Managerial Horizon and Corporate Labor Policies: Evidence from Fixed-Term Boards*

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Abstract

We examine the relationship between managerial incentives and firms' human resources policies by combining Italian social security records with newly collected data on board terms. We exploit the fact that boards of listed companies are appointed every three years as a source of cyclical variation in the horizon of bord members. Our evidence shows that wages drop sharply as managers approach the end of their tenure, consistent with executives trying to cut labor expenses and, hence, maximize cash flow when they are up for re-appointment. Consistent with a simple theoretical framework, these effects are not present in firms where managers are entrenched, such as family-managed firms, and when executives have a long tenure. By leveraging from individual high frequency administrative data, we show that workers employed by listed companies are disproportionately more likely to be separated exactly 12 months before the election and the month following the appointment of a new board. We relate these findings with existing agency models and empirical evidence on managerial preferences.

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1 Introduction

Many economic relationships are characterised by employers evaluating an employee at fixed points in time, and deciding whether to fire her or not. Voters decide whether to reappoint a politician at the end of a term of fixed length. An assistant professor is granted tenure or not at the end of an evaluation period, the tenure track. The skills of a hedge fund manager can be evaluated only when she is scheduled to release information regarding the performance of her fund, typically every three months.

These principal-agent relationships are dynamic, meaning that the employee decides whether and how to perform the task over time. In this paper, we examine such decisions by focusing on the employment and wage polices set by executives, and how they vary over the course of a board tenure. We use Italian listed companies as a laboratory. Italian corporate law requires each board member to be appointed for at most three years. Although they could be appointed for shorter tenures and in a staggered fashion, in practice, the vast majority of companies elect all board members together, and for the maximum length.

Every three years a shareholders' meeting will therefore appoint a new board, and each member of the previous board can be replaced or confirmed. As shown in our analysis, turnover of board members, including top executives such as the CEO, tends to cluster in the last year of the board term. The resulting cyclical pattern of turnover of board members, including top executives, allows us to analyze how corporate policies vary over the course of the horizon of the board members. In other words, as they approach the end of their term, executives have strong incentives to improve their performance to maximize their chances of being reappointed.

We show in a simple model how this institutional arrangement can lead to a predictable cyclical pattern in the wage policies. We assume that cutting labor expenses can help maximizing cash flow; however, for a manager, adopting a confrontational approach towards workers is unpleasant and she would like to avoid that, unless worthwhile. We show that

a manager has incentives to concentrate her effort in cutting wages as she approaches the end of her tenure. Intuitively, when there is high uncertainty between the quality of the match between her and the firm, the manager uses the option of waiting to know whether she will be more or less likely to be reappointed, before facing workers and forcing them to accept higher wages. We also show that such pattern should not arise only if managers have long tenure, and if they are entrenched (such as when they belong to the controlling family).

We collect information regarding board appointment dates for Italian listed firms between 2001 and 2015. We merge this information on a large administrative database of Italian social security records so that we can track the individual history of millions of Italian workers both when they were employed by a public firm as well as when their employer was not listed in the Italian stock exchange market.

We establish several novel facts. First, we provide evidence that monthly earnings fall by roughly 46 basis points in the last year of a board term. This effect is highly statistically significant and is concentrated in firms whose managers are less likely to be entrenched. Indeed, the earnings drop by 86 basis points in firm in which none of the top executives (CEO, president and vice-president) belong to the controlling family, but are unaffected in famaily-managed firms. We also split firms according to the average tenure of the top three executives, and find evidence consistent with our predictions. Our coefficient of interest is very close to zero in firms managed by long-tenured managers, but we find that there is a drop of 91 basis points in earnings for firms run by less experienced managers in the third year of the board term. We find that the effects are larger for female and short term workers, that are likely to be in a weaker bargaining position vis-a-vis the managers. Indeed, we also find that also the composition of the workforce tends to change as the last shareholders' meeting of the board term approaches, with fewer long term and male workers being employed.

Next, we leverage from longitudinal high frequency data from social security records

to fully characterize the consequences of a board election from a worker perspective. Our treatment group comprises individuals hired by a listed firm 36 to 12 months before the election of new board. We match each individual in the treatment group a control worker hired by a non-listed firm in the same period, in the same local labor market, with a similar starting wage, demographic characteristics and employment arrangement (e.g. temporary vs. permanent contract) as the treated individual.

We find that separation rates from the corresponding original employer between treated and control workers are indistinguishable until we enter the last year of the board term. There, we see the probability of separation for workers in listed firms to jump discontinuously by approximately 3 percentage points relative to the control group. This gap jumps again by around 3 percentage points in the months right before and after the election of a board. The total cumulative increase in separation rates from the original employers is roughly 9 percentage points higher for the treatment group one year following the election, a 40% increase when benchmarked against the average separation rate observed two years prior to the election. We find that around 30% of these separated workers in listed firms are unable to find a new employer 12 months following an election. This suggests that the incoming corporate board election might induce "scarring" effects.

Interestingly, we uncover strong cyclical effects in the earnings profile of newly hired individuals by listed firms that are highly heterogeneous across workers. Conditional on "surviving" the election of a board, an individual in a listed firm earns about 7% more compared to a matched control worker that also remained with her initial employer. The majority of this premium is obtained by these "stayers" following the election of the corporate board. Conversely, individuals that ended up being separated by a listed firm, earn about 14% less compared to a matched worker who is also no longer with her initial employer 12 months following the election of the board. These earnings losses match precisely the dynamic effects found when evaluating separation rates: they are indistinguishable from zero up to 12 months before the election, then jump discontinuously

to around 2% as soon as we enter the last year of tenure of the board, with a further jump of around 7% in the month right after the election of the board.

Taken altogether, these findings can be mapped to the existing empirical evidence on the relationship between managerial horizon and corporate investment. Edmans, Fang, and Lewellen (2017) show that, as the vesting of options approaches, managers cut investment and research and development expenditure; this is in line with the drop in days worked and investment in human capital (as represented by lower wages) observed towards the end of the board tenure. On the other hand, the fall in employment and wages corresponding to the appointment of the new board accords well with evidence on the increase in divestures that follows the appointment of a new CEO (Weisbach, Wang, and Pan (2016)). Finally, the increase in wages in retained workers can be reconciled with the new board reshaping the corporate strategy, and investing in the retention of employees perceived as particularly valuable.

Overall, our paper improves our understanding of the relationship between managerial incentives and investment in human capital. Changes in the board composition can have real costs for employees; such costs are also heterogeneous, and depend on their age and contractual arrangement.

This paper is structured as follows. Section 3 presents a simple model and highlights its testable predictions. Section 2 relates this work to the relevant literature. Section 4 presents the institutional setting and the data. Section 5 describe the econometric strategy. The empirical results are presented in Section 6, and Section 7 concludes.

2 Related Literature

Theoretical work has shown how managerial short-termism might induce suboptimal decisions. Stein (1989) shows theoretically that managers might prefer to maximize short-term earnings over pursuing projects that could maximize firm value in the long run in the con-

text of a simple signal-jamming model (Holmström (1999)). Importantly, this inefficiency arises whenever managers put some weight on firm value in the short run (for example, because of takeovers pressure or stock-based compensation) even when investors are rational and fully incorporate the effects of managerial moral hazard.

Empirical work is, however, more recent, as detailed data on executives' compensation and other contractual arrangements, as well as board composition have become available. Edmans et al. (2017) show that corporate investment and R&D expenditure are negatively related to the the amount of stock and options scheduled to vest in a given quarter. This evidence can be interpreted as causal, as stock options are typically issued several years in advance. In a setting more similar to ours, Fos, Li, and Tsoutsoura (2017) show that directors' effort appears to increase as their re-election date approaches, as measured for example by CEO turnover-performance sensitivity. Additional related evidence is provided by González-Uribe and Groen-Xu (2017), who collect information on fixed-term contracts for CEOs of US corporations and find a negative link between CEO horizon and innovative activity, the prototypical long-term investment. They show that, as the expiration of the contract approaches, the number and quality of patents applied for and subsequently granted declines.

Other papers have focused on the effects of the appointment of new CEO on corporate outcomes. Denis and Denis (1995) find evidence of poor performance preceding forced CEO resignation, followed by small improvements in operating performance once a new CEO is appointed. Ravenscraft and Scherer (1987) study instead changes of management due to acquisitions, and find that the probability of sell-off of a line of business rises when there is a management change in the parent company. Weisbach (1995) goes one step further and finds that divested assets are those likely to be considered unprofitable by the press, and are sold possibly at a loss.

Weisbach et al. (2016) find evidence consistent with a "CEO investment cycle". With the appointment of a new CEO, disinvestment rises and then decline with the CEO's

tenure, whereas investment follows an opposite pattern. They support a causal interpretation of the results by showing that such cycles arise also when the replacement of a CEO is due to exogenous reasons, such as death or illnesses.

Extensive empirical work has also looked at how shareholders' pressure affects managers' attitude towards workers. Bertrand and Mullainathan (2003) study the effects of "antitakeover laws" passed across several US states between the mid-1980s and the early 1990s, and that that partially insulate CEOs from takeovers' pressure. They find evidence contrasting the view of CEOs as empire-builders; instead, they show that they appear to prefer enjoying the "quite life", raising wages of white-collar workers and reducing both plant disruption and creation.

The idea that workers and mangers might form alliances against the influence of shareholders or riders has later found supporting empirical evidence. Such alliances are more likely in the presence of weak investor protection. For example, Atanassov and Kim (2009) show that in presence of poor governance combined with strong union laws, managers of underperforming firms are able to avoid layoffs by large-scale asset sales. Such strategic alliances arise quite naturally in the context of takeovers, as shown theoretically by Pagano and Volpin (2005). When takeover pressure arises, managers can also sell equity stakes to workers as a defense mechanism (see Rauh (2006) and Chaplinsky and Niehaus (1994), among others).

Other papers examine more directly the relationship between workers' earnings and shareholder pressure. Using Swedish social security data, Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos (2009) show that more entrenched managers, as measured by their voting rights, pay their workers more controlling for a number of worker and firm characteristics. Brav, Jiang, and Kim (2015) also find consistent evidence, by showing that wage stagnate, as opposed to rising labor productivity, in firms that are subject to hedge fund intervention, using data from the U.S. Census Bureau. More recently, Liskovich (2016), however, studying the passage of shareholder-sponsored proposals to declassify the board

of directors (a measure likely to improve governance) find that workers' earnings fall on average, but due to a composition effect, with managers tilting the workforce composition more towards less-skilled employees.

This paper is also related to a vast literature that has quantified the role of firm-specific wage policies in explaining wage inequality (Card, Heining, and Kline, 2013; Song, Price, Guvenen, Bloom, and Von Wachter, 2018). Goldschmidt and Schmieder (2017) show that firms are increasingly relying, often due to increasing pressures from shareholders, to outsourcing practices and these practices tend to have negative wage effects on outsourced workers. Kline, Petkova, Williams, and Zidar (2018) show that when a firm is awarded a patent, the extra "rents" generated by the patent are not shared equally across different employees within the firm.

3 Theoretical Framework

3.1 Set-Up

In this section we develop a simple model that characterizes the CEO's optimal effort throughout the course of her tenure. At time 0, shareholders hire a CEO, whose type is unknown both by the CEO and by the shareholders: she can be a High (H) or Low (L) type, with equal probabilities. The CEO is hired for a full term, that is composed of two subperiods. At the substage $t = 1/2$, the CEO obtains a signal $s \in \{q, 1-q\}$ regarding her type. The signal reveals the probability that the CEO is a type H, with $q > 1/2$. At time 1 the CEO type is fully revealed, and cash flow V_H or V_L are realized, depending on whether the CEO is a type H or L, where $i \in \{H, L\}$, and $V_H > V_L$.

The CEO also needs to compensate a unit mass of workers, that are paid both at time 0 and at time 1/2, with wages w_0 and $w_{1/2}$. The CEO and the workers bargain over the wage paid both at both times. We assume that confronting workers over the wage paid is costly for the CEO, who faces an effort cost $c\left(K - w_t\right)^2/2$, where c and K are

some constants. Cutting wages is costly for the CEO, because she may have to deal with unhappy employees, protests or strikes, but increases the cash flow available to both CEO and shareholders.

At time 1, the cash flow net of labor expenses (equal to $V - w_0 - w_{1/2}$) is realized. Shareholders decide whether to fire or retain the incumbent CEO. If they fire her, they pocket the cash flow and hire a new CEO, whose type is, ex ante, also uncertain. If they keep the incumbent CEO, the cash flow is distributed between CEO and shareholders according to Nash bargaining, where the bargaining weights are β and $1-\beta$, respectively. A fired CEO simply gets zero. The (new or incumbent) CEO is the appointed for another full term, and the game restarts. If the CEO is new, at time $t + 1/2$ a signal s regarding her type is again revealed, and at time 2 shareholders again decide whether to fire or reappoint her. The horizon is infinite, and every term is discounted at the rate ρ.[1] The timeline of the game is depicted in Figure 1.

3.2 Solution

We solve the model using backward induction. At stage 1, shareholders' decision is straightforward. If the CEO is a high type, they will keep her forever; otherwise, they will replace her. The CEO will, in every period, set wages such as to maximize her utility. It is easy to show that the wage of a reappointed CEO, for any $t \geq 1$, is equal to $K - \beta/c$ Call the resulting discounted utility of a reappointed CEO V.[2]

At $t = 1/2$, the CEO maximizes the following utility function:

$$\max_{w_{1/2}} s\left[\beta\left(V_H - w_0 - w_{1/2,s}\right) + V\right] - c\left(K - w_0\right)^2/2 - c\left(K - w_{1/2,s}\right)^2/2 \tag{2}$$

[1] We assume for simplicity that the intermediate subperiod is not discounted

[2] Straightforward calculations show that V is as follows:

$$V \equiv \frac{\rho}{1-\rho}\left\{\beta\left[V_H - 2\left(K - \frac{\beta}{c}\right)\right] - \frac{\beta^2}{c}\right\} \tag{1}$$

that gives us the equilibrium intermediate wage $w^*_{1/2,s} = K - s\,\beta/c$.

At time 0 the CEO solves instead the following problem:

$$\begin{aligned}\max_{w_0} \frac{1}{2}\left[\beta\left(V_H - w_0 - \sum_{s\in\{q,1/2\}} s\, w^*_{1/2,s}\right) + V\right] \\ -\frac{c}{2}\left(K - w_0\right)^2 - \frac{c}{2}\sum_{s\in\{q,1/2\}} s\left(K - w^*_{1/2,s}\right)^2\end{aligned} \tag{3}$$

that gives us $w^*_0 = K - \beta/(2c)$.

Comparing the equilibrium wage w^*_0 and the expected wage $w_{1/2,s}$ we obtain our key result:

$$w^*_0 - \mathbb{E}\left[w^*_{1/2,s}\right] = \frac{\beta}{2c}\left[q^2 + (1-q)^2 - \frac{1}{2}\right] > 0 \tag{4}$$

where the last inequality is due to the fact that $q > 1/2$. Hence, as long as the intermediate signal is informative, the CEO is going to exert higher effort, on average, in compressing workers' wages and maximize the net cash flow.

Intuitively, because at stage 1/2 the CEO can estimate more precisely the marginal benefit of her effort, wages will be higher than at stage 0 upon receiving a positive signal, and lower upon receiving a negative signal. Because the expected wage at stage 1/2 is convex in q, on average the former effect prevails over the latter; hence, even if the workers' wage can either increase or decrease over the course of the CEO's tenure, it will decrease on average. An alternative interpretation is that, because the CEO faces the risk of seeing her effort in confronting workers to be wasted if not reappointed, there is an "option" value in waiting for obtaining a more precise signal regarding her chances of being confirmed and concentrate her effort in the second subperiod of her tenure.

3.3 Predictions

The simple framework presented above can be used to make testable predictions that can be taken to the data. The inequality 3.2 suggests that wages should decline over the course

of the CEO tenure. Hence, our main prediction can be summarized as follows:

Prediction 1. Wages should decline as the top executives approach the end of her appointment.

The model relies on a principal-agent framework to generate its implications regarding the behavior of wages over time. In other words, a CEO has an incentive to cut wages after receiving a good signal regarding her type as long as she knows that there is a significant chance of not being reappointed by the shareholders following a poor performance. Hence, the model does not apply to firms with a significant degree of entrenchment, where there should be a much weaker relationship between turnover and performance. Executives who are controlling shareholders (or that belong to the controlling family) clearly present a high degree of entrenchment, as the principal and the agent coincide. Indeed, there is evidence that family managed firms exhibit low sensitivity of turnover to performance (Volpin (2002)). Thus, our second prediction is the following:

Prediction 2. The relationship between wages and board tenure should be weaker if the top management is entrenched, as in family-managed firms.

In Section 3.2, we have showed that the effort of the CEO does not vary over time once she is reappointed. Intuitively, as the CEO has learned her type, there is no further opportunity for learning and her best choice, given a concave utility function, implies smoothing of effort, and so of wages, over time. On the other hand, a new CEO is uncertain about her likelihood of being reappointed, and has stronger incentives to manipulate strategically her effort, and hence labor expenses. This translates into our third empirical prediction.

Prediction 1. The relationship between wages and board tenure should be stronger when top executives have short tenure.

4 Institutional Setting and Data

4.1 Institutional Setting

In Italy, shareholders' meetings can be either "ordinary" or "extraordinary" (Article 2363 of the Civil Code). Extraordinary meetings are convened for non-recurring events, such as mergers, equity issuances or amendments in the corporate chart. In ordinary meetings, those we focus on, shareholders approve the financial report, appoint or revoke board members, and determines their compensation, among other tasks.

Ordinary meetings must occur at least once a year, within 120 days of the the fiscal year-end. This deadline can be prorated to 180 days for conglomerate companies. The law establishes spcecific rules regarding the appointment of members of the board of directors. More specifically, Article 283 of the Civil Code states that: "Directors shall not be appointed for a period longer than three fiscal years, and shall step down on the date of the shareholders' meeting convened for the approval of the financial statement related to the last financial year of their appointment."

In practice, in the vast majority of companies, board members are appointed all together for the maximum length, namely three years. Exceptions are quite rare. In our sample, only one company opted for a staggered board.

Even if most of the companies' boards have a three year-horizon, firms may occasionally opt for shorter term lengths. The reasons for these deviations from their typical behavior are typically explicitly stated in the financial reports.[3]

Other exceptions, that are more problematic for our purposes, arise when boards may be reappointed earlier than originally planned because all the board members resign. [4] This may occur for example if the company is the target of an acquisition, if there are significant changes in the ownership structure, or if the company is subject to a reorgani-

[3]Such instances may occur for example to synchronize the board appointment across companies belonging to the same group.

[4]Some corporate charters also include a clause commonly known as *simul stabunt simul cadent*, that requires a new board to be appointed as soon as a single member resigns.

zation due to inability to meet financial obligations (similarly to Chapter 11 procedure in the U.S. system).

4.2 Boards and Shareholders' Meetings

Our empirical analysis is based on a novel linkage of several databases which provides us with panel data on shareholder meetings, composition and election of boards, firm and worker level outcomes derived from Italian social security records.

We build an hand-collected dataset of shareholders' meetings dates drawing from several sources. We get from Compustat Global, a dataset managed by Standard & Poor's, a list of all the companies listed on the Milan Stock Exchange between 2001 and 2016. For each company-fiscal year, we then obtain the dates of the shareholders' meeting called for the approval of the annual financial report.

We use the following sources, in this order:

(i) Corporate websites. Our starting point are websites of listed companies, that typically have an "investors relations" section, with notices of shareholders' meetings. They are either attached to financial reports, or made available as separate documents. Delisted companies, either because of bankruptcy or mergers, do not typically have active websites. Moreover, most companies report on their websites documents only for the most recent fiscal years. This is our primary data source (1449 observations).

(ii) The Official Gazette, the official journal of record of the Italian government. Notices of ordinary shareholders' meetings need to be made public between 15 and 30 days before the actual meeting dates through advertisements either on national newspapers or the Official Gazette. We download the archive of the gazette, that has been fully digitized starting from 1998 and is available online, and search manually for company names (or their variations) to collect information on their meeting dates. By searching through this archive we obtain 867 observations.

(iii) The archives of financial documents made available on the websites of Borsa Italiana

and Euroborsa. The first is the official website of the Italian Stock Exchange, whereas the second is a privately managed website directed at investors. We recover meeting dates for 456 firm-years

(iv) Newspapers articles. As residual source (265 observations), we search the web for newspapers' articles mentioning corporate shareholders' meetings, primarily on the archive of the main Italian financial newspaper, "Il Sole 24 Ore".

We keep, for each firm, only the fiscal years in which a company has been listed. We are able to recover dates of shareholders meetings for 60.3% of the firm-years in Compustat Global, or 3,037 out of 5,036 observations. Not surprisingly, the matched firms are larger, although not substantially so. The median matched firm has 1,745 employees, versus 1,621 for the non-matched one (means are 9,085 and 7,407, respectively).

As a matter of practice, shareholders' meetings tend to be delayed as much as possible. Given that most firms have december as fiscal-year end and that they have 120 days to schedule the meeting, most of them occur in April: 2,376 out of 3,037. Still, there is significant variation, for two reasons. First, as explained before, some firms have up to 180 days to convene the meeting, and there is a significant number of meetings occurring in May or June (272 and 158, respectively). Second, a large number of firms chooses fiscal year-end months different from December. While in some cases this choice appears related to seasonality in the business activity at the industry level,[5] there is also significant variation within industry. For example, one of the largest banks in the country, Mediobanca, end it fiscal years in June, when December is the dominant choice for the other banks.

We also collect and hand-match information on board composition and shareholders' composition from the website of CONSOB, the authority responsible for the regulation of the Italian stock market. Data on board members allow us to test whether turnover of both board members and top executives is concentrated around board reappointments. Moreover, we construct a proxy for entrenchment by identifying firms in which at least one

[5]For example, three soccer clubs are publicly listed and they all have fiscal years ending in June, right after the end of the premier soccer league competition, that typically occurs in April or May.

of the top three executives belongs to the controlling family, again by reading through the financial reports or collecting biographical information about each manager in newspapers' archives.

4.3 Matched Employer-Employee Data

Worker level outcomes are drawn from the Italian social security institute INPS. This dataset provides the complete employment histories of all individuals that at one point in their career between 1990 and 2016 transition in a firm covered by a survey run called "INVIND" and conducted by the Bank of Italy.[6]

This social security data – henceforth labelled INPS-INVIND - is a matched employer-employee dataset that for each job spell in a given year provides information on variables such as the number of months in which the worker is employed, total gross earnings, total number of days worked, employment contract (temporary vs. permanent), an indicator for full time jobs, etc. Regarding the worker, we have demographic information such as birth year, gender and nationality. Unfortunately, education is not recorded by INPS-INVIND. Regarding the firm, we have additional information on monthly employment, wage bills across different occupations (blue collar - white collar - managers - apprenticeships), year of creation, etc. Importantly, for incorporated firms, we also have information on national tax identifiers (*codice fiscale*) which we will use to link the information described in Section 4.2 with the matched employer-employee data.

Overall, we have information on approximately 130 million spell-year observations, corresponding to a total of 9 million individuals. According to our own estimates based on the universe of Italian social security records[7], in INPS-INVIND we are able to recover information on roughly 85% of the population of individuals employed in a listed firm.

[6]Initially, the survey was representative of manufacturing firms with 50 or more employees. Starting in 1999, more industrial sectors were added (e.g., the energy sector). In 2001 also firms with 20–49 employees were included and finally in 2002 service firms were added to INVIND (Bank of Italy, 2008).

[7]We thank Edoardo di Porto at the VisitINPS program for allowing us to calculate the total number of individuals employed by listed firms over 1998-2015.

Using the detailed spell-level information contained in INVIND, we derive a month by year by individual panel that contains information on key worker level outcomes such as employment and earnings. We next describe how we are going to leverage from this monthly panel to conduct our empirical analysis.

5 Econometric Framework

This section introduces the econometric framework used to assess the dynamic effects associated with an election of a board for Italian listed companies. Throughout the analysis, we limit our attention to the impact of elections of corporate boards that are "regular" and that occurred between 2001 and 2015. An election is defined as regular if the associated corporate board lasted exactly three years. Approximately 94% of Italian elections of corporate board of listed firms satisfy this definition, leaving us with a total of 237 firms.

As a preliminary, "first stage", type of analysis, we test whether board members departures and new hires tend to cluster in the years of the board renewals

Next, we describe two empirical strategies. The first one quantifies how the election of a board affects firm level outcomes both before and after the election. The second one quantifies whether such election impacts the career trajectories of of newly hired employees of listed firms.

5.1 Firm Level Evidence

To test whether wage policies set by top executives vary as a new board appointment approaches, we estimate this baseline econometric model:

$$\log\left(Earnings\right)_{ft} = \beta Last\,Year_{ft} + \theta_f + \delta_t \tag{5}$$

where f indexes firms and t indexes month-years, and θ and δ are firm and time fixed effects, respectively. The dependent variable is the average monthly wage paid to firm

workers, and our main regressor of interest is $LastYear$, a dummy equal to 1 if the firm is in the third year of its board term and zero otherwise. Notice that this definition implies that we are not inferring whether the firm is in its last year of the board term *ex post*; for example, if the expected board term is three years but it unexpectedly ends after only two years, the dummy will be set to zero throughout the term. The model is estimated through weighted least squares, using as weight the total firm employment.

To gauge more information regarding the dynamics of wages over the board term, we also estimate the following model:

$$\log(Earnings)_{ft} = \sum_{j=2}^{12} \beta_j \mathbb{1}(Quarter_{ft} = j) + \theta_f + \delta_t \tag{6}$$

In this equation, we estimate a coefficient for each quarter of a firm board term, starting from the second quarter up to the twelfth. We hypothesize lower values of the β coefficients as the firm approaches the end of its term, i.e., for high values of j.

5.1.1 Firm Level Data: Summary Statistics

We are able to match to social security data 97% of the listed firms that had regular elections of their board between 2001 and 2015. Table 1 shows summary statistics for the 30,030 firm-month observations in our sample. Earnings are in 2010 Euros. The average worker earns about €3,255. There is a subsantial gender gap, with men earnings almost €800 more than female employees (€3,582 versus €2,796). Even larger differences arise between part time and full time workers (€1,858 versus €3,340). The firms in our sample are substantially more likely to employ men; the average share of female employees is 0.36. The fraction of part time workers is even smaller, just 0.05 on average.

5.2 Worker Level Evidence

Does an incoming election of a new corporate board impact the career of a simple employee at the firm? To answer this question, we begin by focusing on a particular group of workers that might be particularly exposed by the election of the board: individuals that were hired by a listed firm *prior* to an election. The empirical strategy will compare the outcomes of these treated individuals in the months preceding and following the election of a board to a matched control group of new hires.

To construct this control group, we start by finding for a given firm-election combination the set of workers who were hired by the listed firm f 36 to 12 months before the corresponding date of the election, e. The pool of potential control workers are going to be individuals never employed by a listed a firm that were hired also 36 to 12 months before e by a non-listed firm $f' \neq f$, belonging to the same 3-Digit industry and province of firm f.

Within each (f, e) cell, we then estimate a probit regression of whether a worker is employed in a listed firm, controlling for month of hiring from reference firm fixed effects, gender, age, earnings, full time status and employment contract. The last four variables all measured in the first month of hiring. For each newly hired worker in a listed firm we then find the control worker with the closest propensity score.

Using the full employment histories of these new hires, belonging either to the treatment or to the control group, we then estimate the following event study regression model

$$y_{it} = \alpha Listed_i + \gamma_{TimeHired(i)} + \lambda_{llm(i),t} + \sum_{k=-23}^{12} \beta_k (D_{it}^k \times Listed_i) + r_{it} \tag{7}$$

where y_{it} is the outcome of individual i in month t. $Listed_i$ is a dummy variable equal to 1 if individual i was initially hired by a listed firm and therefore represents a "treated" worker; $llm(i)$ is a function that outputs the local labor market of individual i at the moment of hiring. Therefore, $\lambda_{llm(i),t}$ are local labor market specific time effects. $TimeHired(i)$ is a

function that outputs for individual i the month-year in which the worker was originally hired by the baseline firm, so that $\gamma_{TimeHired(i)}$ control for month-year of hiring fixed effects. D_{it} are event study indicators defined as $D_{it} = \mathbf{1}\{t = t^*_{f(i)} + k\}$, where $t^*_{f(i)}$ is when the firm that originally hired individual i - denoted by the function $f(i)$ - is going to have the election of the associated corporate board.

Our interest lies in the coefficients $\{\beta_k\}$. These coefficients measure how the outcome variable y_{it} has changed for workers employed in a listed firm relative to the control group in the kth month before or after the election occurred.

5.2.1 Worker Level Data: Summary Statistics

Table 2 provides summary statistics for the worker level analysis. Column 1 and 2 report averages of several variables for a monthly worker level panel of individuals that at some point during their career transitioned in a listed firm. Column 1 consider person-month observations associated with a listed firm while Column 2 selects person-month observations where the person is not employed by a listed firm.

Being employed in a listed firm is associated with higher earnings, days worked, log daily wages, higher probability to be on a permanent contract and on full time jobs. Part of these differences are explained by compositional changes: observations in Column 1 correspond to older, more senior individuals that are slightly less likely to be females. Similar gaps arise when benchmarking the averages reported in Column 1 with the ones reported in Column 3 which correspond to the sample of individuals that were never employed by a listed firm.

Column 4 and Column 5 summarize the matched sample of new hires described in the previous section. Our algorithm does a good job in matching average characteristics of new hires from listed firms to a control group of workers never employed by listed firms. Importantly, around 30% of these newly hired workers are originally employed under a temporary employment contract. Figure 1 shows the distance in months relative to the

election when treated workers were hired by listed firm compared to control group workers. The distribution seems relatively well balanced. Interestingly, there is a spike of workers being hired by listed firm about 2 years before the incoming board election. Figure 2 shows the exit rate of treated and control workers in time defined relative to the election of the corporate board. Exit is defined as the fraction of workers no longer employed by the original firm. Interestingly, there is a jump corresponding to the beginning of the last year before the occurrence of the election.

6 Results

6.1 Evidence on Board Members' Turnover

In principle, board members can leave the company, or be fired, at any point in time. Hence, as a starting point, it is important to test whether directors' turnover is concentrated in the third year of each term. Because most of the main strategic decision are delegated to the three top decision makers, we also test whether such association holds when we restrict the analysis to CEO, president and vice-president who, in Italian companies, are the key decision-makers (Volpin (2002)). Our data on board composition are from Consob, and are released every June and December; hence, our time unit is a semester.

We proceed as follows. Let $N_{f,t}$ be the number of board members of firm f in time (semester) t, and let $T_{f,t}$ be the number of members working for the company in time t but not in time $t+1$. Our turnover measure is simply given by T over N. Figure 2 plots the average turnover for each of the six semester of the board terms, both for the entire board and the top executives. In semesters 1 through 5 turnover is never above 5%; however, jumps to 24.6% for the full board, and 17.9% for the top three managers. To run a more formal test, we also estimate:

$$\frac{T_{f,t}}{N_{f,t}} = \beta Last\,Semester_{f,t} + \delta_t + \eta_f + \varepsilon_{f,t} \tag{8}$$

where $Last\,Semester_{f,t}$ is a dummy equal to 1 if a the company board is in the final (6^{th}) semester of its term, and η_f and δ_t are vectors of firm and time dummies, respectively. Table 3 confirms that much of the board members' turnover does indeed concentrate in the final year of the term. In column 1, we find that the fraction of board members who leave the company in an election semester rises by 0.20, relative to a full sample average of 0.07. When we concentrate to the top three executives, we obtain similar results. The coefficient β is equal to 0.14, in the same order of magnitude of the average in the full sample (0.06). Hence, we find that the effects of a new board appointment on directors and top executives turnover is substantial, giving support to our hypothesis that board terms may give rise to significant changes in the horizon of board members.

6.2 Baseline Results

Table 4 tests the model presented in Section 3 by estimating equation 5. In all the regressions the dependent variable is multiplied by 100, so that the coefficient of interest can be interpreted as telling by how many percentage points monthly earnings change in the third year of a board term. Column 1 presents the main result of the paper, and shows that earnings fall by about 0.46% when top executives are in the final year of their appointment. The coefficient is significant at the 1% level (standard error= -0.17). Given that, as showed in Table 1, the average monthly earnings in our sample are equal €4,245, the coefficient implies a drop in monthly earnings equal to €14.97, that adds up to about €180 over 12 months.

Figure 3 plots the coefficients obtained by estimating equation 6, together with confidence intervals. The coefficients give rise to an inverse U-shaped relationship between time since the board appointment and average wages. Wages are somewhat flat at the beginning of the term, rise reaching their peak in the second year, and fall sharply afterwards, reaching the minimum in the last two quarters of the term.

The second implication of our model is that this compression of earnings should not

be present in firms where managers are entrenched, such as those where the managers belong to the controlling family. In column 2 we re-estimate our model on the sample of firms in which at least one of the top three executives belongs to the controlling family. In this subsample, the coefficient on the Last Year dummy becomes essentially equal to zero, and insignificant. When we re-estimate our model on the remaining firms, instead, we find a much larger effect, with the coefficient that almost doubles relative to the baseline estimate of column 1, and is now equal to -0.86, significant at the 1% level. Figure 4 and 5 confirm the findings, and show that, while in the subsample of family-managed firms no pattern arises, there is a clear strong drop in earnings towards the end of the board term, with all the coefficients associated with the last four quarters of the term being negative and significant.

The third implication of our model is that the compression of earnings towards the end of the board term should be present primarily for firms where executives are uncertain regarding the quality of their match with the company they are managing. We compute a measure the average tenure of the top executives, and then each month sort firms according to whether they are above or below the median executives' tenure. In column 4, where we concentrate on firms run by executives with short tenure, we find a highly significant coefficient on the Last Year dummy equal to -0.91. On the other hand, the coefficient becomes an insignificant 0.01 when we look at firms with more experienced executives (column 5). Hence we find support to the story presented in our model: the incentives to compress workers' earnings towards the end of the board term are present as long as there is opportunity for learning; when the managers are aware of the likelihood of being further reappointed, their optimal strategy consists in smoothing their effort in managing labor expenses. Figures 6 and 7 show, indeed that the familiar inverse U-shape is present only in firms run by short-tenured executives.

6.3 The Effect of Board Terms across Workers' Groups

Our detailed administrative data allow us to test whether the effects of managerial incentives vary across different categories of workers. We are particularly interested in testing whether executives are better able to reduce earnings of workers who are likely to be in a weaker bargaining position, such as women, or those who have more flexible contractual arrangements, such as part time workers.

Table 5 shows evidence consistent with this conjecture. In particular, columns 1 and 2 compare the overall effects on earnings for both male and female employees. While both fall in the last year of a board term, the effect is more pronounced for women, who experience a drop of 63 basis points, relative to a decrease of 39 basis points for men.[8] Similarly, we find much stronger effects for part time workers, relative to full time workers, whose salaries and working hours are arguably harder to cut. The coefficient for part time workers is -80, and drops in magnitude to -0.47 when only full time workers are included.

Given this evidence, it is natural to ask whether executives reduce labor expenses not only by cutting wages but also by changing the mix of employees, and so the fraction of male and full time workers employed. We follow Nagel (2005) in mapping the share of employees belonging to either group to the real line, by using as dependent variable the logit transformation $\log[1/(1-x)]$, where x is either the fraction of male or the fraction of full time workers, censored below at 0.0001 and above 0.9999. We demean and standardize the dependent variable for ease of interpretation. As columns 5 and 6 show, there is a small but significant drop in the share of male and full time workers, corresponding to 0.6% standard deviations. Hence, executives appear to tilt their employment composition towards towards the categories of workers who suffer the largest cuts in earnings.

[8] In these regressions the weights used are the number of employees belonging to each category considered (female, male, part time and full time.)

6.4 Worker Level Evidence

In this section we move from a firm-level to a worker-level analysis to analyze how workers' turnover relates to board tenure. We compare workers employed in a listed firm that are hired between 36 and 12 months before the appointment of a new board to otherwise similar workers who are hired by a private firm, matched following the procedure described in Section 5.2. Recall that the evidence in Section 6.2 points strongly towards a peak in the employment level reached in the second year of the board term, and a fall in employment in the third year, that drops further once the new board is appointed.

We follow each worker in both treatment and control group from the month of hiring from the base firm, up to 12 months after the appointment of the new board. This analysis has at least three advantages. First, and more obviously, our sample size increases substantially. Second, we can perform a matching on a number of characteristics at the worker level, hence obtaining a more precise counterfactual. Finally, we can perform a graphical, non-parametric analysis, that allows us to test whether there are significant "jumps" in the probability of separation, and whether these jumps correspond to the relevant shareholders' meetings.

Figure 10 shows an interesting pattern. The probability of separation from the baseline firm is indistinguishable between the treated and the control group up until treated workers enter the last year of the board term. In this month, the fraction of workers separated by the listed firms jumps by about 3 percentage points. A second jump occurs in the two months surrounding the shareholders' meeting appointing the new board , and correspond to an additional increase in the separation rate of approximately 3 percentage points. The probability of separation from the initial firm keeps increasing in the first year of the board term up to a 9 percentage point gap between treated and control workers, relative to 12 months prior to the election. This accounts for a percentage increase of roughly 40% when benchmarked against the share of workers separated from their initial firm in the pre period (defined as 24 to 12 months prior to the election).

Figure 10 shows that a consistent fraction of separated workers in the treatment group remained non-employed following the election. In particular, event study estimates on an indicator equal to 1 if the worker is employed in the corresponding month shows that workers originally hired by a listed firm are less likely to be employed immediately after entering the last year of the board. The gap in employment probabilities between treatment and control group grows to around 3 percentage points 12 months following the election of the board. Combining our estimates on separation and employment, we conclude that around 30% of workers separated due to the board incoming election are unable to find a new job 12 months following the shareholders' meeting to appoint a new corporate board.

Figure 10 shows the earnings profile of treated vs. control group workers. Interestingly, we find that monthly earnings, while being relatively flat in the months corresponding to the second year prior to the election, modestly *increase* on average in the treatment group in the last 12 months prior to election. Following the election, we find an even larger increase in earnings for new hires by listed firms up to approximately 80 monthly euros (about 3% of monthly earnings in the pre-election period). Table 6 shows that the effect of the incoming board election on log daily wages, hence when conditioning on person-month observations where the worker is employed, are even large - up to 5%, 12 months following the election.

In the next section we show that these positive earnings impacts are driven by one particular group: workers that "survived" the incoming election of the listed firm and therefore were not subject to a separation event.

6.5 Leavers and Stayers

Equation (7) estimates *average* effects on newly hired employees by listed firms relative to a counterfactual matched group of workers of newly hired workers by non-listed firms. As a consequence, the evidence presented in the previous section can hide substantial heterogeneity. In this section, we decompose our earnings estimates into two components.

The first component is based on individuals that are still employed with their initial employer following the election, while the second component is based on workers that instead are no longer with their baseline firm following the election.

Clearly, leavers and stayers are likely to be selected (e.g. those who stay with the firm revealed them selves to have higher unobserved quality than average). To account for this selection, we estimate the impact of a board election on earnings by estimating equation (7) *separately* for stayers and leavers.

Figure 12 shows the earnings impact of board election on earnings across leavers and stayers, see also Table 7. Leavers and stayers share a remarkably similar earnings profile two years prior to the election relative to their corresponding counterfactual. Interestingly, also the levels are quite similar: a leavers earns about 2683 monthly real euros two years prior to an election while a stayer earns about 2690 euros.

Earnings trajectories become suddenly very different as we start approaching the election of a board. One year prior to the election, stayers experience a disproportional increase in their earnings while leavers instead have a large earnings decline. Following the election, patterns become even more enhanced: 1 year following the election a stayer in the treatment group earns about 7% higher earnings compared to the pre-period (two years prior to election). A leaver originally employed by a listed firm on the other hand has 15% lower earnings compared to the pre-period.

The evidence presented here is consistent with learning and managerial specific wage policy. In the middle of its tenure, the board decides to hire a pool of new workers. As the election approaches, however, the board decides to concentrate only on a specific set of individuals (i.e. the stayers) and separate from the remaining ones (i.e. the leavers). Stayers are rewarded with an earnings premium that however is only perceived by these individuals *after* the initial corresponding test period (-23 to -12 months to an election). An interesting question going forward is to explore whether such premium can be approximated by the corresponding firm specific earnings premium, that could be estimated, for

example, with an AKM style decomposition (Abowd, Kramarz, and Margolis, 1999). The idea of firm-specific rents only getting shared with a target group of workers within the firm is consistent with the recent differential rent sharing literature (Kline et al., 2018; Goldschmidt and Schmieder, 2017; Daruich, Di Addario, and Saggio, 2017).

The earnings losses for leavers closely resemble the earnings losses of layoff workers (Jacobson, LaLonde, and Sullivan, 1993). Figure 13 shows that these earnings losses are due to higher rates of separations into non-employment for the treatment group. Figure 13 shows that the separation rate of leavers is highly cyclical and tracks the incoming corporate board election, as the separation rate from the listed firm in the treatment group increases disproportionately exactly 11 months prior to the election of board with another, more modest jump, following the election of the board. Figure 13 shows that the labor market is able to absorb only around 60-70 percent of these separated workers, as the share of non-employed individuals 12 months following the election is 20 percentage points higher in the treatment group relative to the control group within the sample of leavers.

7 Conclusions

In this paper we have established some novel facts regarding the relationship between directors and executives' horizon and their policies. We have provided evidence of a hump-shaped relationship between average earnings and board tenure, that in Italy typically lasts three years. In particular, while workers' earnings rise mildly halfway during the executives' term, they drop sharply as managers approach their reappointment decision. Such effects are absent when managers belong to the controlling family, and so are entrenched, on when they have long tenure, and so are subject to lower uncertainty regarding the quality of their match with the firm.

The advantage of our design, relative to existing work on the impact of board turnovers,

is that our evidence is not contaminated by the potentially confounding effects of other events that are typically associated with directors or executives' replacement (such as firings, retirement or takeovers). Moreover, the detailed information available in our administrative data allows a deeper understanding of managerial actions, as related to human resources policies.

In particular, we show that these managerial incentives have distributional effects. The gap in the average wage earned by men and women widens in the last year of a board term; however, precisely because managers are better able to cut salaries of female employees, the fraction of women employed tends to rise. Similar results arise when we focus on part time and full time workers.

We also show that workers hired at listed firms suffer higher separation rates relative as they approach the end of board terms, and that many of the separated workers are unable to quickly find a new job. However, those that remain employed experience a rapid rise in earnings; hence, managers appear to cut labor expenses primarily by firing less valuable employees, and rewarding those that are instead retained. These results suggest that there are substantial effects of managerial incentives on firm workforce composition, wages and investment in human capital.

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8 Tables

Table 1
Summary Statistics: Firm-Level Data

Table 1 shows summary statistics for the firm-level data. The sample is composed by firm-month observations for firms listed in the Italian public stock exchange. Earnings are denoted in euros and deflated using the 2010 CPI. All reported statistics are weighted by number of firm-month-year observations and computed across firm-month observations. Total number of firm-month-year observations is equal to 30,030. Source: INPS-INVIND.

	Mean	10^{th} p.	Median	90^{th} p.	St. Dev.
# Employees	835.16	5.00	164.00	1,340.00	3,710.89
Earnings	3,255.47	2,187.62	3,123.56	4,436.52	874.83
Earnings (Females)	2,796.38	1,824.13	2,724.41	3,793.97	811.28
Earnings (Males)	3,582.09	2,470.64	3,385.92	4,984.32	946.49
Earnings (Part Time)	1,858.38	1,232.74	1,755.03	2,530.87	604.18
Earnings (Full Time)	3,340.66	2,286.08	3,208.49	4,497.87	852.10
Share Female Empl.	0.36	0.09	0.35	0.63	0.21
Share Part Time Empl.	0.05	0.00	0.03	0.11	0.09

Table 2
Summary Statistics: Worker-Level Data

Table 2 shows summary statistics for the worker-level data. The first two columns consider the panel of person-month observations of individuals that were employed for at least a month in a listed firm. The first column presents average statistics for person-month observations associated with a listed firm while the second column consider person-month observations not associated with a listed firm. The third column presents summary statistics for a 25% random sample panel of person-month observations of individuals never employed by a listed firm. Column 4 and 5 consider the matched samples of new hires used for the worker-level analysis. In particular, in column 4 we consider individuals hired by a listed firm 36 to 13 months before a regular election of a board that occurred between 2001-2015. These new hires are then matched to a sample of invididuals hired in the same period as the treated workers but by a non listed firm, see text for details. The summary statistics of these control workers are showed in column 5. Statistics to both Column 4 and 5 refer to the first month of hiring. Column 5 computes a pvalue on the difference between treatment and control, the corresponding standard error is clustered at the local labor market level.

	At Least Once in Listed Firm		Never in Listed Firm	Treated	Control	p-value difference
	Listed	Unlisted				
Monthly Earnings	2588.96	2099.81	1750.35	2471.66	2451.86	0.21
Monthly Days Worked	22.98	20.59	19.65	23.46	23.31	0.17
Log Daily Wage	4.62	4.45	4.35	4.50	4.50	0.84
Temporary Contract	0.06	0.14	0.09	0.31	0.31	0.39
Permanent Contract	0.85	0.70	0.71	0.67	0.67	0.97
Full Time	0.85	0.74	0.69	0.94	0.94	0.30
Tenure (Months)	54.98	33.35	40.98	1.00	1.00	.
Age	41.70	36.93	37.91	33.56	33.50	0.47
Female	0.34	0.36	0.35	0.32	0.31	0.08
Industry	0.64	0.53	0.54	0.83	0.82	0.34
Banking	0.04	0.04	0.01	0.07	0.07	0.22
Services	0.10	0.34	0.36	0.11	0.11	0.38
# of Individuals	707,782	707,782	963,894	50,386	46,405	96,791
# of Firms	230	601,631	1,071,117	217	16,357	16,574
# of Listed Firms	230	0	0	217	0	217

Table 3
Board Turnover

Table 3 has regressions where the dependent variable is the fraction of board members employed in the current year but not employed with the same firm in the following year. In column 1 we compute this ratio considering all board members; in column 2 we restrict the attention to top executives (CEO, president and vice-president). Election is a dummy equal to 1 in the final year of the board term, and zero otherwise. All regressions include firm and year fixed effects. Standard errors, in parentheses, are clustered at the firm level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

Dependent Variable: *Turnover of...*	All Board Members (1)	Top 3 Executives (2)
Election	0.203***	0.137***
	(0.013)	(0.015)
Avg. of Dep. Variable	0.071	0.058
Observations	2,335	2,335
R^2	0.391	0.197
Firm FE	X	X
Year FE	X	X

Table 4
Effect of Board Terms on Earnings

Table 4 presents regressions where the dependent variable is the logarithm of average monthly earnings at the firm level. The variable Last Year is a dummy equal to 1 in the third year after the board appointment. In column 1 the full sample is used. The sample of column 2 includes only firms where at least one of the top executives belongs to the controlling family, whereas column 3 includes all the other firms. Columns 4 and 5 include firms whose the average tenure of the top executives is below or above the median, respectively. All regressions include firm and year-month fixed effects and observations are weighted by firm employment. Standard errors are clustered at the firm level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

	Baseline	/w Family Ex.	/w no Family Ex.	Short Tenure	Long Tenure
	(1)	(2)	(3)	(4)	(5)
Last Year	-0.455***	0.003	-0.860***	-0.912***	0.012
	(0.167)	(0.116)	(0.276)	(0.291)	(0.167)
Observations	30,030	15,950	11,829	14,515	12,484
R^2	0.957	0.954	0.949	0.961	0.971
Firm FE	X	X	X	X	X
Year-Month FE	X	X	X	X	X

Table 5
Effect of Board Terms on Earnings

In columns 1 through 4 of Table 5, the dependent variable is the logarithm of average monthly earnings at the firm level, for different categories of workers. Column 1 includes only female workers, column 2 male workers, column 3 part time workers, and column 4 full time workers. In columns 5 and 6 the dependent variable is a logit transformation of the share of male and full time workers employed at the firm, respectively. The logit transformation is equal to the share divided by 1 minus the share. The variable Last Year is a dummy equal to 1 in the third year after the board appointment. All regressions include firm and year-month fixed effects. In columns 1 through 4 observations are weighted by total female, male, part time, and full time employees, respectively. In columns 5 and 6 they are weighted by total employment Standard errors are clustered at the firm level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

Dep. Var.	log(Earnings)				Share of Empl.	
	Females	Males	Part Time	Full Time	Males	Full Time
	(1)	(2)	(3)	(4)	(5)	(6)
Last Year	-0.626**	-0.389***	-0.799***	-0.473**	-0.006**	-0.006**
	(0.250)	(0.149)	(0.258)	(0.201)	(0.003)	(0.002)
Observations	25,993	26,864	20,294	27,110	30,030	30,030
R^2	0.966	0.953	0.960	0.949	0.957	0.904
Firm FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X

Table 6
Newly Hired Workers

Table 6 shows regression estimates from model (3), all expressed relative to the 12th month prior to the election. Sample correspond to a panel of person-month observations of treated and control workers. Treated workers are individuals hired by a listed firm 36 to 12 months before an incoming election of the corporate board. The corresponding pool of control workers is represented by individuals hired in the same time frame by a non listed firm belonging to the same local labor market of the reference listed firm. For a given treated worker, we match a control worker from this pool on the basis of a propensity score, see text for details. A local labor market is defined as the interaction of a 3 Digit sector code and province. Outcomes are listed in the header of each column. Outcome in column 1 is an indicator equal to 1 if worker i is separated from the its original firm of hiring in month t. Employment is a dummy equal to 1 if the individual earn positive earnings in the corresponding month. Earnigs and Days worked are set to 0 for person-month observations where the corresponding individual is not employed. Average of the dependent variable reports the average of the corresponding outcome in the column header 24 to 12 months before the election took place. Regression model control for an indicator for individual originally hired by a listed firm, month-year by LLM fixed effects and month of hiring relative to election fixed effects. Standard errors are clustered at the local labor market level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

Dependent Variable:	Separated	Employed	Monthly Earnings	Days Worked	Log Daily Wages
	(1)	(2)	(3)	(4)	(5)
24 Months to election	-0.02*	0.01	-43.07	0.07	-0.02**
	(0.01)	(0.00)	(26.62)	(0.12)	(0.01)
18 Months to election	-0.01	0.00	-17.35	-0.05	-0.01
	(0.01)	(0.00)	(24.55)	(0.11)	(0.01)
11 Months to election	0.03***	-0.01***	-10.92	-0.25***	0.00
	(0.01)	(0.00)	(9.04)	(0.08)	(0.00)
6 Months to election	0.04***	-0.01**	10.92	-0.22*	0.01***
	(0.01)	(0.01)	(12.13)	(0.11)	(0.00)
1 Month post election	0.06***	-0.02***	30.55*	-0.33***	0.03***
	(0.02)	(0.00)	(16.18)	(0.12)	(0.01)
6 Months post election	0.08***	-0.03***	53.15***	-0.37***	0.04***
	(0.02)	(0.01)	(17.38)	(0.13)	(0.01)
12 Months post election	0.09***	-0.03***	76.80***	-0.52***	0.05***
	(0.02)	(0.01)	(25.80)	(0.18)	(0.01)
Avg of Dependent Variable	.22	.9	2491.06	22.23	4.55
# Local Labor Markets	337	337	337	337	337
# of Person-Month Obs	3342676	3342676	3342676	3342676	3299064

Table 7
Newly Hired Workers - Leavers and Stayers

Table 7 This table shows regression estimates from equation (3), all expressed relative to the 12th month prior to the election. Equation (3) is estimated on two samples. Stayers correspond to individuals that remained with the original employer 3 quarters following the election. Leavers correspond to individuals that were employed by the original firm 12 months prior to the election but were no longer employed with the original employer 12 months following the election. Separated is an indicator equal to 1 if worker i is separated from the its original firm of hiring in month t. Employment is a dummy equal to 1 if the individual earn positive earnings in the corresponding month. Earnigs and Days worked are set to 0 for person-month observations where the corresponding individual is not employed. Average of the dependent variable reports the average of the corresponding outcome in the column header 24 to 12 months before the election took place. Regression model control for an indicator for individual originally hired by a listed firm, month-year by LLM fixed effects and month of hiring fixed effects. Standard errors are clustered at the local labor market level. ***, **, and * indicate statistically different from zero at the 1%, 5%, and 10% level of significance, respectively.

Sample:	Stayers	Leavers			
Dependent Variable:	Monthly Earnings	Monthly Earnings	Separated	Employment	Days Worked
24 Months to Election	-56.98*	-34.16	0.01	-0.01	0.29
	(29.90)	(41.81)	(0.02)	(0.01)	(0.23)
18 Months to Election	-11.79	17.51	0.01	-0.01	0.35**
	(26.06)	(29.23)	(0.01)	(0.00)	(0.14)
11 Months to Election	13.57	-57.72***	0.10***	-0.03***	-0.60***
	(10.76)	(16.70)	(0.03)	(0.01)	(0.16)
6 Months to Election	36.22**	-85.68**	0.16***	-0.05***	-0.98***
	(15.45)	(35.78)	(0.04)	(0.01)	(0.23)
1 Month post Election	86.80***	-262.85***	0.30***	-0.12***	-2.71***
	(15.56)	(58.92)	(0.04)	(0.02)	(0.37)
6 Months post Election	141.43***	-298.44***	0.38***	-0.14***	-3.15***
	(18.22)	(60.00)	(0.04)	(0.02)	(0.38)
12 Months post Election	170.43***	-396.02***	0.52***	-0.19***	-4.33***
	(19.14)	(66.02)	(0.04)	(0.02)	(0.42)
Avg of Dep. Variable	2690	2683	0	1	24.76
# Local Labor Markets	240	197	197	197	197
# of Person-Month Obs	1688860	608305	608305	608305	608305

9 Figures

Figure 1
Timeline of the Model

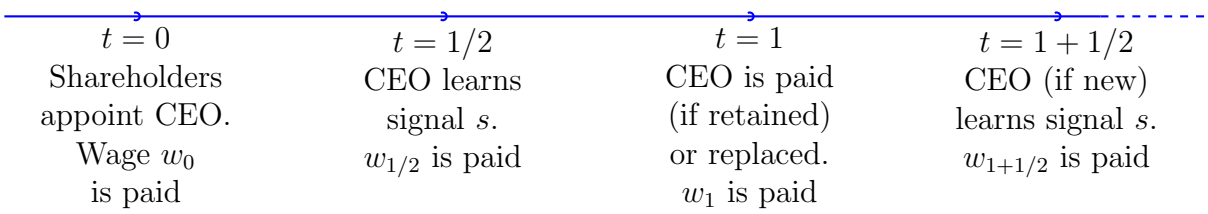


Figure 2
Board Members Turnover over the Course of the Board Term

The histogram plots the average turnover of board members for each of the six semesters of a board term. Turnover in a given semester is defined as the fraction of board members present in the previous semester but not in the current one. This measure is computed either for all the board members (light blue bars) or just for the top three executives (dark blue bars.

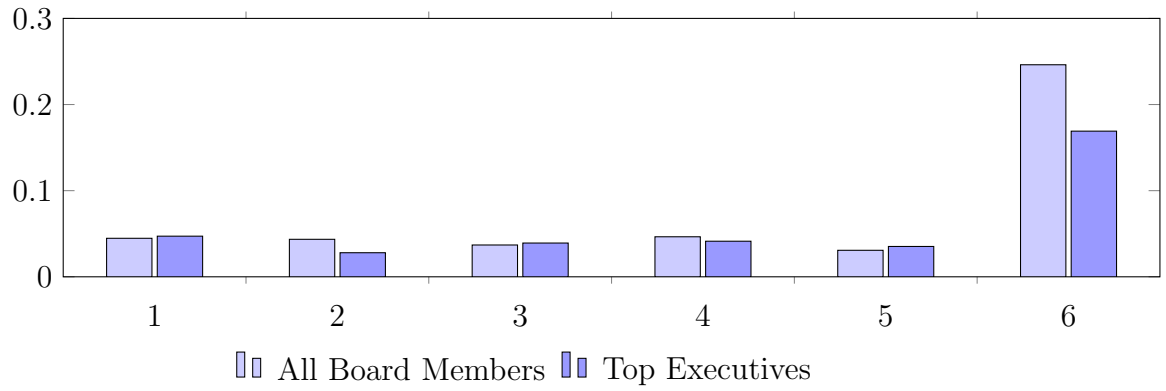


Figure 3
Earnings over the Board Term

Figure 3 presents coefficients, together with 95% confidence intervals, obtained from estimating equation 6 over the sample of firms that have at least one of the top executives belonging to the controlling family.

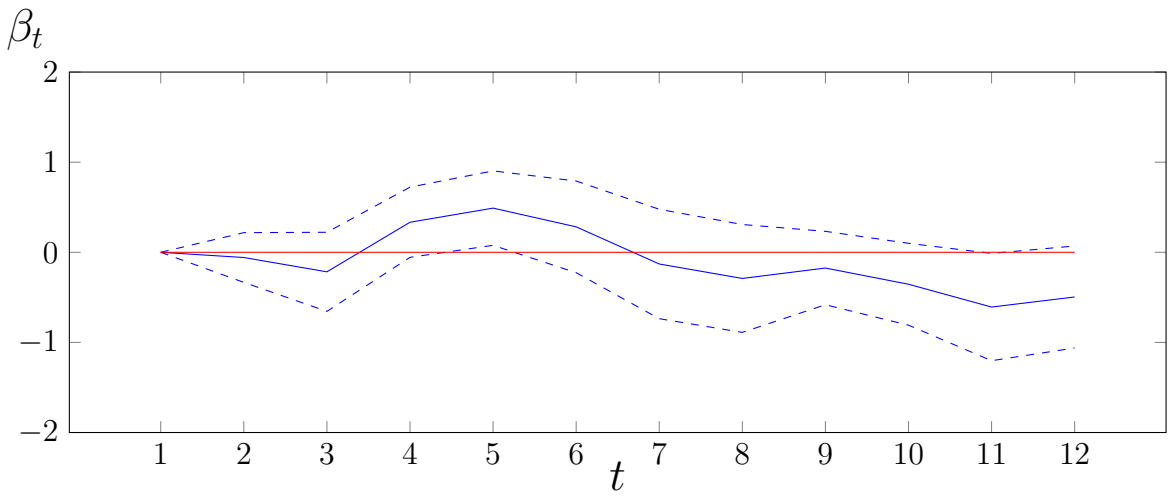


Figure 4
Earnings over the Board Term:
Family Managed Firms

Figure 4 presents coefficients, together with 95% confidence intervals, obtained from estimating equation 6 over the sample of firms that have at least one of the top executives belonging to the controlling family.

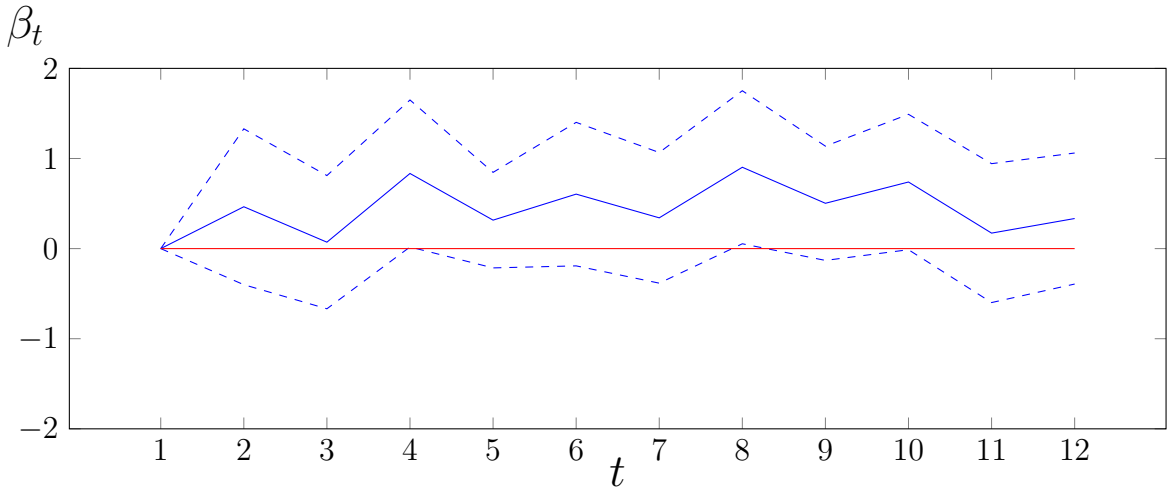


Figure 5
Earnings over the Board Term:
Non-Family Managed Firms

Figure 5 presents coefficients, together with 95% confidence intervals, obtained from estimating equation 6 over the sample of firms that have none the top executives belonging to the controlling family.

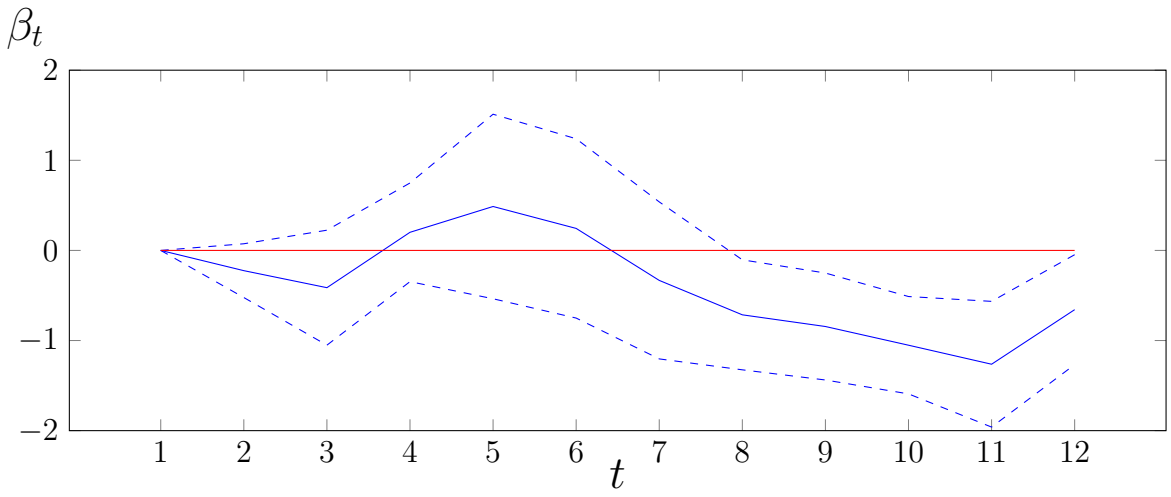


Figure 6
Earnings over the Board Term:
Executives with Short Tenure

Figure 6 presents coefficients, together with 95% confidence intervals, obtained from estimating equation 6 over the sample of firms who are managed who have average tenure below the median.

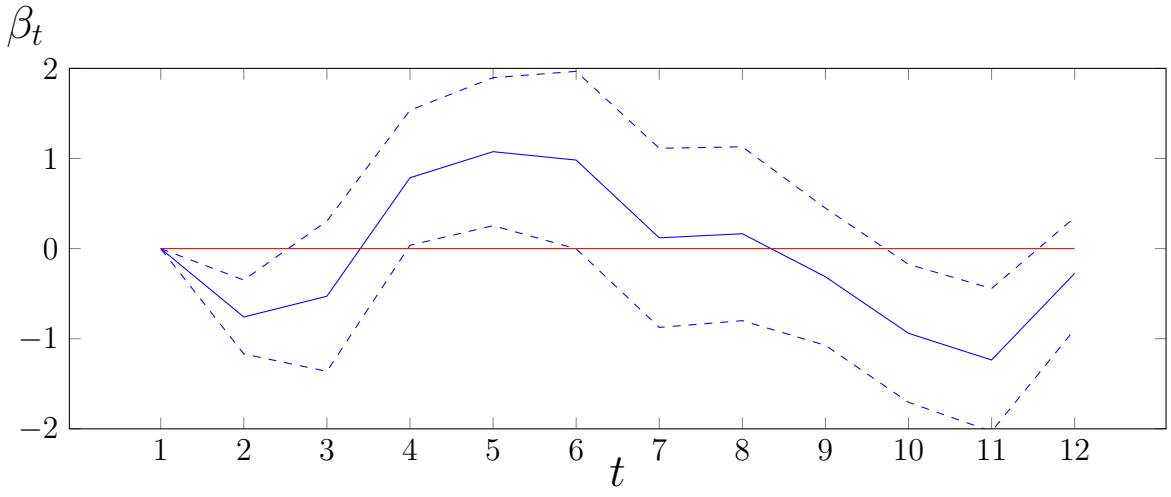


Figure 7
Earnings over the Board Term:
Executives with Long Tenure

Figure 7 presents coefficients, together with 95% confidence intervals, obtained from estimating equation 6 over the sample of firms who are managed who have average tenure above the median.

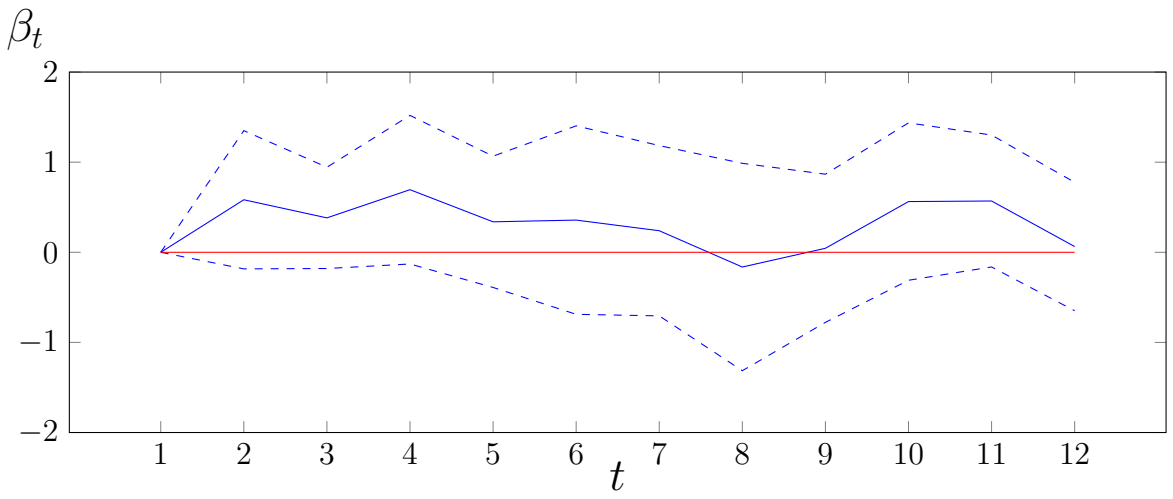
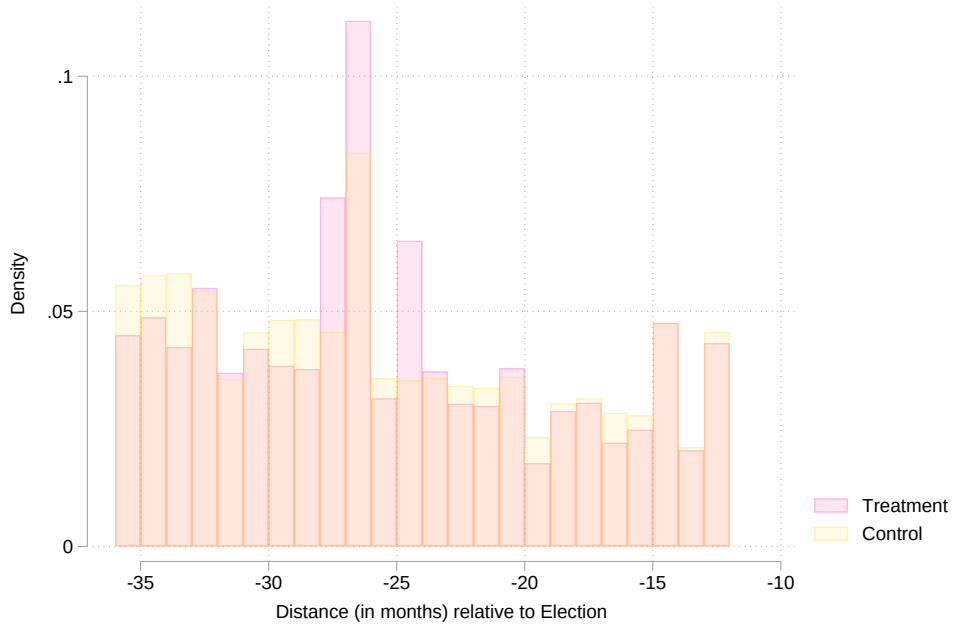
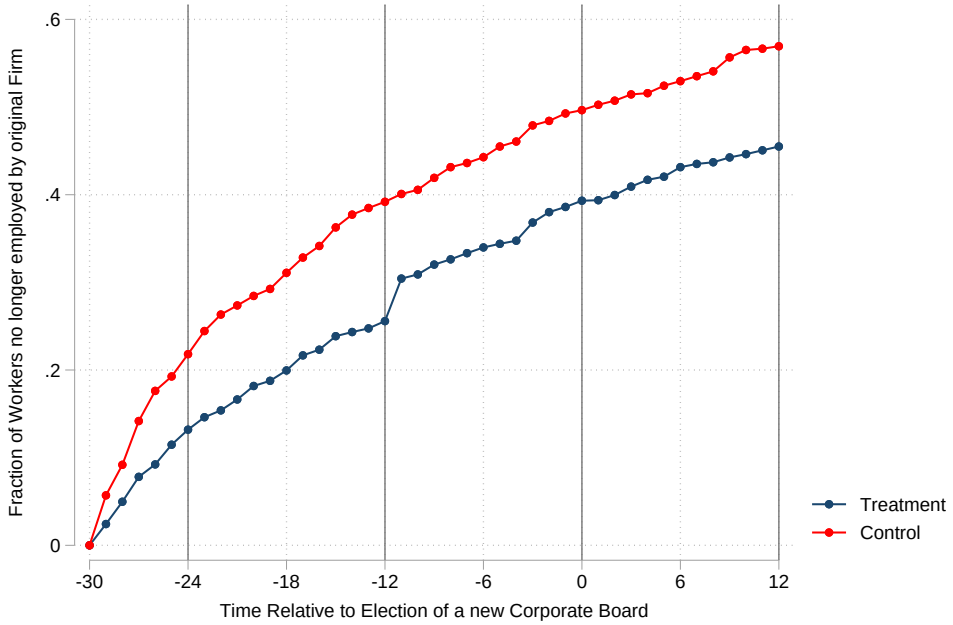


Figure 8
Month of Hiring



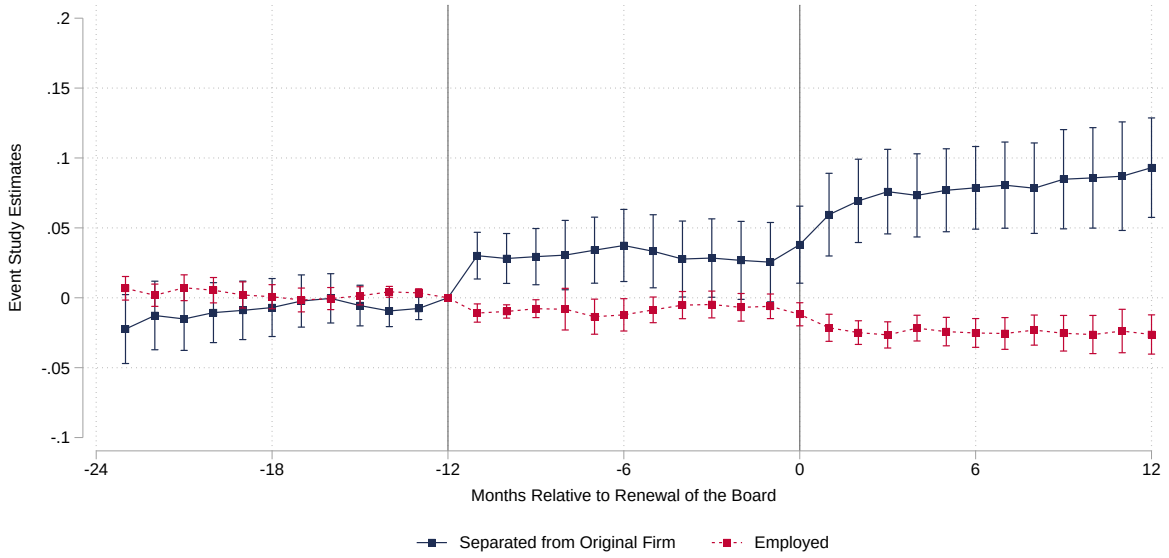
Note: This graph plots the month of hiring relative to the month of a board election for newly hired workers by listed firms (the treatment group) and for the corresponding matched control group, see Section 5.2 for details.

Figure 9
Exit Rates in Event Time



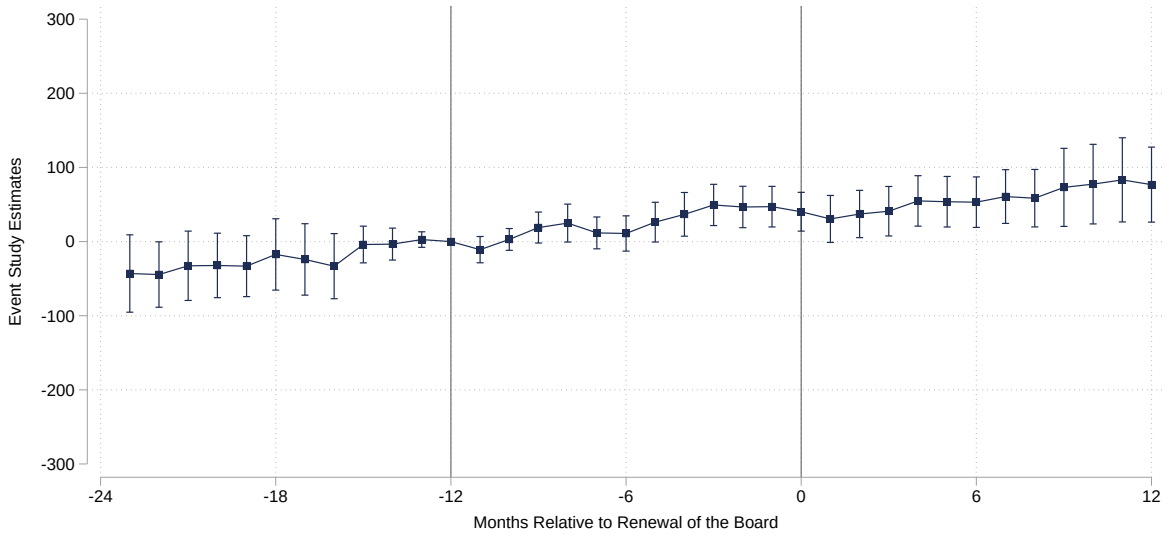
Note: This graph plots the share of newly hired workers no longer employed by their original firm k months before or after the election of new corporate board. Treated workers represent individuals hired by a listed firm 30 months before an election and control represent the corresponding matched sample of individuals as defined in Section 5.2.

Figure 10
Separation and Employment of New Hires



Note: This graph plots the event study coefficients from equation (7) on two outcomes. The first outcome variable is an indicator equal to 1 if the individual is still employed with her original employer. The second outcome variable is an indicator equal to one if in the corresponding month the individual is employed according to Italian social security records.

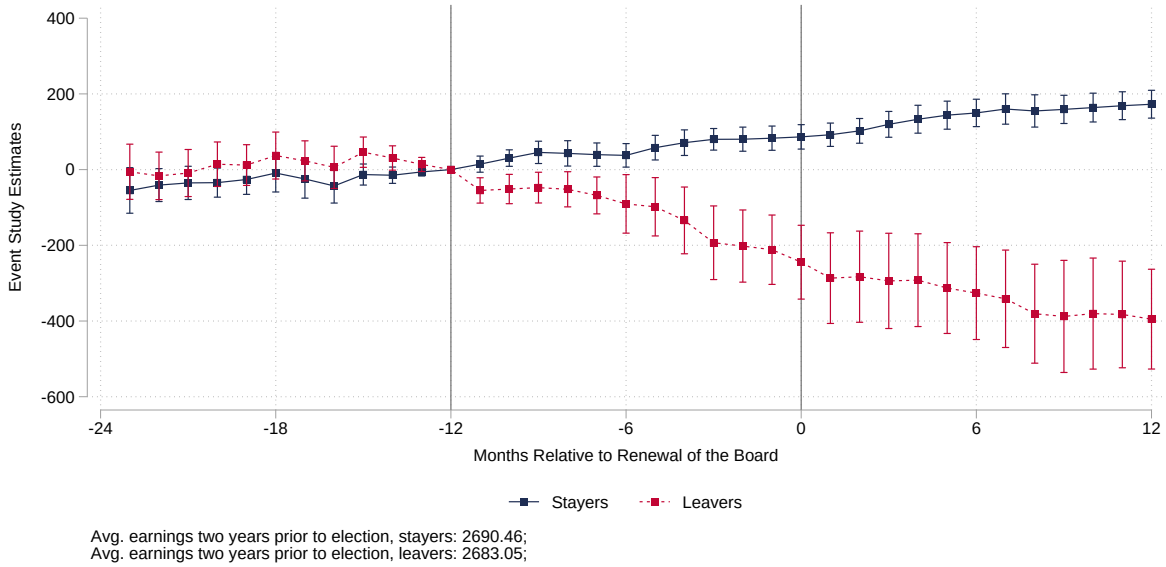
Figure 11
Earnings of New Hires



Avg. earnings two years prior to election: 2334.27;

Note: This graph plots the event study coefficients from equation (7) when y_{it} correspond the earnings of individual i in period t. If in the corresponding person-month observation an individual does not result employed to Italian social security records, we set $y_{it} = 0$.

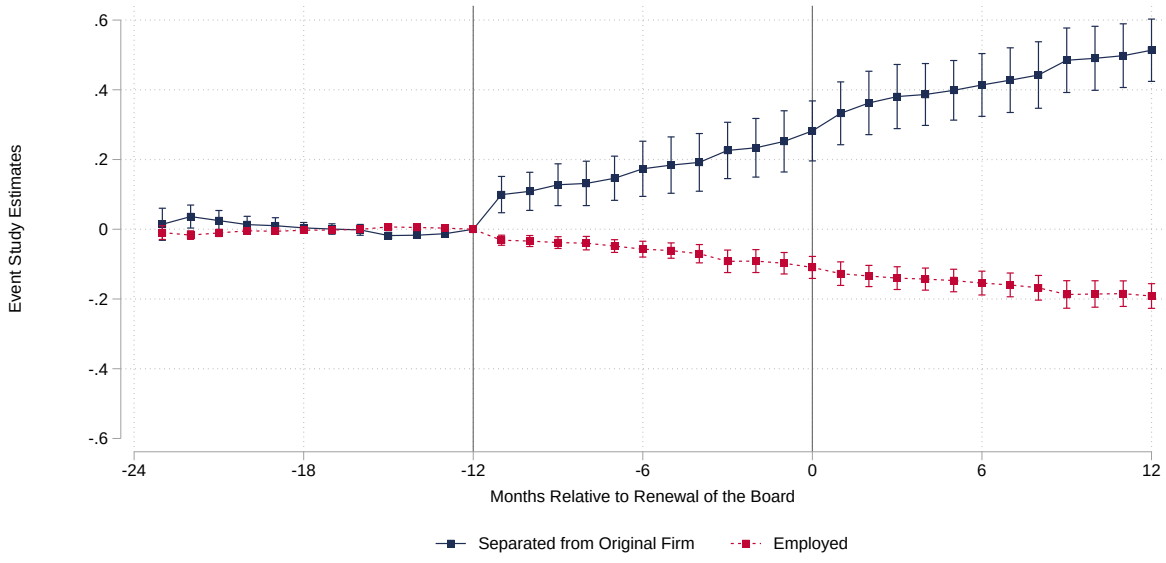
Figure 12
Earnings of New Hires - Stayers vs. Leavers



Avg. earnings two years prior to election, stayers: 2690.46;
Avg. earnings two years prior to election, leavers: 2683.05;

Note: This graph plots the event study coefficients from equation (7), setting y_{it} equal to the earnings of individual i in month t. We report event study estimates computed separately on two different samples. The first sample conditions on individuals in both treatment and control group that are still with the initial employer three quarters following the election of a new board - call this the sample of stayers. The second sample conditions on individuals in both treatment and control group that are are no longer with their initial employer 12 months following the election of a new board but were still employed by their original employer two years prior to the election- call this the sample of leavers. See text for further details.

Figure 13
Separation and Employment of Leavers



Note: This graph plots the event study coefficients from equation (7) on the sample of leavers. Results on two outcome variables are shown. The first outcome variable is an indicator equal to 1 if the individual is still employed with her original employer. The second outcome variable is an indicator equal to one if in the corresponding month the individual is employed. The sample leavers consists of individuals in both treatment and control group that are are no longer with their initial employer 12 months following the election of a new board but were still employed by their original employer two years prior to the election.