Do Female Executives Make a Difference? The Impact of Female Leadership on Gender Gaps and Firm Performance*

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May 4, 2018

Abstract

We investigate the effects of female executives on gender-specific wage distributions and firm performance. Female leadership has a positive impact at the top of the female wage distribution and negative at the bottom. The impact of female leadership on firm performance increases with the share of female workers. We account for the endogeneity induced by non-random executives' gender by including firm fixed-effects, by generating controls from a two-way fixed effects regression, and by using instruments based on regional trends. The findings are consistent with a model of statistical discrimination where female executives are better at interpreting signals of productivity from female workers. This suggests substantial costs of women under-representation among executives.

JEL Codes: M5, M12, J7, J16. Keywords: executives' gender, gender gap, firm performance, glass ceiling, statistical discrimination

1 Introduction

This paper investigates how female executives affect firm-level wage distributions and firm performance using a unique matched employer–employee panel dataset representative of the Italian manufacturing sector.

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A growing literature shows that executives' characteristics, such as age, education, confidence, and attitudes towards risk can have an effect on management practices and firm outcomes.[1] We focus on one of these characteristics: gender. Our focus on gender follows from the abundant evidence of systematic gender differentials in the labor market.[2] With respect to executives, research has highlighted that women are almost ten times less represented than men in top positions in firms.[3] For example, recent U.S. data show that even though women are a little more than 50% of white collar workers, they represent only 4.6% of executives.[4] Our own Italian data show that about 26% of workers in the manufacturing sector are women compared with only 3% of executives and 2% of CEOs. These facts suggest the importance of studying the relatioshipn between women's under-representation among executives and firms' outcomes.

This paper provides four contributions to the existing literature. First, we develop a theoretical framework highlighting a potential channel of interaction between female executives, female workers, wage policies, job assignment, and overall firm performance. The model implies efficiency costs of women's under-representation in leadership positions, and generates original empirical predictions. Second, we investigate the relationship between female leadership and the gender-specific wage distributions at the firm level. Unlike most of the previous literature, our theoretical framework leads us to focus not on the effects at the mean but on the differential impacts at different points of the wage distribution. Third, we investigate the relationship between female leadership and firm performance, focusing on the interaction between female leadership and gender composition of the workforce. We concentrate

[1]Bloom and Van Reenen (2007) is one of the first contributions emphasizing differences in management practices. See also a recent survey in Bloom and Van Reenen (2010). A growing literature showing the effects of CEO characteristics follows the influential paper of Bertrand and Schoar (2003). Among recent contributions, see Bennedsen et al. (2012), Kaplan et al. (2012), or Lazear et al. (2012). For research on executives' overconfidence, see Malmendier and Tate (2005). For theoretical contributions, see for example Gabaix and Landier (2008) and Tervio (2008). For contributions focusing on both executive and firm characteristics, see Bandiera et al. (2011) and Lippi and Schivardi (2014).

[2]For an overview of the gender gap in the U.S. labor market in the last twenty years, see Blau and Kahn (2004), Eckstein and Nagypal (2004) and Flabbi (2010).

[3]Evidence from U.S. firms is based on the Standard and Poor's ExecuComp dataset, which contains information on top executives in the S&P 500, S&P MidCap 400, and S&P SmallCap 600. See for example, Bertrand and Hallock (2001), Wolfers (2006), Gayle et al. (2012), Dezsö and Ross (2012). The literature on other countries is quite thin: see Cardoso and Winter-Ebmer (2010) (Portugal), Ahern and Dittmar (2012) and Matsa and Miller (2013) (Norway), Smith et al. (2006) (Denmark), Gagliarducci and Paserman (2015) (Germany), and Flabbi et al. (2016) (Latin America). A related literature is concerned with under-representation of women at the top of the wage distribution, see for example Albrecht et al. (2003). Both phenomena are often referred to as a *glass-ceiling* preventing women from reaching top positions in the labor market.

[4]Our elaboration on 2012 Current Population Survey and ExecuComp data.

on indicators of firm performance that are less volatile and closer to firm productivity than the financial performance measures typically used by previous literature. Finally, we perform a series of partial-equilibrium counterfactual exercises to compute the cost of women's under-representation in top positions in organizations.

In Section 2 we present our theoretical framework and derive its empirical implications. Our model extends the seminal statistical discrimination model of Phelps (1972) to include two types of jobs, one characterized by complex tasks and the other by simple tasks, and two types of CEOs, male and female. Based on a noisy ability signal, CEOs assign workers to jobs and wages. We assume that CEOs are better (more accurate) at reading signals from workers of their own gender.[5] We also assume that complex tasks require more skills to be completed successfully so that it is optimal to assign more skilled workers to complex tasks. After defining the equilibrium generated by this environment, we describe the empirical implications of a woman replacing a man as a firm's CEO. Thanks to the more precise signal they receive from female workers, female CEOs reverse the statistical discrimination suffered by women, adjusting their wages and reducing the mismatch between female workers' productivity and job requirements. The model delivers two sharp empirical implications:

1. Female workers at the top of the wage distribution receive higher wages when employed by a female CEO than when employed by a male CEO. Female workers at the bottom of the wage distribution receive lower wages when employed by a female CEO. The impact of female CEOs on the male workers' wage distribution has the opposite signs: negative at the top and positive at the bottom of the distribution.

2. The performance of a firm led by a female CEO increases with the share of female workers.

These results follow from the assumption that female CEOs are better at processing information about female workers' productivity. Therefore, wages of females employed by female CEOs are more sensitive to individual productivity, delivering the first implication. Moreover, female CEOs improve the allocation of female workers across tasks, delivering the second implication.

Our data, described in detail in Section 3, include all workers employed by firms with at least 50 employees in a representative longitudinal matched employer-employee sample of Italian manufacturing firms observed between 1982 and 1997.[6] Because we observe all workers and their compensation, we can evaluate the impact of female leadership on the wage distribution at the firm level. Moreover, we can

[5] We discuss this assumption in Subsection 2.3.

[6] This is the only available period, and the reason why we cannot provide an analysis with more recent data.

obtain a precise measurement of the leadership at the firm and assess the proportion of women among its ranks. Because the data set is rich in firm-level characteristics, we can compute several measures of firm performance: sales per worker, value added per worker, and Total Factor Productivity (TFP). Finally, since we can merge this sample with social security data containing the complete labor market histories of all workers who ever transited through any of the firms in the sample, we can identify firm, worker and executive fixed effects that help in dealing with the endogeneity issues affecting the regressions of interest.

We describe our empirical strategy and present the estimation results in Section 4. We analyze the impact of female leadership on wages of male and female workers allowing for heterogenous effects across the distribution. We also study the impact of female leadership on firm performance allowing for its interaction with the gender composition of the firm's workforce. Assessing the impact of female executives on any firm-level outcome faces a fundamental identification problem: executives are endogenously matched with firms. A typical strategy used by previous literature to reduce the bias has been to focus only on within-firm variation. We use the same firm fixed-effects strategy but we add additional controls for both observed and unobserved heterogeneity of the executives and of the non-executive workforce at the firm. Unobserved heterogeneity controls are obtained from a first step two-way fixed effect wage regression that we can estimate thanks to the large number of complete labor market histories we observe in our data. To address the potential endogeneity induced by unobservable time-varying firm characteristics, we also propose an instrumental variable (IV) strategy. The identifying assumption is that aggregate trends in the proportion of female executives at the regional level are exogenous with respect to the time-varying firm-level heterogeneity we are concerned about.

Our results show that the impact of female executives is positive on women at the top of the wage distribution but negative on women at the bottom of the wage distribution. We estimate the opposite effect on men. As a result, we find that female leadership at the firm reduces the gender wage gap at the top of the wage distribution and widens it at the bottom, with essentially no effects on average.

We estimate that the impact of female leadership on firm performance – as measured by sales per worker, value added per worker, and total factor productivity (TFP) – is a positive function of the proportion of female workers employed by the firm. The magnitude of the impact is substantial: a female CEO would increase overall sales per employee by about 3.7% if leading a firm employing a proportion of women equal to the average in the sample (about 20%.) All results are robust to the use of our IV strategy and to an additional set of robustness exercises dealing with the selection induced by entry and exit of firms and workers, the definition of female leadership, the specific empirical specification, and the measure of firm performance.

Using our estimates, we perform a partial equilibrium counterfactual exercise to compute the cost of women's under-representation in leadership positions. The effects of increasing the share of female-led firms critically depends on how female CEO are assigned to firms. If they are randomly scattered across firms, productivity increases by little or, in some cases, it even decreases. If instead, female CEOs are assigned to all firms with at least a 20% proportion of female workers (about 50 percent of the sample), sales per worker would increase by 14% in the "treated" firms, and by 6.7% in the overall sample of firms. Our findings have implications for both firm policy and public policy. Specifically, the mechanism that we highlight implies that firms with a substantial share of women in their workforce would benefit the most from appointing women to managerial or executive positions. In terms of public policy, our findings suggest that gender quotas might not generate positive effects on average, particularly in the short term. Gender quotas are most effective when targeting firms that employ a significant proportion of female employees.

Even if our theory provides a unified explanation for all the estimation results we obtain, we discuss in Section 5 other relevant theories of gender inequality that can plausibly explain at least some of them. We focus on two classes of explanations: taste-based discrimination models and workers' gender-specific complementarities. Among the second class of explanations, we find that mentoring (Athey et al., 2000) is consistent with all of our most precisely estimated results and is at odds with only a subset of our least precisely estimated results. We conclude that, although the evidence lines up more precisely with the mechanism we propose, additional work is needed to precisely disentangle the source of gender-specific complementarities.

There is a large literature studying gender differentials in the labor market, and a fairly developed literature studying gender differentials using matched employer-employee data. However, the literature on the relationship between the gender of the firm's executives and gender-specific wages is scant and has focused on the effect on average wages, not on the effect over the wage distribution. Bell (2005) and Albanesi et al. (2015) study the impact of female leadership in US firms but only on *executives'* wages. Cardoso and Winter-Ebmer (2010) consider the effect on all workers in a sample of Portuguese firms but without allowing for heterogeneous effects over the wage distribution. Fadlon (2015) tests a model of statistical discrimination similar to ours and assesses the impact of supervisors' gender on workers' wages using U.S. data but does not focus on the wage distribution and does not look at wages at the firm level. Gagliarducci and Paserman (2015) use German linked employer-employee data to study the effect of female leadership on establishments and workers outcomes. They find that, controlling for firm fixed-effects, no significant association exists between outcomes and female leadership. However, they do not look at impacts at different points of the wage distribution. Lucifora and Vigani (2016) study the impact of the gender of the immediate supervisor on worker outcomes for a large set of European countries. They focus on self-reported discrimination and they estimate that female supervisors reduce the gender gap. However, their data do not allow for a firm fixed effects approach and the results are from cross-sectional data with some controls for selection. A related literature, sparked by recent reforms in European countries, looks at the impact of the gender of a firm's board members instead of its executives. Bertrand et al. (2014) show that a reform mandating gender quotas for the boards of Norwegian companies reduced the gender gap in earnings within board members but did not have a significant impact on overall gender wage gaps.

Existing literature on the effect of female leadership on firm performance is also limited. Most of the contributions focus on stock prices, stock returns and market values as measures of firms performance[7] and on the proportion of women among board members as measure of female leadership.[8] Instead, we propose measures of firm performance that are less volatile and closer to actual firm productivity, and measures of female leadership relating more directly to the day-to-day running of the firm (female CEO and female proportion of women among top executives).[9]

2 Theoretical Framework

We present a simple signal extraction model where inequalities are generated by employers' incomplete information about workers' productivity and where employers' gender matters. Crucially, we assume that executives are better equipped at assessing the skills of employees of the same gender. As discussed in greater detail below, this may be the result of better communication and better aptitude at interpersonal relationships among individuals of the same gender, of more similar cultural background shared by individuals of the same gender, or other factors. From the model we derive a set of implications that we test in our empirical analysis.

[7]See for example, Wolfers (2006), Albanesi and Olivetti (2009), Ahern and Dittmar (2012); in the strategy literature, Dezsö and Ross (2012), Adams and Ferreira (2009), Farrell and Hersch (2005).

[8]See for example, Matsa and Miller (2013), which looks at operating profits, and Rose (2007), which looks at Tobin's Q.

[9]Among the few contributions looking at the impact of female executives on non-volatile measures of firm performance are Smith et al. (2006), with information on value added and profits from a panel of Danish firms and Gagliarducci and Paserman (2015), with information on business volume and investment from a sample of German linked employer-employee data.

2.1 Environment

We extend the standard statistical discrimination model in Phelps (1972) to include two types of employers (female and male), and two types of jobs (simple and complex). The two-jobs extension allows us to obtain implications for efficiency (productivity), which is one focus of our empirical analysis.[10] Female (f) and male (m) workers have ability q which is distributed normally with mean μ and variance σ^2. Ability, productivity and wages are expressed in logarithms. CEOs observe a signal of ability $s = q + \epsilon$, where ϵ is distributed normally with mean 0 and variance $\sigma^2_{\epsilon g}$ where g is workers' gender m or f. The signal's variance can be interpreted as a measure of the signal's information quality. Employers assign workers to one of two jobs: one requiring complex (c) tasks to be performed and the other requiring simple tasks (e) to be performed. To capture the importance of correctly assigning workers to tasks, we assume that mismatches are costlier in the complex job, where workers with higher ability are more productive. One way to model this requirement is by assuming that the dollar value of workers' productivity in the complex (simple) job is h (l) if workers have ability $q > \overline{q}$, and $-h$ ($-l$) otherwise, with $h > l \geq 0$.[11]

Firms compete for workers and maximize output given wages. Workers care only about wages and not about job assignment *per se*.

2.2 Homogenous CEOs

It is helpful to start the analysis by exploring the effect of the worker's signal precision on labor market outcomes when all CEOs are of the same gender, let us assume male; in subsection 2.3 we will extend the environment to include female CEOs.

Firms' competition for workers implies that in equilibrium each worker is paid his or her expected marginal product, which depends on her expected ability $E(q|s)$. Standard properties of the bivariate normal distribution[12] imply that $E(q|s) = (1 - \alpha_g)\mu + \alpha_g s$, where $\alpha_g = \sigma^2/(\sigma^2_{\epsilon g} + \sigma^2)$. Expected ability is a weighted average of the population average skill and the signal, with weights equal to the relative variance of the two variables. When the signal is perfectly informative ($\sigma_{\epsilon g} = 0$), the population mean is ignored; when the signal is pure noise ($\sigma_{\epsilon g} = \infty$), expected ability is equal to the population average. With a partially informative signal, the conditional expected

[10] In the standard model of Phelps (1972) discrimination has a purely redistributive nature. If employers were not allowed to use race as a source of information, production would not increase, but this is due to the extreme simplicity of the model. See Fang and Moro (2011) for details.

[11] The threshold rule for productivity is a strong assumption, which we adopted to simplify the derivation of the model's outcome, but it is not crucial. What is crucial is that productivity increases with ability, and that lower ability workers are more costly mismatched in the complex job.

[12] See Eaton (1983)

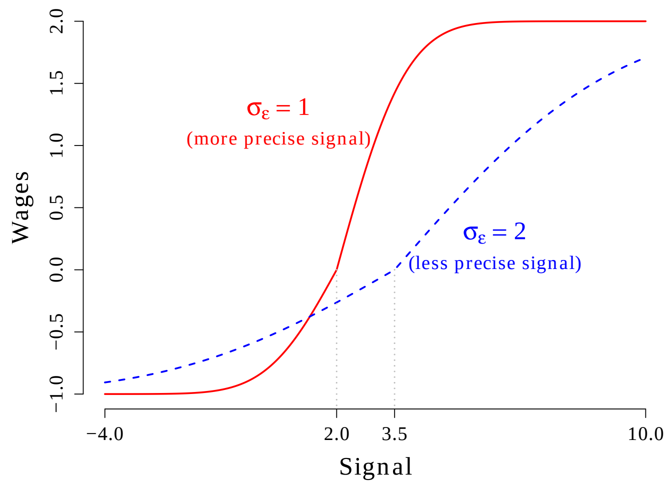

Figure 1: Simulation of the solution to the problem with parameters σ=1, $\overline{q} = 1.5$, $\mu = 1, h = 2$, $l = 1$.

ability is increasing in both q and s.

The conditional distribution, which we denote with $\phi_g(q|s)$ is also normal, with mean equal to $E(q|s)$ and variance $\sigma^2(1-\alpha_g)$, $g = \{m, f\}$. Denote the corresponding cumulative distributions with $\Phi_g(q|s)$.

It is optimal for employers to use a cutoff job assignment rule: workers will be employed in job c if $s \geq \overline{s}_g$. The cutoff $\overline{s}_g$ is computed by equating expected productivity in the two jobs, as the unique solution to

$$h\left(\Pr(q \geq \overline{q}|s, g) - \Pr(q < \overline{q}|s, g)\right) = l\left(\Pr(q \geq \overline{q}|s, g) - \Pr(q < \overline{q}|s, g)\right). \tag{2.1}$$

We denote this solution with $\overline{s}(\sigma_{\epsilon g})$ to stress its dependence on the signal's precision. The worker with signal $\overline{s}(\sigma_{\epsilon g})$ has the same expected productivity (zero) in both jobs.[13] Competition ensures that wages w are equal to expected marginal products, which are functions of the signal received and the worker's gender:

$$w(s; \sigma_{\epsilon g}) = \begin{cases} l\left(1 - 2\Phi_g(\overline{q}|s)\right) & \text{if } s < \overline{s}_g \\ h\left(1 - 2\Phi_g(\overline{q}|s)\right) & \text{if } s \geq \overline{s}_g \end{cases}. \tag{2.2}$$

We now explore the properties of the wage schedule as a function of the signal's

[13]Equation 2.1 is satisfied when $\Pr(q \geq \overline{q}|s, g) = 1/2$ because of the extreme symmetry of the setup. This implies also that expected productivity is zero for workers with signal equal to the threshold. This can be relaxed: all that is needed to obtain our qualitative implications is that productivity increases with ability, and a comparative advantage to place higher ability workers in the complex job.

noise variance $\sigma^2_{\epsilon g}$. Figure 1 displays the outcome for workers with two different values of $\sigma^2_{\epsilon g}$. The red continuous line displays the equilibrium wages resulting from a more precise signal than the blue dashed line. As standard in statistical discrimination models, the line corresponding to the more precise signal is steeper than the line corresponding to the less precise signal. This is the direct implication of putting more weight on the signal in the first case than in the second. As a result of one of our extensions - the presence of job assignment between simple and complex jobs - the two lines also display a non-standard feature: a kink in correspondence to the threshold signal. The kink is a result of the optimal assignment rule: workers with signals below the threshold are assigned to the simple job, where ability affects productivity less than in the complex job, therefore both wage curves are flatter to the left of the thresholds than to the right of the thresholds.

The following proposition states that the expected marginal product of a worker is higher when the signal is noisier if the signal is small enough. Conversely, for a high enough signal, the expected marginal product is lower the noisier the signal.

Proposition 1. *Let $w(s;\sigma_{\epsilon g})$ be the equilibrium wage as a function of the workers' signal for group g, extracting a signal with noise standard deviation equal to $\sigma_{\epsilon g}$. If $\sigma_{\epsilon f} > \sigma_{\epsilon m}$ then there exists $\widehat{s}$ such that $w(s;\sigma_{\epsilon f}) > w(s;\sigma_{\epsilon m})$ for all $s < \widehat{s}$ and $w(s;\sigma_{\epsilon f}) < w(s;\sigma_{\epsilon m})$ for all $s > \widehat{s}$.*

The proof is in Web Appendix A.[14] The next proposition states that productivity is higher when the signal is more precise. This follows observing that expected ability is closer to the workers' signal when $\sigma^2_{\epsilon g}$ is smaller.

Proposition 2. *Let $y_g(\sigma_{\epsilon g})$ be the total production of workers from group g when their signal's noise has standard deviation $\sigma_{\epsilon g}$. Production y_g is decreasing in $\sigma_{\epsilon g}$.*

For example, with the parameters used in Figure 1, and assuming that female workers are those with the larger signal noise variance ($\sigma_{\epsilon m}= 1$ and $\sigma_{\epsilon f} = 2$), 24 percent of males and 13.2 percent of females are employed in the complex job. Because there are fewer females than males in the right tail of the quality distribution conditional on any given signal, more females are mismatched, therefore males' total value of (log) production is equal to -0.29, whereas females' is -0.35. To assess the inefficiency cost arising from incomplete information, consider that if workers were efficiently assigned, the value of production would be 1.31 for each group.

[14] This "single-crossing" property of the wage functions of signals of different precision relies on assuming symmetry of the production function and of the signaling technology. However the result that a more precise signal implies higher wages at the top of the distribution, and lower wages at the bottom, is more general, and holds even if the wage functions cross more than once.

2.3 Heterogenous CEOs: Female and Male

Consider now an environment in which some firms are managed by female CEOs and some by male CEOs.[15] We assume that female CEOs are characterized by a better ability to assess the productivity of female workers, that is, female workers' signal is extracted from a more precise distribution, with noise variance $\sigma^2_{\epsilon F} < \sigma^2_{\epsilon f}$ (where the capital F denotes female workers when assessed by a female CEO, and lowercase f when assessed by a male CEO). Symmetrically, female CEOs evaluate male workers' with lower precision than male CEOs: $\sigma^2_{\epsilon M} > \sigma^2_{\epsilon m}$.

This assumption may be motivated by any difference in language, verbal and non-verbal communication styles and perceptions that may make it easier between people of the same gender to provide a better understanding of personal skills and attitudes, improve conflict resolutions, assignment to job-tasks, etc. A large socio-linguistic literature has found differences in verbal and non-verbal communication styles between groups defined by race or gender that may affect economic and social outcomes (see e.g. Dindia and Canary (2006) and Scollon et al. (2011)).[16] Recent employee surveys also indicate that significant communication barriers between men and women exist in the workplace (Angier and Axelrod (2014), Ellison and Mullin (2014)). In the economics literature, several theoretical papers have adopted an assumption similar to ours. Lang (1986) develops a theory of discrimination based on language barriers between "speech communities" defined by race or gender. To motivate this assumption, Lang surveys the socio-linguistic literature demonstrating the existence of such communication barriers. Cornell and Welch (1996) adopt the same assumption in a model of screening discrimination. Morgan and Várdy (2009) discuss a model where hiring policies are affected by noisy signals of productivity, whose informativeness depends (as in our assumption) on group identity because of differences in "discourse systems". More recently, Bagues and Perez-Villadoniga (2013)'s model generates a similar-to-me-in-skills result where employers endogenously give higher valuations to candidates who excel in the same dimensions as them. This result can also provide a foundation to our assumption if female workers are more likely to excel on the same dimensions as female executives. Conde-Ruiz et al. (2017) also study the effects of a model with gender differences in the observability of productivity signals.

The equilibrium features described in Section 2.2 carry through in the environ-

[15] We do not model the change in CEO gender or how the CEO is selected as we are interested in comparing differences in gender-specific wage distributions between firms where the top management has different gender.

[16] There exists also an extensive medical literature showing how physician-patient interactions are affected by the gender of both the physician and the patient (see Cooper-Patrick et al. (1999) Rathore et al. (2001), and Schmid Mast et al. (2007)).

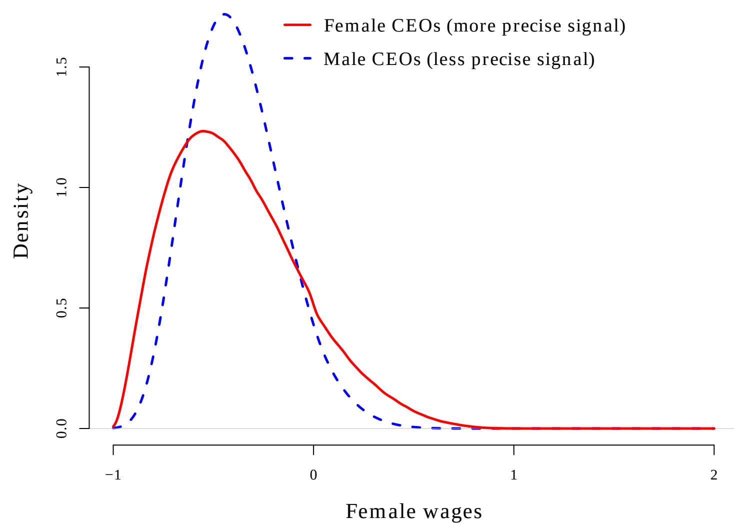

Figure 2: Simulation of the wage distributions of female workers

ment with heterogenous CEOs. In this new environment we compare equilibrium wages and firm performance in firms with CEOs of different gender. Focus for example on the wage distributions of female workers. Figure 2 displays the wage distributions of female workers employed at firms with female or male CEOs. The distribution displayed by the dashed line was computed using a signal with noise variance $\sigma^2_{\epsilon f} = 3$, representing draws from the (less precise) signals received by male CEOs; the distribution displayed by the solid line was computed using a signal with noise variance $\sigma^2_{\epsilon F} = 2$, representing draws from the (more precise) signals received by female CEOs. As stated in Proposition 1, Figure 2 shows that the wage distribution of female workers employed at female CEOs firms has thicker tails.[17] Women working for a female CEO are more likely to be assigned to the complex task and they earn higher wages at same signals for all the signals above the single crossing reported in Figure 1. This generates the fatter right tail. Below the threshold corresponding to the single crossing, women working for a female CEO earn lower wages at same signal than women working for a male CEO because the female CEO has a better assessment of how low productivity really is in those cases. This generates the fatter left tail. The following prediction follows from Proposition 1:

Empirical implication 1. *Wages of female workers in firms with female CEOs are*

[17] Other parameters used in this simulation: $\sigma = 1, \overline{q} = 1, \mu = .5, h = 1.1, l = 1$. We picked these parameters to produce a graph that could show the qualitative features of the proposition.

higher at the top of the wage distribution, and lower at the bottom of the wage distribution relative to wages of female workers employed by male CEOs. Symmetrically, wages of male workers in firms with female CEOs are higher at the bottom and lower at the top of the wage distribution relative to wages of male workers employed by male CEOs.

As a result, and as Figure 2 makes clear, we should also expect the variance of the wage distribution of female workers employed by female CEOs to be higher than the variance of the wage distribution of female workers employed by male CEOs. The opposite should be true on the variance of male wages.

The model has implications for firm performance as well. Proposition 2 states that total production is higher the lower the signal noise since CEOs can better match workers to jobs. As a result, we should observe that female CEOs can improve firm performance by implementing a better assignment of female workers.[18] Therefore, we will test the following empirical prediction:

Empirical implication 2. *The productivity of firms with female CEOs increases with the share of female workers.*

We derived Propositions 1 and 2 using specific distributional assumptions, but the empirical implications are robust to alternative distributions of the signal's noise and of the underlying productivity. A higher signal precision always implies less mismatching of workers to jobs, and a higher correlation of signals with productivity always implies lower wages when the signal is small and higher wages when the signal is high. These implications are also robust to alternative specifications of the signal extraction technology. In Web Appendix (B), we derive the same empirical implications assuming a dynamic model where signals are extracted every period. Assuming all firms initially have male CEOs, firms acquiring female CEOs update the expected productivity of female workers with higher precision. The implications follow because female CEOs will rely on a larger number of more precise signals from female workers than male CEOs.

We do not model explicitly the selection process into executive positions (although in the empirical part we will take into account that such process might differ by gender) or how labor force dynamics are affected by CEO gender in a "general equilibrium". The change of a CEO's gender in our model will affect incentives for

[18] Symmetrically, female CEO's assignment of *male* workers has the opposite effect. However, this effect might be weakened if female CEOs, upon assuming leadership, could "trust" the previous assignment of male workers made by a male CEO. We are agnostic about the details of job reassignment of workers of a different gender, which we do not model, and we focus on an implication that is robust to such details.

workers to leave the firm hoping to extract a more advantageous signal. Our results therefore assume rigidities in workforce mobility, which can be motivated by hiring and firing costs. However, the equilibrium wages by CEO and worker's gender derived in our propositions are nevertheless optimal and are indicative of the directions of wage changes we should expect when a CEO of different gender is appointed.[19]

3 Data and Descriptive Statistics

3.1 Data Sources and Estimation Samples

We use data from three sources that we label INVIND, INPS and CADS. From these three sources of data, we build a matched employer-employee panel data set and from this matched data set we extract our final estimation samples.

INVIND stands for the *Bank of Italy's annual survey of manufacturing firms*, an open panel of about 1,000 firms per year, representative of Italian manufacturing firms with at least 50 employees. INPS stands for the *National Social Security Institute* which provided the work histories of all workers ever employed at an INVIND firm in the period 1980-1997,[20] including spells of employment in firms not included in the INVIND survey. We match the INVIND firms with the INPS work histories using unique worker and firms identifiers to create what we call the INVIND-INPS data set. This data set includes for each worker: gender, age, tenure[21], occupational status (production workers, non-production workers, executives), annual gross earnings (including overtime pay, shift work pay and bonuses), number of weeks worked, and a firm identifier. We exclude all records with missing entries on either the firm or the worker identifier, those for workers younger than 15, and those corresponding to workers with less than four weeks worked in a given year. For each worker-year, we kept only the observation corresponding to the main job (the job with the highest number of weeks worked). Overall, the INVIND-INPS data set includes information on about a million workers per year, more than half of whom are employed in INVIND firms in any given year. The remaining workers are employed in about 450,000 other companies of which we only know the firm identifier.

In Table 1 we report summary statistics on workers' characteristics for the INVIND-INPS data set. About 67% of observations pertain to production work-

[19]This motivates in the empirical analysis the inclusion, in our benchmark specification, only of data from workers that were not hired after the new CEO was appointed. We check the robustness of our results to the inclusion of all workers.

[20]The provision of these work histories data for the employees of the INVIND firms was done only once in the history of the data set and therefore can only cover firms and workers up to 1997. This is the reason why we cannot work on more recent data.

[21]Tenure information is left-censored because we do not have information on workers before 1981.

ers, 31% to non-production employees, and 2.2% to executives. Even though they represent about 21% of the workforce, women are only 2.5% of executives. On average, workers are 37 years old and have 5 years of tenure. Average gross weekly earnings at 1995 constant prices are around 388 euros, with women earning about 24% less than men (309 euros vs. 408 euros). The gender gap is in line with aggregate statistics but looking at weekly earnings may arise a concern related to the intensive margin of labor supply. If women works significantly less hours than men, weekly earnings may over estimate the hourly wage gap. We think the concern is limited in our sample. First, the manufacturing sector in Italy in the period under consideration is quite rigid, with hours worked collectively negotiated at the national level. Second, part-time work was only starting to be introduced when our observation period starts[22] and it is still very rare in the manufacturing sector.[23]

CADS, the third data source we use, stands for *Company Accounts Data Service* and includes balance-sheet information for a sample of about 40,000 firms between 1982 and 1997, including almost all INVIND firms. The data include information on industry, location, sales, revenues, value added at the firm-year level, and a firm identifier. Thanks to a unique and common firm identifier, we can match CADS with INVIND-INPS.

Most of our empirical analysis focuses on the balanced panel sample consisting of firms continuously observed in the period 1988-1997. In Table 1 we report summary statistics both on this sample and on the entire, unbalanced INVIND-INPS-CADS sample for the same period. Notice that the unit of observation on the sample is a firm in a given year while in the INVIND-INPS was a worker in a given firm in a given year. The unbalanced INPS-INVIND-CADS panel includes 5,029 firm-year observations from a total of 795 unique firms. Of these, 234 compose the balanced panel. In the unbalanced sample, average gross weekly earnings at 1995 constant prices are equal to about 405 euros. On average, workers are 37.2 years old and have 8 years of tenure. 68% of workers are blue collar, 30% white collar, and 2.5% are executives. The corresponding statistics in the balanced sample are very similar.

[22]The possibility of part-time work was introduced in Italy by Law N. 863 in 1984.

[23]For example, using more recent data, Bardasi and Gornick (2007) find that only 4.4% of women in Italy were employed part-time across all sectors compared to 15.4% in the UK, 16.6% in Germany and 20.6% in Sweden. ILO (2016) using data starting in 1995 reports that part-time work is highly concentrated in the service sector, specifically in jobs related to health, education and hospitality.

Table 1: Descriptive statistics: INVIND-INPS sample and INVIND-INPS-CADS sample

	INVIND-INPS		INVIND-INPS-CADS			
			Unbalanced panel		Balanced panel	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
% Prod. workers	66.5		67.6	(18.7)	67.4	(18.3)
% Non-prod. wrk	31.3		29.8	(17.7)	30.0	(17.3)
% Executives	2.2		2.5	(1.7)	2.6	(1.8)
% Females	21.1		26.2	(20.9)	25.8	(20.1)
% Fem. execs.	2.5		3.3	(10.3)	3.4	(10.1)
% Female CEO			2.1		1.8	
Age	37.0	(10.1)	37.2	(3.6)	37.4	(3.4)
Tenure	5.1	(4.1)	8.1	(2.6)	8.7	(2.3)
Wage (weekly)	387.2	(253.8)	400.3	(86.0)	404.5	(88.7)
Wage (males)	408.1	(271.8)	429.3	(92.7)	433.9	(97.5)
Wage (females)	309.5	(146.6)	343.3	(67.0)	346.4	(68.5)
Firm size (empl.)			675.0	(2,628.6)	704.2	(1,306.9)
Sales ('000 euros)			110,880	(397,461)	118,475	(231,208)
Sales/worker (ln)			4.93	(0.62)	4.95	(0.57)
Val. add./wkr (ln)			3.77	(0.43)	3.79	(0.41)
TFP			2.49	(0.50)	2.49	(0.49)
N. Observations	18,664,304		5,029		2,340	
N. Firms	448,284		795		234	
N. Workers	1,724,609					
N. Years	17		10		10	

3.2 Female Leadership

We identify female CEOs and female executives from the job classification *executive* in the data[24]. As already observed by Bandiera et al. (2011), one advantage of using data from Italy is that this indicator is very reliable because executives are registered in a separate account with the social security administration agency (INPS). We identify the CEO as the executive with the highest compensation in a firm-year. This procedure is supported by the following: i) Salary determination in the Italian manufacturing sector is such that the compensation ordering follows very closely the hierarchical ranking within each of the three broad categories we observe (executives,

[24]The original job description in Italian is *dirigente*, equivalent to an executive in a US firm.

non-production workers, production workers); ii) The firm's CEO is classified within the executive category; iii) We have a very detailed and precise measure of compensation because we have direct access to the administrative data that each firm must report by law (and each worker has the incentive to verify is correctly reported); iv) We have access to all the workers employed by a given firm in a given year[25].

We do not observe the gender of female workers' direct supervisors, who may conduct the evaluation of the workers' performance, and make decisions (or give recommendations) about their subordinates' task assignment and salary. Instead, we test the relationship between the CEO's and executives' gender and workers' wages. A possible interpretation of our findings is therefore that female CEO's and executives establish a corporate culture leading to better outcomes for high-productivity women in the organization. This limitation of our data goes against us finding an effect, because of the distance between CEOs/executives and the workforce[26].

Using our definitions, we find that although females are 26.2% of the workforce in INVIND firms, they are only 3.3% of the executives, and only 2.1% of CEOs. The descriptive statistics for the balanced panel are quite similar to those referring to the unbalanced sample and confirm the under-representation of women in top positions in firms found for other countries. In particular, the ratio between women in the labor force and women classified as executives is very similar to the ratio obtained from the ExecuComp[27] data for the U.S. This leads to a quite low number of firms with female CEO in our estimation sample (see bottom of right column in Table 2). One advantage of using the proportion of female executives as an additional measure of female leadership is that more firms in our estimation have at least some female executives. In the period 1988-1997, there are 201 firms with at least one female executive in at least one year and 1,078 firm-year observations with at least one female executive. This corresponds to 21.4% of the total number of firm-year observations. Women are at least 10% of the executives in 128 out of these 201 firms and in 553 out of these 1,078 firm-year observations, .

Women's representation in executive positions in Italy has increased over time

[25] We have the complete set of workers only for the INVIND firms and as a result we can only assign CEO's gender to INVIND firms. However, this is irrelevant for our final estimation sample at the firm level since for other reasons explained below we limit our main empirical analysis to a subset of INVIND firms. For another example in the literature using wage ranking to infer top managers, see Tate and Yang (2015).

[26] Kunze and Miller (2017) use detailed data from Norway and find that greater female representation at higher ranks is associated with higher promotion probabilities for women in lower ranks. Huffman et al. (2012) find similar patterns using longitudinal US data.

[27] Execucomp is compiled by Standard and Poor and contains information on executives in the S&P 500, S&P MidCap 400, S&P SmallCap 600. See for example, Bertrand and Hallock (2001), Wolfers (2006), Gayle et al. (2012), Dezsö and Ross (2012).

Table 2: Descriptive statistics: Firms with Male and Female CEO in INVIND-INPS-CADS sample

	Male CEO		Female CEO	
	Mean	St.Dev.	Mean	St.Dev.
CEO's age	49.5	(7.1)	46.6	(7.1)
CEO's tenure	4.4	(3.7)	4.0	(2.8)
CEO's annual earnings	199,385	(144,508)	128,157	(54,643)
% Production workers	67.5	(18.7)	75.4	(13.5)
% Non-prod. workers	30.0	(17.8)	22.2	(13.1)
% Executives	2.5	(1.7)	2.4	(1.4)
% Females	25.9	(20.7)	37.2	(27.0)
% Female executives	2.4	(6.9)	46.8	(29.5)
% Female executives (excl. CEO)	3.3	(10.3)	15.9	(28.6)
Firm size (employment)	683.7	(2,655.4)	270.3	(409.9)
Age	37.2	(3.6)	35.9	(3.5)
Tenure	8.1	(2.6)	8.6	(2.2)
Wage (earnings/week)	401.6	(86.0)	341.3	(61.7)
Wage (males)	430.6	(92.8)	369.4	(64.2)
Wage (females)	343.3	(66.2)	345.4	(97.1)
Sales (thousand euros)	112,467	(401,486)	37,185	(55,982)
Sales per worker (ln)	4.9	(0.6)	4.7	(0.6)
Value added per worker (ln)	3.8	(0.4)	3.6	(0.4)
TFP	2.5	(0.5)	2.4	(0.5)
N. Observations	4,923		106	
N. Firms	788		33	
N. Years	10		10	

but remains small: In 1980, slightly above 10 percent of firms had at least one female executive, and females represented 2% of all executives and 1% of CEOs; In 1997, these figures were 20%, 4% and 2%, respectively. There is substantial variation across industries in the presence of females in the executive ranks, but no obvious pattern emerges about the relationship between female leadership and the presence of females in the non-executive workforce in the various industries.[28]

In Table 2 we compare firms with a male CEO with those with a female CEO. Firms with a female CEO are smaller, both in terms of employment and in terms of revenues, pay lower wages, and employ a larger share of blue collar workers. Firms

[28] See Table B1 in the Web Appendix for details.

with a female CEO also employ a larger share of female workers (37 vs. 26 percent). However, when one looks at measures of productivity (sales per employee, value added per employee, and TFP), the differences shrink considerably. For instance, total revenue is on average about 3 times higher in firms with a male CEO than in firms with a female CEO, but revenue per employee, value added per employee and TFP are only about 21 percent, 19 percent and 4 percent higher, respectively.

4 Empirical Analysis

4.1 Specification and Identification

The unit of observation of our analysis is a given firm j observed in a given year t. We are interested in the impact of female leadership on workers' wage distributions and firms' performance, and therefore estimate regressions of the following form:

$$y_{jt} = \beta FLEAD_{jt} + FIRM'_{jt}\gamma + WORK'_{jt}\delta + EXEC'_{jt}\chi + \lambda_j + \eta_t + \tau_{t(j)}t + \varepsilon_{jt} \quad (4.1)$$

where: y_{jt} is the dependent variable of interest, either moments of the workers' wage distribution or measures of firm performance, and β is the coefficient of interest. The regressors and controls are defined as follows: $FLEAD_{jt}$ is the measure of female leadership: either a female CEO dummy or the fraction of female executives; $FIRM_{jt}$ is a vector of observable time-varying firm characteristics (dummies for size, industry, and region); $WORK_{jt}$ is a vector of observable workforce characteristics aggregated at the firm-year level (age, tenure, occupation distribution, fraction female) plus worker fixed effects aggregated at the firm-year level and estimated in a "first step" regression described in detail below; $EXEC_{jt}$ is a vector of observable characteristics of the firm leadership (age and tenure as CEO or executive) plus CEO's or executives' fixed effects estimated in the first step regression described in detail below;[29] λ_j are firm fixed effects; η_t are year dummies and $\tau_{t(j)}$ are industry-specific time trends.

The main challenge in estimating the impact of female CEOs (or female leadership in general)[30] on workers' wages and firms' performance is the sample selection bias induced by the non-random assignment of CEOs to firms. In particular, it is possible that:

1. unobservable firm characteristics may make some firms more productive than

[29] When the female leadership measures is the female CEO dummy we simply use the CEO's value of the listed regressors; when the leadership measure is the proportion of female executives at the firms, we use the average of the listed regressors over the firm's executives.

[30] To simplify the discussion, we present the identification for the case in which female leadership is represented by the dummy female CEO. The same discussion carries through when we use the fraction of female executives as a measure of female leadership.

others, and this unobserved firm-level component may not be randomly assigned between male- and female-led firms;

2. the workforce composition of firms led by women might systematically differ from that of firms led by men;

3. the selection on unobserved individual ability in the position of CEO may not be the same by gender so that women CEOs may be of systematically higher or lower ability than men CEOs. As a result, female leadership indicators might be capturing such differences rather than gender effects.

To address these issues we control for firm fixed effects, workforce composition effects, and CEO effects. First, we include controls for a set of time-varying, observable firm characteristics, workforce characteristics, and CEO characteristics. Second, we control for time-invariant firm-level heterogeneity by estimating equation (4.1) with firm fixed effects both for wages and firm performance. Using within-firm variation to identify the impact of female leadership at the firm level has so far been the identification strategy most commonly employed in the literature.[31] Third, and new in the literature, we add controls for unobservable workforce and CEO heterogeneity obtained from a first step two-way fixed effects regression, as described next.

Our matched employer-employee data includes the entire work history (between 1980 and 1997) of all the workers who ever transited through one of our J INVIND firms. This matched employer-employee data set (almost 19 million worker-year observations) contains a large number of transitions of individuals across (INVIND and non-INVIND) firms and is thus well suited to estimate two-way fixed effects as in Abowd et al. (1999) - henceforth, AKM. An individual fixed effect estimated from such a regression has the advantage of controlling for the firms the worker or executive has ever worked for. As a result, it can capture those fixed effects in individual productivity which are usually captured by education, other time-invariant human capital or other proxies for "ability" and skills.[32]

Our strategy is to use the individual fixed effects from an AKM regression to construct proxies for executive and average worker ability at the firm-year level to include as controls in regression 4.1. Specifically, we perform the two-way fixed effects

[31] See Smith et al. (2006), Albanesi and Olivetti (2009), and Gagliarducci and Paserman (2015).

[32] Including these controls as described in detail below also helps to alleviate the fact that our data set, as is frequently the case with administrative data, does not include a particularly rich set of controls at the individual worker level. For example, we have no measure of education or other formal training in the data which are usually included as controls in wage regressions.

procedure proposed by Abowd et al. (2002) by estimating the following equation:

$$w_{it} = \mathbf{s}'_{it}\beta + \eta_t + \alpha_i + \sum_{j=1}^{J} d_{jit}\Psi_j + \zeta_{it}. \tag{4.2}$$

The dependent variable is the natural logarithm of weekly wages. The vector of observable individual characteristics, $\mathbf{s}'$, includes age, age squared, tenure, tenure squared, a dummy variable for non-production workers, a dummy for executives (occupational status changes over time for a considerable number of workers), as well as a full set of interactions of these variables with a female dummy (to allow the returns to age, tenure and occupation to vary by gender), and a set of year dummies; α_i is worker's i fixed effect, Ψ_j is firm's j fixed effect and d_{jit} is a dummy equal to 1 if worker i works for firm j in year t.

Our original sample consists of essentially one large connected group (comprising 99% of the sample). Thus, in our estimation we focus only on this connected group and disregard the remaining observations. The identification of firm effects and worker effects is delivered by the relatively high mobility of workers in the sample over the relatively long period under consideration: about 70 percent of workers have more than one employer during the 1980-1997 period, and between 8 and 15 percent of workers change employer in a given year. The $\hat{\alpha}_i$ obtained by this procedure for the firms' CEOs and executives are included in the vector $EXEC_{jt}$ to control for CEO's individual heterogeneity. Moreover, we also used them to compute the mean fixed effect of the workers of a given firm j in time t, which we then include among the controls for workforce composition ($WORK_{jt}$).[33] Notice that the CEO and executive fixed effects are estimated using only the wages they received when they were executives and CEOs. We made this choice because we are interested in proxying their ability as executives and not as regular employees.[34]

The AKM method hinges on the assumptions of exogenous mobility of workers across firms conditional on observables. We follow Card et al. (2013) (CHK henceforth) in performing several tests to probe the validity of the assumption.[35] We

[33] Under our model, the workers' wages from which we have estimated these worker fixed effects are affected by statistical discrimination. This does not introduce a bias in the estimate of the mean $\hat{\alpha}_i$ on the workers of a given firm j in time t because the model, as common in standard statistical discrimination models, does not imply "group discrimination". Moreover, the group of workers we have at each firm is large enough to deliver credible estimates: Our firms are relatively large by construction (firms are included in the INVIND sample only if they employ at least 50 employees) and the median number of workers in INVIND firms is around 250.

[34] As a robustness check, we also estimated the CEO and executive fixed effects using their entire wage history and we have re-estimated our benchmark specifications using them. The results were not significantly affected and they are reported in Web Appendix F Table A.1

[35] Specifically, a model including unrestricted match effects delivers only a very modest improved statistical fit compared to the AKM model, and the departures from the exogenous mobility as-

conclude that in our context, similarly to what found by CHK with German data, the additively separable firm and worker effects obtained from the AKM model can be taken as reasonable measures of the unobservable worker and firm components of wages. Tests and results are reported in detail in Web Appendix D.[36]

The three steps described above provide controls for: (i) time-varying, observable firm, workforce, and CEO characteristics; (ii) unobservable time-invariant firm characteristics; and, (iii) unobservable time-invariant CEO/executive and average worker heterogeneity. These steps constitute our preferred identification strategy and generate our benchmark specifications. In a fourth step, we provide extensive robustness analyses, including an IV procedure to account for unobservable time-varying firm-level shocks potentially correlated with the CEO's gender.

Estimates of equation (4.1) for the dependent variables of interest are presented in Section 4.2 and 4.3. All regressions share the same baseline specification which includes firm fixed-effects, time-varying firm controls, workforce composition controls and controls for CEOs' characteristics. Specifically, the complete set of controls is: CEO age, tenure as CEO, CEO fixed effects computed in the 2-way fixed-effects regression described above, the fraction of non-executive female workers, the fraction of non-production workers, the mean age of the workforce, the average of workers' fixed effects computed in the 2-way AKM fixed-effects regression, a set of 20 region dummies, 20 industry dummies, 4 firm-size dummies, year dummies, and industry-specific time trends. The firm fixed effects also control for the firms' ownership type, which does not change over time for the vast majority of the companies in our sample.[37]

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sumption suggested by the AKM residuals are small in magnitude. Moreover, wage changes for job movers show patterns that suggest that worker-firm match effects are not a primary driver of mobility in the Italian manufacturing sector. Instead, the patterns that we uncover are consistent with the predictions of the AKM model for job movers.

[36]The two-way fixed effect regressions generate expected results: wages exhibit concave age and tenure profiles, and there is a substantial wage premium associated with white collar jobs and, especially, with executive positions.

[37]There are four types of firms in our dataset: *family firms*, *conglomerates*, *foreign/institution owned* and *government owned*. The presence of female executives appears to be similar across ownership types, with the exception of government owned firms, where female executives are almost non-existent (government-owned firms are less than 5% of the sample). In the full sample, women are 3.3% of executives in family firms, 2.4% in conglomerates, and 3.3% in foreign/institution-owned firms. The proportions of firm-year observations with a female CEO is 1.8% among family firms, 1.2% among conglomerates, and 1.1% among foreign/institution-owned firms. In the balanced panel, proportions are also relatively stable with, respectively, 4.6%, 2.8% and 5.5% of female executives and 3.0%, 1.5% and 2.7% of female CEOs.

Figure 3: Coefficients of female CEO dummy on average wages by quantile of the female and male wage distributions

4.2 Female Leadership and Firm-Level Workers Wages Distributions

In the model we presented in Section 2, female CEOs extract more precise signals of productivity from female workers. A more precise signal implies that women at the top of the wage distribution should earn higher wages than females at the top of the distribution employed by male CEOs. Women at the bottom of the wage distribution, on the other hand, should earn lower wages when employed by female CEOs. As a result, the overall wage dispersion of female workers in each firm should be higher in firms managed by women CEOs.

4.2.1 Results

Motivated by this prediction, we estimate equation (4.1) using as dependent variables y_{jt} a set of firm- and gender-specific moments of the workers' wages distribution: the standard deviation, average wages below and above the median, below the 10th and above the 90th percentile, and average wages within each quartile of the wage distribution. Our main regressor of interest is the measure of female leadership: a female CEO dummy or the share of female executives. As described in Section 4.1, all regressions include firm fixed-effects, time-varying firm controls, workforce composition controls, and controls for CEOs' characteristics.

Figure 3 summarizes our main results by reporting the estimated coefficients on the female CEO dummy on the four quartiles and the two extreme deciles of the wage distribution using our benchmark specification. The continuous line shows that female leadership has a positive effect on female wages at the top of the distribution and a negative effect at the bottom of the distribution. The effect on the male wage distribution, illustrated by the dashed line, is symmetric and of the opposite sign. The effects are consistently increasing, moving from the bottom to the top of the female wage distribution, and decreasing moving from the bottom to the top of the male distribution.[38] These results conform to Empirical prediction 1 of Section 2.

To provide details on the precision and robustness of these results, we report the estimated effects of female leadership on various moments of the female wage distribution in Table 3 and of the male wage distribution in Table 4, according to six different specifications (panels (a) through (g)).[39] Coefficient estimates for the more relevant controls for the benchmark specification are reported in Web Appendix E and in Web Appendix H for the other specifications.

Panel (a) reports the results of our benchmark specification, where the measure of female leadership is a dummy variable indicating whether the firm is managed by a female CEO. This specification is estimated using the balanced sample to avoid the selection of firms entering and exiting the sample. In addition, we used only observations on workers hired under the previous CEO and who do not leave the firm during the female CEO's tenure ("stayers") to avoid the selection of workers entering and exiting the firm. The wage effects highlighted by our theory may induce labor mobility: women with low signals employed under female management may be induced to seek employment in firms with male management. The same is true for women with high signals employed under male management. While these predictions are in principle testable, our sample size is too small to investigate them.[40] In the next subsection we describe other specifications where we remove most of the restrictions imposed in the benchmark specification – including the focus on the

[38]The whiskers report the 10% bounds of the one-tail test consistent with the prediction of the model. See page 24 for details.

[39]Dependent variables in columns (4)-(9) are defined as follows: Decile 1 (column 4): average wage of earners below the 10th percentile of the wage distribution. Decile 10 (column 5): average wage of earners above the 90th percentile. Quartile 1: average wage below the 25th percentile; Quartile 2: average wage between the 25th and 50th percentile; Quartile 3: average wage between the 50th and the 75th percentile; Quartile 4: average wage above the 75th percentile of the wage distribution.

[40]Specifically, we do not have sufficient transitions to identify different turnover behavior at different points of the wage distribution at the firm. First, we only observe a subsample of firms. Second, during the years we consider (1984-1998), Italy had among the most stringent rules for individual dismissal, limiting the possibility to use firings as a reallocation device and reducing the number of transitions (OECD-IDB, 2017). If this reduction of dynamic reallocation does not allow to identify predictions on turnover behavior, it also lends stronger empirical relevance to the wage effects we highlight in our static model.

balanced sample and on stayers – showing that they do not significantly affect our results.

For each specification, the tables report the estimated coefficient on the measure of female leadership. In addition, because our model makes specific predictions on the effects of female leadership on the wage variance, and on wages at the top and bottom of the gender-specific wage distributions, we report the p-values of 1-tailed tests of the model's predictions.[41] Specifically, we follow the convention used in significance tests and we set as alternative hypothesis the impact predicted by the model and as null the opposite effect. Small p-values indicate that the model is not rejected. The model's predictions are unambiguous at the top and at the bottom of the distribution: the model predicts positive effects at the top of the female wage distribution and negative effects at the bottom; the opposite predictions hold on the male distribution. Since the model does not predict at which specific point of the distribution the change of sign occurs, the predictions in the middle of the distribution are ambiguous. In these cases, we set as alternative hypothesis the sign of the actual point estimate so as to identify where the switch in sign occurs.[42]

The results on the benchmark specification (panel (a)) show that:

(i) Female leadership has a strong, economically and statistically significant positive effect on the variance of women's wages, as predicted by the theory. The standard deviation of female wages is almost 50% larger when the firm is managed by a female CEO. The effect on the male wage variance is also strong (about 10%) and, as predicted by our model, of the opposite sign, though less precisely estimated.

(ii) The effect of female leadership on wages at the top of the female wage distribution (columns 3, 5, and 9) is strongly positive and statistically significant, with p-values of less than 1%. Females with wages above the median earn on average 7.8% more when working for a female CEO than for male CEOs (Table 3, column 3). The effect of female leaderships is stronger at the right end of the wage distribution: the (highly significant) positive impact of female leadership is 10.4% for females with wages above the 25th percentile (column 9) and 16.7% for those earning above the

[41] Given that individual CEO effects are generated regressors from a first-step estimation, in all specifications except (g), (IV2) and (IV4), p-values are computed using bootstrapped standard errors with 300 replications. As described in detail in the Appendix, our bootstrapping procedure resamples firms, stratifying by firms that never had a female CEO and firms that had a female CEO. In specification (IV2)-(IV4), standard errors are clustered at the firm level. Standard errors are reported in the Web Appendix E for the benchmark specification and in Web Appendix H for the other specifications.

[42] Note that in all sixteen specifications but one the sign change occurs as predicted by theory. The only exception is specification (b) for male workers: the estimated effect of female leadership has the appropriate sign on the standard deviation of wages, but the effects are negative, though imprecisely estimated, at all points of the distribution.

Table 3: Impact of female leadership on moments of the firm-level female wage distributions

Dependent variable: →	Std. dev.	Average wages							
		Median		Decile		Quartiles			
		Below	Above	1	10	1	2	3	4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Benchmark									
Coefficient	0.475	-0.030	0.078	-0.043	0.167	-0.031	-0.026	0.006	0.104
1-tail p-value	0.000	0.090	0.003	0.124	0.004	0.147	0.112	0.424	0.001
(b) All workers									
Coefficient	0.418	-0.032	0.049	-0.038	0.121	-0.036	-0.027	-0.020	0.072
1-tail p-value	0.000	0.041	0.062	0.146	0.009	0.108	0.043	0.176	0.027
(c) With controls for new CEO									
Coefficient	0.477	-0.030	0.079	-0.043	0.168	-0.030	-0.026	0.007	0.105
1-tail p-value	0.000	0.090	0.003	0.124	0.004	0.148	0.112	0.410	0.001
(d) Full panel									
Coefficient	0.403	-0.016	0.073	-0.004	0.170	-0.007	-0.022	0.006	0.096
1-tail p-value	0.000	0.206	0.000	0.448	0.000	0.390	0.121	0.370	0.000
(e) Different measure of female leadership: fraction of female executives									
Coefficient	2.108	-0.036	0.310	-0.114	0.789	-0.053	-0.022	-0.007	0.421
1-tail p-value	0.000	0.188	0.000	0.109	0.000	0.172	0.286	0.429	0.000
(f) Different measure of female leadership: female CEO for at least one year									
Coefficient	0.431	-0.088	0.032	-0.143	0.117	-0.112	-0.072	-0.041	0.055
1-tail p-value	0.007	0.003	0.130	0.000	0.073	0.000	0.023	0.213	0.070
(g) Without controls for unobservable workforce and CEO ability									
Coefficient	0.460	-0.035	0.072	-0.045	0.159	-0.035	-0.032	0.001	0.097
1-tail p-value	0.000	0.037	0.009	0.086	0.007	0.093	0.051	0.481	0.004

Note: Firms fixed-effects regressions. Dependent variables are in logs. Dependent variables in columns (4-9) are defined in Footnote 39. Coefficients for a larger set of explanatory variables and standard errors are reported in online appendices E and H. Number of observations: 2,340 (234 Firms, 10 years), all specifications except (d); specification (d): 5,029 observations (795 firms). P-values are computed using bootstrapped standard errors with 300 replications in specifications (a)-(f) and are computed using standard errors clustered at the firm level in specification (g).

Table 4: Impact of female leadership on moments of the firm-level male wage distribution

Dependent variable: →	Std. dev.	Median		Average wages					
				Decile		Quartiles			
		Below	Above	1	10	1	2	3	4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Benchmark									
Coefficient	-0.107	0.021	-0.027	0.029	-0.069	0.031	0.016	0.010	-0.039
1-tail p-value	0.130	0.118	0.188	0.091	0.116	0.047	0.193	0.333	0.148
(b) All workers									
Coefficient	-0.113	-0.016	-0.037	-0.023	-0.071	-0.016	-0.015	-0.014	-0.047
1-tail p-value	0.095	0.106	0.080	0.081	0.078	0.099	0.130	0.183	0.076
(c) With controls for new CEO									
Coefficient	-0.105	0.022	-0.027	0.029	-0.067	0.031	0.016	0.010	-0.039
1-tail p-value	0.133	0.115	0.194	0.090	0.120	0.046	0.188	0.330	0.153
(d) Full panel									
Coefficient	-0.152	0.030	-0.038	0.058	-0.092	0.049	0.019	0.005	-0.054
1-tail p-value	0.021	0.002	0.069	0.000	0.035	0.000	0.038	0.370	0.049
(e) Different measure of female leadership: fraction of female executives									
Coefficient	-0.232	0.008	-0.091	-0.024	-0.203	-0.004	0.016	0.004	-0.128
1-tail p-value	0.132	0.409	0.067	0.352	0.039	0.461	0.303	0.450	0.048
(f) Different measure of female leadership: female CEO for at least one year									
Coefficient	-0.322	0.006	-0.101	0.022	-0.189	0.021	-0.004	-0.019	-0.129
1-tail p-value	0.003	0.207	0.000	0.053	0.007	0.033	0.550	0.347	0.000
(g) Without controls for unobservable workforce and CEO ability									
Coefficient	-0.187	0.018	-0.043	0.023	-0.104	0.026	0.013	0.007	-0.060
1-tail p-value	0.013	0.154	0.059	0.148	0.027	0.077	0.231	0.371	0.037

Note: see note to Table 3.

10th percentile (column 5). Symmetrically, the effect of female leadership on wages at the *bottom* of the *male* wage distribution (Table 4, columns 2, 4, 6, and 7) is positive and significant at levels close or below the 10% level.

(iii) The effect of female leadership is monotonically increasing moving from the bottom to the top of the female wage distribution (compare the estimates of columns

2 and 3, 4 and 5, and of columns 6 through 9). The opposite holds true for the effect on male wages. This is the result illustrated in Figure 3.

(iv) The effect of female leadership at the bottom of the female wage distribution (columns (2), (4), and (6)) is negative and large across all specifications, although it is less precisely estimated than the effects at the top of the wage distribution.

(v) The estimates on the third quartile of the wage distribution (column 8 in both tables) do not reject the hypothesis that the coefficients are zero. This is still consistent with the theory, which predicts that the effects of female leadership should be zero somewhere in the interior of the wage distribution, but does not predict non-parametrically where the change of sign should occur.

4.2.2 Robustness

We describe here robustness analyses of the results. To ease the comparison, results are reported in the same tables (3 and 4).

We first address the sensitivity of our results to excluding workers hired by female CEOs. Since we do not explicitly model selection of workers or hiring decisions, our benchmark specification only included workers hired under the previous (male) CEO and who stayed at the firm during the female CEO's tenure. Panel (b) presents results after removing this restriction (i.e., including all workers). On the female samples (Table 3), there is only a small decrease in the magnitude of the effects at the top of the distribution, but otherwise all the patterns are confirmed. On the male samples (Table 4), the point estimates have the same sign and similar magnitudes at the top of the distribution; , but we lose significance at the bottom of the distribution and the signs are reversed.

The second concern relates to the treatment effect we are trying to identify. A firm changing from being led by a male CEO to a female CEO implies a change of CEO. The effect that we are estimating could be due to the change of leadership rather than to the change in the leadership's gender. In panel (c) all the regressions include an additional control for whether the current CEO was recently appointed. The control is a dummy=1 for the first two years of the CEO's appointment. Results are almost identical to those from the benchmark specification.

The third concern relates to the selection of firms. In the benchmark specification we focus on a balanced panel sample since we do not explicitly model entry and exit of firms and the data do not specify whether firms no longer in the data were liquidated or acquired. Panel (d) reports the results on the full (unbalanced) panel. The sample size increases significantly: from 234 firms observed over 10 years in the balanced panel, to 795 firms observed over a maximum of 10 years for a total of 5,029 observations. Despite this major change in the estimation sample, most results are very similar to the benchmark specification. The estimates at the bottom of the female distribution are less precise and have smaller magnitudes than in the benchmark, but they maintain the expected sign.

The fourth concern refers to the measure of female leadership we use. The CEO's gender is an important measure of female leadership, has been used in previous literature, and fits well with our theoretical model. However, the identification hinges on the relatively small number of firms experiencing a change in CEO's gender. In panel (e), we use a firm's proportion of female executives to evaluate the impact of a female leadership measure displaying more variation than the female CEO dummy. On the female sample, all the signs are consistent with the benchmark specification, the estimates are very precise on the regressions on the standard deviation and the top of the wage distribution whereas they have p-values slightly higher than 0.1 in the regressions at the bottom of the wage distribution. On the male sample, the bottom decile and quartile show the wrong sign, but the coefficients are very imprecisely estimated. All of the other estimates are consistent with the benchmark.

Our model does not yield a precise prediction about what level of female presence in the firm's leadership is necessary to produce effects. To analyze this issue, we estimated the model using dummies for different proportions of female executives.[43] The results, reported in Web Appendix Section F.3, show that the effects we uncovered emerges clearly when firms have at least 30 percent of female executives. This may simply be because, when women in top positions are just a few, they may be insufficient to have an impact. In fact, a very small presence of women might be due to "tokenism" (Niemann, 2016; Smith and Parrotta, 2015), i.e., the promotion of a small number of women to executive roles in order to satisfy some quota or P.R. considerations, but without conferring them real decision power.

The fifth robustness check relates to the impact of the female CEO at different points of her tenure as CEO in a given firm. Even if changes are desirable, it may require time to implement them. In our benchmark specification, the measure of female leadership includes all the years that the female CEO is in charge. As a result, our effects average out the immediate impact and the impact occurring later in the CEO's tenure. Our sample size does not allow us to identify the impact of female CEO by tenure, but we have enough information to separate the immediate effects from the lagged effects. Panel (f) focuses on the lagged effect, defined as the average impact over all the years as CEO except the first. The estimates show that the contemporaneous effect is strong when the impact on wages is positive while

[43] We thank one referee for this suggestion.

it is the lagged effect which is strong when the impact on wages is negative. For example, looking at the impact on quartiles in Specification (f) in Table 3, we see that the lagged effect is estimated at -11.2% at the bottom compared to the -3.1% of the benchmark. At the top, the positive impact is estimated at 10.4% in the benchmark but it is reduced to 5.5% when only considering the lagged effect. The result is broadly confirmed by specifications jointly estimating both effects (see web appendix Table F.3) The results on male wages are less stark but similar. These effects can be explained by the wage determination process. Nominal wages are typically characterized by downward wage rigidity: it is much easier to increase than to decrease them. As a result, the CEO may be more effective in immediately increasing wages than in immediately decreasing wages, even if both may be equally desirable. In our specification, this dynamic should generate a larger contemporaneous impact when the wage effect is positive and a larger lagged impact when the wage effect is negative. This is in fact what we find.

The sixth robustness check regards the calculation of the standard errors. All of our specifications include generated regressors (the workers' and CEOs' fixed effect obtained from the two-way fixed effects regression). For this reason (and since we do not have an expression for the variance and covariance matrix from the AKM procedure), we have bootstrapped the standard errors. Panel (g) reports results from a specification which does not include the generated regressors so that we can compute standard errors clustered at the firm level in the conventional way. All of the estimates have the expected sign and comparable magnitudes. With respect to the benchmark, p-values are generally smaller on the female sample and at the top of the distribution of the male sample, and higher at the bottom of the male sample.

An additional robustness exercise accounts for potential endogeneity induced by time-varying firm-level shocks. For example, a significant change to unobservable traits such as "corporate culture" may lead to the appointment of a female CEO, but also directly impact wages and performance. Such shocks cannot be controlled for by the firm fixed-effects we have in our specifications because the fixed-effects are time-invariant while these shocks would be time-varying. To address this problem, we construct a set of instruments inspired by Bartik (1991). Bartik's original paper instruments local employment growth using beginning-of-the-period sectoral employment shares at the local level interacted with the sectoral growth rate at the national level. We use beginning-of-the-period female leadership measures at the firm level and we interact them with growth in female leadership measures at the regional level.[44] The regional trend should be correlated with the leadership mea-

[44]There are a total total of 21 regions in our data.

sures of firms in the region in a given year, but not correlated with the time-varying firm-level heterogeneity that may endogenously affect wages and female leadership in a specific firm.

We construct two instruments, one for the fraction of female managers and one for the female CEO dummy. We assume that their base-year values[45] are exogenous conditioning on the other controls (including firm fixed effects).[46] We then compute, for each firm j, the average value of the fraction of female executives by year and region over all firms with the exclusion of firm j. Denote this average by $\overline{f_{t,-j}^{r(j)}}$, where $r(j)$ is the geographical regional location of firm j. We exclude firm j from the average to prevent endogenous factors affecting one firm's female leadership contaminating the average. Next, we compute the yearly growth rates of these averages by region relative to the base year. We denote these growth rates as: $\gamma_{t,j}^{r(j)} = \frac{\overline{f_{t,-j}^{r}}}{\overline{f_{1988,-j}^{r}}}$. Our instrument $\tilde{f}_{t,j}$ is constructed multiplying these growth rates by the base-year value of the fraction of female managers ($f_{1988,j}$):

$$\tilde{f}_{t,j} = f_{1988,j} \cdot \gamma_{t,j}^{r(j)} \tag{4.3}$$

The instrument we use for the female CEO dummy is constructed using a slightly different method because the base year values contain many zeros. The details, together with the first-stage results, are provided in Appendix G.

Results are reported in Table 5. Panel (IV1) reports IV results on the benchmark specification. Panel (IV2) reports IV results on specification (g), i.e. the specification without the generated regressors obtained from the two-way fixed effects regression. Panels (IV3) and (IV4) report IV results on specifications using the other measure of female leadership: fraction of female managers, respectively with and without generated regressors. The first stages of our IV procedure do not indicate the presence of weak instruments problems.[47] Yet, they explain a limited amount of variation. This issue, coupled with the already relatively small number of identifying observations, leads to larger standard errors and higher p-values in most of our IV regressions. Only the regressions on the standard deviation and on the bottom of the wage distribution for women deliver p-values within the range of conventional significance levels. If the IV estimates are imprecise, the sign and magnitude are entirely in line with our benchmark specifications. This is true both in the specifications using female CEO as the measure of female leadership and in

[45]We use 1988, the first year of the panel. Using an older base year would strengthen the exogeneity assumption but induces too many missing values in our sample.

[46]The firm fixed effects are a good control for the endogeneity induced by heterogeneity in the initial conditions since they are firm-specific and time-invariant.

[47]See Web Appendix G.

Table 5: Impact of female leadership on wages: IV estimates

Dependent variable: →	Std. dev.	Average wages							
		Median		Decile		Quartiles			
		Below	Above	1	10	1	2	3	4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female Wages									
(IV1) IV on specification (a)									
Coefficient	0.991	-0.077	0.083	-0.423	0.193	-0.194	0.002	0.001	0.093
1-tail p-value	0.000	0.051	0.112	0.000	0.123	0.011	0.442	0.460	0.156
(IV2) IV on specification (g)									
Coefficient	0.948	-0.096	0.059	-0.441	0.165	-0.213	-0.018	-0.018	0.067
1-tail p-value	0.000	0.111	0.287	0.015	0.194	0.023	0.387	0.382	0.315
(IV3) IV on specification (e)									
Coefficient	1.544	-0.126	0.122	-0.626	0.297	-0.296	-0.012	-0.009	0.138
1-tail p-value	0.000	0.025	0.116	0.000	0.105	0.004	0.413	0.417	0.152
(IV4) IV on specifications (e) and (g) combined									
Coefficient	1.464	-0.162	0.075	-0.666	0.243	-0.333	-0.047	-0.043	0.088
1-tail p-value	0.000	0.066	0.333	0.002	0.216	0.009	0.306	0.320	0.350
Male Wages									
(IV1) IV on specification (a)									
Coefficient	-0.248	0.095	-0.017	0.093	-0.089	0.092	0.094	0.076	-0.051
1-tail p-value	0.112	0.025	0.402	0.181	0.272	0.098	0.004	0.033	0.293
(IV2) IV on specification (g)									
Coefficient	-0.342	0.078	-0.054	0.075	-0.142	0.076	0.077	0.054	-0.093
1-tail p-value	0.063	0.019	0.258	0.131	0.167	0.037	0.047	0.154	0.178
(IV3) IV on specification (e)									
Coefficient	-0.400	0.133	-0.030	0.120	-0.148	0.123	0.133	0.114	-0.080
1-tail p-value	0.123	0.025	0.380	0.181	0.268	0.101	0.004	0.036	0.286
(IV4) IV on specifications (e) and (g) combined									
Coefficient	-0.559	0.103	-0.101	0.090	-0.246	0.095	0.102	0.071	-0.162
1-tail p-value	0.030	0.040	0.208	0.226	0.117	0.119	0.049	0.168	0.138

Note: see note to Table 3. P-values are computed using bootstrapped standard errors with 300 replications in specifications (IV1)-(IV3) and using standard errors clustered at the firm level in specifications (IV2) and (IV4) those using the fraction of female executives. It also holds both on the regressions using the female wage distribution and on those using the male wage distribution.

4.2.3 Summing up

To summarize, the point estimates' signs of the differential impact of female leadership over the wage distribution are consistent with the predictions of our theoretical framework. These effects are substantial in magnitude and appear to be robust across specifications. They are also strongly statistically significant when predicted to be positive, with the exception of the IV specifications. When they are predicted to be negative, the estimates that we obtain are close to and often below conventional levels of statistical significance.

These results are remarkably consistent with our proposed mechanism. There are at least four factors that support this claim. First, the sign of the point estimates is consistent across specifications. In particular, results are robust to extending the data to include the full panel of firms or to using all workers, rather than just those that were already employed by the firm at the time the female CEO was appointed. They are also robust to excluding the generated regressors computed in the two-way fixed-effects regression. In this case, the point estimates are more precisely estimated even though the reported standard errors are clustered at the firm level. As expected, the IV estimates are more imprecise, but signs and magnitudes are in line with benchmark specifications. Second, the effects are generally stronger at the extremes of the distribution, as one can observe by comparing the extreme deciles to the first and fourth quartiles. A stronger impact at the extremes is predicted by our model (see Figure 2 in Section 2). Third, the effects are increasing from the left to the right of the female wage distribution, consistent with the theory. The opposite occurs on the male wage distribution. Finally, downward wage rigidity works against finding large negative effects at the bottom of the wage distribution. It is therefore possible that the lower precision of the estimates at the bottom is related more to institutional factors that reduce downward wage adjustments than to a significant violation of the mechanism we propose.

4.3 Female Leadership and Firm Performance

This section is motivated by the second empirical implication of our theory. Female executives improve the allocation of female talents within the firm by counteracting pre-existing statistical discrimination from male executives. The efficiency-enhancing effects of female leadership is stronger the larger the presence of female workers.

Table 6: Impacts of female leadership on firm-level performance

Dependent variable: →	Sales per employee (1)	Sales per employee (2)	Value added per employee (3)	Value added per employee (4)	TFP (5)	TFP (6)
(a) Benchmark						
Female leadership	0.033	-0.120	-0.046	-0.245	-0.059	-0.213
2-tail p-value	0.623	0.125	0.482	0.001	0.242	0.001
Interaction		0.610		0.795		0.616
1-tail p-value		0.007		0.003		0.019
(b) Full panel						
Female leadership	0.029	-0.009	-0.049	-0.093	-0.061	-0.096
2-tail p-value	0.495	0.876	0.265	0.090	0.140	0.074
Interaction		0.123		0.144		0.115
1-tail p-value		0.192		0.122		0.158
(c) Different measure of female leadership: fraction of female executives						
Female leadership	0.025	-0.322	-0.208	-0.429	-0.236	-0.413
2-tail p-value	0.794	0.097	0.081	0.052	0.060	0.082
Interaction		1.098		0.697		0.559
1-tail p-value		0.007		0.083		0.146
(d) Different measure of female leadership: female CEO for at least 1 year						
Female leadership	0.062	-0.133	-0.038	-0.236	-0.070	-0.206
Interaction		0.727		0.737		0.507
1-tail p-value		0.023		0.027		0.013
(e) Without controls for unobservable workforce and CEO ability						
Female leadership	0.027	-0.104	-0.064	-0.234	-0.072	-0.200
2-tail p-value	0.652	0.147	0.266	0.000	0.110	0.000
Interaction		0.523		0.677		0.513
1-tail p-value		0.001		0.000		0.002

Note: Firms fixed-effects regressions. Coefficients for a larger set of explanatory variables and standard errors are reported in online appendices E and H. p-values are computed using bootstrapped standard errors with 300 replications in specifications (a)-(d) and using standard errors clustered at the firm level in specification (e)

4.3.1 Results

Table 6 presents the estimation results from firm performance regressions, i.e. coefficients from estimating equation (4.1) where the dependent variable y_{jt} is one of our three measures of firm performance: sales per employee, value added per employee, and TFP.[48] The female leadership measures and the controls are the same as in the wage regressions. As in the previous subsection, our benchmark specification focuses on the balanced panel of firms that were continuously observed from 1988 through 1997 (panel (a) in the table). In Table 6 we only report the coefficients on the variables of interest for our results. The online appendices E and H report the coefficients for the other explanatory variables.

Columns 1, 3, and 5 present specifications without interacting the female leadership variable with the share of females in the firm's workforce, and confirm previous results from the literature: as found by Smith et al. (2006), Albanesi and Olivetti (2009), and Gagliarducci and Paserman (2015)[49] female executives do not have a significant impact on firm performance once one controls for firm fixed effects.[50]

However, when we augment the specification in order to assess Empirical Implication 2 from our model, the results are different. Columns 2, 4, and 6 of Table 6 report the estimated coefficients from specifications where female leadership is interacted with the firms' proportion of non-executive female workers.[51] Our model predicts a positive effect of this interaction because female CEOs can better allocate female workers. The higher the proportion of women, the higher the potential for gains from improved allocation. Consistently with this prediction, we find a positive and significant coefficient on the interaction variable for each of the three measures of firm performance. The magnitude of the impact is substantial: for example, looking at column (2) we see that a female CEO taking over a firm employing the average proportion of women in the sample (about 25%) would increase sales per employee by about 3.2%; if half of the firm's workers were women the impact would be about 18.5%.

As in the wage regressions section, we provide a robustness analysis of the results presented in the benchmark specification. They are reported in panels (b) to (e) and in Table 7 and they address the same set of concerns. The main results of the benchmark specification are confirmed on regressions using the full panel (panel (b)); using fraction of female executives as measure of female leadership (panel (c)); focusing on lagged effects (panel (d)) and on regressions not including the gener-

[48] We computed TFP using the Olley and Pakes (1996) procedure. See Iranzo et al. (2008) for details.

[49] Recent work on the impact of gender quotas for firms' boards have found a negative impact on short-term profits (Ahern and Dittmar, 2012; Matsa and Miller, 2013). However, these papers consider the composition of boards, not of executive bodies.

[50] The only exception is TFP, delivering a marginally significant negative impact.

[51] Just as in the wage regressions, we only focus on non-executives because the theory presented in Section 2 is not modeling promotion to executive, executive pay or interaction within executives.

Table 7: Impacts of female leadership on performance: IV estimates

Dependent variable: →	Sales per employee		Value added per employee		TFP	
	(1)	(2)	(3)	(4)	(5)	(6)
	(IV1) IV on specification (a)					
Female leadership	0.235	-0.009	-0.184	-0.989	-0.374	-1.541
2-tail p-value	0.686	0.996	0.834	0.696	0.725	0.531
Interaction		0.454		1.530		2.205
1-tail p-value		0.279		0.388		0.370
	(IV2) IV on specification (e)					
Female leadership	0.212	-0.131	-0.219	-1.174	-0.399	-1.701
2-tail p-value	0.182	0.772	0.433	0.129	0.257	0.128
Interaction		0.645		1.821		2.472
1-tail p-value		0.165		0.064		0.076
	(IV3) IV on specification (c)					
Female leadership	0.341	-0.061	-0.265	-1.113	-0.560	-1.700
2-tail p-value	0.493	0.986	0.615	0.838	0.333	0.776
Interaction		0.827		1.747		2.350
1-tail p-value		0.236		0.308		0.283
	(h) IV on specifications (c) and (e) combined					
Female leadership	0.292	-0.218	-0.323	-1.336	-0.593	-1.887
2-tail p-value	0.267	0.673	0.367	0.120	0.165	0.128
Interaction		1.051		2.087		2.668
1-tail p-value		0.080		0.067		0.086

Note: see note to table 6. P-values are computed using bootstrapped standard errors with 300 replications in specifications (IV1)-(IV3) and using standard errors clustered at the firm level in specifications (IV2) and (IV4)

ated regressors (panel (e)). The Instrumental Variable estimates also confirm the main results - a positive interaction term - but both the point estimates and the p-values are more sensitive to the specification. Focusing on female CEO impacts (Panels (IV1) and (IV2)), the IV point estimates are similar to benchmark on the sales-per-employee performance measure but they are twice as large on the Value Added measure and more than three times larger on the TFP measure. Both IV estimates have much larger p-values than the benchmark but the specification without generated regressors still delivers result within standard significance levels.

4.3.2 Summing up

Empirical Prediction 2 in our model implies that the productivity of firms with female leadership increases with the share of female workers. We assess this prediction by introducing an interaction term in firm fixed effects regressions which use three measures of firm performance as dependent variable. The crucial interaction is between the measure of female leadership and the proportion of female workers in the firm. The model predicts a positive coefficient on this interaction. All our point estimates return an economically significant positive coefficient. A series of 1-tail tests broadly reject a negative interaction in 13 out of 18 specifications. The specifications not rejecting the null at least at a 10% level report p-values fairly close to it with the exception of one of our IV specifications. We view these results as being very consistent with Empirical prediction 2 derived from the model.

We conclude our empirical analysis claiming that our theoretical model provides a unified framework able to explain the full set of empirical results found in this section. Our proposed mechanism can account for both the differential impact of a CEO's gender along the wage distribution and the positive interaction between female leadership and female workers when increasing firms' productivity. In Section 5 we discuss alternative theories that could account for these findings.

4.4 Potential efficiency gains from gender quotas

To provide a measure of the potential efficiency gains generated by increasing the presence of women in corporate leadership positions, we consider a partial-equilibrium exercise using the parameter estimates from our benchmark specification reported in Table 6. We performed two sets of counterfactual experiments where we increased the proportion of female CEOs running the firms in our sample. These higher proportions may be achieved by quota policies, by other pro-active policies or by market incentives. In the experiments, we do not factor in any cost to attain these quotas.

The first set of counterfactuals is a targeting exercise where we allocate a female CEO to all firms whose performance would improve as a result. We call this "efficient" allocation. Recall that our estimation results find a positive and significant effect only on the interaction between the female CEO dummy and the proportion of female workers at the firm. As a result, only firms with a sufficiently high proportion of women among the workforce benefit from the presence of a female CEO. The threshold value for this proportion depends on the parameter estimates and the performance measure considered. For our three measures – Sales, Value Added and

Table 8: Impact of gender quotas (average % changes)

Performance measure	Sales		Value Added		TFP	
	All firms	Treated firms	All firms	Treated firms	All firms	Treated firms
Random allocation	2.1	4.1	-1.1	-3.8	-1.9	-5.8
Efficient allocation	6.7	14.1	5.0	16.0	3.1	12.0
Quota (%):	51.4		34.7		29.1	

Average percent changes relative to benchmark. Treated firms are firms that acquire a female CEO. Random allocation: female CEOs allocated randomly to firms; Efficient allocation: female CEOs allocated to firms with a female worker share larger than the threshold generating positive gains.

TFP – the thresholds are, respectively, 19.7 percent, 30.8 percent and 34.6 percent.[52] Because the thresholds are different, so are the proportions of firms targeted by the policy: they are, respectively, 51.4 percent, 34.7 percent, and 29.1 percent (last row of Table 8). The second set of counterfactuals removes the targeting component of the previous exercise. The same proportion ("quota") of female CEOs is introduced in the economy, but female CEOs are now allocated at random instead of being assigned to the firms above the threshold. Results are reported in Table 8.

The first row of the table shows that if Female CEOs were allocated randomly, the average percent change would be small and positive when considering sales per employee and small but negative when considering the other performance measures. In contrast, our "efficient" allocation obtains large positive effects in the firms that are assigned a female CEO, and positive effects overall (second row). For example, in this scenario sales per worker would increase by 14.1% in the "treated" firms (firms whose CEO's gender has changed) and by 6.7% in the overall sample of firms. Although our exercises ignore general equilibrium effects, these results confirm that, based on our estimates, the order of magnitude of the efficiency gains from having a larger female representation in firm leadership can be large.

[52] Simplifying our performance specifications, we have: Performance $= \beta(\text{female CEO}) + \delta(\text{fem. CEO} \cdot \text{fraction of females}) +$ other variables. The threshold fraction of female workers such that a female CEO improves performance relative to a male CEO is $-\beta/\delta$. In presence of full gender symmetry, the threshold is exactly 50 percent. However, deviations from such symmetry produce thresholds away from the 50-percent cutoff. For example, if signal precision is higher when female CEOs evaluate female workers than when male CEOs evaluate males workers. Dynamic effects ignored by the model may also produce thresholds away from 50%. For example, if there is more to gain from efficiently allocating female workers than to lose by misallocating male workers. Nothing in our estimation restricts this threshold to be between zero and one but our point estimates deliver the result for all three performance measures.

5 Alternative Theories of Gender Inequality

Our empirical analysis is motivated by a theory of gender inequality focusing on asymmetric information on productivity. We find in Section 4 that our results are consistent with the predictions of this theoretical framework, which provides a unified explanation for the variety of estimation results we obtain. However, other relevant theories of gender inequality can plausibly explain or provide mechanisms for at least some of our results. We will discuss alternative models focusing on three aspects: (i) preferences for workers of a specific gender, (ii) workers' gender-specific complementarities, and (iii) compensating differentials.

5.1 Gender Preferences

In the standard setup of Becker (1971) taste-discrimination theory, employers' discrimination is modeled by assuming a disutility from employing workers of the opposite gender. The first implication of this model is that male executives pay women less than equally productive men. The gender wage gap will be larger the larger the disutility from employing women. Female executives will engage in a similar symmetric behavior against men. The result is a gender gap that is homogenous across the wage distribution, which is difficult to reconcile with the differential effects we find along the female and male wage distributions and that we report in Section 4.2.

Specifically, a theory based on preferences can explain very well the impact of a female CEO at the top of the wage distribution: a positive effect for female workers (which are not discriminated against) and negative for male workers (which are discriminated against). However, it cannot explain the effects at the bottom of the distribution, where the impact of a female CEO is a wage *decrease* for female workers and a wage *increase* for male workers. To be reconciled with all of our evidence, this theory would require the ad-hoc assumption that CEOs preferences differ in intensity by workers' wage or skill level. For example that female CEO have preferences for female workers at the top of the distribution, and hold prejudice against women at the bottom of the distribution.

A preference-based theory is also difficult to reconcile with one of our strongest findings in Section 4.3: the large, statistically significant, and robust effect on firms' performance of the interaction of female leadership with the share of female workers. Prejudiced CEOs optimize when workers' marginal products is equal to their marginal cost (wage plus a psychic cost if the worker is discriminated against). Therefore, employers have no reason to allocate workers to a task where their marginal product is lower, regardless of the worker's gender or the strength of the employer's prejudice. Productivity effects are negative only if employers misallocate workers across tasks, hence the gender composition of the workforce in itself has no implications on productivity without assuming special preferences for task assignment[53].

5.2 Complementarities Between Female Leadership and Female Workers

A second possible theory focuses on cross-workers complementarities that may affect productivity by gender differentially.

The first relevant example of this complementarity is mentoring. Mentoring is an activity in which upper-level employees are able to increase the productivity of lower-level employees with whom they are in contact.[54] Complementarities by gender take place when mentoring is more effective between employees of the same gender. We find that female CEOs and executives have a positive impact on wages of women at the top of the distribution. This is consistent with a model of mentoring under the assumptions that CEOs and top executive are more in contact with workers at the top of the wage distribution and that mentoring is more common or effective with employees of the same gender. If mentoring also has a positive impact on improving the matching of mentored workers to tasks, this theory is also consistent with our main results on productivity reported in Section 4.3.

However, mentoring has difficulty explaining our results at the bottom of the wage distribution, where women wages decrease. One possibility is that, by increasing human capital, mentoring is implicitly paid by female workers with lower current wages. Of course, this story has the opposite difficulty than the previous one: explain the increase at the top of the wage distribution. One could argue that mentoring has different effects at different stages of the workers' careers: it decreases wages in the early phase and increases it later on. To properly test this theory, one would need to follow careers development over time, something that we cannot do because of limited sample size.[55] We note however that the wage quantiles we use are computed averaging workers at various phases of their career and that we control for the average tenure of the workforce, so that it is unlikely that possible differential effects of

[53] These implications are robust to the market structure allowing the discriminating firm to survive. As pointed out by Arrow (1973), discriminating firms, being less profitable, cannot survive in a competitive environment, but when non-competitive features of the product market are present discrimination may persist. See for example Black (1995) and Rosén (2003) for theoretical contributions and Flabbi (2010) and Charles and Guryan (2008) for empirical evidence.

[54] We follow the formal definition provided in Athey et al. (2000). For examples of empirical works discussing and assessing empirical implications related to this concept, see Matsa and Miller (2013), Bell (2005), Bednar and Gicheva (2014), and Gagliarducci and Paserman (2015).

[55] We thank an anonymous referee for suggesting this mechanism.

mentoring at different stages of a worker's career can account for our results.

All in all, we believe that a mentoring explanation is less straightforward than our statistical discrimination model in explaining the opposite effects on wages at the tails of the distribution. The crucial discriminating evidence between the two models is the differential impact of female CEOs and executives at the bottom of the wage distribution, but this is also admittedly the weakest set of results in our empirical analysis. To further assess the likelihood of the two models, we have also surveyed the literature, without finding a definitive answer. Many articles confirm the positive impact of female leadership at the top of the distribution,[56] while, to the best of our knowledge, we are the first to point to a negative effect at the bottom.[57] We therefore conclude that, while our theory can give a more linear explanation of the results, additional work is needed – possibly using larger data sets with more data variation – in order to fully tell these alternative explanations apart.

Results at the bottom of the wage distribution are more consistent with another type of complementarity, which has the opposite effect: the so-called "Queen Bees".[58] In this theory, women who have managed to reach top positions in male-dominated environments intentionally damage other women's career prospects. We could not find strong supporting evidence of this behavior in the empirical economics literature. Moreover, we find it difficult to rationalize an effect where female CEOs act as benevolent mentors for women at the top of the wage distribution and as malevolent "Queen Bees" at the bottom. Finally, it is not clear what the overall impact on productivity should be of such a mixed technology. We therefore conclude that this alternative form of complementarity is at odds with our results.

5.3 Compensating differentials

An additional important explanation of gender differentials in the labor market is the presence of compensating differentials. Men and women may have different preferences over job amenities and the asymmetric provision of these amenities may be reflected in gender wage gaps and sorting by gender over jobs.[59] The argument may be relevant in our case if female-led firms offer job amenities favored by women. Women would accept lower wages to work for female leaders and the sorting between

[56] See for example Matsa and Miller (2013) and Bell (2005).

[57] Tate and Yang (2015) find significant impacts at the bottom of the wage distribution but with opposite signs with respect to us. Bednar and Gicheva (2014) look at career progression by gender in lower levels of the job ladder, instead of wages. The find no significant impact of executives' gender.

[58] See Bednar and Gicheva (2014) for use in the economics literature and Staines et al. (1974) for the original concept from psychology.

[59] E.g., preferences for job flexibility, see Wiswall and Zafar (2018) and Flabbi and Moro (2012).

workers and firms may lead to some productivity gains because female-led firms may retain more productive women at a lower wage. However, the productivity gains would be significant only if the provision of the amenity is less costly for female-led firms than for male-led firms. Moreover, the mechanism does not explain the novel result of our empirical analysis: the differential effects over the wage distribution. To obtain these differential effects, the compensating mechanism must be combined with either our statistical discrimination model or with one of the two alternative classes of explanations we discussed above. For this reason, we view this explanation as an additional channel that may magnify or reduce the effects of the other explanations.

6 Conclusion

Motivated by a recent literature showing the importance of executives' personal traits in determining firm policies and outcomes, and by the traditional literature on gender differentials in the labor market, we investigate the impact female executives have on gender-specific wage distributions and on firm performance. We find that female leadership increases the variance of women's wages within firms because of a positive impact on wages at the top of the distribution and a (smaller) negative impact on wages at the bottom. In our preferred specification, female CEOs increase wages for women in the top 25% of the women's wage distribution by about 10 percentage points and decrease wages for women in the bottom 25% by about 3 percentage points. The impact on the men's wage distribution has opposite signs. The interaction between female CEOs and the share of female workers employed has a large and statistically significant impact on firm performance. In our preferred specification, a female CEO taking over a male-managed firm with at least 25% women in the workforce increases sales per employee by 3.25%.

Our main specification accounts for endogeneity induced by non-random assignment of executives to firms by using fixed-effects and controls for firms and executives characteristics. To address the possible impact of unobservable, time-varying firm characteristics, we also propose an IV strategy. In addition, we provide several robustness exercises concerning alternative measures of female leadership, different sample selection criteria, and different measures of firm productivity.

Our empirical analysis is motivated by a model of statistical discrimination where female executives are better equipped at interpreting productivity signals from female workers. Because of this information asymmetry, female CEOs taking charge of previously male-led firms can reverse statistical discrimination, paying women wages that are closer to their actual productivity and matching them to jobs that are more in line with their skills. Our estimation results are consistent with the prediction of this theory, which provides a unified explanation for the variety of estimation results we obtain. We acknowledge that an alternative theory of gender inequality – gender-specific mentoring – can explain most of our empirical results and is in contradiction only with our least robust and least precise estimates. Additional research is needed to confirm the form of gender complementarities taking place within the firm.

Interpreting our results through the implications of our theoretical model suggests that there are potentially high costs associated with the under-representation of women at the top of corporate hierarchies. Companies with a substantial female presence are likely to benefit from assigning women to leadership positions. A partial-equilibrium counterfactual experiment based on our point estimates shows that if all the firms with at least 20% of female workers were lead by female CEOs, they would see their sales per worker increase by about 14.1%. From a public policy point of view, the same counterfactual experiment shows that an generalized extension of women in leadership positions would lead to a much smaller effect on productivity.

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