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# Managers and wage policies*

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**Abstract**

We investigate the effects of individual top managers on wages and wage policies. A large longitudinal administrative dataset from Portugal allows us to match workers, firms and top managers, and follow the movements of the latter across different firms over time. We estimate the role of top manager fixed-effects in determining wages and wage policies, while also accounting for the effect of worker and firm heterogeneity. Our results reveal that top managers have a significant influence on wages, the returns to schooling and tenure, the gender wage gap, and the extent of rent sharing. Furthermore, they point to the existence of managerial *styles* in the setting of wage policies. Finally, we relate worker compensation to observable managerial attributes, and find that returns to schooling tend to be higher in firms led by more educated top executives, while longer-tenured managers appear on average to engage in more rent sharing.

**Keywords:** Top managers, wage policies, linked worker-firm-manager data.

**JEL Classification:** D21, M5, J31, J16

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# 1 Introduction

Facilitated by the increased availability of longitudinal matched worker-firm datasets, the empirical analysis of wage determination has undergone important progress in recent years. In the presence of such data it has become possible to estimate, amongst many other things, the worker and firm components of compensation determination, allowing for observable and unobservable factors in both dimensions. As a result, key advances have been made in the study of several classical questions in labour economics − e.g. the source of inter-industry wage differentials, the wage effects of seniority, and the basis of the wage-firm size relationship (Abowd et al., 1999; Abowd and Kramarz, 1999; Abowd et al., 2006).

Worker and firm characteristics are clearly important determinants of compensation. Yet casual observation would suggest that the employment relationship in general and wage policies in particular tend to reflect as well other, perhaps less tangible, factors, such as the preferences and *style* of the firm's top managers. For instance, top executives seem to differ in the extent to which they are willing to engage in rent sharing with their workers. Also, some top managers appear to attribute relatively more importance to formal education, while others seem more prone to rewarding professional experience or seniority. Often, executives are perceived to hold different views on the importance of promoting gender equality in the workplace.

In this paper we study the effects of individual top managers on wages and wages policies. Drawing on unusually rich administrative panel data from Portugal, we are able to match the records of individual workers, firms and top managers, and track the movements of the latter across different firms over time. The conditions are met, thus, to begin tackling several new questions: Can the heterogeneity in wages and wage policies be explained by manager fixed-effects? Is there any evidence of differences in *style* across managers in the setting of wages and wage policies? Is worker compensation systematically associated with managerial observable attributes?

To investigate whether and how manager fixed-effects matter for wage determination, while also accounting for the role of worker and firm − observable and unobservable − heterogeneity, we resort to a simple combination of well known estimators, namely the spell fixed-effects and the least squares dummy variables (LSDV) estimators. Given the longitudinal (and linked) nature of our worker-firm data, the by now standard spell fixed-effects estimator enables us to sweep away worker and firm fixed-effects (Abowd et al., 1999; Andrews et al., 2006). Adding the possibility of tracking the top managers across different firms over time implies that the LSDV estimator may then be used to explicitly estimate manager fixed-effects.

Our results provide evidence that manager fixed-effects are significant determinant of wages, the returns to schooling and tenure, the gender wage gap, and the extent of rent sharing. Adding manager fixed-effects to a wage equation that already controls for the role of worker and firm heterogeneity leads to a significant improvement in the goodness of fit. Furthermore, the analysis of manager effects points to the existence of managerial styles in the setting of wage policies. Top executives who on average engage in more rent sharing also tend to lower the returns to schooling and tenure, suggesting that managers may differ in their preferences towards more egalitarian pay

policies. Finally, we investigate whether and how managerial observable attributes matter for wages and wage policies. Our estimates indicate that returns to schooling tend to be higher in firms led by more educated top executives, and that top managers with more time in office appear on average to engage in more rent sharing.

While this paper is, to the best of our knowledge, the first to explicitly estimate the importance of manager fixed-effects for wage determination, it is clearly not the first to be concerned with the effects of managers on wage policies. Thus far, research in this vein has drawn particular attention to the relation between the gender of managers and the gender wage gap amongst workers. Using worker-firm matched data for Sweden, Hultin and Szultin (2003) find that the gender wage gap tends to increase if males are relatively more represented amongst organisational managers, while Bell (2005), based on individual data for the US, provides evidence that female top managers appear to narrow the wage gap between female and male executives. Also for the US, but using industry-level data, Cohen and Huffman (2007) find that wages of both males and females tend to be lower in industries with a high female representation among managers. Employing the same dataset used in the current paper, Cardoso and Winter-Ebmer (2007) show that wages of female employees tend to be higher, while compensation of male workers tends to be lower, in female-led firms.

Our investigation is also stimulated in part by the creative paper of Bertrand and Schoar (2003) who study the effects of top managers on firm policies by means of an empirical framework which shares many similarities with our own. Using longitudinal matched manager-firm data for the US, they find that manager fixed-effects are an important determinant of a wide range of corporate policies, such as acquisition or diversification decisions, dividend and cost cutting policies, and interest coverage. Additionally, their results point to the existence of general differences in *style* across managers, especially with regard to their approach towards company growth and financial aggressiveness, and suggest that the degree of managerial aggressiveness is systematically associated with observable attributes of top executives, notably age and holding an MBA.[1]

Our paper can also be seen as a contribution to the personnel economics literature, particularly to the strand of research concerned with the design of *optimum* compensation policies within a firm; see Lazear and Oyer (2009) for a recent review. In sharp contrast with such an analysis, our paper is totally free of normative content. Yet our positive analysis of manager wage effects might provide complementary insights to this line of work, notably by quantifying the extent to which there is scope for top managers to influence worker compensation once the role of worker and firm characteristics is accounted for.

A sizeable body of research exists on the drivers of executive pay, and much is by now known about the factors explaining its steep rise in recent years. Key references include Murphy (1999), Bertrand and Mullainathan (2001), Murphy and Zabojnik (2004), Frydman (2005), Cuñat and Guadalupe (2006) and Gabaix and Landier (2008). The current paper is related to this literature but

[1] Using very different data and methods, Bloom and Van Reenen (2007) provide a related analysis on the role of managerial practices on firm performance in the US, France, Germany and the UK.

is not a contribution to it. Our interest lies in the effects of top managers on worker compensation, not in the determinants of their own pay.

The plan of the paper is as follows. In Section 2, we describe the data used in the empirical analysis. Section 3 outlines the empirical strategy for estimating manager fixed-effects and presents the corresponding results. In Section 4, we analyse the magnitude of the manager fixed-effects for wages and wage policies. We then examine if the relationship between manager fixed-effects points to general differences in *style* across managers in the setting of worker compensation. In Section 5, we investigate whether and how wages and wage policies are systematically associated with manager observable attributes. Finally, Section 6 offers some concluding remarks.

## 2 Data

We draw on data from *Quadros de Pessoal* for the period 1995-2005.[2] This is an administrative dataset which gathers information on virtually all workers and firms from the private sector in Portugal. It is the result of a compulsory census run by the Ministry of Employment which covers the population of firms with wage earners in manufacturing and services. Each firm is required to provide information on an annual basis about its characteristics and those of each employee. Firm-level records include information on annual sales, number of employees, industry code, geographical location and date of constitution. The set of worker characteristics includes wages (monthly base wage and other components of pay), gender, schooling, date of starting, occupation and hours worked. The worker and firm records contain unique and time invariant identifiers, allowing to match workers and firms in each year and follow them over time.

An important attribute of these data is the high reliability of the information. Indeed, the data are used by the Ministry of Employment for checking the employer's compliance with labour law. Additionally, Portuguese law makes it compulsory for firms to make this information available to every worker in a public place of the establishment.

Crucially for our purposes, the worker records in *Quadros de Pessoal* contain detailed information on the individual's occupation in the corresponding firm in each year, thereby allowing to directly identify the top managers. Specifically, based on the Portuguese Classification of Occupations (1994 version), the top managers may be identified as the individuals performing the occupations: "corporate directors and chief executives" (code 121) and "directors of small firms" (code 131). Given that the unique and time invariant identifiers in the worker data also apply to the top managers, tracking their movements across different firms over time is straightforward.

We have performed extensive checks to guarantee the accuracy of the employee and firm data, according to the procedures outlined in the Appendix. After these checks, we kept for analysis full-time wage earners working at least 25 hours a week, aged between 16 and 65, earning at least the national minimum wage, employed in manufacturing and services in firms located in mainland Portugal. The resulting panel comprises information on 2,855,549 workers, 415,910 firms and

[2] Except 2001 for which worker data are unavailable.

292,497 top managers, yielding a total of 13,551,045 worker-year observations.[3]

Firms in Portugal may chose among a wide variety of legal settings to regulate their ownership structure, with more than 40 different juridical regimes being available. The vast majority of firms, however, is under one of the following three regimes: "sole proprietorship" (28.1% of firms, 10.4% of workers and 6.0% of worker-year observations in the checked panel), "partnership" (65.4% of firms, 64.9% of workers and 52.5% of worker-year observations) and "public limited company" (3.2% of firms, 36.6% of workers and 32.7% of worker-year observations).[4] Since our identification strategy relies on the movements of the top managers across different firms, and the role of the top executives is likely to be influenced by the firm's ownership structure, we restrict our analysis to "partnerships". The resulting dataset comprises 1,853,423 workers, 271,841 firms and 251,661 (1,061,499 observations) top managers, yielding a total of 7,119,323 worker-year observations for the period 1995-2005.

# 3 Estimation of manager fixed-effects

## 3.1 Construction of the linked worker-firm-manager panel

In line with Bertrand and Schoar (2003), the construction of the linked worker-firm-manager panel data involves the following steps. We begin by restricting our attention to the top managers that we observe in at least two firms over the sample period. This reduces the number of top managers in our sample to 14,800. We then keep only those top managers that stay at least three years in each firm (imposing also that matching worker data are available for each three-year period the top manager is in office in each firm).[5] As a result, the number of top executives is further reduced to 687. Finally, we keep all yearly observations for the firms (and their corresponding workers) in which these top managers are observed (thus including the yearly observations in which the firm is led by top managers that we do not observe in multiple firms). The resulting matched worker-firm-manager dataset includes information on 1,282 firms, 687 top executives and 13,433 workers, yielding a total of 39,840 worker-year observations.

## 3.2 Sample description

Table 1 presents means and standard deviations of the worker and firm variables for two different samples. The information reported in the first two columns refers to the worker-firm-manager sample for the top executives that we observe in multiple firms, after imposing the restrictions described above. The last two columns refer to the total worker-firm-manager data for "partnerships" wherein

[3] The count of worker-year observations excludes top managers.

[4] The original denominations in Portuguese are, respectively, "empresário em nome individual" (code 81), "sociedade por quotas" (code 33) and "sociedade anónima" (code 31). Notice that the sum of the share of workers and firms can be greater than 100% as some firms change their legal setting over time. This does not happen with the share of worker observations.

[5] Worker data may not be available due to the consistency checks and restrictions described in the preceding section.

Table 1: Descriptive statistics: Worker-firm-manager matched data

| | Switchers sample | | Full sample | |
|---|---|---|---|---|
| Variables | Mean | St. Dev. | Mean | St. Dev. |
| Manager length of stay | 4.56 | 0.70 | 4.77 | 3.12 |
| | | | | |
| Firm size (log) | 3.29 | 1.677 | 3.24 | 1.82 |
| Firm age (years) | 16.66 | 15.52 | 16.75 | 14.56 |
| Firm labour productivity (log) | 11.06 | 1.15 | 10.87 | 1.09 |
| | | | | |
| Wage (log of wages in Euros) | 6.53 | 0.49 | 6.46 | 0.44 |
| Male | 0.62 | | 0.60 | |
| Schooling (years) | 8.04 | 3.68 | 7.40 | 3.56 |
| Tenure (years) | 5.05 | 6.17 | 6.41 | 7.23 |
| Age (years) | 35.28 | 10.25 | 35.91 | 10.68 |
| Location | | | | |
| North | 0.28 | | 0.37 | |
| Algarve | 0.06 | | 0.04 | |
| Center | 0.15 | | 0.21 | |
| Lisbon | 0.44 | | 0.32 | |
| Alentejo | 0.07 | | 0.06 | |
| Observations | 39,840 | | 2,178,421 | |

the top managers can be identified and information on three managerial attributes (age, schooling and gender) is reported. The latter sample relates to 238,726 top executives and comprises 119,075 firms and 783,673 workers, yielding a total of 2,178,421 worker-year observations.

In our "switchers" sample, top executives that move across firms stay on average more than four years in a given firm, slightly less than those in the full sample. As in the Bertrand and Schoar (2003), switching top executives tend to work in larger and more productive firms. These firms also pay better on average, and tend to employ more educated and slightly younger workers. In addition, the two samples differ in terms of geographical location: switching executives work mainly in Lisbon, while overall top managers are mainly employed in firms from the North of Portugal.

An interesting feature of the "switchers sample" is that a large proportion of top executives tends to move within rather than across two-digit industries. Indeed, the proportion of moves within industries is 71 percent, well above the 37 and 29 percent reported by Bertrand and Schoar (2003) for CEO and CFO, respectively.

### 3.3 Econometric method

We estimate the effects of top managers on wages through a linear equation of the form:

$$\ln w_{ijt} = \mathbf{x}_{it}\beta + \mathbf{y}_{jt}\gamma + \alpha_i + \psi_j + \lambda_{TM} + \nu_k + \kappa_r + \mu_t + \epsilon_{ijt} \quad (1)$$

where: $\mathbf{x}_{it}$ is a vector of time-varying individual characteristics, $\mathbf{y}_{jt}$ a vector of time-varying characteristics for firm $j$ at which worker $i$ is employed in year $t$; $\alpha_i$ an individual unobserved fixed-effect; $\psi_j$ a pure firm unobserved effect; $\lambda_{TM}$ a top manager fixed effect; $\nu_k$ a pure industry effect; $\kappa_r$ a pure region effect; $\mu_t$ a fixed time effect; and, finally, $\epsilon_{ijt}$ is an exogenous disturbance.

The vector of worker observable characteristics includes gender, age and its square, years of schooling, tenure and a dummy variable indicating whether the worker has less than one year of tenure. The vector of firm characteristics comprises firm size (log of number of employees), age, and firm sales per worker (log of firm annual sales per employee). To control for unobserved pure industry effects, we add a full set of eighteen industry-dummies, corresponding to the economic classification code ($1^{st}$ revision) defined at the 2-digit level for manufacturing and services industries. Region-effects are captured by four dummy variables defined for five regions in mainland Portugal, according to the classification NUTS 2. Wages and firm sales are converted to real terms (2005 prices) using, as is standard in the literature, the CPI and the GDP deflator, respectively.[6]

To account for the role of worker and firm unobserved heterogeneity, we estimate spell (worker-firm) fixed-effects models. By time-demeaning within each unique worker-firm combination (or spell), this estimator sweeps away both $\alpha_i$ and $\psi_j$ (Abowd et al., 1999; Andrews et al., 2006). Manager fixed-effects can then be explicitly estimated by means of the standard LSDV estimator, i.e. a dummy variable for each of the 687 top executives in our "switchers" sample (equal to 1 if the top manager is at the firm and 0 otherwise).

To investigate the effects of top managers on rent sharing, we add an interaction term between $\lambda_{TM}$ and firm sales per worker ($\lambda_{RS}$). This is essentially the same strategy adopted by Bertrand and Schoar (2003) for estimating the effects of top managers on firm policies. Similarly, to study the effects of top managers on returns to schooling and tenure, and on the gender wage gap, we interact $\lambda_{TM}$ with the corresponding worker-level variables (thereby obtaining, respectively, the interaction terms $\lambda_S$, $\lambda_T$, $\lambda_G$).

It is worth noting that since we restrict the analysis to top executives that we observe in at least two firms (for a period of at least three years in each firm), manager fixed-effects will only matter if wages and wage policies are correlated across firms during the period in which the top manager is in office.

### 3.4 Results

In Table 2 we report F-tests, adjusted $R^2$s, the Akaike Information Criterion (AIC), and the Bayesian Information Criterion (BIC) yielded by the estimation of several variants of equation (1). The first row reports the results from a pooled OLS regression which includes only worker and firm time-varying controls, region fixed-effects, industry fixed-effects and time-effects. The specification reported in the second row includes also all these regressors, but is estimated by an individual fixed-effects model. The third row refers to the case where a spell (worker-firm) fixed-effects model is used instead, and constitutes our benchmark for analysing the role of manager fixed-effects

[6] Data on CPI and GDP deflator come from the National Statistics Institute of Portugal.

and interaction terms. Manager fixed-effects are then added to this benchmark specification in the fourth row. The remaining rows report the results yielded by specifications that additionally include interactions terms between manager fixed-effects and *inter alia* schooling, tenure, gender, and firm sales per worker. For each of these specifications, we also report F-statistics from tests of the joint significance of manager fixed-effects and interaction terms.

Table 2. Top executives, wages and wages policies

| Specification | F-tests on TM effects | Adjusted $R^2$ | AIC | BIC |
|---|---|---|---|---|
| (1) OLS | | .4889 | 28,803 | 29,138 |
| (2) = (1) + Indiv. fixed-effects | | .8939 | -50,265 | -49,955 |
| (3) = (1) + Spell fixed-effects | | .9006 | -54,609 | -54,351 |
| (4) = (3) + $\lambda_{TM}$ | $\lambda_{TM}$: 6,144 (<.0001, 169) | .9079 | -58,126 | -56,640 |
| (5) = (4) + $\lambda_S$ | $\lambda_{TM}$: 11,858 (<.0001, 158)<br>$\lambda_S$: 3,810 (<.0001, 170) | .9093 | -59,322 | -57,818 |
| (6) = (5)+ $\lambda_T$ | $\lambda_{TM}$: 47,403 (<.0001, 337)<br>$\lambda_S$: 1.1e+06 (<.0001, 507)<br>$\lambda_T$: 5.1e+05 (<.0001, 653) | .9123 | -60,779 | -55,047 |
| (7) = (6)+ $\lambda_G$ | $\lambda_{TM}$: 1.1e+05 (<.0001, 259)<br>$\lambda_S$: 1.4e+05 (<.0001, 502)<br>$\lambda_T$: 1.3e+05 (<.0001, 651)<br>$\lambda_G$: 48,530 (<.0001, 263) | .9127 | -61,265 | -55,542 |
| (8) = (7)+ $\lambda_{RS}$ | $\lambda_{TM}$: 1,122 (<.0001, 257)<br>$\lambda_S$: 1.4e+05 (<.0001, 510)<br>$\lambda_T$: 92,865 (<.0001, 650)<br>$\lambda_G$: 9,872 (<.0001, 263)<br>$\lambda_{RS}$: 3.7e+05 (<.0001, 652) | .9141 | -63,028 | -57,289 |
| Observations | | | | 39,840 |

Notes: Results are based on the manager-firm-worker "switchers sample" described in sub-section 3.2 and Table 1. Standard errors are clustered at the firm-level. Reported in the second column are F-tests on the joint significance of the top manager fixed-effects and interaction terms. For each F-test we report the value of the F-statistic, the p-value and the number of constraints. The last three columns report the adjusted $R^2$, AIC and BIC from each specification.

The statistical results suggest that top manager fixed-effects are significant determinant of wages and wage policies. Adding manager fixed-effects and manager interaction terms to a wage equation that already accounts for the role of worker and firm − observable and unobservable − heterogeneity (as well as industry, region and time effects) leads to an improvement in the goodness

of fit (irrespective of whether it is measured by the adjusted $R^2$, the AIC, or the BIC). Furthermore, F-tests clearly indicate that we can reject the null hypothesis that each set of manager fixed-effects and manager interaction terms are zero.

Our estimates also shed light on the relative importance of worker, firm and top manager unobserved heterogeneity in explaining worker compensation. In line with previous studies, they suggest that unobserved firm heterogeneity has considerably less explanatory power in determining log wages than unobserved worker heterogeneity does (Abowd et al., 1999; Cornelissen and Hubbel, 2007). An important contribution of this paper is to show that the same conclusion applies to unobserved managerial heterogeneity.

### 3.4.1 Robustness checks

One potential concern with our estimates of manager fixed-effects is that they might not be capturing the active influence of top managers on wages and wage policies. For instance, one might worry that the estimated top manager fixed-effects might reflect in part the spurious correlation of wage policies across firms during the period in which the top managers happen to be in office, rather than their active influence.

We carry out a number of checks to address this concern. First, we assume that each switching top manager in our data joins the second firm three years before the actual year of entry and leaves that firm at the moment of actual entry. The construction of this counterfactual scenario requires that matching worker data for the second firm in those years are available, which reduces the number of switching top managers to 119. We then compare the change in the goodness of fit yielded by the inclusion of such fake manager fixed-effects, with that resulting from the inclusion of the true manager fixed-effects for the same 119 top managers. The corresponding results are reported in the upper part of Table 3. Clearly, the improvement in the goodness of fit yielded by the true manager fixed-effects is greater than in the counterfactual (fake) scenario, suggesting that our estimates are indeed capturing the active influence of top managers on wage policies. As an alternative robustness check, we assume that the each top manager is at the first firm for a period of three years after the actual turnover date (and is absent from the firm before that date). In this case, we are left with information on 304 switching top executives. The corresponding results for the adjusted $R^2$s, AIC and BIC are reported in the lower part Table 3 and, once again, reassure us that our estimates are capturing the active influence of top managers on wage policies.

Table 3. Top executives effects in counterfactual scenarios

| 3 years earlier, $2^{nd}$ firm | True data | | | Counterfactual | | |
|---|---|---|---|---|---|---|
| | Adjusted $R^2$ | AIC | BIC | Adjusted $R^2$ | AIC | BIC |
| (1) Baseline | .9230 | -23,103 | -22,885 | .9230 | -23,103 | -22,885 |
| (2) = (1) $+\lambda_{TM}$ | .9279 | -24,276 | -23,531 | .9253 | -23,664 | -22,873 |
| (3) = (2) $+\lambda_S$ | .9288 | -24,660 | -23,938 | .9260 | -23,979 | -23,219 |
| (4) = (3)+ $\lambda_T$ | .9305 | -25,204 | -24,195 | .9268 | -24,295 | -23,271 |
| (5) = (4)+ $\lambda_G$ | .9307 | -25,355 | -24,347 | .9272 | -24,484 | -23,460 |
| (6) = (5)+ $\lambda_{RS}$ | .9317 | -25,783 | -24,774 | .9275 | -24,753 | -23,714 |
| Observations | 17,286 | | | | | |
| 3 years later, $1^{st}$ firm | True data | | | Counterfactual | | |
| | Adjusted $R^2$ | AIC | BIC | Adjusted $R^2$ | AIC | BIC |
| (1) Baseline | .8550 | -26,927 | -26,682 | .8550 | -26,927 | -26,682 |
| (2) = (1) $+\lambda_{TM}$ | .8680 | -29,034 | -28,015 | .8645 | -28,504 | -27,454 |
| (3) = (2) $+\lambda_S$ | .8710 | -29,829 | -28,849 | .8661 | -29,092 | -28,010 |
| (4) = (3)+ $\lambda_T$ | .8745 | -30,476 | -27,980 | .8687 | -29,586 | -27,067 |
| (5) = (4)+ $\lambda_G$ | .8757 | -30,851 | -28,356 | .8691 | -29,882 | -27,356 |
| (6) = (5)+ $\lambda_{RS}$ | .8782 | -31,730 | -29,211 | .8705 | -30,582 | -28,055 |
| Observations | 19,857 | | | | | |

Notes: Results are based on sub-samples of the manager-firm-worker "switchers sample" described in sub-section 3.2 and Table 1, contructed under the assumption that the period in which each top manager is at one of the firms is different, according to the procedures described in sub-section 3.4.1. For all specifications (including the baseline) the results are obtained from spell fixed-effects panel regressions, where the standard errors are clustered at the firm-level. For each scenario, the three columns report the adjusted $R^2$, AIC and BIC from each specification.

### 3.4.2 The role of firm- and managerial team-size

Intuitively, we might expect the influence of switching top managers to depend on factors such as firm- or managerial team-size. Indeed, it can be argued that larger firms tend to have relatively more rigid wage policies, making it potentially more difficult for top managers to print their own mark. Similarly, it is arguably plausible to speculate that top managers have more room to impose their own style in the setting of wage policies when they integrate a relatively small managerial

team.

Table 4. Top executives effects by firm- and managerial team-size

| Firm size | Less than 10 employees | | | Other | | |
|---|---|---|---|---|---|---|
| | Adjusted $R^2$ | AIC | BIC | Adjusted $R^2$ | AIC | BIC |
| (1) Baseline | .8176 | -17,526 | -17,313 | .9081 | -37,682 | -37,401 |
| (2) = (1) $+\lambda_{TM}$ | .8339 | -18,850 | -18,089 | .9145 | -39,968 | -39,207 |
| (3) = (2) $+\lambda_S$ | .8383 | -19,479 | -18,762 | .9157 | -40,634 | -39,816 |
| (4) = (3)+ $\lambda_T$ | .8460 | -20,059 | -16,964 | .9185 | -41,692 | -39,492 |
| (5) = (4)+ $\lambda_G$ | .8472 | -20,272 | -17,170 | .9189 | -42,005 | -39,813 |
| (6) = (5)+ $\lambda_{RS}$ | .8491 | -21,149 | -18,040 | .9206 | 43,034 | -40,851 |
| Observations | | 11,102 | | | 28,831 | |
| Management team | Majority | | | Other | | |
| | Adjusted $R^2$ | AIC | BIC | Adjusted $R^2$ | AIC | BIC |
| (1) Baseline | .8617 | -17,207 | -16,987 | .9089 | -37,977 | -37,713 |
| (2) = (1) $+\lambda_{TM}$ | .8690 | -17,981 | -17,446 | .9164 | -40,748 | -39,716 |
| (3) = (2) $+\lambda_S$ | .8701 | -18,272 | -17,700 | .9178 | -41,662 | -40,655 |
| (4) = (3)+ $\lambda_T$ | .8734 | -18,616 | -16,820 | .9208 | -42,755 | -39,071 |
| (5) = (4)+ $\lambda_G$ | .8733 | -18,699 | -16,902 | .9212 | -43,143 | -39,466 |
| (6) = (5)+ $\lambda_{RS}$ | .8754 | -17,500 | -44,409 | .9227 | -44,409 | -40,707 |
| Observations | | 11,318 | | | 28,621 | |

Notes: Results are based on the manager-firm-worker "switchers sample" described in sub-section 3.2 and Table 1, split by firm- and managerial team-size as described in sub-section 3.4.2. For all specifications (including the baseline) the results are obtained from spell fixed-effects panel regressions, where the standard errors are clustered at the firm-level. For each TM classification, the three columns report the adjusted $R^2$, AIC and BIC from each specification.

To empirically examine these conjectures, we split our sample according to both these criteria. Specifically, columns (2) to (4) in the upper part of Table 4 consider only the data for the top managers that move across firms with less than ten employees, while columns (5) to (8) comprise all remaining cases. In the lower part of Table 4, columns (2) to (4) consider only top executives

that move across firms in which they represent at least 50 percent of the managerial team, whereas columns (5) to (8) comprise the remainder cases.[7]

While the differences in sample size across groups call for a particularly cautious reading of the results, the evidence presented in Table 4 suggests that firm size is relatively more important for the explanatory power of top manager fixed-effects than the size of the managerial team. Indeed, for a fairly similar number of observations, we find that the improvement in the goodness of fit associated with the inclusion of top manager fixed-effects and interaction terms is relatively more important in the sub-sample of small firms than in the sub-sample of small managerial teams.

# 4 Analysis of manager fixed-effects

## 4.1 Magnitude of manager fixed-effects

We now proceed by examining the magnitude of the estimated manager fixed-effects for wage policies. Table 5 reports descriptive statistics on the distribution of each set of estimated manager fixed-effects, on the basis of specification (8) in Table 2. To account for estimation error, we also compute weighted statistics where each estimated fixed-effect is multiplied by the inverse of its standard error.

Table 5. Size of the effects of top executives on wage policies

| Specification | Weighted | Median | St. Dev. | 10th | 25th | 75th | 90th | N |
|---|---|---|---|---|---|---|---|---|
| Returns to schooling | No | -.003 | .268 | -.185 | -.041 | .032 | .116 | 514 |
| | Yes | .001 | .061 | -.053 | -.018 | .010 | .035 | 514 |
| Returns to tenure | No | -.004 | .053 | -.052 | -.026 | .015 | .047 | 652 |
| | Yes | -.001 | .033 | -.032 | -.011 | .008 | .041 | 652 |
| Gender pay | No | .005 | 1.512 | -.629 | -.127 | .152 | .513 | 266 |
| | Yes | -.002 | .234 | -.111 | -.047 | .075 | .156 | 266 |
| Rent-sharing | No | .009 | .275 | -.193 | -.061 | .105 | .230 | 657 |
| | Yes | -.001 | .143 | -.109 | -.036 | .045 | .135 | 657 |

Notes: The fixed effects used in this table are retrieved from the regressions reported in specification (8) of Table 2. If "Yes" in the second column, the fixed effect is weighted by the inverse of its standard error to account for estimation error.

An inspection of Table 5 reveals that the heterogeneity in the magnitude of top manager fixed-effects is economically important. For instance, the difference between a manager at the twenty-fifth percentile in the returns to schooling distribution and one in the seventy-fifth is 0.07, which compares with an average estimate of returns to schooling in our sample of 0.004 (see Table A1). Also, the difference between the twenty-fifth and seventy-fifth percentile in the distribution of rent

[7] The sum of the number of observations in each of the two groups slightly exceeds the "switchers sample" total because some firms have multiple switching top executives, which sometimes fall in different groups.

sharing is 0.16, against an average estimate of rent sharing in our sample of 0.006. Further, is worth noting that like in Bertrand and Schoar (2003) the median manager fixed-effects for most variables are close to zero, suggesting that our focus on outside hires does not entail significant selection problems.[8]

### 4.2 Management *styles*

Is there any evidence of managerial *styles* in the setting of wage policies? To address this question we analyse relationship between the estimated manager fixed-effects for the various wage policies. In other words, we estimate the following equation:

$$FE(a)_k = \alpha + \beta FE(b)_k + \mu_k \tag{2}$$

where $k$ indexes top managers and $FE(a)$ and $FE(b)$ are the estimated interaction terms between manager fixed-effects and any two worker (or firm) variables from specification (8) in Table 2. Following Bertrand and Schoar (2003), we use a GLS estimation technique to account for the measurement error in the right-hand-side variable, by weighing each observation by the inverse of the standard error on the independent variable (obtained from the first step estimations).

Table 6. Relationship between the top manager fixed-effects

| | Schooling | Gender | Tenure |
|---|---|---|---|
| Rent-sharing | -.559*** | .133** | -1.099*** |
| | (.142) | (.064) | (.226) |
| Schooling | | -.426*** | -.058 |
| | | (.063) | (.187) |
| Gender | | | -.451 |
| | | | (.738) |

Notes: Significance levels: ** : 5% *** : 1%.

The results reported in Table 6 point indeed to the existence of managerial styles in the setting of wage policies. Top executives who on average engage in more rent sharing also tend to lower the returns to schooling and tenure, which seems to support the view that top managers differ in their preferences towards more egalitarian pay policies. Additionally, our results suggest that top managers who tend to increase the returns to schooling appear also more prone to narrowing the gender wage gap.

## 5 Observable managerial attributes

Do managerial observable attributes matter for wages and wage policies? This is the question we set out to address in this section. In particular, we investigate the extent to which there is a

[8] For further discussion on this matter see Bertrand and Schoar (2003) pp. 1191.

systematic association between wages and wage policies, on the one hand, and the age, gender, schooling and tenure of the firm's top managers on the other hand.

### 5.1 Econometric method

To investigate the effects of top manager observable characteristics on wages, we estimate a linear equation of the form

$$\ln w_{ijt} = \mathbf{x}_{it}\beta + \mathbf{y}_{jt}\gamma + \mathbf{z}_{jt}\delta + \alpha_i + \psi_j + \nu_k + \kappa_r + \mu_t + \epsilon_{ijt} \tag{3}$$

where: $\mathbf{z}_{jt}$ is a vector of average observable attributes of the top managers at firm $j$ in year $t$, and the remaining variables have the meaning defined above. $\mathbf{z}_{jt}$ includes the proportion male top managers in firm $j$ in year $t$, and the average age, schooling and tenure of all top executives at the firm in each year. Tenure is defined as the number of years the individual has been at the firm in the capacity of top manager.

As before, we estimate spell (worker-firm) fixed-effects models to account for worker and firm unobserved heterogeneity. Therefore, identification comes from the within-spell variation in average top manager attributes over time. To study the effects of top manager observable attributes on wage policies, we interact each of these variables with firm and worker characteristics, namely sales per worker, schooling, tenure and gender. Conceptually, this is the same empirical framework used by Bertrand and Schoar (2003) to investigate the effects of CEO observable attributes on firm policies.

It is worth noting that our identification strategy no longer relies on the ability to follow the top managers across different firms over time. Rather, it is based on changes over time in the average characteristics of the firms' top managers, which can result from both internal and external hirings. Accordingly, our econometric analysis is based on the full longitudinal matched dataset for "partnerships", as defined in Section 3.2.

### 5.2 Results

Table 7 reports the regression results. Column (1) presents the effects of managerial attributes on wages, while column (2) includes as well the interaction terms between each managerial observable attribute and the worker and firm variables of interest. The econometric results do not show a direct impact of changes in managerial observable attributes on wages. They do suggest, however, that top manager observable characteristics matter for wage policies. Managerial teams composed of more educated top executives appear on average to increase the returns to schooling and tenure, and reduce the extent of rent sharing. All else equal, top managers with more time in office appear on average to engage in more rent sharing and increase the returns to schooling. Finally, our results suggest that younger and female top executives are relatively more prone to rewarding formal education.

Table 7. Top manager observable attributes, wages and wage policies

| Effects on | TM attributes | (1) | (2) |
|---|---|---|---|
| Wages | Average education | .0001 | -.001 |
| | | (.0007) | (.003) |
| | Average age | .0003 | .002 |
| | | (.0002) | (.001) |
| | Average tenure | -.002 | -.045*** |
| | | (.001) | (.003) |
| | Proportion of male | .0007 | .034 |
| | | (.005) | (.023) |
| Returns to schooling | Average education | - | .0005*** |
| | | | (.0001) |
| | Average age | - | -.00007* |
| | | | (.00004) |
| | Average tenure | - | .002*** |
| | | | (.00008) |
| | Proportion of male | - | -.003*** |
| | | | (.001) |
| Returns to tenure | Average education | - | .0002** |
| | | | (.00007) |
| | Average age | - | 2.59e-06 |
| | | | (.00002) |
| | Average tenure | - | -.0002*** |
| | | | (.00005) |
| | Proportion of male | - | -.0002 |
| | | | (.0004) |
| Gender pay | Average education | - | .0004 |
| | | | (.0009) |
| | Average age | - | -.0004 |
| | | | (.0003) |
| | Average tenure | - | .003*** |
| | | | (.0005) |
| | Proportion of male | - | -.005 |
| | | | (.007) |
| Rent-sharing | Average education | - | -.0004* |
| | | | (.0002) |
| | Average age | - | -.00006 |
| | | | (.00008) |
| | Average tenure | - | .003*** |
| | | | (.0003) |
| | Proportion of male | - | -.0005 |
| | | | (.002) |
| $F$-statistic | | 271.74 | 284.85 |
| P-value | | .000 | .000 |
| Observations | | 2,178,421 | 2,178,421 |

Notes: Results are based on the manager-firm-worker "full sample" described in sub-section 3.2 and Table 1. They are obtained from spell fixed-effects panel regressions. The reported coefficients in each column are from a unique regression, as described in section 5.1. Standard errors are in parenthesis, and are clustered at the firm-level. Significance levels: $*$ : 10% $**$ : 5% $***$ :1%.

# 6 Concluding remarks

In this paper we have investigated the effects of top managers on wages and wage policies. Drawing on longitudinal matched worker-firm-manager data from Portugal, we were able to track the top managers across different firms over time, and study the role of top manager fixed-effects in explaining wages, the extent of rent sharing, the returns to schooling and tenure, and the gender wage gap. Our results indicate that manager fixed-effects are a significant determinant of wages and wage policies, and point to the existence of general differences in managerial *style* in the setting of worker compensation. Additionally, they provide evidence that observable attributes of top executives, notably age, schooling and tenure, are systematically related to wage policies.

By way of conclusion it should be emphasised that while the ability to track the top managers across different firms is a key ingredient of our empirical framework, top executives are clearly not randomly assigned to firms. Accordingly, our estimates of manager fixed-effects should not be interpreted as the *causal* impacts of top managers on wages and wage policies, but rather as evidence that there is a systematic association between the identity of the top managers and worker compensation.

## Appendix

### A.1. Longitudinal linked employer-employee dataset

#### Checks on the consistency of data

After deleting observations for which the worker identification code was invalid or missing, the initial worker panel comprises 5,462,513 workers and 23,819,162 worker-year observations. Inconsistencies were identified if the worker gender or date of birth was reported changing, or the highest schooling level achieved by a worker was reported decreasing over time. In line with Cardoso (2006), the following procedures were implemented to correct such inconsistencies:

(i) *Dealing with missing values when reported data for the rest of the periods was absolutely consistent.* Whenever the gender, age or education of an individual was reported in a consistent way but missing in some year(s), we have assigned the reported value to the missing observation. These corrections affected 0.00 percent, 0.31 percent and 0.01 percent of the observations in the initial panel, respectively, for gender, age and schooling.

(ii) *Dealing with inconsistent data on gender, birth date or schooling over time.* When information was reported inconsistently over time, the information reported more than half of the times has been taken as the correct one. Inconsistent values on gender were replaced, after checking that the date of birth in the observation to be corrected was the same as the most frequently reported date of birth for that worker. A similar procedure was followed for the birth date and education, replacing inconsistent values with that reported more than half of the times. According to this procedure, 0.73 percent, 4.89 percent and 5.32 percent of the observations in the initial panel have been corrected for gender, birth date and education, respectively. All information on a worker was dropped in case of remaining inconsistencies after the implementation of the previously described corrections. This led to dropping 7.48 percent of the observations in the initial panel due to inconsistencies for gender, 2.41 percent for age and 5.88 percent for education.

(iii) *Deleting data on workers with remaining missing data on gender, age or schooling.* Workers with missing data after the implementation of the previous corrections were dropped. This led to dropping 0.03 percent of the observations in the initial panel due to missing age and 0.89 percent due to missing data on schooling. The checked panel included 19,848,753 observations, regarding 3,368,398 workers, 303,725 top managers and 511,754 firms.

#### Constraints imposed

(i) *Keeping full time workers, aged between 16 and 65 years old, earning at least the national minimum wage.* Only full-time workers working at least 25 hours a week, aged between 16 and 65 years old, earning at least the national minimum wage were kept for the analysis (the national minimum wage constraint might imply dropping workers in particular categories, such as apprentices and workers aged less than 18 years old). These restrictions led to dropping, respectively, 7.11, 9.60, 0.39 and 4.06 percent of the observations in the checked panel.

(ii) *Keeping observations from manufacturing and services.* After the previous constraints, the worker panel includes 14,394,425 worker-year observations, 3,019,005 workers, 303,725 top managers and 447,307 firms. We then merged the worker data with firms operating in manufacturing and services. After further keeping data only from mainland Portugal, the final worker-firm panel gathers information on 2,855,549 workers (13,551,045 observations), 292,497 top-managers, 415,910 firms for the years 1995 to 2005, yielding a total of 14,787,805 observations.

## A.2 Regression results for the baseline specifications

Table A1. Baseline results for the two samples of top executives

| Variables | (1) | (2) |
|---|---|---|
| Schooling | .004 | .003*** |
| | (.003) | (.0003) |
| Age | .048*** | .045*** |
| | (.003) | (.0004) |
| Age squared | -.0003*** | -.0003*** |
| | (.00003) | (4.45e-06) |
| Tenure | .002 | .0007*** |
| | (.002) | (.0003) |
| Tenure less than 1 | -.016 | -.011*** |
| | (.004) | (.0006) |
| Firm age | - | - |
| Firm labour productivity (log) | .006*** | .010*** |
| | (.001) | (.0003) |
| Firm size (log) | .059*** | .070*** |
| | (.005) | (.001) |
| North | -.067 | .024** |
| | (.045) | (.008) |
| Algarve | - | -.022 |
| | | (.018) |
| Center | -.079* | .002 |
| | (.041) | (.007) |
| Lisbon | -.047* | .014** |
| | (.026) | (.006) |
| Industry effects | Yes | Yes |
| Year effects | Yes | Yes |
| $F$ statistic | 56.02 | 2,154.31 |
| P-value | .000 | .000 |
| Observations | 39,840 | 2,178,421 |

Notes: Results are based on the manager-firm-worker "switchers" and "full" samples described in sub-section 3.2 and Table 1. They are obtained from spell fixed-effects panel regressions, where the standard errors in parenthesis are clustered at the worker-level. Significance levels: $*$ : 10% $**$ : 5% $***$ : 1%.

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