The Effect of Minority Veto Rights on Controller Tunneling

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Abstract

A central challenge in the regulation of controlled firms is curbing controller tunneling. As independent directors and fiduciary duties are widely seen as not up to the task, a number of jurisdictions have given minority shareholders veto rights over these transactions. To assess these rights' efficacy, we exploit a 2011 regulatory reform in Israel that gave the minority the ability to veto pay packages of controllers and their relatives ("controller executives"). We find that the reform curbed the pay of controller executives and led some controller executives to quit their jobs, or work for free, in circumstances suggesting their pay would not have received approval. These findings suggest that minority veto rights can be an effective corporate governance tool.

JEL classification: G18, G34, G38, J33, J38, K22, L20, M12, M52

Key words: controlling shareholders, tunneling, corporate governance, minority shareholders, shareholder voting, executive compensation, related party transactions

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1. Introduction

Most publicly traded firms around the world have a controlling shareholder, also known as a controller (Claessens, Djankov and Lang, 2000; Faccio and Lang, 2002; Khanna and Yafeh, 2007; Holderness, 2009; Gutiérrez and Sáez Lacave, 2018). In these firms, a key objective of corporate governance is protecting minority shareholders from tunneling via related party transactions (Shleifer and Vishny, 1997; Gilson and Gordon, 2003; Enriques and Volpin, 2007; Djankov et al., 2008; Jackson and Roe, 2009).

The standard tools for constraining controllers — the use of independent directors and the duty of loyalty — are often seen as insufficient. Independent directors are typically appointed and terminated by the controller, making them at least somewhat loyal to her (Bebchuk and Hamdani, 2017; Enriques et al., 2017). And procedural impediments to shareholder litigation and controller-friendly substantive law can vitiate the legal system's potential deterrent effect (Enriques et al., 2017).

A potentially more powerful protective tool is subjecting related party transactions to advance minority approval (Goshen, 2003; Djankov et al., 2008). This approach, now favored by the OECD (2012), has been adopted by Israel, the securities regulators of the major Canadian provinces (including Ontario, home to the Toronto Stock Exchange),[1] Australia, Hong Kong, India, Indonesia, Mexico, Russia, and a number of other former members of the Soviet Union.[2] The European Union has also recently considered it.[3] Delaware uses a softer version of this approach, which rewards a controller who voluntarily conditions a related party transaction on minority approval by granting the transaction more

[1] Canadian Securities Administrators (2017).

[2] OECD (2012), Black and Kraakman (1996), Enriques and Tröger (2018), Li (2018).

[3] The European Union considered requiring minority veto rights for conflict transactions, in the end leaving this decision to member states. *Compare* Article 9c of the Proposal for a Directive of the European Parliament and of the Council Amending Directive 2007/36/EC as Regards the Encouragement of Long-Term Shareholder Engagement and Directive 2013/34/EU as Regards Certain Elements of the Corporate Governance Statement (April 9, 2014) *with* Article 9c of the Directive (EU) 2017/828 of the European Parliament and of the Council of 17 May 2017 Amending Directive 2007/36/EC as Regards the Encouragement of Long-Term Shareholder Engagement, 2017 O.J. L 132/1.

deferential judicial review.[4] Similarly, in the United Kingdom, controlled firms with premium listing on the stock exchange can be required to obtain minority approval for transactions with the controller.[5]

However, there is scant empirical evidence on whether minority veto rights work. While Delaware has long rewarded controllers for obtaining minority approval, it does not require this approval. A Delaware controller chooses whether to grant the minority a veto right over a transaction, raising significant identification concerns. Even in regimes that require minority approval, substantial empirical challenges remain. First, the controller chooses whether to propose the transaction, creating endogeneity problems. Second, it is difficult to find comparable transactions not subject to minority veto rights. Third, mandatory minority veto rights are a recent regulatory innovation, limiting the size of potential samples.

A 2011 regulatory reform in Israel offers a unique setting for testing the efficacy of minority veto rights. A key element of this reform, known as Amendment No. 16 to the Israeli Companies Law of 1999,[6] was to give minority shareholders of controlled firms veto rights over proposed related party transactions, including the proposed pay of controllers and their relatives serving as officers or directors ("controller executives"). In particular, their pay packages require approval by a majority of the minority votes cast in a shareholder meeting ("MoM approval") within three years of the last approval. Absent MoM approval, a controller executive can continue to work, but only without pay. Until 2011, pay packages of controller executives had required the approval of only a third of the minority ("ToM approval") and this approval was valid indefinitely, so that a controller executive could continue to draw the same amount of pay even if the minority came to believe that this amount had become excessive. The reform did not alter the approval mechanisms for the pay of

[4] Kahn v. M&F Worldwide Corp., 88 A.3d 635 (Del. 2014);

[5] Davies (2018); Listing Rules, Section 11.1.1. To avoid a minority vote, the firm must have an agreement with the controller containing certain independence provisions, including the requirement that related party transactions be conducted at arm's length and on normal commercial terms and that there be no circumvention of the listing rules. See Listing Rules, Section 6.1.4D R.

[6] Companies Law (Amendment No. 16), 5771–2011, Section 34, which amends Companies Law, 5759–1999, Section 275.

executives unrelated to controllers ("non-controller executives"), creating a viable control group.[7]

Contemporaneous anecdotal accounts suggest that the reform had real bite. For example, Rami Levy, the controller and CEO of an eponymous supermarket chain, had to cut his bonus in half to secure minority support for his pay contract (Calcalist, October 16, 2011). According to our calculations, his post-approval package was 26% lower than the previous one. Ilan Ben Dov, the controller of cellular holding company Suny Electronics, forfeited most of his compensation as board chair to win minority shareholder approval of his compensation package (Calcalist, October 16, 2011; Globes, November 14, 2011). According to our calculations, this led to a pay drop of 83%. Other controllers and their relatives left their executive positions or continued to work without pay due to inability to reach an agreement with the minority on their compensation. At wireless technology firm MTI, the threat of minority veto felled a father-and-son team: MTI's controller and board chair (Zvi Borovitz) and CEO (Zvi's son, Moshe Borovitz) both announced their departure (Globes, December 7, 2011).

We use the reform to determine whether giving the minority veto rights had a systematic effect on the pay of controller executives by hand-collecting and analyzing data on hundreds of firms and thousands of executives, some related to controllers and others not, over a six-year period around the reform.

We find that the grant of minority veto rights constrained the pay of controller executives. In particular, we find that, controlling for other factors, the reform is associated with an average decline of 10% in controller-executive pay relative to the pay the same executive would have been expected to receive on the basis of firm size, profitability and other factors. We also find that this decline is at least partly driven by a substantial increase in the frequency of pay cuts for controller executives. Minority shareholders appear to be selective in wielding their veto power, forcing some controller executives to accept massive pay cuts, but not others.

We also examine the reform's effect on the rate at which controller executives disappear from a firm's list of highest paid executives. Such a disappearance means that the controller executive either stops working at the firm or continues

[7] Below we discuss the possibility that the reform affected non-controller pay indirectly.

working for limited or no pay. We find that the likelihood of controller executives disappearing increased by about 40% after the reform, often in circumstances indicating that the controller executives might have had their pay package vetoed. About 10% of the disappearing controller executives were replaced by professional managers not related to the controller, suggesting that the reform affected corporate management. Our direct estimates of the effect of minority veto rights on controller-executive pay are therefore probably downward-biased.[8]

Our study does not assess the desirability of the reform because minority veto rights can also thwart value-increasing transactions (Enriques, 2015; Rock, 2018). In addition, to the extent private benefits motivate controllers to generate value (Burkart et al., 1997; Gilson and Schwartz, 2015), curbing these benefits can be harmful. Furthermore, impeding controller tunneling through a particular type of related party transaction can induce controllers to use costlier forms of tunneling. That said, we do not observe a post-reform increase in related party transactions or a change in dividend policies.[9]

Also worth noting is that controller-executive pay may be different from other related party transactions, making generalization difficult. Specifically, the cost to the minority of mistakenly vetoing a desirable controller-executive pay arrangement will generally be low, as a controller whose pay is improperly rejected still has an incentive to ensure the firm is run well. By contrast, the cost to the minority of mistakenly vetoing a desirable commercial or financial arrangement with the controller can be high. The minority may accordingly be more reluctant to veto non-pay related party transactions, reducing the utility of minority veto rights in non-pay contexts. Despite our inability to assess the overall welfare implications of the reform, our results demonstrate that granting

[8] The average decline in controller executive pay understates the effect of the reform also because many MoM approvals were due only in 2012 or 2013 (for pay contracts last approved in 2009 or 2010). Much of the controller-executive pay observed in the initial post-reform years thus reflects the lingering effect of pre-reform pay arrangements.

[9] We cannot rule out the possibility of an increase in other forms of tunneling, including indirect tunneling that do not involve a related party transaction (Bebchuk and Hamdani, 2018). For example, a controller of several firms may impose the costs of a joint investment on one of the controlled firms (where she has low cash-flow rights), while the benefits accrue primarily to other controlled firms (where she has high cash-flow rights).

the minority veto rights over a particular transaction makes it more difficult for controllers to extract value through that channel.

Our findings are related to four broad corporate governance questions.

First, we shed light on the potential use of minority veto rights to constrain tunneling through related party transactions. Because the requirement of minority approval in related party transactions is relatively recent, almost all prior work relating to minority veto rights concerns controllers who voluntarily grant the minority veto rights in Delaware freezeouts to reduce judicial scrutiny of the transaction (Subramanian, 2007; Restrepo, 2013; Restrepo and Subramanian, 2015). However, it is impossible to determine empirically the effect of minority veto rights in this setting, as both the timing of the freezeout proposal and the decision to grant the minority veto rights are endogenous, and the grant of minority veto rights changes the legal treatment of the transaction. The advantage of our study is that it examines a largely exogenous event: we study mandatory minority approval of existing related party transactions by a state-imposed deadline. In addition, the availability of pay data for non-controller executives, combined with executive fixed effects, enables us to construct robust controls. In contemporaneous work, Li (2018) studies binding minority shareholder votes in India on related party transactions and demonstrate that this mechanism can reduce minority shareholder expropriation.[10]

Second, our findings contribute to the literature on the effectiveness of shareholder voting generally, including in widely held firms. Shareholders typically have veto rights over fundamental corporate actions like charter amendments and mergers. In addition, depending on the jurisdiction and the firm, shareholders may have veto rights over corporate actions like equity issuances (Yermack, 2010; Holderness, 2018), acquisitions (Kamar, 2006; Becht et al., 2016), equity compensation plans (Armstrong et al., 2013), or extraordinary transactions involving directors (Enriques et al., 2017).

[10] Also related is Chen, Bin, and Yang's (2013) study of the effect of a Chinese regulation requiring advance minority approval for stock issuances, transactions that often facilitate tunneling (Fried, 2018; Fried and Spamann, 2018). Chen et al. (2013) find that mean cumulative abnormal stock returns associated with stock issuances are negative before and positive after the regulation, suggesting that minority veto rights improve the quality of stock issuances. This study does not include controls in the form of stock issuances unaffected by the reform.

The protection that these veto rights afford is difficult to measure because — outside the say-on-pay (SoP) context, where periodic votes are mandated — firms put proposals to shareholder vote only when expecting approval, possibly after negotiating with institutional investors (Carleton, Nelson, and Weisbach, 1998). This endogeneity makes it difficult to infer from voting outcomes whether a shareholder vote affects transaction outcomes, as the outcome that would occur absent a vote is unknown. By contrast, our setting features largely exogenously timed votes, permitting us to test whether shareholders use their voting power to constrain insiders.

Third, we contribute to the line of research investigating tunneling through pay. While pay is not the most lucrative channel for tunneling, as it is salient and often small relative to firm value, it is readily accessible because controllers and their relatives often work for the firm and it is accepted that they should be paid. Prior work has therefore sought to determine whether pay tunneling occurs by comparing the pay of controller executives to that of non-controller executives. This work has found pay tunneling in some jurisdictions — including Italy (Bozzi et al., 2017), Chile (Urzua, 2009), and Israel (Barak et al., 2011) — but not in others — including Germany (Elston and Goldberg, 2003) and Continental Europe generally (Croci et al., 2012).

Our setting enables us to test for the presence of pay tunneling more directly by examining the effect on pay of a reform that, in midstream, introduced arm's-length bargaining between the minority and the controller. Even before the reform there was a constraint on pay tunneling, as controller executives needed ToM approval to obtain pay increases. However, the reform substantially tightened the constraint, as all controller-executive pay arrangements now needed MoM approval every three years. Our findings, that the reform increased the likelihood of pay reductions for controller executives, constrained controller-executive pay, and caused some controller executives to quit, suggest that the minority perceived some controller executives as overpaid — although it is possible that the minority penalized these controller executives for reasons unconnected to tunneling.

Finally, our findings contribute to the extensive literature on the effect of SoP votes on executive pay levels and structure. In the United Kingdom, nonbinding SoP has caused certain shareholder-favored changes in the structure of executive pay (Ferri and Maber, 2013; Gregory-Smith et al., 2014). By contrast, in the United States, nonbinding SoP has had little effect on pay (Ertimur et al., 2011;

Brunarski et al., 2015; Cuñat et al., 2016; Iliev and Vitanova, 2018).[11] Using a large multi-jurisdiction sample of firms, Correa and Lel (2016) find that SoP restrains the growth of executive pay and increases the sensitivity of pay to performance, primarily in regimes with nonbinding SoP. However, in all jurisdictions studied to date, SoP requires a simple majority, guaranteeing approval in controlled firms. Our study is the first to examine the effect of binding SoP requiring MoM support, and shows that binding SoP can not only restrain executive pay but also cause executives to step down or work without pay.

The remainder of the article is as follows. Section 2 presents the data and our empirical approach. Section 3 describes our main empirical results. Section 4 presents several extensions and robustness tests and Section 5 concludes.

2. Methodology and data

Our analysis focuses on a 2011 Israeli reform of the regulation of public firms. Prior to 2011, related party transactions — including pay packages of controller executives — had to receive approval by a third of the minority (ToM) once for the duration of the transaction.[12] The reform raised the threshold to a majority of the minority (MoM) and, importantly, required long-term transactions to receive new approval every three years. The reform thus gave the minority real veto rights: the ability to deny a controller executive any pay going forward.

We study the effect of these veto rights on the pay of controller executives using hand-collected data on executive compensation for firms listed on the Tel Aviv Stock Exchange in the years 2009–2015. We exclude financial firms (for which measures of performance are different), dual-listed firms (which did not report individual executive compensation until 2014), and firms with public debt but no public equity (which are not subject to minority veto rights). Our sample,

[11] Other work related to SoP in the United States examines the effects on stock prices of nonbinding shareholder proposals to adopt nonbinding SoP (before the Dodd–Frank Wall Street Reform and Consumer Protection Act of 2010 mandated nonbinding SoP) and the adoption of regulation increasing shareholder influence over pay (Cai and Walking, 2011; Larcker et al., 2013).

[12] Hamdani and Yafeh (2013) describe the Israeli corporate governance landscape before the reform.

an unbalanced panel described in Panel A of Table 1, consists of 591 firms, of which 31% are in manufacturing, 27% are in services and 25% are in real estate.

Like Delaware law, Israeli law defines a shareholder (or a group of affiliated shareholders) as a controller if she can direct the firm's actions. For purposes of the requirement to obtain minority shareholder approval of controller transactions, including executive pay, Israeli law presumes that a 25% shareholder is a controller unless another shareholder holds 50% of the shares. Virtually all firms in our sample have a controller.

Panel A of Table 1 also presents accounting data on firm size and profitability obtained from the commercial provider A-Online. Firm size, measured by total assets, varies considerably across firms, with a mean that is much higher than the median. Accordingly, we control for the natural logarithm of total assets. As is standard in the executive compensation literature (for example, Bebchuk and Grinstein 2005), operating profitability is measured by return on assets (ROA).[13] In our sample, ROA averages about zero, with a median of 2.4%, indicating the presence of many poorly performing firms. In fact, ROA is negative in about 30% of the observations. Accordingly, we control for ROA and in some specifications also use a dummy variable to denote negative profitability.

Israel requires the types of firms in our sample to disclose the compensation of individual executives (like the United States in its regulation of domestic public firms). In particular, these firms must report the individual annual compensation of the five highest paid executives in the firm and its subsidiaries, each of the three highest paid executives in the firm itself, and any holder of at least 5% of the shares (if paid by the firm). The precise definition of covered executives and the possibility of mid-year turnover mean that firms sometimes report the pay of fewer or more than five executives. For each firm, we obtain from annual reports and proxy statements the names, positions, compensation packages, and pay

[13] In many specifications we use ROA together with individual or firm fixed effects, capturing changes in profitability relative to the firm-specific mean ROA over time. The results reported below remain qualitatively unchanged in unreported regressions using lagged profitability and market-to-book ratio instead of ROA.

approvals of reported executives, typically including both controller executives and non-controller executives.[14]

In our sample, the mean and median number of reported executives is five (Panel A of Table 1). More than 40% of all firms report the compensation of exactly five executives, making five the modal number of reported executives. Another 30% of the firms report the compensation of six or seven executives, 10% of the firms report the compensation of eight to ten executives, and another 10% report the compensation of three to four executives.

While virtually all firms in the sample have a controller, only about two-thirds of the firms report at least one controller executive. The median and mode of the number of controller executives per firm is one, and the mean is 1.35 (Panel A of Table 1), with 63% of controller executives serving as board chair or CEO. In one of the robustness tests described below, we exclude firms with no reported controller executives, obtaining similar results to those of the main specifications.

Panel B of Table 1 presents the executive compensation data, consisting of about 13,600 observations of about 4,500 executives during the period 2009–2015. Controller executives comprise about a fifth of the executives in the sample but, because their turnover is lower than that of other executives, they comprise about a quarter of the observations.

The average level of total compensation of an executive in the sample is about NIS 1.3 million (about $325,000) and the median is about NIS 800,000 (about $200,000), with controller executives earning on average about NIS 1.5 million — 15% above the sample average. Some controller executives are relatives of the controller, who may occupy less senior positions than some non-controller executives and bring down the average.

As expected, equity compensation is more common in compensation packages of non-controller executives. Non-equity compensation (total compensation minus equity-based pay) accounts, on average, for 88% of total compensation in the full sample and for 95% of total compensation of controller executives. Although controller executives are less likely to receive equity-based pay, they

[14] We classify executives as controller executives according to the type of pay approval they obtain and verify the classification using the executive roster in the annual report.

typically hold much larger equity stakes than non-controller executives: 23% on average (with a median of 16%), compared to 0.4% on average (with a median of 0%) for non-controller executives. Here too, some of the controller executives are relatives of the controller, bringing down the group average.[15]

Panel C of Table 1 presents compensation approvals by type and year. There are 205 pre-reform ToM approvals and 718 post-reform MoM approvals. MoM approvals appear to occur in two rounds. The first round starts in 2011, when the new law became effective. The second round starts in 2014, when the MoM approvals obtained in 2011 expired. In an extension of our analysis, we distinguish between the two MoM approval rounds.

Panel C of Table 1 also presents the distribution over time of compensation approvals for non-controller executives. These approvals include board approvals for the compensation of officers and shareholder approvals by a simple majority for the compensation of board members.[16] Both allow a controller to increase the pay of non-controller executives even if the minority objects. We use this information to compare the effect of different approval types on the likelihood of a compensation reduction.

Finally, Panel D of Table 1 reports the numbers of controller- and non-controller executives disappearing from their firm's list of highest paid executives each year. Casual observation suggests that the number of disappearing controller executives is higher in the post-reform period. We examine these data in more detail below.

Our main empirical tests are divided into three parts. In the first part of our analysis, we use a difference-in-difference model to estimate the post-reform change in the compensation levels of controller executives. In essence, we compare the post-reform change in compensation of controller executives, who

[15] A recent Bank of Israel internal memorandum reports very similar figures for the entire population of listed firms in Israel around the same period. Within the controller group (controller plus related parties), the average blockholder holds 23.3% of the equity; firms have 2.74 blockholders, on average, holding together about 64% of the equity. Because our focus in the present study is on the effect of the 2011 reform on the compensation of controller executives, where equity-based pay is not a major feature, we do not examine further the structure of compensation contracts.

[16] Starting in 2013, new pay contracts of non-controller executives required MoM approval in certain circumstances. Our findings do not materially change when excluding these approvals.

were directly affected by the reform, with the post-reform change in compensation of non-controller executives in the same firm, who were not directly affected. For example, this can be a comparison of the change in the compensation of a controller who serves as board chair with the change in compensation of a hired CEO at the same firm.

Ideally, the control group in an analysis of this type should be totally unaffected by the reform. In our setting, this assumption may be violated if the compensation of non-controller executives is linked to that of the most senior executive in the firm — and that senior executive is a controller executive.[17] However, if such linkage exists between the compensation of non-controller executives and the compensation of controller executives, it would only bias our estimates *against* finding pay differences post-reform between controller executives and non-controller executives.[18] Our estimates would then be conservative.

[17] Dittmann et al. (2018) show that, while employee compensation in Germany is only modestly affected by CEO compensation, the compensation of top executives tends to move together more closely.

[18] Our estimation can also be inaccurate if the controller executives anticipated the reform and rushed to obtain ToM approvals ahead of the change. This could affect our estimates in two ways: if compensation increases prior to the reform were unusually frequent, observed pre-reform compensation levels would be unusually high and the estimated effects of the post-reform change (reduction) in compensation would be larger than their true value. On the other hand, a rush to obtain ToM approvals would postpone these executives' post-reform MoM approval deadlines and consequently postpone the post-reform decline in compensation, if there was one, by up to three years. This would make estimated effects of the post-reform change (reduction) in compensation smaller than their true value. In practice, we believe that biased estimates due to the reform being anticipated are unlikely. First, controller executives enjoying minority shareholder support for a pay increase before the reform would have obtained ToM approval even if the reform was not anticipated. Second, while the possibility of some kind of reform was publicly discussed for several years, its timing and content evolved during a lengthy legislation process. A MoM approval requirement without a requirement to renew approval periodically was part of the Companies Law of 1999. In 2000, the law was amended to require only ToM approval. The idea of requiring MoM approval resurfaced in a 2006 report of a public committee. That report was the basis of an Israel Securities Authority proposal in 2008 and a government bill in March 2010. The bill introduced for the first time, in addition to a MoM approval requirement, a requirement to renew the approval every three years. In March 2011, the law was enacted with further modifications.

In the second part of the analysis, we explore one mechanism by which the reform affected controller-executive pay levels: the minority shareholders' use of their veto right to force controller executives to take pay cuts.[19]

In the third part of the analysis, we examine the extent to which the reform caused controller executives to disappear from the firm's list of highest paid employees and the likelihood that a disappearing controller executive was replaced by a hired professional manager.

Finally, towards the end of the paper, we extend the core three parts of the analysis and present a variety of additional results and robustness tests.

3. Main results

3.1. The reform's effect on controller-executive pay level

We begin by examining whether the reform affected the pay levels of controller executives. We use a standard difference-in-differences specification:

$$\text{Log}(\textit{Total Compensation})_{ijt} = \alpha + \beta * \textit{Controller Executive}_{ijt} * \textit{Post Reform} + \textit{Firm-Level Controls}_{jt} + \textit{Executive Fixed Effects} + \textit{Year Fixed Effects} + \varepsilon_{jt}, \quad (1)$$

where *i*, *j* and *t* denote the individual executive, the firm and the year, respectively. *Controller Executive*Post Reform* is a dummy for a controller executive (the treated group) in the year 2011 or later; the dummy for a controller executive prior to the reform is subsumed by individual executive fixed effects.[20] Executive fixed effects capture (among other things) each executive's average level of compensation over time. Firm-level controls and year fixed effects capture other determinants of pay.

[19] Minority veto rights can also be used to keep pay from increasing.

[20] If we were to include a dummy variable for controller executives, as in a classic difference-in-differences specification, its coefficient would merely reflect the few executives whose relation to the controller varies over time (otherwise, that status would be absorbed by the individual executive fixed effect), or who serve in two firms and are related to the controller only in one of them. In unreported regressions that include this variable, we find that its coefficient is positive and the remaining coefficients are similar to those in Table 2.

Our main dependent variable in this part of the analysis is the natural logarithm of total compensation of an individual executive *i* in the year *t*, a variable commonly used in the executive compensation literature (for example, Bertrand and Mullainathan, 2001; Bebchuk and Grinstein, 2005). For accounting reasons, firms may report equity-based pay after the grant year, potentially distorting our pay measure. To address this, some specifications use total compensation minus equity-based pay.

Columns 1, 3, 4 and 6 of Table 2 present regression results for the full sample. Columns 2 and 5 of Table 2 present results for a more homogenous subsample of the two highest paid executives in each firm and year. In Columns 1, 2, 4, and 5, the dependent variable is the natural logarithm of total compensation. In Columns 3 and 6, the dependent variable is the natural logarithm of non-equity compensation. In Columns 1, 2 and 3, we control only for executive- and year fixed effects. Columns 4, 5 and 6 include commonly used additional controls for firm size and ROA. We also control for whether the firm employs the executive for less than a full year or only part-time (*Partial Employment*).[21] We cluster standard errors by firm and year.

The coefficients of the interaction term *Controller Executive*Post Reform* in Table 2 indicate the existence of a negative effect of the reform on the compensation of the treated group — controller executives. Although absolute compensation levels for executives in aggregate do not materially change during the sample period, the effect of the post-reform period on controller-executive pay levels implies that controller executives earn 7%–13% less in 2011–2015 than they would have earned absent the reform.[22] These numbers are highly statistically significant. Importantly, this is an *average* effect. Many controller

[21] The inclusion of executive fixed effects requires that we use only time-varying controls. The variable Partial Employment equals one in a year in which an executive works less than 12 months or less than full time. This variable, which equals one in about 28% of the observations, varies over time for some executives and thus can be included in the regressions. In robustness regressions reported below, we exclude executives working part time and obtain similar results. While our sample does not contain other executive-specific variables (such as age or education), the individual fixed effects largely capture their effects.

[22] The results in Column 4 are unchanged when the post-reform dummy, which takes the value one starting in 2011 for all executives, is replaced by an individual post-reform dummy, which takes the value one after the MoM deadline of each controller executive. The results remain qualitatively unchanged also when we use other measures of firm performance, such as market-to-book ratio or lagged ROA.

executives (including the ones named in the Introduction) saw their pay fall by substantial amounts, with some experiencing pay cuts of over 50%. Conditional on pay reduction, 25% of controller executives saw their pay fall by at least 33%.

Table 3 presents an alternative regression specification, in which the dependent variable is the ratio of each executive's pay to the aggregate executive pay that the firm reported. Following Bebchuk et al. (2011), we refer to this measure of relative pay as the "pay slice". In line with the results in Table 2, Table 3 shows that the pay slice of controller executives (averaging about 26% in the years 2009–2010) declines by about one percentage point in the years 2011–2015. This decline is statistically significant in the full sample. It is similar in magnitude but not statistically significant in a subsample of the two highest paid executives.

3.2. The reform's effect on the likelihood of pay reductions

We study the mechanism by which the reform may have affected the pay of controller executives by examining the likelihood of pay reductions. Table 4 presents several regression specifications in which the dependent variable indicates whether total compensation or non-equity compensation is lower than in the preceding year. We use logit and linear probability models with and without executive fixed effects for the full sample and for a subsample of the two highest paid executives. The results are consistent across the various specifications.

In general, approvals of pay packages are associated with compensation increases across executives: the coefficient of *Any Approval* is negative and statistically significant (that is, negatively correlated with the probability of a compensation reduction). This is not surprising. First, over half of all approvals in our sample involve pay packages of non-controller executives. These pay packages are not subject to minority approval and thus depend solely on the controller. A controller or a board carrying out a controller's will and wishing to retain an executive is more likely to raise pay than to cut it. Second, many of the remaining approvals are pre-reform ToM approvals of controller executives' pay packages. A controller is likely to seek these approvals only when planning to raise the pay and expecting to obtain approval. Accordingly, the coefficient of *ToM Approval* is not statistically different from that of *Any Approval*.

MoM approvals are economically and statistically different from all other approvals in not being associated with compensation increases: the sum of the coefficients of *Any Approval* and *MoM Approval* is close to zero. This is because many MoM approvals are associated with compensation reductions. Specifically, of the 718 MoM approvals in our sample, 37% are associated with a reduction in total pay; the comparable figure for other approval types is only 15%. Similarly, 36% of MoM approvals are associated with a reduction in non-equity compensation, compared to 14% of other approvals. This is illustrated in Figure 1, which shows that MoM approvals are far more likely to result in reductions of non-equity pay than other approval types. The same holds for reductions in total compensation.

We seek to explore why the reform affected certain controller executives more than others. In some specifications, we observe a negative and statistically significant relation between ROA and the likelihood of a pay reduction. For example, in Column 1 of Table 4, negative profitability is associated with a higher likelihood of compensation reduction. In unreported regressions, we find an even stronger negative relation between ROA and compensation reduction of at least 25%.[23] However, unlike Fisch et al. (2018), we do not find that the effect of MoM approval on the likelihood of compensation reductions varies with firm performance or with excess pay, defined as the residual from a regression of compensation on firm size, industry and profitability. This suggests that minority shareholders use other indicators to determine whether a controller executive's pay is excessive.

We conclude that the requirement of MoM approval has real bite. Before their introduction in 2011, the alternative to seeking ToM approval for a raise was to keep a controller-executive's compensation unchanged. Starting in 2011, the option of continuing at the existing level of pay indefinitely is no longer available. The firm now has to seek MoM approval within three years of the previous approval, which can result in a pay cut if the minority perceives the controller executive as overpaid.

[23] The results in Table 4 remain qualitatively unchanged when we use other measures of firm performance, such as lagged ROA or market-to-book ratio. In unreported regressions, we also define dummy variables corresponding to ROA levels: one for negative ROA, another for positive-but-below-median ROA, and one for above-median ROA. We find that negative ROA and positive-but-below-median ROA are more correlated with pay reductions relative to the benchmark of above-median ROA.

3.3. The reform's effect on controller-executive disappearances

As discussed above, contemporaneous anecdotal accounts indicate that, after the reform, certain controller executives quit or remained in office with no pay when they were unable to obtain MoM approval. These effects do not show up in our measure of controller-executive pay, which is based on the reported pay of executives who continue to appear on the list of the firm's highest paid employees, causing any observed decline to understate the reform's true impact on controller-executive pay levels and firm governance generally.

To investigate this effect, we identify all executives whose pay is no longer reported by a firm that remains in the sample and continues to report the pay of other executives. These executives disappear from their firm's list of highest paid executives because they no longer hold a senior position in the firm or because they continue to hold the same or other position but at a low pay or no pay at all.

Consistent with contemporaneous media reports, we find that the reform sharply increased the disappearance rate for controller executives. We also find that this effect is correlated with failure to obtain MoM approval.

We begin by observing that controller executives are less likely to disappear than non-controller executives during the entire sample period. While controller executives constitute about 25% of our sample, they constitute only 13% of disappearances. However, the disappearance rate for controller executives increases significantly after the reform. Between 2009 and 2010, 7% of controller executives disappeared. In the post-reform period, the corresponding figures are 9% to 12%, an increase of 33% to 40%. There is no similar trend for non-controller executives. This is illustrated in Figure 2, where the 2009 to 2010 disappearance rate is normalized to 100.

In Columns 1 and 2 of Table 5, we corroborate this result by running logit regressions estimating the coefficients of several determinants of the probability of disappearance. We find that this probability increases for controller executives after the reform relative to that of non-controller executives. Moreover, Column 3 of Table 5 shows that the likelihood of disappearance increases after a MoM approval deadline. In particular, it is failure to meet the deadline that is correlated with disappearance: Column 4 indicates that the likelihood of disappearance falls after obtaining MoM approval.

We read corporate filings to examine if disappearing controller executives were replaced by hired professionals. Before the reform, this was virtually unheard of. After the reform, it occurred in about 10% of controller-executive disappearances (about 30 out of about 300).[24] This appears to be a real, if modest, effect of the reform on corporate management (we examine additional effects in Section 4). In addition, after the reform, about 50 controller executives remained in office but were no longer on the list of top paid executives because they worked for little or no pay.[25]

In sum, the reform not only restrained the pay of controller executives whose pay continued to be reported, but also drove the pay of other controller executives below reported levels, often to zero. It also prompted a modest shift in the staffing of management positions, from controllers to hired professionals. Our estimates of the reform's effect on controller-executive pay therefore understate the reform's full effect on firms.[26]

4. Extensions and Robustness Tests

4.1. Results for sub-periods: first-round vs. second-round MoM approvals

The reform required controller executives to obtain MoM approval for their pay within three years of the last approval. The first approval deadline thus took place in the period 2011–2013, depending on the executive's last pay approval date, with over half of controller executives in office in 2011 having their initial MoM deadline in 2011. The second MoM approval deadline came three years later, in the period 2014–2016.

[24] The figures are approximate because the fate of some disappearing controller executives is unclear. It appears that 33 controller executives ceased to hold their positions not as a result of control changes and the person who replaced them was not related to the controller.

[25] Here too the figures are approximate. Other reasons for controller-executive disappearance include replacement by different controller executives, control changes, and court ordered receivership.

[26] Ra and Kim (2018) discuss a related phenomenon in Korea, where a mandatory pay disclosure rule applies to board members whose compensation levels exceed a certain level. They report that board members attempt to evade the rules and disappear through pay cuts or by relinquishing their formal positions.

To examine the long-term effects of the reform, we distinguish in Table 6 between the first round of MoM approvals, in 2011–2013 (about 60% of the sample), and the second round of MoM approvals, in 2014–2015 (about 40% of the sample). We find that second-round MoM approvals are less likely to be associated with compensation reductions than first-round MoM approvals.[27]

One interpretation of the lower likelihood of pay reductions in second-round MoM approvals is that the first round of MoM approvals adjusted the pay of controller executives as much as minority shareholders wanted, allowing firms to revert to normal raises at the time of second-round MoM approvals. Another interpretation is that firms had more time to prepare for the second round of MoM approvals and chose opportune moments to hold them.

4.2. Early vs. on-time MoM approvals

To better understand the mechanism driving the results in Table 4, we run similar regression specifications while distinguishing between MoM approvals obtained at a date before the calendar year of the deadline stipulated by law ("early MoM approvals") and MoM approvals obtained in the calendar year of the legal deadline or later ("non-early MoM approvals").[28] We examine if controller executives facing an approval deadline (perhaps because they did not expect to win approval earlier in the cycle) are more likely to take a pay cut than controller executives who seek approval earlier in the cycle.

Table 7 presents the results. Non-early MoM approvals tend to have larger and more significant coefficients, suggesting that compensation reductions are somewhat more likely to follow non-early MoM approvals obtained closer to the deadline stipulated by law, than to follow early MoM approvals. However, the differences between the coefficients of early- and non-early MoM approvals are small and not statistically significant, preventing us from drawing firm conclusions from this distinction.

[27] While the difference between the coefficient of first-round approvals and the coefficient of second-round approvals is not statistically significant in any regression, in all regressions the coefficient of first-round approvals is larger and more statistically significant.

[28] Our results do not materially change if we classify MoM approvals as early if obtained more than a certain number of days (for example, 180) before they were due and as non-early otherwise.

4.3. Results for subsamples

In Table 8, we repeat the benchmark regression specification from Column 4 of Table 2 for several subsamples.

In Column 1, we exclude firms without controller executives on the list of highest paid executives. These firms help us to estimate the effects of the control variables more precisely, but do not contribute to the estimation of the post-reform change in controller-executive pay. These firms may also be different from firms with controller executives on the list of highest paid executives. The results are similar to those in Column 4 of Table 2, except that the effect of profitability on compensation is larger and more statistically significant.

In Column 2, we include only firms with a controller executive whose MoM approval deadline is in 2011 to estimate the effect of the reform on controller executives that could not plan for it. In this, much smaller subsample, the effect of the reform on the compensation of controller executives is much larger than in the full sample: about –18% versus about –10% in Column 4 of Table 2.[29]

In Column 3, we exclude executives who disappear from the sample before its last year. In Column 4, we also exclude executives who appear in the sample only after its initial year. In Column 5, we exclude executives employed part-time. In these three specifications, the effect of the post-reform years on the compensation of controller executives remains. The effect in Column 3 is smaller and less significant than in the full sample, and the effect in Columns 4 and 5 is of similar magnitude and significance to the effect in the full sample.

In unreported regressions, we exclude parents and their subsidiaries from the sample. The reason is that compensation figures in the reports of parents include compensation paid by subsidiaries, even if the subsidiaries are public and report it in their own filings. This can distort our analysis of the relation between compensation, approvals, and performance. The results of Table 2 remain unchanged in this subsample.

[29] It is also possible to run the pay slice regressions of Table 3 for this subsample. The results are similar to those reported in Table 3.

4.3. Additional effects of the reform on firm behavior?

We explore possible effects of the reform on other aspects of corporate behavior.

First, we examine if other types of related-party transactions replaced executive pay as a way for controllers to extract rent. Such a shift is unlikely because the reform subjected all related party transactions not in the ordinary course of business to the same MoM approval requirement as executive pay. It is therefore not surprising that the value of nonrecurring related-party transactions normalized by firm assets declines for the 109 firms in our sample with available data (the non-financial firms among the largest 150 firms on Tel Aviv Stock Exchange) from an average of 1.2% before the reform to 0.6% in the post-reform years. In percentage terms, the decline is similar in firms where a controller executive in the firm experienced a pay decrease post-reform. In our sample, controller-executive pay and other related party transactions thus appear to be complements, rather than substitutes.

Second, we examine if an increase in dividend payout replaces executive pay as a way of controllers to benefit from the firm. Such a shift is also unlikely because, contrary to executive compensation, dividends are paid to all shareholders and are thus costly to controllers. Accordingly, in unreported regressions we find that the ratio of dividends to profits (or other measures of dividends normalized by firm size) remains roughly constant post-reform and that firms with controller-executive pay reductions are not different from other firms.

Finally, we also do not detect significant changes in Tobin's Q following the reform or following compensation reductions. This is perhaps not surprising given the modest magnitude of compensation reductions and the fact that the managerial incentives of controller executives are tied primarily to their shareholdings.

4. Conclusion

To better protect minority shareholders from tunneling by controllers, a number of jurisdictions have introduced reforms designed to give the minority veto rights over related party transactions. We test the effect of this right by exploiting a 2011 Israeli reform that gave minority shareholders, in midstream, the ability to veto the pay of controller executives.

We find that this veto right constrains the pay of controller executives, in part by increasing the frequency of pay reductions. The threat of minority veto also induces some controller executives to relinquish their positions or continue to work without pay. Following the reform, there is a 40% increase in the rate at which controller executives disappear from their firm's list of highest paid executives. The estimated effect of the reform on controller-executive pay thus understates its actual effect.

Our work contributes to a better understanding of controller-pay tunneling. We find that the grant of a minority veto constrains controller-executive pay levels, leads to more pay reductions and to the disappearance of certain controller executives from the firm's list of highest paid executives, suggesting that pre-reform minority shareholders considered some controller executives to be overpaid.

This paper also provides a unique setting for identifying the power of minority veto rights for policing related party transactions more generally. Unlike other settings, where the controller chooses whether to give the minority a veto right (as in Delaware) or the veto right is mandatory but the controller can choose whether and when to propose a transaction (as in Canada), our setting raises minimal endogeneity concerns because the Israeli reform makes the veto right mandatory and sets forth an exogenous deadline for obtaining minority approval.

Our work also contributes to the literature on SoP by showing that a mandatory vote can have an effect both on the level of executive pay and on whether executives remain in their jobs. This may be relevant to policymakers in many jurisdictions given the ubiquity of shareholder voting schemes around the world.

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Figure 1: Percent of Approvals Ending in Non-Equity Pay Reduction by Approval Type

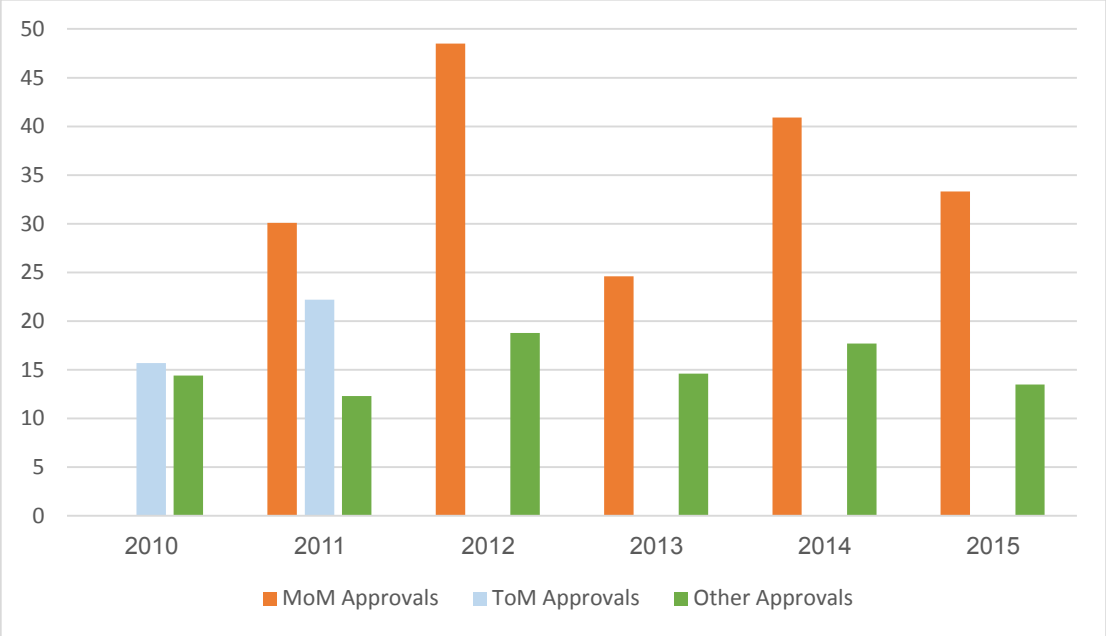

Figure 2: Disappearance Rates of Controller Executives and Non-Controller Executives

A disappearing executive is defined as an executive who no longer appears on the firm's list of highest paid executives after the current year. The disappearance rate is the likelihood that a particular type of executive (controller or non-controller) will disappear, with the 2009 disappearance rate (corresponding to executives who last appear in 2009) normalized to 100.

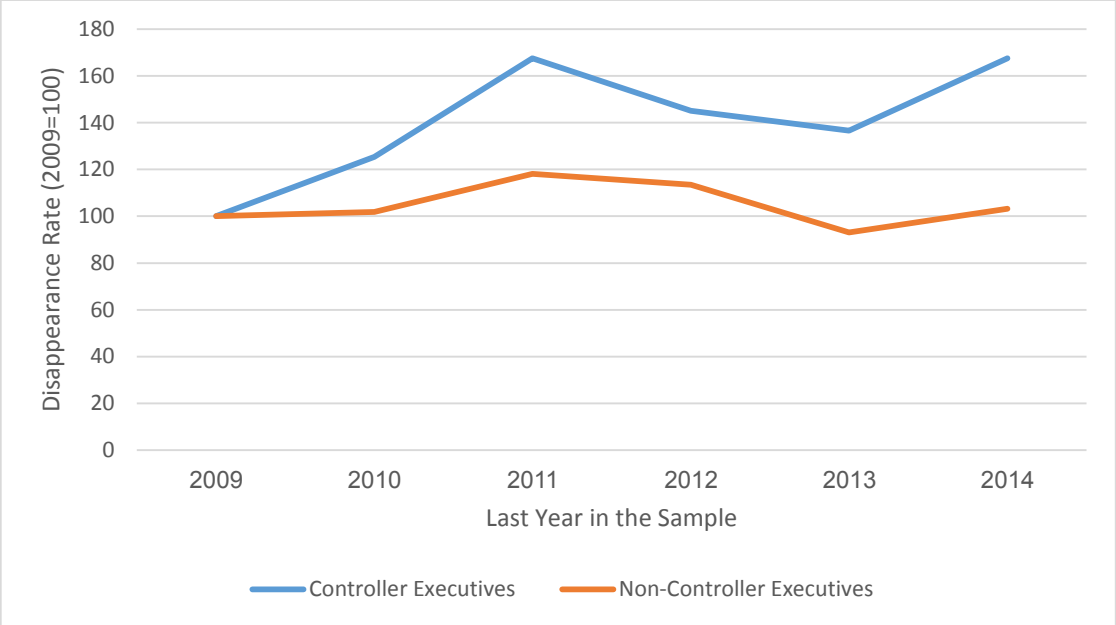

Table 1, Panel A: Firm-Level Data

The sample consists of an unbalanced panel of 4,507 executives from 591 Israeli public firms in the years 2009–2015. Panel A reports annual firm-level data. Firm-level financial variables are from commercial provider A-Online. Other data come directly from firms' annual reports. All monetary values are in New Israeli Shekels (about 4 NIS per 1 USD).

Definition	Units	Mean	Std.	25%	50%	75%	Firms
Total Assets	Millions of NIS	3,341	13,200	106	349	1,160	591
ROA	Annual operating profits to assets, in percent	–0.2	16.6	–1.4	2.4	6.5	591
Equity Held by Individual Controller Executives	In percent	23	25	0	16	39	590
Number of Reported Executives		5	2	5	5	6	591
Number of Reported Controller Executives		1.35	1.35	0	1	2	591

Table 1, Panel B: Executive-Level Pay Data

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. Panel B reports the annual compensation of each executive based on annual reports. All financial values are in NIS (about 4 NIS per 1 USD).

Definition	Units	Mean	Std.	25%	50%	75%	Obs.
Total Compensation (reported value of all compensation components)	Thousands of NIS	1,333	3,323	427	808	1,448	13,576
Total Compensation of Controller Executives	Thousands of NIS	1,540	2,378	505	989	1,774	3,429
Non-Equity Compensation (Total Compensation excluding equity-based components)	Thousands of NIS	1,185	3,126	410	776	1,354	13,576
Non-Equity Compensation of Controller Executives	Thousands of NIS	1,460	2,143	491	967	1,731	3,429
Partial Employment	Equals one if an executive is employed for less than a full year or less than full-time	28.7%					13,576

Table 1, Panel C: Pay Approvals

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. Panel C reports, for each of those years, pay approvals for controller and non-controller executives in our sample. All variables are based on annual reports and proxy statements.

Approval Type	Definition	2009	2010	2011	2012	2013	2014	2015	Total
MoM Approval	Majority of the minority approval of a controller executive's pay (after mid-2011)	N/A	N/A	183	130	114	191	65	718
ToM Approval	Third of the minority approval of a controller executive's pay (before mid-2011)	70	108	27	N/A	N/A	N/A	N/A	205
Other Approvals	Various approvals by the board or shareholders of the pay of non-controller executives (all years)	206	341	276	250	240	186	155	1654

Table 1, Panel D: Executive Disappearances by Last Year of Appearance

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. Panel D reports for each of those years the number (percent) of disappearing executives (executives whose pay is no longer reported after the current year by a firm that continues to report the pay of other executives). All variables are based on annual reports and proxy statements.

Definition	2009	2010	2011	2012	2013	2014	2015	Total
Number of controller-executives (percent of all controller-executives) whose pay is not reported in the following year	33 (7.1)	54 (8.9)	64 (11.9)	50 (10.3)	46 (9.7)	52 (11.9)	N/A	299 (8.7)
Number of non-controller executives (percent of all non-controller executives) whose pay is not reported in the following year	306 (21.5)	369 (21.9)	405 (25.4)	352 (24.4)	266 (20.0)	296 (22.2)	N/A	1,994 (19.6)

Table 2: Pay Before and After the Reform

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. In Columns 1, 2, 4, and 5, the dependent variable is the natural logarithm of (annual) total compensation. In Columns 3 and 6, the dependent variable is the natural logarithm of non-equity compensation. Executive and year fixed effects are included in all specifications. Columns 1, 3, 4 and 6 report results for the full sample and Columns 2 and 5 report results for a subsample of the two highest paid executives in each firm and year. *Controller Executive*Post Reform* equals one for controller executives starting in 2011 and zero otherwise. *Partial Employment* equals one for executives employed less than a full year or less than full time and zero otherwise. The coefficient of ROA is multiplied by 100. All control variables are measured in the same year as the dependent variable. Robust standard errors, clustered by firm and year, are in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Controller Executive*Post Reform	–0.12** (0.04)	–0.10*** (0.04)	–0.13*** (0.03)	–0.10*** (0.03)	–0.07** (0.03)	–0.11*** (0.02)
Partial Employment				–0.36*** (0.03)	–0.19*** (0.05)	–0.35*** (0.02)
Log (Total Assets, in thousands of NIS)				0.20*** (0.02)	0.19*** (0.03)	0.19*** (0.01)
ROA				0.02 (0.12)	0.28** (0.12)	0.14*** (0.05)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Executive Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,576	5,198	13,530	13,576	5,198	13,530
R-Squared	0.88	0.91	0.87	0.89	0.91	0.89

Table 3: Executive's Pay Slice Before and After the Reform

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. The dependent variable is the pay slice, defined as the ratio of an executive's total compensation to the total compensation of all executives reported by the firm in the same year. Executive and year fixed effects are included in all specifications. Columns 1 and 3 report results for the full sample excluding the lowest paid executive reported in each firm and year to ensure the pay slices do not add up to one. Columns 2 and 4 report results for a subsample of the two highest paid executives in each firm and year. *Controller Executive*Post Reform* equals one for controller executives starting in 2011 and zero otherwise. *Partial Employment* equals one for executives employed less than a full year or less than full time and zero otherwise. The coefficient of ROA is multiplied by 100. All control variables are measured in the same year as the dependent variable. Robust standard errors, clustered by firm and year, are in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively.

	(1)	(2)	(3)	(4)
Controller Executive*Post Reform	–0.010*	–0.011	–0.013**	–0.013
	(0.006)	(0.009)	(0.006)	(0.009)
Partial Employment			–0.027***	–0.010
			(0.006)	(0.009)
Log (Total Assets, in thousands of NIS)			–0.026***	–0.021***
			(0.003)	(0.005)
ROA			0.022*	0.010
			(0.013)	(0.022)
Year Fixed Effects	Yes	Yes	Yes	Yes
Executive Fixed Effects	Yes	Yes	Yes	Yes
Observations	11,025	5,198	11,025	5,198
R-Squared	0.79	0.82	0.80	0.82

Table 4: The Determinants of Pay Reduction

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. Using a truncated sample starting in 2010 (compensation changes relative to 2009) this table presents logit and linear probability regressions where the dependent variable is compensation reduction. Column 1 presents a logit regression with a dummy for controller executives and no executive fixed effects. The dependent variable is a dummy that equals one if total compensation declines relative to the previous year and zero otherwise. Column 2 presents results of a linear probability regression with executive fixed effects (and hence no dummy for controller executives) for the same dependent variable. Columns 3 and 4 present similar specifications for a subsample of the two highest paid executives. Columns 5 and 6 present similar specifications using reduction in non-equity compensation as the dependent variable. *Any Approval* is a dummy variable that equals one if there was a pay approval in the year, and zero otherwise. *MoM Approval* and *ToM Approval* are similarly defined dummy variables that equal one if a MoM approval or a ToM approval, respectively, took place in the current year and zero otherwise. *Log (Total Assets)* is the natural logarithm of total assets in NIS. The coefficient of *ROA* is multiplied by 100. *Negative ROA* and *Partial Employment* are dummy variables denoting negative ROA and partial employment (less than a full-year or less than full-time). All control variables are measured in the same year as the dependent variable. Robust standard errors, clustered by executive in the logit regressions and by firm and year in the LPM regressions, are in parentheses. ***, ** and * denote statistical significance at 1%, 5% and 10% respectively.

	(1) Logit	(2) LPM	(3) Logit	(4) LPM	(5) Logit	(6) LPM
Any Approval	–0.71*** (0.07)	–0.14*** (0.02)	–0.56*** (0.11)	–0.15*** (0.03)	–0.68*** (0.08)	–0.11*** (0.02)
MoM Approval	0.50*** (0.12)	0.09** (0.04)	0.41** (0.16)	0.10** (0.05)	0.50*** (0.12)	0.07* (0.04)
ToM Approval	0.09 (0.26)	0.06 (0.05)	0.17 (0.33)	0.08 (0.08)	0.07 (0.26)	0.05 (0.05)
Log (Total Assets in Thousands of NIS)	0.04*** (0.01)	–0.00 (0.01)	0.03 (0.02)	–0.01 (0.02)	0.01 (0.01)	–0.01 (0.01)
ROA	–0.22 (0.15)	0.00 (0.08)	–0.21 (0.24)	–0.16 (0.13)	0.09 (0.16)	–0.06 (0.08)
Negative ROA	0.11* (0.06)	0.03 (0.02)	0.07 (0.09)	0.04 (0.04)	0.03 (0.06)	0.02 (0.02)
Partial Employment	0.08* (0.05)	0.04** (0.02)	–0.09 (0.08)	0.05 (0.04)	0.07 (0.05)	0.06** (0.02)
Controller Executive	0.38*** (0.05)	N/A	0.21*** (0.08)	N/A	0.44*** (0.05)	N/A
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Executive Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	11,681	11,681	4,418	4,418	11,681	11,681
R-Squared	N/A	0.36	N/A	0.41	N/A	0.35

Table 5: Executive Disappearance Before and After the Reform

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. Using a truncated sample ending in 2014, this table presents the results of logit regressions in which the dependent variable is a dummy that equals one if the firm reports the pay of an executive in a given year but not thereafter while the firm continues to report the pay of other executives, and zero otherwise. *Controller Executive*Post Reform* equals one for controller executives starting in 2011 and zero otherwise. *MoM Approval Due* equals one if there is a MoM approval deadline in the current year, and zero otherwise. *MoM Approval* equals one if a MoM approval occurs in the year and zero otherwise. *Partial Employment* equals one for executives employed less than a full year or full time, and zero otherwise. The coefficient of ROA is multiplied by 100. All control variables are measured in the same year as the dependent variable. Robust standard errors, clustered by executive, are in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% respectively.

	(1)	(2)	(3)	(4)
Controller Executive	–1.13***	–1.16***	–1.66***	–0.90***
	(0.12)	(0.12)	(0.10)	(0.07)
Controller Executive *Post Reform	0.23*	0.22		
	(0.14)	(0.14)		
MoM Approval Due			0.71***	
			(0.16)	
MoM Approval Obtained				–0.60***
				(0.18)
Partial Employment		0.62***	0.62***	0.62***
		(0.05)	(0.06)	(0.06)
Log (Total Assets in thousands of NIS)		0.03*	0.03*	0.03*
		(0.01)	(0.01)	(0.01)
ROA		–0.97***	–0.91***	–0.97***
		(0.14)	(0.15)	(0.14)
Year Fixed Effects	Yes	Yes	Yes	Yes
Executive Fixed Effects	No	No	No	No
Observations	11,855	11,855	11,476	11,855

Table 6: Pay Reductions by MoM Approval Round

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. Using a truncated sample starting in 2010 (compensation changes relative to 2009), this table presents linear probability regressions with executive fixed effects where the dependent variable is a dummy that equals one if compensation declines relative to the previous year, and zero otherwise. Column 1 presents results for the full (truncated) sample. Column 2 presents results for a subsample of the two highest paid executives. Column 3 presents full-sample results of a regression in which the dependent variable is a dummy that equals one if non-equity pay declines relative to the previous year, and zero otherwise. *MoM Approval*2011–2013* and *MoM Approval*2014–2015* equal one if a MoM approval occurs in the year and the year is in 2011–2013 or 2014–2015, respectively, and zero otherwise. *Any Approval* is a dummy variable that equals one if there is any pay approval for the executive in the year, and zero otherwise. *ToM Approval* is a similarly defined dummy variable that equals one if a ToM approval occurs in the year, and zero otherwise. *Log (Total Assets)* is the natural logarithm of total assets in NIS. The coefficient of *ROA* is multiplied by 100. *Negative ROA* and *Partial Employment* are dummy variables denoting negative ROA and partial employment (less than full-time or less than a full year). All control variables are measured in the same year as the dependent variable. Robust standard errors, clustered by firm and year, are in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% respectively.

	(1) LPM	(2) LPM	(3) LPM
Any Approval	–0.14*** (0.02)	–0.15*** (0.03)	–0.11*** (0.02)
MoM Approval*2011–2013	0.10** (0.04)	0.13** (0.06)	0.08** (0.04)
MoM Approval*2014–2015	0.07 (0.05)	0.05 (0.07)	0.05 (0.05)
ToM Approval	0.06 (0.05)	0.07 (0.08)	0.05 (0.05)
Log (Total Assets in thousands of NIS)	–0.00 (0.01)	–0.01 (0.02)	–0.00 (0.01)
ROA	0.00 (0.08)	–0.16 (0.13)	–0.06 (0.08)
Negative ROA	0.03 (0.02)	0.04 (0.04)	0.02 (0.02)
Partial Employment	0.05** (0.02)	0.05 (0.04)	0.06** (0.02)
Year Fixed Effects	Yes	Yes	Yes
Executive Fixed Effects	Yes	Yes	Yes
Observations	11,681	4,418	11,681
R-Squared	0.36	0.41	0.35

Table 7: Pay Reductions by MoM Approval Timing

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. Using a truncated sample starting in 2010 (compensation changes relative to 2009), this Table presents regression specifications similar to those of Table 4 except that MoM approvals are divided into *Non-Early MoM Approval* (a dummy variable that equals one for MoM approvals in the calendar year of the legal deadline or later, and zero otherwise) and *Early MoM Approval* (a dummy variable that equals one for MoM approvals before the calendar year of the legal deadline, and zero otherwise). Column 1 presents results of a logit regression with a dummy for controller executives but no executive fixed effects. The dependent variable is a dummy that equals one if total compensation declines relative to the previous year, and zero otherwise. Column 2 presents results of a linear probability regression with executive fixed effects (and hence no dummy for controller executives) for the same dependent variable. Columns 3 and 4 present similar specifications for a subsample of the two highest paid executives. Columns 5 and 6 present similar specifications for the full sample using the reduction in non-equity pay as the dependent variable. *Any Approval* is a dummy variable that equals one if there is any pay approval for the executive in the year, and zero otherwise. *ToM Approval* is a dummy variable that equals one if a ToM approval occurs in the current year, and zero otherwise. *Log (Total Assets)* is the natural logarithm of total assets in NIS. The coefficient of *ROA* is multiplied by 100. *Negative ROA* and *Partial Employment* are dummy variables denoting negative ROA and partial employment (less than full-year or full-time). All control variables are measured in the same year as the dependent variable. Robust standard errors, clustered by executive in the logit regressions and by firm and year in the LPM regressions, are in parentheses. ***, ** and * denote statistical significance at 1%, 5% and 10% respectively.

	(1) Logit	(2) LPM	(3) Logit	(4) LPM	(5) Logit	(6) LPM
Any Approval	–0.73*** (0.07)	–0.13*** (0.02)	–0.60*** (0.10)	–0.15*** (0.03)	–0.71*** (0.07)	–0.11*** (0.02)
Non-Early MoM Approval	0.72*** (0.14)	0.10** (0.04)	0.61*** (0.18)	0.13** (0.06)	0.73*** (0.13)	0.08* (0.04)
Early MoM Approval	0.63*** (0.17)	0.08 (0.06)	0.63*** (0.23)	0.09 (0.08)	0.70*** (0.17)	0.08 (0.06)
ToM Approval	0.11 (0.25)	0.06 (0.05)	0.22 (0.33)	0.08 (0.08)	0.10 (0.26)	0.05 (0.05)
Log (Total Assets in Thousands of NIS)	0.04*** (0.01)	–0.00 (0.01)	0.03 (0.02)	–0.01 (0.02)	0.01 (0.01)	–0.01 (0.01)
ROA	–0.22 (0.15)	0.00 (0.08)	–0.23 (0.24)	–0.15 (0.13)	0.09 (0.16)	-0.06 (0.08)
Negative ROA	0.11* (0.06)	0.03 (0.02)	0.07 (0.09)	0.04 (0.04)	0.04 (0.06)	0.02 (0.02)
Partial Employment	0.08* (0.05)	0.05** (0.02)	–0.08 (0.08)	0.05 (0.04)	0.08* (0.05)	0.06** (0.02)
Controller Executive	0.37*** (0.05)	N/A	0.19*** (0.08)	N/A	0.44*** (0.05)	N/A
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Executive Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	11,681	11,681	4,418	4,418	11,681	11,681
R-Squared	N/A	0.36	N/A	0.41	N/A	0.35

Table 8: Pay Before and After the Reform by Subsamples

This table repeats the benchmark regressions from Column 4 of Table 2 for several subsamples. In all regressions, the dependent variable is the natural logarithm of total compensation. In Column 1, we include only firms with at least one controller executive on the list of highest paid executives. In Column 2, we include only firms with controller executives whose MoM approval deadline is in 2011. In Column 3, we include only executives who remain in office until the end of the sample period. In Column 4, we include only executives who are in office for the entire sample period. In Column 5, we exclude executives with *Partial Employment*. *Controller Executive*Post Reform* equals one for controller executives starting in 2011, and zero otherwise. *Partial Employment* equals one for executives employed less than a full year or less than full time, and zero otherwise. The coefficient of ROA is multiplied by 100. All control variables are measured in the same year as the dependent variable. Robust standard errors, clustered by firm and year, are in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels respectively.

	(1)	(2)	(3)	(4)	(5)
Controller Executive*Post Reform	–0.12***	–0.18***	–0.06*	–0.11***	–0.12***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Partial Employment	–0.36***	–0.24***	–0.36***	–0.21***	N/A
	(0.04)	(0.05)	(0.03)	(0.06)	
Log (Total Assets in Thousands of NIS)	0.24***	0.01	0.20***	0.20***	0.11***
	(0.03)	(0.09)	(0.02)	(0.03)	(0.02)
ROA	0.35**	0.60	0.02	0.27*	0.11
	(0.17)	(0.44)	(0.12)	(0.15)	(0.12)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Executive Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	9,174	2,970	13,278	3,655	9,680
R-Squared	0.89	0.89	0.89	0.87	0.88