The Effect of Minority Veto Rights on Controller Pay Tunneling

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A central challenge in the regulation of controlled firms is curbing rent extraction by controllers. As independent directors and fiduciary duties are often insufficient, some jurisdictions give minority shareholders veto rights over related-party transactions. To assess these rights' effectiveness, we exploit a 2011 Israeli reform that gave minority shareholders veto rights over related-party transactions, including the pay of controllers and their relatives ("controller executives"). We find that the reform curbed controller-executive pay and led some controller executives to resign or go with little or no pay in circumstances suggesting their pay would be rejected. These findings suggest that minority veto rights can be an effective corporate governance tool.

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1. Introduction

Most publicly traded firms 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 governance objective is protecting minority shareholders from tunneling by the controller (Shleifer and Vishny, 1997; Gilson and Gordon, 2003; Enriques and Volpin, 2007; Djankov et al., 2008; Jackson and Roe, 2009).

Independent directors and fiduciary duties provide insufficient protection: independent directors are typically appointed by the controller, and impediments to shareholder litigation undermine fiduciary duties (Bebchuk and Hamdani, 2017; Enriques et al., 2017). A potentially more powerful protection is giving minority shareholders veto rights over related-party transactions (Goshen, 2003; Djankov et al., 2008). Versions of this approach, now favored by the OECD (2012), have become the law in several countries.[1]

However, there is scant evidence on whether minority veto rights have any impact. A 2011 reform in Israel offers a unique setting for answering this question.[2] A key element of this reform was to give the minority veto rights over related-party transactions, including the pay of controllers and their relatives serving as officers or

[1] Black and Kraakman (1996) (Russia); OECD (2012) (France, Israel, Italy); Canadian Securities Administrators (2017) (Canada); Enriques and Tröger (2019) (Hong Kong, Singapore); Davies (2019) (United Kingdom); Li (2018) (India).

[2] The Companies Law (Amendment No. 16), 5771–2011, Section 34, which amended the Companies Law, 5759–1999, Section 275.

directors ("controller executives"). Before the reform, pay packages of controller executives above a low statutory amount (requiring only board approval) had required the approval of a third of the minority, and this approval was valid indefinitely. The reform substantially empowered the minority by requiring the approval of these pay packages by a majority of the minority ("MOM approval") every three years. The reform did not alter the approval mechanism for the pay of officers or directors unrelated to controllers ("non-controller executives"), which had not required minority support before the reform, creating a viable control group.

Anecdotal accounts suggest that the reform had real bite. Rami Levy, the controller and CEO of a supermarket chain, had to cut his bonus in half to secure minority support for his pay (Calcalist, October 16, 2011). According to our calculations, his post-approval pay was 26% lower than his previous pay. Ilan Ben Dov, the controller of a cellular holding company, forfeited most of his pay as board chair to win minority approval (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. For example, at one wireless technology firm, the threat of minority veto drove the controller and board chair (Zvi Borovitz) and the CEO (Zvi's son, Moshe Borovitz) to announce their departure (Globes, December 7, 2011).

Using hand-collected data on hundreds of firms and thousands of executives over an eight-year period around the reform, we find that the reform is associated with a 13%–17% decline in controller-executive pay relative to benchmarks and a substantial increase in the frequency of pay cuts for controller executives. Moreover, after the

reform, the likelihood that controller executives forgo pay or leave office increases by about 50%, often following an unmet deadline for MOM approval. Non-controller executives replace about 10% of these controller executives.

Our findings relate to four governance questions.

First, we shed light on the effectiveness of minority veto rights. Early work in this area examines controllers who voluntarily grant such rights in Delaware freezeouts to reduce judicial scrutiny (Subramanian, 2007; Restrepo, 2013; Restrepo and Subramanian, 2015). The limitation of this setting is that both the timing of the deal and the decision to grant minority veto rights are endogenous, and the grant of these rights changes the legal treatment of the deal. Chen et al. (2013) study the effect of a Chinese reform requiring minority approval of stock issuances, which often facilitate tunneling (Fried, 2019; Fried and Spamann, 2019). But the study does not include stock issuances unaffected by the reform as controls. Li (2018) finds that Indian firms with a high volume of related-party transactions experience a positive market reaction to a reform requiring minority approval of these transactions and an increase in the association between these transactions and profitability. Lin (2019) reports that minority veto rights are the most commonly used and most effective tool for shareholder activism in Hong Kong.

Our study sidesteps the endogeneity concern by studying how an exogenous grant of minority veto rights with state-imposed deadlines affects preexisting pay levels. In addition, pay data for non-controller executives and executive fixed effects enable us to construct robust controls.

Second, we contribute to the literature on shareholder voting generally. Shareholders typically have veto rights over fundamental decisions like charter amendments and mergers. Sometimes they have veto rights over additional decisions like equity issuances (Yermack, 2010; Holderness, 2018), acquisitions (Kamar, 2006; Becht et al., 2016), equity-based pay plans (Armstrong et al., 2013), or transactions involving directors (Enriques et al., 2017). It is difficult to measure the effectiveness of these rights because firms put proposals to shareholder vote only when expecting approval (Carleton, Nelson, and Weisbach, 1998). In contrast, our setting features largely exogenously timed votes.

Third, we contribute to the literature on pay tunneling. Prior work finds controller-executive pay premiums in some jurisdictions (Urzua, 2009; Barak et al., 2011 Bozzi et al., 2017) but not in others (Elston and Goldberg, 2003; Croci et al., 2012), and does not rule out the possibility that controller executives occupy higher positions than they would if they were non-controller executives. Our setting enables us to measure pay tunneling directly by examining a midstream increase in the minority's influence over controller-executive pay relative to the pay of non-controller executives.

Finally, we contribute to the literature on shareholder voting on pay, also known as say on pay (SOP), which is binding in certain jurisdictions and advisory in others. Prior studies find a relation between simple-majority SOP and shareholder-favored changes in pay in certain jurisdictions but not in others (for example, Ertimur et al., 2011; Ferri and Maber, 2013; Gregory-Smith et al., 2014; Brunarski et al., 2015; Correa and Lel, 2016; Cuñat et al., 2016; Iliev and Vitanova, 2019). However, simple-majority SOP, whether binding or not, guarantees approval in controlled firms. Our study finds that

binding SOP requiring MOM support restrains pay and causes executives to step down or work with little or no pay in controlled firms.

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

2. Data and methodology

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—required approval by only a third of the minority ("TOM approval") once for the duration of the transaction (Hamdani and Yafeh, 2013). The reform required MOM approval. Moreover, it required long-term transactions, including ongoing pay arrangements, to receive new approval every three years. The reform thus enabled the minority to deny a controller executive *any* pay above a low statutory amount once the three-year approval deadline is reached.

We study the effect of these veto rights on the pay of controller executives using hand-collected data on executive pay for corporations listed on the Tel Aviv Stock Exchange in the years 2008–2015. The data set excludes financial firms (for which measures of performance are different), dual-listed firms (which did not report individual executive pay until 2014), and firms with public debt but no public equity (which are not subject to minority veto rights). The sample, an unbalanced panel described in Panel A of Table 1, consists of 554 firms, of which 30% are in manufacturing, 27% are in services, and 23% are in real estate.

Israeli law defines a shareholder 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, the law presumes that a 25% shareholder is a controller unless another shareholder holds 50% of the shares. 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. Virtually all firms in the sample have a controller.

Panel A of Table 1 also presents accounting data on firm size and profitability from 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), we measure operating profitability primarily by return on assets (ROA). We examine the market-to-book ratio, stock returns, and lagged ROA as alternative measures.

The sample firms must report annually the individual pay 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 5% shareholder. The definition of reported executives and the possibility of midyear 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, pay, and pay approvals of reported executives.

The median number of reported executives per firm is five and the mean is slightly higher. More than 40% of the firms report the pay of five executives, making five the modal number of reported executives. Another 30% of the firms report the pay of six

or seven executives, 10% of the firms report the pay of eight to ten executives, and another 10% report the pay of three to four executives.

While virtually all of the firms have a controller, only about 60% report at least one controller executive. The median and mode of the number of controller executives per firm is one, and the mean is 1.3, with 63% of controller executives serving as board chair or CEO. One of the robustness tests reported below excludes firms with no reported controller executives.

Panel B of Table 1 presents the data, consisting of 16,880 observations on about 4,900 executives during the years 2008–2015. Controller executives comprise about 20% of the executives in the sample. However, because their turnover is lower than that of non-controller executives, they comprise about a quarter of the observations in the sample.

The average level of total pay of an executive is about NIS 1.38 million (about $345,000) and the median is about NIS 850,000 (about $215,000), with controller executives earning on average about NIS 1.6 million—15% above the average. Some controller executives are relatives of the controller who occupy less senior positions and thereby bring down the average.

Non-equity-based pay (total pay minus equity-based pay) accounts on average for 88% of total pay in the full sample and for 95% of total pay of controller executives. Nevertheless, controller executives typically hold much larger equity stakes than non-controller executives: 23% on average (with a median of 15%), 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 and bring down the average.

Panel C of Table 1 presents pay approvals by type and year. There are 310 pre-reform TOM approvals and 773 post-reform MOM approvals for controller-executive pay. Relatives of the controller must obtain TOM approval pre-reform or MOM approval post-reform to receive pay even if their equity stake is zero.

There are also 1,677 pay approvals for non-controller executives. Most are either board approvals for new pay contracts of officers or shareholder approvals by a simple majority for new pay contracts of directors. These approvals are not subject to minority veto, have no statutory deadline, and last indefinitely. In addition, since 2013, new pay contracts for non-controller executives are subject to *nonbinding* MOM approval in three situations: the firm has no pay policy (116 observations); the contract is inconsistent with the firm's pay policy (zero observations); or the contract is with an incumbent CEO (100 observations). Because this approval is neither binding nor subject to a statutory deadline, we do not expect it to influence pay levels. Nevertheless, we conduct robustness tests in which we end the sample period in 2012, before this new requirement appears.

Finally, Panel D of Table 1 reports the numbers of controller executives 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.

Our analysis proceeds as follows. First, we compare the post-reform change in the pay of controller executives, whom the reform affected directly, with the post-reform change in the pay of non-controller executives, whom the reform did not affect directly. Second, we explore one mechanism by which the reform could affect controller-executive pay levels: the minority shareholders' use of their veto right 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 and the likelihood that a non-controller executive replaces a disappearing controller executive. Finally, we present robustness tests and additional results.

Our estimates would be conservative if controller executives anticipated the reform and rushed to obtain TOM approvals ahead of the change, spurring pay increases pre-reform and postponing by three years any pay reduction post-reform. However, Panel C of Table 1 does not reveal such a rush. This is not surprising. First, controller executives who could obtain a pay increase pre-reform would have done so even if there were no reform on the horizon. Second, while a possible reform had been in discussion for several years, its timing and content evolved over a multi-year legislative process, and its enactment was not a foregone conclusion.

Ideally, the control group in an analysis of this type should be unaffected by the reform. In our setting, this assumption may be violated if firms index the pay of non-controller executives to that of controller executives or raise non-controller-executive pay post-reform to justify higher controller-executive pay.[3]

[3] Dittmann et al. (2018) show that the pay of top executives within a firm tends to move together.

However, to the extent firms index the pay of non-controller executives to that of controller executives, our findings on the reform's effect on the controller executives' pay premium would be understated.

As for the possibility that firms raise non-controller-executive pay post-reform to justify higher controller-executive pay, there is no evidence of this practice. First, while the average pay of non-controller executives post-reform is 2% higher than pre-reform, the average wage in Israel in the latter period is 12% higher than in the former according to data published by the National Insurance Institute of Israel. Second, the likelihood of non-controller executives' pay approvals, a precondition for increasing their pay, does not increase post-reform (Panel C of Table 1). Third, non-controller executives appointed post-reform receive on average much lower pay than incumbent non-controller executives, ruling out the possibility that high-paid non-controlling executives replaced low-paid ones.

All of these facts are inconsistent with firms strategically raising the pay of incumbent or new non-controller executives to justify higher controller-executive pay. There has also been no mention of such a strategy in media reports. The failure to engage in this strategy is not a surprise, as the controller would personally bear much of the cost of higher non-controller-executive pay, undercutting the gain from using it to justify higher controller-executive pay.

3. Main results

This section reports our main results. First, controller-executive pay decreases post-reform relative to that of non-controller executives. Second, unlike other pay approvals,

post-reform MOM approvals are not associated on average with pay increase and often are associated with pay decrease. Third, the likelihood that controller executives disappear from the firm's list of highest paid executives increases post-reform relative to that of non-controller executives. This increase is associated with unmet MOM approval deadlines.

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 using a difference-in-differences specification:

$$Ln(Total\ Pay)_{ijt} = \alpha + \beta Controller\ Executive_{ijt} \times Post\ Reform + Firm\text{-}Level\ Controls_{jt} + Executive\ Fixed\ Effects + Year\ Fixed\ Effects + \varepsilon_{jt}, \quad (1)$$

where i, j and t denote the individual executive, the firm, and the year, respectively. *Controller Executive* × *Post Reform* indicates a controller executive in the year 2011 or later. Executive fixed effects capture each executive's time-invariant attributes (such as education) as well as her average level of pay.[4] Firm-level controls and year fixed effects capture other determinants of pay.

[4] There is no need for a dummy for a controller executive prior to the reform as we use executive fixed effects. If we were to include a dummy variable for controller executives, as in a standard difference-in-differences specification, its coefficient would reflect the few executives whose relation to the controller varies over time (and thus not be captured by executive fixed effects), or who serve in two firms but are related to the controller only in one. In unreported regressions containing this variable, we find that its coefficient is positive and that the other coefficients are similar to those in Table 2. Pooled regressions with no executive fixed effects produce qualitatively similar results.

Our main dependent variable is the natural logarithm of total pay of an 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 avoid this potential distortion, some specifications use non-equity-based pay. We also examine an alternative specification in which the dependent variable is the ratio of the executive's pay to the aggregate pay of the firm's reported executives. Following Bebchuk et al. (2011), we refer to this ratio as the pay slice.

Table 2 presents the results for the full sample, except for Columns 2 and 6, which use a more homogenous subsample containing only the two highest paid executives in each firm and year. In Columns 1, 2, 5, and 6, the dependent variable is the natural logarithm of total pay. In Columns 3 and 7, the dependent variable is the natural logarithm of non-equity-based pay. In Columns 4 and 8, the dependent variable is the pay slice. Columns 1 through 4 control only for executive fixed effects and year fixed effects. Columns 5 through 8 add commonly used controls for firm size and ROA, and control for whether the firm employs the executive part-time or for less than a full year (*Partial Employment*).[5]

Following Petersen (2009), we examine two approaches for the calculation of standard errors in the regressions: clustering standard errors by firm and clustering

[5] The inclusion of executive fixed effects requires using only time-varying controls. *Partial Employment*, which indicates a year in which an executive works less than 12 months or less than full time, varies over time for about a quarter of the executives.

standard errors separately by firm and by year, in either case while including year fixed effects. The two approaches yield similar results and so we present only the results of the former approach.

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 pay of controller executives. Although absolute pay levels for controller executives do not materially change post-reform (the median pay remains about one million NIS), the coefficient of the interaction term in the full sample implies that these executives earned 13%–17% less in 2011–2015 than they would have earned without the reform.[6]

Importantly, this is an *average* effect. Many controller executives saw their pay fall by substantial amounts, with some experiencing pay cuts of over 50%. Conditional on pay reduction, nearly a quarter of controller executives saw their pay fall by at least 30%. Consistent with the previous results, Columns 4 and 8 show that the pay slice of controller executives declined by about one percentage point: it averaged about 19% in the years 2008–2010, and about 18% in the years 2011–2015. This decline is statistically significant.

[6] The high values of R-squared in all regression estimates result from including thousands of executive-specific fixed effects. The estimates are smaller and less statistically significant when the sample is restricted to the two highest paid executives. In unreported regressions, the results remain when we replace the post-reform dummy, which equals one starting in 2011 for all executives, by an individual post-reform dummy, which equals one after the calendar year of the controller executive's MOM approval deadline.

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

One mechanism by which the reform could affect controller-executive pay is forcing absolute pay reductions. Table 3 presents several specifications in which the dependent variable indicates whether total pay or non-equity-based pay is lower than in the preceding year. We use probit regressions showing marginal effects without executive fixed effects and linear probability models with executive fixed effects for the full sample and for a subsample containing only the two highest paid executives. As in Table 2, we cluster standard errors by firm and include year fixed effects. In unreported regressions, clustering standard errors separately by firm and by year while including year fixed effects yields similar results.

All approvals other than controller-executive MOM approvals are associated with pay increases: the coefficient of *Any Approval* is negative and statistically significant. This is natural as such approvals need not meet any statutory deadline and most require no minority support. Firms seek them only when planning to raise pay and expecting to obtain any needed approval. Consistently, the coefficient of *TOM Approval* is small, suggesting that its effect is similar to that of *Any Approval*.

Controller-executive MOM approvals are economically and statistically different from other approvals in not being associated with pay 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 pay reductions. Specifically, of 773 MOM approvals, 280 (36%) are associated with a reduction in total pay; the comparable figure for other approval types is about half. Similarly, 36% of MOM approvals are associated with a reduction in non-equity-based pay, compared to 17% of other approvals. Fig. 1

shows that MOM approvals are far more likely to result in reductions of non-equity-based pay than other approval types. The same holds for reductions in total pay.

We explore why the reform affected certain controller executives more than others. In all of the specifications in Table 3 but one, there is a negative and statistically significant relation between ROA and the likelihood of pay reduction. However, unlike Fisch et al. (2018), we do not find that the effect of MOM approval on the likelihood of pay reduction varies with firm performance or excess pay, defined as the residual from a regression of pay on firm size, industry, and profitability. This suggests that the minority uses other indicators to determine whether a controller-executive's pay is excessive.

We conclude that the requirement of MOM approval has real bite. Before its introduction in 2011, the alternative to seeking TOM approval for a pay raise was to keep a controller-executive's pay 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 considers the controller executive to be overpaid.

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

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. Our estimates of changes in the reported pay of executives who continue to appear on the list of the firm's highest paid employees do not capture this effect.

To investigate this effect, we identify all executives whose pay no longer appears in their firms' annual reports even though the firm remains in the sample and continues to report the pay of other executives. These executives either left the firm or stayed in office for 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 correlates with failure to obtain MOM approval.

We begin by observing that controller executives are less likely to disappear than are non-controller executives during the entire sample period. While controller executives constitute about 25% of the sample, they account for only 12% of disappearances. However, their disappearance rate increases significantly after the reform, from 6% a year to 9%–12% a year. There is no increase in the disappearance rate of non-controller executives. Fig. 2 presents this comparison. The disappearance rate of controller executives increases post-reform, whereas that of non-controller executives does not (Panel A). As a result, the ratio of controller-executive disappearances to non-controller-executive disappearances increases post-reform (Panel B).

Columns 1 and 2 of Table 4 corroborate this result by displaying the marginal effects of several determinants of the probability of disappearance. Here too, we cluster standard errors by firm and include year fixed effects. In unreported regressions, clustering standard errors separately by firm and by year while including year fixed effects yields similar results. We find that post-reform the probability of disappearance increases for controller executives from 16% below that of non-controller executives to only about 10% below.

Moreover, Column 3 of Table 4 shows that the probability of controller-executive disappearance increases by an additional 9% following a MOM approval deadline. Controller executives who fail to meet the deadline drive this effect: Column 4 indicates that the probability of controller-executive disappearance falls to its pre-reform level after obtaining MOM approval.

We read corporate filings to inquire if non-controller executives replaced disappearing controller executives. Before the reform, this almost never happened. After the reform, it occurs in about 10% of controller-executive disappearances (about 30 of about 300). The figures are approximate because the fate of some disappearing controller executives is unclear. About 30 controller executives cease to hold their positions for reasons unrelated to changes in corporate control and are replaced by non-controller executives. About 50 other controller executives remain in office but fall off the list of top-paid executives because they work for little or no pay. Other reasons for controller executive disappearance include replacement by different controller executives, corporate-control transactions, and court-ordered receivership.

In sum, the reform not only restrains the pay of controller executives whose pay continued to be reported, but also drives the pay of other controller executives below reported levels, often to zero. It also prompts a modest shift in the staffing of management positions, from controller executives to non-controller executives. These findings are consistent with Ra and Kim (2018), who find that directors of Korean firms took pay cuts and relinquished formal positions to avoid disclosure under a reform that required disclosure of pay above a certain threshold.

Our estimates of the reform's effect on controller-executive pay therefore understate its full effect on firms. 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. Much of the controller-executive pay observed in early post-reform years therefore reflects the lingering effect of pre-reform pay contracts.

4. Robustness tests and extensions

This section reports several robustness tests and extensions. First, our results remain when using various subsamples and different performance measures. Second, there is no evidence of post-reform substitution from controller-executive pay into other forms of value extraction and no change in firm value or pay structure.

4.1. Results for subsamples and different performance measures

Table 5 presents the results of repeating the benchmark specification from Column 5 of Table 2 (pay before and after the reform) using several subsamples. The results are robust.

Column 1 excludes firms lacking controller executives on the list of highest-paid executives. These firms help us estimate the effects of the control variables more precisely, but do not contribute to estimating 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 5 of Table 2.

Column 2 includes only firms with a controller executive whose MOM approval deadline falls in 2011 to estimate the effect of the reform on those who were the least able to plan for it. In this much smaller subsample, the reform's effect on the pay of controller executives is much larger than in the full sample: about –20% versus about –13% in Column 5 of Table 2.

Column 3 excludes firms belonging to corporate groups with three or more layers of public firms using data from the Bank of Israel. These firms were subject to a 2013 law requiring corporate groups to have at most three layers of public firms by the end of 2017, and at most two such layers by the end of 2019. The results remain, ruling out the possibility that the decrease in controller-executive pay premiums is due to changes in corporate groups. The results also remain in unreported regressions that exclude firms belonging to corporate groups with *any* number of layers of public firms (including groups with only two such layers, which were not subject to the 2013 law) to avoid duplicative reports of the same payment by different firms in a group.

Column 4 excludes disappearing executives. The results remain, suggesting that the effects of the reform are not driven by the small number of executives who eventually leave the sample, but rather reflect a wider phenomenon.

Column 5 excludes the years 2013–2015 to avoid a possible confounding effect due to the change in 2013 in the approval requirements of non-controller-executive pay. This also rules out any confounding effects of the 2013 law requiring the restructuring of corporate groups. Again, the results hold.

In Columns 6, the market-to-book ratio replaces ROA as a control variable. The results hold, suggesting that they are insensitive to the measure of performance. The results hold also in unreported regressions using lagged ROA or stock returns as measures of performance.

In unreported regressions, the results remain when running all of the robustness tests above on the benchmark specification in Column 1 of Table 3 (determinants of pay reductions). Similarly, the results remain when running all of the robustness tests that include disappearing executives on the benchmark specification in Column 2 of Table 4 (executive disappearance before and after the reform).

4.2. Additional effects of the reform on firm behavior

This subsection explores possible effects of the reform on additional aspects of corporate behavior.

First, we examine if other related-party transactions replace executive pay as a way for controllers to extract rents. 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 controller-executive pay.

Accordingly, the value of non-pay related-party transactions normalized by assets declines after the reform for the 108 firms with available data (non-financial firms among the largest 150 firms on Tel Aviv Stock Exchange) by slightly more than 50%. The decline is similar (slightly less than 50%) for firms where a controller executive

experienced a pay decrease post-reform. Nevertheless, more data and a measure of the extent to which each transaction extracts rent are needed to draw firm conclusions.[7]

Second, we examine if post-reform dividends replace executive pay as a way for controllers to obtain cash from the firm. Such a shift is also unlikely because dividends are payable to all shareholders and are therefore costly to controllers. Accordingly, we find that the ratio of dividends to profits as well as other measures of dividends normalized by firm size remain roughly constant post-reform, and that firms with controller-executive pay reductions are not different from other firms.

We also do not detect significant increases in Tobin's Q post-reform in firms with controller-executive pay reductions. This is not surprising. The reform targeted only the pay of controller executives, and their incentives are due primarily to the controller's shareholdings.

Finally, we find no significant change in the variable pay of controller executives (or non-controller executives) after the reform, whether or not a controller executive in the firm experienced a pay reduction. In addition, when adding the interaction term *ROA* × *Post Reform* to the benchmark specification in Column 5 of Table 2 (and when adding the interaction term *Market to Book* × *Post Reform* to Column 6 of Table 5), both its coefficient and statistical significance are close to zero, consistent with an absence of

[7] Licht (2019) argues that, in recent years, Israeli courts have relaxed their scrutiny of related-party transactions. This would mean that rent extraction can increase despite a decrease in transaction volume.

change in the sensitivity of pay to performance after the reform. This is true for the entire sample and for controller executives and non-controller executives separately.

5. Conclusion

We find that an Israeli reform that required minority approval for controller-executive pay constrained that pay relative to benchmarks and increased the likelihood of absolute pay cuts. The need to obtain approval also induced some controller executives to relinquish their positions or continue to work with little or no pay.

This study provides a useful setting for testing the effectiveness of minority veto rights over related-party transactions generally. In other settings, the controller chooses whether and when to propose the transaction (and, under Delaware law, whether to give the minority veto rights), raising endogeneity concerns. The Israeli reform imposed in midstream a deadline for obtaining approval of controller-executive pay, mitigating these concerns.

Our findings also contribute to the literature on controller-pay tunneling by suggesting that minority shareholders considered certain controller executives to be overpaid pre-reform, and contribute to the literature on SOP by showing that a mandatory vote can affect both pay and executive turnover.

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Figure 1: Percentage of Approvals Associated with Pay Reductions

This figure presents, for each year and approval type, the percentage of approvals associated with a reduction of non-equity-based pay. Non-equity-based pay is total pay minus equity-based pay. Years appear on the horizontal axis.

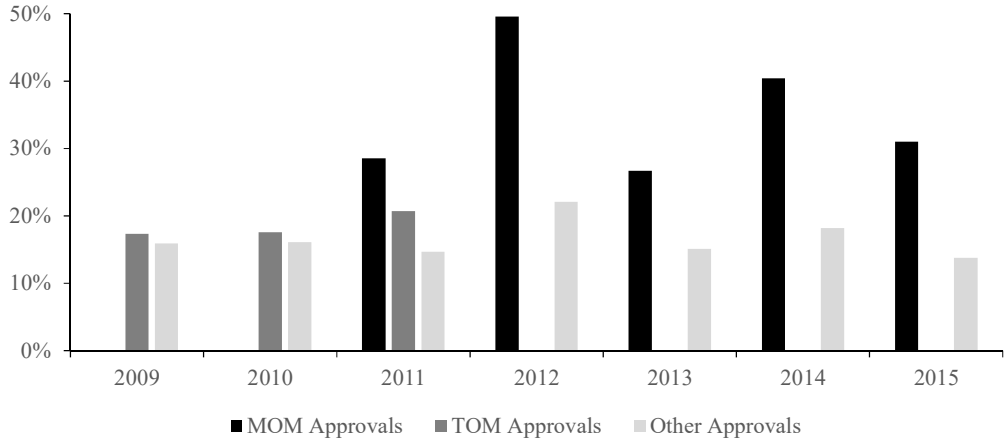
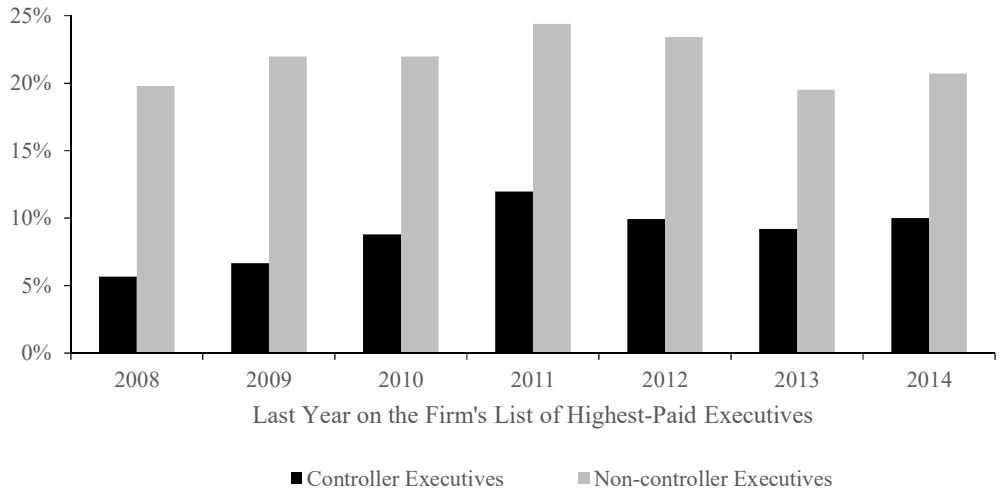


Figure 2: Executive Disappearances

This figure compares disappearance rates of controller executives and non-controller executives. A disappearing executive is an executive who no longer appears on the firm's list of highest-paid executives after the current year. Years appear on the horizontal axis. Panel A presents the disappearance rates of controller executives and non-controller executives. Panel B presents the ratio of controller-executive disappearance rate to non-controller-executive disappearance rate.

Panel A: Rate of Executive Disappearance from Firms



Panel B: Ratio of Controller-Executive Disappearance Rate to Non-Controller-Executive Disappearance Rate

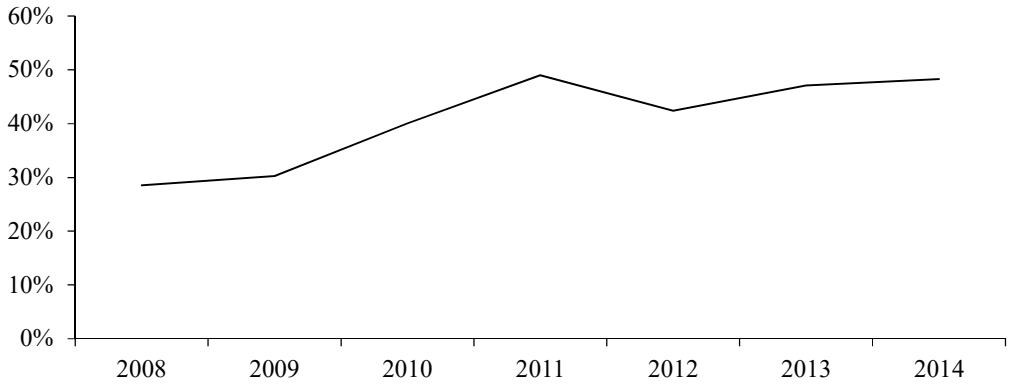


Table 1: Data Description

The sample is an unbalanced panel of 4,922 executives from 554 Israeli public firms in the years 2008–2015. Panel A reports firm-level annual data. Panel B reports executive-level annual data. Panel C reports executive pay approvals by year (data on *Other Approvals* for the year 2008 were not collected). Panel D reports by year the number (percentage) of executives who disappear from the firm's list of highest paid executives in the following year. Firm-level financial variables are from A-Online. Other data are from firms' annual reports and proxy statements. Monetary values are in New Israeli Shekels (about 4 NIS per 1 USD).

Panel A: Firm-Level Data

Definition	Units	Mean	Std.	25%	50%	75%
Total Assets	Millions of NIS	3,616	13,600	116	376	1,284
ROA	Annual operating profits to assets, in percent	0.0	16.0	–1.2	2.5	6.8
Number of Reported Executives		5.8	1.9	5	5	6
Number of Reported Controller Executives		1.43	1.37	0	1	2

Table 1 (continued)

Panel B: Executive-Level Data

Definition	Units	Mean	Std.	25%	50%	75%	Obs.
Total Pay (reported value of all pay components)	Thousands of NIS	1,378	1,992	462	849	1,520	16,880
Total Pay of Controller Executives	Thousands of NIS	1,585	2,369	522	1,015	1,840	4,141
Non-Equity-Based Pay (Total Pay excluding equity-based components)	Thousands of NIS	1,212	1,575	446	817	1,422	16,880
Non-Equity-Based Pay of Controller Executives	Thousands of NIS	1,493	2,097	510	995	1,795	4,141
Equity Held by Individual Controller Executives	Percent	23	25	0	15	39	4,126
Partial Employment	Equals one if an executive is employed for less than a full year or less than full-time	24.8%					16,880

Panel C: Pay Approvals

Approval Type	Definition	2008	2009	2010	2011	2012	2013	2014	2015	Total
MOM Approval	Majority of the minority pay approval	N/A	N/A	N/A	210	143	117	209	94	773
TOM Approval	Third of the minority pay approval	91	75	117	27	N/A	N/A	N/A	N/A	310
Other Approvals	Other pay approval		220	335	278	258	238	181	167	1,677

Panel D: Executive Disappearances by Last Year of Appearance

Definition	2008	2009	2010	2011	2012	2013	2014	2015	Total
Number (percentage) of controller executives who disappear from the firm's list of highest-paid executives in the following year	28 (5.7)	33 (6.7)	56 (8.8)	71 (12.0)	52 (9.9)	46 (9.2)	45 (9.9)	N/A	331 (8.0)
Number (percentage) of non-controller executives who disappear from the firm's list of highest-paid executives in the following year	316 (19.8)	345 (22.0)	415 (22.0)	437 (24.4)	370 (23.4)	283 (19.5)	295 (20.7)	N/A	2,461 (19.3)

Table 2: Pay Before and After the Reform

In Columns 1, 2, 5, and 6, the dependent variable is the natural logarithm of annual total pay. In Columns 3 and 7, the dependent variable is the natural logarithm of non-equity-based pay (omitting observations in which the entire pay package is equity based). In Columns 4 and 8, the dependent variable is the pay slice (the percentage of the executive's total pay out of the total pay of all reported executives in each firm and year), omitting the lowest paid executive reported in each firm and year so that the slices do not add up to a hundred. Columns 2 and 6 report the results for a subsample of the two highest paid executives in each firm. *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. *Ln(Total Assets)* is the natural logarithm of total assets in thousands of NIS. We multiply the coefficient of *ROA* by 100. All control variables are from the same year as the dependent variable. Robust standard errors clustered by firm are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Controller Executive × Post Reform	–0.16**	–0.08*	–0.17***	–1.18*	–0.13***	–0.05	–0.15**	–1.44**
	(0.04)	(0.05)	(0.04)	(0.67)	(0.04)	(0.04)	(0.04)	(0.65)
Ln(Total Assets)					0.22***	0.22***	0.20***	2.91***
					(0.03)	(0.03)	(0.03)	(0.36)
ROA					–0.02	0.18	0.03	3.34**
					(0.11)	(0.12)	(0.10)	(1.48)
Partial Employment					–0.31***	–0.16***	–0.30***	–2.45***
					(0.03)	(0.04)	(0.05)	(0.56)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Executive Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,880	6,352	16,826	13,624	16,880	6,352	16,826	13,624
R-Squared	0.86	0.90	0.86	0.77	0.87	0.91	0.87	0.78

Table 3: Determinants of Pay Reduction

Using a truncated sample starting in 2009 (pay changes relative to 2008), this table presents probit and linear-probability regressions of pay reduction. In Columns 1, 2, 3, and 4, the dependent variable is a dummy that equals one if total pay declines relative to the previous year and zero otherwise. Column 1 presents the results of a probit regression (with the coefficients normalized to show marginal effects) with a dummy for controller executives and no executive fixed effects. Column 2 presents the results of a linear-probability regression with executive fixed effects (and no dummy for controller executives). Columns 3 and 4 present similar specifications to Columns 1 and 2, respectively, for a subsample of the two highest paid executives. Columns 5 and 6 present similar specifications to Columns 1 and 2, respectively, using reduction in non-equity-based pay as the dependent variable. *Any Approval* is a dummy that equals one if the executive received a pay approval in the current year and zero otherwise. *MOM Approval* and *TOM Approval* are similarly defined dummies that equal one if the executive received a MOM approval or a TOM approval, respectively, in the current year and zero otherwise. *Ln(Total Assets)* is the natural logarithm of total assets in thousands of NIS. We multiply the coefficient of *ROA* by 100. *Partial Employment* equals one for executives employed less than a full year or less than full time and zero otherwise. All control variables are from the same year as the dependent variable. Robust standard errors clustered by firm are in parentheses. ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively.

	(1) Probit	(2) LPM	(3) Probit	(4) LPM	(5) Probit	(6) LPM
Any Approval	–0.13*** (0.01)	–0.14*** (0.02)	–0.12*** (0.02)	–0.16*** (0.03)	–0.12*** (0.01)	–0.11*** (0.02)
MOM Approval	0.11** (0.03)	0.09** (0.04)	0.12** (0.04)	0.11* (0.05)	0.11*** (0.03)	0.08** (0.04)
TOM Approval	–0.01 (0.04)	0.02 (0.05)	0.02 (0.06)	0.06 (0.07)	–0.00 (0.04)	0.01 (0.04)
Ln(Total Assets)	0.008*** (0.003)	–0.007 (0.009)	0.007* (0.004)	–0.013 (0.015)	0.000 (0.002)	–0.009 (0.008)
ROA	–0.13*** (0.03)	–0.16** (0.08)	–0.12** (0.05)	–0.23* (0.13)	–0.04 (0.03)	–0.18** (0.08)
Partial Employment	0.01 (0.01)	0.07*** (0.02)	–0.00 (0.02)	0.05 (0.04)	0.01 (0.01)	0.07*** (0.02)
Controller Executive	0.07*** (0.01)	N/A	0.03* (0.02)	N/A	0.09*** (0.01)	N/A
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Executive Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	14,790	14,790	5,552	5,552	14,790	14,790
R-Squared	N/A	0.30	N/A	0.35	N/A	0.29

Table 4: Executive Disappearances Before and After the Reform

Using a truncated sample ending in 2014, this table presents the marginal effects derived from probit 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 the executive has a MOM approval deadline in the current year and zero otherwise. *MOM Approval* equals one if the executive received a MOM approval in the current year and zero otherwise. *Partial Employment* equals one for executives employed less than a full year or full time and zero otherwise. *Ln(Total Assets)* is the natural logarithm of total assets in thousands of NIS. We multiply the coefficient of *ROA* by 100. All control variables are from the same year as the dependent variable. Robust standard errors clustered by firm are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% respectively.

	(1)	(2)	(3)	(4)
Controller Executive	–0.16*** (0.02)	–0.16*** (0.02)	–0.19*** (0.01)	–0.16*** (0.02)
Controller Executive × Post Reform	0.06** (0.03)	0.06** (0.03)	0.06** (0.03)	0.07*** (0.03)
MOM Approval Due			0.09*** (0.03)	
MOM Approval				–0.07*** (0.02)
Ln(Total Assets)		0.004 (0.003)	0.004 (0.003)	0.004 (0.003)
ROA		–0.17*** (0.03)	–0.15*** (0.03)	–0.17*** (0.03)
Partial Employment		0.09*** (0.01)	0.09*** (0.01)	0.09*** (0.01)
Year Fixed Effects	Yes	Yes	Yes	Yes
Executive Fixed Effects	No	No	No	No
Observations	14,998	14,998	14,614	14,998

Table 5: Robustness Tests of Pay Before and After the Reform

This table presents robustness tests of the benchmark regression in Column 5 of Table 2. The dependent variable is the natural logarithm of total pay. Column 1 includes only firms with at least one controller executive on the list of highest paid executives. Column 2 includes only firms with controller executives whose MOM approval deadline is in 2011. Column 3 excludes firms belonging to groups with three or more layers of public firms. Column 4 excludes executives who disappear from their firm's list of reported executives before the end of the sample period. Column 5 restricts the sample period to 2008–2012. In Column 6, we use the full sample and replace ROA with the market-to-book ratio. *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. *Ln(Total Assets)* is the natural logarithm of total assets in thousands of NIS. We multiply the coefficients of *ROA* and *Market-to-Book* by 100. All control variables are from the same year as the dependent variable. Robust standard errors clustered by firm 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	–0.14*** (0.04)	–0.20*** (0.05)	–0.15*** (0.04)	–0.12*** (0.04)	–0.10*** (0.04)	–0.12*** (0.04)
Ln(Total Assets)	0.24*** (0.04)	0.21*** (0.06)	0.19*** (0.03)	0.23*** (0.03)	0.24*** (0.02)	0.22*** (0.03)
ROA	0.18 (0.15)	0.68** (0.29)	–0.01 (0.12)	0.00 (0.10)	–0.11 (0.10)	
Market-to-Book						0.17*** (0.03)
Partial Employment	–0.30*** (0.04)	–0.22*** (0.06)	–0.31*** (0.04)	–0.30*** (0.04)	–0.24*** (0.04)	–0.30*** (0.03)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Executive Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,978	6,395	13,594	14,088	11,170	16,406
R-Squared	0.88	0.91	0.87	0.88	0.90	0.88