Do Executive Compensation Contracts Maximize Firm Value? Indications from a Quasi-Natural Experiment

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Abstract:
We find significant positive abnormal returns surrounding a surprising and quick enactment of a law that restricts executive pay to a binding upper limit in a few industries. We find that the effect is concentrated only for firms in which the restriction is binding. We also find that the increase in value is greater for firms with weaker corporate governance and smaller for firms that grant a greater portion of equity-based compensation to their executives. These results provide indications that, on average, compensation contracts can be set in a way that does not maximize firm value.

Keywords: Executive compensation; governance; optimal contracts

JEL Classification: G30, G38, M12, M48, M52

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1. Introduction

There is considerable debate among academics and practitioners regarding executive compensation practices, which led to the emergence of two views (Frydman and Jenter, 2010). On the one hand, rent extraction theories note that executive compensation practices sharply contrast the predictions of traditional optimal contracting theories. These theories suggest that, on average, contracts are set suboptimally to enable executives to extract rents at the expense of shareholders, as the contract setting process by the board is "captured" by the executives (e.g., Bebchuk and Fried, 2003; Kastiel, 2015). On the other hand, modern value maximization theories claim that traditional compensation contract theories fail to explain observed contracts only because they do not take into account the specific features of the executive labor market. Modern value maximization theories suggest that once these features are accounted for, on average, executive compensation contracts are optimally designed to compete for talent in the market for executives (e.g., Gabaix and Landier, 2008; Edmans and Gabaix, 2016). This debate has important implications for different literature streams in economics, including those examining contract theory, corporate finance, corporate governance, labor economics, and income inequality. The debate also has significant policy implications, given that numerous proposals to limit executive pay have been promoted by both media commentators and politicians.

Despite the importance of this debate, Edmans and Gabaix (2016) point out that even the basic question of whether executive pay is, on average, optimally designed to maximize firm value has not been satisfactorily answered, as there are significant challenges to assigning causality. Specifically, compensation contracts are inevitably correlated with unobservable firm, industry, and executive characteristics, which in turn affect firm behavior, performance, and value (Edmans,

Gabaix, and Jenter, 2017). Therefore, Edmans and Gabaix (2016) note that the first-order task in this literature should be "to find good instruments for, or quasi-exogenous shocks to, CEO pay, to allow the identification of the effects of incentives." We do exactly that in this study and use an exogenous shock to provide some causal indications on this open question. To be clear, value maximization theories and rent extraction theories are not mutually exclusive in the economy. There are obviously some contracts that are set sub-optimally and allow rent extraction, and there are some contracts that maximize firm value. The challenge in this literature is to identify a setting that allows to make clear causal statements on the average outcome and then, like in most natural experiments, extrapolate from this setting.

Our setting utilizes the first time, to the best of our knowledge, a legislature in a developed economy passes a law that puts a binding restriction on total executive pay in certain firms.[1] On March 16, 2016, the Israeli Treasury Committee of the Knesset (the Israeli parliament) surprisingly and unanimously approved a law proposal to restrict the compensation of executives.[2] As discussed in detail in Section 2.2, if a proposal passes at the Treasury Committee with both coalition and opposition parties supporting it, the actual vote by the full Knesset body (i.e., the Israeli parliament) is just a formality (and as expected, the final vote occurred on March 29, 2016, without change). Therefore, the passage of the law in the Treasury Committee is the main event

[1] The law imposes an actual restriction on total pay and not only caps the tax deductibility of executive compensation, as exists in other countries. Importantly, the tax deductibility cap under the law is not binding because the limit on total pay is smaller than the effective tax deduction cap.

[2] For a full description of the dramatic and surprising meeting of the Treasury Committee, see (in Hebrew) http://www.calcalist.co.il/articles/0,7340,L-3683702,00.html. For journalist reporting on the bill see, for example, https://www.haaretz.com/israel-news/business/1.744507 and https://www.haaretz.com/israel-news/business/.premium-1.710605. We note that some international news agencies and newspapers erroneously only mention the tax sanctions on compensation exceeding the cap but fail to note the binding compensation cap. We view the inaccurate news articles as another indication of the surprising nature of the pay cap, which was introduced only in the final stage of the Treasury Committee's hearing.

we examine. Nevertheless, for completeness, we also examine all the dates that are associated with the passage of the law.

The final version of the law, as approved by the Treasury Committee, applies only to insurance, banking, and asset management firms (including parent companies of those firms), which we refer to as financial institutions. The law restricts total employer compensation costs (including but not limited to salaries, bonuses, share-based compensation, deferred compensation, and other benefits, excluding retirement compensation) to be not higher than 35 times that of the lowest-paid employee, including indirect employees such as employees of subcontractors (this translates to a ratio of up to 44 in net, after-tax, compensation). According to Bank of Israel data, at least 10 percent of bank employees are paid the Israeli minimum wage (approximately 72,000 ILS a year). Therefore, this restriction translates to an effective upper limit on total pay of 2.53 million ILS a year (0.66 million USD). Given that the average compensation of the highest-paid executives in the financial institutions subject to this law is 4.8 million ILS (4.7 million ILS median) before its enactment, this represents a significant pay cut for those executives.

The passage of the law is a unique quasi-natural experiment that allows us to examine the key differential prediction between the value maximization and rent extraction theories. According to value maximization theories, compensation contracts are optimally set to maximize firm value. Therefore, an outside restriction on these contracts, such as a limit on executive pay, is suboptimal and should lower firm value, as affected executives in our setting have alternative employment options. In contrast, rent extraction theories predict that a pay limit can reduce executive rent extraction, which should boost firm value. As the passage of the law in its restrictive form was a surprise and exogenous to the financial institutions, we use an event study methodology to examine how firm value changed in the short time window around its passage. This research design limits

the possibility that unobservable factors contribute to the changes in the firms' value.[3] Moreover, the fact that law limits executive pay in some financial institutions but not in others and the fact that non-financial institutions were unaffected allows us to perform additional tests that reinforce our causal interpretation. Given the significance of the question we examine and the inherent limitations of our setting, we view our investigation as indicative rather than providing definitive answers on the question.

We find that financial institutions experienced statistically significant 1.82% abnormal returns in the three days surrounding the approval of the law by the Knesset Treasury Committee.[4] As the Tel-Aviv 100 index (the main index for the Israeli stock market) did not change significantly during these days, the abnormal returns are driven primarily by the increase in the value of financial institutions. We conduct additional tests to buttress the causal interpretation of our results. First, we show that the positive effect of the approval of the law on firm value is concentrated among financial institutions bound by the pay limit, namely, firms that paid their executives more than the pay limit before the law enactment. These financial institutions experienced significant abnormal returns of 2.05%, compared to a statistically insignificant increase of 0.53% in the value of financial institutions for which the pay limit was not binding. Second, we examine the effect of the passage of the law on financial institutions that are not within the scope of the bill and find statistically insignificant abnormal returns for this subset of firms.[5] Third, we employ a sharp regression discontinuity design and examine the effect of the law on financial institutions that were just below or above the pay limit. We find that only firms paying just above the pay limit

[3] As in most capital markets event studies, we assume a reasonably efficient capital market, in which prices impound all available public information within a few days. Research on the Israeli stock exchange suggests that this is a reasonable assumption in our setting (e.g., Amihud et al., 1997; Kalay et al., 2002; Abudy and Wohl, 2018).
[4] We obtain similar inferences when we shorten the event window to two days or lengthen it to five days.
[5] The scope of the bill is limited to banks, insurance companies, asset management firms, mutual fund managers, ETF issuers, and their parent firms. Hence the bill does not affect firms operating in other financial segments, such as factoring and underwriting.

experienced significant abnormal returns surrounding the event window. These results suggest that, on average, rent extraction theories dominate in our setting. Our findings also limit the possibility that differential uncontrolled risk factors in our estimations explain the observed abnormal returns as the observed abnormal returns are significantly higher than any reasonable expected returns for a comparable event window. The results are consistent with the theoretical analysis in Thanassoulis (2014), arguing that a compensation cap can mitigate a competitive externality in the market for managers, leading to an increase in firm value.

There may be other alternative explanations for our findings. For example, if executives do not have viable employment alternatives, the law may simply transfer welfare from optimally paid executives to shareholders (by enabling a non-market mechanism—coordination—that reduces the reservation wage of the executives). Although lack of outside options is not likely to be the case in this setting, we perform several cross-sectional tests to rule out this and other unspecified alternative explanations. First, we find that the observed increase in firm value is greater for financial institutions with weak corporate governance. Specifically, we find that financial institutions with a proportion of independent directors below the sample median experienced higher abnormal returns around the event window than those with a proportion of independent directors above the median. Relatedly, we find that firms with a proportion of busy directors above the sample median experienced higher abnormal returns than those with a proportion of busy directors below the median. We also find that positive abnormal returns are lower when the executive compensation structure better aligns the interests of executives with those of shareholders. Specifically, we find that financial institutions that award equity-based compensation experienced lower abnormal returns around the event day compared to those that did not award executives with equity-based compensation. These results further support the rent

extraction theories and help rule out alternative explanations, as other explanations do not predict variation in abnormal returns based on corporate-governance and pay-structure characteristics.

For completeness, we perform eight more analyses. First, we calculate a rough estimate of the expected annual compensation savings per firm as a result of the law and find that these savings have a significantly positive correlation of 82% with the increase in firm value at the event window. Moreover, according to our estimates, the cumulative present value of the annual cost savings roughly equals the sum of the increase in firms' value. Second, we find that approximately 85% of the financial institutions in our sample experienced positive abnormal returns during the event window. Third, in placebo tests, we change the event window to periods where we do not expect abnormal returns. We find no significant abnormal returns in the treated and untreated groups in a three-day window just before the event date. These results suggest that momentum in returns or a reversal of returns due to prior unspecified events are not likely explanations for our results. Also, we find no significant abnormal returns for the financial institutions in the treated and untreated groups in a three-day window just after the event date. These results suggest that there was no reversal of the observed positive abnormal returns during the event window due to overreaction to the law or other reasons. Taken together, the results from the placebo tests suggest that the abnormal returns are concentrated in the event window.

Fourth, we show that all the other events that are associated with the passage of the law (e.g., preliminary vote on an earlier version of the bill without a compensation cap and formal enactment) generate insignificant abnormal returns. Fifth, we calculate a long-run CAR to examine the possibility of a reversal in the long-run and find a positive and significant CAR from the time the law was adopted to 180 days after its adoption. Therefore, we do not find evidence that is consistent with the reversal of the short-term returns. Sixth, we search and describe all other news

that were released on the financial institutions in our sample during our primary event window. We find that no other event likely caused the positive abnormal returns we observe. Seventh, we examine the effect of the law on executive departures in the affected institutions and on their performance. Interestingly, we find that the number of departures before the law is higher than the number of departures after the law. Our search yielded a list of 84 executive departures before the law, compared to 35 departures after it for the same time length.

We examine the CAR in the three days around the announcements of executive departures and find that the average CAR for these events is insignificant. Moreover, we do not find a significant difference between the CARs of executive departures before the law and after it. Lastly, we compare the performance of our sample firms before versus after the law using six different performance measures. We find that all performance measures improved after the law. This result is even more pronounced if one considers that the GDP growth—which strongly affects the performance of financial firms—decreased during this period.

Our results provide causal evidence consistent with compensation contracts, on average, being set in a way that does not maximize firm value. However, as in many natural experiments, our findings have limitations. Our experiment occurred in a relatively small developed market and derived from a relatively small number of firms. In addition, the fact that the regulatory intervention in pay occurred in Israel perhaps suggests that rent extraction was more severe in Israel than in other countries. Therefore, it is unclear whether our results can be generalized to other countries and industries. Although those concerns are certainly valid, several institutional factors lead us to believe similar results could arise in other settings. First, Israel is an OECD member, a developed economy that practices common law. These countries have been shown to have the strongest governance, as well as public and private institutions that protect minority

shareholders and enforce contracts (e.g., La Porta et al., 1998; Djankov et al., 2008). Moreover, corporate governance in Israel resembles that of the United States and other advanced economies (for example, Israel, too, has say-on-pay rules). Also, many Israeli firms, and in particular financial institutions, use international consulting firms to help design executive compensation contracts.

Second, Israeli financial institutions have an additional layer of monitoring compared to industrial firms. Israeli banks are supervised by the Bank of Israel, and Israeli insurance firms are supervised by the Capital Market, Insurance, and Savings Supervisor in the Israeli Finance Ministry. These regulators are recognized as some of the best in the world.[6] Third, Israeli financial institutions were among those that suffered the least in the 2008 financial crisis, suggesting that they are well managed and well governed. Moreover, Amir and Sharony (2017) find that the profitability of Israeli banks is statistically indistinguishable from those of banks in other advanced economies. Lastly, the fact that our sample, which represents all financial institutions in Israel, is relatively small makes it more difficult for us to reject the null hypothesis of no effect of the law. Despite the generalizability limitations, there are numerous published studies that use the advantages of the Israeli setting and analyze a small number of firms to draw conclusions on important questions that otherwise would have remained unexplored (see, for example, Schwartz-Ziv and Weisbach's 2011 study, which utilizes a sample of 11 Israeli government business companies).

Taken together, these factors suggest that, if anything, our findings supporting rent extraction theories may underestimate the effect in other countries or industries. Nevertheless, we acknowledge that the study suffers from external validity concerns. Therefore, although we believe this study contributes to the literature, we encourage readers and policymakers to consider the

[6] See, for example, https://www.centralbanking.com/central-banking/news/2481620/bank-of-israel-and-sama-triumph-in-this-year-s-central-banking-awards#cxrecs_s

differences among countries and industries when extrapolating our conclusions to other settings. At a minimum, this experiment provides evidence that executive compensation contracts in a developed, common-law country with a modern banking system can be, on average, designed in a way that fails to maximize firm value.

2. Motivation and institutional details

2.1. Motivation

Executive compensation is a heavily debated topic. Since at least as early as the 1950s, the media, public, politicians, and academic researchers have remarked on the high levels of CEO pay and questioned whether these levels are fair and appropriate (e.g., Murphy, 2012). At the same time, many commentators argue that executive compensation is determined in a free and competitive market and therefore represents optimal compensation.

The popular view that executive pay is excessive has led regulators worldwide to try to curb executive pay. In the United States, federal legislators capped the tax deduction on executive pay in 1993. The U.S. Securities and Exchange Commission mandated increased disclosure requirements on compensation in 2006. Say-on-pay legislation was passed as part of the Dodd-Frank Act in 2010, and the SEC has passed a rule requiring firms to disclose the ratio of CEO pay to median employee pay as a measure of within-firm pay disparity (Rouen, 2020). On the other side of the Atlantic, in 2013, the European Union capped bankers' bonuses at the level of their salary—or twice their salary if shareholders approve. In November 2013, Switzerland held an

ultimately unsuccessful referendum to limit CEO pay to 12 times the pay of the lowest-earning employee.[7,8]

Core and Guay (2010) argue that the popular resentment of executive compensation and the following legal actions appear to at least partly stem from a perception of growing income inequality. Frydman and Saks (2007) use the ratio of CEO pay to worker pay as a measure of income inequality. They note: "A comparison of executive pay to the earnings of a typical worker provides insight into the evolution of earnings inequality at the top of the income distribution." Reproducing data from Frydman and Saks (2007), Core and Guay (2010) show that CEO pay, relative to the average worker's pay, has increased sharply since 1970. Specifically, it has risen from a level of about 30:1 to approximately 120:1 by 2000. Reproducing the Piketty and Saez's (2003) findings, Core and Guay (2010) also show an increase of about 33% in the share of income earned by the top 10% of taxpayers during roughly the same period, suggesting a link between those trends. The academic debate on the two non-mutually exclusive views of executive compensation is thoroughly discussed in Edmans and Gabaix (2016) and Edmans et al. (2017). One side of the debate advances the rent extraction view, which claims that compensation practices

[7] An additional example in Europe includes the proposal of the former French president, Francois Hollande, to cap executive pay of state-owned firms at 20 times that of the lowest paid employee. There are other cases where regulations limited executive pay but these regulations are not suitable to examine a causal relation between pay and firm value. During the financial crisis of 2007–2009, limitations on executive pay were imposed. However, these limitations were partial and applicable only to financial institutions that received government support or were restricted to only one component of executive pay. This makes them problematic from a research design perspective. In the United States, the American Recovery and Reinvestment Act limited executive compensation at firms that received financial assistance from the Troubled Asset Relief Program (For a review of TARP limitations on executive pay, see, for example, Cadman et al., 2012). In Germany, the German Financial Markets Stabilization Act (from October 2008) restricted total annual executive compensation for all firms that received government aid from the stabilization fund to 500,000 euro (Dittmann et al., 2011). In 2009, the Financial Services Authority published the UK Remuneration Code, which required executives in large banks (and later on in financial institutions) to defer a larger portion of their bonus compensation and introduced performance vesting conditions for these bonuses to increase pay-performance sensitivity (Kleymenova and Tuna, 2020). Another example is pay restriction by the Chinese government in 2009 on state-owned enterprises (Bae et al., 2019)

[8] Albuquerque et al. (2019) show analytically that without a regulatory constraint on relative performance evaluation, a cap on total pay may backfire and will eventually cause an increase in banks' systematic risk and leverage. In Dittmann et al.'s (2011) model, a cap on total CEO pay has a small impact on firm value.

sharply contrast with the predictions of optimal contracting models (e.g., Bertrand and Mullainathan, 2000; 2001). Thus, contracts are not chosen by boards to maximize shareholder value but instead by the executives themselves to maximize their rents. This perspective is espoused most prominently by Bebchuk and Fried (2004). Similar views on executive rent extraction in closely controlled firms are discussed by Kastiel (2015), among others.

On the other hand, modern value-maximization theories reach a different conclusion. While the proponents of those theories acknowledge that standard agency models are inconsistent with practice, they argue that such models do not capture the specifics of the CEO setting since they were created as general frameworks for the principal-agent problem. For example, CEOs can have a very large effect on firm value. Thus, in a competitive labor market, it may be optimal to pay high wages to attract talented CEOs even though doing so requires paying a premium. These models aim to capture the specifics of CEO employment and can indeed generate predictions consistent with the data. Under this perspective, regulation will do more harm than good. This perspective is most prominently modeled by Gabaix and Landier (2008).

The literature also includes some justification for governmental involvement in setting executive pay. Acharya and Volpin (2010) and Dicks (2012) suggest that firms with weak governance (and hence higher compensation) impose a negative externality on better-governed firms through competition for managers, inducing inefficiently high levels of pay in all firms. In a similar vein, Thanassoulis (2012) analytically shows that in a competitive market, competition by banks for executives generates a negative externality that increases the default risk of the competing banks. Therefore, an optimal financial regulation would intervene in the bankers' labor market and impose a cap that is proportional to the banks' balance sheet. Subsequently, Thanassoulis (2014) analyzes a regulatory pay cap that is proportional to the banks' assets and

demonstrates that such a cap mitigates the competitive externality in the labor market. Thus, the cap is expected to lower banks' executive compensation while preserving allocative efficiency, leading to a rise in banks' value and a reduction in their risk.

Israel does not differ from the trends observed around the world and exemplifies the issues discussed above. A rise in inequality in Israel spurred a populist move to curb executive pay. Politicians strongly argued that executives earn too much at the expense of employees and consumers. This upsurge in populist sentiment led the Israeli parliament to enact a law aimed at curbing executive pay in financial institutions. The first draft of the bill, which was approved in two preliminary votes by the Knesset, introduced a tax deduction cap, which resembles the ineffective tax deduction cap used in other countries. However, in a surprise move, the Treasury Committee of the Knesset introduced and passed a revised version of the bill limiting total executive pay. We discuss the details of the passing of the law in the following section.

More importantly for us, the surprising nature of the law's passage creates a unique research opportunity. The major limitation of examining the different views on executive compensation is that executive compensation is endogenously determined. This limits the usefulness of any cross-sectional or time-series examination of the relation between executive pay and firm value. Hence the first-order question in this literature—whether executive pay is, in fact, set to maximize firm value—remains unanswered (Edmans and Gabaix, 2016). Israel's pay limit is an exogenous shock to financial institutions' executive compensation contracts and therefore allows us to overcome many of the challenges in prior research.

2.2. Institutional details

Laws in Israel are enacted following a preliminary vote in the Knesset, discussions and a vote in the relevant Knesset's parliamentary committee, and two additional votes in the Knesset.

The preliminary vote serves as an initial screening. A bill that receives a majority in that vote is then directed to one of the Knesset committees where it is discussed and prepared for the second and third votes in the Knesset.

The preliminary vote on the "Executive Compensation in Financial Institutions" bill in the Israeli Knesset occurred on July 28, 2014. We term this vote "Event 1." The initial draft of the bill stated that executive compensation exceeding 3.5 million ILS (0.9 million USD) would not be tax deductible for financial institutions. The bill was approved in the preliminary vote with a majority of 24 in favor and 0 against. A tax deduction cap on executive pay exists in several countries, including the United States, and has been shown to be generally ineffective as firms usually bear the tax consequences of the regulation or can avoid the tax implications altogether (e.g., Murphy, 2012).

Following the adjournment of the 19th Knesset on December 3, 2014, and the subsequent elections, the bill was reintroduced with no significant changes on May 4, 2015, by two other *opposition* members of Knesset (hence the reintroduction was not supported by the coalition, nor by the government). Subsequently, the bill was approved in another preliminary vote on November 9, 2015, with 25 votes in favor and 0 against. We term the bill re-initiation "Event 2" and the second preliminary vote "Event 3." On January 4, 2016, the Knesset's Treasury Committee had the first discussion on the bill. We term this discussion "Event 4." The material issues discussed in the meeting include some committee members advocating to (1) broaden the scope of the bill to all public firms, not just financial institutions, and (2) lower the threshold of the tax deductibility of executive compensation. On February 15, 2016, the Knesset's Treasury Committee reconvened to discuss the bill. We term this discussion "Event 5." The material issues discussed in the meeting include some committee members advocating to (1) broaden the scope of the bill to all public

firms, not just financial institutions, (2) impose the tax burden on excess executive compensation on the receiving executive rather than the awarding firm, and (3) lower the threshold of the tax deductibility of executive compensation to 2.5 million ILS (0.7 million USD) or even to 0.8 million ILS (0.2 million USD). The meeting adjourned without any agreements among the committee members.

On March 16, 2016, the Knesset's Treasury Committee reconvened for a final discussion and a vote on the bill. We term this discussion and vote "Event 6." In this meeting, the committee surprisingly introduced and approved a pay limit for the first time. During the discussions, all members of the committee agreed on a more restrictive bill.[9] Most importantly, instead of a 3.5 million ILS (0.9 million USD) tax deduction cap, the committee agreed on a binding pay limit of 35 times the gross salary of the lowest-paid employee at the firm (or 44 times the lowest-paid employee's after-tax annual compensation). Firm employees include both direct employees and personnel employed indirectly through outsourcing firms. In addition, if the firm is part of a business group, such as a holding company, or belongs to a group of firms with a joint major shareholder, the compensation cap applies to the compensation from all the firms that belong to the group. The minimum annual wage in Israel is 72,000 ILS (18,947 USD), which implies an effective executive compensation cap of 2.5 million ILS (0.66 million USD). The compensation under the scope of the bill consists of both monetary and nonmonetary components, including salaries, bonuses, share-based compensation, deferred compensation, and other benefits, excluding

[9] Some opposition members of the Treasury committee proposed to restrict the tax deductibility of executive compensation to 0.8 million ILS in Event 5. If market participants expected that proposition to have an adverse effect on firm value and viewed the version that eventually passed on March 16 as an improvement, the positive returns that we observe may be driven by a partial reversal of the market's expectations following Event 5. That seems unlikely for two reasons. First, we do not observe a significantly negative market reaction around the events that preceded Event 6. Second, the tax savings resulting from increasing the tax deductibility cap form 0.8 million ILS to 2.5 million ILS do not explain the magnitude of the positive abnormal returns we observe around Event 6.

retirement compensation.[10] The committee also decided to limit the scope of the bill to financial institutions, as previously proposed. Note that if a bill is approved unanimously in the Treasury Committee by all coalition and opposition members, the official vote on the law by all members of the Knesset is a formality. Because of the surprising nature of this event and the complete support for the law, which sealed its passing, the committee vote serves as our primary event.

Given the unanimous support of the bill in the Treasury Committee by all coalition and opposition members, the bill's approval by the Knesset in a second and third and final vote on March 29, 2016, was only a formality. We term these votes "Event 7." The bill received unanimous support with no amendments on both votes, with 56 in favor and 0 against. The effective date of the bill was January 1, 2017.[11]

3. Research design

We conduct an event study to test the market reaction to the main event (the unanimous vote by the Treasury Committee, i.e., Event 6). Since the events are clustered in their effect on institutions, we expect the error terms to be correlated across firms. Therefore, following the literature, we aggregate all financial institutions into a single portfolio. Using this portfolio, we estimate the following model:

$$R_{p,t} = \alpha + \beta R_{m,t} + \gamma D_t + \varepsilon_t, \quad (1)$$

[10] In addition, executive compensation that is below the cap but more than 2.5 million NIS (0.66 million USD) is not tax deductible and requires the approval of (1) the compensation committee, (2) board of directors, (3) a majority of outside or independent directors, and (4) the shareholders at the annual shareholders' meeting.

[11] The law was scheduled to enter into effect in October 2016. However, on June 1, 2016, the Association of Banks in Israel filed a petition to the High Court of Justice against the law (see http://www.globes.co.il/en/article-israeli-banks-petition-high-court-against-pay-curbs-1001129235). The petition was dismissed, subject to a ruling that past compensation rights of banks' executives will not be violated. In addition, the High Court of Justice postponed the law's effective date to January 1, 2017, to allow executives to prepare for the law's consequences (see https://www.themarker.com/markets/1.3083793, in Hebrew). We also examine the market reaction around the major events related to this petition (untabulated). The results are consistent with the rent extraction view.

where $R_{p,t}$ is the equal-weighted portfolio returns on day t.[12] We use a total of 432 trading days, beginning 10 trading days before event 1 (i.e., the preliminary vote on July 28, 2014) and ending 10 trading days after the last event (i.e., the passage of the bill on the second and third votes on March, 29, 2016, or Event 7). $R_{m,t}$ is the Tel-Aviv 100 return index on day t. The Tel-Aviv 100 index is the weighted index of the largest 100 firms on the Tel-Aviv Stock Exchange (TASE) based on market capitalization.[13] The total market capitalization of firms in the index is approximately 189.61 billion USD, compared to (1) a total market capitalization of 23.28 billion USD for all financial institutions in our sample and (2) a total market capitalization of 201.682 billion USD for all firms publicly traded on the Tel Aviv Stock Exchange (TASE).

D_t is an indicator variable equal to one on any one of the three days surrounding Event 6, from (t-1) to (t+1), and zero otherwise. The coefficient on D_t is our coefficient of interest. A negative coefficient on D_t supports optimal contracting theories, while a positive coefficient on D_t supports rent extraction theories. Lastly, ε_t is the error term. Following the literature, we employ two different specifications for the standard errors: (1) Huber-White and (2) unadjusted. We also follow Campbell, Lo and MacKinlay (1997, p. 160) and report p-values for the significance of abnormal returns using a conditional covariance matrix that fits an event study using portfolio returns with a single date.

4. Sample selection and data

Our tests are limited to financial institutions that fall under the scope of the executive compensation law. We identified a total of 20 such financial institutions on the Tel-Aviv Stock

[12] Inferences are unchanged if we use value-weighted portfolios. We do not use value weighted portfolios in our main analyses as they may bias inferences because both pay and the expected effect on firm value are correlated with size.
[13] In February 2017, the Israeli stock exchange revised its indexes, and the Tel-Aviv 100 index became the Tel-Aviv 125 index.

Exchange (TASE). Eight institutions are classified as banks and seven as insurance firms. Four additional institutions are classified as investment firms, and one of the institutions is a holding company of an insurance firm. Industry classification and returns data are obtained from the TASE website. We obtain executive compensation data from the annual reports of our sample firms. These reports provide information on the five highest-paid executives in each of the financial institutions in our sample as well as information on the directors in those firms, which we use in our corporate governance analyses.

Since the events related to the passage of the bill are expected to affect all financial institutions, we create daily portfolios of all publicly traded financial institutions. Our sample period spans from July 15, 2014, to April 12, 2016. Consequently, our sample size in all the analyses is 432 days (representing each trading day in our sample).

Table 1 provides the descriptive statistics for the institutions represented in our sample. The median maximum total compensation is 4.7 million ILS (1.2 million USD). The 25^{th} percentile of maximum total compensation is 3.3 million ILS (0.875 million USD), which implies that more than 75% of the institutions in our sample were required by law to lower their maximum executive compensation in 2017. The mean market capitalization of financial institutions in our sample is 4.5 billion ILS (1.1 billion USD), compared to a mean of 3.4 million ILS (0.9 million USD) for all firms traded on the TASE. The average market-to-book ratio is 1.128, consistent with the low market-to-book ratios of financial institutions observed in the United States. The average proportion of independent directors is 0.308, lower than the average observed in the United States and comparable to the proportion for all firms in the Tel Aviv Stock Exchange (Weiss 2011). The mean proportion of busy directors is 0.518, consistent with the results of Fish and Shivdasani (2006) for US firms. Lastly, the mean equity-pay to total-pay ratio in our sample is fairly small,

merely 0.084, with a median of 0. This implies that executive compensation in our sample firms consists mostly of cash and other short-term components.

5. Results

5.1. Main results

The main results for the market reaction to the unanimous vote in the Treasury Committee (Event 6) are presented in Table 2. For brevity, we discuss only the results with the Huber-White standard errors (column 1). The mean equal-weighted return in the three days surrounding the event and after controlling for the market return is 0.606 (*t*-statistic = 2.16). This implies that the share value of financial institutions increased by a total of 1.818% (0.606*3 = 1.818) in the three days surrounding the unanimous vote in the Treasury Committee.[14] Although 1.818% is an economically significant abnormal return, it is not too large to be infeasible. The *p*-value for the 3-day CAR using Campbell, Lo, and MacKinlay's (1997, p. 160) methodology is 0.033. Overall, the results in Table 2 suggest that investors view the compensation cap as, on average, value increasing. This suggests that investors in financial institutions in Israel associate the companies' executive compensation more with rent extraction than with value maximization.

5.2. Additional primary tests

We perform additional tests to enhance the causal interpretation of our results. In Table 3, we partition the sample of financial institutions based on the expected impact of the legislation on them and examine the market reaction to the main event (Event 6). To the extent that the executive compensation limits are, on average, value increasing for shareholders, we expect financial

[14] We obtain similar inferences when we use a sample limited to banks and when the sample is constrained to insurance firms. Moreover, the results remain qualitatively similar when we exclude from the sample three banks that were subject to an investigation by the U.S Department of Justice for past actions of their foreign subsidiaries.

institutions that award executive compensation above the new legislative limit to experience a stronger market reaction compared to those awarding compensation below the new limit and compared to those unaffected by the restriction.

We present the results from estimating Eq. (1) for financial institutions with maximum executive compensation above 2.5 million ILS (0.66 million USD) in Panel A. Since the results are similar across the two specifications we use, for brevity, we only discuss the results in column 1. The average daily abnormal return in the three days surrounding the main event is 0.682 (*t*-statistic = 2.32; the *p*-value using the Campbell et al. (1997) method is 0.027). This implies that the value of institutions awarding executive compensation above 2.5 million ILS (0.66 million USD) increased, on average, by 2.05% (0.682%*3) in the three days surrounding the main event, after controlling for the market return.

In Table 3 Panel B, we present the results from estimating Eq. (1) for institutions with maximum executive compensation below 2.5 million ILS (0.66 million USD). As before, since the results are similar across both specifications, we discuss the results in column 1. The average daily increase in firm value in the three days surrounding the main event is 0.176 (*t*-statistic = 0.39; the *p*-value using the Campbell et al. (1997) method is 0.723). This implies that the value of institutions awarding executive compensation below 2.5 million ILS (0.66 million USD) increased by 0.528% (0.176%*3) in the three days surrounding the main event, significantly lower than the increase in the value of firms awarding executive compensation above 2.5 million ILS (0.66 million USD). Specifically, the difference in the daily abnormal returns around the event date for

firms that award maximum executive compensation in excess and below the legislative cap is 0.506% and is statistically and economically significant (*p*-value = 0.007).[15]

We present the results for eight financial firms from sub-industries that are not within the scope of the law in Table 3 Panel C. Since these firms are not within the scope of the law, we do not expect to find a significant market reaction around the main event. Consistently, we find a statistically insignificant average daily increase of 0.220 (*t*-statistic = 0.66; the *p*-value using the Campbell et al. (1997) method is 0.836) in the value of the portfolio consisting of those firms. The difference in daily abnormal returns for firms awarding executive compensation above the legislative cap and are within the scope of the law and financial firms that are not within its scope is 0.462% (*p*-value = 0.10). Overall, the results in Table 3 suggest that the positive abnormal returns around the main event are concentrated in firms that are forced to lower their executive compensation levels to conform to the law.

In Table 4, we present the results from estimating Eq. (1) for a subset of financial institutions awarding executive compensation immediately above and below the 2.5 million ILS (0.66 million USD) threshold. We implement this approach to facilitate a better-identified research design.[16] In Panel A, we present the results for a subset of four institutions awarding executive compensation above 2.5 million ILS (0.66 million USD) and below 4 million ILS (1.05 million USD). The results show a positive and significant market reaction to the unanimous vote for the

[15] We calculate the statistical significance of the difference in the coefficients across the two subsamples using a Monte-Carlo non-parametric simulation technique where we randomly assign observations from the two subsamples into two partitions and take the difference between the coefficients 200 times to create a distribution of the differences (see Edgington and Onghena, 2007).

[16] We implement a sharp regression discontinuity design subject to the constraints arising from our small sample size. We examine the appropriateness of the regression discontinuity design by comparing the means for multiple descriptive variables between the firms that award maximum executive compensation this is just above and just below the legislative cap. We find that, except for the proportion of busy directors, there is no statistically significant difference in the means between the two groups. However, the lack of significance could be a result of the low number of observations.

approval of the bill in its restrictive form in the Treasury Committee. The average daily abnormal returns in the three days surrounding the main event are 0.434 (*t*-statistic = 4.17; but the *p*-value using the Campbell et al. (1997) method is 0.239), which implies an average increase in firm value of 1.302% (0.434%*3) in the three days surrounding the event. The positive reaction is smaller than the reaction for all institutions awarding executive compensation above 2.5 million ILS (0.66 million USD), which is presented in Table 3 Panel A, where the average daily abnormal returns in the three days surrounding the main event are 0.682 (*t*-statistic = 2.32). These results provide further support for the conclusion that the market reaction to the main event is more favorable when the impact of the executive compensation cap is greater.

We present the results for a portfolio consisting of a subset of three financial institutions awarding executive compensation below 2.5 million ILS (0.66 million USD) and above 1 million ILS (0.26 million USD) in Table 4 Panel B.[17] The results show no significant market reaction to the main event. Average daily abnormal returns in the three days surrounding the unanimous vote for the approval of the bill in its restrictive form in the Treasury Committee are 0.176 (*t*-statistic = 0.39; the *p*-value using the Campbell et al. (1997) method is 0.723). Since the highest executive compensation awarded by the three firms in this subsample before the law is below the legislative cap, the results are consistent with investors not expecting to see a further decrease in executive compensation in those firms. Overall, the results in Tables 3 and 4 are consistent with investors' belief that the prevailing executive compensation contracts in financial institutions in Israel reflect rent extraction.

[17] This subset of firms is identical to the one in Table 3 Panel B, but we repeat the analysis here for the ease of the reader.

5.3. Cross-sectional tests

Our findings may prompt alternative explanations. For example, if the market for executives is not well developed and the executives do not have viable employment alternatives, the law may simply extract welfare from optimally paid executives and transfer it to shareholders. However, several institutional factors make this explanation unlikely. First, the law is imposed on insurance, banking, and investment firms and not on the entire financial industry, nor firms in other industries. Credit card issuers, private equity funds, and hedge funds, for example, are not subject to the law, and neither are the vast majority of Israeli firms. In addition, the law does not apply to subsidiaries of financial institutions engaging in other financial activities, such as investment banking and underwriting. Moreover, subsidiaries operating outside of Israel are considered foreign firms and are not subject to the law.[18] While the law is relatively new, there are already examples of executives who resigned from the banks but remained with their unaffected foreign subsidiaries.[19] There are also examples of executives in affected firms who move to non-financial firms that provide their executives with generous pay.[20] Hence outside options exist both within and outside the financial industry. Lastly, as many executives of Israeli financial institutions have experience in the U.S. and other foreign financial institutions, foreign companies could also serve as an outside option. We examine the changes in executive turnover following the law in Section 5.5.

[18] Examples of foreign banks owned by Israeli banks include Bank Leumi USA, IDB Bank (located in the United States), and Leumi ABL, located in London. Despite the fact that subsidiaries of banks are exempt from the law, we learned from conversations with lawyers that it is illegal for executives to hold a position in the Israeli firm but be registered as a foreign employee to receive an unrestricted compensation. The legal experts we talked with did not see a way in which this law could be circumvented.

[19] One example is Dani Zidon, who resigned from his position as the deputy CEO of Bank Leumi in April 2016, but remained the board chairman of Leumi partners (the investment banking subsidiary of Bank Leumi, which is not subject to the law) and as a board member in Leumi USA. It was also announced he is considering joining a private equity fund (and private equity funds are not subject to the law). Another example is Erez Goldsmith, who left his position as the CEO of IBI to start his own underwriting business, citing the pay limit as the reason.

[20] See, for example, https://www.calcalist.co.il/markets/articles/0,7340,L-3765475,00.html (in Hebrew).

Nevertheless, to rule out this alternative explanation and other unspecified alternative explanations, we perform a series of cross-sectional tests.[21] In our first set of tests, we build on prior studies that document that weak corporate governance is associated with management rent extraction (Core et al. 1999). Therefore, if the reduction in rent extraction is the reason of the observed positive abnormal returns around the main event date, then the positive market reaction following this legislation is likely to be stronger for financial institutions with weaker corporate governance. We examine two corporate governance characteristics: board independence and director busyness.

We present the results from estimating Eq. (1) for financial institutions with a proportion of independent directors below (above) the sample's median value in Table 5 Panel A. Our definition of independent directors follows the definition in the Israeli Companies Act. We find that the market reaction for financial institutions with low board independence is economically and statistically significant. The coefficient estimate is 0.759 (representing 2.28% abnormal return in the three days surrounding the main event), and the *t*-statistic is 2.45 (column 1; the *p*-value using the Campbell et al. (1997) method is 0.011). Conversely, we find statistically insignificant results for financial institutions with strong board independence. The coefficient estimate is 0.453, and the *t*-statistic is 1.44 (column 3; the *p*-value using the Campbell et al. (1997) method is 0.194).

We examine the market reaction to the legislation for financial institutions with low (high) director busyness in Table 5 Panel B. Director busyness is defined as the fraction of directors serving on three or more boards. We find that there is a stronger market reaction for firms with busy board members, which proxy for lower monitoring ability. The three-day abnormal returns

[21] We examine the cross-sectional correlation between the three variables we use to partition the sample in our cross-sectional tests and find that none of these correlations is high or statistically significant.

surrounding the main event are 2.83% (0.943%*3, *t*-statistic = 3.66; the *p*-value using the Campbell et al. (1997) method is 0.004) for firms with busy boards. Conversely, the three-day abnormal return for financial institutions with a proportion of busy board members below the median is statistically insignificant (coefficient estimate = 0.269; *t*-statistic = 0.82; the *p*-value using the Campbell et al. (1997) method is 0.405). The difference in the coefficients for firms with low and high director business is 0.674 and is economically and statistically significant (*p*-value = 0.031) Overall, the results from the cross-sectional tests that exploit corporate governance characteristics are consistent with the predictions of rent extraction theories on executive pay.

In our second set of cross-sectional tests, we examine the effect of executive pay structure on the observed positive abnormal returns around the main event date. Prior studies find that equity-based pay better aligns the interests of managers and shareholders. Therefore, rent extraction is less likely when executives are paid with equity. In this test, we compare the market reaction for firms that grant equity-based compensation and firms that only award cash pay. We present the results in Table 6 and find that the positive market reaction to the new legislation is stronger among financial institutions that only granted cash pay. The three-day abnormal return is 2.37% (0.790%*3; *t*-statistic = 2.86; the *p*-value using the Campbell et al. (1997) method is 0.0202). In contrast, the three-day abnormal return for financial institutions that granted equity-based pay is 0.792% (0.264%*3) and is statistically insignificant (*t*-statistic = 0.88; the *p*-value using the Campbell et al. (1997) method is 0.410). The difference in daily abnormal returns in the two subsamples is 0.526 and is economically and statistically significant (*p*-value = 0.035). These results provide further evidence in support of the rent extraction theories. Taken together, these results also suggest that alternative explanations are unlikely, as other explanations do not predict

variation in abnormal returns around the event based on corporate-governance and pay-structure characteristics.

To complete the aforementioned analysis, we also compare firms' characteristics between the subsamples in each of the three cross-sectional cuts. The untabulated results indicate that, generally, the differences between the subsamples for each cross-sectional cut are not statistically nor economically significant. An exception is a significant difference is the ROE in the cross-sectional split based on busy directors, where above-median firms have higher ROE than below-median firms. However, the difference in a second profitability measure, ROA, is insignificant in the same split, and therefore, the interpretation of this difference is unclear. A second significant difference is in market cap in the cross-sectional split based on whether firms grant equity-based pay. Firms that grant equity-based compensation have larger market caps than firms that grant only cash compensation. This is consistent with the higher, albeit not statistically significant, compensation paid by the former group. However, the market response to the law was not significant for firms that grant equity-based pay (even though they are expected to record larger savings). We believe that this evidence reinforces our inference because performance-based compensation is less likely to be the result of rent extraction.

Lastly, we examine whether prior stock performance affects shareholders' response to the law. To the extent that weaker governance or ineffective compensation resulted in lower stock returns prior to the law, we would expect a more positive market reaction to the approval of the law for firms with lower returns prior to the main event. To test this, we partitioned the sample into two groups based on the stock return for the year ending 10 days before the main event. Untabulated results show that firms with below-median prior stock returns demonstrate a

significant and positive response to the law, compared to firms with above-median prior returns, which showed an insignificant reaction to the law.

The results of this subsection indicate a negative relation between governance and the change in firm value as a result of the enactment of the law. That is, the increase in value is greater for firms with weaker corporate governance. However, it should be noted that these tests are based on the premise that better-governed firms paid lower excusive pay, and therefore we should expect fewer savings and a smaller market reaction for those firms.

5.4. Further robustness analyses

In this section, we discuss further analyses we conducted to enhance confidence in our results. First, we present the cumulative three-day abnormal returns for each of the financial institutions in our sample around Event 6 in Table 7. The CAR estimation and the test of its statistical significance is conducted using the Kolari and Pynnonen (2010) methodology.[22] The table reveals that out of the 20 financial institutions, 16 had a positive CAR during the event period, and four had a negative CAR. Out of the four financial institutions with the negative CAR, one institution was not bound by the law. The findings in Table 7 provide comfort that our results are not likely driven by a small subset of financial institutions or by the error structure of the data.

Second, we estimate the closeness and correlation between an estimate of the expected annual compensation savings and the increase in the market value of the financial institutions. We estimate the annual expected cost savings for each financial institution, assuming that the

[22] Kolari and Pynnonen (2010) methodology corrects for the possible bias due to the dependence in standard errors at the event study at the firm level. Kolari and Pynnonen (2010) propose a new *t*-test statistic that takes into account both cross-correlations among abnormal returns and inflation in the event-date variance. They show that their statistic performs better than those in prior studies.

compensation of executives earning more than the cap will be set equal to the cap. Hence the estimated annual savings of each financial institution is the difference between the actual compensation of the five highest-paid executives in 2015 that were paid more than the legislative cap, as disclosed in the firms' annual reports, and the compensation of those executives adjusted to the cap. The mean and median estimated annual saving for our sample firms is 5.6 million ILS. Three firms in our sample are not expected to record any savings since none of their executives reached the compensation cap in 2015. Conversely, in nine firms, all five highest-paid executives exceeded the cap in 2015. Since firms are obligated to disclose the compensation of only the five highest-paid executives, there may be additional executives in those firms earning more than the cap in 2015. If so, our estimation of the savings is biased downward. Another caveat is that our estimate does not account for the effect of the law on lower-ranking executives, whose compensation may also be adjusted to maintain a sensible pay hierarchy within the firm. Nevertheless, we believe our calculation provides a rough estimate of the firm-specific annual cost savings following the law. To calculate the total value of savings per firm, we compute the value of an annuity consisting of the annual saving using a cost of capital of 7%.[23] The cumulative present values of savings for the entire sample amounts to 1,167 million ILS.

Next, we compute the change in market value for each financial institution by multiplying its abnormal return in the three days surrounding the approval of the bill at the Treasury Committee by its market value just before the event. In our sample, the mean (median) increase in market value is 83 (33) million ILS. We find that the sum of the changes in firms' value using the CAR

[23] Practitioners commonly use a cost of capital of 7% for the financial industry during this time period. To gauge the reasonableness of this assumption we use a beta of the industry of 0.8 and a risk-free rate of 1.5% based on Israel's long term government bonds, and a risk premium of 7% for the Israeli market based on Damodaran's website (all parameters are as of the beginning of March 2016). These parameters yield a cost of capital of 7% as well. Our results are qualitatively unchanged if we use an interval of +/- 2% around the 7% point estimate.

approach at the event date amounts to 1,332 million ILS. This amount is reasonably close to the estimated savings detailed above (the savings amount is 88% of the increase in firms' value). Moreover, focusing on the median firm with respect to its market value of equity, such firm has a market value of about 2 billion ILS (the average of Menorah and Phoenix, the 10th and 11th firms in the sample). The average compensation saving of these median firms is 76 million ILS, whereas the change in their market value was 73 million ILS, which are comparable. We also find that the Pearson correlation between firms' specific expected compensation cost savings and their change in market value is 0.819, and is statistically significant. This correlation suggests that the positive market response to the law is strongly associated with the cost reduction it is expected to generate and provides additional evidence of investors' favorable view of the pay cap.

Third, in a series of placebo tests, we change the event window to periods when we do not expect to observe abnormal returns. In our first placebo test, the event window ends two days before the first day of our main event window.[24] We find no significant abnormal returns for financial institutions in the treated and untreated groups in the three-day window just before the event date.[25] These results suggest that momentum in returns or a reversal of returns of a prior unspecified event do not likely explain our results. In our second placebo test, we change the event window so that the first day of the event window starts two days after the last day of the main event window. We find no significant abnormal returns for financial institutions in the treated and untreated groups in the three-day window just after the event date. These results suggest that there was no reversal of the positive abnormal returns observed around the main event. Taken together,

[24] In the placebo tests, we keep one extra day between the real event date and the placebo dates to avoid confounding the real event window with the placebo window.

[25] We find statistically significant but economically negligible (-0.5%) abnormal returns for the treated institutions. These returns are 3.5 times lower (in absolute value) than the positive abnormal returns of 1.8% we observe for the treated institutions during the event window (documented in Table 3 Panel A).

the results from the placebo tests suggest that the abnormal returns are concentrated in the main event window.

Fourth, we calculate the long-run CAR to examine the possibility of a reversal in the long run. The results are presented in Table 8. Denoting day *t* as the day in which the law was adopted, we run two specifications: in the first, we introduce a dummy variable that equals 1 from day *t*-1 to day *t*+180, and zero otherwise. In the second specification, we include a dummy variable that equals 1 from day *t*+1 to day *t*+180, and zero otherwise. Column 2 (Column 4) of Table 8 suggests an average daily increase of 0.125% (0.117%) from the day before (after) the law was adopted to 180 days after its adoption. These results are inconsistent with a reversal of the short window returns. We caution the reader against putting too much weight on these results as long term event window tests are susceptible to confounding events (for example, in our case, the Bank of Israel allowed institutional investors to increase their holdings in banks from 5% to 7.5%, as detailed in footnote 13).

Fifth, we estimate Eq. (1) for all other events related to the executive compensation law and described in Section 2. Untabulated results show that, although the abnormal returns during all the events that relate to the passing of the law are positive, the only consistently statistically significant and economically meaningful abnormal returns are around Event 6, which is our main event window. This result is consistent with investors incorporating the news of the unanimous vote for the approval of the bill in the Treasury Committee into firms' stock prices.

Moreover, we examine whether limiting executive pay has a spillover effect to Tel-Aviv 100 index firms that are not within the scope of the law. We find that the Tel-Aviv 100 index did not change significantly during the event window: over the three days surrounding the law's approval at the Treasury Committee, the index declined by -0.5%. Therefore, it seems that there is

little evidence of a spillover effect. This finding is consistent with the mixed expectations regarding a spillover effect that prevailed in the market at the time of the enactment.

Finally, to ensure that our results are not driven by a confounding event, we search for other news related to the affected financial institutions that occurred in the three days surrounding the main event. The outcome of this analysis is presented in Table 9. We searched for filings made by our sample firms in the three days surrounding our main event. Two firms issued their annual reports in that window—Bituach Yashir and Meitav. The average market reaction of both of those firms in the three-day event window is below the overall average for all firms in our sample. Therefore, we do not believe that the release of these annual reports is driving our results.

We also searched on Google for the words “banks” and "insurance", in Hebrew, with a date restriction of March 14 through March 18, 2016. Our assumption is that any significant news related to the financial industry would appear in our search. We identified a few relevant articles that are unrelated to the main events. The first article is about banks in Israel being stable but inefficient.[26] It is unclear whether this article should induce positive returns. In addition, our sample includes insurance firms, not just banks. A second article states that Deutsche Bank closed its trading division in Israel.[27] Again, this article is limited to banks, and it is not clear whether such news should result in a positive or a negative market reaction.

In addition, we searched for all articles (not limited to any specific term) in the Israeli financial website *The Marker* with a date restriction of March 14 through March 18, 2016. We identified an article on the increased competition in the pension management industry, which

[26] http://www.ynet.co.il/articles/0,7340,L-4780179,00.html
[27] http://www.calcalist.co.il/markets/articles/0,7340,L-3683645,00.html

would suggest negative returns.[28] We also identified an article that details the executive compensation in 2015 based on firms' annual disclosure.[29] This article is a summary of firm-specific disclosures that were already released to the market. Lastly, we identified an article claiming that the two leading banks in Israel (Poalim and Leumi) may be able to issue credit cards, but not to their clients. This is part of the discussions by regulators to require banks to sell off their credit card businesses. Since our sample is not restricted to those two banks, we do not believe that this event is driving our results.[30]

5.5. Consequences of the law

By the time the law came into effect, all the financial institutions that previously awarded their executives with compensation that exceeded the pay cap had modified their compensation structure. According to public disclosures, the new compensation structure is comprised mostly of cash compensation. Furthermore, the highest cash compensation is set to 35 times the lowest salary paid by the financial institution. Third, bonuses on performance are still available, but their scope is limited.[31] Lastly, there are some reports that the compensation structure of the entire organization has been revised as well, and even lower-level managers, whose salary was below the pay cap, suffered from a decrease in their salaries (the pay cut is mostly in the performance-based compensation, but cash compensation also declined).

Since the law came into effect on January 1, 2017, we provide additional insights regarding its consequences. First, we examine how the law affected departures of executives from the affected financial institutions. This test aims to examine whether the law triggered a mass of

[28] http://www.themarker.com/news/1.2885575.
[29] http://www.themarker.com/markets/reports/1.2883434.
[30] http://www.themarker.com/markets/1.2881721.
[31] See, for example, http://www.globes.co.il/en/article-leumi-cuts-exec-pay-as-new-law-bites-1001154435.

resignations. To analyze the effect of the law on executive departures, we compare departure patterns before versus after the law, based on hand-collected data from immediate filings (similar to 8-k filings). In order to compile a list as comprehensive as possible, we reviewed all immediate filings made by the sample firms and identified reports on departures. Notably, since the reason for resignation is rarely states, and even when there is some information about the reason (for example, in interviews or press coverage) we cannot determine its reliability, we document every departure that was reported in an immediate filing. The period that we consider for departures before the enactment of the law is from January 1, 2013, to January 31, 2016. The period we consider for departures after the enactment of the law is from March 30, 2016, to October 7, 2019.

Interestingly, the number of departures before the law is higher than the number of departures after the law. Our search yielded a list of 84 executive departures before the law, compared to 35 departures after the law for roughly the same time length. This finding suggests that the rate of executive departures from financial institutions did not increase following the law.

Furthermore, we analyze the market reaction to these departures in several ways. First, we calculate the CAR for each departure using the standard event study methodology. Specifically, we employ the market model to compute abnormal returns in the three-day window surrounding each immediate filing (*t-1* to *t+1*). The beta for each event (i.e., departure announcement for each executive) is estimated by regressing firm's returns in the year preceding the departure on the market returns. Column (1) of Table 10, Panel A, presents the average CAR around executive departure announcements. The results indicate that the market reaction to the announcements of executive departures is insignificant both before and after the enactment of the law. Mean CAR around departure announcements before the law is -0.14% (*p*-value = 0.999), while mean CAR after the law is -0.30% (*p*-value = 0.983). The difference of -0.16% is statistically insignificant,

with a *p*-value of 0.729. The median CAR before (after) the law is 0.00% (-0.59%). This difference is statistically insignificant, with a *p*-value of 0.330 (untabulated). For robustness, we also calculate the CAR using an analysis analogous to estimating Equation (1). That is, we regress firm returns on contemporaneous market returns and a dummy variable denoting the three days around the executive departure announcement date, controlling for firm fixed effects. The results, presented in column (2) of Table 10, Panel A, indicate that the coefficient on the departure dummy variable is insignificant, equal to -0.0001 (*p*-value = 0.888) before the law, and -0.0022 (*p*-value = 0.133) after it.

Moreover, we directly compare the CARs in the three days surrounding executive departures before and after the law enactment. This comparison is conducted by regressing the sample firms' returns on the following variables: *Departure*, which is a dummy variable with the value of one in the three-day window surrounding each executive departure announcement, and zero otherwise; the dummy variable *After* that equals one in the period following the law's enactment; the interaction between these dummy variables; and the TA-100 index returns. We estimate three specifications of the regression differing in the fixed effects used. The results, presented on in Panel B of Table 10, demonstrate that except for the TA-100 index returns, none of the explanatory variables is significant. These results suggest that, on average, the CARs around executive departure announcements before the law are not significantly different than the CARs around executive departure announcements after the law.

Taken together, the findings in Panels A and B of Table 10 suggest that, on average, executive departures did not generate a significant negative market response, neither before the law nor after it. Combined with the evidence that the law did not increase executive turnover, we conclude that insofar, there is no evidence of "brain drain" as a result of the law.

The second analysis we conduct examines whether the performance of financial institutions deteriorated following the law. A caveat of the analysis is that it is limited by the fact that we have only two years of data in the post-law period (2017-2018). Nevertheless, we collected data on six performance measures of financial firms: return on equity (ROE), return on assets (ROA), market to book value of equity (MV_BV), the ratio of net income to revenues (profit margin), annual sales growth, and average stock returns. We compare the mean and median values of these measures in the years before the enactment of the law (2014-2015) and the years after it (2017-2018). As Table 10 Panel C presents, all performance measures for the sample firms are higher in the post-law period than they were before the law (except from the median profit margin, which slightly decreased). This result is even more pronounced if one considers that the GDP growth—which strongly affects the performance of financial firms—decreased during this period: during the years 2014-2015, the average annual GDP growth was 5.02%, while during the years 2017-2018, the average annual GDP growth decreased to 4.22%.

We also perform a difference-in-difference analysis on the performance measures before and after the approval law and control for a set of macro-economic control variables. The macro-economic variables include the 10-year risk-free interest rate, GDP growth, unemployment rate, TA-100 annual index return, CPI rate, Israel's CDS spread and a time trend. Our control group in the difference-in-difference analysis includes financial institutions that are not under the scope of the law. The results (untabulated) show all these explanatory variables are statistically insignificant in explaining the change in the financial firms' performance measures.

Taken together with the results on executive departures, we do not find evidence that the law negatively affected the performance of financial institutions in our sample. These results provide additional support to the rent extraction theory. If the pay-cap imposed by the law shifted

executive compensation contracts from their optimal level to a level in which executives are underpaid, we would have expected to observe an exodus of talent and a deterioration in performance. We fail to find evidence consistent with this hypothesis. However, we caution against putting too much weight on these results given the possibility of confounding events.

6. Conclusion

We examine the optimality of executive compensation contracts. There is considerable debate on executive compensation in both the public arena and academia. On the one hand, value maximization contracting theories imply that executive compensation contracts are optimally designed to attract talented executives and incentivize those executives to maximize shareholder value. On the other hand, rent extraction theories suggest that compensation contracts enable executives to extract rents at the expense of shareholders.

We use a quasi-natural experiment that allows us to test the key differential prediction between the two theories. In 2016, the Israeli Parliament surprisingly passed a law limiting executive pay in financial institutions. Value maximization theories predict that this intervention should reduce firm value, while rent extraction theories predict that the opposite should occur. We find significantly positive abnormal returns for financial institutions around the passage of the law. We also find that the positive abnormal returns are significantly larger for financial institutions bound by the pay limit. We further find that the financial institutions that had executive pay just above the pay threshold experienced larger abnormal returns than those that were just below the threshold. These results support the rent extraction view of executive compensation, on average, in our setting. Lastly, we find that the positive market reaction is greater for financial institutions with weaker governance and for those that do not award equity-based pay.

Our results have implications to several literature streams in economics, including contract theory, corporate finance, corporate governance, labor economics, and income inequality. Moreover, the results may be relevant to policy discussions, given that numerous proposals to limit executive pay have been advanced both by the media and politicians. Nevertheless, we caution the reader when extrapolating our results to other settings.

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Table 1: Descriptive Statistics

The table reports descriptive statistics for our sample of the 20 financial institutions that were traded on the Tel-Aviv Stock Exchange during the legislation process leading to the executive compensation cap law. *Max total compensation* is the compensation of the firm's highest-paid executive in 2015. *Market Cap* is the firm's market value of equity on December 31, 2015. *Total Assets* is the firm's total assets at the end of 2015. *Gross Revenues* are the firm's annual revenues in 2015. *ROA* is return on assets, calculated as net income in 2015 over average total assets in the same year. *ROE* is return on equity, calculated as net income, scaled by average total shareholders' equity in 2015. *Market to Book* is the market value of equity divided by the book value of equity at the end of 2015. *P/E ratio* is the ratio between share price and earnings per share in 2015. *Proportion of independent directors* is the fraction of directors considered independent under the Israeli Companies Act. *Proportion of busy directors* is the fraction of directors serving on three or more boards. *Equity-pay to total-pay ratio* is the ratio of share-based compensation and total compensation for the firm's highest-paid executive in 2015.

	N	Mean	Std Dev	P25	P50	P75
Max total compensation (ILS, 000s)	20	4,766	2,165	3,250	4,738	6,621
Market Cap (ILS, millions)	20	4,534	7,024	620	2,140	3,901
Total Assets (ILS, millions)	20	98,199	129,134	8,825	43,052	125,909
Gross Revenues (ILS, millions)	20	5,981	5,968	643	3,088	11,900
ROA (%)	20	2.150	4.909	0.348	0.510	1.669
ROE (%)	20	10.991	9.091	6.236	6.872	10.441
Market to Book	20	1.128	1.216	0.601	0.714	0.903
P/E ratio	20	10.832	6.362	7.365	9.125	11.403
Proportion of independent directors	20	0.308	0.110	0.250	0.300	0.333
Proportion of busy directors	20	0.518	0.226	0.300	0.570	0.643
Equity-pay to total-pay ratio	20	0.084	0.132	0.000	0.000	0.1993

Table 2: Abnormal returns at the approval of the bill by the Treasury Committee for financial institutions that are under the scope of the bill (March 16, 2016)

The table presents the coefficient estimates from Equation (1), which measures the average abnormal returns in the three days surrounding the date of the approval of the executive compensation cap bill by the Treasury Committee. Our sample firms are described in Table 1. The sample period is from July 15, 2014, to April 12, 2016 (432 trading days). The dependent variable is the return of an equally weighted portfolio consisting of the sample firms. *TA-100 index* is the return of the Tel-Aviv 100 index, the main index for the Israeli stock market. *Unanimous vote in the Treasury Committee* is an indicator variable that equals 1 on the three days surrounding the approval of the bill at the Treasury Committee (Event 6) and 0 otherwise. *t*-values are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. *Campbell et al. (1997) P-value* presents the *p*-value of abnormal returns with conditional covariance matrix for an event study of portfolio returns with a single date, based on Campbell, Lo and MacKinlay (1997).

Dependent Variable:	(1) Portfolio Returns	(2) Portfolio Returns
Intercept	0.000	0.000
	(0.01)	(0.01)
Unanimous vote in the Treasury Committee for the approval of the bill	0.606**	0.606**
	(2.16)	(2.12)
TA-100 Index	0.724***	0.724***
	(16.60)	(24.17)
Standard Errors	Huber-White	None
# of Firms	20	20
Observations	432	432
Adjusted R-squared	0.576	0.576
Campbell et al. (1997) *P*-value for a 3-Day CAR	0.0331	

Table 3 Panel A: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial institutions with top executive compensation that exceeds the bill's compensation threshold (2.5 million ILS)

The table presents the coefficient estimates from Equation (1), which measures the average abnormal returns in the three days surrounding the date of the approval of the executive compensation cap bill by the Treasury Committee. Our sample firms are described in Table 1. The sample period and variables are defined in Table 2. Panel A (Panel B) examines the market reaction for the portfolio of financial firms with top executive compensation that exceeds (is below) the bill's compensation threshold (2.5 million ILS). Panel C examines the market reaction for the portfolio of financial firms that are not subject to the executive compensation cap bill. *t*-values are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. *Campbell et al. (1997) P-value* is defined in Table 2.

Dependent Variable:	(1) Portfolio Returns	(2) Portfolio Returns
Intercept	-0.008 (-0.30)	-0.008 (-0.30)
Unanimous vote in the Treasury Committee for the approval of the bill	0.682** (2.32)	0. 682* (2.22)
TA-100 Index	0.770*** (17.88)	0.770*** (23.87)
Standard Errors	Huber-White	None
# of Firms	17	17
Observations	432	432
Adjusted R-squared	0.570	0.570
Campbell et al. (1997) *P*-value for a 3-Day CAR	0.0267	

Table 3 Panel B: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial institutions with top executive compensation below the bill's compensation threshold (2.5 million ILS)

Dependent Variable:	(1) Portfolio Returns	(2) Portfolio Returns
Intercept	0.045	0.045
	(1.06)	(1.06)
Unanimous vote in the Treasury Committee for the approval of the bill	0.176	0.176
	(0.39)	(0.35)
TA-100 Index	0.463***	0.463***
	(5.97)	(8.71)
Standard Errors	Huber-White	None
# of Firms	3	3
Observations	432	432
Adjusted R-squared	0.146	0.146
Campbell et al. (1997) *P*-value for a 3-Day CAR	0.7232	

Table 3 Panel C: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial institutions excluded from the bill (not under the scope of the bill)

Dependent Variable:	(1) Portfolio Returns	(2) Portfolio Returns
Intercept	-0.012	-0.012
	(-0.13)	(-0.13)
Unanimous vote in the Treasury Committee for the approval of the bill	0.220	0.220
	(0.66)	(0.20)
TA-100 Index	0.463***	0.463***
	(4.05)	(4.02)
Standard Errors	Huber-White	None
# of Firms	8	8
Observations	432	432
Adjusted R-squared	0.032	0.032
Campbell et al. (1997) *P*-value for a 3-Day CAR	0.8355	

Table 4 Panel A: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial institutions with top executive compensation that exceeds the bill's compensation threshold (2.5 million ILS) but below 4 million ILS.

The table presents the coefficient estimates from Equation (1), which measures the average abnormal returns in the three days surrounding the date of the approval of the executive compensation cap bill by the Treasury Committee. Our sample firms are described in Table 1. The sample period and variables are defined in Table 2. Panel A examines the market reaction for the portfolio of financial institutions with top executive compensation that exceeds the bill's compensation threshold (2.5 million ILS) but below 4 million ILS. Panel B examines the reaction of the portfolio of financial institutions with top executive compensation below the bill's compensation threshold (2.5 million ILS) but above 1 million ILS. *t*-values are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. *Campbell et al. (1997) P-value* is defined in Table 2.

Dependent Variable:	(1) Portfolio Returns	(2) Portfolio Returns
Intercept	-0.052* (-1.65)	-0.052* (-1.65)
Unanimous vote in the Treasury Committee for the approval of the bill	0.434*** (4.17)	0.434 (1.15)
TA-100 Index	0.699*** (13.63)	0.699*** (17.70)
Standard Errors	Huber-White	None
# of Firms	4	4
Observations	432	432
Adjusted R-squared	0.420	0.420
Campbell et al. (1997) *P*-value for a 3-Day CAR	0.2389	

Table 4 Panel B: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial institutions with top executive compensation below the bill's compensation threshold (2.5 million ILS) but above 1 million ILS. (Identical to table 3 panel B, and is added here for the convenience of the reader)

Dependent Variable:	(1) Portfolio Returns	(2) Portfolio Returns
Intercept	0.045	0.045
	(1.06)	(1.06)
Unanimous vote in the Treasury Committee for the approval of the bill	0.176	0.176
	(0.39)	(0.35)
TA-100 Index	0.463***	0.463***
	(5.97)	(8.71)
Standard Errors	Huber-White	None
# of Firms	3	3
Observations	432	432
Adjusted R-squared	0.146	0.146
Campbell et al. (1997) *P*-value for a 3-Day CAR	0.7232	

Table 5 Panel A: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial firms with a proportion of independent directors below (above) the sample median.

The table presents the coefficient estimates from Equation (1), which measures the average abnormal returns in the three days surrounding the date of the approval of the executive compensation cap bill by the Treasury Committee. Our sample-firms are described in Table 1. The sample period and variables are defined in Table 2. In Panel A, the sample is divided into two groups of firms with a proportion of independent directors that is below (above) the sample median value. In Panel B, the sample is divided into two groups of firms with a proportion of busy directors below (above) the sample median value. *t*-values are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. *Campbell et al. (1997) P-value* is defined in Table 2. We calculate the statistical significance of the difference in the coefficients across two subsamples using a Monte-Carlo non-parametric simulation technique where we randomly assign observations from the two subsamples into two partitions and take the difference between the coefficients 200 times to create a distribution of the differences (see Edgington and Onghena 2007).

	(1)	(2)	(3)	(4)
Sample: % of independent directors	< Median	< Median	> Median	> Median
Dependent Variable:	Portfolio Returns	Portfolio Returns	Portfolio Returns	Portfolio Returns
Intercept	0.017	0.024	-0.017	-0.017
	(0.67)	(1.00)	(-0.57)	(-0.57)
Unanimous vote in the Treasury Committee for the approval of the bill	0.759**	0. 759**	0.453	0.453
	(2.45)	(2.52)	(1.44)	(1.29)
TA-100 Index	0.707***	0.707***	0.741***	0.741***
	(14.80)	(22.43)	(16.17)	(20.18)
Standard Errors	Huber-White	None	Huber-White	None
# of Firms	10	10	10	10
Observations	432	432	432	432
Adjusted R-squared	0.540	0.540	0.485	0.485
Campbell et al. (1997) *P*-value for a 3-Day CAR	0.0112		0.1938	
Difference between coefficients	0.306			
p-value	0.3302			

Table 5 Panel B: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial institutions with a proportion of busy directors below (above) the sample median.

Sample: Proportion of busy directors Dependent Variable:	(1) < Median Portfolio Returns	(2) < Median Portfolio Returns	(3) > Median Portfolio Returns	(4) > Median Portfolio Returns
Intercept	0.014	0.014	-0.013	-0.013
	(0.51)	(0.51)	(-0.48)	(-0.49)
Unanimous vote in the Treasury Committee for the approval of the bill	0.269	0.269	0.943***	0.943**
	(0.82)	(0.83)	(3.66)	(2.87)
TA-100 Index	0.628***	0.628***	0.820***	0.820***
	(13.25)	(18.55)	(17.19)	(23.78)
Standard Errors	Huber-White	None	Huber-White	None
# of Firms	10	10	10	10
Observations	432	432	432	432
Adjusted R-squared	0.443	0.443	0.569	0.569
Campbell et al. (1997) *P*-value for a 3-Day CAR	0. 4053		0.004	
Difference between coefficients	0.674			
p-value	0.0309			

Table 6: Abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016) for financial institutions with and without equity-based compensation

The table presents the coefficient estimates from Equation (1), which measures the average abnormal returns in the three days surrounding the date of the approval of the executive compensation cap bill by the Treasury Committee. Our sample firms are described in Table 1. The sample period and variables are defined in Table 2. The sample is divided into two groups based on whether the firm awards equity-based compensation. *Campbell et al. (1997) P-value* is defined in Table 2. We calculate the statistical significance of the difference in the coefficients across two subsamples using a Monte-Carlo non-parametric simulation technique where we randomly assign observations from the two subsamples into two partitions and take the difference between the coefficients 200 times to create a distribution of the differences (see Edgington and Onghena 2007). *t*-values are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

	(1)	(2)	(3)	(4)
Sample: Equity-based compensation	No	No	Yes	Yes
Dependent Variable:	Portfolio Returns	Portfolio Returns	Portfolio Returns	Portfolio Returns
Intercept	-0.018	-0.018	0.034	0.034
	(-0.64)	(-0.64)	(1.23)	(1.24)
Unanimous vote in the Treasury Committee for the approval of the bill	0.790***	0.790**	0.264	0.264
	(2.86)	(2.34)	(0.88)	(0.80)
TA-100 Index	0.717***	0.717***	0.737***	0.737***
	(14.54)	(20.24)	(16.54)	(21.28)
Standard Errors	Huber-White	None	Huber-White	None
# of Firms	13	13	7	7
Observations	432	432	432	432
Adjusted R-squared	0.489	0.489	0.511	0.511
Campbell et al. (1997) *P*-value for a 3-Day CAR	0. 0202		0.4101	
Difference between coefficients	0.526			
p-value	0.0348			

Table 7: Firm-level abnormal returns at the approval of the bill by the Treasury Committee (March 16, 2016)

The table presents the cumulative abnormal returns for each of the 20 firms in our sample in the three days surrounding the date of the approval of the executive compensation cap bill by the Treasury Committee. We calculate CAR and statistical significance using the Kolari and Pynnonen (2010) method, which adjusts the cross-sectional dependence between the events. *P*-values are reported in parentheses.

Panel A: Firms with executive compensation in excess of the bill's limit (2.5M ILS):

Firm	**CAR**		
Ayalon	-0.150		(0.975)
Beinleumi	1.705		(0.306)
BituachYashir	1.395		(0.453)
Clal	1.403		(0.586)
Discount	1.726		(0.337)
Harel	5.795	***	(0.005)
IBI	0.225		(0.911)
IDI	-0.484		(0.810)
Igud	4.048	**	(0.015)
Leader	5.751		(0.133)
Leumi	3.488	*	(0.061)
Meitav	-0.480		(0.819)
Menorah	2.411		(0.221)
Migdal	1.513		(0.477)
Mizrahi	0.535		(0.778)
Phoenix	4.491	**	(0.047)
Poalim	1.361		(0.419)
Average	2.055	**	

Average 3-Day CAR, Kolari and Pynnonen (2010): 2.055% (*p*-value = 0.0267)

positive: 14

Negative: 3

Tel-Aviv 100 Index 3-day returns: -0.168%

Panel B: Firms with executive compensation below the bill's limit (2.5M ILS):

Firm	CAR Kolari and Pynnonen	
Analyst	-0.138	(0.959)
Dexia	0.789	(0.727)
Jerusalem	0.927	(0.927)

Average 3-Day CAR, Kolari and Pynnonen (2010): 0.526% (*p*-value = 0.7232)

Table 8: Abnormal long-run returns at the approval of the bill by the Treasury Committee for all financial institutions that are under the scope of the bill (March 16, 2016)

The table presents the coefficient estimates from Equation (1), which measures the average abnormal returns following the date of the approval of the executive compensation cap bill by the Treasury Committee. Our sample firms are described in Table 1. The sample period starts 10 days before Event 1 and ends 180 days after Event 6. The variables are defined in Table 2. Day *t* denotes the day the law was adopted (i.e., event 6). *Event window* is an indicator variable that equals 1 on days *t*-1 to *t*+180 [*t*+1 to *t*+180] in columns (1) and (2) [(3) and (4)] and 0 otherwise. We exclude the three days surrounding the main event in columns 3 and 4. *t*-values are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. *Campbell et al. (1997) P-value* is defined in Table 2.

	(1)	(2)	(3)	(4)
Event Window:	(t-1 , t+180)	(t-1 , t+180)	(t+2 , t+180)	(t+2 , t+180)
Dependent Variable:	Portfolio Returns	Portfolio Returns	Portfolio Returns	Portfolio Returns
Intercept	-0.001	-0.001	-0.001	-0.001
	(-0.05)	(-0.05)	(-0.05)	(-0.05)
Event Window	0.125***	0.125***	0.117**	0.117**
	(2.73)	(2.62)	(2.55)	(2.44)
TLV-125 Index	0.696***	0.696***	0.699***	0.699***
	(25.26)	(18.33)	(25.37)	(18.42)
Standard errors	None	Huber-White	None	Huber-White
# of firms	20	20	20	20
Observations	594	594	591	591
Adjusted R-squared	0.520	0.520	0.523	0.523
Event CAR	28.72%**		27.84%**	
Campbell et al. (1997) P-value	0.0114		0.0134	

Table 9: Firm-specific disclosures around the main event

The table presents firm-specific events that appear in the economic media, firm-filings, and as outcomes of a Google search from March 14 through March 18, 2016, which encapsulate the five days that surround the date of the unanimous vote for the approval of the bill by the Treasury Committee (March 16, 2016). The table also specifies the closest annual report filing date.

Firm	Type	Filings +/- 3days around main event (March 16, 2016)	Closest 10K filing
Analyst	Other	None	March 23, 2016
Ayalon	Insurance	None	March 31, 2016
Beinleumi	Bank	None	February 28, 2016
Bituach Yashir	Insurance	2015 10K filing and declaration of a dividend	March 17, 2016
Clal	Insurance	None	March 23, 2016
Dexia	Bank	None	February 23, 2016
Discount	Bank	None	February 29, 2016
Harel	Insurance	List of common stock and options	March 23, 2016
IBI Investments	Other	None	March 29, 2016
IDI	Insurance	None	February 28, 2016
Igud	Bank	None	February 29, 2016
Jerusalem	Bank	None	February 23, 2016
Leader	Other	None	March 30, 2016
Leumi	Bank	9.5 million USD acquisition of enVerid Systems Inc. by subsidiary Leumi Partners (1.158 *CAR*). Only one Israeli financial newspaper discussed this (Globes)	February 29, 2016
Meitav	Other	2015 10K filing - 3 day *CAR* is -0.507 so doesn't explain the positive *CAR*	March 16, 2016
Menorah	Insurance	On March 15, 2016, the firm announced a class-action lawsuit against one of its subsidiaries	March 31, 2016
Migdal	Insurance	None	March 30, 2016
Mizrahi	Bank	Changes in holdings by related parties on March 16, 2016	February 25, 2016
Phoenix	Insurance	None	March 28, 2016
Poalim	Bank	Extension of appointment of two directors. Affirmation of A- credit rating by Fitch. (0.449 *CAR*)	February 29, 2016

Table 10: The performance of financial institutions and market reaction to executive departure announcements before versus after the executive compensation cap law

The table compares the performance of financial institutions and the market reaction to executive departure announcements before versus after the executive compensation cap law. The sample firms are described in Table 1. Panel A examines the three-day market reaction to announcements of executive departures before and after the enactment of the law. Column (1) reports the mean of the CARs calculated for each departure separately. Column (2) reports the CAR estimated using equation (1), where the events are the executive departure announcements. Panel B compares the difference in the 3-day market reaction to announcements of executive departures before and after the law using OLS regressions. Panel C presents the mean and median values of six performance metrics before and after the enactment of the law. The performance metrics include return on equity (ROE), return on assets (ROA), market to book value (MV_BV), net income to revenues (profit margin), the annual sales growth, and average stock returns. The years 2014-2015 (2017-2018) represent the evaluation period before (after) the enactment of the law. *t*-values are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

Panel A: CARs surrounding the announcements of executive departures from financial institutions before versus after the approval of the bill

	Number of departures	(1) CAR, event-study	(2) CAR, Equation (1)
Before the Law: January 1, 2013 – January 31, 2016	84	-0.14% (-0.02)	-0.01% (-0.14)
After the Law: March 30, 2016 – October 7, 2019	35	-0.30% (-0.01)	-0.22% (-1.50)

Panel B: Comparison of CARs in the three-day period surrounding announcements of executive departures from financial institutions before versus after the approval of the bill

Dependent Variable:	(1) Returns	(2) Returns	(3) Returns
Intercept	0.022* (1.940)	0.022 (0.630)	-0.314*** (-3.760)
Departure	-0.081 (-0.880)	-0.072 (-0.780)	-0.073 (-0.790)
After	0.014 (0.920)	0.014 (0.940)	0.057 (1.260)
Departure*After	-0.119 (-0.700)	-0.119 (-0.700)	-0.127 (-0.750)
TA-100 Index	0.744*** (69.740)	0.744*** (69.740)	0.744*** (69.710)
Firm fixed effects	No	No	Yes
Year fixed effects	No	Yes	Yes
Observations	34,520	34,520	34,520
Adjusted R-squared	0.1236	0.1242	0.1260

Panel C: Performance measures of financial institutions before versus after the approval of the bill

Period:		2014-2015	2017-2018
ROE	Mean	9.13%	9.81%
	Median	7.32%	8.66%
ROA	Mean	1.12%	1.52%
	Median	0.50%	0.60%
MV_BV	Mean	101.23%	126.83%
	Median	77.50%	89.50%
Profit Margin	Mean	16.99%	17.70%
	Median	10.60%	10.13%
Sales growth	Mean	1.30%	7.97%
	Median	0.92%	8.74%
Stock return	Mean	2.10%	7.13%
	Median	-1.28%	9.75%