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## CEO Pay Disclosure and Within-Firm Wage Inequality

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DISCUSSION PAPER SERIES

IZA DP No. 17243

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Wage Inequality**

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## DISCUSSION PAPER SERIES

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# CEO Pay Disclosure and Within-Firm Wage Inequality

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AUGUST 2024

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## ABSTRACT

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# CEO Pay Disclosure and Within-Firm Wage Inequality\*

We analyze the effect of CEO pay disclosure on wage distribution by exploiting a 1998 reform requiring Italian publicly listed companies to disclose top executives' compensation. In firms where CEOs disclose high total compensation, the top 5 percent and 1 percent of the within-firm wage distribution rise substantially. Instead, the effect on average wages is small and only marginally significant. As a result, wage inequality increases. These effects are stronger for workers with low experience or located in the main region of the firm's operations. Moreover, they are driven by changes in workers' bargaining power, rather than by sorting.

**JEL Classification:** J31, D63, D9, M12

**Keywords:** CEO compensation, wage disclosure, income inequality, wage bargaining

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# 1 Introduction

In recent years, executives' pay in developed countries has been rising to unprecedented levels. Such a trend has caused alarm among media, practitioners, and casual observers, who often see this phenomenon as part of a more general growing trend in income inequality. As a result, policymakers have tried to address such concerns, often through mandated disclosure provisions. Most notably, starting from 2017, the US requires listed companies to disclose the CEO-to-median pay ratio, and a similar provision was enacted in 2019 for companies listed in the UK with more than 250 employees. The rationale for this policy is that increasing transparency regarding within-firm inequality may lead to a reduction in excessive disparity in compensation to prevent negative reactions in public opinion.

The idea of establishing transparency provisions is not new. Higher CEO pay can be a reward for the skills required to manage large and complex organizations (Gabaix and Landier, 2008) but could also be the result of poor governance (Bertrand and Mullainathan, 1999). Policymakers have historically enforced disclosure requirements for CEO pay as an attempt to improve accountability to investors and the general public. Although in the US listed firms have been disclosing compensation of top executives starting from the 1934 Securities and Exchange Act, in other countries analogous requirements are much more recent. For example, in 2010 only a minority of jurisdictions in OECD economies required disclosure of top managers' individual remuneration. However, as of 2021 disclosure was mandatory for about three-quarters of the jurisdictions surveyed.<sup>1</sup>

While these provisions have by now become common, to our knowledge the effects of transparency of board compensation on the distribution of wages within the firm have never been formally measured. Individuals do not care only for their own well-being; if they perceive that they are being treated unfairly, they may take actions to "hurt" others (Rabin, 1993; Fehr and Schmidt, 1999). Intuitively, CEO pay disclosure may affect workers' morale and perception of fairness. If employees believe that the top decision-maker is pocketing an excessive share of the firm's surplus, they may respond by reducing their effort to punish management. To counteract this reaction, CEOs may have no choice but to increase wages.<sup>2</sup> Depending on which workers are

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<sup>1</sup>See OECD (2021), "Corporate Governance Factbook," <https://www.oecd.org/corporate/corporategovernance-factbook.htm>

<sup>2</sup>Even in a setting where workers have no bargaining power and no possibility of "sabotaging"

affected, inequality may increase or decrease as a result.

In this paper we study the effect of the disclosure mandate for executives and members of the board of directors that went into effect in Italy in 1998 for all the publicly listed companies. The Italian setting is an interesting one, as Italy was a relatively early adopter of such a disclosure requirement. More importantly, we can combine newly digitized data on board composition and executives' pay together with employer-employee data from the Italian Institute of Social Security (INPS).<sup>3</sup> Thus, we have a unique opportunity to examine the effects of CEO pay transparency on within-firm inequality. Specifically, we can investigate several questions. Does disclosure of high CEO remunerations lead to changes in compensation of the other employees? Does its impact differ across workers? If so, what are the implications for wage inequality? Addressing these questions has been a challenging task primarily for lack of data. For example, in the US it would be virtually impossible to obtain detailed information on individual workers' wages dating back to the 1930s, and one of the few studies on the topic (Mas, 2019) uses aggregate time series data.

To perform our analysis, we digitize data on top managers' pay, obtained from firms' financial statements, and merge it with social security data on private sector employees. Our final sample includes information on wages and careers of all the employees of 89 publicly listed companies affected by the disclosure mandate.

In our econometric design, we adopt a simple difference-in-differences approach. Our "treatment" is the 1998 compensation of the top executive (usually the CEO), disclosed for the first time at the end of the fiscal year (i.e., at the beginning of the year 1999 for most companies). Then, we examine how workers' wages evolve following CEO pay disclosure. Our objective is to test whether the wages' trajectories post disclosure differ depending on the level of disclosed CEO pay.

We first show that there is a positive relationship between the disclosed level of CEO pay and subsequent average wages. This effect is, however, economically small and only marginally significant. However, we uncover highly heterogeneous effects across the wage distribution. Specifically, wages at the highest percentiles of the within-firm distribution increase substantially in firms that disclose high CEO pay.

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the firm's operations, disclosure could still result in higher wages. Intuitively, workers' utility must still be equal to their reservation level. If the perception of unfairness in compensation reduces the utility of working for the firm, the monetary compensation must increase accordingly, to prevent workers from quitting.

<sup>3</sup>The data have been accessed through the VisitINPS Scholars program.

Our baseline estimates of the “pass through” suggest that a 1 percent increase in disclosed compensation of the top executive leads to a 6-basis-point increase in wages for the workers at the 95<sup>th</sup> percentile and to a 17-basis-point increase for workers at the 99<sup>th</sup> percentile. Focusing on the 1<sup>st</sup>, 5<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles, we find that coefficients generally increase as we move from the lower to the higher percentiles. As a result, within-firm inequality, as measured by either the log difference between the top and bottom percentiles of the within-firm wages distribution, or by the Gini coefficient, exhibits a positive relationship with disclosed CEO pay.

This result is robust to several additional tests. In our baseline tests we control, beyond firm fixed effects, for industry-year fixed effects, following the recommendations of Gormley and Matsa (2016). However, our results remain similar when employing a less conservative specification, where we only control for year fixed effects, and a more conservative one, where we further control for year–size quintile fixed effects (where size is proxied by market capitalization). In this last test, we find that the coefficient of interest increases in magnitude. This is not surprising: As size and firm industry are strong predictors of CEO compensation (Gabaix and Landier, 2008), our model in this case effectively captures the *unexpected* component of disclosed CEO compensation. We also find that our results are robust to the exclusion of workers classified as managers, suggesting that changes in pay of executives and board members are not driving the results.

In sum, our evidence suggests that CEO pay disclosure leads to wage increases in firms that disclose high CEO pay. However, the effect is highly heterogeneous and benefits only the workers on the right tail of the within-firm wage distribution. One possible explanation is that only the top earners of the firm perceive the CEO as a “quasi-peer” and can therefore be influenced by the disclosure. Moreover, such workers are likely to interact more frequently with the top management of the firm and are thus able to more forcefully bargain for higher salary. Conversely, the effect of disclosure does not appear to “trickle down” to workers in lower positions of the corporate hierarchy.

We should point out that we do not have exogenous variation in the level of CEO compensation, which imposes caution on a causal interpretation of the evidence. However, we are able to rule out the most plausible alternative stories. A first possibility is that high CEO compensation is simply reflecting an upward trend in the compensation of the firm’s top earners. However, once we run event-study regressions to

examine the timing of the effect, we find that average wages start rising in firms that disclose high CEO pay only *after* the actual disclosure. In addition, the 1998 CEO pay is uncorrelated with changes in investment rates, profitability, size, or valuation. Hence, it is not proxying for trends in firm characteristics which may, in turn, be related to wages.

A second possibility is that it is not the disclosure per se that leads to a change in the firm’s compensation practices. Rather, the results could be mechanical or spurious, and could be observed independent of the actual disclosure event. To rule out this possibility, we perform a simple “placebo” test. We run the exact same specification in a different year, 2011, when the disclosure requirement had been in place for several years, and show that this “pseudo”-event has no effect on realized wages.

We then move to identify the economic channels driving our results. We explore heterogeneity across workers’ characteristics, such as gender, experience, and location. We find no evidence of differential effects of disclosure on wages between men and women. However, wages of workers with low experience appear to react more to CEO pay disclosure than high-experience workers. This is consistent with the disclosure of CEO pay being a significant informational shock for workers with a relatively low knowledge of the labor market and thus of prevailing compensation practices. Moreover, we find that the effect is also heterogeneous with respect to the location of the workers; we uncover a higher elasticity of response to the disclosed CEO compensation when workers are employed in the main region of the firm’s operation, suggesting that they may be able to put stronger pressure on executives (Landier et al., 2009). In both cases, we find positive and significant impacts on average wages driven by the effects on the top percentiles of the wage distribution.

We do not find evidence that the effect of wage disclosure is driven by changes in workforce composition: CEO pay disclosure does not appear to affect age or turnover and, if anything, is negatively related to the proportion of males and full-time workers. Moreover, we find qualitatively similar results once we restrict our attention to workers employed in the firms *prior* to the enactment of the disclosure mandate. Thus, disclosure does not seem to affect wages through sorting, namely by attracting a different pool of workers. Rather, disclosure appears to affect the bargaining position of incumbent workers.

This paper contributes to the literature on the real effects on disclosure, by study-



ing its impact on wage setting in the private sector. It also contributes to the literature studying the determinants of inequality. While most of the empirical studies focus on aggregate trends in inequality,<sup>4</sup> this paper studies how an informational shocks, namely disclosure of CEO compensation, affects *within-firm* wage inequality.

**Related Literature.** Our paper is related to a fairly recent, but active, literature on wage disclosure. The first and most well-known example of mandated pay disclosure occurred in the United States in 1934. Mas (2019) examines the evolution of CEO compensation during the Great Depression and shows that, after the mandated pay disclosure, average CEO compensation *increased* relative to the upper quantiles of the non-CEO labor income distribution. Moreover, pay disclosure appears to have led to a compression of the distribution of CEO earnings.

More recently, Pan et al. (2022) exploit a 2018 law that mandated disclosure of the CEO-worker pay ratio for US public companies. They find that companies disclosing higher pay ratios experience significantly lower abnormal announcement returns. This is in part due to more inequality-averse investors rebalancing their portfolios.

Also in the corporate setting, Cullen and Perez-Truglia (2022), using data from a bank in Southeast Asia, show that employees systematically underestimate managers' compensation. As a result, disclosure of their actual compensation has significant effects on their behavior. Although in this paper we do not have information on employees' priors, we believe that this evidence further validates our assumption that disclosure induces significant revisions in employees' beliefs regarding top managers' remuneration.

In an influential study, Luttmer (2005) finds that individuals' well-being is affected not only by their income level but also by their earnings relative to neighbors. Hence, policies enhancing earnings transparency may influence individuals' perceptions regarding their position in the income distribution and, as a result, their level of happiness. Based on this insight, Perez-Truglia (2020) exploits the decision of Norwegian policymakers to make tax records easily accessible online and finds that the disclosure increased the gap in happiness and life satisfaction between richer and poorer individuals. Similar conclusions are reached by Card et al. (2012), who conduct a field experiment by sharing with a random sample of University of California

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<sup>4</sup>Time-series evidence on income inequality, measured using from micro data, has been presented, for example, by Piketty and Saez (2003) for the United States, by Dustmann et al. (2009) for Germany, and by Acciari et al. (2022) for Italy.

employees the existence of a new website listing the pay of University employees. They show that those with below-the-median compensation report lower levels of happiness and satisfaction relative to employees who were not informed about the existence of the website.

This evidence suggests that disclosure can affect employees' well-being. The hypothesis underlying the analyses in this paper is that changes in workers' utility affects the pay setting process as well. There is evidence supporting this hypothesis, although primarily from the public sector. [Mas \(2017\)](#) studies the effect of disclosure of salaries of municipal managers in 2010 in California and finds that it led to a decline in compensation, as well as an increase in the quit rate. [Baker et al. \(2023\)](#) focus instead on the gender gap and find that public sector salary disclosure laws on university faculty salaries in Canada substantially reduced the gender pay gap between men and women.

The paper closest to ours is [Dittmann et al. \(2018\)](#). They find that average wages are positively related to CEO compensation in a sample of German companies. To establish causality, they leverage a regulatory shock mandating the disclosure of CEO pay (for those firms that were not already voluntarily disclosing it). Our paper differs because of our focus on within-firm inequality; indeed, different from [Dittmann et al. \(2018\)](#), we detect only a slightly significant effect of disclosure on average compensation and stronger effects for workers in the top percentile of the within-firm wage distribution. Exploiting our worker-level data, we also detect substantial heterogeneity over several dimensions, such as proximity to the headquarters and experience in the labor market, in the effect of disclosure on workers' wages.

The rest of this paper is organized as follows. Section [2](#) describes institutional setting, data, and the econometric strategy. Section [3](#) presents the main results and some robustness checks. Section [4](#) presents additional analyses to highlight the economic channels. Section [5](#) concludes.

## 2 Institutional Setting, Data, and Econometric Strategy

### 2.1 Institutional Setting

The policy experiment studied in this paper relies on the so-called “Legge Draghi” (Legislative Degree 58, 1998), which was shortly thereafter implemented by the public authority responsible for regulating the Italian financial markets, the Commissione Nazionale per le Società e la Borsa (CONSOB), through its rules (Delibera 11520, July 2, 1998). These rules require all publicly listed companies in Italy to draft and release a remuneration report.

The remuneration report includes two sections. The first section reports companies’ remuneration policies for the board members and directors, as well as the procedures adopted to implement these policies. The second section lists analytically all the remuneration components received by board members. The disclosure obligation concerns not only the compensation received by the main employer but also those received by subsidiaries and affiliates of the listed company. All the components of the remuneration package are split between fixed compensation, remuneration for the participation to board committees, bonuses, etc. Detailed information is also given for the variable part of the remuneration, distinguishing between stock options, equity-based compensation other than stock options, and nonequity variable compensation.

At the time, the new regulation was widely discussed in the media and generated some controversy. For example, in May, the most widely circulated Italian daily newspaper, *Corriere della Sera*, lauded the decision as promoting transparency and realigning the Italian governance system to the American one. A few weeks later, in the same newspaper, an influential jurist argued in an op-ed that the disclosure requirement had the purpose of merely satisfying investors’ “morbid and futile curiosity.”<sup>5</sup> *Assonime*, the association representing Italian publicly listed firms, was also strongly opposed, suggesting that the disclosure of managers’ compensation was dangerous, as it could lead to kidnappings.<sup>6</sup>

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<sup>5</sup>See “Più trasparenza nei compensi dei manager” by Luigi Zingales and “Diritti degli azionisti e compensi degli amministratori” by Natalino Irti (*Corriere della Sera*, May 24, 1998 and July 2, 1998, respectively).

<sup>6</sup>See “Il segreto più caro del manager” (*La Repubblica*, November 23, 1998).

This anecdotal evidence suggests that there was widespread awareness of the new disclosure provision. Moreover, the actual *release* of the compensation of members of the board was, and is, generally accompanied by detailed reports in the popular press, often with attention-grabbing headlines. Hence, the assumption that not only market participants, but also less sophisticated observers, including firms' workers, were aware of the new regulation and of the actual disclosed compensations, appears to be a plausible one.

## 2.2 Data Sources

The empirical analysis is conducted by linking data from three different sources. We start from the list of companies listed with the Italian stock exchange in 1998, which are subject to the disclosure mandate. We obtain scanned copies of financial statements for the Italian listed companies from Infocamere, a database managed by the Chamber of Commerce. We were ultimately able to obtain full compensation reports for 116 companies, out of the 227 that were publicly listed at the end of 1998, and manually digitize information on pay of all board members for roughly 1,800 individual records. For each board member we know total pay and how it is split between base salary, bonus, compensation from controlled companies, etc. From this sample, we identify the highest paid executive. In the rest of this paper, we will slightly abuse terminology and refer to them as the CEO, even though it is in some cases the president who is the most influential and highest paid executive (Volpin, 2002). In some rare cases, executives are granted stock options as well. We use the Black-Scholes formula to estimate their value; Appendix A.1 reports additional details regarding the assumptions made for the computation.

In the INPS data, every employer is identified by its fiscal code, which we retrieve from the financial statements. Hence, we can match our set of listed companies to the worker-level information available in the INPS data and to the firm-level information from Compustat Global.<sup>7</sup> The INPS data cover the entire population of workers

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<sup>7</sup>At this stage it is worth pointing out that our dataset does not include the universe of workers employed by the firms in our sample for two reasons. First, we do not observe workers employed overseas. Second, we do not have information on subsidiaries employed by the same corporate group. We match the CEO compensation data using the fiscal identifier of the parent company; hence, if a worker is employed by a subsidiary with a different fiscal identifier, they will not be included in the sample. This is not necessarily a disadvantage, as it will reduce the impact of sharp compositional changes due to mergers or spin-offs.

in the Italian private sector, excluding agriculture, and include detailed information on about 18 million workers and 1.5 million firms per year. We focus on the nine years surrounding the reform (that is, from 1994 to 2002). For each worker we know wage, days worked during the year, type of contract, and job location as well as basic demographic information, such as gender, age, and city of birth. Given our emphasis on within-firm inequality, we also compute different percentiles of the within-firm wage distribution, the difference between percentiles, and the Gini coefficient.

Finally, we merge this dataset with Compustat Global, which provides us with balance sheet information for publicly listed firms. From Compustat we retrieve information on companies' industries (defined at the two-digit SIC level), which we use in all the econometric specification, as we will control for industry-year fixed effects. We also collect data on market capitalization, which we use in some robustness checks. After merging these three datasets, our final sample includes 89 firms.

## 2.3 Econometric Strategy

As discussed in the introduction, in our analysis we put special emphasis on the distributional consequences of CEO pay disclosure. Specifically, we want to test whether the policy leads to a uniform shift in wages or if it affects some workers more than others. For this reason, our baseline model is the following:

$$\log(Wage_{i,t,p}) = \beta_1 \log(CEO\ Compensation_i) \times Post_t + \delta_{t,j} + \gamma_i + \varepsilon_{i,t,p} \quad (1)$$

where  $i$ ,  $j$ , and  $t$  index firms, industries, and years, respectively.  $p$  indexes different percentiles of the within-firm wage distribution. For each firm and year, we rank workers' daily wages and record the 1<sup>st</sup>, 5<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles. In some specifications, the dependent variable is simply the logarithm of the average wage. *CEO Compensation* is the total compensation of the highest paid executive (usually the CEO), held fixed at 1998, the first year in which executives' compensation was disclosed. *Post* is a dummy equal to 1 from 1998 onward and  $\delta_{t,j}$  and  $\gamma_i$  are year-industry and firm fixed effects, respectively. Our key coefficient of interest,  $\beta_1$ , has an intuitive interpretation. It corresponds to the elasticity of the wage corresponding to the  $p^{\text{th}}$  percentile to the disclosed CEO compensation. Notice that we include the year 1998 as a "treatment year," as the disclosure policy was announced in July 1998, and managers could already start adjusting workers' compensation before the actual

disclosure of their compensation, which would occur with the release of the financial statements (usually 4–6 months after the end of the fiscal year). Moreover, as some firms have their fiscal year ending prior to the end of the calendar year, usually in June, for those the disclosure of directors’ compensation had already occurred in 1998.

Following Gormley and Matsa (2016), we do not include control variables in our baseline model, which may be endogenous,<sup>8</sup> but control for year-industry fixed effects to account for the possibility that industries may be characterized by different trends in their compensation practices. However, as we show below, results are qualitatively similar if we choose either more or less restrictive approaches. In all the tests that follow, observations are weighted by firm size, measured by the number of employees. Standard errors are clustered at the firm level.

Table 1 reports descriptive statistics for the 89 firms included in the sample, for a total 798 firm-year observations. The average daily wage is €123.25. The rows that follow show that there is substantial heterogeneity in the compensation practices of the firms included in the sample. The average wage for a worker included in the bottom percentile of the distribution is €44.83. If we move to workers in the top percentile, the daily wage is over 10 times higher, €536.84.

The last two rows report statistics regarding CEO compensation and total market capitalization (that is, stock price times the number of shares outstanding), again measured at the end of 1998. Both measures are winsorized at the 2.5<sup>th</sup> and 97.5<sup>th</sup> percentiles to reduce the influence of outliers. The typical CEO earns about €465 thousand per year, but there is again enormous variation. As expected, the distribution is highly skewed. The CEO compensation ranges between €12 thousand and €2.7 million, and the median is €361.5 thousand. Firms differ also with regard to the total equity value, which ranges between €10 million and €45 billion. Given the link between firm size and CEO pay (Gabaix and Landier, 2008; Gabaix et al., 2014), we will explicitly account for such heterogeneity in some specifications.

## 2.4 Trends in CEO Compensation and Firm-Level Variables

As we will discuss in the next section, a key threat to the identification strategy is that CEO compensation may be correlated with some omitted variable which, in turns, affects workers’ wages. While we discuss and address this concern in several

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<sup>8</sup>See also Angrist and Pischke (2009) for a discussion of the biases generated by the inclusion in a regression model of “bad controls.”

ways in Section 3.2, we present some reassuring evidence in Figure 1. Specifically, we show six scatter plots, with the the logarithm of CEO compensation on the  $x$ -axis and the 1997-to-1998 change in proxies for growth opportunity (logarithm of sales, investment, and Tobin’s Q), profitability (return-on-income and operating return-on-income) and size (logarithm of total assets).<sup>9</sup> As in our tests we will be controlling for industry-year fixed effects, we regress all the variables on industry dummies and plot the residuals. (Conclusions are unchanged if we plot the raw values.)

As it is apparent from the six panels, CEO compensation does not appear to be related to unusual growth in any of the firm-level variables. We also overlay the lines obtained from a linear regression of each firm variable on the logarithm of CEO compensation (again, after taking the residuals). Not only all the slope coefficients are insignificant, but they also do not point towards a consistent pattern. For example, CEO compensation has a negative correlation with sale growth and Tobin’s Q growth, but a positive relationship with investment growth. The correlations between CEO pay and asset growth, return-on-income, and operating return-on-income are all close to zero. Hence, there is little evidence that CEO compensation may be proxying for changes in firm-level characteristics, at least with regard to observable ones.

## 3 Results

### 3.1 Baseline Results

In Table 2 we present our baseline results, by reporting estimates of the coefficient  $\beta_1$  in equation (1). In column 1 the dependent variable is the logarithm of the average wage. We find a positive coefficient, equal to 0.036 and significant at the 10% level (standard error=0.019). Hence, there is some evidence that CEO compensation disclosure impacts average wages in a meaningful way, although the pass-through from CEO pay to workers’ wages is not quantitatively large.

In columns 2 through 8 we test whether CEO pay disclosure has *distributional* consequences. Specifically, the dependent variables are now different percentiles of

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<sup>9</sup>Investment is defined as capital expenditures scaled by lagged total assets. Return-on-income (ROA) and operating return-on-income (operating ROA) are defined as net income and earnings before interest, depreciation, and amortization scaled by lagged total assets, respectively. Tobin’s Q is total assets minus market value of equity (number of shares times fiscal-year-end share price) minus book value of equity, all scaled by total assets.

the wage distribution. As discussed in Section 2.3, we focus on the following percentiles: 1, 5, 25, 50, 75, 95, and 99. The coefficients generally increase as we move from the lowest to the highest percentiles. They are fairly small in the percentiles 1 through 75, ranging between  $-0.017$  and  $0.028$ . However, the elasticity of wages to CEO compensation increases to  $0.059$  for the 95<sup>th</sup> percentile and  $0.170$  for the 99<sup>th</sup> percentiles. In both cases, the coefficients are statistically significant at conventional levels (at the 5% and 1%, levels, with standard errors equal to  $0.026$  and  $0.060$ , respectively). Hence, the effects of CEO pay disclosure are highly heterogeneous and primarily affect the right tail of the wage distribution. These results imply that an increase in disclosed CEO pay leads to a more rightly skewed within-firm wage distribution.

An advantage of our design is that we can examine the timing of the effect of disclosure, strengthening a causal interpretation of the results. Specifically, we examine the *dynamic* effect of CEO pay disclosure on different percentiles of the wage distribution by estimating an event-study version of equation (1), given by:

$$\log(Wage_{i,t,p}) = \sum_{\tau} \beta_{\tau} \log(CEO\ Compensation_i) \times \mathbb{1}(t = \tau) + \delta_t + \gamma_i + \eta_{i,t,p} \quad (2)$$

In Figure 2,  $\beta_0$  corresponds to the year of the disclosure policy, 1998, and we normalize the coefficient  $\beta_{-1}$  to zero.<sup>10</sup> We do not find indication of diverging trends prior to the year 1998 for any of the dependent variables. Starting from the year of the policy reform, wages at the 1<sup>st</sup>, 25<sup>th</sup>, and 50<sup>th</sup> percentiles exhibit little reaction. Conversely, wages at the 95<sup>th</sup> and, especially, 99<sup>th</sup> percentiles exhibit large elasticities to CEO disclosed compensation. For completeness, Figure 3 restricts attention to the 99<sup>th</sup> percentile of the within-firm wage distribution, where we detect the strongest effects, and displays not only the coefficients but also the confidence intervals.

## 3.2 Robustness Tests

In this section we establish the robustness of our key result of the effect of CEO pay disclosure on wages. We start by estimating, in Table 3, two variations over the baseline econometric design presented in equation (1). In Panel A we replace the year-industry fixed effects with year fixed effects. Without accounting for heterogeneity

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<sup>10</sup>To avoid clutter, the figure omits the results for the 25<sup>th</sup> and the 75<sup>th</sup> percentiles, which are available upon request.



at the industry level, results remain qualitatively similar, with the coefficients rising almost monotonically as we move from the lowest to the highest percentiles. Although the magnitudes of the coefficients are smaller, we still detect a statistically significant effect on wages at the 99<sup>th</sup> percentile, with a coefficient equal to 0.097 (standard error=0.044).

In Panel B we employ instead a more *demanding* specification. Inspired by Gabaix and Landier (2008), who model CEO compensation as a function of firm size and industry, we now further control for time-size quintiles, where size is measured as the market value of equity. In this way, we control for differences in wage trends that may affect firms depending on heterogeneity not only with respect to their industry but also to their size. We find estimates that are quantitatively similar to those found in the baseline tests of Table 2 but more precise. Although the effect on average wages is weak, the coefficients estimated in the regressions that have, as dependent variables, the logarithm of wages at the 95<sup>th</sup> and 99<sup>th</sup> percentiles are slightly larger, equal to 0.074 and 0.219, respectively, and significant at the 5% and 1% level, respectively.

Although we use as our “treatment” the disclosure of CEO pay, her compensation may be closely related to that of her peers, such as directors or top executives. As these employees are likely to be located in the top percentiles of the within-firm wage distribution, there is the possibility that we are simply capturing some common component in top earners’ compensation. Unfortunately we cannot identify individuals in the social security data, so we cannot directly exclude them from the analysis. As an alternative solution, we use the information on workers’ occupation in the INPS data and exclude all the individuals classified as managers (*dirigenti*). This category is likely to include not only top executives and directors but also managers in lower positions of the corporate hierarchy (such as managers of subsidiaries), and it is thus broader. We re-estimate equation (2) by excluding all the individuals classified as managers in 1997, the year prior to the enactment of the disclosure requirement. The results, reported in Appendix-Table A1 are very similar to the baseline ones of Table 2.

A caveat underlying our analysis is that we do not have exogenous variation in CEO compensation. Thus, alternative interpretations not relying on a causal interpretation of the evidence are also possible. One possibility is that our design is merely picking up firms that, for unobserved reasons, decide to increase compensation for their top earners. As a result, the key coefficient of interest may simply be

capturing the comovement of wages across the top firm earners, including the CEO, in the firms in our sample.

Crucially, in this story, the identical pattern would be observed even if our reference treatment year was not the year of the enactment of the CEO pay disclosure policy. This observation motivates a simple “placebo” test, run in Table 4. We replicate exactly the same design of equation (1) but impose a “pseudo”-reform year. The dataset now comprises the publicly listed firms for which we have CEO compensation data between the years 2007 and 2015, and we estimate equation (1) using the CEO compensation from the year 2011.<sup>11</sup>

As shown in Table 4, all the coefficients are, however, small and insignificant. Indeed, the coefficient in column 8, where the dependent variable is the logarithm of the wage measured at the 99<sup>th</sup> percentile of the firm distribution, is, if anything, *negative*. Figure 4 displays coefficients obtained after estimating equation (2) with the 99<sup>th</sup> percentile of the logarithm of wages as dependent variable and shows that, again, all the coefficients are very close to zero and insignificant. This is consistent with our hypothesis that it is indeed the effect of disclosure that drives our results. Given that, in 2011, the CEO compensation disclosure had already been in place for several years, it should have no effect on the distribution of workers’ wages. Conversely, in the year of enactment of the policy, there is an actual revelation of new information, leading to changes in the wage distribution.

### 3.3 Within-Firm Wage Inequality

As an alternative way to present our results, inspired by the evidence in Section 3.1, we can also examine the effect of CEO pay disclosure on several measures of within-firm wage inequality. In this exercise, we again estimate equation (1) but use as dependent variables the log-differences of wages measured at different percentiles. We study the 75<sup>th</sup>–25<sup>th</sup>, the 90<sup>th</sup>–10<sup>th</sup>, and the 95<sup>th</sup>–5<sup>th</sup>, and the 99<sup>th</sup>–1<sup>st</sup> differences. In addition, as a comprehensive measure of wage inequality, we compute the Gini coefficient.

In columns 1, 2, and 3 of Table 5 we find consistently positive coefficients, albeit insignificant for the 75<sup>th</sup>–25<sup>th</sup>, the 90<sup>th</sup>–10<sup>th</sup>, the 95<sup>th</sup>–5<sup>th</sup> differences. Conversely, in column 4 we detect a more precise effect of disclosure on the log-difference 99<sup>th</sup>–1<sup>st</sup>,

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<sup>11</sup>The data were shared with us by Faia et al. (2021).

with a coefficient equal to 0.187 and significant at the 1% level. Notice also the coefficients are monotonically increasing. Given the increase of wages in the right tail of the distribution, without a corresponding change in the left tail, we also ask, in column 5, whether disclosure affects the Gini coefficient (multiplied by 100 for ease of interpretation). We do indeed find a positive coefficient, equal to 0.016 and significant at the 10% level.

## 4 Heterogeneous Effects of Disclosure

### 4.1 Cross-Sectional Heterogeneity

In this section we exploit heterogeneity in workers' characteristics to shed light on the economic channels that can rationalize these results. We exploit the rich set of characteristics that we can measure in the INPS data and compare the effect of disclosure across different dimensions, such as gender, experience, and location. These comparisons are presented in Table 6. For brevity, we report only regressions where the dependent variables are average wages, wages measured at the 1<sup>st</sup>, 5<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles, as well as the log-difference between the wages measured at the 99<sup>th</sup> and 1<sup>st</sup> percentiles. Wage percentiles are recomputed for each firm-year-worker category combination.

We start our analysis by distinguishing, in Panels A and B, between female and male workers. This test is motivated by the evidence that women may have lower bargaining power (Dittrich et al., 2014; Card et al., 2016); thus, top male workers may be better equipped to capture wage increases resulting from the disclosure of high CEO compensation. We find that a 1 percent increase in CEO compensation leads, on average, to similar changes in average wages for women and men (0.037 and 0.040, respectively). In both cases, these effects are imprecisely estimated. As in the baseline analysis of Table 3.1, only wages of workers in the top percentiles of the within-firm distribution do exhibit a statistically significant and economically meaningful reaction. The coefficients are slightly higher for female workers than for male workers at the 95<sup>th</sup> percentiles (0.098 versus 0.072) and slightly higher for male workers than for female workers at the 99<sup>th</sup> percentiles (0.139 versus 0.083). Finally, once we focus on the log-difference between wages at the top and the bottom 1% of the wage distribution, our measure of inequality, we find similar effects, with

coefficients around 0.11–0.12, significant at the 10% level. Thus, there is no evidence of a differential effect of disclosure on wages based on gender. As a result, disclosure does not appear to affect the gender pay gap.

Next, we analyze the effect of disclosure on workers who differ with respect to their experience in the labor market. Our hypothesis is that, as the CEO pay disclosure is an informational shock, it is going to be more significant for workers with relatively low knowledge of the labor market and, thus, may not be able to predict the compensation of the top firm earners. We take advantage of the fact that the INPS dataset has information on employment spells starting from 1972 and define workers’ experience as the number of years with at least one employment spell. Then, each year we classify workers in “high” and “low” experience categories, based on whether they are below or above the firm-level median.

Panels C and D reveal a significant heterogeneity with respect to workers’ experience. Wages of workers with low experience appear to react more to disclosure: The coefficient we estimate when examining the effect on average wages is 0.072, significant at the 5% level. Conversely, the point estimate estimated in the high-experience subsample is very close to zero and insignificant. Examination of the effect on low-experience workers at higher percentiles of the wage distribution shows that there are also large and significant effects for workers at the 95<sup>th</sup> and 99<sup>th</sup> percentiles. The effect is more muted for high-experience workers, although we do detect a marginally significant effect for workers at the 99<sup>th</sup> percentile (coefficient equal to 0.104, significant at the 10% level). The impact on inequality (column 6) is significant in both subsamples, but the coefficient is higher in the low experience subgroups (0.230 versus 0.146). As a robustness check, in Appendix [A.2](#) we sort workers using different proxies for knowledge of the labor market, namely tenure with the current employer and age, and find qualitatively similar results (see Appendix-Table [A2](#)).

As an additional layer of heterogeneity, we test whether results differ with respect to the *location* of the workers. Existing empirical evidence supports the view that workers can benefit from being closer to the corporate headquarter. [Landier et al. \(2009\)](#) show that dismissals of workers employed in divisions closer to the headquarters are less common. They argue that such employees can benefit from more frequent social interactions with the managers. Similarly, [Cronqvist et al. \(2009\)](#) find that entrenched managers pay their employees more especially if they are closer to the headquarters.

Building on this insight, we examine the heterogeneity of the effect of disclosure with respect to the workers’ region of employment. For each firm and region we compute the total number of workers, and we identify the region with the highest number of employees as the “main region,” where the key operations of the firm are likely to be concentrated. Panel E presents regressions run on the subsample of workers employed in the main region; the regressions in Panel F include all the other workers. We find large and significant effects of disclosure on average wages of workers employed in the main region, with a coefficient equal to 0.063 and significant at the 5% level. The coefficient drops to an insignificant 0.009 for workers employed in other regions. Panel A also shows that the effect is monotonically increasing in the percentile of the within-firm wage distribution, being driven as usual by the top earners. The coefficients are 0.104 and 0.271 for the 95<sup>th</sup> and 99<sup>th</sup> percentiles, respectively, both significant at the 1% level. We also find a strong effect on inequality (column 6), with a coefficient equal to 0.296. With regard to workers employed in regions other than the main one, the coefficients are consistently small and insignificant.

## 4.2 Workforce Composition and Incumbent Workers’ Wages

The increase in salaries for the highest paid workers can occur through two channels. First, a “sorting” channel could be in place. Firms disclosing high compensation for their CEOs could become more appealing to workers attracted by the possibility of higher salaries. In this case, we should observe a change in the workforce composition, with a higher tilt towards workers who can, on average, earn higher salaries, resulting in the effect of pay disclosure on wages observed in Section 3. The effect on *incumbent* workers, that is, workers hired prior to the disclosure shock, may be muted or even absent.

Conversely, if the revelation of CEO compensation affects the bargaining power of all the workers, and thus the ability to extract higher rents in firms with high CEO pay, we should also observe an effect on incumbent workers. The revelation of this information should equally affect both existing employees and workers who join the firm after the disclosure.

In Table 7, we estimate a variation of equation (1) where we use as dependent variables several characteristics of the workforce. We start, in column 1, with the average age, measured in years. Intuitively, disclosure of high CEO pay may attract more

experienced workers, who may more realistically aim to obtain managerial positions. We find, however a coefficient small in magnitude and insignificant.

In columns 2 and 3, we examine whether disclosure induces a change in the fraction of males or full-time workers. As these workers enjoy, on average, higher compensation, a recomposition in this respect may be responsible, at least in part, for the change in average wages. The coefficients are both negative, and significant at the 5% and 10% level, respectively. As male and full-time workers enjoy, on average, a higher compensation, these results suggest that compositional effects induced by the disclosure requirement would, if anything, lead to a *reduction* in wages.

Finally, in column 4 we more directly test whether disclosure leads to a reshuffling of the workforce. We use as dependent variable the number of new employees, namely workers who were not employed in the same firm in the previous year, scaled by the total number of workers in each year. Even though we find a positive coefficient, equal to 0.095, it is noisily estimated.

Thus, this evidence suggests that the effect of disclosure on wage distribution is unlikely to be driven by changes in workforce composition. In Table [8](#), we perform a different exercise, where we include only “stayers,” namely those workers who were in the firm in the year prior to the disclosure regulation, 1997, and exclude the workers who join the company afterwards. In this way, we compute average compensation and within-firm percentiles net of any effects due to new hires.

It is worth noticing that in this specification we are conditioning on an “ex post” decision, namely the choice to remain in the firm. The fact that the decision to stay with the same employer is itself an outcome variable, as it can be potentially affected by the CEO pay disclosure, can introduce a bias in the estimates. Also notice that, due to attrition, average wages and quantiles are computed on a smaller set of workers as we move away from the event year. Hence, the estimates are not directly comparable to those presented in Table [2](#). Notwithstanding these caveats, this analysis can still be informative, as its purpose is to isolate the effect of CEO pay disclosure on wages that is solely due to a change in the bargaining position of the incumbent workers.

We find that, even in this sample, disclosure has an effect on the wages of the top firm earners. The coefficient when the dependent variable is the wage measured at the top 99<sup>th</sup> percentile is 0.129, significant at the 1% level. All the other coefficients are small and insignificant. As a result, the effect on the log-difference between the 99<sup>th</sup>

and 1<sup>st</sup> percentiles is also fairly large, 0.144, and significant at the 1% level. Hence, although results are somewhat weaker once we focus on this smaller subsample of workers, they broadly confirm the pattern observed in the full sample, suggesting that CEO pay disclosure leads to changes in wage distribution primarily by affecting the compensation of incumbent workers.

## 5 Conclusion

This paper presents new empirical findings on the effects of disclosure of salaries of firms' managers and directors. To study this question, we take advantage of the passage of a disclosure mandate in Italy in 1998 and make use of unique hand-collected data on the compensation of managers and directors of publicly listed firms, combined with detailed administrative data on wages of private sector employees.

We then examine the trajectory of wages in companies affected by the mandate, conditional on the disclosed compensation of the top earner of the firm, typically the CEO. We uncover a positive relationship between disclosed top compensation and average wages. Although this relationship is not very precisely estimated, it becomes strong once we focus on the top percentiles of within-firm wage distribution. In our baseline tests, we find that a 1 percent increase in disclosed compensation of the top executive leads to a 59-basis-point increase in the wages for the workers at the 95<sup>th</sup> percentile, and to a 17-basis-point increase for workers at the 99<sup>th</sup> percentile. The causal effect of the policy is supported by an event-study analysis and by a “placebo” test and is robust to the use of different econometric specifications.

We then examine the cross-sectional heterogeneity of the results by running our baseline model on different subsamples of workers. We do not uncover strong evidence of a “gender gap” in the effects of disclosure. We find, however, that results are driven by the subsample of workers with short experience in the labor market, suggesting that they might have had, prior to the disclosure, an informational gap. Moreover, we find that wages of workers located in the main region of the firm's operations also exhibit a stronger response, as they are better able to exploit the new information to their advantage in the bargaining process. CEO pay disclosure does not appear to lead to compositional changes of the workforce. Conversely, we obtain results similar to the baseline when we focus on workers employed prior to the disclosure. Hence, our results are best explained by CEO pay disclosure affecting the bargaining power

of incumbent workers, rather than leading to sorting of more motivated or skilled workers.

Our results suggest that disclosure can have heterogeneous consequences on within-firm wage inequality. We document that, in firms where the CEO enjoys high remuneration, wages of top earners tend to rise in the years following the disclosure. As wages up to the 75<sup>th</sup> percentile experience little to no effect, the level of disclosed CEO compensation positively affects within-firm wage inequality.

The implications with regard to welfare require further investigation and depend on whether changes in compensation of top earners are due to the impact of disclosure on opportunities for rent seeking or, rather, in a realignment of wages and productivity or growth prospects, of which CEO pay could be a proxy. With regard to inequality, however, our results suggest that disclosure may have potentially unintended consequences. Although we are not able to study the effect of disclosure on CEO compensation, we show that disclosing high CEO remunerations tends to benefit only the other top earners of the firm, with no effect on the median earners. Hence, if reducing within-firm wage inequality is itself an objective of policymakers, other policies, including more complete and detailed disclosure requirements, such as those recently adopted in the US and in the United Kingdom (Pan et al., 2022), may be more appropriate.



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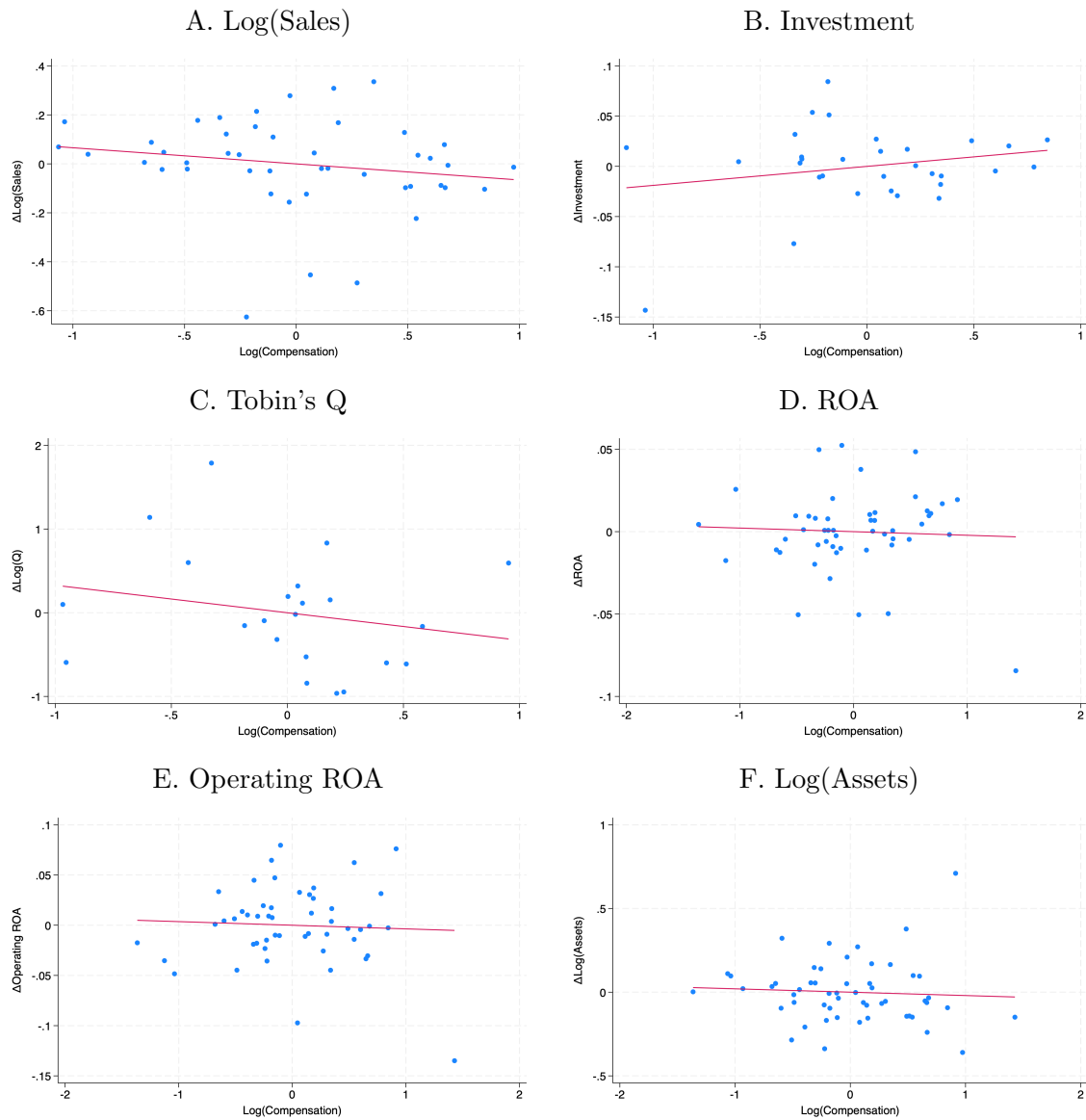
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## 6 Figures and Tables

**Figure 1**

### **CEO Compensation and Trends In Key Firm Variables**

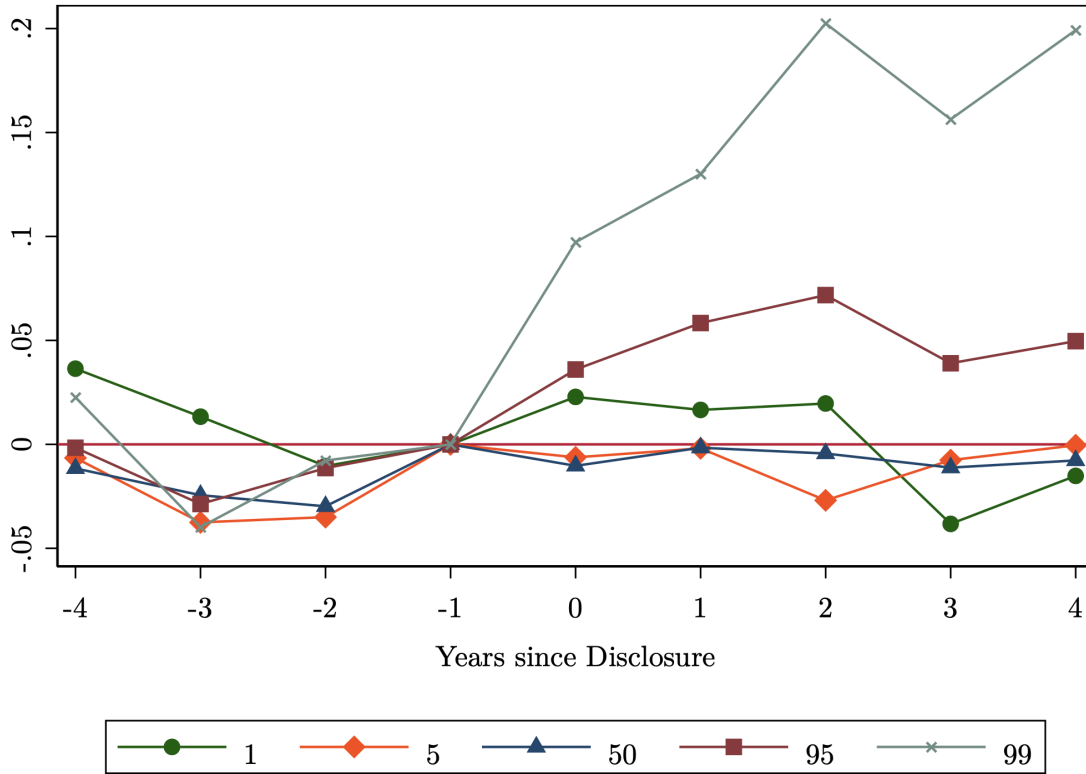
Figure 1 presents scatter plots of the logarithm of CEO compensation (on the  $x$ -axis) versus the 1997-to-1998 change in the variable indicated on the top of each panel (on the  $y$ -axis). Investment is capital expenditures scaled by lagged total assets. Tobin's  $Q$  is total assets plus market value of equity (fiscal-year-end share price times number of shares outstanding) minus book value of equity, all scaled by total assets). ROA is return-on-equity, defined as net income scaled by lagged total asset. Operating ROA is earnings before interests, depreciation, and amortization, scaled by lagged total assets. We regress all the variables on two-digit SIC code industry dummies and then plot the residuals from the regressions. The ordinary least squares regression lines are plotted in red.



**Figure 2**

**CEO Compensation and Wages: Different Percentiles**

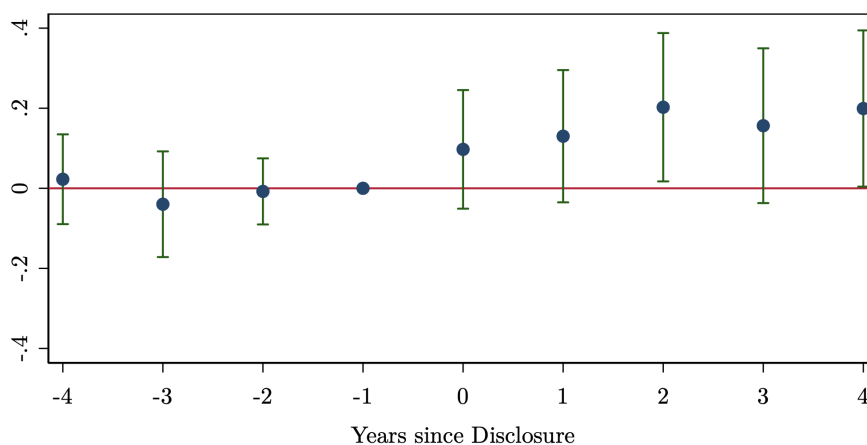
Figure 2 presents regression coefficients where the dependent variables are the logarithms of daily wages at the 1<sup>st</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup>, and 99<sup>th</sup> percentiles. The coefficients are obtained by estimating the event-study equation (2). The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Standard errors are clustered at the firm level.



**Figure 3**

**CEO Compensation and Wages: 99<sup>th</sup> percentile**

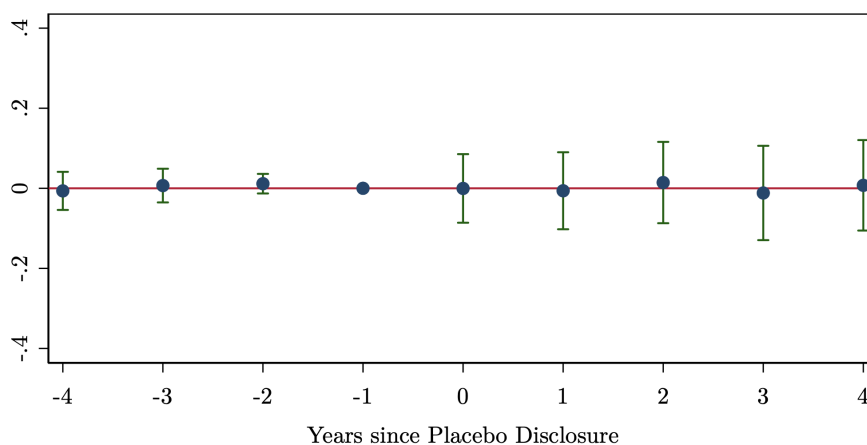
Figure 3 presents regression coefficients where the dependent variables are the logarithms of daily wages at the 99<sup>th</sup> percentiles. The coefficients are obtained by estimating the event-study equation (2). The regressors of interest are year dummies interacted with either the logarithm of the compensation of the highest paid executive in 1998. The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Standard errors are clustered at the firm level.



**Figure 4**

**CEO Compensation and Wages: A Placebo Test**

Figure 4 presents regression coefficients where the dependent variables are the logarithms of daily wages at the 99<sup>th</sup> percentiles. The coefficients are obtained by estimating the event-study equation 2. The regressors of interest are year dummies interacted with the logarithm of the compensation of the highest paid executive in 2011. The sample comprises all the publicly listed firms for which executives' compensation in 2011 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 2007–2015. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Standard errors are clustered at the firm level.



**Table 1**  
**Descriptive Statistics**

Table 1 presents descriptive statistics for the variables used in the paper. We report mean, median, standard deviation, minimum, and maximum of the daily wage. CEO pay is the total compensation of the highest paid firm executive, in thousand euros. Equity value is the market value of the firm's equity, given by the number of shares times the share price, in billion euros. Both CEO pay and equity value are measured at the end of 1998. The sample comprises the 89 publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002.

	Obs.	Mean	Median	St. Dev.	Min	Max
<i>Daily Wage:</i>						
Mean	798	123.25	100.02	83.59	45.17	944.85
Percentile 1	798	44.83	39.91	27.80	3.85	307.62
Percentile 5	798	55.57	50.66	26.64	5.43	307.62
Percentile 25	798	71.89	66.19	30.80	31.25	307.62
Percentile 50	798	93.65	81.22	59.33	37.17	674.62
Percentile 75	798	136.17	104.86	102.41	48.28	789.44
Percentile 95	798	290.26	202.70	299.78	48.89	3,284.86
Percentile 99	798	536.84	350.85	816.58	48.89	15,907.39
CEO Pay (€000)	798	464.63	361.52	417.81	6.20	2,741.35
Equity Value (€bil)	798	1.59	0.20	5.50	0.01	44.62

**Table 2**  
**CEO Pay Disclosure and Wages**

Table 2 presents regressions where the dependent variables are either the logarithm of average daily wages (column 1) or the logarithm of daily wages at the 1<sup>st</sup>, 5<sup>th</sup>, 10<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles in columns 2 through 8, respectively. Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive in 1998. The sample comprises the 89 publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Perc.	Mean (1)	1 (2)	5 (3)	25 (4)	50 (5)	75 (6)	95 (7)	99 (8)
Post $\times$ Pay	0.036* (0.019)	-0.017 (0.024)	0.012 (0.023)	0.014 (0.026)	0.008 (0.020)	0.028 (0.018)	0.059** (0.025)	0.170*** (0.060)
Observations	798	798	798	798	798	798	798	798
R <sup>2</sup>	0.964	0.942	0.960	0.980	0.979	0.964	0.957	0.858
Industry-Year FE	X	X	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X	X	X



**Table 3**  
**Robustness Checks – Controlling for Different Fixed-Effect Combinations**

Panels A and B of Table 3 present regressions where the dependent variables are either the logarithm of average daily wages (column 1) or the logarithm of daily wages at the 1<sup>st</sup>, 5<sup>th</sup>, 10<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles in columns 2 through 8, respectively. Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive in 1998. The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All the regressions include firm fixed effects and, in addition, as well as year fixed effects (in Panel A) and industry-year fixed effects and year-size quintile fixed effects (in Panel B). Observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

<b>A. Controlling for Year Fixed Effects</b>								
Perc.	Mean (1)	1 (2)	5 (3)	25 (4)	50 (5)	75 (6)	95 (7)	99 (8)
Post × Pay	0.026 (0.025)	-0.011 (0.022)	0.001 (0.021)	0.01 (0.019)	0.011 (0.026)	0.034 (0.030)	0.023 (0.037)	0.097** (0.044)
Observations	798	798	798	798	798	798	798	798
R <sup>2</sup>	0.907	0.906	0.958	0.967	0.934	0.884	0.820	0.794
Year FE	X	X	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X	X	X
<b>B. Controlling for Year-Industry and Year-Size Quintile Fixed Effects</b>								
Post × Pay	0.043 (0.026)	-0.021 (0.028)	0.014 (0.030)	0.014 (0.031)	0.009 (0.024)	0.035 (0.023)	0.073** (0.036)	0.217*** (0.081)
Observations	798	798	798	798	798	798	798	798
R <sup>2</sup>	0.965	0.946	0.962	0.981	0.980	0.965	0.959	0.867
Industry-Year FE	X	X	X	X	X	X	X	X
Size Quintile-Year FE	X	X	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X	X	X

**Table 4**  
**Placebo Test**

Table 4 presents regressions where the dependent variables are either the logarithm of average daily wages (column 1) or the logarithm of daily wages at the 1<sup>st</sup>, 5<sup>th</sup>, 10<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles in columns 2 through 8, respectively. Post is a dummy equal to 1 from 2011 onward. Pay is the logarithm of the compensation of the highest paid executive in 2011. The sample comprises all the publicly listed firms for which executives' compensation in 2011 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 2007–2015. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Perc.	Mean (1)	1 (2)	5 (3)	25 (4)	50 (5)	75 (6)	95 (7)	99 (8)
Post $\times$ Pay	0.005 (0.018)	-0.031 (0.022)	-0.003 (0.012)	-0.001 (0.012)	0.007 (0.012)	0.005 (0.016)	0.006 (0.026)	-0.003 (0.046)
Observations	1,225	1,225	1,225	1,225	1,225	1,225	1,225	1,225
R <sup>2</sup>	0.929	0.863	0.953	0.966	0.963	0.951	0.896	0.838
Industry-Year FE	X	X	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X	X	X

**Table 5**  
**CEO Pay Disclosure and Within-Firm Wage Inequality**

Table 5 presents regressions where the dependent variables are, in columns 1 through 4, the differences of the logarithm of average daily wages measured at different percentiles of the within-firm wage distribution, indicated on top of each column. In column 5 the dependent variable is the Gini coefficient. Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive in 1998. The sample comprises the 89 publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Dep. Var.	75 <sup>th</sup> – 25 <sup>th</sup> (1)	90 <sup>th</sup> – 10 <sup>th</sup> (2)	95 <sup>th</sup> – 5 <sup>th</sup> (3)	99 <sup>th</sup> – 1 <sup>st</sup> (4)	Gini Coeff. (5)
Post × Pay	0.014 (0.019)	0.040 (0.040)	0.047 (0.035)	0.187*** (0.067)	0.016* (0.008)
Observations	798	798	798	798	798
R <sup>2</sup>	0.894	0.926	0.897	0.846	0.879
Industry-Year FE	X	X	X	X	X
Firm FE	X	X	X	X	X

**Table 6**  
**Heterogeneity of the Disclosure Effects**

Table 6 presents regressions where the dependent variables are either the logarithm of average daily wages (column 1) or the logarithm of daily wages at the 1<sup>st</sup>, 5<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles (in columns 2 through 5) or the difference between the logarithm of daily wages at the 99<sup>th</sup> and 1<sup>st</sup> percentiles (in column 6). Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive in 1998. The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. Each row considers only a subset of workers, namely females (row A), males (row B), workers with labor market experience, as measured by the number of years since the first job, above the firm median (row C), workers with labor market experience below the firm median (row D), workers employed in the region where the firm has the highest number of workers employed (row E), and workers employed in the other regions (row F). All the regressions include firm and year-industry fixed effects, and observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Perc.	Mean (1)	1 (2)	5 (3)	95 (4)	99 (5)	99 <sup>th</sup> – 1 <sup>st</sup> (6)
<i>A. Females</i>						
Post × Pay	0.037 (0.024)	-0.035 (0.036)	-0.021 (0.028)	0.099*** (0.027)	0.083* (0.042)	0.119* (0.066)
<i>B. Males</i>						
Post × Pay	0.040* (0.021)	0.030 (0.024)	0.021 (0.026)	0.072** (0.029)	0.139*** (0.055)	0.109* (0.064)
<i>C. High Experience</i>						
Post × Pay	0.002 (0.015)	-0.042* (0.024)	-0.030 (0.019)	0.028 (0.027)	0.105* (0.058)	0.148** (0.068)
<i>D. Low Experience</i>						
Post × Pay	0.072** (0.029)	-0.030 (0.030)	0.018 (0.025)	0.122*** (0.038)	0.199*** (0.049)	0.229*** (0.052)
<i>E. Main Region</i>						
Post × Pay	0.064** (0.031)	-0.025 (0.026)	0.010 (0.026)	0.105*** (0.035)	0.273*** (0.083)	0.298*** (0.084)
<i>F. Other Regions</i>						
Post × Pay	0.009 (0.028)	-0.001 (0.037)	0.017 (0.032)	0.041 (0.035)	0.004 (0.074)	0.004 (0.080)

**Table 7**  
**Workforce Composition**

Table 7 presents regressions where the dependent variables are different firm-level averages of workers' characteristics. Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive in 1998. The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. In column 1 the dependent variable is the average age. In columns 2 through 4 the variables are the fraction of male workers, of employees with full-time contracts, and of new employees, respectively. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Dep. Var.	Average Age (1)	<i>Fraction of...</i>		
		Males (2)	Full Time (3)	New Employees (4)
Post $\times$ Pay	0.374 (0.321)	-0.013** (0.006)	-0.007* (0.003)	0.095 (0.076)
Observations	798	798	798	798
R <sup>2</sup>	0.963	0.931	0.900	0.866
Industry-Year FE	X	X	X	X
Firm FE	X	X	X	X

**Table 8**  
**CEO Pay Disclosure and Wages – Only Stayers**

Table 8 presents regressions where the dependent variables are either the logarithm of average daily wages (column 1) or the logarithm of daily wages at the 1<sup>st</sup>, 5<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles (in columns 2 through 5) or the difference between the logarithm of daily wages at the 99<sup>th</sup> and 1<sup>st</sup> percentiles (in column 6). Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive. The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All the regressions include firm and industry-year fixed effects, and observations are weighted by firm size, measured by the number of workers. Averages and percentiles are computed only over workers employed in the firm in 1997. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Perc.	Mean (1)	1 (2)	5 (3)	95 (4)	99 (5)	99 <sup>th</sup> – 1 <sup>st</sup> (6)
Post × Pay	0.003 (0.020)	-0.015 (0.023)	-0.006 (0.022)	0.018 (0.021)	0.130*** (0.045)	0.145*** (0.054)
Observations	791	791	791	791	791	791
R <sup>2</sup>	0.982	0.942	0.966	0.978	0.896	0.876
Industry-Year FE	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X

# A Appendix

## A.1 Valuation of Stock Options

While executives' stock options were common in the US in the 1990s, only two companies in our sample granted stock options in 1998. As the Italian law did not require boards to provide a valuation, we estimate their value using the Black and Scholes formula. For the formula's inputs, we follow closely the approach adopted by Execucomp, the major provider of data used in academic research for US executives' compensation.

We assume that stock options are issued on June 30th of the fiscal year, and use as dividend-price ratio the average of the previous three years. For the volatility, we use the annualized standard deviation of the monthly stock returns of the previous 60 months. Finally, we assume that the options have a 7-year duration, and as risk-free rate we use the yields on the Italian 7-year government bonds issued at the end of 1998. The same approach is employed by Faia et al. (2021), whose data we use for the "placebo test" discussed in Section 3.2.

## A.2 Additional Results

As discussed in Section 3.2, Table A1 replicates the main analysis displayed in Table A1 but excludes from the sample of workers all the employees that are classified as managers in the year 1997. Average wages and percentiles are then computed over the remaining sample of workers.

Table A2 runs the baseline tests by including different subgroups of workers, along the lines of the heterogeneity tests presented in Section 4 (Table 6). Specifically, the table shows the effects of wage disclosure on workers with tenure and age below or

above the sample median.



**Table A1**  
**CEO Pay Disclosure and Wages – Excluding Managers**

Table [A1](#) presents regressions where the dependent variables are either the logarithm of average daily wages (column 1) or the logarithm of daily wages at the 1<sup>st</sup>, 5<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles in columns 2 through 6, respectively. Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive. The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. All workers classified as managers (*dirigenti*) in 1997, the year prior to the the enactment of the disclosure requirement, are excluded from the sample of workers. All the regressions include firm and year-industry fixed effects, and observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Perc.	Mean	1	5	95	99	99 <sup>th</sup> – 1 <sup>st</sup>
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Pay	0.036 (0.022)	-0.019 (0.024)	0.012 (0.024)	0.050* (0.026)	0.182*** (0.058)	0.201*** (0.064)
Observations	791	791	791	791	791	791
R <sup>2</sup>	0.975	0.944	0.959	0.963	0.931	0.854
Industry-Year FE	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X

**Table A2**  
**Heterogeneity of the Disclosure Effects**

Table A2 presents regressions where the dependent variables are either the logarithm of average daily wages (column 1) or the logarithm of daily wages at the 1<sup>st</sup>, 5<sup>th</sup>, 95<sup>th</sup>, and 99<sup>th</sup> percentiles in columns 2 through 5 or the difference between the logarithm of daily wages at the 99<sup>th</sup> and 1<sup>st</sup> percentiles. Post is a dummy equal to 1 from 1998 onward. Pay is the logarithm of the compensation of the highest paid executive in 1998. The sample comprises all the publicly listed firms for which executives' compensation in 1998 can be obtained from firm financial reports and that could be matched with the Italian social security (INPS) database for the years 1994–2002. Each row considers only a subset of workers, namely workers with tenure, as measured by the number of years since the first job, above the firm median (row A), workers with labor market experience below the sample median (row B), workers with age above the sample median (row C), and workers employed in the other regions workers with age below the sample median (row D). All the regressions include firm and year-industry fixed effects, and observations are weighted by firm size, measured by the number of workers. Industries are identified using the two-digit SIC code classification. Standard errors, in parentheses, are clustered at the firm level. \*, \*\*, and \*\*\* indicate statistically different from zero at the 10%, 5%, and 1% levels of significance, respectively.

Perc.	Mean	1	5	95	99	99 <sup>th</sup> – 1 <sup>st</sup>
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A. High Tenure</i>						
Post × Pay	0.035 (0.023)	0.043 (0.027)	0.036* (0.019)	0.022 (0.037)	0.155 (0.139)	0.112 (0.148)
<i>B. Low Tenure</i>						
Post × Pay	0.045** (0.022)	-0.030 (0.028)	0.005 (0.027)	0.068** (0.031)	0.181*** (0.053)	0.211*** (0.071)
<i>C. High Age</i>						
Post × Pay	0.025 (0.019)	-0.025 (0.024)	0.001 (0.023)	0.057* (0.030)	0.101* (0.060)	0.126** (0.063)
<i>D. Low Age</i>						
Post × Pay	0.044* (0.022)	-0.023 (0.027)	0.006 (0.022)	0.070*** (0.024)	0.171*** (0.045)	0.193*** (0.052)