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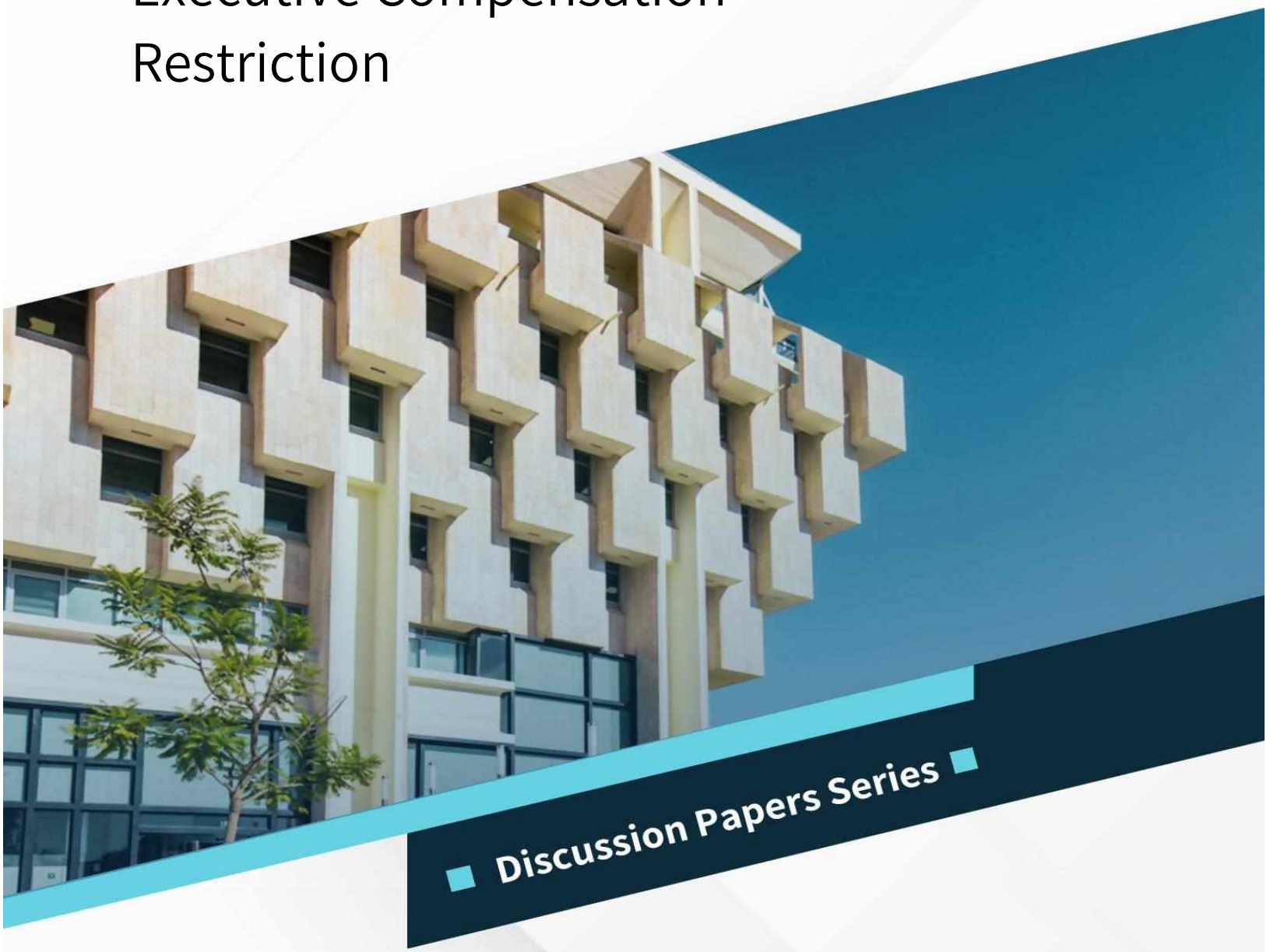
Unintended Consequences of Executive Compensation Restriction



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Unintended Consequences of Executive Compensation Restriction*

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Abstract

This paper examines unintended consequences of executive compensation regulation using Israel's 2016 law that imposed binding caps on financial firms' executive pay. Using a dynamic difference-in-differences design, I analyze how these caps affected compensation in unrestricted firms to test whether executive pay reflects optimal contracting or rent extraction. The findings reveal that executives at unrestricted firms whose compensation previously exceeded the cap experienced significant pay reductions following implementation. I document two mechanisms driving these effects: executive mobility between restricted and unrestricted sectors, and direct anchoring — whereby the regulatory threshold became a salient reference point influencing compensation decisions at firms entirely outside the regulatory scope. The absence of significant transmission through ownership and director network channels supports the direct anchoring interpretation. These findings provide evidence consistent with rent extraction theory, showing that targeted sector-specific regulations can be associated with significant unintended consequences extending well beyond their explicit scope.

Keywords: Executive compensation, Rent extraction, Financial regulation, Corporate governance
JEL Classification: G34, G38, M12, M52, H11

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השפעות משניות של חוק שכר הבכירים

מיטל גראם רוזן[A]

תקציר

מחקר זה בוחן את ההשלכות הלא מכוונות של רגולציה על תגמול מנהלים, תוך שימוש בחוק הישראלי משנת 2016 — חוק ייחודי בעולם המגביל את שכר הבכירים בחברות פיננסיות. באמצעות שיטת הפרש-הפרשים דינמית, אני בוחנת כיצד השפיע החוק על תגמול בכירים בחברות שאינן כפופות לו, במטרה לבדוק האם תגמול המנהלים משקף חוזה אופטימלי או מיצוי רנטות. הממצאים מצביעים על אפקט עיגון: תגמול מנהלים בחברות שאינן כפופות לחוק, שקיבלו שכר מעל הסף הרגולטורי לפני החקיקה, ירד באופן משמעותי בעקבות כניסתו לתוקף. המאמר מתעד שני מנגנונים המניעים השפעה זו: ניידות בכירים בין המגזר המוגבל למגזר הלא-מוגבל, ועיגון ישיר — שבו הסף הרגולטורי הפך לנקודת ייחוס המשפיעה על החלטות תגמול בחברות שאינן נכללות כלל בתחולת החוק. הניתוח מצביע על כך שאפקט העיגון אינו מוסבר על ידי קשרי בעלות או רשתות דירקטורים משותפות בין חברות מוגבלות ולא-מוגבלות, אלא נובע ישירות מהסף הרגולטורי שהפך לנקודת ייחוס רחבה בשוק. ממצאים אלו מספקים תמיכה אמפירית לתיאוריית מיצוי הרנטות, ומראים כי רגולציה סקטוריאלית ממוקדת עשויה לייצר השלכות לא מכוונות המתפרשות הרחק מעבר לתחולתה המפורשת.

מילות מפתח: תגמול בכירים, רגולציה פיננסית, ממשל תאגידי

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1. Introduction

Executive compensation has emerged as one of the most contentious issues in corporate governance, with debates intensifying significantly following the 2007-08 financial crisis. Critics argue that excessive pay packages not only reflect poor governance but may also encourage excessive risk-taking that threatens broader financial stability (Murphy, 2013). In response, regulators worldwide have implemented various forms of oversight, ranging from enhanced disclosure requirements to direct restrictions on compensation levels and structures.

The literature on regulatory and legal restrictions on executive compensation (see for example, Core and Guay (2010), Dittmann et al. (2011), Kleymenova and Tuna (2021), Colonnello et al. (2018), and Abudy et al. (2020)) typically deals with the effects of the introduction of the regulations on the restricted firms themselves, focusing almost exclusively on the direct effects of compensation regulations on the firms explicitly subject to these rules.

A fundamental challenge in regulatory policy is understanding the full scope of regulatory interventions beyond their intended targets. When governments impose restrictions on specific sectors, these actions may trigger responses throughout the broader economy that extend well beyond the regulated firms themselves. The magnitude and nature of such responses represent important considerations for policymakers, yet they remain poorly understood in the context of executive compensation regulation.

I exploit a unique natural experiment provided by Israel's 2016 executive compensation law (hereafter: "the Act") to examine this question[1]. The legislation imposed binding caps on executive pay in financial firms, limiting total compensation to no more than 35 times that of the lowest-paid employee[2]. Crucially, the law applies only to specific financial institutions—banks, insurance companies, investment firms, asset management firms, and mutual funds[3,4] (hereafter: restricted firms)—while leaving all other sectors unrestricted.

[1] This is the only law worldwide that sets a binding upper limit on total executive compensation in non-state-owned firms. (In 2009, the Chinese central government introduced regulations limiting executive salaries for the country's centrally administered state-owned enterprises (CSOEs).)

[2] For more details, see Graham Rozen (2024b).

[3] Including parent companies that hold at least 30 percent of these financial institutions.

[4] Other financial industries, such as credit card issuers, private equity funds, and hedge funds, are not subject to the law. In addition, the law does not apply to subsidiaries of affected firms that engage in other financial activities, such as investment banking, underwriting, and insurance agencies. Similarly, foreign subsidiaries of affected firms are exempt. The Israeli Minister of Finance may extend the law to additional financial corporations with approval from the Knesset Finance Committee.

Throughout this paper, I refer to these specific financial institutions as restricted firms, and to all other publicly traded firms as unrestricted firms.

This selective application creates an ideal setting to examine whether compensation restrictions in one sector generate unintended consequences for firms operating outside the regulatory scope, with important implications for regulatory policy design and understanding of market-wide consequences of sector-specific interventions.

The Israeli context amplifies this research opportunity: while restricted firms constitute only six percent of all public firms, they represent over 20 percent of total market capitalization on the Tel Aviv Stock Exchange. This concentration suggests that despite their limited number, restricted financial institutions may serve as influential benchmarks for compensation decisions across the broader economy, whether through sector-based comparisons for other financial firms or size-based benchmarking for large non-financial corporations. The possibility of an interconnected executive labor market further suggests potential channels through which regulatory effects might propagate beyond their intended scope.

The theoretical foundation for understanding these potential consequences centers on a fundamental debate in corporate finance regarding the effect of restricting executive compensation on firm performance. Edmans and Gabaix (2016) identify two main approaches: the "rent extraction" view and the "shareholder value" view. Under the "shareholder value" view, executive pay reflects managers' marginal productivity and efficiently aligns incentives with shareholder value maximization. Applied to the Israeli setting, this theory would predict that the 2016 law should have no effect on unrestricted firms' compensation, as their pay packages would continue to reflect efficient market-determined contracts based on executives' marginal contribution to firm value, regardless of restrictions imposed on other sectors.

Under the "rent extraction" view, managers exploit their power to extract excessive compensation beyond what their contribution to firm value would justify, with pay levels decoupled from performance and economic fundamentals (Bebchuk and Fried, 2004). Applied to the Israeli setting, this theory suggests that the 2016 law could generate consequences for unrestricted firms by disrupting existing rent extraction dynamics. Under this view, when regulation constrains excessive compensation at major financial institutions, it may create pressure for compensation adjustments at unrestricted firms to maintain relative pay hierarchies across the executive labor market. From a systemic

perspective, such consequences could potentially create economy-wide distortions in risk-taking incentives and talent allocation, with implications that may extend well beyond the immediate regulatory targets.

My empirical analysis employs comprehensive hand-collected data on CEOs and board chairpersons (hereafter: "top executives") of all non-restricted Israeli listed firms from 2013 to 2019, spanning three years before and after the law's implementation. I classify executives as "Above" if their position's compensation exceeded NIS 2.5 million in at least two of the three pre-law years (2014-2016), and as "Below" otherwise. This classification enables me to test whether unrestricted firms paying above the regulatory threshold experienced different compensation changes compared to those paying below it, despite neither group being legally required to modify their practices. The NIS 2.5 million threshold emerged as the focal point for two key reasons: first, compensation above this level loses tax deductibility and requires extensive approval processes including compensation committees, boards of directors, independent directors, and shareholders; second, thirty-five times the Israeli minimum wage approximates NIS 2.5 million. Following the law's passage, media coverage consistently referenced NIS 2.5 million as the effective cap, further establishing its prominence as a reference point[5,6]. I employ a dynamic difference-in-differences design with year-by-year interaction terms to estimate the differential compensation trends between Above and Below executives around the Act's implementation.

My findings are consistent with the interpretation that the compensation cap in financial firms is associated with significant changes in unrestricted firms. Executives in non-restricted companies who were previously compensated above the NIS 2.5 million threshold experienced substantial pay reductions in the years following implementation. These results are robust to alternative identification strategies addressing selection bias (propensity score matching, entropy balancing), timing verification through placebo tests, industry×year fixed effects, median regression, and various sensitivity analyses including

[5] See for example, https://www.calcalist.co.il/local/articles/0,7340,L-3707258,00.html.

[6] The threshold is not fixed but variable due to its formula-based calculation ('35 times minimum wage') and because compensation under the law excludes certain social provisions and allowances for past rights that accrued prior to the Act. Additionally, almost half of top executives in restricted firms continue to earn above NIS 2.5 million (between NIS 2.5–4 million) after implementation (for details, see Graham Rozen (2024b)), reflecting in part the continued honoring of pre-existing contracts. However, media and regulatory discourse consistently treated NIS 2.5 million as the operative benchmark regardless of these technical flexibilities. For my empirical analysis, I therefore use NIS 2.5 million as the threshold given its prominence in public discourse.

alternative threshold levels and sample definitions. The results are consistent with direct anchoring — the regulatory threshold became a salient reference point influencing compensation decisions beyond its formal scope — as well as with an interconnected executive labor market through which regulatory effects propagated across sectors[7].

The pattern of results strongly favors the rent extraction theory over efficient contracting explanations. I test these theoretical predictions directly by examining three specific mechanisms: executive mobility patterns between sectors, changes in benchmarking behavior, and cross-sectional variation based on industry-level connections to restricted firms.

I document substantial executive mobility between restricted and unrestricted sectors, with 26% of restricted financial firms' top executives[8] during 1995-2018 having moved to, from, or simultaneously served in unrestricted firms. Moreover, industries with higher rates of executive movement to/from restricted firms experienced larger compensation declines following the Act. This pattern, where compensation changes respond to regulatory thresholds rather than changes in executive productivity or firm performance, contradicts efficient contracting theory.

This interconnected labor market is consistent with theories of competitive talent markets (Holmstrom & Kaplan, 2003), which argue that compensation packages are driven by market forces such as supply and demand for executive talent, with benchmarking enabling firms to gauge "market compensation." Under this framework, executives compensated above the threshold experienced decreases because firms could now pay them less while still competing effectively with restricted firms for talent, assuming similar skill requirements across sectors. My findings align with network effects documented by Kieschnick and Shi (2023) in their study of U.S. tax law changes affecting executive compensation.

Additionally, I find that the regulatory threshold became a stronger benchmarking reference point after implementation, with unrestricted firms' compensation decisions showing greater sensitivity to the NIS 2.5 million ceiling than to traditional peer group comparisons. This pattern is consistent with anchoring theory (Kahneman & Tversky, 1974), where salient reference points influence decision-making. The economic literature provides

[7] Boards approving CEO compensation above the threshold may face increased scrutiny and criticism from media and shareholders, creating reputational risks that encourage voluntary compliance with the regulatory benchmark.

[8] I.e. CEOs and the chairpersons of the board.

extensive evidence of anchoring effects in contexts such as negotiation, stock prices, acquisition activities (Baker et al., 2012), and 401(k) pension investment choices (Choi et al., 2004; Hurwitz et al., 2020). The NIS 2.5 million threshold received extensive media coverage, potentially amplifying its salience as an anchoring point for compensation committees when setting executive compensation.

I also examine whether cross-ownership by restricted firms influenced compensation decisions through a potential "jealousy" mechanism, where restricted firm executives might vote against high compensation at unrestricted firms. Ideally, this mechanism would be tested using detailed voting records on executive compensation proposals at annual shareholder meetings. However, I was unable to obtain such granular voting data. I therefore employ ownership stakes by restricted firms in unrestricted firms as a proxy for potential voting influence, but find no significant association. Another potential mechanism is public criticism and media scrutiny of high compensation levels following the law's passage, though this channel is not directly testable with my data. Taken together, the absence of significant transmission through ownership and director network channels, combined with the persistence of effects among firms with no executive mobility to or from restricted firms, provides empirical support for the direct anchoring interpretation over indirect spillover channels.

These findings make several important contributions to our understanding of regulatory unintended consequences. First, I provide novel evidence that sector-specific policies can generate effects beyond their immediate targets through behavioral and psychological channels. These insights are relevant for policymakers designing targeted regulations, as they demonstrate how interventions may have broader effects than initially anticipated.

Second, my results contribute to the longstanding debate over executive compensation by providing evidence more consistent with rent extraction theory than with efficient contracting. The voluntary nature of compensation reductions at unrestricted firms suggests previous pay levels may have exceeded efficient contracting outcomes, with regulatory intervention serving as a coordination mechanism enabling boards to justify reductions they may have previously been reluctant to implement.

Third, I contribute to the anchoring literature by demonstrating how regulatory thresholds can influence compensation decisions at firms not subject to regulation. This extends existing research on compensation spillovers by showing how prominent policy

interventions can create anchoring effects that affect corporate practices beyond their intended scope.

While Israel represents a relatively small market with the law affecting only a limited number of firms, the mechanisms I identify—anchoring effects, social norm formation, and interconnected executive labor markets—operate in most developed economies. The lessons from Israel's natural experiment therefore have broad relevance for regulatory authorities worldwide considering similar interventions, particularly as they balance effectiveness with unintended market-wide consequences.

The remainder of this paper proceeds as follows: Section 2 reviews the relevant literature on executive compensation regulation and unintended consequences. Section 3 presents my data and institutional setting. Section 4 describes my methodology and main results. Section 5 conducts robustness tests, and Section 6 concludes.

2. Literature Review

When rates of external hiring of executives were low, social scientists debated whether executives were paid on the basis of comparisons with the "external" labor market or the "internal" labor market (Roberts 1956; Simon 1957), even as earlier empirical studies suggested that executive pay was determined by a combination of these forces (Patton, 1951). By the late 1970s, however, the setting of executive pay often involved institutionalized comparisons between the company's own executive compensation practices and those of "a list of companies comparable to [the compensation director's] organization" (Cook, 1981). As the practice of recruiting external CEOs became more prevalent and developed its own set of institutions (Khurana, 2002; Murphy and Zabojnık, 2004), so did the explicit use of benchmarking and labor market analysis as the normatively prescribed model for setting executive compensation.

In this paper, I examine whether an exogenous regulatory intervention affecting executive compensation in one group of companies creates unintended consequences for executive compensation in other companies operating in the same market. While this phenomenon may appear similar to what the literature refers to as "spillover effects," there is an important distinction. Spillover effects represent indirect transmission mechanisms where a policy first affects targeted firms, and their subsequent actions then influence untargeted firms through specific economic channels. In contrast, unintended consequences can result from direct effects of the regulation itself on untargeted firms, such as when a regulatory

threshold becomes a salient reference point that influences compensation decisions through media attention or social norms, independent of changes at restricted firms. Understanding this distinction is crucial for empirical analysis and helps explain why the Israeli case represents unintended consequences rather than spillover effects: the regulatory threshold became a direct reference point for compensation decisions at unrestricted firms, influencing behavior through media salience and social norms rather than through economic transmission channels from restricted firms' actions.

Spillover effects are extensively documented across various economic contexts. Recent literature provides evidence that shareholder governance actions create spillover effects for peer firms – see Aslan and Kumar (2016), Feng, Xu, and Zhu (2018), and Gantchev, Gredil, and Jotikasthira (2018) for hedge fund activism. Denis, Jochem and Rajamani (2020) document that firms whose compensation peers experience weak say-on-pay votes reduce CEO compensation following those votes, with reductions reflecting proxy adviser concerns and occurring through both learning and compensation targeting channels. Complementing this peer-based literature, Kieschnick and Shi (2023) examine spillover effects in managerial compensation using the Omnibus Budget Reconciliation Act of 1993, which affected executive compensation tax treatment. They find that changes in compensation of treated firms diffused to untreated firms within their industry, with stronger effects when more firms within an industry were affected. Their findings demonstrate that spillover effects in executive compensation can be substantial and should be considered when evaluating regulatory impacts.

The literature on benchmarking and peer groups provides important context for understanding how regulatory thresholds might influence compensation decisions. Diprete et al. (2010) demonstrate, using counterfactual simulations based on Standard and Poor's ExecuComp data, that a small and shifting fraction of CEOs have regularly been able to "leapfrog" their compensation benchmarks by moving to the right tail of the benchmark distribution and receiving larger than normative compensation increases, even after accounting for job mobility and executive performance. These events, according to the authors, produce subsequent "legitimate" pay increases for others and potentially explain an important fraction of the overall upward movement in executive compensation since the early 1990s. Diprete, Eirich and Pittinsky further argue that the benchmarking process creates linkages among firms whereby governance failures at one firm influence the compensation environment of others. This implies that rent extraction can occur even when

CEOs are paid their "market wage" as established by competitive benchmarks, supporting the view of rent extraction as a manifestation of systemic rather than firm-specific factors. This dynamic is particularly relevant to my analysis, as it suggests that salient reference points can fundamentally alter compensation norms across firms. The Israeli compensation cap creates precisely such a salient reference point, potentially functioning as a new benchmarking standard for unrestricted firms.

The institutionalization of benchmarking practices provides additional context for these dynamics. Following SEC requirements from 2006 for US firms to disclose peer groups used in compensation decisions, research has documented widespread benchmarking practices. Bizjak, Lemmon, and Naveen (2008) find that 96% of S&P 500 firms used peer groups to set pay. Unlike the US, Israel has no such disclosure requirements, potentially making regulatory thresholds even more salient as public reference points.

However, the way firms select peers for benchmarking reveals important strategic considerations. Research indicates that while firms generally select peers based on sensible criteria such as size and industry, many engage in opportunistic peer selection to justify higher compensation. Studies reveal different mechanisms: Faulkender and Yang (2010) find firms choose peers with unusually high pay given their characteristics, while Bizjak, Lemmon, and Nguyen (2011) show firms select larger peers that pay more due to size effects. Both approaches result in upward pressure on compensation, with virtually no firms benchmarking below the median of their chosen peer group and over 30% choosing higher percentiles. However, the interpretation of this strategic behavior is debated. While these studies view peer selection as self-serving behavior, Albuquerque et al. (2013) argue it reflects compensation for unobserved CEO talent, finding that peer selection effects mostly represent talent-based pay adjustments. Edmans et al. (2017) conclude that peer group usage could be consistent with shareholder value (helping determine market pay), rent extraction (justifying high pay), or simple imitation of other firms' contracts.

The role of media and public disclosure provides additional context for understanding regulatory effects on compensation. CEO pay becomes a public matter influenced by politicians, media, and other stakeholders (Murphy, 2012). Edmans et al. (2017) argue that greater disclosure aims to provide shareholders with information to decide whether the current contract is optimal and act against it if appropriate, and to "name and shame" firms with excessive compensation policies, encouraging stakeholders to walk away from such firms. However, research suggests disclosure may paradoxically increase pay levels as

CEOs gain better visibility into peer compensation (Park, Nelson, and Huson, 2001; Mas, 2016). Hayes and Schaefer (2009) note that the widely cited "Lake Wobegon Effect" occurs because no firm wants to admit having a below-average CEO, preventing pay packages from lagging market expectations. This phenomenon is particularly relevant to the Israeli case, where extensive media coverage of the NIS 2.5 million threshold created a highly salient public reference point that could influence compensation decisions across all firms, restricted and unrestricted alike.

While the literature provides extensive evidence on spillover effects, benchmarking practices, and media influences on executive compensation, few studies examine how regulatory interventions create unintended consequences for firms outside the regulation's scope, as noted. The Israeli compensation cap provides a unique opportunity to test whether such regulations can influence compensation decisions through direct mechanisms - such as regulatory thresholds becoming salient reference points - rather than through indirect spillover channels. This setting allows us to distinguish between rent extraction and efficient contracting theories by examining how unrestricted firms respond to a prominent regulatory intervention targeting a different sector.

The closest study to the present paper is Abudy et al. (2020), who examine the same Israeli legislation using an event-study approach. They document significant positive abnormal returns surrounding the law's enactment, concentrated among restricted financial firms for which the cap was binding, and interpret this as evidence that compensation contracts do not always maximize firm value. I complement their work by examining the opposite side of the market: unrestricted firms that faced no legal obligation to adjust compensation. While Abudy et al. identify the direct effects of the law on restricted firms through stock market reactions, I document unintended consequences for firms entirely outside the regulatory scope — a channel that their event-study design, which focuses on restricted firms, cannot capture. The two papers together provide a more complete picture of the law's market-wide impact.

3. Characterization of the data

I use a panel of all Israeli publicly traded companies not directly affected by the 2016 law, with hand-collected data on top executives—CEOs and board chairpersons[9] (hereafter: top

[9] CEOs and board chairpersons included among the top five highest paid executives (public companies in Israel are required to report compensation for the five highest paid people in the company).

executives)—over the period 2013 to 2019 (annual data). The firms are listed on the Tel Aviv Stock Exchange (TASE)[10] and belong to eight industries: Biomed, Investment and Holdings, Financial Services, Manufacturing, Oil and Gas Exploration, Real Estate, Technology, and Trade and Services (according to TASE's industry classification). The sample is an unbalanced panel due to firm de-listings during the sample period and occasional non-publication of periodic or financial reports. I use only years after 2012 to avoid potential confounding effects from Amendment No. 16 and Amendment No. 20[11] to Israeli corporation law.

The database contains information on top executive compensation (total compensation and its components), personal characteristics (age, tenure, education, holdings, etc.), firm characteristics (industry, number of trading years, etc.), firm accounting data (total assets, net profit, etc.), and stock market data. Compensation consists of two parts: fixed compensation, including salary, social provisions, and other benefits; and variable compensation, including performance-sensitive components such as bonuses and share-based compensation. The data also include information on executive and director turnover during this period. Appendix A provides details on all variables and their sources. When multiple individuals serve in the same position within the same company during a given year (due to mid-year turnover, Co-CEO arrangements, Co-Chairperson roles, etc.), I retain only the longer-serving executive in the dataset.[12]

As noted in the introduction, the Act restricts executive pay only in specific types of financial firms—banks, insurance companies, investment firms, asset management firms, and mutual funds, including parent companies of these firms. The law was enacted in 2016, with restricted firms required to comply beginning in 2017. This restriction translates into an effective upper limit of NIS 2.5 million per year on total compensation.

The data include 475 companies. I first exclude companies absent from the dataset in either the pre-Act years (2013–16) or the post-Act years (2017–19). This filtering results in a final dataset of 350 companies. Second, I identify which executives have the potential to be affected by the law. I classify each position (CEO or board chairperson) according to

[10] Dual-listed firms are omitted.
[11] These two amendments to corporation law changed how executive compensation is approved at shareholder meetings. For more information, see Graham Rozen (2024b).
[12] In cases of turnover, I make proportional compensation adjustments for the remaining executive.

whether the compensation paid to the executive serving in that position exceeded NIS 2.5 million in at least two of the three pre-law years (2014–16) (hereafter: Above executives).

Table 1 reports the distribution of non-restricted firms by industry and by whether they paid above NIS 2.5 million to their CEO and/or board chairperson in the years before the Act, according to the criterion mentioned above (hereafter: Above firms). Twenty-five percent of non-restricted firms paid above NIS 2.5 million to their CEO and/or board chairperson before the Act, with the highest rates in Trade and Services (36.5%) and Real Estate (34.1%), and the lowest in Biomed (6.5%).

Table 2 reports summary statistics for key executive variables—age, tenure, equity stake, proportion with Master's degrees or higher, total compensation, and fixed compensation—and firm-level variables including total assets, ROE, ROA, stock return, leverage, and executive turnover rate. The table divides data among all executives (Panel A), Above executives (Panel A1), and Below executives (Panel A2), with each panel comparing the pre-Act period (2014–16) to the post-Act period (2017–19). Panel A shows that average total compensation for executives in non-restricted firms remained unchanged following the Act's implementation, though the standard deviation of compensation decreased notably. As expected, Above executives receive a higher proportion of performance-based pay and typically work for larger firms, consistent with standard practices of providing greater incentives to top management in larger organizations.

Visual inspection of average total compensation from 2013 to 2019 reveals a decline between 2013 and 2016. In the years following the Act, average compensation remained relatively stable in 2017-2018, before rising noticeably in 2019 (Fig. 1(a)). To examine whether the Act had differential effects on Above executives (those potentially affected by the law) and Below executives, Fig. 1(b) presents total compensation trends for CEOs and board chairpersons during the sample period, separated by these two groups. The graph shows that compensation trends in both groups followed similar patterns until 2016. After the Act's implementation, however, the trends diverged—compensation for Below executives continued to increase (12% between 2016 and 2019), while compensation for Above executives decreased during the first two years (15% between 2016 and 2018)[13], before recovering in 2019. This initial reaction likely reflects convergence toward the "legitimate" compensation threshold of NIS 2.5 million.

[13] This result remains consistent when examining CEOs and board chairpersons separately.

Figures 1(c) and 1(d) display the cumulative distribution function (CDF) of total compensation before (2014–16) and after (2017–19) the Act for Above and Below executives, respectively. The CDF for Above executives (Fig. 1(c)) shows that the Act primarily affected those compensated above NIS 3 million. The CDF for Below executives (Fig. 1(d)) shows a less definitive pattern—only a slight decrease around NIS 2 million. This modest change reflects the fact that, unlike restricted companies required to adjust compensation immediately upon the Act's implementation, unrestricted companies continued to honor existing compensation contracts. Adjustments could occur only when contracts came up for renewal or new executives were hired. Consequently, any effects would manifest gradually over time.[14]

The results depicted in Figure 1 align with theories based on the market for CEO talent, though only temporarily following the Act's implementation. Specifically, executives executives compensated above the threshold experienced decreased compensation following the Act's implementation, as unrestricted firms could offer lower compensation while still competing effectively with restricted firms for top executive talent. This explanation assumes that the skills required of executives in restricted financial firms are comparable to those needed in unrestricted firms (whether financial or non-financial). By the third year, however, compensation levels had recovered to approximately their pre-Act levels. In contrast, Below executives continued to experience compensation growth following the Act's implementation

Using the database described in Graham Rozen (2024a), which comprises a panel of all Israeli publicly traded companies (listed on the Tel Aviv Stock Exchange) with available information on top executive compensation from 1995 to 2018, I examined whether this assumption holds in the Israeli executive market. The evidence strongly supports this mobility hypothesis: 26% of top executives[15,16] in public restricted financial firms during 1995–2018 either came from (24%), moved to (50%), or served simultaneously (19%) in executive positions at public unrestricted firms. Additionally, 7% of these executives

[14] Given that compensation levels typically do not change drastically in short periods, I supplement the analysis with an untabulated test computing the CDF of compensation changes (measured as differences in natural logarithm) before the Act (comparing 2013–2014 with 2015–2016) and after the Act (comparing 2015–2016 with 2017–2018) for Below executives. This analysis confirms that the percentage of executives receiving compensation increases was substantially higher following the Act's implementation.

[15] I.e., CEOs and board chairpersons.

[16] Data for restricted firms include all five highest-paid individuals, while for unrestricted firms, only top executives (CEOs and board chairpersons) are included. When analyzing mobility for all highly compensated executives, not just top executives, the percentages are substantially higher.

moved in both directions between restricted and unrestricted sectors during this period. Regarding specific roles, 37% served as board chairpersons in both sectors, 23% as CEOs, and 40% held both positions. These figures likely represent a considerable underestimate, as the database includes only publicly traded companies and excludes executive movement to or from private firms.

To verify that the trends presented above do not reflect industry-specific patterns, and to examine whether the aggregate compensation changes in Figure 1 mask cross-industry differences, Figure 2 presents changes in total compensation between the pre-Act period (2014–16) and post-Act period (2017–19) by industry. The figures show compensation declines for Above executives in most industries: Investment and Holdings (34%), Real Estate (18%), Biomed[17] (19%), Financial Services (12%), Trade and Services (7%), and Manufacturing (2%). However, the Technology and Oil and Gas Exploration industries, with small numbers of Above executives (five and two, respectively), experienced significant compensation growth during this period. For comparison, restricted firms showed a 33% decline in Above executive compensation. Below executives showed compensation increases in most industries following the Act—only in Investment and Holdings did compensation decrease.

Mapping the industries from which public restricted financial firms' top executives came, moved to, or worked simultaneously during 1995–2018 might partially explain the significant compensation decreases described above. Figures 3(a)–3(c) present these distributions and transition matrices showing executive movements between industries (rows to columns) during this period. Figure 3(a) shows that among restricted financial firms' top executives who came from unrestricted public firms, 36% came from Investment and Holdings, 29% from Trade and Services, 21% from Real Estate, and 14% from Financial Services. Examining destination industries (Fig. 3b), 33% moved to Real Estate firms, 19% to Investment and Holdings, 19% to Trade and Services, 15% to Manufacturing, 11% to Financial Services, and 4% to Technology firms. Three of these executives moved to Real Estate firms after the Act's introduction.

The industry distribution of unrestricted firms where top executives work simultaneously while employed at restricted firms (Fig. 3c) shows that 25% belong to Investment and Holdings, 25% to Real Estate, 25% to Manufacturing, 16% to Trade and Services, and 8% to Financial Services. In 60% of these cases, the firms share the same controlling

[17] The number of Above executives in the Biomed industry is small (only two).

shareholders.[18] This analysis reveals a positive correlation (74%) between the rate of compensation decline after the Act and the average proportion of executives who moved to or from restricted firms within the same industry.[19] Industries with more frequent executive movements to or from restricted firms experienced higher rates of compensation decline following the Act. This supports the hypothesis of an interconnected market for top executives.

As mentioned above, the underlying assumption of the hypothesis that there is an interlinked market for top executives is that the skills of executives in restricted financial firms are similar to those required in unrestricted firms. Using age as a proxy for executive experience, I find no significant difference between top executives in restricted firms and those in unrestricted industries that experienced high rates of compensation decline after the Act (Financial Services, Real Estate, Investment and Holdings, and Trade and Services). However, examining educational differences, I find that the percentage of executives with Master's degrees or higher is much higher in restricted firms compared to unrestricted firms in these industries (46% vs. 31%). Notably, Graham Rozen (2024a) finds that education level has no effect on executive compensation. This suggests that Israeli companies do not assess managerial skills based on educational attainment.

Restricted financial firms are significantly larger than unrestricted firms—the upper quartile of unrestricted firms (by total assets) is roughly equivalent in size to restricted financial firms in the 25th percentile. Moreover, numerous studies find a significant positive correlation between company size and CEO compensation levels. Gabaix and Landier (2007), for example, show that the substantial increase of over 500% in average real CEO compensation in the US during 1980–2003 can be attributed almost entirely to the growth rate of the companies that US CEOs managed. Therefore, I expect that companies most affected by the Act would be those closest in size to restricted firms. To examine this, I divide unrestricted firms into four quartiles by total assets and present total compensation before (2014–16) and after (2017–19) the Act for each group. Figure 4 shows the results, demonstrating that compensation declines after the Act occurred primarily among executives in companies in the upper quartile (by size).

[18] The Business Concentration Law prohibits groups from owning both financial and non-financial enterprises since December 2019. Groups with both must divest one or the other.
[19] For each industry, I averaged the three relative proportions mentioned above.

4. Empirical approach and results

4.1 The level of compensation

I use the introduction of Israel's Executive Compensation Cap Law in 2016 to examine whether the law indirectly affects compensation of executives in unrestricted financial and non-financial firms. To examine the dynamic effects of the Act and validate the parallel trends assumption, I estimate the following difference-in-differences specification with year-by-year interaction terms, where the unit of observation is executive i at firm j in year t as follows:

(1)

$$\ln(Comp)_{ijt} = \beta_0 + \sum_{t \neq 2016} \beta_t \left(Year_t \; x \; Above\; executive_{ij}\right) + Above\; executive_{ij} + \gamma E_{ijt} + \delta F_{jt} + \alpha_j + \varepsilon_{ijt}$$

Where $\ln(Comp)_{ijt}$ is the natural log of total compensation. Above executive is a dummy variable equal to 1 if the executive is classified as Above and 0 if classified as Below. As mentioned above, an executive is classified as "Above" if compensation to that position exceed NIS 2.5 million in at least two years during 2014–16. The coefficient β_t capture the differential compensation gap between Above and Below executives in year t relative to the base year 2016. Under the parallel trends assumption, the pre-law coefficients ($\beta_{2013}, \beta_{2014}, \beta_{2015}$) should be statistically indistinguishable from zero, indicating that Above and Below executives followed similar compensation trends prior to the Act. Post-law coefficients ($\beta_{2017}, \beta_{2018}, \beta_{2019}$) capture the dynamic evolution of the treatment effect.

E_{ijt} is a vector of executive control variables including age, tenure, education (a dummy variable equaling 1 if the executive holds a Master's degree), executive holdings in the company, turnover (a dummy variable equaling 1 if it is the executive's first year in the position) and appointment percentage variable (full-time or part-time). F_{jt} is a vector of firm-level controls: size (natural logarithm of total assets[20]) and performance (lagged stock

[20] According to Gabaix and Landier (2008), total assets (debt + shareholders' equity) is the best proxy for company size.

return, ROA, or ROE[21,22]).[23] The regressions include firm fixed effects (α_j) to control for unobserved time-invariant firm characteristics[24,25]. In robustness tests, I augment the baseline specification with industry×year fixed effects to account for potential industry-specific shocks during the sample period; results are qualitatively unchanged (Table 9, Column 4). Errors are clustered within firm. All data are in constant 2016 prices.

An alternative identification strategy would be a regression discontinuity design (RDD) centered around the NIS 2.5 million threshold. However, this approach is not applicable in the current setting for two reasons. First, all firms in my sample are legally unrestricted and face no binding obligation to adjust compensation at any threshold; therefore, no mechanical discontinuity in behavior is expected at the 2.5 million cutoff. This distinguishes my design from studies such as Abudy et al. (2020), who apply an RDD to restricted financial institutions that were legally required to comply with the cap. Second, the statutory threshold is not a fixed numerical cutoff but is defined as 35 times the lowest-paid employee's wage, making the precise threshold firm-specific and time-varying — a property that precludes a clean RDD implementation (see footnote number 6). In my setting, the threshold is relevant only insofar as it operates as a salient behavioral anchoring point for compensation decisions at unrestricted firms — a hypothesis I test directly in Table 8.

Table 3 presents the main regression results examining compensation dynamics in unrestricted firms around the Act. The dependent variable is the natural logarithm of total compensation in columns (1), (2), and (4), and the natural logarithm of non-equity compensation in columns (3) and (5). Since firms may report equity-based compensation in years following the grant date for accounting purposes, potentially distorting my

[21] I use lagged performance measures to avoid endogeneity. Using contemporaneous measures does not change the results. The correlation between executive compensation and performance has weakened over time—see Graham Rozen (2024a) for details.

[22] Using lagged stock return or lagged ROA or lagged ROE for performance did not change the significance of the results.

[23] Control variables are selected based on their theoretical relevance to executive compensation and their availability in the data.

[24] While both firm and industry fixed effects would be appropriate, some firms change industries during the sample period. Firm fixed effects subsume industry fixed effects for firms that remain in the same industry throughout the sample, and the addition of industry×year fixed effects in robustness tests (Table 9) confirms that results are not driven by industry-specific shocks.

[25] An alternative within-firm specification would compare Above and Below executives within the same firm using executive-firm pair fixed effects. However, only 13% of unrestricted firms (41 of 315) employ multiple highly-compensated executives with variation around the NIS 2.5M threshold, reflecting Israel's concentrated executive compensation structures. Given this limited variation, I focus on firm fixed effects combined with matching methods (Section 5.2) to address selection concerns.

compensation measures, I use non-equity compensation as an alternative dependent variable in columns (3) and (5) to ensure robustness.

Column 1 presents year fixed effects for the full sample without distinguishing between Above and Below executives. The year coefficients are positive but statistically insignificant in the post-law period, indicating no meaningful average effect of the Act on executive compensation when all executives are pooled together.

Columns 2–5 present the dynamic difference-in-differences estimates. The pre-law interaction coefficients (2013×Above, 2014×Above, 2015×Above) are small and statistically indistinguishable from zero across all specifications (Pre-trend F-test p-values range from 0.117 to 0.384), confirming that Above and Below executives followed parallel compensation trends prior to the Act. Figure 5 presents the estimates from column 2 graphically; the pre-law coefficients are jointly indistinguishable from zero ($F(2,322) = 0.26$, $p = 0.77$), and the post-law coefficients turn sharply negative, illustrating the divergence between Above and Below executives following the Act.

Following the law's implementation, a clear pattern emerges. In column 2, the post-law interaction coefficients turn negative and statistically significant, with Above executives experiencing a compensation decline of approximately 9 percent in 2017, deepening to 22 percent in 2018, and remaining at 21 percent in 2019 relative to the base year 2016. This finding is robust to alternative compensation measures — the negative effect on Above executives remains statistically significant when using non-equity compensation as the dependent variable (column 3). A potential concern is whether this compensation decline reflects executive turnover rather than genuine pay adjustments for continuing executives. Columns 4 and 5 address this by restricting the sample to executives who remained in their positions throughout the sample period. The interaction coefficients remain negative and significant, confirming that incumbent executives also experienced compensation reductions following the Act. Column 2 therefore serves as the baseline specification for subsequent analyses.

A potential concern is that the documented compensation decline reflects mean reversion rather than a response to the Act. In the United States, mean reversion in executive pay is often driven by the episodic nature of equity grants, which create lumpiness in measured compensation. However, Israeli executive compensation is substantially more fixed-salary based than its U.S. counterpart. As shown in Table 2, fixed compensation accounts for approximately 57 percent of total pay for Above executives — the group of primary interest

— and this proportion remains stable across the pre- and post-Act periods (57.3 percent versus 59.5 percent), suggesting that the compensation structure itself did not change following the legislation. The relative stability of pay composition mitigates concerns that the observed declines reflect mechanical mean reversion driven by episodic equity grants.

To examine whether the Act affected Above executives across different sectors, I estimate equation (1) separately for financial and non-financial firms. Table 4 presents these results.

Column 2 shows that in non-financial firms, Above executives experienced significant compensation declines following the Act, with coefficients of -0.074 in 2017, deepening to -0.235 in 2018, and remaining at -0.202 in 2019 — corresponding to declines of approximately 7%, 21%, and 18% respectively. The pre-law interaction coefficients are jointly insignificant (p=0.276), confirming parallel pre-trends. Column 1 examines financial firms. Although the post-law coefficients are negative in all three years (ranging from -0.126 to -0.317), none are statistically significant at conventional levels. This likely reflects the small sample size — only 9 firms and 90 observations — which substantially limits statistical power rather than indicating an absence of an economic effect. The pre-trend F-test yields p=0.096, which is marginally insignificant at the 5% level, again likely driven by the limited sample.

4.2 Additional tests

According to the previous section, compensation declined among Above executives following the Act, in both financial and non-financial unrestricted firms. I attribute this finding to an interconnected market for top executives. In this section, I examine potential mechanisms that led to these compensation changes. Restricting executive compensation in one group of companies can affect compensation in other companies through various channels: (1) Voting by restricted firms. Institutional investors in Israel, classified as restricted firms subject to the 2016 law, are obligated to vote at shareholder meetings of companies in which they invest. Hence, these firms participate in compensation approval votes at some unrestricted firms. Executives of restricted firms, following policy changes,[26] may be unwilling to approve compensation exceeding their own threshold while their compensation declines. If so, they would vote for pay cuts or against pay raises. (2) Information flows through director networks. While executive compensation information

[26] Some institutional investors published voting policies under their Investment Committee decisions, explicitly referencing the 2016 law's threshold.

is publicly available through periodic reports, soft information about the Act's internal consequences—such as executive motivation, the new incentive structure under the limitation, or changes in compensation determination—is not public. Fifteen percent of directors in Israel sit on multiple boards.[27] Therefore, directors serving on both restricted and unrestricted firm boards may transmit information regarding compensation reduction processes. However, since board membership is not exogenous,[28] I cannot draw causal conclusions from this analysis. (3) Other unmeasurable channels, such as public criticism at the time and mandatory compensation disclosure.

To examine each mechanism, I estimate difference-in-differences specifications that include a post-law indicator interacted with the Above dummy, rather than year-by-year interactions. This more parsimonious specification is appropriate for testing the presence and direction of mechanism-specific effects; the parallel trends assumption underlying this identification is validated by the pre-trend tests in Table 3.

I begin with the first mechanism. Ideally, this would be tested using voting data on compensation approvals at unrestricted firms. However, the data required to systematically match votes to specific compensation proposals are not readily available. Therefore, I use restricted firm holdings[29] in unrestricted firms as a proxy. According to the 2013–19 data, 55% of unrestricted firms are held by restricted firms. Among these, the average holdings rate is 15%, the median is 12.7%, the maximum is 64%, and the minimum is 0.08%. To examine whether restricted firm holdings affect executive compensation levels after the Act, I compare changes in total compensation of Above executives between the pre-Act period (2014–16) and post-Act period (2017–19) for firms held versus not held by restricted firms (Figure 6(a)). The figure shows almost no difference in the rate of compensation decline between the two groups—10% versus 12%. Figure 6(b) presents a similar comparison, distinguishing between firms held by restricted firms with stakes below 5% (or not held at all) and firms held with stakes above 5%. This graph suggests that compensation declines among Above executives were larger in firms held by restricted firms with stakes above 5%—13% versus 8%.

To test this mechanism formally, I estimate a difference-in-differences specification and add the variable restricted firm holdings rate —Res_hold_{ij} - as a control variable both

[27] Michelson, N. (2022).
[28] According to Hermalin and Weisbach (2001), a key obstacle to understanding causal effects of board overlap on corporate outcomes is the difficulty of isolating exogenous influences.
[29] Such as mutual funds, insurance companies, etc.

independently and interacted with the $Post\ 2016_t$ and $Above\ executive_{ij}$, where $Post\ 2016_t$ is a dummy variable equal to 1 for years after 2016. To keep the triple interaction structure tractable, I use this post-law indicator rather than year-by-year interactions; the parallel trends assumption underlying this identification is validated in Table 3.

Table 5 presents the results. Column 1 shows that the coefficient on the triple interaction term is not statistically significant, indicating no differential effect of restricted firm holdings on compensation changes for Above versus Below executives following the Act. In Column 2, I replace the continuous holdings variable with a dummy variable equaling 1 if the firm is held by restricted firms and 0 otherwise. The coefficient increases in magnitude but remains insignificant. In Column 3, I use a dummy variable equaling 1 if holdings exceed 5% and 0 otherwise. The results remain unchanged.[30] These findings suggest that compensation declines among Above executives following the Act were not systematically related to the extent of restricted firm ownership.

The second mechanism examines whether information flows through director networks. According to 2017–18 data, 25% of unrestricted firm boards include one or more directors who sat on a restricted firm board in 2016 (the year the Act was enacted) or serve simultaneously on both boards (hereafter: "mutual directors"). This proportion is higher among Above firms (38%) than Below firms (21%). To examine the correlation between board overlap and compensation changes following the Act, Figures 7(a) and 7(b) present differences in total compensation between the pre-Act period (2015–16) and post-Act period (2017–18) for Above and Below executives, respectively. Each figure displays four groups: (a) firms with one or more board members also serving on restricted firm boards; (b) firms with exactly one such board member; (c) firms with more than one such board member;[31] and (d) firms with no such overlap. The figures show that board overlap correlates positively with compensation changes following the Act for both groups, with a stronger correlation when multiple directors overlap. This pattern may reflect information transmission about negative consequences of sharp compensation reductions in restricted firms (frustration, dissatisfaction, potential resignations), leading boards to moderate compensation cuts—particularly given periodic proposals to extend the law to unrestricted

[30] An alternative approach—estimating equation (1) with the restricted firm holdings rate for Above executives only—yields consistent results.
[31] Approximately 24% of firms with at least one mutual director have more than one such director.

firms.[32] An alternative explanation is that these directors may have recommended raising compensation to attract executives departing from restricted firms following compensation cuts.[33] However, as noted above, this result is unlikely to be causal, as board overlap tends to occur between related firms or industries.

5. Robustness Tests

In this section, I conduct extensive robustness tests to validate my main findings and address potential concerns regarding identification and alternative explanations. First, I conduct placebo tests to verify that the effects I observe are specific to the timing of the legislation. Second, I employ propensity score matching and entropy balancing to address potential selection bias. Third, I examine the mechanism underlying these effects by testing whether the regulatory threshold became a reference point in compensation decisions. Finally, I perform various sensitivity analyses including alternative sample specifications, threshold definitions, industry×year fixed effects, median regression, and a subsample restricted to firms with no executive mobility to or from restricted firms.

5.1 Placebo Tests

To rule out the possibility that my results are driven by pre-existing trends, I conduct a placebo test using an alternative time period well before the actual legislation. If my effects genuinely stem from the 2016 legislation, I should not observe similar patterns when artificially designating earlier years as the treatment period.

Table 6 presents a placebo specification that designates 2013-2014 as the "pre-period" and 2015-2016 as the "post-period," both entirely before the law's enactment. The coefficient on Post × Above is small in magnitude and statistically insignificant. The absence of effects in this placebo period, contrasted with the strong effects observed for the actual treatment period (Table 3), provides evidence that my results are not driven by pre-existing differential trends between Above and Below executives.

[32] Since the Act's introduction, proposals have occasionally emerged to extend the law to other firms as well.
[33] Graham Rozen (2024b) shows that significant compensation decreases in restricted firms following the Act.

5.2 Propensity Score Matching and Entropy Balancing

A key concern is that Above and Below executives differ systematically on observable characteristics that may independently affect compensation trends. To address this, I implement two matching approaches that balance treatment and control groups on pre-period observables. For each executive in my sample, I calculate average firm characteristics and executive characteristics during the pre-reform period (2014-2016). These pre-period averages serve as the basis for balancing treatment and control groups.

First, I employ propensity score matching (PSM). I estimate the propensity of being an Above executive using a logit model based on these pre-period averages. Using nearest-neighbor matching with a caliper of 0.05, I match each Above executive to a Below executive with similar predicted probability of treatment. This procedure yields 759 matched pairs. Second, I implement entropy balancing (Hainmueller, 2012), which reweights the control group to achieve exact covariate balance without discarding observations. This approach retains nearly the full sample (N=2,203) while ensuring balance on all pre-period characteristics.

Table 7 presents results from both approaches alongside the baseline specification. All three specifications use the same sample restrictions and include the same time-varying control variables. The only difference is the weighting: Column 1 uses equal weights (baseline), Column 2 uses PSM weights, and Column 3 uses entropy balancing weights. Column 2 shows that the PSM coefficient remains negative and of similar magnitude (-0.210 vs. -0.179), though precision is reduced due to the smaller matched sample Column 3 shows that with entropy balancing, the coefficient remains negative and significant (-0.184, $p<0.05$). The consistency in coefficient sign and magnitude across all three specifications—ranging from -0.179 to -0.210—demonstrates that the decline in Above executive compensation following the reform is robust to controlling for observable differences between Above and Below.

5.3 Benchmarking Analysis: The Threshold as a Reference Point

A key mechanism through which the law may have directly affected unrestricted firms is by establishing the NIS 2.5 million cap as a salient reference point in executive compensation decisions. A typical benchmark for executive compensation is the median compensation of peer executives—firms of similar size in the same industry. Compensation

committees are assumed to adjust CEO pay toward these peer benchmarks to correct prior-year deviations (Grinstein et al., 2020). I test whether the regulatory threshold became an additional benchmark that influenced compensation decisions after the legislation, potentially explaining the direct unintended effects I observe. Specifically, I estimate the following specification for CEOs:

(2)

$$\Delta\ln(\text{Comp}_{ijt}) = \beta_0 + \beta_1\left(\frac{\text{CEO Total compensation}}{\text{Peer Total compensation}}_{i,t-1} - 1\right) + \beta_2\left(\frac{\text{CEO Total compensation}}{\text{"Threshold" Level}}_{i,t-1} - 1\right) + \beta_3\left[\left(\frac{\text{CEO Total compensation}}{\text{"Threshold" Level}}_{i,t-1} - 1\right) x\ Post\ 2016_t\right] + \gamma E_{ijt} + \delta F_{jt} + \alpha_j + \varepsilon_{ijt}$$

where $\Delta\ln(\text{Comp}_{ijt})$ is the annual change in log compensation from year t−1 to t. I construct two relative compensation measures. *Relative Comp. to Peer* is the CEO's lagged compensation divided by the peer group median, minus one. Peer firms are defined as those in the same industry and size tercile (dividing each industry into three equal groups by total assets)[34]. It is worth noting that according to the Graham-Rozen (2024) database, 42 percent of executive movements between public firms occur within peer groups as defined above. *Relative Comp. to Threshold* measures the distance between the firm's CEO compensation and the NIS 2.5 million threshold at year t-1, minus one. Post 2016 s an indicator variable equal to one for years after 2016. E_{ijt} is a vector of executive-level controls including changes in the executive's shareholdings, turnover, and appointment status (full-time versus part-time). F_{jt} is a vector of firm-level controls including changes in total assets (log) and stock returns. The regressions include firm and year fixed effects. Standard errors are clustered at the firm level.

The key coefficient of interest is β_3, which tests whether the threshold became a significant convergence point after the legislation. Table 8 presents the results. Column 1 examines the full sample period. The peer benchmarking coefficient is negative and highly significant, confirming the well-documented pattern that firms adjust CEO pay toward peer levels. Critically, the threshold shows no significant effect before the legislation, but the

[34] Bizjak et al. (2011) use Ln(Relative compensation_{i,t−1}) as their benchmarking variable, defining it as the natural logarithm of peer-based target pay divided by the firm's CEO pay level.

Post×Threshold interaction is negative and significant, demonstrating that the regulatory cap became a benchmark only after 2016. Column 2 restricts to 2017-2019 to compare effect magnitudes directly. Both benchmarking effects are significant, but the threshold effect is nearly twice as large as the peer effect[35]. Column 3 presents a placebo test using 2014-2016 as the sample period. The Post×Threshold coefficient is small and statistically insignificant, confirming that the anchoring effect emerged only after the Act and was not present in the pre-legislation period.

These results indicate that the legislation established the NIS 2.5 million cap as a particularly salient reference point in compensation decisions, with the regulatory threshold becoming at least as influential as traditional peer benchmarking at unrestricted firms.

5.4 Additional Sensitivity Tests

Table 9 presents results from various alternative specifications to assess sensitivity to sample definition, threshold choice, potential industry-specific shocks, and the influence of extreme observations. Column 1 restricts the sample to 2015-2018, focusing on the two years immediately before and after the Act. Column 2 uses an alternative threshold of NIS 2.8 million, narrowing the definition of Above executives. Column 3 modifies the Above classification to require only one year (instead of two) with compensation above NIS 2.5 million during 2014-2016. Column 4 replicates the baseline specification augmented with industry×year fixed effects to account for potential industry-specific shocks during the sample period. Column 5 estimates the baseline specification using median regression to assess whether the findings are driven by extreme observations within the Above group. Across all five specifications, the post-law interaction coefficients remain negative and statistically significant, with pre-trend F-tests confirming parallel trends in all cases (p-values of 0.240, 0.604, 0.606, 0.201, and 0.840, respectively). The 2018 coefficient in Column 5 remains negative and significant, confirming that the findings are not exclusively driven by outliers. At the same time, the smaller magnitude relative to the OLS estimates suggests that the effect is more pronounced among higher-paid executives within the Above group. This is consistent with the anchoring mechanism: executives whose compensation

[35] An F-test confirms these coefficients differ (F=2.65, p=0.10). In economic terms, a CEO earning 20% above the threshold experiences roughly 9 percentage points slower annual pay growth, compared to only 4 percentage points for a similar deviation from peer median. The correlation between peer-relative and threshold-relative compensation is 0.70; estimating the model with each measure separately yields similar results, indicating that multicollinearity does not drive my findings.

was furthest above the threshold had greater room for adjustment. The consistency across sample definitions, thresholds, and fixed effects structures demonstrates that the results are not driven by specific modeling choices.

As an additional test of the direct anchoring interpretation, I restrict the sample to firms with no executive mobility to or from restricted firms during 2013–2019. The post-law interaction coefficients remain negative among this subsample, with the 2019 coefficient statistically significant ($p=0.033$; results untabulated), suggesting that direct anchoring contributes to the observed compensation changes beyond what can be attributed to executive labor market spillovers alone.

5.5 Summary of Robustness Tests

My main results remain robust across multiple specifications. Propensity score matching and entropy balancing confirm that findings are not driven by selection bias. Placebo tests verify that effects are specific to the 2016 legislation. Benchmarking analysis reveals that the regulatory threshold became a reference point in compensation decisions. Results also hold when using alternative threshold definitions, different classifications of above-threshold status, and various sample period restrictions. These findings provide consistent evidence that sector-specific compensation regulations can generate unintended effects, with the regulatory cap becoming a salient focal point in executive compensation decisions economy-wide. Additionally, the documented compensation decline is unlikely to reflect mean reversion, given that Israeli executive compensation is predominantly fixed-salary based with stable pay composition across the pre- and post-Act periods. Finally, the persistence of effects among firms with no executive mobility to or from restricted firms supports the direct anchoring interpretation over indirect spillover channels.

6. Conclusion

The literature on regulatory restrictions on executive compensation has focused almost exclusively on direct effects within firms explicitly subject to regulation. This paper examines whether sector-specific compensation regulations generate unintended consequences for firms outside the regulatory scope. Exploiting Israel's 2016 executive compensation law, which imposed binding caps on pay at financial institutions while leaving all other sectors unrestricted, I find that executives at unrestricted firms previously

compensated above the NIS 2.5 million threshold experienced substantial pay reductions in the years following implementation. These results are robust to a dynamic difference-in-differences design, propensity score matching, entropy balancing, placebo tests, industry×year fixed effects, median regression, and various sensitivity analyses.

I identify two primary mechanisms. First, substantial executive mobility between restricted and unrestricted sectors created an interconnected labor market where regulatory interventions in one sector affected compensation dynamics economy-wide. Industries with more frequent executive movement between sectors experienced larger compensation declines following the legislation. Second, benchmarking analysis reveals the regulatory threshold became a dominant reference point in compensation decisions, with firms showing significantly greater sensitivity to the regulatory cap than to traditional peer benchmarks — consistent with anchoring theory where salient reference points influence decision-making. The absence of significant transmission through ownership and director network channels, combined with the persistence of effects among firms with no executive mobility to or from restricted firms, supports the direct anchoring interpretation over indirect spillover channels.

This pattern supports rent extraction theory over efficient contracting: the voluntary nature of reductions at legally unrestricted firms suggests previous pay levels exceeded efficient outcomes, with regulation serving as a coordination mechanism enabling boards to justify reductions they were previously reluctant to implement.

These findings open avenues for future research. Access to detailed voting records on compensation proposals would enable direct examination of whether institutional investors from restricted firms systematically influenced compensation decisions at unrestricted firms through their voting behavior — a mechanism my ownership-based proxies cannot definitively test. Additionally, analyzing how media coverage and public discourse around regulatory thresholds affect the salience and persistence of anchoring effects could provide insights into the conditions under which such unintended consequences emerge.

The Israeli natural experiment offers valuable lessons for regulatory authorities worldwide. As regulators continue grappling with executive compensation concerns, understanding these unintended consequences becomes crucial for designing effective policies that balance targeted intervention with broader governance implications. My findings demonstrate that prominent regulatory thresholds can influence corporate practices well

beyond their intended scope, creating economy-wide effects even without direct legal mandates.

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Appendix A. Variable Definitions

Variable	Definition	Source
Firm's Issuer Number	Company number (according to Stock Exchange symbol)	TASE data
Executive's position	Categorical variable: 0 if board chairperson; 1 if CEO; 2 if both chairperson and CEO; 3 otherwise	Manually collected from periodic and annual reports
Co-CEO	Dummy variable equal to 1 if the executive is a Co-CEO or Co-Chairperson, 0 otherwise	Manually collected from periodic and annual reports
Deputy CEO	Dummy variable equal to 1 if the executive is a Deputy CEO, 0 otherwise	Manually collected from periodic and annual reports
Name	Executive's name	Manually collected from periodic and annual reports
ID	Executive's ID number	Manually collected from periodic and annual reports
Age	Executive's age	Manually collected from periodic and annual reports
Education	Categorical variable: 0 if high school education; 1 if Bachelor's degree; 2 if Master's degree or higher	Manually collected from periodic and annual reports
Board Membership	Categorical variable: 0 if not a board member; 1 if board member; 2 if board chairperson	Manually collected from periodic and annual reports
Family Relation	Dummy variable equal to 1 if the executive is a relative of a controlling shareholder, 0 otherwise	Manually collected from periodic and annual reports
Tenure in the Company	Executive's tenure measured as years with the firm	Manually collected from periodic and annual reports
Tenure in position	Executive's tenure measured as years in current position	Manually collected from periodic and annual reports
Turnover	Dummy variable equal to 1 if executive change occurs in a given year, 0 otherwise	Manually collected from periodic and annual reports
Appointment percentage	Employment percentage (part-time or full-time)	Manually collected from periodic and annual reports

Variable	Definition	Source
Holdings	Executive's ownership percentage in the company	Manually collected from periodic and annual reports
Total Compensation	Total compensation paid to executive (current prices, NIS)	Manually collected from periodic and annual reports
Of which: Salary	Base salary paid to executive (current prices, NIS)	Manually collected from periodic and annual reports
Of which: Social Provisions	Social provisions paid to executive (current prices, NIS)	Manually collected from periodic and annual reports
Of which: Bonuses	Bonuses paid to executive (current prices, NIS)	Manually collected from periodic and annual reports
Of which: Share based compensation	Share-based compensation paid to executive (current prices, NIS)	Manually collected from periodic and annual reports
Of which: Other benefits	Other benefits paid to executive (current prices, NIS)	Manually collected from periodic and annual reports
ROE	Return on equity (net income / total equity)	Annual financial statements
ROA	Return on assets (net income / total assets)	Annual financial statements
Size	Natural logarithm of total assets	Computed by author using annual financial statements
Business Group Affiliation	Dummy variable equal to 1 if firm is business group-affiliated, 0 otherwise	Computed by author using Bank of Israel data
Market Value	Natural logarithm of market value (year-end)	Computed by author using TASE data
Industry	Firm's industry classification	Annual financial statements
Industry ROE	Average return on equity in industry (weighted by total assets)	Computed by author using annual financial statements
TA 100 Index	Dummy variable equal to 1 if company belongs to TA-100 Index, 0 otherwise	Computed by author using TASE data
Trading Years	Number of years company has been trading on TASE	Computed by author using annual financial statements
Leverage	Ratio of book value of liabilities to book value of assets	Computed by author using annual financial statements
Stock Return	Company stock return	Computed by author using TASE data

Variable	Definition	Source
Industry stock return	Industry stock return (weighted by market value)	Computed by author using TASE data
Tobin's Q ratio	(Book value of debt + market value of equity) / book value of assets	Computed by author using TASE data
Public	Dummy variable equal to 1 if company stock is traded on TASE, 0 otherwise	Computed by author using TASE data
Above (Firm-Level)	Dummy variable equal to 1 if at least one executive earned above NIS 2.5 million on average during 2014–2016, 0 otherwise	Computed by author using annual reports
Above (Executive-Level)	Dummy variable equal to 1 if executive's position paid above NIS 2.5 million in at least two years during 2014–2016, 0 otherwise	Computed by author using annual reports
Post 2016	Dummy variable equal to 1 if year is after 2016, 0 otherwise	Computed by author
Restricted firms Holdings	Restricted firms' ownership percentage in the company	Computed by author using TASE data
Number of mutual directors	Number of board directors who also serve on restricted firm boards	Computed by author using annual reports
Ta 125 Index Return	TA-125 Index annual return	Computed by author using TASE data
All Stocks Index Return	All Stocks Index annual return	Computed by author using TASE data

Figure 1: Executive Compensation in Unrestricted firms

Figure 1(a) presents the evolution of average top executive compensation from 2013 to 2019. Figure 1(b) presents the same trends, distinguishing between Above executives (red line) and Below executives (green line). Total compensation in 2013 (in millions of NIS, 2016 fixed prices) is normalized to 100. Dashed vertical lines denote the Act's introduction in 2016. Figures 1(c) and 1(d) present the cumulative distribution function (CDF) of total compensation before (2014–2016) and after (2017–2019) the Act for Below (1c) and Above (1d) executives.

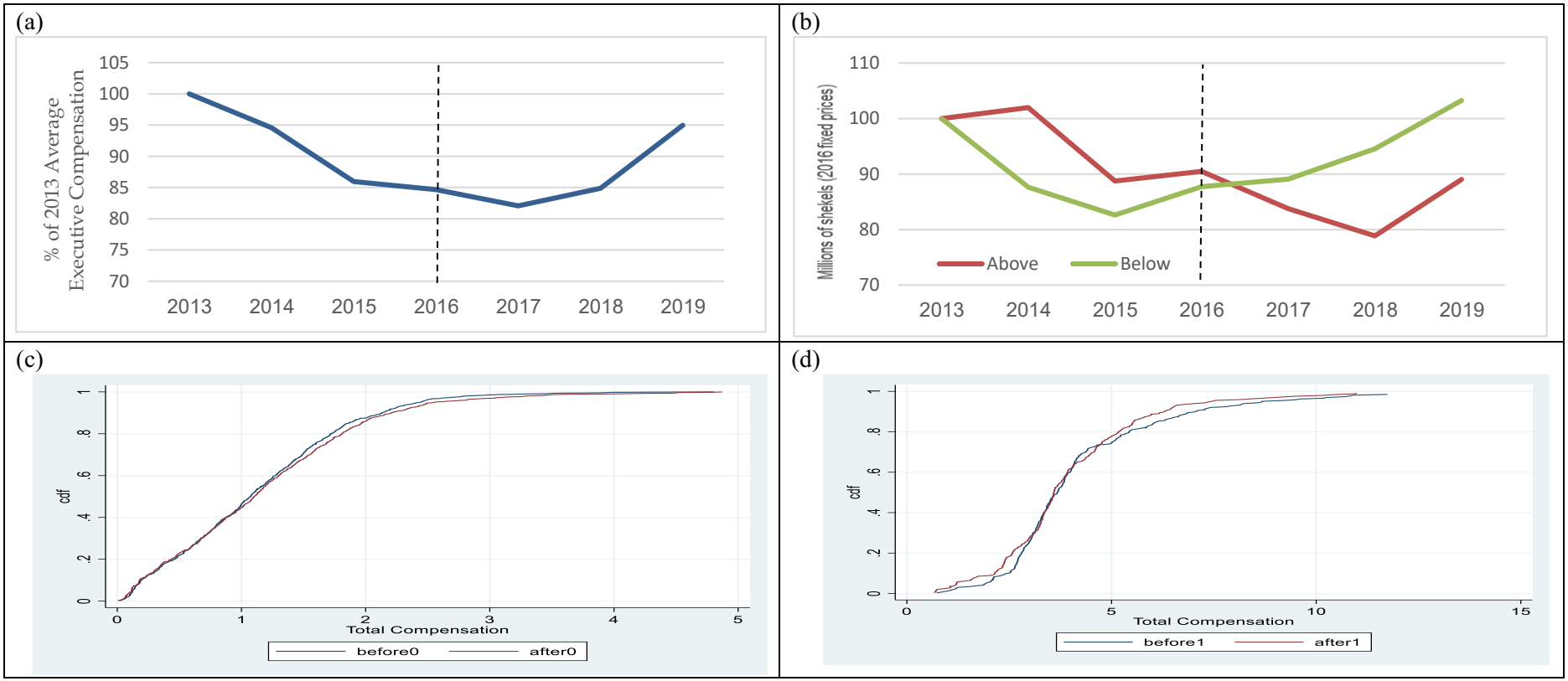


Figure 2: Executive Compensation in Unrestricted firms, by industry

The figure presents differences in total compensation between the pre-Act period (2014–16) and post-Act period (2017–19) for Above and Below executives in each industry. Compensation is in millions of NIS (2016 fixed prices).

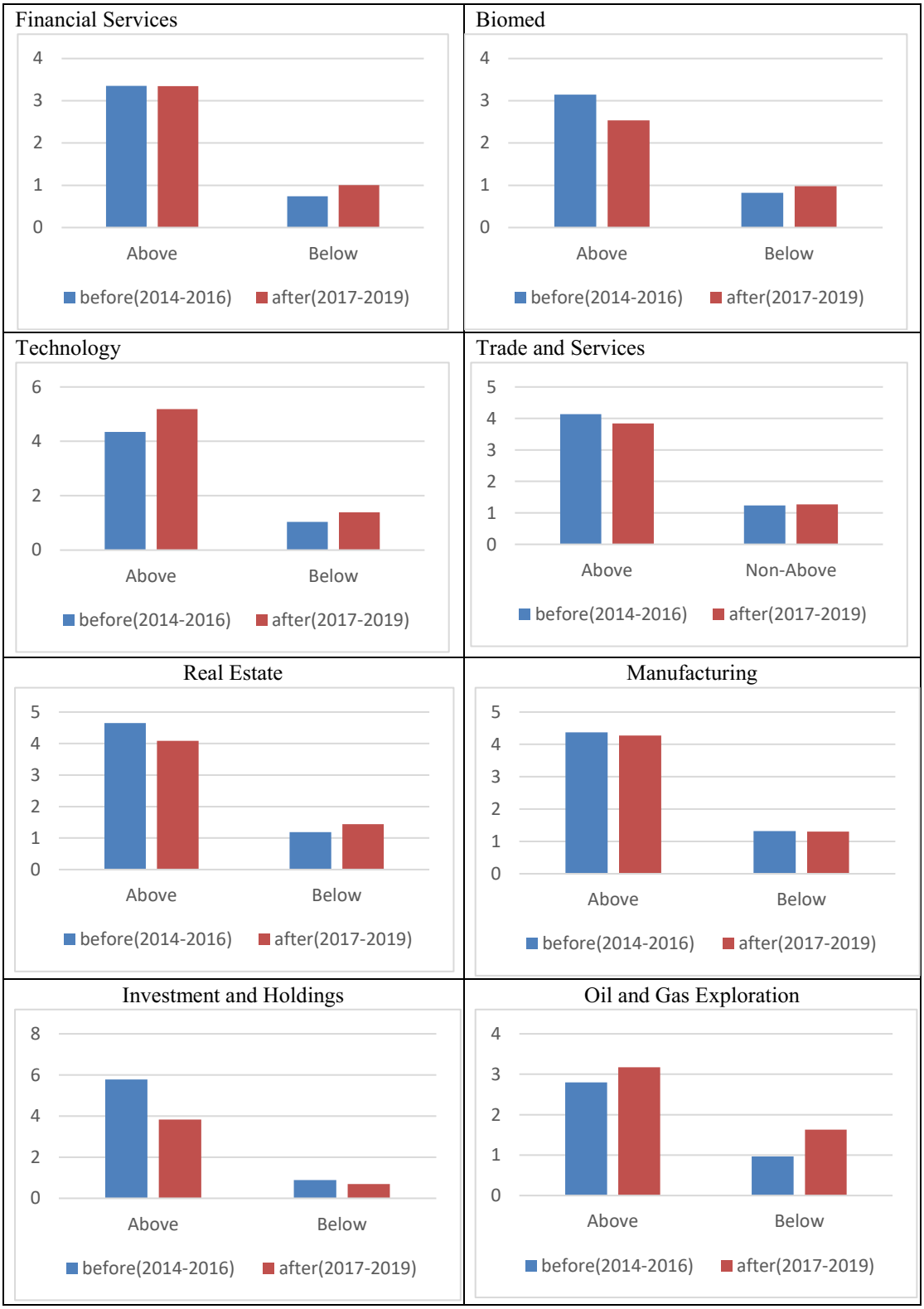
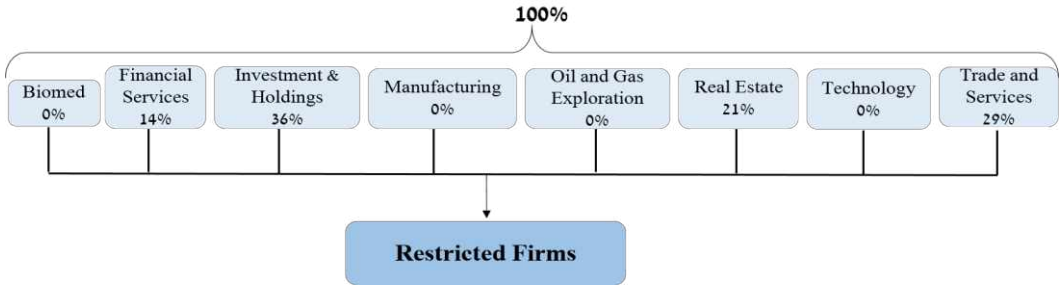


Figure 3: Industry Distribution of Unrestricted Firms with Executive Mobility from Restricted Financial Firms

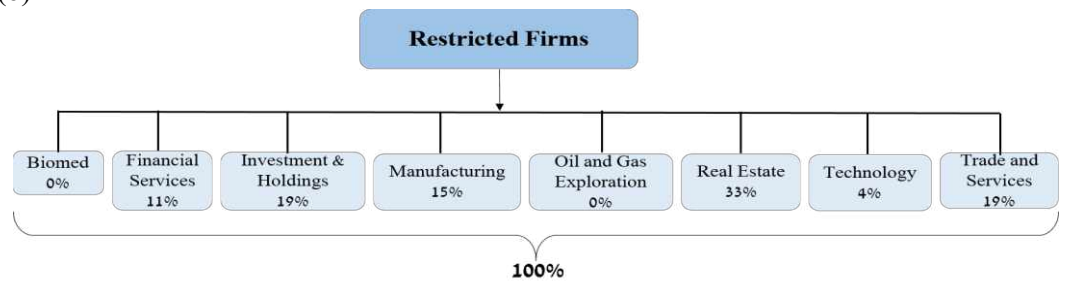
Figures 3(a)–3(c) present the industry distribution of unrestricted firms from which public restricted financial firm executives came (3a), moved to (3b), or worked simultaneously (3c) during 1995–2018, in percent. The figures present transition matrices showing executive movements between industries (rows to columns) during this period.

(a)



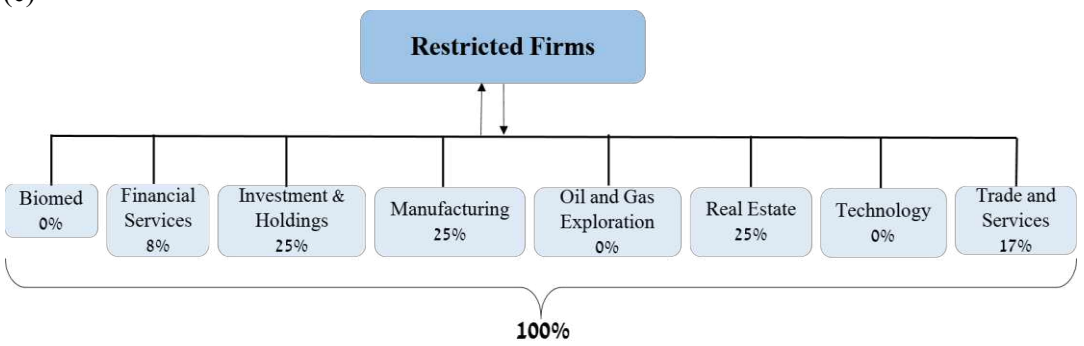
	Investment and Holdings	Financial Services	Insurance	Banks	Total
Investment and Holdings	0	2	3	0	5
Real Estate	1	0	2	0	3
Trade and Services	0	2	2	0	4
Financial Services	0	0	0	2	2
Total	1	4	7	2	14

(b)



	Financial Services	Trade and Services	Real Estate	Investment and Holdings	Technology	Manufacturing	Total
Banks	2	2	5	3	0	2	14
Insurance	0	2	4	0	0	2	8
Financial Services	1	0	0	2	1	0	4
Investment and Holdings	0	1	0	0	0	0	1
Total	3	5	9	5	1	4	27

(c)



	Financial Services	Trade and Services	Real Estate	Investment and Holdings	Manufacturing	Total
Banks	1	1	1	1	1	5
Insurance	0	1	2	2	1	6
Financial Services	0	0	0	0	0	0
Investment and Holdings	0	0	0	0	1	1
Total	1	2	3	3	3	12

Figure 4: Executive Compensation in Unrestricted firms, by size

The figure presents differences in total compensation between the pre-Act period (2014–16) and post-Act period (2017–19) for top executives, divided into four quartiles by firm total assets. Compensation is in millions of NIS (2016 constant prices).

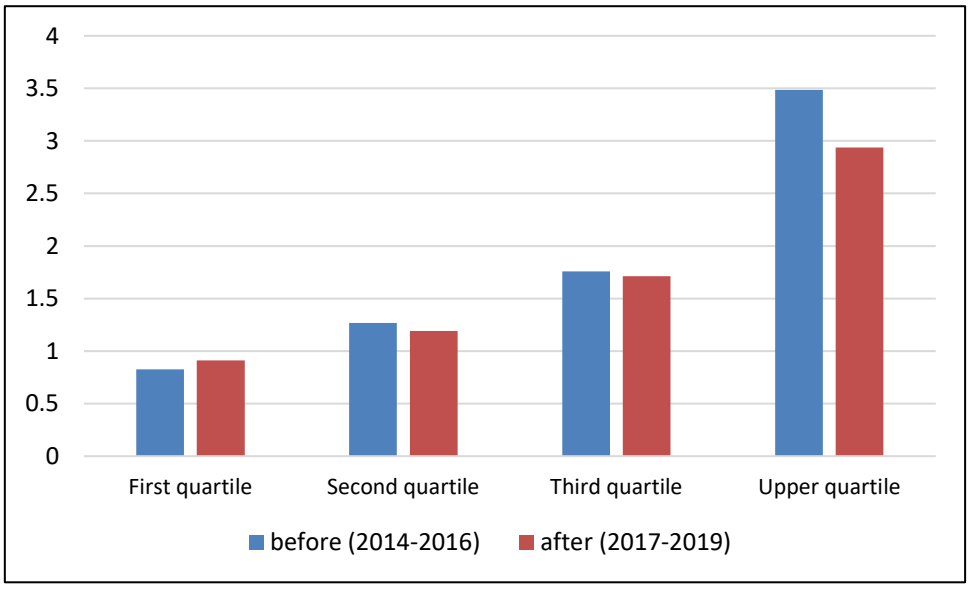


Figure 5: Event Study — Compensation Dynamics of Above vs. Below Executives Around the Act

This figure presents the year-by-year interaction coefficients from equation (1), column (2) of Table 3. Each coefficient captures the differential compensation gap between Above and Below executives in year t relative to the base year 2016. The dashed vertical red line marks the year of the Act's implementation. The vertical bars represent 95% confidence intervals based on standard errors clustered at the firm level.

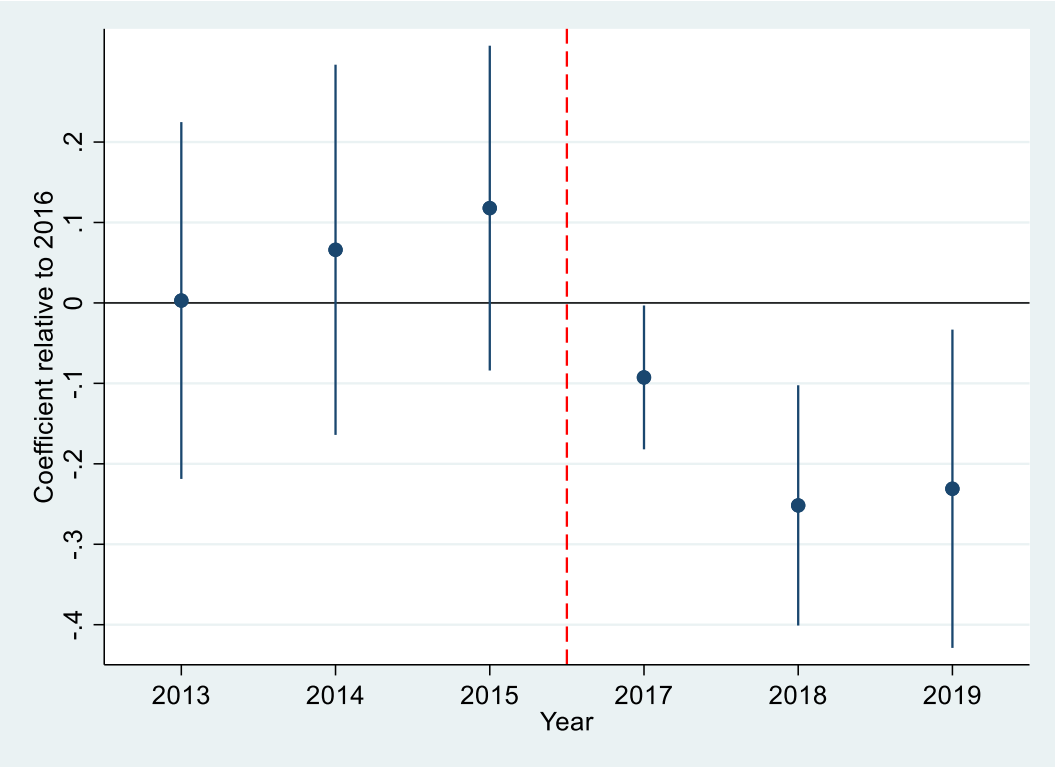


Figure 6: Executive Compensation in Unrestricted Firms by Restricted Firms' Ownership Stakes

This figure shows the change in total compensation for Above executives between 2014–2016 and 2017–2019, by ownership level of restricted firms. Panel (a) compares firms with and without restricted firm ownership. Panel (b) distinguishes between firms with ownership stakes below versus above 5%. Compensation is in millions of shekels (2016 prices).

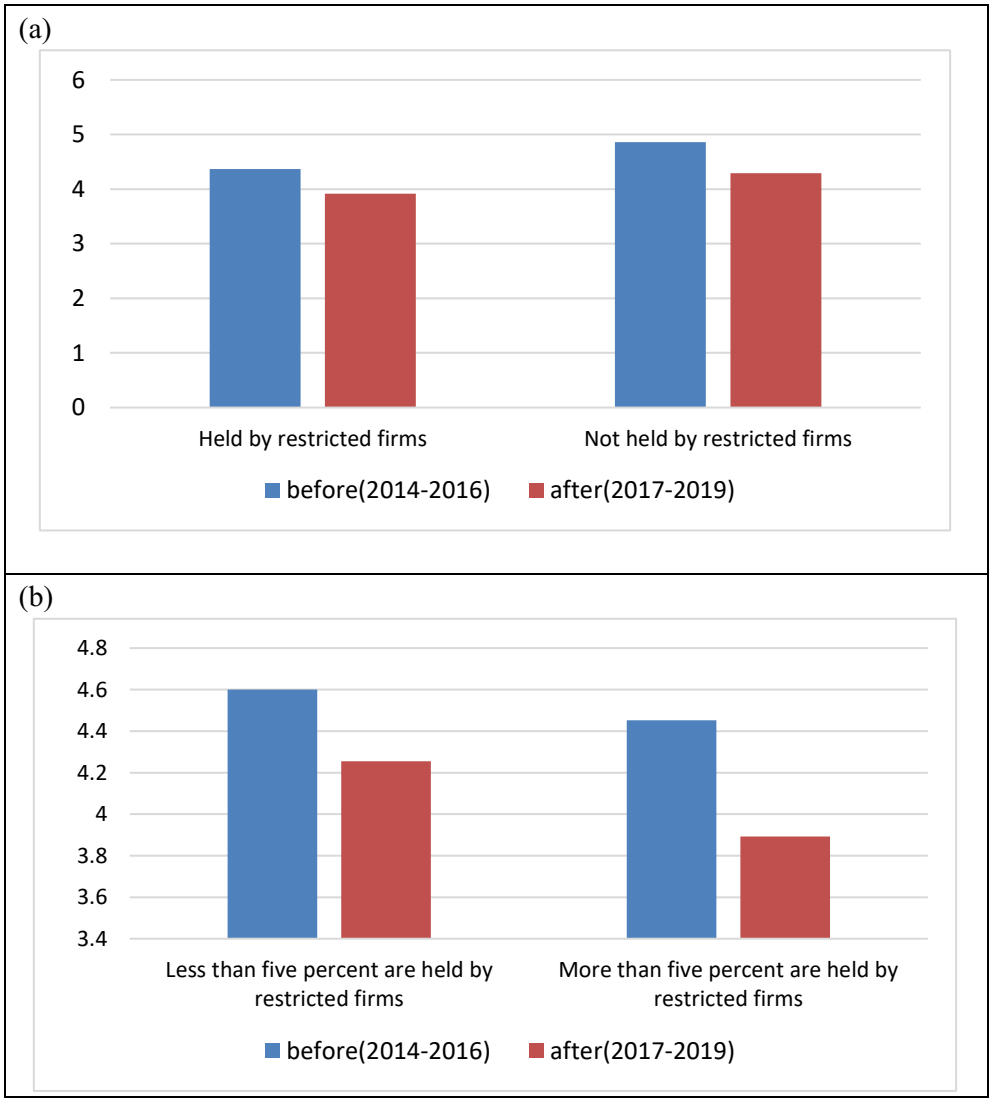


Figure 7: Executive Compensation in Unrestricted Firms by Number of Overlapping Directors

Figures 7(a) and 7(b) present the change in total compensation between the pre-Act period (2015–2016) and the post-Act period (2017–2018) for Above and Below executives, respectively. Each figure distinguishes between three groups based on board overlap with restricted firms: (i) firms with no directors who also serve on a restricted firm's board; (ii) firms with exactly one overlapping director; and (iii) firms with two or more overlapping directors. Compensation is expressed in millions of shekels at 2016 fixed prices.

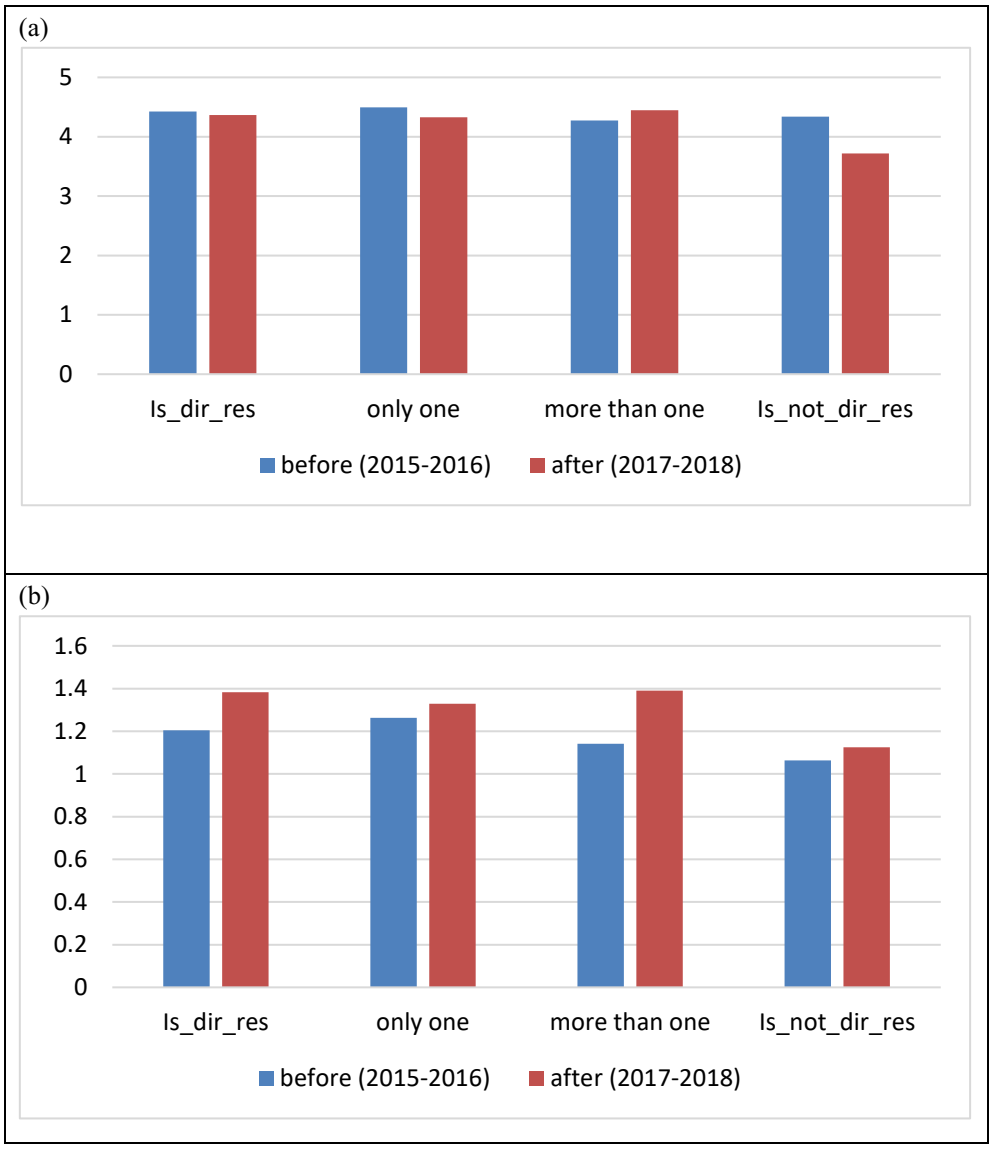


Table 1: Industry Distribution of Unrestricted Firms

This table shows the distribution of unrestricted firms by industry, reporting the number of firms (percentages in parentheses) and distinguishing between Above firms (paying CEO and/or board chairperson above NIS 2.5 million pre-Act) and Below firms. Firms are classified by primary industry affiliation.

	Number of firms	**Of which: Number of "Above" firms**	**Of which: Number of "Below" firms**
Biomed	31 (8.9%)	2 (6.5%)	29 (93.5%)
Financial Services	11 (3.1%)	3 (12.5%)	8 (87.5%)
Investment and Holdings	37 (10.6%)	9 (24.3%)	28 (75.7%)
Manufacturing	65 (18.6%)	16 (24.6%)	49 (75.4%)
Oil and Gas Exploration	8 (2.3%)	2 (25.0%)	6 (75.0%)
Real Estate	88 (25.1%)	30 (34.1%)	58 (65.9%)
Technology	47 (13.4%)	5 (10.6%)	42 (89.4%)
Trade and Services	63 (18.0%)	23 (36.5%)	40 (63.5%)
Total	350	90 (25.7%)	260 (74.3%)

Table 2: Summary statistics

This table presents summary statistics for the sample period 2014–2019. Executive-level variables include age, tenure, ownership stake in the firm, proportion with a Master's degree or higher, total compensation, and fixed compensation. Firm-level variables include total assets, ROE, ROA, stock return, leverage, and executive turnover rate. Panel A reports statistics for all executives, with Panels A1 and A2 showing separate statistics for Above and Below executives, respectively. Above executives are those whose compensation exceeded NIS 2.5 million in at least two of the three pre-Act years (2014-2016); Below executives are the reminder. Each panel distinguishes between the pre-Act period (2014–2016) and the post-Act period (2017–2019).

Panel A: All Executives								
	2014-2016				2017-2019			
	N	Mean	S.D.	Median	N	Mean	S.D.	Median
Executive characteristics:								
Age	1427	56.1	10.2	56.0	1426	57.6	10.5	57.0
Tenure in position (in years)	1439	7.8	8.3	5.0	1430	8.6	8.7	6.0
Executive's holdings (%)	1445	10.4	18.9	0.1	1455	11.1	19.7	0.1
Executives with a Master degree or higher (%)	1417	39.9	49.0	0.0	1435	40.8	49.2	0.0
Compensation Structure:								
Total Compensation (in 2016 fixed prices, NIS, millions)	1445	1.8	2.2	1.3	1456	1.8	1.9	1.3
Fixed comp. (%)	1445	77.5	87.4	89.4	1455	78.5	27.9	85.6
Firm-level information:								
Total assets (NIS, millions)	970	3,512	11,860	458	924	3,478	10,198	610
ROE (%)	989	-2.9	40.6	9.0	967	1.0	37.3	10.1
ROA (%)	989	-5.4	23.7	2.1	967	-3.6	23.0	2.6
Stock return (%)	989	20.2	42.6	14.6	967	18.0	43.8	9.0
Leverage (%)	989	17.2	20.2	12.2	967	19.8	24.7	12.2
CEO turnover (%)	950	12.7	33.4	0.0	931	10.7	31.0	0.0

Panel A1: Above Executives								
	2014-2016				2017-2019			
	N	Mean	S.D.	Median	N	Mean	S.D.	Median
Executive characteristics:								
Age	295	56.3	8.8	57.0	288	58.2	9.0	58.5
Tenure in position (in years)	302	9.6	8.3	7.5	293	10.8	9.1	9.0
Executive's equity (%)	302	10.4	19.6	0.0	296	12.9	22.1	0.1
Executives with a Master degree or higher (%)	296	36.8	48.3	0.0	291	35.1	47.8	0.0
Compensation Structure:								
Total Compensation (in 2016 fixed prices, NIS, millions)	302	4.5	3.4	3.7	296	4.0	2.7	3.5
Fixed comp. (%)	302	57.3	23.1	55.9	295	59.5	27.9	55.9
Firm-level information:								
Total assets (NIS, millions)	275	9,412	18,926	2,818	270	8,298	14,958	2,948
ROE (%)	275	11.1	19.8	13.4	271	12.2	17.3	12.7
ROA (%)	275	3.8	5.3	3.8	271	4.1	4.5	4.0
Stock return (%)	275	23.8	37.6	19.8	271	22.9	41.1	14.6
Leverage (%)	275	12.5	5.3	12.2	271	12.5	5.3	12.2
CEO turnover (%)	231	7.8	26.9	0.0	223	8.5	28.0	0.0

Panel A2: Below Executives								
	2014-2016				2017-2019			
	N	Mean	S.D.	Median	N	Mean	S.D.	Median
Executive characteristics:								
Age	1132	56.0	10.5	55.5	1138	57.4	10.8	57.0
Tenure in position (in years)	1137	7.3	8.2	4.0	1137	8.1	8.4	5.0
Executive's equity (%)	1143	10.4	18.7	0.2	1159	10.6	19.0	0.1
Executives with a Master degree or higher (%)	1121	40.8	49.2	0.0	1144	42.2	49.4	0.0
Compensation Structure:								
Total Compensation (in 2016 fixed prices, NIS, millions)	1143	1.1	0.9	1.0	1160	1.2	1.1	1.1
Fixed comp. (%)	1143	82.8	96.9	100.0	1160	83.3	25.7	96.8
Firm-level information:								
Total assets (NIS, millions)	695	1,178	5,978	236	654	1,488	6,423	314
ROE (%)	714	-8.3	45.1	6.7	696	-3.3	41.9	7.5
ROA (%)	714	-8.9	26.9	1.2	696	-6.5	26.4	1.9
Stock return (%)	714	18.9	44.4	12.2	696	16.0	44.7	6.5
Leverage (%)	714	19.0	23.3	12.2	696	22.6	28.4	12.2
CEO turnover (%)	719	14.3	35.1	0.0	708	11.4	31.9	0.0

Table 3: Executive Compensation in Unrestricted Firms around the Act

This table presents OLS regression estimates examining compensation dynamics in unrestricted firms around the Act. The dependent variable is the natural logarithm of total compensation (columns 1, 2, and 4) or non-equity compensation (columns 3 and 5), expressed in 2016 fixed prices. The sample includes CEOs and board chairpersons of unrestricted firms in Israel between 2013 and 2019. Column 1 presents year fixed effects for the full sample without distinguishing between Above and Below executives. Columns 2–5 present year-by-year interaction terms between each sample year and the Above indicator, with 2016 as the base year. An executive is classified as Above if their position's compensation exceeded NIS 2.5 million in at least two of the three years during 2014–2016. The Pre-trend F-test reports the p-value of a joint significance test of the pre-law interaction coefficients (2013×Above, 2014×Above, 2015×Above); failure to reject confirms parallel pre-trends. Columns 4–5 exclude executives who changed positions during 2017–2019. All specifications include firm fixed effects. Standard errors are clustered at the firm level.

Dependent Variable:	Log (Total Compensation)	Log (Total Compensation)	Log (Non-equity Compensation)	Log (Total Compensation)	Log (Non-equity Compensation)
		All Executives		No Turnover	
	(1)	(2)	(3)	(4)	(5)
Year = 2013	0.151***				
	(0.049)				
Year = 2014	0.110**				
	(0.047)				
Year = 2015	0.046				
	(0.041)				
Year = 2017	0.005				
	(0.029)				
Year = 2018	0.054				
	(0.036)				
Year = 2019	0.095*				
	(0.049)				
Above		0.884***	0.642***	0.841***	0.648***
		(0.140)	(0.144)	(0.180)	(0.181)
2013 x Above		0.003	0.003	0.049	0.043

		(0.113)	(0.100)	(0.120)	(0.103)
2014 x Above		0.066	0.070	0.099	0.116
		(0.117)	(0.101)	(0.126)	(0.110)
2015 x Above		0.118	0.116	0.182	0.183*
		(0.103)	(0.092)	(0.114)	(0.097)
2017 x Above		-0.093**	-0.046	-0.043	-0.007
		(0.045)	(0.071)	(0.045)	(0.079)
2018 x Above		-0.252***	-0.124	-0.153**	-0.059
		(0.076)	(0.083)	(0.077)	(0.092)
2019 x Above		-0.231**	-0.164*	-0.095	-0.069
		(0.101)	(0.092)	(0.104)	(0.103)
Stock Return in t-1	0.000	0.001**	0.000**	0.001*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log (Total Assets)	0.096***	0.119***	0.093**	0.141***	0.092**
	(0.033)	(0.035)	(0.037)	(0.043)	(0.046)
Executive's holdings	0.005**	0.002	0.004*	0.004**	0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Tenure	0.031***	0.030***	0.029***	0.039***	0.038***
	(0.011)	(0.011)	(0.010)	(0.012)	(0.011)
Tenure - squared	-0.001**	-0.001**	-0.001**	-0.001**	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MA	-0.044	-0.074	-0.113	-0.061	-0.112
	(0.073)	(0.074)	(0.073)	(0.082)	(0.082)
Turnover	0.121**	0.118*	0.043		
	(0.058)	(0.060)	(0.054)		
Appointment percentage	0.014***	0.013***	0.014***	0.014***	0.014***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)
Fixed compensation (%)	-0.113	-0.071	-0.623***	-0.062	-0.630***
	(0.125)	(0.093)	(0.183)	(0.111)	(0.220)

Constant	12.977***	10.911***	11.645***	10.518***	11.541***
	(0.492)	(0.475)	(0.546)	(0.598)	(0.686)
Observations	3,278	2,857	2,843	2,331	2,320
R-squared	0.665	0.698	0.681	0.744	0.709
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	No	No	No
No turnover in 2017-19	No	No	No	Yes	Yes
Pre-trend F-test (p-value)		0.384	0.329	0.235	0.117

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Executive Compensation in Unrestricted Firms around the Act, by Sector

This table presents OLS regression estimates examining compensation dynamics in unrestricted firms around the Act, comparing the financial sector (column 1) and non-financial sectors (column 2). The dependent variable is the natural logarithm of total compensation in 2016 fixed prices. The sample includes CEOs and board chairpersons of unrestricted Israeli firms between 2013 and 2019. Column 1 and column 2 present year-by-year interaction terms between each sample year and the Above indicator, with 2016 as the base year. An executive is classified as Above if their position's compensation exceeded NIS 2.5 million in at least two of the three years during 2014–2016. The Pre-trend F-test reports the p-value of a joint significance test of the pre-law interaction coefficients (2013×Above, 2014×Above, 2015×Above). All specifications include firm and executive control variables as in Table 3, as well as firm fixed effects. Standard errors are clustered at the firm level.

Dependent Variable:	Log (Total Compensation)	
	Financial firms	Non-Financial firms
	(1)	(2)
Above	0.298	0.879***
	(0.288)	(0.143)
2013 x Above	-0.385	-0.001
	(0.343)	(0.113)
2014 x Above	-0.347	0.073
	(0.195)	(0.118)
2015 x Above	-0.135	0.130
	(0.161)	(0.104)
2017 x Above	-0.317	-0.074
	(0.176)	(0.045)
2018 x Above	-0.126	-0.235***
	(0.282)	(0.077)
2019 x Above	-0.309	-0.202**
	(0.409)	(0.102)
Constant	12.200***	11.141***
	(2.219)	(0.475)
Observations	90	2,767
R-squared	0.824	0.696
Firm and executive controls	Yes	Yes
Firm Fixed Effects	Yes	Yes
Pre-trend F-test p-value	0.096	0.276

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Cross-Ownership by Restricted Firms as a Transmission Channel

This table examines whether ownership by restricted firms moderates the Act's effect on executive compensation in unrestricted firms. The dependent variable is the natural logarithm of total compensation in 2016 fixed prices. The sample includes CEOs and board chairpersons of unrestricted Israeli firms between 2013 and 2019. Post 2016 is a dummy variable equal to 1 for years after 2016. An executive is classified as Above if their position's compensation exceeded NIS 2.5 million in at least two of the three years during 2014–2016. Column 1 uses the continuous ownership stake held by restricted firms (Res_Own). Column 2 uses a dummy variable equal to 1 if restricted firms hold any ownership stake (D_Res_Own). Column 3 uses a dummy variable equal to 1 if restricted firms hold more than 5% (D_5_Res_Own). All specifications include firm and executive control variables as in Table 3, as well as firm fixed effects. Standard errors are clustered at the firm level. The parallel trends assumption underlying the difference-in-differences identification is validated in Table 3.

Dependent Variable:	Log (Total Compensation)		
	(1)	(2)	(3)
Post 2016	-0.0691	-0.0614	-0.0639
	(0.046)	(0.061)	(0.059)
Res_Hold	0.0112***		
	(0.004)		
Above	1.1068***	1.1829***	1.1719***
	(0.199)	(0.241)	(0.234)
Above x Res_Hold	-0.0124***		
	(0.005)		
Post x Res_Hold	0.0023		
	(0.003)		
Post x Above	-0.2655***	-0.4815**	-0.4812**
	(0.096)	(0.212)	(0.212)
Post x Above x Res_Hold	0.0071		
	(0.005)		
D_Res_Hold		0.0827	
		(0.070)	
Above x D_Res_Hold		-0.2963*	
		(0.174)	
Post x D_Res_Hold		0.0334	
		(0.078)	

Post x Above x D_Res_Hold		0.3597	
		(0.227)	
D_5_Res_Hold			0.0872
			(0.058)
Above x D_5_Res_Hold			-0.2821*
			(0.159)
Post x D_5_Res_Hold			0.0368
			(0.074)
Post x Above x D_5_Res_Hold			0.3549
			(0.225)
Constant			0.0872
			(0.058)
Observations	2,331	2,331	2,331
R-squared	0.744	0.742	0.742
Firm and executive controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
No turnover in 2017-19	Yes	Yes	Yes

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Placebo Test Using Pre-Legislation Years

Placebo test using pre-legislation years: 2013-2014 designated as "pre-period" and 2015-2016 as "post-period" (both before the 2016 legislation). The specification replicates the baseline model from Table 3, Column 4.

Dependent Variable:	Log (Total Compensation)
Post 2014	-0.0476
	(0.072)
Above	0.8791***
	(0.180)
Post x Above	0.0349
	(0.082)
Constant	13.0737***
	(1.012)
Observations	1,402
R-squared	0.759
Firm and executive controls	Yes
Industry Fixed Effects	Yes
Firm Fixed Effects	Yes
No turnover in 2015-16	Yes

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Propensity Score Matching and Entropy Balancing

This table presents robustness tests using matching methods to address selection on observables. All columns restrict the sample to executives who did not experience executive turnover during 2017-2019. Column 1 replicates the baseline specification. Columns 2-3 implement two matching approaches based on pre-period (2014-2016) averages of firm characteristics and executive characteristics. Column 2 uses propensity score matching (PSM) with nearest-neighbor matching (caliper=0.05). Column 3 uses entropy balancing, which reweights observations to achieve exact balance. All specifications include firm and executive control variables as in Table 3, as well as firm fixed effects. Standard errors are clustered at the firm level. The parallel trends assumption is validated in Table 3.

Variable:	Baseline (unmatched)	Propensity Score Matching	Entropy Balancing
	(1)	(2)	(3)
Post 2016 x Above	-0.179**	-0.210	-0.184**
	(0.069)	(0.135)	(0.088)
Observations	2,331	762	2,235
R-squared	0.740	0.802	0.791
Firm and executive controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
No turnover in 2017-19	Yes	Yes	Yes

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Benchmarking Analysis – Peer Compensation versus Regulatory Threshold

This table examines whether the NIS 2.5 million regulatory threshold became a benchmark for CEO compensation decisions in unrestricted firms. The dependent variable is the annual change in log compensation. Relative Comp. to Peer measures the CEO's lagged compensation relative to the peer median, where peers are defined as firms in the same industry and size tercile. Relative Comp. to Threshold measures the CEO's lagged compensation relative to NIS 2.5 million. Post × Relative Comp. to Threshold interacts the threshold measure with a post-2016 indicator. Column 1 spans the entire sample period; Column 2 restricts to 2017-2019 to compare effect magnitudes in the post-period. Column 3 presents a placebo test using 2014-2016 as the sample period, with 2014 designated as the pre-period and 2015-2016 as the pseudo-post period; the absence of a significant Post × Threshold coefficient confirms that the anchoring effect emerged only after the Act. Controls include changes in executive holdings, turnover, appointment percentage, firm assets, and stock returns. All specifications include firm fixed effects and year fixed effects. Errors are clustered within firm.

Dependent Variable:	Δ Log (Total Compensation)		
	All sample period	2017-2019	Placebo 2014-2016
	(1)	(2)	(3)
Relative Comp. to peer in t-1	-0.1802***	-0.2234***	-0.2459***
	(0.063)	(0.044)	(0.090)
Relative Comp. to threshold in t-1	-0.1564	-0.4324***	-0.1535
	(0.159)	(0.100)	(0.200)
Post 2016	-0.0300		
	(0.045)		
Post x Relative Comp. to threshold in t-1	-0.1414**		
	(0.056)		
Post 2013			0.0521
			(0.040)
Post 2013 x Relative Comp. to threshold in t-1			-0.0426
			(0.046)
Constant	0.0195	-0.0050	0.0652
	(0.046)	(0.032)	(0.063)
Observations	1,593	807	1,235
R-squared	0.255	0.318	0.303
Number of Firms	310	309	376
Firm and executive controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Alternative Samples and Specifications

This table presents robustness checks that modify the baseline specification from Column 2 of Table 3. Column 1 restricts the sample to 2015-2018 (two years before and two years after the Act). Column 2 redefines the regulatory threshold as NIS 2.8 million instead of NIS 2.5 million. Column 3 reclassifies executives as "Above" if their compensation exceeded NIS 2.5 million in at least one year during 2014-2016 (rather than requiring two years as in the baseline). Column 4 replicates the baseline specification augmented with industry×year fixed effects to account for potential industry-specific shocks during the sample period. Column 5 estimates the baseline specification using median regression (quantile regression at p=0.5) rather than OLS, to assess whether the findings are driven by extreme observations within the Above group. Note that firm fixed effects are not included in Column 5 as they are not supported by the quantile regression estimator. All specifications present year-by-year interaction terms between each sample year and the Above indicator, with 2016 as the base year. The Pre-trend F-test reports the p-value of a joint significance test of the pre-law interaction coefficients. Columns 1–4 include the same control variables and fixed effects as the baseline model; Column 5 includes the same control variables but without firm fixed effects, as they are not supported by the quantile regression estimator. Standard errors are clustered at the firm level for Columns 1–4; Column 5 reports robust standard errors.

Dependent Variable:	Log (Total Compensation)				
	Sample years changed to 2015-2018	Threshold level changed to 2.8 million ILS	Above criterion changed to 1 year	Adding Industry x Year FE	Median regression
	(1)	(2)	(3)	(4)	(5)
Above	0.909***	0.984***	1.066***	0.910***	0.486***
	(0.151)	(0.162)	(0.175)	(0.138)	(0.047)
2013 x Above		0.026	0.029	-0.071	-0.052
		(0.124)	(0.122)	(0.107)	(0.080)
2014 x Above		0.087	0.121	0.031	0.017
		(0.130)	(0.142)	(0.112)	(0.054)
2015 x Above	0.128	0.101	0.053	0.087	-0.001
	(0.109)	(0.114)	(0.096)	(0.099)	(0.060)
2017 x Above	-0.087*	-0.111**	-0.146***	-0.086*	-0.017
	(0.047)	(0.047)	(0.050)	(0.045)	(0.056)
2018 x Above	-0.268***	-0.281***	-0.306***	-0.280***	-0.100**
	(0.072)	(0.088)	(0.079)	(0.074)	(0.049)
2019 x Above		-0.247**	-0.272***	-0.214**	-0.076
		(0.116)	(0.094)	(0.095)	(0.065)
Constant	11.832***	10.933***	10.804***	12.992***	11.622***
	(0.677)	(0.475)	(0.474)	(0.549)	(0.117)
Observations	1,709	2,857	2,857	2,857	2,857
R-squared	0.776	0.699	0.714	0.713	
Firm and executive controls	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	No
Industry x Year FE	No	No	No	Yes	No
Pre-trend F-test p-value	0.240	0.605	0.606	0.201	0.840

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1