary**, calculated as a sum of base salary, bonuses and pay for extra hours.

In section 1.4.5 I use four input variables, taken from the IES data, besides the proxy for CEO quality calculated in section 1.4.1, to explain firm productivity. As variable inputs, I use **services** and intermediate **materials**. Both variables are taken directly from the firm's Profit & Loss Statement and deflated to 2000 euros according to two-digit firm sector price indices. **Labor** and **capital** are the state variables used in this section. Labor is measured as the number of non-CEO workers employed by the firm. I measure capital as deflated book value of fixed assets.

In section 1.4.5 I focus on a well-defined output that is transversally applicable across different economic sectors: deflated revenue productivity. Revenues from production or service delivery within the private sector are a primary evidence of firm performance. In fact, revenue productivity is frequently used in the Organization and Growth literatures (Bender et al., 2016; Hsieh & Klenow, 2009, 2014). I deflate revenues according to two-digit firm sector price indices.

A.3 Appendix: Econometric Model

A.3.1 Largest Connected Set

In section 1.4.2, when constraining the data to the largest connect set of employee-firm pairs, my samples are slightly modified in two predictable ways. One is the fact that larger-sized firms become more represented in the largest connected set sub-sample, both measured in operating revenue and number of employees. The other is the fact that these firms seem to last slightly longer within the whole sample. Yet, these differences do not present a worrisome outlook on the representativeness of the fixed-effect analysis. The two-way fixed-effects approach relies on manager job mobility as a source of variation. Therefore, the most important attribute of the largest connected set is that the composition of manager characteristics does not change substantially from that of the whole sample. This seems to be the case when comparing the descriptive statistics of the two samples, detailed in Table 1.1 and Table 1.2.

A.3.2 Intuition Behind Endogenous Mobility: An Example

To provide intuition for the consistent estimation of individual fixed effects referred to in section 1.4.3, let us consider the simple case in which there are two CEO and two firms, producing in two time periods, $t = \{1, 2\}$. Consider Figure A.1. At $t = 1$, the wage functions based on equation (1.5) can be evaluated as follows

$$\begin{aligned} y_{i=1,t=1} &= \alpha_1 + \psi_1 + \boldsymbol{X_{1,t=1}\beta} + \varepsilon_{1,t=1} \\ y_{i=2,t=1} &= \alpha_2 + \psi_2 + \boldsymbol{X_{2,t=1}\beta} + \varepsilon_{2,t=1} \end{aligned} \tag{A.4}$$

where $j(1, 1) = 1$ and $j(2, 1) = 2$. At $t = 2$, the wage functions based on equation (1.7) can be evaluated as follows

$$\begin{aligned} y_{i=1,t=2} &= \alpha_1 + \psi_2 + \boldsymbol{X_{1,t=2}\beta} + \varepsilon_{1,t=2} \\ y_{i=2,t=2} &= \alpha_2 + \psi_1 + \boldsymbol{X_{2,t=2}\beta} + \varepsilon_{2,t=2} \end{aligned} \tag{A.5}$$

where $j(1, 2) = 2$ and $j(2, 2) = 1$. The expected change in output for employee $i = \{1, 2\}$ from time period 1 to 2 moving from firm j to j' is given by a difference-in-difference analysis,

$$E[y_{i,t=2} - y_{i,t=1}|j_{i,t=2} = j', j_{i,t=1} = j] = \psi_{j'} - \psi_j + E[\varepsilon_{i,t=2}|j_{i,t=2} = j'] - E[\varepsilon_{i,t=1}|j_{i,t=1} = j] \tag{A.6}$$

Taking equation (1.6) into account, if $\lambda_{i,j(i,t)}$, ω_{it} and υ_{it} are uncorrelated with job mobility from firm 1 to 2 or vice-versa (which is the determinant of the personal fixed effect), then $E[\varepsilon_{i,t=2}|j_{i,t=2} = j'] - E[\varepsilon_{i,t=1}|j_{i,t=1} = j]$ can be cancelled out and the whole expression simplifies to the difference in firm fixed effects[13], $\psi_{j'} - \psi_j$. Notice that, in case this holds, we should observe in the data that movements from firm j to j' should yield employees a symmetric gain as compared to movements from j' to j. This is what I test for in Figure 1.1.

[13]In this context, firm fixed effects can be interpreted as firm-specific payment policies or incentives.

A.3.3 Finite Sample Issues: Variance Shrinkage

Estimating individual fixed effects in panel data sets entail an inherent challenge related to the panel structure. Each individual is observed a limited amount of instances in the time-series and therefore, the finite sample available for each individual may result in a generally upward incidental parameter bias[14] on estimated fixed effects.

Several corrections for this type of bias have been proposed in the literature. These corrections are often referred to as variance shrinkage procedures. In this chapter, I use a method proposed by Kane & Staiger (2008) and used in Best et al. (2017). This method estimates sampling error via a bootstrap method and shrinks the variance of the fixed effects by that factor.

By this method, it is assumed that the estimated sample variance of employee and firm contains its true value (σ_α^2 and σ_A^2) and a term that represents noise due to sampling error (σ_ξ^2 and σ_ν^2) which arises from the finite nature of the individual sample:

$$Var(\hat{\alpha}) = \sigma_\alpha^2 + \sigma_\xi^2 \qquad Var(\hat{A}) = \sigma_A^2 + \sigma_\nu^2 \tag{A.7}$$

where the parameters of interest are σ_α^2 and σ_A^2. I use a bootstrap technique to calculate standard errors of each of the two estimated fixed effects, which yields estimates of the sampling errors of this finite sample for both employee and firm fixed effects. Denote them s_e^2 and s_f^2. I take the expected value of these estimated sampling error terms across employees, $E_e(s_e^2)$, and firms $E_f(s_f^2)$. This finally yields the following estimated variances:

$$\sigma_\alpha^2 = Var(\hat{\alpha}) - E_e(s_e^2) \qquad \sigma_A^2 = Var(\hat{A}) - \mathrm{E}_f(s_f^2) \tag{A.8}$$

The results of this variance shrinkage can be found in Table 1.5.

A.3.4 Additively Separable: Sorting Tests

I test for the assumption of additive separability (log-linarity) assumed in the model described in section 1.4.1 by resorting to heat map tests. I take the firm and employee fixed effect, estimated in non-CEO years, and divide each of them into vingtiles of the distribution. I then group each pair of vingtiles corresponding to each observation and compute the average regression residual. I then plot the result into a color coded map.

Results can be found in Figures A.2 and A.3. No distinct pattern can be identified in either of the figures, which means that, after controlling for separately additive firm and employee heterogeneity, the remaining unexplained term is not driven by non-separable interactions between the two fixed effects.

A.3.5 GMM Estimation of Firm Production Function

Since Marschak & Andrews (1944) first introduced the concept of simultaneity bias in the estimation of production functions, several attempts at correcting this problem have been suggested in the literature, the most widely used being Olley & Pakes (1996) (OP for

[14]See Andrews et al. (2008, 2012) for detailed explanations of this type of bias.

short) and Levinsohn & Petrin (2003) (LP for short). OP show that investment can be used as a proxy variable for unobserved productivity by employing a two-step estimation method. They assume that the industry produces with a Cobb-Douglas technology and that factors underlying profitability differences among firms are neutral efficiency differences. The first step is the estimation of a production function that is linear in labour and non-parametric in $g(A_{jt}, k_j t)$ a function of productivity and capital, which are considered state variables. In a second step, to identify the capital elasticities, further assumptions need to be made. Here, the authors use Markov process assumptions on A_{jt}. They regress output net of labor (variable input) on capital and a consistent estimate of $E[A_{jt}]$.

LP propose modifications to the OP approach to address the problem of lumpy investment. The authors claim that evidence of costly adjustment to capital investments explains the fact that many firms present zero investment at certain years. This leads to kinks in the firm investment demand function, meaning that firms or plants may not swiftly respond to certain productivity shocks. In that case, correlation between the regressors and the error term can remain. If it is less costly to adjust the intermediate input (materials) and LP argue that it may respond more readily to productivity. They use intermediate inputs to proxy for unobserved productivity instead, maintaining the two-step estimation method.

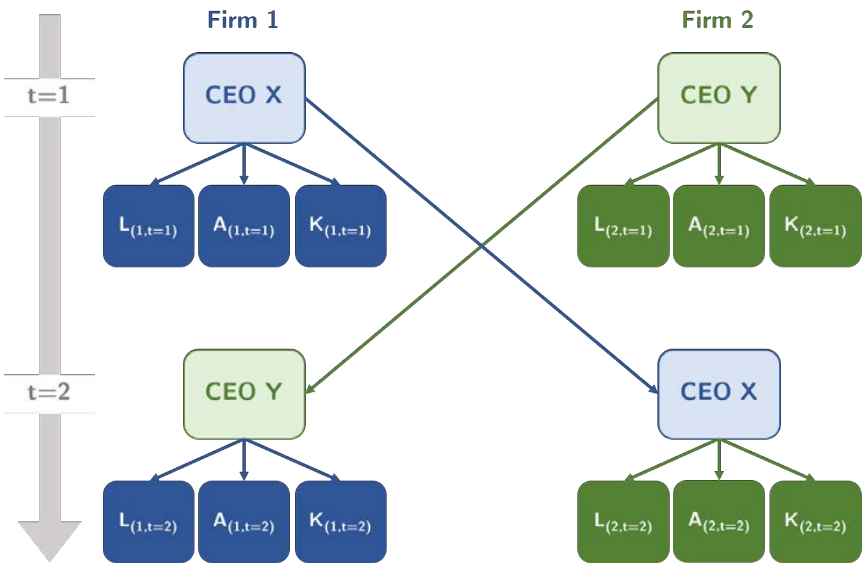
More recently, Wooldridge (2009) uses both of the former works' proxy variables while implementing a Generalized Method of Moments (GMM) approach, which bypasses the two-step method and its subsequent need to bootstrap standard errors. I follow this approach as it is now standard in the literature. The estimated production function takes the form:

$$q_{jt} = \beta_{CEO} CEO_{jt} + \beta_l l_{jt} + \beta_k k_{jt} + \beta_m m_{jt} + \beta_s s_{jt} + \epsilon_{jt} \tag{A.9}$$

where $A_{jt} + \eta_{jt}$ and A_{jt} is the persistent productivity term and η_{jt} is assumed to be an iid transitory shock to productivity. Results of this part of the estimation can be found in section 1.4.5 and Table 1.6. The estimation of A_{jt} is non-parametric and instrumented by a function on lagged state variables and instruments $g(X_{j,t-1}, m_{j,t-1})$. The non-parametric estimation is conducted by approximating the function $g(.,.)$ with third-degree polynomials in both state variables (CEO_{jt}, l_{jt} and k_{jt}) and instruments (m_{jt} and s_{jt}).

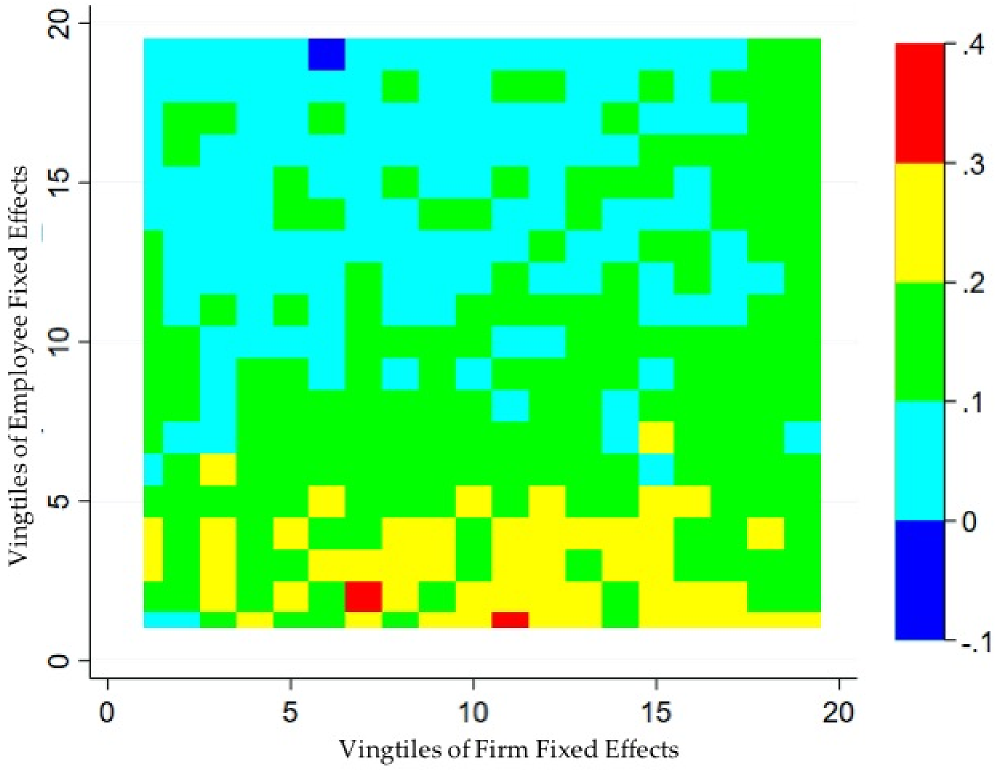
A.4 Appendix: Additional Figures & Tables

Figure A.1: CEO Mobility: Example in a Two Period, Two Firm Environment.



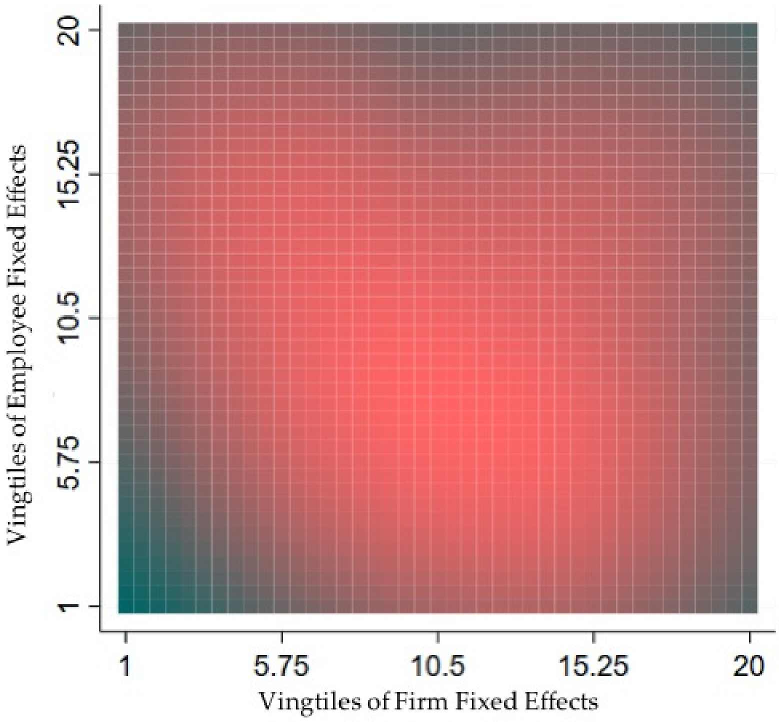
Notes: This figure contains an illustrative diagram of CEO job mobility in a 2-period, 2-firm model. In period 1, CEO X works at firm 1 and CEO Y works at firm 2. In period 2, CEO X moves to firm 2 and CEO Y moves to firm 1.

Figure A.2: Heat Maps. Vingtiles of CEO and Firm Fixed Effects.



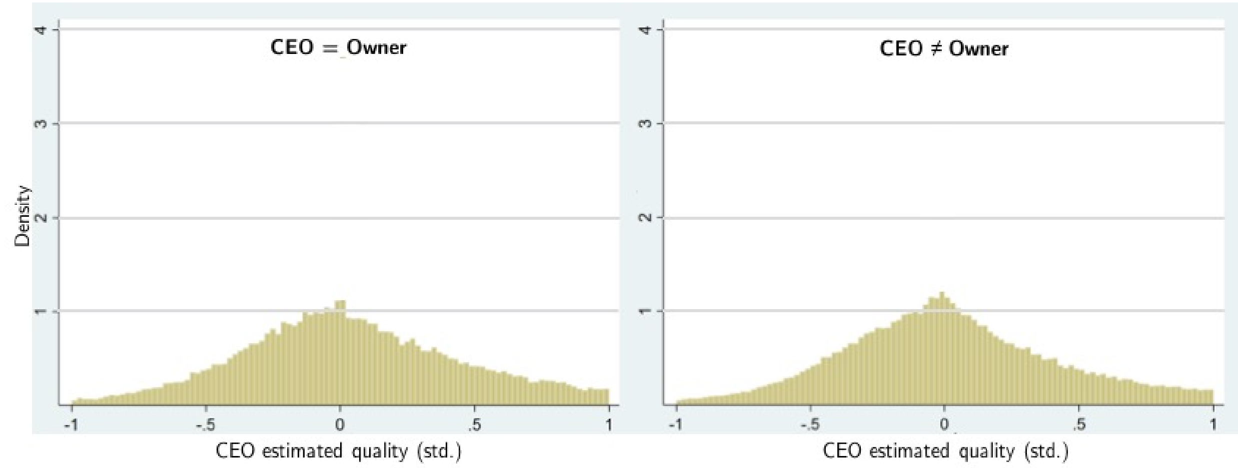
Notes: This figure presents a heat map of averages of the residuals from the estimation of equation (1.5), $y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it}$. I bin the residuals of this regression into vingtiles of the estimated employee fixed effect effect $\hat{\alpha_i}$ and firm fixed effect $\hat{\psi_j}$ within each connected set of firms. For this analysis, I use the sample defined in sections 1.4.1 and 1.4.2, which includes employees that never become a CEO or CEOs during their employee years.

Figure A.3: Density Heat Maps. Vingtiles of CEO and Firm Fixed Effects.



Notes: The figure presents a heat map of residuals from the estimation of equation (1.5), $y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it}$. The figure is derived from the estimation bivariate kernel density of vingtiles of estimated employee fixed effects $\hat{\alpha}_i$ and firm fixed effects $\hat{\psi}_j$ using a symmetric triangle kernel with bandwidth given as a proportion of sample range. For this analysis, I use the sample defined in sections 1.4.1 and 1.4.2, which includes employees that never become a CEO or CEOs during their employee years.

Figure A.4: CEO Quality Histograms. CEO Ownership Status.

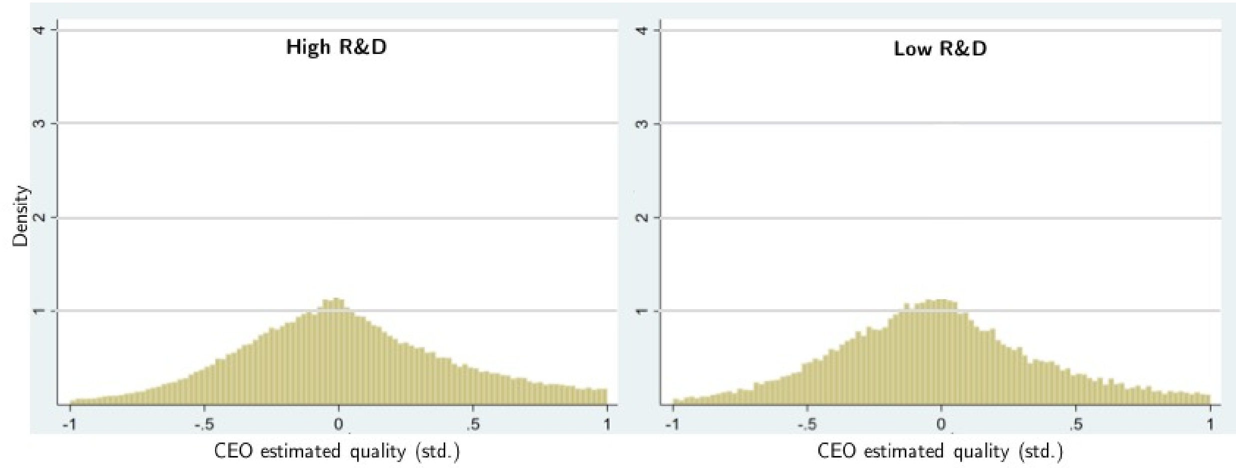


Notes: The figure presents a histogram of CEO quality as estimated in equation (1.5), $y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it}$ according to the ownership status of the CEO.

Figure A.5: CEO Quality Histograms. CEO Actual Experience.

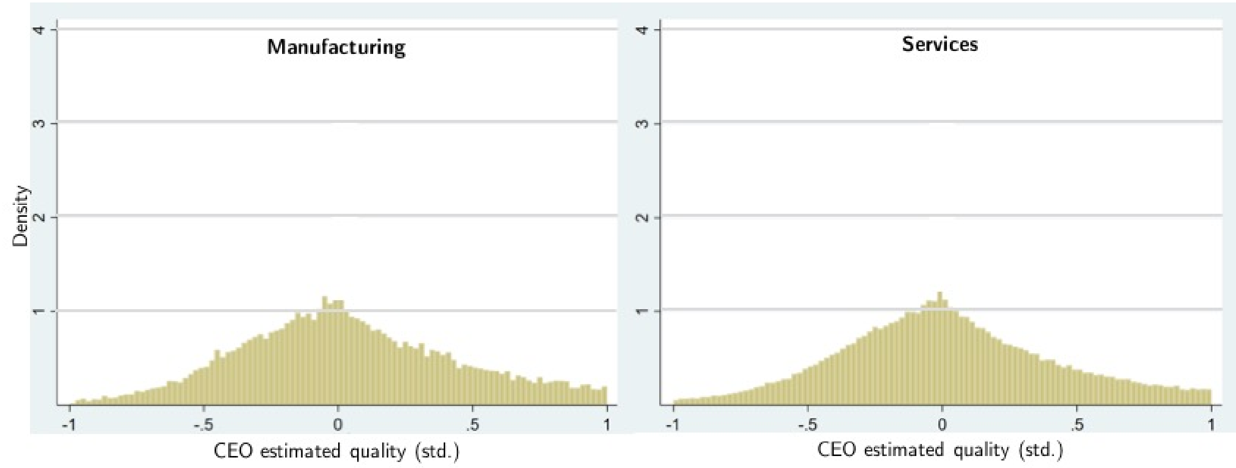
Notes: The figure presents a histogram of CEO quality as estimated in equation (1.5), $y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it}$ according to the actual experience of the CEO.

Figure A.6: CEO Quality Histograms. Firm RD Expenditure.



Notes: The figure presents a histogram of CEO quality as estimated in equation (1.5), $y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it}$ according to the level of RD expenditures of the firm.

Figure A.7: CEO Quality Histograms. Manufacturing vs Services Sector.



Notes: The figure presents a histogram of CEO quality as estimated in equation (1.5), $y_{it} = \alpha_i + \psi_{j(i,t)} + \boldsymbol{X_{it}\beta} + \varepsilon_{it}$ in different economic sectors.

Appendix: Chapter 2

B.1 Appendix: Finite Mixture Model

B.1.1 Assumptions

Formal Bonhomme et al. (2017b) dynamic model assumptions, applied to the context of CEO-firm revenue distribution:

Assumptions *(dynamic model)*

1. *Job mobility*: m_{it}, $k_{i,t+1}$ and $X_{i,t+1}$ are independent of $Y_i^{t\text{-}1}$, $m_i^{t\text{-}1}$ and $X_i^{t\text{-}1}$ conditional on Y_{it}, α_i and X_{it}.

2. *Serial dependence*: $Y_{i,t+1}$ is independent of $Y_i^{t\text{-}1}$, $k_i^{t\text{-}1}$, $m_i^{t\text{-}1}$ and X_i^t conditional on Y_{it}, α_i, m_{it}, k_{it}, $k_{i,t+1}$ and $X_{i,t+1}$.

In the context of the CEO-firm complementarities, this assumptions imposes a one-period restrictio on the degree of path dependence in CEO mobility and revenues deriving from the CEO-firm pair. Bonhomme et al. (2017b) discuss this and mobility assumptions at length in their paper.

B.1.2 Reduced Form and Structural Models

The reduced form model of wage determination with two-sided heterogeneity put forth by Abowd et al. (1999) and used in section 1.4.1 of this chapter relies on important an possibly limited assumptions. By relying on fixed effects to represent individual heterogeneity and therefore leaving it unrestricted, it strongly conditions how this heterogeneity enters the model of wage setting. The model does not allow for complementarities in wages between employee and firm, meaning that on average each employee should behave the same way at each firm in terms of his individual role in wage determination. Abowd et al. (1999) is also a static model, relying on the assumption that employee mobility is random after accounting for employee and current (only) firm type. On the other hand, structural models (Shimer & Smith, 2000; Postel-Vinay & Robin, 2002) improve upon the strict Abowd et al. (1999) model assumptions by accounting for wage dynamics in mobility and match outcomes, but face significant computation challenges as they often rely on the estimation of a very large number of parameters to accommodate the flexibility in model assumptions.

Bonhomme et al. (2017b) present an innovative "hybrid" model which keeps the flavour of a fixed-effects model when accounting for firm-level heterogeneity -by grouping firms into discrete classes- and leaves the employee (CEO, in the case of this chapter) type as random effect represented by a finite mixture model, therefore restricting heterogeneity at the employee level but leaving the CEO-firm complementarities unrestricted[1].

B.1.3 Identification

As argued in section 2.4, identification of this model relies on the comparison of differences in log-revenues resulting from different CEO types who move from firm class k to k'. In this section, I derive a simplified version of the model used by Bonhomme et al. (2017b) to illustrate identification.

Consider the interactive version (equation 2.1) of a two-period model where there are no additional firm covariates X_{it}. Suppose also that neither $a_t(k_{it})$ nor $b_t(k_{it})$ depend on the time period t. There are two job movers between firms of different classes k and k'. In that case, log-revenues in period 1 and 2 for the CEOs who move from firm class k to class k' are given by:

$$\begin{aligned} Y_{i1} &= a(k) + b(k)\alpha_i + \varepsilon_{i1} \\ Y_{i2} &= a(k') + b(k')\alpha_i + \varepsilon_{i2} \\ E[\varepsilon_{it}|\alpha_i, k_{i1} = k, k_{i2} = k', m_{i1} = 1] &= 0 \end{aligned} \tag{B.1}$$

Log-revenues in period 1 and 2 for the CEOs who move from firm class k' to class k are given by:

$$\begin{aligned} Y_{i1} &= a(k') + b(k')\alpha_i + \varepsilon_{i1} \\ Y_{i2} &= a(k) + b(k)\alpha_i + \varepsilon_{i2} \\ E[\varepsilon_{it}|\alpha_i, k_{i1} = k', k_{i2} = k, m_{i1} = 1] &= 0 \end{aligned} \tag{B.2}$$

Given the $E[.] = 0$, the expected value of Y_{it} for both CEO movers who switch from k to k' and vice-versa:

$$\begin{aligned} &E_{kk'}(Y_{i1}) = a(k) + b(k)E_{kk'}(\alpha_i) \qquad E_{kk'}(Y_{i2}) = a(k') + b(k')E_{kk'}(\alpha_i) \\ &E_{k'k}(Y_{i1}) = a(k') + b(k')E_{k'k}(\alpha_i) \qquad E_{k'k}(Y_{i2}) = a(k) + b(k)E_{k'k}(\alpha_i) \end{aligned} \tag{B.3}$$

Therefore,

$$\begin{aligned} \frac{E_{kk'}(Y_{i2})E_{k'k}(Y_{i1})}{E_{kk'}(Y_{i1}) - E_{k'k}(Y_{i2})} &\Leftrightarrow \frac{a(k') + b(k')E_{kk'}(\alpha_i) - ak' - b(k')E_{k'k}(\alpha_i)}{a(k) + b(k)E_{kk'}(\alpha_i) - a(k) - b(k)E_{k'k}(\alpha_i)} \Leftrightarrow \\ &\Leftrightarrow \frac{b(k')[E_{kk'}(\alpha_i) - E_{k'k}(\alpha_i)]}{b(k)[E_{kk'}(\alpha_i) - E_{k'k}(\alpha_i)]} \Leftrightarrow \frac{b(k')}{b(k)} \end{aligned} \tag{B.4}$$

[1]Note that the firm class fixed effect can also be viewed as a different type of random effect, since this model computes a probability distribution of CEO types rather than assign one fixed type to each CEO.

For the model to be identified, it must be that $b(k) \neq b(k')$ which is only verified if $E_{kk'}(\alpha_i) \neq E_{k'k}(\alpha_i)$, meaning that CEO type proportions moving from k and k' must be different from those moving from k' to k. This implies that, in order to separately identify firm and CEO effects on revenues, we need sufficiently many CEOs of different types moving in opposite directions. The empirical test used to verify if $b(k) + b(k') \neq 0$ is to check if $E_{kk'}(Y_{i1} + Y_{i2}) \neq E_{k'k}(Y_{i1} + Y_{i2})$.

B.1.4 Kmeans Clustering

The *kmeans* (MacQueen et al., 1967) clustering is a type of unsupervised learning, which is used when you have unlabelled data (i.e., data without defined categories or groups). The goal of this algorithm is to find groups in the data, with the number of groups represented by the variable K. The algorithm works iteratively to assign each data point to one of K groups based on the features that are provided. Data points are clustered based on feature similarity. The *kmeans* algorithm is one of the simplest unsupervised learning algorithms that solve the clustering problem.

The procedure follows a simple method of classification of a given data set through a certain number of clusters (assuming the number of K clusters is known) fixed *apriori*. The main idea is to define k centers, one for each cluster. The algorithm places initial cluster centers as far away from each other as possible. The next step is to take each point belonging to a given data set that we want to classify and associate it to the nearest center, by computing the Euclidean distance[2]. When no data point is unclassified, the first step is completed. At this point we need to re-calculate k new centroids as barycenter of the clusters classified in the previous step. Step 1 is now repeated to refine the classification. These two steps result in a loop that is finalized when the average distance between each observation and its centroid is minimized.

The *kmeans* clustering mechanism contains a challenge from which I abstract in this chapter. It requires the correct knowledge of the number of underlying discrete heterogeneity classes. There is a large literature dedicated to answering this challenge. In this chapter, I assume prior knowledge in the number of classes and fix it at $K = 10$ for the analysis presented.

B.1.5 Finite Mixture Model

The finite mixture model provides a natural representation of heterogeneity in a finite number of latent classes[3] and translates into modelling a statistical distribution of a certain data sample as a weighted sum of different distributions. The idea behind this type of model, when applied to a matched CEO-firm dataset, is that the distribution of revenues is CEO-firm match type specific; that is, each distribution depends on the latent type of the CEO and the firm.

Experience suggests that usually only few latent classes are needed to approximate density well. I assume $L = 5$, although I experiment with 6 and 7 (without significant changes to the results).

[2]The Euclidean distance is the most commonly used metric. Singh et al. (2013) discuss different distance metrics.

[3]Finite mixture models are also referred to as latent class models or unsupervised learning models.

Appendix: Chapter 3

C.1 Appendix: Experimental Instructions

C.1.1 General

Welcome and thank you for participating in our experiment.

You will receive €2 (two euros) for coming on time and participating. As well, if you follow the instructions correctly, you will be able to each more money for yourself, and also to donate to a Non-Governmental Organization (NGO).
If you wish to ask a question during the remainder of the experiment, please raise your hand in order to do so. Apart from asking questions in this way, you must not communicate with anybody in this room (including talking, looking around or standing up). Please turn off mobile phones and any other electronic devices until the end of the session. You should leave your MP3 player on the table (without turning it on). If you talk, exclaim out loud, etc, you will be asked to leave and you will not be paid. We thank you for your cooperation. Your performance in this experiment will determine your earnings, which will be paid to you in cash at the end of the session (both the €2 participation fee and all proceeds from carrying out the task).
To ensure anonymity, your actions in this session are linked to the computer ID number and at the end of this session you will be paid by ID number.
You will perform a total of 6 tasks. At the end of the experiment, the computer will randomly choose 2 out of those 6 tasks to determine your payoffs. It will pick either 2 out of the tasks 1, 2 and 3, or 2 out of the tasks 4, 5 and 6.

To begin with, the first condition which has been assigned to you will be stated on the screen. Thank you for participating.

C.1.2 Task 1

The task consists of adjusting the location of a set of sliders (see Figure C.1).
Each slider ranges from 0 to 100 and the marker starts on the far left. When looking at a slider, two numbers appear: the one on the left-side, which indicates the target value (e.g. 37 in Figure 3.1) and the one on the right-side, which lets you know where your marker is at each moment (21 in Figure 3.1). Your goal is to position the marker onto the target value, by using the mouse (you can readjust as many times as you wish).

For each slider you complete correctly, your income increases by €0.05. You have 90 seconds per screen to complete a maximum of 30 sliders. After this, a new screen appears with the same number of sliders. There will be a total of 4 screens, totalling 6 minutes. You can keep on completing sliders until the 6 minutes expire. Each round has a maximum time of 90 seconds and your remaining time appears on screen at all times.

In case task 1 is one of the 2 tasks picked by the lottery at the end of the experiment, **the payoffs of this task (1), that is, your number of correct answers multiplied by €0.05, will go entirely to you.**

C.1.3 Task 2

Before beginning this task, you must choose which percentage of the total payoff you get from this task you wish to be assigned to the NGO *Doctors Without Borders* (MSF) in case task 2 is one of the ones picked by the payment lottery. You will have to indicate a number X between 0 and 100. This means that, if the computer randomly chooses task 2 as one of the tasks that matter for payment, ***X%* of the payoffs you accumulate in this task will go to MSF, whereas you will keep the remaining *(100-X)%***.
Task 2 is exactly the same as tasks 1. That is, you'll need to again place the marker of each slider in the target value, and for each correct slider you accumulate €0.05 (of which *X%* is for MSF, and *(100-X)%* for you). You will again have 90 seconds per screen and a total of 4 screens.
Remember you are still working under the same condition that was presented in the screen, before task 1.

C.1.4 Task 3

Task 3 is exactly the same as tasks 1 and 2. That is, you will need to again place the marker of each slider in the target value, and for each correct slider you accumulate €0.05. You will again have 90 seconds per screen and a total of 4 screens.
In case task 3 is one of the 2 tasks picked by the lottery at the end of the experiment, **the payoffs of this task (3), that is, your number of correct answers multiplied by €0.05, will go entirely to an NGO.** In this experiment, if nothing says otherwise, that NGO is MSF.

Remember you are still working under the same condition that was presented in the screen, before task 1.

C.1.5 Task 4

From now onwards, you will be assigned to a new condition. That is, the condition assigned for tasks 1, 2 and 3 no longer matters. In order to assign you a new condition, we will take your preferences into account. We present you with a list of the 5 available conditions which might be given to you, and you will get only one of them. We ask you to order them according to your preferences, choosing, for each of them, a number from 1 to 5, where 1 represents the condition you prefer the most, 2 your second preferred, and so on until 5 (your least preferred). Once you finish and submit your ranking on conditions, a biased lottery will randomly pick one of the 5 conditions that will actually be yours. The lottery will pick the condition in position 1 with 30% probability, the second with 25% probability, third with 20%, fourth with 15% and fifth with 10%. This way, it is more likely that you will be assigned to your preferred condition, so you should order them from most wanted to least wanted. Please refer to Figure C.2 below.

You will be informed of the condition once the lottery has completed the assignment. After this information, task 4 will begin, which is exactly the same as tasks 1, 2 and 3. That is, you will need to again place the marker of each slider in the target value, and for each correct slider you accumulate €0.05. You will again have 90 seconds per screen and a total of 4 screens.
In case task 1 is one of the 2 tasks picked by the lottery at the end of the experiment, **the payoffs of this task (1), that is, your number of correct answers multiplied by €0.05, will go entirely to you.**

Remember you are still working under the same condition that was presented in the screen, before task 1.

C.1.6 Task 5

Before beginning this task, you must choose which percentage of the total payoff you get from this task you wish to be assigned to the NGO *Doctors Without Borders* (MSF) in case task 2 is one of the ones picked by the payment lottery. You will have to indicate a number X between 0 and 100. This means that, if the computer randomly chooses task 2 as one of the tasks that matter for payment, ***X%* of the payoffs you accumulate in this task will go to MSF, whereas you will keep the remaining *(100-X)%*.**
Task 5 is exactly the same as tasks 1, 2, 3 and 4. That is, you will need to again place the marker of each slider in the target value, and for each correct slider you accumulate €0.05 (of which *X%* is for MSF, and *(100-X)%* for you). You will again have 90 seconds per screen and a total of 4 screens.

Remember you are still working under the same condition that was picked by the biased lottery, before task 4.

C.1.7 Task 6

Task 3 is exactly the same as tasks 1 and 2. That is, you will need to again place the marker of each slider in the target value, and for each correct slider you accumulate €0.05. You will again have 90 seconds per screen and a total of 4 screens.
In case task 3 is one of the 2 tasks picked by the lottery at the end of the experiment, **the payoffs of this task (3), that is, your number of correct answers multiplied by €0.05, will go entirely to an NGO.** In this experiment, if nothing says otherwise, that NGO is MSF.

Remember you are still working under the same condition that was picked by the biased lottery, before task 4.

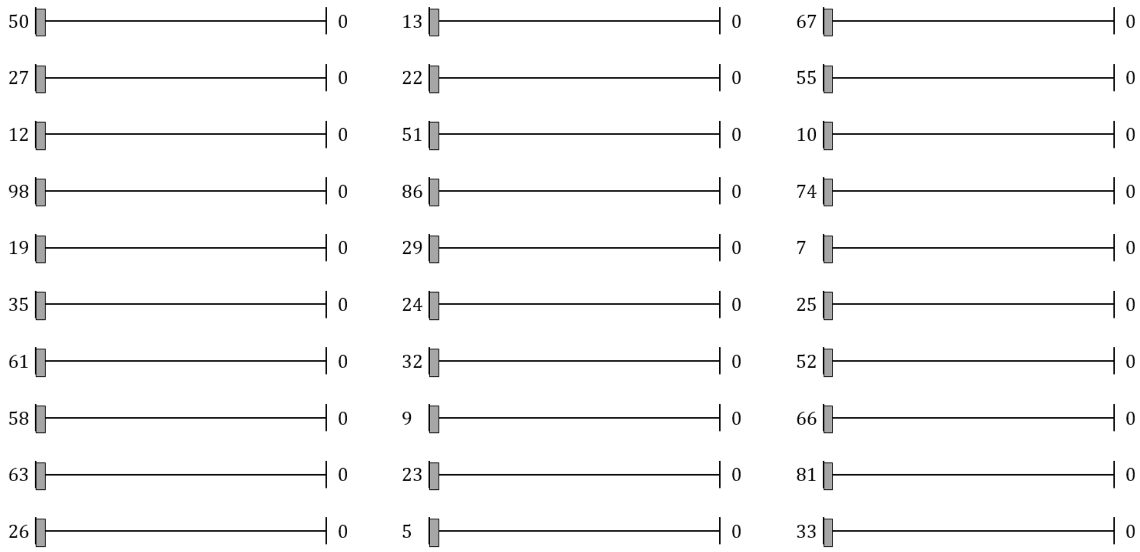
C.2 Appendix: Questionnaire

Please answer the following questions:

1. Gender:
2. Age:
3. In which city were you born?
4. What do you study?
5. Have you had any volunteering experience before?
6. In which organization have you volunteered?
7. How often do/did you volunteer with this organization? Weekly/Monthly/Annualy/Only once
8. What would you say was your motivation to become a volunteer in this organization?
9. Was this motivation fulfilled? Strongly Agree 1 2 3 4 5 6 7 Strongly Disagree
10. How do you think you performed in the task? Below Average 1 2 3 4 5 6 7 Above Average
11. Are you a religious person? Strongly Agree 1 2 3 4 5 6 7 Strongly Disagree
12. What is your religion?

C.3 Appendix: Additional Figures

Figure C.1: Lab Experiment. Screen of the Slider Task.



Notes: Figure C.1 presents a sample computer screen of the task performed by the participants in this lab experiment.

Figure C.2: Lab Experiment. Lottery Probability Distribution.

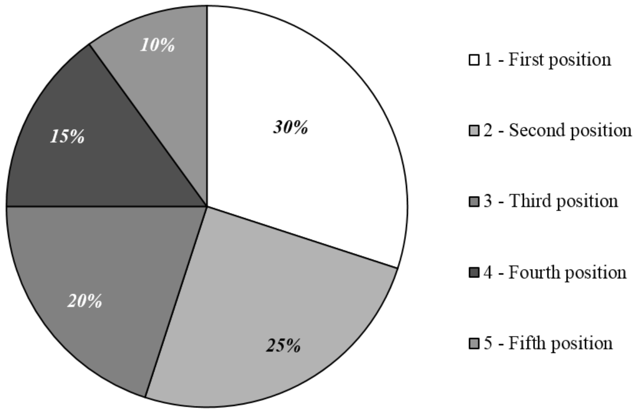


Figure C.3: Lab Experiment. NGO List.

Name of the institution	**Description**	**Area**	**Contact**
The One Boston Fund	Created to help those most seriously affected by the bombings, the One Boston Fund was started by Massachusetts Governor Deval Patrick and Boston Mayor Tom Menino. You can donate online or by mail.	Ayuda a catástrofes	http://www.onefundboston.org/
Médicos Sin Fronteras	It is the largest independent humanitarian, private non-profit emergency medical aid in the world. Its mission is to preserve life and alleviate suffering through respect for dignity, with the goal to restore human beings' choice and autonomy.	Health	http://www.msf.es/
Asociación Española Contra el Cancer	Non-profit organisation which integrates patients, families, volunteers and professionals working together to prevent, raise awareness, accompany people, and fund cancer research projects that will allow for better diagnosis and treatment of cancer.	Health	http://www.aecc.es/
Cruz Roja	Humanitarian organization whose objectives are the pursuit and promotion of peace, national and international cooperation, dissemination and teaching of international humanitarian law, the defense of human rights, aid to the victims in conflict situations, accidents or disasters, attention to all those suffering, promotion and collaboration in actions of solidarity, cooperation for development and social welfare, the development of training for peace, mutual respect and understanding among all people.	Peace	http://www.cruzroja.es/
UNICEF	Nonprofit organization whose goal is to ensure compliance with children's rights to health, education and protection worldwide. Its work is based on the Convention on Children's Rights, the most ratified human rights treaty in the world.	Children and Youth	http://www.unicef.es/
Save the Children	Private, nonprofit, independent and plural institution. Its main objective is the active defense of the interests of children, particularly the most vulnerable and disadvantaged. Main areas: education, health, nutrition, child labour, sexual abuse prevention, reunification of children with their families after disasters and wars, etc.	Children and Youth	http://www.savethechildren.es/
Sonrisas de Bombay	Non Governmental Organisation which focuses in the peaceful struggle against poverty and human rights in the slums of Mumbai. Education, health and socioeconomic development are their primary framework for action.	Children and Youth	http://www.sonrisasdebombay.org/
Aldeas Infantiles SOS	Private, nonprofit, interfaith organisation, independent from any political orientation. Its work focuses on the development of children until they become self-sufficient and well-integrated into society: strengthen vulnerable families, protect children who have been deprived of parental care, support young people in their growth and independence.	Children and Youth	http://www.aldeasinfantiles.es/
World Vision España	Non Governmental Organization for Development performing sustainable development programmess and humanitarian aid through sponsorship of children.	Education	http://www.worldvision.es/
Greenpeace	International Non Governmental Organization, environmentalist, pacifist and independent. Promotes campaigns to change attitudes and habits in order to protect the environment and promote peace.	Environment	http://www.greenpeace.org/espana
Igualdad Animal	International organization dedicated to the defense of the animals currently present in Germany, Spain, India, Italy, Mexico, United Kingdom and Venezuela. They work through advocacy, awareness and research with the aim of promoting changes in society and laws which favour animals.	Animals	http://www.igualdadanimal.org/
Fundación Mujeres	Non-governmental non-profit organisation which works towards implementing intervention projects in the various areas of society, politics, economics and culture, in order to ensure that equality of opportunities is real between genders.	Women	http://www.fundacionmujeres.es/
Manos Unidas	Spanish Non Governmental Organization of volunteers who fight to eradicate hunger and poverty in the world. One of their main objectives is to fund development projects in Africa, Latin America, Asia and Oceania. The other is to sensitize the Spanish population through awareness campaigns.	Hunger and Poverty	http://www.mansunides.org/
OXFAM	Organization which fights alongside disadvantaged populations and as part of a wider global movement, with the aim to eradicate injustice and poverty, and to ensure that all human beings can fully exercise their rights and enjoy a dignified life.	Hunger and Poverty	http://www.intermonoxfam.org/es/
Proyecto Hombre	Organization which helps over 20,000 addicts and their families in Spain. It work three main areas: preventing drug use in different areas, rehabilitation and reintegration of addicts.	Drug Addiciton	http://www.proyectohombre.es/
Comisión Española de Ayuda al Refugiado	Non Governmental voluntary, humanitarian, independent and plural organization. Its aim is to work with migrant citizens to defend their right to asylum.	Immigration	http://www.cear.es/

Notes: This figure presents the list of 18 NGOs available for the subject to choose to whom to donate, in case he was assigned to that condition.