DISCUSSION PAPER SERIES

DP12697

THE EFFECT OF MINORITY VETO RIGHTS ON CONTROLLER TUNNELING

Jesse Fried, Ehud Kamar and Yishay Yafeh

FINANCIAL ECONOMICS

ISSN 0265-8003

THE EFFECT OF MINORITY VETO RIGHTS ON CONTROLLER TUNNELING

Jesse Fried, Ehud Kamar and Yishay Yafeh

Discussion Paper DP12697
Published 07 February 2018
Submitted 07 February 2018

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

This Discussion Paper is issued under the auspices of the Centre's research programme in **FINANCIAL ECONOMICS**. Any opinions expressed here are those of the author(s) and not those of the Centre for Economic Policy Research. Research disseminated by CEPR may include views on policy, but the Centre itself takes no institutional policy positions.

The Centre for Economic Policy Research was established in 1983 as an educational charity, to promote independent analysis and public discussion of open economies and the relations among them. It is pluralist and non-partisan, bringing economic research to bear on the analysis of medium- and long-run policy questions.

These Discussion Papers often represent preliminary or incomplete work, circulated to encourage discussion and comment. Citation and use of such a paper should take account of its provisional character.

Copyright: Jesse Fried, Ehud Kamar and Yishay Yafeh

THE EFFECT OF MINORITY VETO RIGHTS ON CONTROLLER TUNNELING

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 help curb controller tunneling.

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

Keywords: controlling shareholders, tunneling, corporate governance, minority shareholders, shareholder voting, veto rights, securities regulation, corporate law, Executive compensation, related party transactions

Jesse Fried - jfried@law.harvard.edu
Harvard Law School

Ehud Kamar - kamar@post.tau.ac.il
Tel Aviv University Faculty of Law

Yishay Yafeh - msyafeh@mscc.huji.ac.il
Hebrew University of Jerusalem and CEPR

The Effect of Minority Veto Rights on Controller Tunneling

Jesse M. Fried, Ehud Kamar, and Yishay Yafeh*

February 2018

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 help curb controller tunneling.

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

Key words: controlling shareholders, tunneling, corporate governance, minority shareholders, shareholder voting, veto rights, securities regulation, corporate law, executive compensation, related party transactions

* Fried is at Harvard Law School and ECGI (jfried@law.harvard.edu). Kamar is at the Tel Aviv University Buchmann Faculty of Law and ECGI (kamar@tau.ac.il). Yafeh is at the Hebrew University School of Business Administration, CEPR, and ECGI (yishay.yafeh@huji.ac.il). This project received support from the Binational Science Foundation (Grant No. 2012/071) and the Raymond Ackerman Family Chair in Israeli Corporate Governance. Yafeh acknowledges financial support from the Krueger Center at the Jerusalem School of Business Administration. We thank Benjamin Alarie, Oded Cohen, Jean-Gabriel Cousin, Luca Enriques, Zohar Goshen, Yaniv Grinstein, Assaf Hamdani, Sharon Hannes, Beni Lauterbach, Amir Licht, Anthony Niblett, Edward Rock, Marco Ventoruzzo, and seminar participants at Bar Ilan University, Bocconi University, the Hebrew University, New York University–Tel Aviv University Corporate Law Conference, Tel Aviv University, the University of Lille, the University of Siena–University of Toronto–Tel Aviv University Law and Economics Workshop, and the University of Vienna for helpful comments and suggestions. We are also grateful to Shai Ashri, Peleg Davidovitz, Dana Gorfine, David Harar, Nufar Kotler, Michal Lavi, Oriya Peretz, Itamar Rahabi, Barak Steinmetz, Hadas Studnik, Tomer Yafeh, and Nadav Yafit for outstanding research assistance.

1. Introduction

Most publicly traded firms around the world have a controlling shareholder (Claessens, Djankov and Lang, 2000; Faccio and Lang, 2002; Khanna and Yafeh, 2007; Holderness, 2009; Gutierrez and Saez, 2017). 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, Indonesia, and Mexico.[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 deferential judicial review.[4] Similarly, in the United Kingdom, controlled companies with premium listing on the

[1] Canadian Securities Administrators (2017).

[2] OECD (2012).

[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.

[4] Kahn v. M&F Worldwide Corp., 88 A.3d 635 (Del. 2014); In re Martha Stewart Living Omnimedia, Inc. Shareholders Litigation, C.A. No. 11202–VCS (Del. Ch. Aug. 18, 2017).

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 controller executives could continue to draw the same amount of pay even if the minority came to believe that this amount had become excessive. The 2011 reform did not alter the approval mechanisms for the pay of executives unrelated to controllers ("non-controller executives"), creating a viable control group.

Contemporaneous anecdotal accounts suggest that the 2011 reform had real bite. For example, Rami Levy, the controlling shareholder and CEO of an eponymous supermarket chain, had to cut his bonus in half to secure minority support for his pay

[5] Davies (2017); 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.

contract (Calcalist, October 16, 2011). According to our calculations, his post-approval package was 26% lower than the previous one. Ilan Ben Dov, the controlling shareholder 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 2011 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—an economically and statistically significant reduction. We also find that this decline is partly driven by a substantial increase in the frequency of pay cuts for controller executives. Minority investors appear to be selective in wielding their veto power, forcing some controller executives (such as Ilan Ben Dov) to accept massive pay cuts, but not others.

Importantly, this average decline is likely to understate the effect of the 2011 reform for another reason: in many firms, the minority could not use their veto right over controller-executive pay immediately. In particular, because the veto right became available three years after the last pay approval, many MoM approvals did not take place until 2012 or 2013 (for pay contracts signed in 2009 and 2010). Thus, much of the controller-executive pay observed in the initial post-reform years reflects the lingering effect of arrangements that had been put in place before the reform, without MoM approval.

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 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. Our estimate of the effect of minority veto rights on controller-executive pay — and on firm govermance — is thus downward-biased.

Our study does not assess the overall desirability of the 2011 reform. While minority veto rights can help protect the minority from controller tunneling, they can also generate costs. First, information asymmetry or other factors can lead to the thwarting or delaying of value-increasing transactions (Enriques, 2015; Rock, 2017). Second, impeding controller tunneling through a particular type of related party transaction can drive controllers to use costlier forms of tunneling. Third, to the extent private benefits motivate controllers to generate value (Burkart et al., 1997; Gilson and Schwartz, 2015), curbing these benefits can be harmful. Thus, we do not know whether the 2011 reform increased the corporate pie or even made the minority better off. Our focus is solely on whether granting the minority veto rights over a particular transaction — like executive pay — makes it more difficult for controllers to extract value through that channel.

In this paper, we contribute to four strands of the corporate governance literature.

First, we contribute to the line of research investigating tunneling through pay. While pay is rarely the most lucrative channel for tunneling, as it tends to be both small relative to firm value and highly salient, controllers may well use pay arrangements — for themselves and for their relatives — to transfer additional value from the minority. Prior work has therefore sought to determine whether pay tunneling occurs, comparing the pay of controller executives to that of non-controller executives. This work has found pay tunneling in some jurisdictions — including Italy (Barontini and Bozzi, 2012), 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). The limitation of this approach, however, is that controller executives and non-controller executives may differ substantially, making comparison difficult.

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 2011 reform,

there was *some* constraint on pay tunneling, as a controller executive needed ToM approval to obtain a pay *increase*. However, after the 2011 reform, all controller-executive pay arrangements 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 controllers in our sample had extracted rents through pay before the reform — although it is possible that the minority used its veto rights to penalize certain controller-executives for reasons unconnected to tunneling.

Second, we contribute to the literature on the use of minority veto rights to constrain tunneling through related party transactions generally. Because the requirement of minority approval in related party transactions is relatively recent, almost all prior work relating to minority veto rights concerns controllers that 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.[7]

Our setting enables us to better isolate the effect of minority veto rights. First, it raises minimal endogeneity concerns, as the Israeli reform compels controller executives to obtain minority approval triennially to receive any pay, even if they do not seek a raise. Second, the availability of pay data for non-controller executives, combined with executive fixed effects, enables us to construct robust controls.

That said, a potential drawback of our setting is that pay may be qualitatively different from other kinds of related party transactions, making generalization difficult. Specifically, the potential 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 see that the firm is run well.

[7] Chen, Bin, and Yang (2013) examines the effect of a Chinese regulation requiring advance minority approval for stock issuances, transactions that often facilitate tunneling (Fried, 2017; Fried and Spamann, 2017), finding 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. The advantage of this study over those involving Delaware freezeouts is that, conditional on the controller proposing a certain type of transaction, minority approval is mandatory. However, as in the Delaware freezeout studies, the timing of the transaction proposal is endogenous, raising identification concerns. In addition, the China study does not include controls in the form of stock issuances unaffected by the reform.

By contrast, the potential cost to the minority of mistakently vetoing a desirable commercial or financial arrangement with the controller can be substantial. The minority may accordingly be more reluctant to veto other types of related party transactions, reducing the potential utility of minority veto rights in non-pay contexts.

Third, our findings contribute to the extensive literature on the effect of say-on-pay votes (SoP) on executive pay levels and structure. In the United Kingdom, non-binding 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, non-binding SoP have had little effect on pay (Ertimur et al., 2011; Brunarski et al., 2015; Cuñat et al., 2016; Iliev and Vitanova, 2017).[8] 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 non-binding SoP.[9] 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.

Fourth, our findings contribute to the literature on the effectiveness of voting by shareholders 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 also have veto rights over additional corporate actions like equity issuances (Yermack, 2010; Holderness, 2017), acquisitions (Kamar, 2006; Becht et al., 2016), equity compensation plans (Armstrong et al., 2013), or extraordinary transactions involving directors (Enriques et al., 2017).

The protection that these veto rights afford is difficult to measure because — outside the 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

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

[9] Thomas and Van der Elst (2015) describe SoP rules in various jurisdictions.

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 exogenously timed votes, permitting us to test whether shareholders use their voting power to constrain insiders.

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

2. Methodology and Data

Our analysis focuses on a 2011 Israeli reform of the regulation of public firms.[10] Prior to 2011, related party transactions — including pay packages of controller executives — had to receive approval by a third of the minority (ToM) once. That approval was then valid indefinitely. The 2011 reform raised the threshold to a majority of the minority (MoM) and, importantly, required new approval for the pay of controller executives within three years of the last approval. The 2011 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. Our sample, an unbalanced panel described in Panel A of Table 1, consists of 591 firms, of which 31% are in manufacturing, 27% are in services, 25% are in real estate, 15% are in oil and gas exploration, and 2% are in other industries.

Like Delaware law, Israeli law defines a shareholder (or a group of aligned 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.

[10] Hamdani and Yafeh (2013) describe the Israeli corporate governance landscape before the 2011 reform.

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).[11] 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 generally must report the compensation paid during the year to each 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. 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 ("reported executives"). For each firm, we obtain from annual reports and proxy statements the names, positions, compensation packages, and pay approvals of reported executives, typically including both controller executives and non-controller executives.[12]

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.

[11] As in Bertrand and Mullainathan (2001), in the presence of fixed effects, which we include in many specifications, levels of accounting returns reflect changes in profitability relative to the firm-specific mean over time. We also examine specifcations using lagged profitability, profitability combined with industry fixed effects, and market-to-book instead of ROA and obtain qualitatively similar results to the ones reported below.

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

While virtually all the 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, they comprise about a quarter of the observations.

The mean 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 of the controller executives are relatives of the controller, who may occupy less senior positions than some of the 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 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.[13]

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 occur in two rounds. The first round starts in 2011, when the new law becomes effective. The second round starts in 2014, when the MoM approvals obtained in 2011 expire. In part of our analysis, we distinguish between the two MoM approval rounds.

[13] 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.

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.[14] 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 empirical analysis consists of three parts. First, we use a difference-in-difference model to estimate the post-reform change in the compensation levels of controller executives. Second, we explore the mechanisms by which the reform may have affected controller-executive pay levels. In particular, we focus on the minority's use of their veto to force controller executives to take pay cuts. Third, we examine the extent to which the reform caused controller executives to disappear from the firm's list of highest paid executives.

3. Main Results

3.1 The Reform's Effect on Controller-Executive Pay Levels

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

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

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, and the dummy for a controller executive prior to the

[14] 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.

reform is subsumed by individual executive fixed effects.[15] 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.

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, 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 (for example, Bertrand and Mullainathan, 2001; Bebchuk and Grinstein, 2005). We also control for whether the firm employs the executive for less than a full year or only part-time (*Partial Employment*).[16] 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 a 12%–

[15] 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 firm. 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.

[16] 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 omit this variable 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.

13% decline in 2011–2015 according to the estimates in Columns 1 and 2, and a 7%–10% decline according to the estimates in Columns 3 and 4, controlling for other variables affecting pay. These numbers are highly statistically significant and economically important. One must also keep in mind that this is the *average* effect: many controller executives (including the individuals we mention in the introduction) saw their pay fall by substantial amounts, with some enduring pay cuts of over 50%. In fact, conditional on pay reduction, 25% of the 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.[17]

3.2 The Reform's Effect on the Likelihood of Pay Reductions

We study the mechanism by which the 2011 reform may have affected the pay of controller executives by examining the frequency 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. 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 generally 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

[17] Occasionally, compensation figures that parents report may include compensation that their subsidiaries pay and report in their own filings. The results reported in this section hold when parents and their subsidiaries are excluded from the sample.

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 after the reform. 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%. 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. Perhaps minority shareholders use a variety of other indicators to determine whether a controller executive's pay is excessive.[18]

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 comes to believe that the controller executive is overpaid.

[18] The results in Table 4 remain qualitatively unchanged when we use other measures of firm performance such as lagged ROA or "market-to-book". We also do not find any effect on the likelihood of compensation reduction for measures of "excessive pay" calculated relative to executives in the same industry.

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 chosen by the firm 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").[19] We conjecture that 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 5 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.

3.3 The Reform's Effect on Controller-Executive "Disappearances"

As discussed above, contemporaneous anecdotal accounts indicate that certain controller executives either quit or remained on the job with no pay at all when, following the 2011 reform, they were unable to obtain MoM approval. These effects do not show up in our measure of controller-executive pay, which is based on reported pay, causing any observed decline to understate the reform's effect 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. Although these executives disappear from their firm's list of highest paid executives, they may still be executives at the firm: we do not presently know whether a disappearing executive has left the firm or remains working at the firm with limited pay or no pay at all.[20]

Consistent with contemporaneous media reports, we find that the 2011 reform sharply increased the disappearance rate for controller executives. We also find that this effect is correlated with failure to obtain MoM approval.

---

[19] 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.

[20] We hope to include this information in the next draft of this study.

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 2011 reform. In the pre-reform period, between 7% and 9% of controller executives disappear each year. In the post-reform period, the corresponding figures are between 10% and 12%, an increase of about 40% over the pre-reform rate. There is no similar trend for non-controller executives. This is illustrated in Figure 2, where the pre-reform disappearance rate is normalized to 100.

In Columns 1 and 2 of Table 6, 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 6 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.

In sum, the 2011 reform not only restrained the pay of controller executives whose pay continued to be reported, but possibly also drove the pay of other controller executives down to zero. Thus, our estimates of the 2011 reform's effect on controller-executive pay understate the reform's full effect.

3.4 Results for Subperiods: First-Round vs. Second-Round MoM Approvals

The 2011 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.

To examine the long-term effects of the reform, we distinguish in Table 7 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.

One explanation for 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 possible interpretation is that firms had more time to prepare for the second round of MoM approvals and chose opportune moments to hold them. Consistent with this interpretation, the percentage of MoM approvals that are early is 23% in the first round and 32% in the second round.

3.5 Results for Subsamples

In Table 8, we repeat the benchmark regression specification from Column 4 of Table 2 while excluding observations that potentially distort the results. In Column 1, we exclude firms without controller 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. In Column 2, we exclude executives who disappear during the sample period to ensure that they do not affect our results. In Column 3, we exclude executives who are not full-time employees to verify that changes from or to partial employment do not drive our results. Our results hold in all of these specifications.

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 levels 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, the rate at which controller executives disappear from their firm's list of highest paid executives increases by about 40%. 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. Our finding that the grant of a minority veto can constrain controller-executive pay levels and lead to more pay reductions and to the disappearance of certain controller executives from the firm's list of highest paid executives suggest that, pre-reform, some controllers were seen (by minority shareholders) as paying themselves and their relatives too much.

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.

We hope our work is useful to researchers, regulators, and market participants seeking to improve corporate governance in both controlled and non-controlled firms. In particular, we hope our work can be a first step in tackling the important question of whether the imposition of minority veto rights, which Israel and several other jurisdictions have begun to employ, benefits the minority and increases firm value.

References

Armstrong, C., Gow, I., Larcker, D., 2013. The efficacy of shareholder voting: evidence from equity compensation plans. Journal of Accounting Research 51, 909–950.

Barak, R., Cohen, S., Lauterbach, B., 2011. The effect of CEO pay on firm valuation in closely held firms. Advances in Financial Economics: International Corporate Governance 14, 19–42.

Barontini, R., Bozzi, S., 2012. CEO compensation and performance in family firms. Unpublished working paper, Scuola Superiore Sant'Anna, Pisa.

Bebchuk, L., Cremers, K., Peyer, U., 2011. The CEO pay slice. Journal of Financial Economics 102, 199–221.

Bebchuk, L., Grinstein, Y., 2005. The Growth of Executive Pay. Oxford Review of Economic Policy 21, 283–303.

Bebchuk, L., Hamdani, A., 2017. Independent directors and controlling shareholders. University of Pennsylvania Law Review 165, 1271–1315.

Becht, M., Polo, A., Rossi, S., 2016. Does mandatory shareholder voting prevent bad acquisitions? Review of Financial Studies 29, 3035–3067.

Bertrand, M., Mullainathan, S., 2001. Are CEOs rewarded for luck? the ones without principals are. Quarterly Journal of Economics 116, 901–929.

Brunarski, K., Campbell, C., Harman, Y., 2015. Evidence on the outcome of say on pay votes: how managers, directors, and shareholders respond. Journal of Corporate Finance 30, 132–149.

Burkart, M., Gromb, D., Panunzi, F., 1997. Large shareholder monitoring and the the value of the firm. Quarterly Journal of Economics 112, 693–728.

Cai, J., Walking, R., 2011. Shareholders' say on pay: does it create value? Journal of Financial and Quantitative Analysis 46, 299–339.

Canadian Securities Administrators, 2017. Multilateral CSA Notice of Adoption of Multilateral Instrument 61–101: Protection of Minority Security Holders in Special Transactions.

Carleton, W., Nelson, J., Weisbach, M., 1998. The influence of institutions on corporate governance through private negotiations: evidence from TIAA–CREF. Journal of Finance 53, 1335–1362.

Chen, Z., Bin, K., Yang, Z., 2013. Minority shareholders' control rights and the quality of corporate decisions in weak investor protection countries: a natural experiment from China. Accounting Review 88, 1211–1238.

Claessens, S., Djankov, S., Lang, L., 2000. The separation of ownership and control in East Asian corporations. Journal of Financial Economics 58, 81–112.

Correa, R., Lel, U., 2016. Say on pay laws, executive compensation, CEO pay slice, and firm value around the world. Journal of Financial Economics 122, 500–520.

Croci, E., Gonenc, H., Ozkan, N., 2012. CEO compensation, family control, and institutional investors in Continental Europe. Journal of Banking and Finance 36, 3318–3335.

Cuñat, V., Giné, M., Guadalupe, M., 2016. Say pays! shareholder voice and firm performance. Review of Finance 20, 1799–1834.

Davies, P., 2017. Related party transactions: UK model. Unpublished working paper, University of Oxford.

Djankov, S., La Porta, R., Lopez de Silanes, F., Shleifer, A., 2008. The law and economics of self-dealing. Journal of Financial Economics 88, 430–465.

Elston, J., Goldberg, L.A., 2003. Executive compensation and agency costs in Germany. Journal of Banking and Finance 27, 1391–1410.

Enriques, L., 2015. Related-party transactions: policy options and real-world challenges, European Business Organization Law Review 16, 1–37.

Enriques, L., Hertig, G., Kanda, H., Pargendler, M., 2017. Related party transactions. In: R. Kraakman et al. (Eds.), The Anatomy of Corporate Law, Oxford University Press, 3d Edition.

Enriques, L., Volpin, P., 2007. Corporate governance reforms in Continental Europe. Journal of Economic Perspectives 21, 117–140.

Ertimur, Y., Ferri, F., Muslu, V. 2012. Shareholder activism and CEO pay. Review of Financial Studies 24, 535–592.

Faccio, M., Lang, L., 2002. The ultimate control of Western European corporations. Journal of Financial Economics 65, 365–395.

Ferri, F., Maber, D., 2013. Say on pay votes and CEO compensation: evidence from the United Kingdom. Review of Finance 17, 527–563.

Fisch, J., Palia, D., Davidoff Solomon, S., 2018. Is say on pay all about pay? the impact of firm performance. Harvard Business Law Review 8, forthcoming.

Fried, J., 2017. Equity-issuance tunneling. Unpublished working paper, Harvard Law School.

Fried, J., Spamann, H., 2017. Cheap-stock tunneling around preemptive rights. Unpublished working paper, Harvard Law School.

Gilson, R., Gordon, J., 2003. Controlling controlling shareholders. University of Pennsylvania Law Review 152, 785–843.

Gilson, R., Schwartz, A., 2015. Corporate control and credible commitment. International Review of Law and Economics 43, 119–130.

Goshen, Z., 2003. The efficiency of controlling corporate self-dealing: theory meets reality. California Law Review 91, 393–438.

Gregory-Smith, I., Thompson, P., Wright, P., 2013. CEO pay and voting dissent before and after the crisis. Economic Journal 124, F22–F39.

Gutierrez, M., Saez, M., 2017. Strong shareholders, weak outside investors. Unpublished working paper, Universidad Carlos III de Madrid and Universidad Autonoma de Madrid.

Hamdani, A., Yafeh, Y., 2013. Institutional investors as minority shareholders. Review of Finance 17, 691–725.

Holderness, C., 2009. The myth of diffuse ownership in the United States. Review of Financial Studies 22, 1377–1408.

Holderness, C., 2017. Equity issuances and agency costs: the telling story of shareholder approval around the world. Working paper.

Iliev, P., Vitanova, S., 2017. The effect of the say-on-pay vote in the U.S. Working paper, Penn State University.

Jackson, H., Roe, M., 2009. Public and private enforcement of securities laws: resource-based evidence. Journal of Financial Economics 93, 207–238.

Kamar, E., 2006. Does shareholder voting on acquisitions matter? American Law and Economics Association Annual Meeting.

Khanna, T., Yafeh, Y., 2007. Business groups in emerging markets: paragons or parasites? Journal of Economic Literature 45, 331–373.

Larcker, D., Ormazabal, G., Taylor, D., 2013. The market reaction to corporate governance regulation. Journal of Financial Economics 101, 431–448.

OECD 2012. Related Party Transactions and Minority Shareholder Rights. OECD Publishing. https://www.oecd.org/daf/ca/50089215.pdf.

Restrepo, F., 2013. Do different standards of judicial review affect the gains of minority shareholders in freeze-out transactions? a re-examination of Siliconix. Harvard Business Law Review 3, 321–359.

Restrepo, F., Subramanian, G., 2015. The effect of Delaware doctrine on freezeout structure and outcomes: evidence on the unified approach. Harvard Business Law Review 5, 205–236.

Rock, E., 2017. MOM's approval in a world of activist shareholders. Unpublished working paper, New York University Law School.

Shleifer, A., Vishny, R., 1997. A survey of corporate governance. Journal of Finance 52, 737–783.

Subramanian, G., 2007. Post-Siliconix freeze-outs: theory and evidence. Journal of Legal Studies 26, 1–26.

Thomas, R., Van der Elst, C., 2015. Say on pay around the world. Washington University Law Review 92, 655-731.

Urzua, F., 2009. Too few dividends? groups' tunneling through chair and board compensation. Journal of Corporate Finance 15, 245–256.

Yermack, D., 2010. Shareholder voting and corporate governance. Annual Review of Financial Economics 2, 103–125.

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

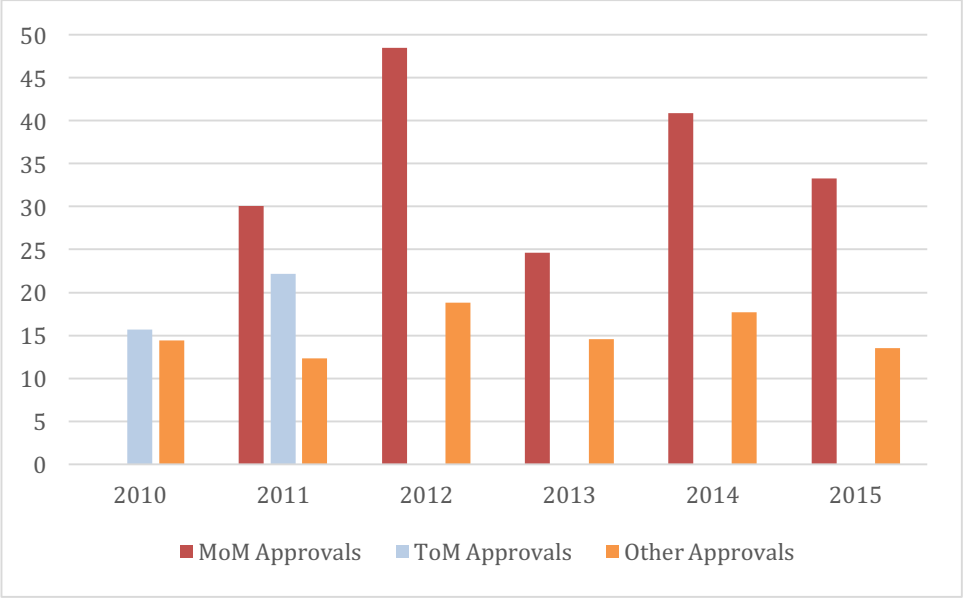

Figure 2: Disappearance Rates of Controller Executives and Non-Controller Executives

A disappearing executive is defined as an executive who fails to appear on the firm's list of highest paid executives the next year. The disappearance rate is the likelihood that a particular type of executive (controller or non-controller) will disappear, with the 2009 disappearance rate normalized to 100.

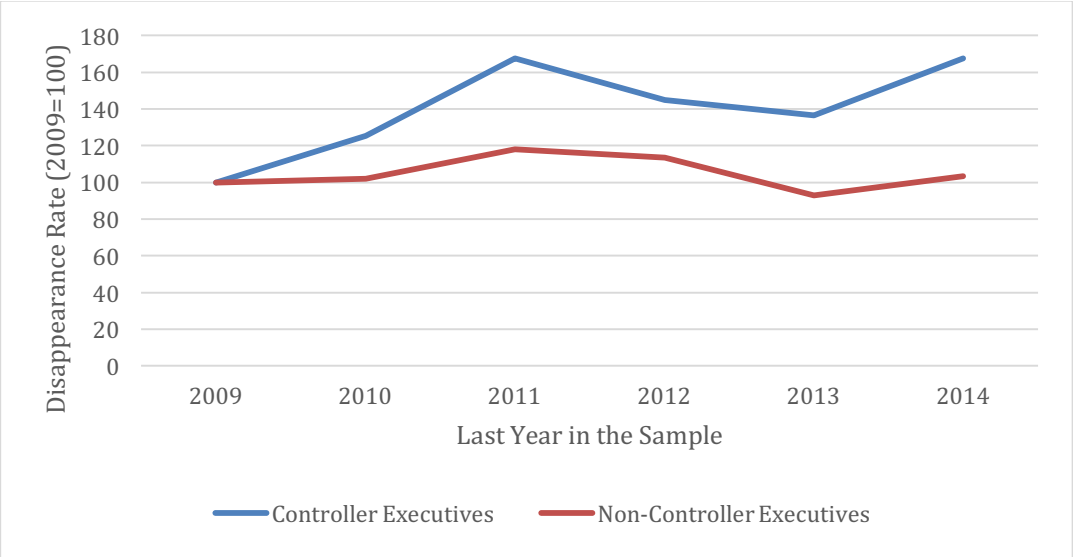

Table 1, Panel A: Firm-Level Data

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 4,522 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
Equity Held by Individual Controller Executives	In percent	23	25	0	16	39	3,426
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 not reported in the following 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 (% 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 (% 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 2011 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**	–0.10***	–0.13***	–0.10***	–0.07**	–0.11***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)
Partial Employment				–0.36***	–0.19***	–0.35***
				(0.03)	(0.05)	(0.02)
Log (Total Assets, in thousands of NIS)				0.20***	0.19***	0.19***
				(0.02)	(0.03)	(0.01)
ROA				0.02	0.28**	0.14***
				(0.12)	(0.12)	(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 2011 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. Table 4, Panel A uses a truncated sample starting in 2010 (compensation changes relative to 2009). 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 using the 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: 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 Panel A 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 6: Executive Disappearance Before and After the 2011 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 the current year but not the next year 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 7: 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 total annual 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 8: Pay Before and After the 2011 Reform by Subsamples

The sample consists of observations on 4,507 executives from 591 Israeli public firms in the years 2009–2015. This table repeats the benchmark regressions from Column 4 of Table 2 for three subsamples. In all regressions, the dependent variable is the natural logarithm of total compensation. In Column 1, we exclude firms with no controller executive on the list of highest paid executives. In Column 2, we exclude observations oncontroller executive who disappear from the sample during the sample period. In Column 3 we exclude firms 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)
Controller Executive*Post Reform	–0.12***	–0.06*	–0.12***
	(0.03)	(0.03)	(0.03)
Partial Employment	–0.36***	–0.36***	N/A
	(0.04)	(0.03)	
Log (Total Assets in Thousands of NIS)	0.24***	0.20***	0.11***
	(0.03)	(0.02)	(0.02)
ROA	0.35**	0.02	0.11
	(0.17)	(0.12)	(0.12)
Year Fixed Effects	Yes	Yes	Yes
Executive Fixed Effects	Yes	Yes	Yes
Observations	9,174	13,278	9,680
R-Squared	0.89	0.89	0.88