Istituto Nazionale Previdenza Sociale

aprile 2024 – numero 77

WorkINPS *Papers*

CEO Pay Disclosure and Within-Firm Wage Inequality*

Agata Maida

Vincenzo Pezone

ISSN 2532 -8565

In copertina: uno storico "Punto cliente" a Tuscania
INPS, Direzione generale, Archivio storico

I WORKINPS PAPER

Le basi dati amministrative dell'*INPS* rappresentano una fonte statistica unica per studiare scientificamente temi cruciali per l'economia italiana, la società e la politica economica: non solo il mercato del lavoro e i sistemi di protezione sociale, ma anche i nodi strutturali che impediscono all'Italia di crescere in modo adeguato. All'interno dell'Istituto, questi temi vengono studiati sia dai funzionari impiegati in attività di ricerca, sia dai *VisitInps Scholars*, ricercatori italiani e stranieri selezionati in base al loro curriculum vitae e al progetto di ricerca presentato.

I **WORKINPS** hanno lo scopo di diffondere i risultati delle ricerche svolte all'interno dell'Istituto a un più ampio numero possibile di ricercatori, studenti e policy markers.

Questi saggi di ricerca rappresentano un prodotto di avanzamento intermedio rispetto alla pubblicazione scientifica finale, un processo che nelle scienze sociali può chiedere anche diversi anni. Il processo di pubblicazione scientifica finale sarà gestito dai singoli autori.

Tommaso Nannicini

CEO Pay Disclosure and Within-Firm Wage Inequality*

Agata Maida

(University of Milan)

Vincenzo Pezone

(Tilburg University, 5000 LE Tilburg, The Netherlands)

CEO Pay Disclosure and Within-Firm Wage Inequality*

La trasparenza sui salari dei CEO e la disuguaglianza salariale nelle imprese

Agata Maida† Vincenzo Pezone‡

Thursday 18th January, 2024

Abstract

We analyze the effect of CEO pay disclosure on wage distribution by exploiting a 1998 reform requiring Italian publicly listed companies to disclose the compensation of top executives. 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. 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.

In questo articolo, analizziamo l'effetto della trasparenza sulla remunerazione dei manager sulla distribuzione salariale, sfruttando una riforma introdotta nel 1998 che ha imposto alle società quotate italiane di pubblicare la retribuzione dei vertici aziendali. Nelle aziende in cui i compensi degli amministratori delegati risultano elevati, si osserva un aumento statisticamente significativo dei salari dei lavoratori nei percentili più alti della distribuzione salariale interna all'azienda. Per quanto riguarda gli stipendi medi, tale effetto è limitato e

*We benefited from discussions with Edoardo Accabi, Carlos Alos-Ferrer, David Card, Carlo Fiorio, Michel Serafinelli, Giuseppe Sorrenti, and Andrea Weber. We thank seminar and conference participants at the University of Milan Department of Economics, LABORatorio Revelli-Collegio Carlo Alberto (Turin), SOLE (Philadelphia), ESPE (Belgrade), and EALE (Prague). We gratefully acknowledge funding and data access by the VisitINPS Scholars program. The findings and conclusions expressed are solely those of the author and do not represent the views of INPS.

†University of Milan, Via del Conservatorio 7, Milan, Italy. E-mail: agata.maida@unimi.it

‡Tilburg University, 5000 LE Tilburg, The Netherlands. Tel. +31 13 466 2215. Email: v.pezone@tilburguniversity.edu.

solo marginalmente significativo. Questi impatti sono più evidenti per i lavoratori con poca esperienza e occupati nella regione in cui si concentra l'attività dell'impresa. Inoltre, sono determinati da cambiamenti nel potere contrattuale dei lavoratori, piuttosto che da un effetto di selezione.

JEL Codes: J31, D63, D9, M12
Keywords: CEO Compensation, Wage Disclosure, Income Inequality, Wage Bargaining, Remunerazione dei Manager, Trasparenza sui Salari,
Disuguaglianza Salariale, Contrattazione Salariale

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 opionion.

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.[1]

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 not been studied in depth. 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). 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, reducing within-firm inequality as a result.[2]

[1] See OECD (2021), "Corporate Governance Factbook," `https://www.oecd.org/corporate/corporategovernance-factbook.htm`.

[2] Even in a setting where workers have no bargaining power and no possibility of "sabotaging"

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).[3] Thus, we have a unique opportunity to examine the effects of CEO pay transparency on within-firm inequality. Specifically, we ca 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 information 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 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. Our baseline estimates of the "pass through" suggest that a 1 percent increase in

the firm's operations, disclosure could still result in higher wages. Intuitively, workers' utility must still be equal to its 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.

[3] The data have been accessed through the VisitINPS Scholars program.

disclosed compensation of the top executive leads to a 6-basis-point increase in wages for the workers at the 95^{th} percentile and to a 17-basis-point increase for workers at the 99^{th} percentile. Focusing on the 1^{st}, 5^{th}, 25^{th}, 50^{th}, 75^{th}, 95^{th}, and 99^{th} 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 diclosed CEO compensation.

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 some 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.

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, and show that this "pseudo"–event has no effect on realized wages.

We then move to identify the economic channels driving our results. To identify the economic channels, 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 os 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 of changes in the workforce composition: CEO pay disclosure does not appear to affect workers' gender, contract status, age, or turnover. 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 studying 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,[4] this paper studies how an informational shocks, namely disclosure of CEO compensation, affects *within-firm* wage inequality.

[4]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.

This paper is organized as follows. Section 2 summarizes the related literature. Section 3 describes institutional setting, data, and the econometric strategy. Section 4 presents the main results and some robustness checks. Section 5 presents additional analyses to highlight the economic channels, and Section 6 concludes.

2 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 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. 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.

3 Institutional Setting, Data, and Econometric Strategy

3.1 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*, first 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."[5] *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.[6]

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.

[5] 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).

[6] See "Il segreto più caro del manager" (*La Repubblica*, November 23, 1998).

3.2 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, who are subject to the disclosure mandate. We obtain 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 board members' 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 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 with firm-level information from Compustat Global.[7] The INPS data cover the entire population of workers 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 year surrounding the reform (i.e., from 1994 to 2002). The dataset has information on workers' careers for the universe of the private sector in Italy. 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. More specifically, we have retrieved information on market value of equity and industry, defined at the two-digit SIC level.

[7] 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.

Our final sample includes 89 firms.

3.3 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:

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

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$^{\text{st}}$, 5$^{\text{th}}$, 25$^{\text{th}}$, 50$^{\text{th}}$, 75$^{\text{th}}$, 95$^{\text{th}}$, and 99$^{\text{th}}$ 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 γ_i are year-industry and firm fixed effects, respectively. Our key coefficient of interest, β_1, has an intuitive interpretation. It corresponds to the elasticity of the wage corresponding to the p^{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,[8] 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.

[8] See also Angrist and Pischke (2009) for a discussion of the biases generated by the inclusion in a regression model of "bad controls."

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$^{\text{th}}$ and 97.5$^{\text{th}}$ percentiles to reduce the influence of outliers. The typical CEO earns about €900 thousand per year, but there is again enormous variation. As expected, the distribution is highly skewed. The CEO compensation ranges between €12 thousand and €5.3 million, and the median is €690 thousand. Firms differ also with regard to the total equity value, which ranges between €14 million and €86 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.

4 Results

4.1 Baseline Results

In Table 2 we present our baseline results, by reporting estimates of the coefficient β_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 very large.

In columns 2 through 8 we test whether CEO pay disclosure has distributional consequences. Specifically, the dependent variables are now different percentiles of the wage distribution. As discussed in Section 3.3, 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$^{\text{th}}$ percentile and 0.170 for the 99$^{\text{th}}$ 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 1, β_0 corresponds to the year of the disclosure policy, 1998, and we normalize the coefficient β_{-1} to zero.[9] 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$^{\text{st}}$, 25$^{\text{th}}$, and 50$^{\text{th}}$ percentiles exhibit little reaction. Conversely, wages at the 95$^{\text{th}}$ and, especially, 99$^{\text{th}}$ percentiles exhibit large elasticities to CEO disclosed compensation. For completeness, Figure 2 restricts attention to the 99$^{\text{th}}$ percentile of the within-firm wage distribution, where we detect the strongest effects, and displays not only the coefficients but also the confidence intervals.

4.2 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 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$^{\text{th}}$ percentile, with a coefficient equal to 0.097 (standard error=0.044).

[9] To avoid clutter, the figure omits the results for the 25$^{\text{th}}$ and the 75$^{\text{th}}$ percentiles, which are available upon request.

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$^{\text{th}}$ and 99$^{\text{th}}$ percentiles are slightly larger, equal to 0.074 and 0.219, respectively, and significant at the 5% and 1% level, respectively.

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.[10]

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$^{\text{th}}$ percentile of the firm distribution, is, if anything, *negative*. Figure 3 displays coefficients obtained after estimating equation 2 with the 99$^{\text{th}}$ 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

[10]The data were shared with us by Faia et al. (2021).

the year of enactment of the policy, there is an actual revelation of new information, leading to changes in the wage distribution.

4.3 Within-Firm Wage Inequality

As an alternative way to present our results, inspired by the evidence in Section 4.1, 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^{th}–25^{th}, the 90^{th}–10^{th}, the 95^{th}–5^{th}, and the 99^{th}–1^{st} 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^{th}–25^{th}, the 90^{th}–10^{th}, the 95^{th}–5^{th} differences. Conversely, in column 4 we detect a more precise effect of disclosure on the log-difference 99^{th}–1^{st}, 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%.

5 Heterogeneous Effects of Disclosure

5.1 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^{st}, 5^{th}, 95^{th}, and 99^{th} percentiles, as well as the log difference between the wages measured at the 99^{th} and 1^{st} 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 4.1, 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^{th} percentiles (0.098 versus 0.072) and slightly higher for male workers than for female workers at the 99^{th} 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 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 form 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. 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^{th} and 99^{th} percentiles. The effect is more muted for high-experience workers, although we do detect a marginally significant effect for workers at the 99$^{\text{th}}$ 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 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$^{\text{th}}$ and 99$^{\text{th}}$ 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.

5.2 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 4. 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 measures aimed at capturing 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 positive, but they are imprecisely estimated. Even though in the case of the fraction of males we do detect a marginally significant coefficient, the coefficient is quite small (0.024, significant at the 10% level).

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^{th} 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^{th} and 1^{st} 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.

6 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^{th} percentile, and to a 17-basis-point increase for workers at the 99^{th} 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. We do not uncover strong evidence that CEO pay disclosure leads 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^{th} percentile experience little to no effect, the level of disclosed CEO compensation positively affects within-firm wage inequality.

The policy implications with regard to efficiency 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. 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.

References

Acciari, P., Polo, A., and Violante, G. L. (2022). And yet it moves: Intergenerational mobility in italy. *American Economic Journal: Applied Economics*, 14(3):118–63.

Angrist, J. D. and Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Baker, M., Halberstam, Y., Kroft, K., Mas, A., and Messacar, D. (2023). Pay Transparency and the Gender Gap. *American Economic Journal: Applied Economics*, 15(2):157–83.

Bertrand, M. and Mullainathan, S. (1999). Is there Discretion in Wage Setting? A Test Using Takeover Legislation. *The RAND Journal of Economics*, 30(3):535–554.

Card, D., Cardoso, A. R., and Kline, P. (2016). Bargaining, Sorting, and the Gender Wage Gap: Quantifying the Impact of Firms on the Relative Pay of Women. *The Quarterly Journal of Economics*, 131(2):633–686.

Card, D., Mas, A., Moretti, E., and Saez, E. (2012). Inequality at Work: The Effect of Peer Salaries on Job Satisfaction. *American Economic Review*, 102(6):2981–3003.

Cronqvist, H., Heyman, F., Nilsson, M., Svaleryd, H., and Vlachos, J. (2009). Do Entrenched Managers Pay Their Workers More? *Journal of Finance*, 64(1):309–339.

Cullen, Z. and Perez-Truglia, R. (2022). How Much Does Your Boss Make? The Effects of Salary Comparisons. *Journal of Political Economy*, 130(3):766–822.

Dittmann, I., Schneider, C., and Zhu, Y. (2018). The Additional Costs of CEO Compensation: The Effect of Relative Wealth Concerns of Employees . *European Corporate Governance Institute-Finance Working Paper*, (559).

Dittrich, M., Knabe, A., and Leipold, K. (2014). Gender Differences in Experimental Wage Negotiations . *Economic Inquiry*, 52(2):862–873.

Dustmann, C., Ludsteck, J., and Schönberg, U. (2009). Revisiting the German Wage Structure. *The Quarterly Journal of Economics*, 124(2):843–881.

Faia, E., Mayer, M., and Pezone, V. (2021). The Value of Firm Networks: A Natural Experiment on Board Connections. *SAFE Working Paper*, (269).

Gabaix, X. and Landier, A. (2008). Why has CEO Pay Increased So Much? *The Quarterly Journal of Economics*, 123(1):49–100.

Gabaix, X., Landier, A., and Sauvagnat, J. (2014). CEO Pay and Firm Size: An Update After the Crisis. *Economic Journal*, 124(574):F40–F59.

Gormley, T. A. and Matsa, D. A. (2016). Playing it safe? Managerial preferences, risk, and agency conflicts. *Journal of Financial Economics*, 122(3):431–455.

Landier, A., Nair, V., and Wulf, J. (2009). Trade-offs in Staying Close: Corporate Decision Making and Geographic Dispersion. *Review of Financial Studies*, 22(3):1119–1148.

Luttmer, E. F. P. (2005). Neighbors as Negatives: Relative Earnings and Well-Being. *The Quarterly Journal of Economics*, 120(3):963–1002.

Mas, A. (2017). Does Transparency Lead to Pay Compression? *Journal of Political Economy*, 125(5):1683–1721.

Mas, A. (2019). Does Disclosure Affect CEO Pay Setting? Evidence from the Passage of the 1934 Securities and Exchange Act. Working Papers 632, Princeton University, Department of Economics, Industrial Relations Section.

Pan, Y., Pikulina, E. S., Siegel, S., and Wang, T. Y. (2022). Do Equity Markets Care about Income Inequality? Evidence from Pay Ratio Disclosure. *Journal of Finance*, 77(2):1371–1411.

Perez-Truglia, R. (2020). The Effects of Income Transparency on Well-Being: Evidence from a Natural Experiment. *American Economic Review*, 110(4):1019–54.

Piketty, T. and Saez, E. (2003). Income Inequality in the United States, 1913–1998. *The Quarterly Journal of Economics*, 118(1):1–41.

Rabin, M. (1993). Incorporating Fairness into Game Theory and Economics. *The American Economic Review*, 83(5):1281–1302.

Volpin, P. (2002). Governance with Poor Investor Protection: Evidence from Top Executive Turnover in Italy. *Journal of Financial Economics*, 64(1):61–90.

7 Figures and Tables

Figure 1
CEO Compensation and Wages: Different Percentiles

Figure 1 presents regression coefficients where the dependent variables are the logarithms of daily wages at the 1st, 25th, 50th, 75th, and 99th 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 year fixed effects, and observations are weighted by firm size, measured by the number of workers. Standard errors are clustered at the firm level.

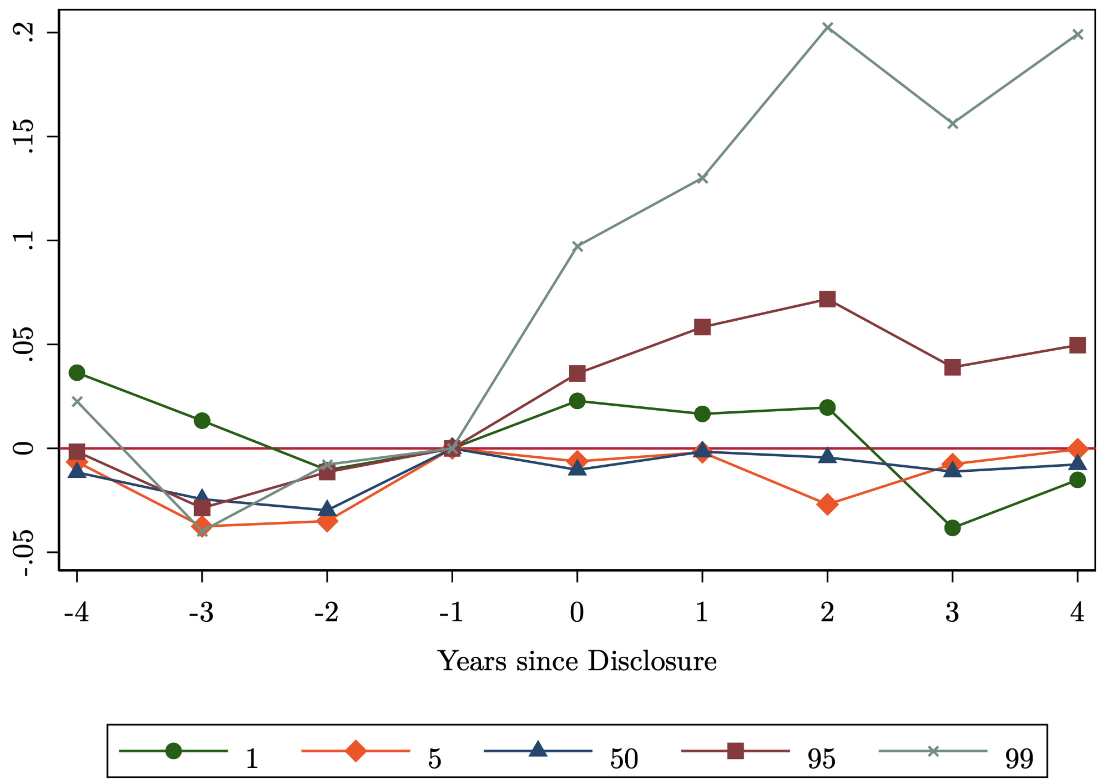

Figure 2
CEO Compensation and Wages: 99th percentile

Figure 2 presents regression coefficients where the dependent variables are the logarithms of daily wages at the 99th 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 year fixed effects, and observations are weighted by firm size, measured by the number of workers. Standard errors are clustered at the firm level.

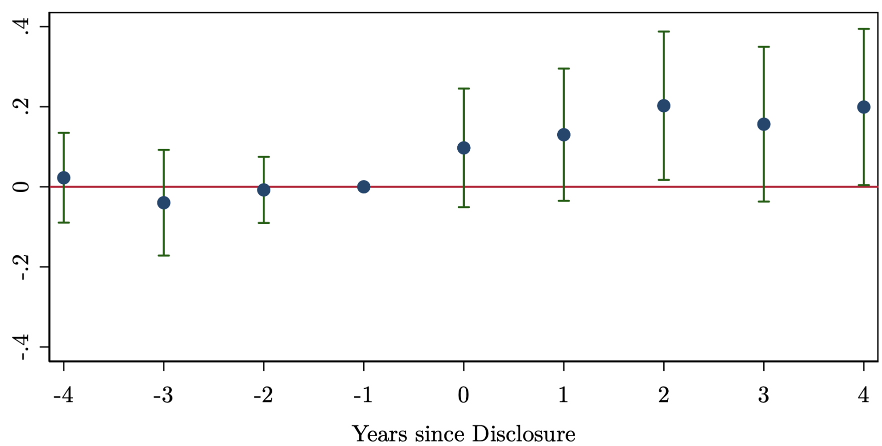

Figure 3
CEO Compensation and Wages: A Placebo Test

Figure 3 presents regression coefficients where the dependent variables are the logarithms of daily wages at the 99$^{\text{th}}$ 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 1998 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 year fixed effects, and observations are weighted by firm size, measured by the number of workers. Standard errors are clustered at the firm level.

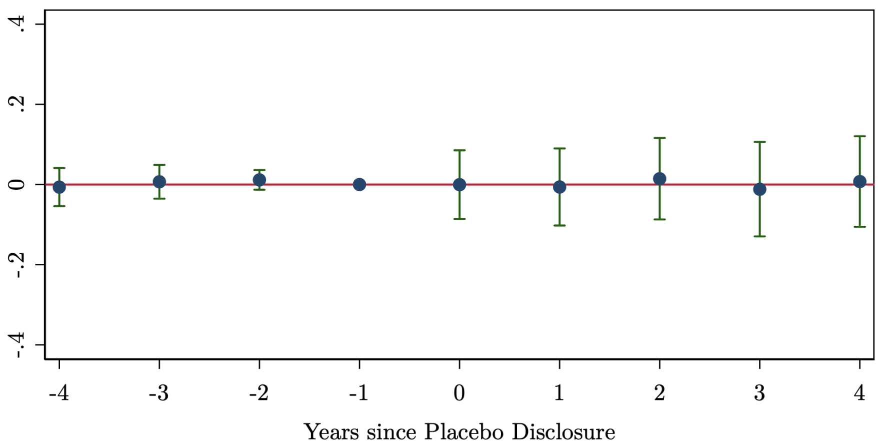

Table 1
Descriptive Statistics

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. Equity value is the market value of the firm's equity, given by the number of shares times the share price. 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
Daily Wage:						
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	897.03	689.90	809.34	12.00	5,308.00
Equity Value (€mil)	798	3,079.63	389.87	10,658.25	13.75	86,401.74

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 1st, 5th, 10th, 25th, 50th, 75th, 95th, and 99th 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 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.

Perc.	Mean	1	5	25	50	75	95	99
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post $\times$ Pay	0.036	-0.017	0.012	0.014	0.008	0.028	0.059	0.170
	(0.019)	(0.024)	(0.023)	(0.026)	(0.020)	(0.018)	(0.026)	(0.060)
Observations	798	798	798	798	798	798	798	798
R^2	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 1st, 5th, 10th, 25th, 50th, 75th, 95th, and 99th 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, year fixed effects and industry-year fixed effects in Panels A and B, respectively. 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.

B. Controlling for Year-Industry Fixed Effects

Perc.	Mean (1)	1 (2)	5 (3)	25 (4)	50 (5)	75 (6)	95 (7)	99 (8)
Post × Pay	0.026	-0.011	0.001	0.01	0.011	0.034	0.023	0.097
	(0.025)	(0.022)	(0.021)	(0.019)	(0.026)	(0.030)	(0.037)	(0.044)
Observations	798	798	798	798	798	798	798	798
R^2	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. Controlling for Year-Industry and Year-Size Quintile Fixed Effects

Post × Pay	0.043	-0.020	0.014	0.014	0.009	0.035	0.074	0.219
	(0.026)	(0.029)	(0.030)	(0.031)	(0.025)	(0.023)	(0.036)	(0.081)
Observations	798	798	798	798	798	798	798	798
R^2	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 1st, 5th, 10th, 25th, 50th, 75th, 95th, and 99th 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 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 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.

Perc.	Mean (1)	1 (2)	5 (3)	25 (4)	50 (5)	75 (6)	95 (7)	99 (8)
Post × Pay	0.005	-0.031	-0.003	-0.001	0.007	0.005	0.006	-0.003
	(0.018)	(0.022)	(0.012)	(0.012)	(0.012)	(0.016)	(0.026)	(0.046)
Observations	1,225	1,225	1,225	1,225	1,225	1,225	1,225	1,225
R^2	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. 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 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.

Dep. Var.	$75^{th} - 25^{th}$ (1)	$90^{th} - 10^{th}$ (2)	$95^{th} - 5^{th}$ (3)	$99^{th} - 1^{st}$ (4)	Gini Coeff. (5)
Post $\times$ Pay	0.014	0.040	0.047	0.187	0.016
	(0.019)	(0.040)	(0.035)	(0.067)	(0.008)
Observations	798	798	798	798	798
R^2	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^{st}, 5^{th}, 95^{th}, and 99^{th} percentiles in columns 2 through 5 or the difference between the logarithm of daily wages at the 99^{th} and 1^{st} 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 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 sample 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.

Perc.	Mean (1)	1 (2)	5 (3)	95 (4)	99 (5)	$99^{th} - 1^{st}$ (6)
A. Females						
Post × Pay	0.037	-0.035	-0.021	0.098	0.083	0.117
	(0.024)	(0.036)	(0.028)	(0.027)	(0.042)	(0.066)
B. Males						
Post × Pay	0.040*	0.029	0.021	0.072	0.139	0.109
	(0.021)	(0.024)	(0.026)	(0.029)	(0.055)	(0.064)
C. High Experience						
Post × Pay	0.002	-0.042	-0.029	0.027	0.104	0.146
	(0.015)	(0.024)	(0.019)	(0.027)	(0.058)	(0.068)
D. Low Experience						
Post × Pay	0.072	-0.030	0.017	0.122	0.200	0.230
	(0.029)	(0.030)	(0.025)	(0.038)	(0.049)	(0.052)
E. Main Region						
Post × Pay	0.063	-0.025	0.010	0.104	0.271	0.296
	(0.031)	(0.026)	(0.026)	(0.035)	(0.083)	(0.084)
F. Other Regions						
Post × Pay	0.009	0.000	0.017	0.041	0.005	0.005
	(0.028)	(0.037)	(0.032)	(0.035)	(0.074)	(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 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. 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.

		Fraction of...		
Dep. Var.	Average Age	Males	Full Time	New Employees
	(1)	(2)	(3)	(4)
Post × Pay	0.479	0.024	0.007	0.095
	(0.419)	(0.013)	(0.007)	(0.076)
Observations	798	798	798	798
R^2	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^{st}, 5^{th}, 10^{th}, 25^{th}, 50^{th}, 75^{th}, 95^{th}, and 99^{th} 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. 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 year-industry 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.

Perc.	Mean (1)	1 (2)	5 (3)	95 (4)	99 (5)	$99^{th} - 1^{st}$ (6)
Post × Pay	0.002	-0.014	-0.006	0.017	0.129	0.144
	(0.020)	(0.023)	(0.022)	(0.021)	(0.045)	(0.054)
Observations	791	791	791	791	791	791
R^2	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