Are CEOs rewarded for luck? Evidence from corporate tax windfalls.*

Martina Andreani
London Business School
Regent's Park
London NW1 4SA, U.K.
mandreani@london.edu

Atif Ellahie
David Eccles School of Business
University of Utah
atif.ellahie@eccles.utah.edu

Lakshmanan Shivakumar
London Business School
Regent's Park
London NW1 4SA, U.K.
LShivakumar@london.edu

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Abstract

We take advantage of a 2017 change in tax rules in the U.S. to re-examine whether CEOs are rewarded for luck. We examine the effect of tax gains and losses associated with deferred tax assets and liabilities on CEO compensation around the Tax Cuts and Jobs Act (TCJA) of 2017. Relative to other years, we find that less visible firms compensated their CEOs more for one-time tax windfall gains during the TCJA-transition period. We also find that the effect is primarily observed in the short-term compensation of CEOs, which suggests that the motivation for this pay was not to align managerial incentives with long-term value creation. Further, we find evidence in support of pay asymmetry; CEOs of less visible firms were compensated more for tax windfall gains but were not compensated less for tax windfall losses. Taken together, the results are consistent with rent-extraction by CEOs of less visible firms.

JEL Classification: G30; H25; J33.

Keywords: Executive compensation; pay for luck; corporate taxes; Tax Cuts and Jobs Act of 2017.

1. Introduction

Do executives get compensated for windfall profits that are unrelated to their efforts? This is a crucial question, as traditional contracting theories suggest that managers would be compensated only for their efforts and not for gains arising out of luck. These theories point out that optimal compensation contracts for chief executive officers (CEOs) are decided directly by shareholders or by their representatives aligned with their interests and that these contracts aim only to mitigate agency problems and incentivize managers to expend the right effort. According to these theories, the only way CEOs would financially benefit from a firm's windfall profits is indirectly through their shareholdings reflecting the gains. But in contrast to these theories, an alternative set of explanations claims that CEOs attempt to extract as much compensation as possible from their firms, constrained only by the availability of funds in the firm and the CEOs desire to avoid adverse media and investor attention (Bertrand and Mullainathan, 2001). If this alternative is valid, CEOs, especially firms with limited investor and external visibility, could be compensated for windfall profits. We test these alternative predictions by focusing on firms' windfall profits from the Tax Cuts and Jobs Act (TCJA) of 2017.[1]

In December 2017, the Trump Administration changed the corporate tax rate significantly, lowering it from 35% to a more globally competitive 21%. While the change in tax rate benefited all firms in the future, it also brought about one-off tax gains (losses) to firms that had deferred tax liabilities (assets) on their balance sheets accumulated from their past transactions. Deferred tax liabilities reflect the tax amount on profits that have been generated and recognized in financial books in the past but for which taxes have not yet become due through the tax laws permitting

[1] We refer to this law as the "Tax Cuts and Jobs Act," following its use in the popular and financial press. While this law was passed by the House under this name, the Senate rules required a name change to "An Act to provide for reconciliation pursuant to titles II and V of the concurrent resolution on the budget for fiscal year 2018."

these profits to be disclosed with future periods' taxable income.[2] The tax rate change effectively reduced the future payments required on such gains and allowed firms to write off their deferred tax liabilities, yielding a tax windfall to the firms and their shareholders. Similarly, the tax rate change lowered the future benefits that accrue to a firm from the deferred tax assets on their balance sheets, which represent taxes that firms have had to pay ahead of their economic recognition in the financial books. These firms had to take a one-off write-off in the value of their deferred tax assets, lowering the reported net income as well as the book value of their deferred tax assets on the balance sheet. Consistent with the tax rate change under TCJA bringing benefits to U.S. stocks, the U.S. stock markets returned 25% between Trump's election in November 2016 and the passage of the TCJA law in December 2017.

Unlike the effect of the ongoing tax benefits to all firms in the future, the one-off tax gains or losses associated with deferred tax assets and liabilities are idiosyncratic to each firm, depending on whether the firm's past transactions had led to an accumulation of deferred tax assets or liabilities. Shareholder-value based theories for compensation suggest that these one-off tax gains and tax losses, which are beyond the control of managers and arise from past activities, should not be considered for executive pay. Thus, examining the impact of TCJA-related windfall tax gains/losses and pay can help identify potential violations of these theories.

The tax rate change under TCJA offers an excellent quasi-natural experiment to test the effect of lucky windfall gains on CEO compensation, as the tax changes were largely unexpected and exogenous to individual firms and their managers' decisions. Although the corporate tax rate reduction was a central part of Trump's election manifesto, it was largely unexpected until

[2] The deferral of taxes to the future on profits recognized in the financial books could arise through tax rules permitting shifting of revenues to the future or by allowing early deduction for expenses (e.g., accelerated depreciation for property, plant, and equipment).

Trump's unlikely victory in 2016, making it even less likely that any firm or its managers could have influenced this tax change or acted in anticipation of this tax change. Further, the tax reform bill was rushed through both the House and the Senate and passed into law in less than three months, without a single Democrat supporting it. The short timeframe, the contentious nature of the bill and its steadily changing provisions made it difficult for firms to predict the final form of this reform law and adjust their behavior accordingly.

Although the TCJA was passed in December 2017 and Generally Accepted Accounting Principles (GAAP) require firms to recognize the transition-year tax effects in that same quarter, the SEC allowed companies additional time to recognize these tax effects on their financial books due to the complicated nature of the tax reforms. Moreover, the SEC also allowed firms an additional year to adjust initial estimates made by the firms for the transition-year tax effects. Thus, even if a firm recognized a provisional amount as the effects of the tax reforms in the same quarter as the passage of TCJA (i.e., the last quarter of 2017), the full effect on these financial statements may not be reflected in the firm's financial statements for several months after that. As many firms would have needed at least three or so months to gather all the relevant data and accurately compute the tax effects, we expect firms to have been able to fully recognize these tax effects in their fiscal years ending between December 2017 and March 2019. Accordingly, we focus on these fiscal years (hereafter, TCJA-transition period) as the event period of interest. Consistent with this, Figure 1 shows that the effects of TCJA were clearly noticeable for firms during this period. In any case, our conclusions remain unaffected if we marginally shift the months covered by the TCJA-transition period.

Since the tax effects reported in a year could be endogenous to managerial actions and the broader economic performance during that year, we use the level of deferred tax assets/liabilities

at the beginning of each year as an instrument for the one-time tax gain or loss arising from the TCJA-driven changes to deferred tax assets and liabilities (hereafter, 'TCJA tax effects'). The lowering of tax rates had an immediate effect of decreasing firms' deferred tax liabilities to the U.S. government, causing the firms to enjoy a one-off tax benefit and jump in earnings. At the same time, the tax-rate change also decreased the U.S. deferred tax assets, leaving firms with a one-off tax expense and reduced earnings. As these one-off tax benefits/expenses were, to a large degree, determined by the deferred tax liabilities (assets) at the beginning of the period (e.g., Donelson, Koutney and Mills, 2021), the deferred tax assets/liabilities at the beginning of the year could be good proxies for the TCJA tax effects. At the same time, these beginning-of-year values are unrelated to managerial activities and the broader economic performance during the year and so cannot influence executive pay through channels other than the TCJA tax effects.

Employing a difference-in-difference approach, we investigate whether the component of CEO pay linked to the beginning-of-year deferred tax assets/liabilities is different in the TCJA-transition year relative to the other years. We find that, while CEO compensation is significantly positively associated with the beginning-of-year deferred tax assets/liabilities in the TCJA-transition year, there is little evidence of such a relationship in other years. This positive link in the TCJA-transition year is mainly observed for firms with poor investor visibility. In contrast to our findings for deferred tax liabilities, we find no significant relationship between CEO pay and beginning-of-year deferred tax assets for any of the other years. These results indicate that the CEOs of low-visibility firms get compensated for the TCJA tax benefits associated with deferred tax liabilities, but their pay is not commensurately decreased to reflect TCJA-related tax losses arising from the reduction in the value of the deferred tax assets.

Since the fiscal years affected by TCJA-transition stagger across calendar years, i.e., 2017 to 2019, we are able to provide an even cleaner identification of firms' tax windfall effects on executive compensation. The staggered nature of the TCJA-transition allows us to compare the executive compensation paid during the same calendar year across firms whose financial books for that calendar year differ in their recognition of TCJA tax effects. So, for 2017, we can compare the executive compensation paid by firms whose fiscal years end before December (implying their financial statement figures cannot reflect the TCJA tax effects) with firms whose fiscal years end in the last quarter of that year (implying their financial books could reflect the TCJA tax effects). Similarly, for 2019, firms whose fiscal years end in the first part of that year are more likely to have their financial books reflect the TCJA tax effects than firms whose fiscal years end in the remaining quarters of that year. Even holding the calendar year constant, we find that CEOs of firms are paid significantly more in the fiscal year in which their firms reflect the TCJA tax effects. Again, these results hold primarily among firms with poor investor visibility.

Although the share price run-up associated with the passage of TCJA would have benefited managers through their bonus compensation being linked to their firms' share performance, our findings do not reflect these share-price-induced changes to executive pay.[3] This is because all our empirical analyses control for share price performance, implying that our tests capture only the pay impact of TCJA over and above those normally associated with share price changes. Additionally, the share price run-up would also have directly increased the wealth of the executives with equity stakes and stock options in their companies. However, as our focus is only on executive

[3] The precise relation between a firm's share price performance and its executives' bonuses depends on a variety of factors, including the compensation committee's use of relative performance evaluation (RPE) and the choice of peer firms in the RPE. But, since the effect of the TCJA on a firm's deferred tax assets and liabilities is determined mainly by factors specific to a firm, compensation committees have significant discretion on adjustments relating to deferred tax gains/losses disclosed by peer firms.

pay rewarded each year rather than on changes in executives' wealth, these benefits would not be captured by our tests. Due to these, our finding on CEO rewards for tax windfall gains can be seen as a lower limit on the CEO's benefits from TCJA-induced gains or losses associated with firms' deferred tax assets/liabilities.

Our study builds on an influential study by Bertrand and Mullainathan (2001), who provide the first archival evidence supportive of the rent-extraction view. They document that weakly-governed CEOs get paid for luck, i.e., for observable firm performance beyond their control. More specifically, they find that CEO compensation in poorly governed firms is as sensitive to industry-wide performance metrics, their proxy for performance related to luck, as it is to more general metrics and conclude that CEOs must effectively be setting their own pay to maximize their personal gain, subject to the availability of funds in the firm and their need to avoid undue media coverage and shareholders' outrage.

However, recent studies counter the conclusions of Bertrand and Mullainathan (2001) by pointing out that their evidence can be explained under optimal contracting models. These studies argue that payments for industry performance are needed to retain executive talent or appropriately incentivize CEOs to take account of industry and economy-wide factors in their decisions (e.g., Oyer, 2004; Bizjak, Lemmon and Naveen, 2008; Brookman and Thistle, 2013; Gopalan, Milbourn and Song, 2010). These theories effectively point out that compensation for industry and economy-wide performance is not "pay for luck". However, none of these alternative models of shareholder value maximization predicts CEOs pay to reflect one-off windfall gains, especially if the windfalls are related to the firm's past transactions and are beyond a CEOs control. Thus, our findings provide strong support for the rent-extraction view of executive pay for firms with low visibility.

Our paper also contributes to the call for more rigorous empirical evidence on the rent-extraction view by Edmans, Gabaix and Jenter (2017, p 386), who observe that:

> *Recent theoretical contributions make clear that shareholder value models can be consistent with a wide range of observed compensation patterns and practices, including the large increase in executive pay since the 1970s. The challenge is now to confront these new models more rigorously with the data, explore their limitations, and contrast them with (mostly yet-to-be-written) rent extraction models.*

Although our study cannot speak to how executive pay responds to other types of lucky windfall gains and losses, at the very least, our findings raise the bar for theoretical models of executive compensation that rely on shareholder value-maximization arguments. These models will also need to explain why paying low-visibility firms' CEOs for tax windfall gains is optimal or in line with shareholders' interests.

Our study also contributes to the debate on the asymmetry in pay for gains and losses. Garvey and Milbourn (2006) argue that if executives can indeed influence decisions on their pay, as implied by the rent-extraction view, they will be rewarded for good luck but insulated from bad luck. They provide evidence consistent with this pay-for-luck asymmetry. However, Daniel, Li, Naveen (2020) point out that the evidence on asymmetric pay for luck is highly sensitive to methodological choices and, based on additional analyses, conclude that there is no evidence to support asymmetry in pay for luck. The findings in Daniel et al. (2020) question a fundamental premise of the rent-extraction review. But, both Daniel et al. (2020) and Garvey and Milbourn (2006) rely on the market or industry-wide returns to proxy for 'luck', and these are likely to be correlated with executives' outside opportunities. These studies also use a two-stage methodology, raising the possibility of errors-in-variables concerns. By comparing executive pay response to TCJA effects on deferred tax assets with those on deferred tax liabilities, we are able to provide cleaner evidence on the asymmetric treatment of pay for luck.

Prior studies (e.g., Gaver and Gaver, 1998; Potepa, 2020; Jang, Urcan and Yoon, 2020) have shed light on the role of nonrecurring items in determining executive pay. But, many nonrecurring items are under the control of a manager and are a direct consequence of the manager's effort (e.g., restructuring charges, M&A charges, gains/losses from discontinued operations and asset sales). Therefore, these studies cannot address the issue of interest here, namely, whether CEOs get paid for lucky windfall gains.

The remainder of the paper is structured as follows. The following section discusses the institutional details relating to the Tax Cuts and Jobs Act. This is followed by a literature review and our empirical predictions in Section 3. Sections 4 and 5 discuss the study's research design and empirical results. We conclude in Section 6.

2. Institutional setting: The Tax Cuts and Jobs Act of 2017

On December 22, 2017, the then President of the United States, Donald Trump, signed and enacted a comprehensive tax law, commonly referred to as the Tax Cut and Jobs Act of 2017. Although legislators had been working on corporate tax reforms for over a decade, the TCJA was passed into law in barely three months from the release of the 'Unified Framework for Comprehensive Tax Reform' on September 27, 2017, to a final bill being signed by President Trump on December 22, 2017. The legislative text of the bill exceeded 400 pages, and the bill itself was highly contentious, with no Democrat (both in the Congress and the Senate) voting in its favor.[4]

The TCJA brought about the most extensive reform of the U.S. corporate tax code since 1986 and hugely affected the corporate earnings of some firms. Its reforms included the elimination of the domestic production activity deduction and the introduction of new limitations

[4] The TCJA bill covers individual and corporate taxes, but our focus is only on the latter.

on certain business deductions. But its most significant changes were a lowering of the U.S. federal corporate income tax rate from 35% to 21% and replacing the U.S. federal income taxes on dividends from foreign subsidiaries with a transition tax on the deemed repatriation of foreign subsidiaries' earnings.[5]

The reduction of corporate tax rates under TCJA required all firms to remeasure their deferred tax assets and liabilities relating to U.S. taxes and appropriately write down their values in the transition year. Additionally, for firms with foreign subsidiaries, the TCJA introduced a deemed repatriation of currently deferred foreign profits at a rate of 15.5 per cent for cash and cash-equivalent profits and 8 per cent for reinvested foreign earnings. As this deemed repatriation tax additionally affects the reported tax expense for multinational firms, our empirical analysis checks for the sensitivity of our conclusions to focusing only on domestic firms.

TCJA had a substantial and immediate impact on corporate earnings, with this impact varying vastly across firms. For instance, Berkshire Hathaway reported a net tax benefit of $29 billion for the 2017 fiscal year, almost all of which "*related to a one-time non-cash reduction of our net deferred income tax liabilities that arose from the reduction in the statutory U.S. corporate income tax rate from 35% to 21%*" (Berkshire Hathaway, 2017 Annual Report). Compared to this transition-year tax benefit, Berkshire Hathaway's reported profit after tax for the previous year was $24 billion. In contrast, another financial services firm, Citigroup, reported a one-off tax charge of $22.6 billion in 2017 on account of TCJA, with a majority of this charge arising from the remeasurement of its deferred tax assets (net of deferred tax liabilities). These examples highlight that, even within similar businesses, TCJA affected firms vastly differently.

[5] Donelson et al. (2021) evaluate non-recurring income taxes and report that a large part of the variation in the TCJA's one-time impact on earnings can be explained by the remeasurement of prior quarter's deferred tax assets, deferred tax liabilities, and prior-year permanently reinvested earnings. Due to the tax reforms affecting foreign earnings, our empirical analysis also checks the sensitivity of our results to limiting the analysis to only domestic firms.

Koutney and Mills (2018) and Wagner, Zeckhauser and Ziegler (2020) also confirm the hugely heterogeneous effects of TCJA through a systematic analysis of the immediate impact of TCJA on S&P 500 firms. They document a roughly equal proportion of firms reporting increases to net earnings as those reporting decreases. Further, consistent with the TCJA having a significant impact on firms' reported taxes and earnings, they find that the aggregate earnings of firms with a positive TCJA effect increased from $79 billion to $315 billion. In contrast, TCJA reduced earnings by $231 billion for the other firms in the sample, causing their aggregate earnings to fall from $160 billion in profits to a net loss of $71 billion. Dyreng, Gaertner, Hoopes and Vernon (2020) further document that U.S. domestic firms tended to gain more from the TCJA tax reforms than U.S. multinational corporations. These clearly indicate that the passage of TCJA would have significant effects on executive pay unless its tax effects are specifically excluded from the pay computations.

The TCJA had a significant recurring as well as a nonrecurring impact on firms' financial statements. On the recurring component, Wagner et al. (2020) report that the effective tax rates for the median firm decreased from 31.7% in the year before the reform to 20.8% in the year after the reform. Regarding the nonrecurring tax effects, they find that a fifth of the firms had nonrecurring tax benefits or expenses that were over 3% of the total assets. Throughout, our focus is exclusively on the nonrecurring tax effects of TCJA and that too on those effects arising from the remeasurement of deferred tax assets/liabilities.

Although the provisions of TCJA were mainly effective for tax years beginning on or after January 1, 2018, generally accepted accounting principles (GAAP) requires firms to recognize the effects of the tax law change in the same quarter as the passage of the law.[6] This

[6] Statement of Financial Accounting Standards 109 and Accounting Standards Codification Topic 740.

means that firms with a fiscal year ending on December 31, 2017, had minimal time to gather all necessary information (including, where relevant, from subsidiaries across the world), analyze the data in reasonable detail to identify the tax effects and recognize the appropriate amounts in the financial books. Recognizing this difficulty, as well as the fact that tax authorities would subsequently issue clarifications and guidance, the Securities Exchange Commission (SEC) issued Staff Accounting Bulletin No. 118 (SAB 118), allowing firms additional time to recognize the TCJA effects.[7]

Based on the SAB 118 allowance, many firms deferred the recognition of the TCJA tax effects to fiscal quarters in 2018. Figure 1 plots the changes in deferred tax assets and tax liabilities over the period 2017 to 2019. For the typical firm, the deferred tax liabilities declined significantly for fiscal years ending after the 4th quarter of 2017 to the 1st quarter of 2019.

Recent studies have attempted to evaluate the direct effects of TCJA reforms on CEO compensation, as TCJA also changed the rules on tax-deductibility of payments made to senior managers. Before TCJA, tax rules restricted the tax deductibility for each executive's pay to $1 million but waived this ceiling for executive pay linked to performance. TCJA removed this exception and thereby increased the after-tax cost of executive compensation. Examining the effect of this change on executive pay, De Simone, McClure and Stomberg (2021) and Luna, Schuchard and Stanley (2020) conclude that the TCJA did not significantly affect the senior managers' pay levels and conclude that tax deductibility of compensation is not a first-order determinant of executive pay. In contrast, Durrant, Gong and Howard (2021) argue that U.S. firms anticipated lower tax deductibility under TCJA and, accordingly, increased CEO compensation before the

[7] SAB 118 allowed firms to make a provisional recognition of the tax effects of TCJA in the first reporting period in which the company had been able to determine this estimate reasonably and also allowed the provisional figures to be revised over a "measurement period", which could not exceed one year.

TCJA was passed. Unlike these studies, we do not evaluate recurring tax effects on CEO pay but instead, focus on the compensation effects of one-off tax gains and losses under TCJA and how these effects vary with firms' deferred tax assets and liabilities. This difference in focus is crucial as the results in the other studies cannot speak on the validity or otherwise of the rent-extraction view.

3. Literature Review and Empirical Predictions

Market participants and the media have been raising concerns about executive pay packages for almost three decades.[8] These concerns have led to a long-running debate about whether executive compensation contracts are optimal or whether executives extract rents in the form of excessive compensation.

Agency-theoretic models, focusing on shareholder value maximization, suggest that managers should be ideally compensated based on their actions rather than on the outcomes of their actions. However, as managerial actions are unobservable, these studies conclude that optimal incentive contracts based on outcome metrics should also include all other pieces of information that help distinguish random fluctuations in output from the agent's level of effort (Holmstrom, 1979; Diamond and Verrecchia, 1982). These conclusions rely on the premise that a manager's effort can be inferred from overall firm performance once the observable components of

[8] Executive pay structures have also been cited as a major cause of the 2008 financial crisis. An oft-quoted example of compensation-driven excesses is that of Angelo Mozilo, CEO of Countrywide Financial Corporation, who received a compensation package of $69 million in 2006 (www.forbes.com). However, in the following year the firm was forced to create substantial loan-loss provisions on its sub-prime mortgages and was subsequently sold to the Bank of America for less than 20% of its peak value just a year earlier.
Also, on the passage of the Corporate and Financial Institution Compensation Fairness Act of 2009 (H.R. 3269) bill that mandated a non-binding shareholder vote to approve executive compensation, the US House Committee on Financial Services (Democrats) called for prohibiting "…any incentive-based pay arrangement, or any feature thereof, that encourage undue risk-taking." Furthermore, this report emphasized the need to prevent a return to a 'heads you win, tails you break even' compensation system that contributed to the 2008 financial collapse. (see full text at https://www.congress.gov/111/crpt/hrpt236/CRPT-111hrpt236.pdf).

performance that are beyond a manager's control, such as the state of the macroeconomy or the fluctuations in the input or output prices, are filtered out. Therefore, optimal contracting theories predict that CEOs would be paid relative to a benchmark that removes the effect of market or sector performance on the firm's own performance.

In contrast to the above predictions, Bertrand and Mullainathan (2001) find that CEO pay is positively correlated with industry performance, especially in firms with weak governance. This is surprising as optimal contracting models indicate that tying a manager's pay to industry performance or other performance factors beyond a CEO's control, i.e., "luck", is not beneficial to shareholders. Managers cannot influence these lucky outcomes, so paying for luck is inconsistent with shareholders' value-maximizing objective. In contrast, tying CEO pay to luck only increases the risks to managers. Due to the inability of the optimal contracting model to explain pay for luck, Bertrand and Mullainathan (2000, 2001) propose an alternative skimming view of executive compensation, under which CEOs have effective control of the pay-setting process and choose their pay level to maximize their personal gains, subject only to the availability of funds and their need to avoid adverse media and investor attention. In this world, good overall firm performance helps loosen the pay constraints and helps CEOs' large pay packages to go unnoticed, causing the CEO pay to be related to both luck and non-luck components of firm performance.

Extending the skimming view arguments of Bertrand and Mullainathan (2000, 2001), Bebchuk and Fried (2004) observe that internal and external governance mechanisms are often too weak to ensure an arm's length pay-setting process. They assert that powerful CEOs can maintain control over their board by selecting and retaining board members that are passive or compliant. As a result, the board or committee members could become unable or unwilling to fulfil their

fiduciary duties to the shareholders, resulting in a suboptimal contract with the managers. Thus, by exerting influence over the board of directors or the compensation committee members, the CEOs can extract rents in the form of excessive compensation.[9] Empirical evidence supportive of these arguments has been presented by Chhaochharia and Grinstein (2009), who document that CEO compensation decreases following regulatory changes that strengthen board oversight, and by Bebchuk, Grinstein and Peyer (2010), who show that opportunistic backdating of option grant occurs at firms with poor corporate governance.

Building on the above theories, Garvey and Milbourn (2006) point out that if executives can truly influence their own pay, they will seek to benchmark their performance only when it is in their interest to do so. That is, managers would index their performance to a benchmark only when the benchmark is down, as this would make the benchmark-adjusted performance" and consequently, their compensation to be higher. This asymmetric benchmarking for pay implies that executive pay should be less related to benchmark returns when benchmarks are down than when they are up. Consistent with this prediction, they document that CEO pay is more positively related to industry or market returns, their proxies for "luck" when these benchmark returns are positive than when they are not.

However, Daniel et al. (2020) caution against relying on the pay-asymmetry evidence in Garvey and Milbourn (2006), claiming that the previous evidence is not robust to alternative specifications and methodological choices. Trying out 205 different specifications, they find evidence supporting the pay-asymmetry exists in fewer than 2% of the cases. They also show that this lack of pay-asymmetry is observed in a wide variety of sub-samples, including poorly governed firms. Their findings strike at the heart of the rent-extraction view by raising a

[9] Kuhnen and Zwiebel (2009) analytically show that inefficient compensation can be sustained in equilibrium because a new CEO would also extract rents and because firing a CEO is costly.

fundamental question about this view: If CEOs truly control their pay-setting process, why do they treat good and unfavorable luck symmetrically for determining their pay? More specifically, why do powerful CEOs willingly accept lower pay due to adverse exogenous shocks to performance?[10]

Further doubts on the validity of the rent-extraction view have been cast by a wide variety of theories that have been presented to explain the evidence in Bertrand and Mullainathan (2001). Although Bertrand and Mullainathan (2001) interpret their evidence of a positive relationship between CEO pay and industry returns as 'pay for luck', many theories now explain this evidence within the context of optimal contracting models. For instance, many scholars argue that 'pay for industry performance' is needed to retain executive talent (Himmelberg and Hubbard, 2000; Oyer, 2004; Bizjak et al., 2008; Brookman and Thistle, 2013). According to their arguments, when skilled managers are in short supply and executives' outside opportunities are correlated with general market conditions, firms will need to compensate their managers for changes in their external opportunities.

Alternatively, Gopalan et al. (2010) present a model where CEOs need to make strategic decisions on their firm's exposure to sector performance. In their model, optimal compensation contracts require CEOs to be compensated for their sector performance in order to incentivize the CEOs to expend effort on gathering informative signals about future sector returns and to take appropriate action on the firm's exposure to sector performance.

Further, Hoffmann and Pfeil (2010) and DeMarzo, Fishman, He and Wang (2012) present a model in which shocks to a "luck" factor signal higher future profitability for the firm and

[10] Chance, Kumar and Todd (2000) document an asymmetric compensation decision in the context of executive stock option repricing. They show that exercise prices in the stock options are revised downward after firm-specific stock price declines, but never upward following price increases. Carter and Lynch (2001), Chidamabaran and Prabhala (2003) and Chen (2004) clarify that these repricings do not reflect agency problems or ineffective corporate governance, as they are more likely to occur in well governed firms.

increase the costs of early termination of the firm's projects. So, to minimize the agency costs of early termination, an optimal contract requires the manager to be compensated for the returns from the "luck" factors. Along similar lines, Feriozzi (2011) contends that payments for good market-wide performance are needed to appropriately incentivize managers, as good news about future economic performance weakens managers' implicit disciplinary mechanisms, such as potential bankruptcy.

However, none of the agency-centric models predicts that CEOs should be rewarded for one-off windfalls, especially if these gains are related to a firm's past transactions and are beyond the CEOs control. As Hoffman and Pfeil (2010) explicitly observe,

> *"if luck shocks are uncorrelated with future productive opportunities and, hence, do not affect prospective agency costs, then it is indeed suboptimal to make the agent's payment contingent on these exogenous shocks."*

Thus, empirically evaluating the link between CEO compensation and the effects of TCJA on a firm's deferred tax assets/liabilities could help shed light on the validity of the rent-extraction view.

The above discussions illuminate three key patterns needed for executive pay to be uniquely consistent with the rent extraction view. Firstly, the pay for luck requires managers' pay to be unrelated to firm-specific and broader economic performance. Secondly, the pay for luck should be observed only in firms with poor managerial monitoring or scrutiny. Finally, the pay for luck should be asymmetric, implying that managers are rewarded for favorable luck but not penalized for negative luck.

We test the above predictions by focusing on the one-off tax losses and windfalls arising from TCJA-induced changes to the value of firms' deferred tax assets and liabilities, respectively. As the changes in the value of these assets and liabilities are (i) exogenous to managerial actions, (ii) relate to past activities of the firm, (iii) unrelated to external opportunities in the CEO-labor

market, and (iv) distinct from future investment opportunities and profitability, evidence of executive pay for these one-off tax windfalls, but not losses, in firms with poor managerial monitoring or scrutiny would be inconsistent with optimal contracting models and instead, would support the rent extraction view.

4. Research Design

To examine whether CEOs of less visible firms are rewarded for luck, we use a quasi-natural experiment presented by the Tax Cuts and Jobs Act (TCJA) of 2017. We employ a difference-in-difference research design to examine the association between CEO compensation levels and corporate tax windfalls attributable to the TCJA and assess whether this association varies with the degree of firms' external and investor visibility. We focus our discussions in this section on the main regression specification and present the detailed definitions of the variables in Appendix A.

The passage of TCJA towards the end of December 2017 and the complicated nature of the TCJA tax reforms imply that corporations with fiscal years ending soon after its enactment (i.e., firms with 31st December year-end) are unlikely to have enough time to gather the needed data, estimate the TCJA-transition effects and reflect these in their 2017 financial statements. Recognizing this difficulty and the fact that tax authorities are likely to issue further clarifications and guidance, the SEC also allowed companies time beyond the TCJA-enacted quarter to reflect the potentially complicated tax effects of TCJA. So, we expect some of these firms, especially the smaller and less-followed ones for whom demand for timely recognition of gains and losses is lower (see Ball and Shivakumar, 2005), to delay recognition of the TCJA-transition effects to the following fiscal year. Moreover, even if some of these firms recognized provisional estimates in their 2017 fiscal year reports, they are likely to incorporate adjustments to their estimates in the next fiscal year. Thus, for example, if a firm has March fiscal year-end, the firm could provide a provisional estimate for TCJA-transition effects in the year ending March 31 2018, with

adjustments to these estimates recognized in the fiscal year-ending March 31, 2019, or alternatively, recognize the full transition effects in the year ending March 31 2019.

To capture all relevant fiscal years reflecting the TCJA-transition effects, we define our event period of interest, i.e., TCJA-transition period, to be between December 31, 2017, and March 31, 2019 (both inclusive). This definition effectively implies that firms whose financial statements end between December 2017 and March 2018 could have recognized their TCJA-transition effects either in the fiscal year ending immediately after the TCJA enactment or in the subsequent year. For all other firms, the expectation is that they would have recognized the transitionary tax effects in the fiscal year that includes the TCJA enactment date. Accordingly, in our empirical model, we include an indicator variable for the TCJA-transition period, *Tax Shock*, set equal to 1 for fiscal years ending between December 31, 2017, and March 31, 2019, and 0 otherwise.[11]

We estimate the following pooled regression for CEO compensation in fiscal year *t* using yearly observations for firm *i* (*i* subscripts suppressed) from January 2013 to December 2019.

$$
\begin{aligned}
&Total\ Comp_t \qquad (1)\\
&= \beta_0 + \beta_1 Tax\ Shock_t + \beta_2 DTA_{t-1} + \beta_3 Tax\ Shock_t \times DTA_{t-1} + \beta_4 DTL_{t-1}\\
&+ \beta_5 Tax\ Shock_t \times DTL_{t-1} + \beta_6 Visibility_{t-1} + \beta_7 Tax\ Shock_t \times Visibility_{t-1}\\
&+ \beta_8 DTA_{t-1} \times Visibility_{t-1} + \beta_9 Tax\ Shock_t \times DTA_{t-1} \times Visibility_{t-1}\\
&+ \beta_{10} DTL_{t-1} \times Visibility_{t-1} + \beta_{11} Tax\ Shock_t \times DTL_{t-1} \times Visibility_{t-1}\\
&+ \gamma' X_{t-1} + \theta' \lambda_j + \pi' \tau_t + \phi' \tau_t \times DTA_{t-1} + \psi' \tau_t \times DTL_{t-1} + \varepsilon_t.
\end{aligned}
$$

Our primary dependent variable is the CEO total compensation (*Total Comp*) in a given year. However, if compensation for tax windfalls is awarded to create appropriate managerial incentives, we expect to observe these effects mainly in the longer-term pay of executives.[12] Thus,

[11] In additional tests, we also assess the sensitivity of our results to this choice of date by setting *Tax Shock* equal to 1 for fiscal years ending between March 31, 2018, and March 31, 2019, and 0 otherwise.

[12] Consistent with this expectation, Edmans et al., 2017, p 386 observe that "*Attempts to improve CEO pay should focus on the incentives created, and especially on the sensitivity of CEO wealth to long-term performance. The level of pay receives the most criticism, but usually amounts to only a small fraction of firm value. Badly structured incentives, on the other hand, can easily cause value losses that are orders of magnitudes larger.*"

in later tests, we also test for this prediction by repeating our analysis after splitting CEO total compensation into short-term and long-term compensation. We use the sum of salary, bonus, and other compensation to measure short-term compensation (*S.T. Comp*) and the sum of stock awards, option awards, non-equity incentive plans, changes in pension plans, and long-term incentive plans to measure long-term compensation (*L.T. Comp*).

To capture the tax losses and gains associated with changes in the deferred tax assets and liabilities in each year, we include the beginning-of-year deferred tax assets (*DTA*) and deferred tax liabilities (*DTL*), respectively. We scale the deferred tax amounts by the beginning-of-year market value of equity and include the main effects of *DTA* and *DTL* in our model. As described earlier, the use of beginning-of-year *DTA* and *DTL* mitigates potential endogeneity concerns that may result from directly considering the tax gains or losses in the income statement. To capture the compensation effects of one-off tax benefits and expenses arising from the impact of TCJA on deferred taxes, we interact *DTA* and *DTL* with the *Tax Shock* indicator variable.

The rent extraction view, as espoused by Bertrand and Mullainathan (2001), predicts that CEOs would maximize their pay subject to the constraint of negative media and shareholder attention. While Bertrand and Mullainathan (2001) evaluate this view by focusing on proxies for internal governance based on the board and ownership structure of the firm, we instead focus our analysis on investor and media visibility. This is motivated by two important changes to compensation-related governance that have occurred since the publication of Bertrand and Mullainathan (2001). First, in the wake of Enron's collapse and a series of financial scandals in the early 2000s, the Securities and Exchange Commission (SEC) enacted a series of rules to improve governance of listed firms (see Chhaochharia and Grinstein (2009) and Armstrong, Core and Guay (2014)). These changes, effective from 2004, resulted in almost all U.S. listed corporations having a majority (> 50%) of independent directors on their boards and with their compensation sub-committees consisting entirely of independent directors. These regulatory changes have substantially reduced cross-sectional variations in these internal governance metrics,

lowering their usefulness to differentiate between well-governed and poorly-governed firms. Consistent with these, several studies have raised concerns with the reliability and construct validity of governance measures derived from board composition and ownership structure (e.g., Larcker, Richardson and Tuna, 2007; Bebchuk, Cohen and Ferrell, 2009; Daines, Gow and Larcker, 2010; Bhagat, Bolton and Romano, 2008).

Second, since 2011, the SEC has required U.S. listed companies to conduct a separate non-binding shareholder advisory vote (popularly known as the "Say on Pay" or SoP vote) at least once every three years and to publicly disclose the vote's results within four business days. Moreover, the SoP voting outcomes have significant reputational and litigation implications for firms. Adverse SoP voting outcomes can lead a firm's management and its board to potentially face political costs, negative public opinion, media backlash, shareholder pressure or labor market effects (Brunarski, Campbell, Harman and Thompson, 2016; Murphy and Jensen, 2018). Katz and McIntosh (2013) also point out that negative SoP voting engenders nuisance litigation from aggressive plaintiffs' lawyers. Therefore, attention from a more diverse set of market participants, stakeholders, and media outlets has become more important to monitor CEO compensation. We capture this broader monitoring of CEO compensation by focusing on investor and external visibility of firms. We expect, in line with the arguments of Bertrand and Mullainathan (2001), that firms with greater visibility are less likely to engage in rent extraction.

We assess the moderating role of the degree of firm visibility on CEO compensation using a firm-specific time-varying measure of visibility that summarizes five individual proxies drawn from prior literature (e.g., Bushee and Miller, 2012; Baker, Powell and Weaver, 1999). Larger firms and those with greater stock liquidity are commonly viewed as being more visible as they tend to be held by a broader set of investors. Hence, two of our proxies for firm visibility are the firm's beginning-of-year market value of equity (*Market Value*) and beginning-of-year stock liquidity. As there is no consensus on the most appropriate liquidity metric, we use three alternative measures for stock liquidity: (i) the inverse of the Amihud (2002) measure (*Liquidity*); (ii) the

dollar volume of stock trading (*Trading Volume*); and (iii) the proportion of trading days over the prior year with non-zero returns (*Non-Zero Return Days*). Following Bushee and Miller (2012), our final proxy is the level of attention from the sell-side research analyst community, measured as the beginning-of-year number of equity research analysts covering the firm in I/B/E/S (*Analyst Coverage*). We summarize these five proxies into a single variable for firm visibility (*Visibility*) by extracting the first (and only) principal component with an eigenvalue above 1.0, which explains 78% of the variation in the five individual proxies. Our regression model includes *Visibility* by itself as well as by interacting with each of the deferred tax variables and with *Tax Shock*. The interactive variables are intended to capture how the relationship between TCJA-tax effects and compensation vary cross-sectionally based on firms' visibilities.

The vector *X* represents time-varying control variables for lagged firm-level and CEO-level economic determinants of executive compensation identified by prior literature (e.g., Cadman, Klasa and Matsunaga, 2010; Conyon, Core and Guay, 2011; Ellahie, Tahoun and Tuna, 2017). We include logarithm of total assets (*Size*), operating return on assets (*Profitability*), size- and industry-adjusted returns over the previous twelve months (*Past Returns*), a proxy for growth opportunities (*Book-to-Price*), idiosyncratic risk (*Volatility*), and financial risk proxied by *Leverage*. We also include the logarithms of CEO age (*CEO Age*) and CEO tenure (*CEO Tenure*) as controls for a CEO's experience and length of service, respectively. To control for time-invariant firm effects and common time effects, the regressions include firm fixed effects (λ_j) and calendar year fixed effects (τ_t). Finally, to account for potential serial correlations in compensation, we cluster standard errors at the firm level.

Our main variables of interest are the two double interaction terms (*Tax Shock* × *DTA* and *Tax Shock* × *DTL*) and the two triple interaction terms (*Tax Shock* × *DTA* × *Visibility* and *Tax Shock* × *DTL* × *Visibility*). The double interaction terms help us assess whether firms with low visibility compensate their CEOs for corporate tax windfalls in the TCJA-transition period more

than in other years. In contrast, the triple interaction terms enable us to infer whether this relationship changes for more visible firms.

5. Empirical Results

5.1. Sample and Descriptive Statistics

We employ publicly available data sources to construct our sample. We collect CEO compensation data for U.S. firms from ExecuComp and Capital I.Q.'s People Intelligence database and combine the data from these two sources to maximize the coverage universe of firms in our sample.[13] We collect data on various components of CEO compensation (i.e., salary, bonus, stock awards, option awards, non-equity incentive plans, change in pension plans, long-term incentive plans, other compensation). We also collect data on CEO age and employment starting and ending dates to calculate the tenure of an executive as CEO. We collect annual data on firm fundamentals from Compustat North America, monthly and daily price data from CRSP, and monthly analyst data from Institutional Brokers' Estimate System (I/B/E/S).

Panel A of Table 1 describes the sample construction procedure. Our sample starts with all available firm-year observations at the intersection of CRSP, Compustat North America, Execucomp, and Capital I.Q. over January 2013 to December 2019. We select January 2013 as the beginning of our sample period to ensure that we have at least five years of data to estimate a model for CEO compensation before the TCJA-transition period (i.e., fiscal years ending between December 31, 2017 and March 31, 2019). We select December 2019 as the end of our sample period to ensure that we have sufficient post-TCJA data while also avoiding any confounding effects related to the COVID-19 pandemic in early 2020.

[13] We use overlapping firm-year observations to match compensation variable codes across the two datasets.

We retain 17,323 observations with available data for CEO compensation, firm-level control variables, and CEO-level control variables. We remove 4,862 observations appearing in the three years around CEO turnover events (i.e., the year before, the year of, and the year after a CEO turnover). We exclude these CEO turnover years to reduce the influence of personnel changes on compensation and ensure that we examine the same CEO around the TCJA-transition period. Finally, for each firm, we require at least one observation before the TCJA-transition period and one during the TCJA-transition period. This requirement reduces the sample by 3,236 observations. Our final sample consists of 9,225 observations and covers 2,081 unique U.S. firms.[14]

Panel B of Table 1 summarizes the composition of the sample by calendar year. We observe that the sample increases after 2016, which is due to a larger number of firms being covered by Capital I.Q.'s People Intelligence in recent years. Panel C of Table 1 summarizes the composition of the sample by industry based on the Fama and French 17 industry classification. The three largest industries represented in our sample are services and other (25.6%), finance (18.9%), and machinery (10.4%).

Table 2 reports descriptive statistics for the variables used in our analyses (Appendix A provides detailed variable definitions). We winsorize the top and bottom 1% of all continuous variables to reduce the influence of outliers due to data errors. Our main dependent variable of interest is the CEO total compensation (*Total Comp*) which is based on the total compensation data reported by the firm as collected by Execucomp or Capital I.Q. When values for *Total Comp* are missing, we calculate it using data on the individual components of compensation available in these

[14] In additional tests, we assess the sensitivity of our results to our sample selection procedures by (1) starting our sample in January 2016 instead of January 2013, and (2) retaining the three-year period around CEO turnover events.

databases. Our secondary dependent variables of interest are the short-term compensation and long-term compensation of the CEO. Short-term compensation (*S.T. Comp*) is equal to the sum of salary, bonus, and other cash compensation, whereas the long-term compensation (*L.T. Comp*) is equal to the sum of stock awards, option awards, non-equity incentive plans, change in pension plans, long-term incentive plans, and other compensation. By construction, *Total Comp* is equal to the sum of *S.T. Comp* and *L.T. Comp*.

The average and median *Total Comp* of CEOs in our sample is $6.3 million and $4.0 million, respectively. These compensation numbers are comparable to those reported in other studies using similar sample periods (e.g., De Simone et al., 2021; Luna et al., 2020). The average long-term component of compensation (*L.T. Comp*) is larger at $5.2 million compared to the average short-term component of compensation (*S.T. Comp*) of $1.1 million. Since we use the natural logarithm of these compensation variables in our regression analysis, the table also reports descriptive statistics for the logged versions of these compensation variables. Table 2 also reports descriptive statistics for the tax variables that we use in our study. *Tax Shock* is an indicator variable that identifies fiscal year ends during the TCJA-transition period from December 31, 2017, to March 31, 2019. This variable has an average of 0.38, indicating that 38% of our sample observations fall during the TCJA-transition period. In our sample, the average *DTA* is 5.6% of the market value of equity, and the average *DTL* is 7.1% of the market value of equity.

Table 2 also reports the distribution of the visibility proxies we use in our analyses. The average (median) firm in our sample has a market value of equity of $9.1 billion ($1.4 billion). This compares to an average (median) market value of equity for the entire CRSP universe over the same sample period of $4.9 billion ($0.4 billion), which suggests that our sample is comprised of relatively large firms. The table also reports the distribution of our five individual visibility proxies (*Market Value*, *Analyst Coverage*, *Trading Volume*, *Liquidity*, and *Non-Zero Return Days*)

after normalizing them to be between 0 and 1.[15] Our primary proxy for visibility (*Visibility*) is the first principal component extracted from the five individual proxies for visibility and is also normalized to be between 0 and 1. In our sample, the average (median) value of *Visibility* is 0.60 (0.62).

The remainder of Table 2 reports the distribution of the control variables that we include in our models. On average, the firms in our sample have total assets of $17.2 billion, *Profitability* based on operating return on assets of 6.2%, *Past Returns* of 3.5%, a *Book-to-price* multiple of 0.54, annual idiosyncratic *Volatility* of stock returns of 32%, and a *Leverage* ratio of 58%. The average CEO in our sample is 56 years old, with a tenure of 9 years. These descriptive statistics tend to be comparable to those reported for CEOs in prior U.S. studies (e.g., Table 2 in Cadman et al., 2010; Table 3 in Conyon et al., 2011). Consistent with prior literature, we reduce the influence of skewness by using log transformations of total assets (*Size*), idiosyncratic volatility (*Volatility*), *CEO Age*, and *CEO* Tenure in our regression analyses.

To investigate the effect of TCJA tax rate change on deferred tax assets and liabilities, we plot, in Figure 1, the average changes in DTAs and DTLs (compared to the prior fiscal year) around the enactment of TCJA. Panel A reports the average change in $ millions, and Panel B reports the average change in %. Consistent with our expectations, we observe that firms change the value of recognized DTAs and DTLs over the quarters following the passage of TCJA in Q4-2017. The average deferred tax assets and liabilities declined by 18% to 24% for firms in their fiscal years ending between Q4-2017 and Q3-2018, indicating that the TCJA had a significant impact on firms' deferred tax assets and liabilities during this period.

[15] To normalize a variable over the [0, 1] interval, we subtract the minimum value of the variable and divide by the range of the variable (i.e., maximum minus minimum). This normalization does not alter the variable's distribution, but enables easier interpretation of regression coefficients.

5.2. Difference-in-Difference Analysis

We start our analysis with a base difference-in-difference regression of equation (1), where we regress the CEO's total compensation on *Tax Shock, DTA, DTL* and the interactions of *Tax Shock* with *DTA* and *DTL*. Specifically, the regression does not condition the interactive variable to vary with firm visibility and includes only controls for firm fixed effects and calendar year fixed effects. Since we include calendar year fixed effects in our multivariate models, our identification strategy takes advantage of the fact that the fiscal years covered by the TCJA-transition period stagger across calendar years. Hence, we are effectively comparing CEO compensation of firms in the same calendar year whose financials are more likely to reflect the tax effects to other firms whose financials are less likely to reflect the tax effects.

The results, reported in Column (1) of Table 1, reveal that the coefficients on *Tax Shock* × *DTA* and *Tax Shock* × *DTL* are indistinguishable from zero. These indicate that, when all firms are treated homogenously, the total compensation of CEOs is not associated with the tax windfall gains and losses associated with deferred tax assets/liabilities in the TCJA-transition period.

Since the rent extraction view predicts that pay for luck would be observed mainly in firms with poor managerial monitoring, we extend the base regression by allowing the coefficients on the interactive terms, *Tax Shock* × *DTA* and *Tax Shock* × *DTL,* to vary with firm visibility (*Visibility)*. The results in Column (2) of Table 3 show that the coefficient on *Tax Shock* × *DTL* is positive and significant (0.92 with a *t*-statistic of 2.80). The coefficient of 0.92 suggests that a firm in the third *DTL* quartile (0.072) paid its CEOs roughly 6% (= 0.92 × [0.072 – 0.005]) more in total compensation for the TCJA-transition period than that paid by a firm in the first *DTL* quartile (0.005). For a firm with median CEO compensation, the 6% greater pay translates to $240,726 (= 6% × 4,012.1).

However, the additional CEO compensation for DTL-related tax gains in the TCJA-transition-period declines as one moves from low visibility firms to higher visibility firms, as seen by the significantly negative coefficient on *Tax Shock* × *DTA* × *Visibility.* These results suggest that firms with low visibility but not high visibility paid their CEOs more in the TCJA-transition period for *DTL*-associated tax gains relative to other years.

In contrast to the coefficients on *DTL*, we find that the coefficient on *Tax Shock* × *DTA* is indistinguishable from zero, which suggests that firms with low visibility did not reduce the compensation of their CEOs in the TCJA-transition period if the firm had higher corporate tax windfall losses related to DTAs. Based on an *F*-test, we also reject the hypothesis (*p*-value of 0.003) that the coefficients on *Tax Shock* × *DTA* and *Tax Shock* × *DTL* are equal in magnitude. This result provides support for the asymmetric treatment of good luck (i.e., tax windfall gains) relative to bad luck (i.e., tax windfall losses) when firms compensate their CEOs, which is more consistent with the findings in Garvey and Milbourn (2006).

Also, in contrast to the results for *DTL*, we do not find any evidence that the relationship between CEO total compensation and *DTA* in the TCJA-transition period varies across firms based on their visibility. The coefficient on *Tax Shock* × *DTA* × *Visibility* is statistically insignificant.

The above results continue to hold even when we extend the regressions to include a set of firm-level and CEO-level control variables. In Column (3), we find that the coefficients on our main variables of interest (i.e., *Tax Shock* × *DTL*; *Tax Shock* × *DTL;* and their interactions with *Visibility*) remain largely unchanged even with the inclusion of these control variables. Further, the coefficients on the control variables are also consistent with those generally reported in prior studies (e.g., Conyon et al., 2011).

Finally, in Columns (4) and (5), we split total CEO compensation into short-term compensation (*S.T. Comp*) and long-term compensation (*L.T. Comp*) and re-estimate the model in Column (3). We find that while the coefficient on *Tax Shock* × *DTL* is positive and significant and the coefficient on *Tax Shock* × *DTL* × *Visibility* is negative and significant for *S.T. Comp* in Column (4), these coefficients are insignificant for *L.T. Comp* in Column (5). This suggests that CEO compensation for lucky tax windfalls was primarily in the form of short-term compensation, which is more consistent with the rent-extraction view.

To check the parallel trends assumptions underlying our analysis in Table 3, we modify that analysis to include interactions of the calendar year fixed effects with *DTA* and *DTL*. The results, reported in Table 4, show that the coefficients on the variables of interest for *DTL* tend to increase in magnitude and significance, but otherwise leave the earlier inferences unchanged. We also find evidence supportive of the parallel trends assumptions. Out of the twelve interaction terms of calendar year fixed effects with *DTA* and *DTL* in each of the five regression specifications in Table 4 (i.e., 60 coefficients in total), only 7 (4.2%) are statistically significant.[16] The results in this table also highlight the unique nature of the relation between *DTL* and compensation observed during the TCJA-transition period for low visibility firms. This relation is insignificant in almost all the years outside the transition period.

Overall, our analysis provides strong support that CEOs in less visible firms are compensated more for lucky tax windfall gains attributable to deferred tax liabilities during the TCJA-transition period. Also, this effect is primarily driven by the short-term component of compensation.

[16] Since four of these significant coefficients are in 2014 (i.e., *2014* × *DTL*), we repeat our analysis after excluding 2014 and find very similar results.

5.3. Additional Analyses

In Table 5, we use the same specification as in Column (3) of Table 4 and conduct a battery of robustness tests to assess the sensitivity of our main result. Columns (1) to (3) assess whether our timing choice related to the TCJA-transition period affects our results. In Column (1), we redefine *Tax Shock* by setting it equal to 1 for fiscal years ending between March 31, 2018, and March 31, 2019 (i.e., a shift of one quarter to the start of the TCJA-transition period). In Column (2), we exclude all observations with fiscal years ending between December 31, 2017, and June 30, 2018 (both inclusive) , while in Column (3), we exclude all observations with fiscal years ending between December 31, 2018, and March 31, 2019 (both inclusive). Across all three specifications, for slightly reduced samples, we find that our earlier inferences are unchanged. Specifically, the coefficients on *Tax Shock* × *DTL* and *Tax Shock* × *DTL* × *Visibility* continue to be statistically significant and comparable in magnitude to those reported in Table 4.

We next assess the sensitivity of our results to our sample selection procedures. Our previous analyses exclude three firm-year observations around each CEO turnover event (i.e., the year before, the year of, and the year after a CEO change). This reduces the noise in our analysis by eliminating the possibility that compensation changes associated with CEO turnovers are spuriously correlated with *Tax Shock*. However, this choice also reduces the sample size significantly. Therefore, in Column (4), we repeat the earlier analysis without excluding the CEO-turnover observations and instead include an indicator variable to identify CEO turnover years. We also include interactions of the turnover indicator with *Tax Shock*, *DTA*, *DTL*, and *Visibility*. As before, we continue to require at least one observation before and during the TCJA-transition period for each firm. This modification increases our sample size to 15,117 firm-years but otherwise makes little difference to our earlier inferences.

In Column (5), we restrict our sample to firm-years for purely domestic U.S. firms. Since the TCJA changed the federal tax rate for only U.S. income, firms with significant foreign pre-tax income may be less affected by the tax windfalls. Also, for these firms, the beginning-of-year *DTL*

and *DTA* would be a weaker proxy for the TCJA tax effects associated with changes in deferred tax assets and liabilities caused by the change in the federal tax rate. Moreover, the TCJA also introduced a transition tax on the deemed repatriation of foreign subsidiaries' earnings, which too could introduce noise in the analysis of firms with significant foreign income. To test the sensitivity of our conclusions to these issues, we exclude all observations with non-zero foreign pre-tax income (Compustat item: PIFO) and re-estimate our main specification for a reduced sample. Our results continue to hold.

In Column (6), we exclude observations before January 1, 2016 from our sample to ensure that our results are not sensitive to the increase in firms covered by Capital I.Q.'s People Intelligence database increasing in recent years. We find that our inferences remain unchanged. Finally, in Columns (7) and (8), we assess the sensitivity of our results to the choice of scalar when measuring our tax windfall proxies (i.e., *DTA* and *DTL*). In our main analysis, we scale deferred tax assets and liabilities by the market value of equity. Instead, we re-calculate *DTA* and *DTL* using either total assets (Column (7)) or book value of equity (Column (8)) as alternative scalars and re-estimate our main specification. Again, our inferences are unchanged.

In Table 6, instead of using a continuous measure of firm *Visibility*, we create an indicator variable to interpret our main result more easily. *Low Visibility* is an indicator variable that equals 1 for below-median values of *Visibility* and 0 otherwise. We re-estimate the main specification from Column (3) of Table 4 for *Total Comp* and find that our earlier results on CEO pay for tax windfall gain are observed only in the low visibility sample. The coefficient on *Tax Shock* × *DTL* is statistically insignificant, indicating that firms with high visibility do not reward their CEOs for the TCJA-driven tax windfall gains. In contrast, the coefficient on *Tax Shock* × *DTL* × *Low Visibility* is positive and significant, indicating that firms with low visibility paid their CEOs significantly more for the tax gains associated with deferred tax liabilities in the TCJA-transition period. When we repeat this analysis after separating CEO's total compensation into its components for short-term compensation (*S.T. Comp*) and long-term

compensation (*L.T. Comp*), we find that the effect is primarily driven by short-term compensation. This is consistent with our results observed in Tables 3 and 4.

Finally, in Table 7, instead of using the summary measure of firm *Visibility*, we re-estimate the main specification from Column (3) of Table 4 for each of the five individual proxies for firm visibility. Across the five individual proxies for firm visibility, we find consistent results with those reported for the summary measure. For four of the five proxies, the coefficient on *Tax Shock* × *DTL* remains positive and significant, and the coefficient on *Tax Shock* × *DTL* × *Visibility* remains negative and significant. For the last proxy (*Non-Zero Return Days*), the interaction terms have consistent signs but are not significant at conventional levels.[17]

Overall, our additional analyses show that the main results reported in Table 4 are not sensitive to our timing choice for the TCJA-transition period, sample selection procedures, choice of variable scalars, or selection of proxies for firm visibility.

6. Conclusion

This study examines whether executives get compensated for windfall profits that are unrelated to their efforts. While traditional contracting theories predict that managers should not be compensated for such gains, a more recent argument suggests otherwise based on the rent extraction view of compensation. The rent-extraction view (Bertrand and Mullainathan, 2001) contends that managers aim to extract maximum rent from their firms subject to the availability of funds and monitoring by investors and media. Although the rent-extraction view has attracted attention from regulators, practitioners and academics, there is little by way of evidence to uniquely support this view. We attempt to provide such evidence by examining how CEOs are

[17] In untabulated results, we also examine the number of institutional investors as an alternative proxy for investor visibility and the proportion of non-insider board members as a proxy for monitoring. However, these analyses employ reduced samples since the investor data is collected from FactSet and the board data is collected from Boardex, and we are unable to match these data to all firm-years in our primary sample. We note that the tenor of our inferences is unchanged when we employ these alternative proxies.

compensated for windfall profits obtained by their firms from the TCJA. We specifically study whether CEOs were rewarded or penalized for the one-off tax windfall gains and losses arising from remeasurements of deferred tax assets and liabilities due to TCJA lowering federal tax rates from 35% to 21%.

Using the beginning-of-year deferred tax assets and liabilities as instruments for the one-off tax gains or losses from TCJA and employing a difference-in-difference regression model, we find that firms with low visibility, but not those with high visibility, increase their CEOs pay in line with the tax gains from remeasured deferred tax liabilities. But neither low visibility nor high visibility firms penalize their CEOs for the one-off tax losses associated with TCJA. Moreover, the rewards for tax windfall profits are also mainly included in managers' short-term compensation packages, rather than being included with long-term compensation plans as would be the case to appropriately align managers' long-term interests with those of shareholders. Additional analyses confirm that these results are robust to various sensitivity checks and tests for parallel trends assumptions.

Our findings are inconsistent with the optimal compensation contracting theories, as the one-off tax windfalls from TCJA are beyond managers' control, relate to firms' past transactions and are unrelated to a firm's future opportunities and profitability. It is unclear how compensating managers for such tax windfall profits would help align managers' interests with those of shareholders and achieve shareholder-value maximization. In contrast, these findings are more consistent with the following three unique predictions of the rent-extraction view: (1) CEOs receive pay for luck, i.e., for performance unrelated to their effort and for factors that are unrelated to maximizing shareholder value; (2) CEOs are not penalized for bad luck; and (3) Only CEOs of low visibility firms receive pay for luck.

Although tax reforms of the extent of TCJA do not occur regularly and so reflect an unusual setting, at the very least, our findings raise the bar for theoretical models of executive compensation that rely on shareholder value-maximization arguments. These models will need to explain why paying low-visibility firms' CEOs for tax windfall gains but not penalize them for tax losses is optimal or in line with shareholders' interests.

It is worth noting that our findings on CEO rewards for tax windfall gains are likely to be a lower limit on the CEO's benefits derived from TCJA. As share prices of firms reflect the very many benefits to firms of TCJA, CEOs with significant stakes in their firms would have seen their wealth commensurately increase. Moreover, the over-performance of shares on account of TCJA could also lead to higher bonuses being paid to managers if share performance is a criterion for determining bonus payments. While many critiques have pointed out how TCJA has benefited CEOs and senior management through increased share prices, our study identifies yet another way CEOs have benefited, namely through the CEOs getting rewarded for one-off tax windfall profits associated with the remeasurement of deferred tax liabilities.

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APPENDIX A

Variable Definitions

Variable	Definition
Dependent Variables	
Total Comp	The natural log of 1 + total annual compensation for the firm's CEO in a given fiscal year. Total compensation is calculated as the sum of the cash compensation components (*Cash Comp.*) and the non-cash compensation components (*Non-Cash Comp.*) as collected by ExecuComp and CapitalIQ.
S.T. Comp	The natural log of 1 + short-term compensation, where short-term compensation is calculated as the sum of annual salary (Execucomp item SALARY; CapitalIQ item CTYPE1), annual bonus (BONUS; CTYPE2), and other annual cash compensation (OTHANN; CTYPE3).
L.T. Comp	The natural log of 1 + long-term compensation, where long-term compensation is calculated as the sum of annual stock awards (STOCK_AWARDS; CTYPE4), option awards (OPTION_AWARDS; Ctype21), non-equity incentive plans (NONEQ_INCENT; CTYPE48), change in pension plans (PENSION_CHG; CTYPE22), long-term incentive plans (LTIP; CTYPE7), and other compensation (OTHCOMP; CTYPE8).
Tax Variables	
Tax Shock	An indicator variable set equal to 1 for fiscal years ending between December 31, 2017, and March 31, 2019, and 0 otherwise.
DTA	Deferred tax assets (Compustat item TXNDBA) at the beginning of a given fiscal year scaled by the total market value of equity at the beginning of that fiscal year. The market value of equity is calculated using CRSP data for the price (PRC) multiplied by the number of shares (SHROUT).
DTL	Deferred tax liabilities (TXNDBL) at the beginning of a given fiscal year scaled by the total market value of equity at the beginning of that fiscal year.
Visibility Variables	
Market Value	The natural log of the market value of equity at the beginning of the year.
Analyst Coverage	The natural log of 1 + the number of unique equity research analysts that provided EPS forecast data to I/B/E/S (item NUMEST in Summary file) in the most recent month before the beginning of the year.
Trading Volume	The natural log of 1 + plus the U.S. average dollar value of trading in the firm's common stock at the beginning of the year. The average dollar value of trading is calculated over the previous year using the CRSP daily price (PRC) multiplied by the daily volume of shares traded (VOL).

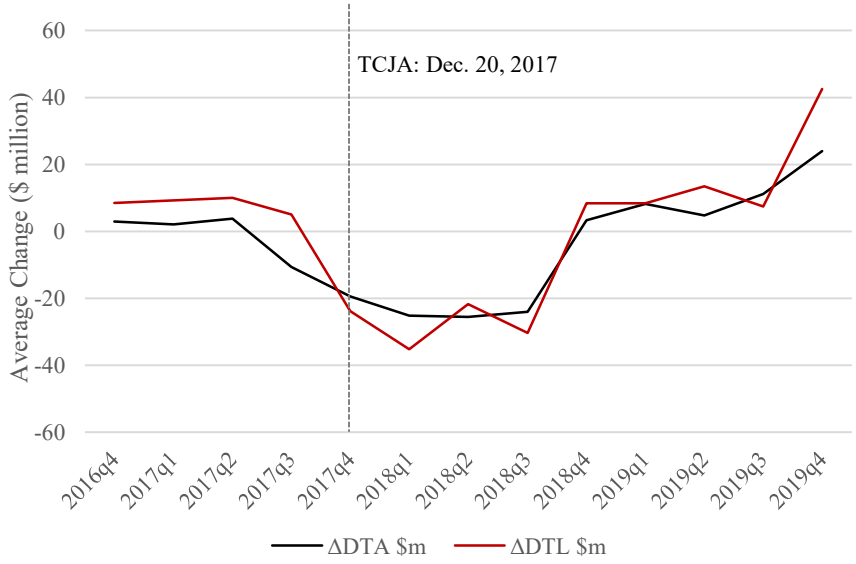
APPENDIX A (*Continued*)

Variable Definitions

Variable	Definition
Liquidity	The natural log of absolute daily returns (RET) divided by daily trading volume (VOL) averaged over the prior year. Specifically, we calculate the Amihud (2002) illiquidity measure as follows: $$Illiquidity = \frac{1}{N}\sum_{t=1}^{N}\frac{\lvert RET_t\rvert}{VOL_t} * 10^3$$ Finally, after applying the natural log transformation, we multiply by –1 to provide an increasing measure of liquidity.
Non-Zero Return Days	The proportion of trading days over the prior year with non-zero returns (i.e., RET ≠ 0).
Visibility	The first principal component extracted from the following five individual beginning-of-year proxies for visibility: *Market Value*, *Analyst Coverage*, *Trading Volume*, *Liquidity*, and *Non-Zero Return Days*.
Control Variables	
Size	The natural log of total assets (AT).
Profitability	Operating income before depreciation (OIBDP) scaled by average total assets.
Past Returns	Size- and industry-adjusted stock returns accumulated over the prior twelve months. Monthly size adjustments are based on NYSE/NASDAQ/AMEX decile portfolio returns using NYSE size breakpoints, and industry adjustments are based on the Fama and French 48 industry classification.
Book-to-price	Book value of equity (CEQ) scaled by the market value of equity.
Volatility	The natural log of the annual standard deviation of the residuals from a market model estimated using monthly returns over the prior twelve months, where market returns are proxied by changes in the CRSP value-weighted index (incl. dividends).
Leverage	Book value of total liabilities (LT) scaled by total assets (AT).
CEO Age	Natural log of the CEO's age in Execucomp (AGE) or calculated using CapitalIQ data (PERIODENDDATE – YEARBORN).
CEO Tenure	Natural log of 1 + the length of service of the person as the CEO. Length of service is measured using the difference between Execucomp items LEFTOFC and BECAMECEO and the difference between CapitalIQ items YEAR and STARTYEAR.

FIGURE 1. Average Change in DTA and DTL Around TCJA

Panel A: Average Change in Deferred Taxes ($ million)



Panel B: Average Change in Deferred Taxes (%)

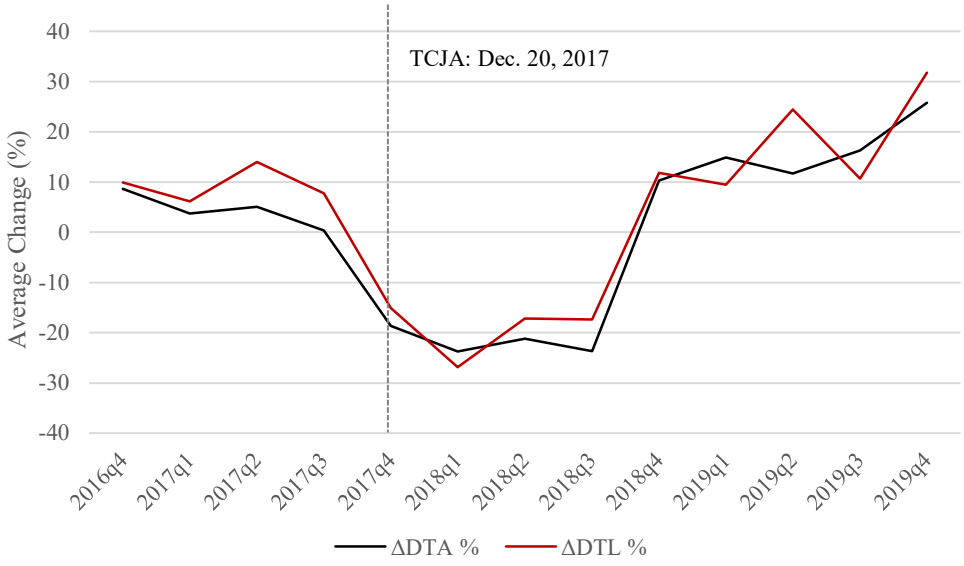


Figure 1 shows the average quarterly change in deferred tax assets (ΔDTA) and liabilities (ΔDTL) in dollar terms (Panel A) and percentage (Panel B) values, around December 20, 2017, i.e., enforcement date of Tax Cuts and Jobs Act (TCJA). See Appendix A for variable definitions.

TABLE 1. Sample Construction and Composition

Panel A: Sample Selection

	Firm-years
Non-missing data for CEO compensation and control variables (01/2013 – 12/2019)	17,323
Less: Firm-year before, firm-year of and firm-year after a CEO turnover	(4,862)
Less: Require at least one observation before and during the TCJA-transition period	(3,236)
Main sample covering 2,081 unique firms from 01/2013 to 12/2019	9,225

Panel B: Observations by Year

Year	Firm-years	Freq. %
2013	889	9.6%
2014	907	9.8%
2015	939	10.2%
2016	1,605	17.4%
2017	1,858	20.1%
2018	1,867	20.2%
2019	1,160	12.6%

Panel C: Observations by Industry

Fama-French 17 Industry	Firm-years	Freq. %
Automotive	191	2.1%
Chemicals	191	2.1%
Clothing	96	1.0%
Construction	289	3.1%
Consumer Products	369	4.0%
Durable Goods	142	1.5%
Fabricated Products	63	0.7%
Finance	1,743	18.9%
Food	207	2.2%
Machinery	963	10.4%
Mining	154	1.7%
Oil	369	4.0%
Services and Other	2,363	25.6%
Retail	441	4.8%
Steel	85	0.9%
Transportation	320	3.5%
Unclassified	935	10.1%
Utilities	304	3.3%

Table 1 reports the sample selection process and the sample composition by year. Panel A describes the selection process for firm-years in our sample. Panel B (Panel C) reports the number of observations by calendar year (industry) in our sample.

TABLE 2. Descriptive Statistics

Variables	Obs.	Mean	Std. Dev.	25th	Median	75th
Dependent Variables						
Total Comp ($000's)	9,225	6,286.1	24,989.8	1,610.7	4,012.1	7,783.8
ST Comp ($000's)	9,225	1,078.1	1,940.6	618.2	875.0	1,147.3
LT Comp ($000's)	9,225	5,221.0	24,854.2	745.9	2,984.8	6,609.1
Total Comp	9,225	8.116	1.237	7.385	8.297	8.960
ST Comp	9,225	6.641	1.080	6.428	6.775	7.046
LT Comp	9,225	7.295	2.379	6.616	8.002	8.796
Tax Variables						
Tax Shock	9,225	0.377	0.485	0.000	0.000	1.000
DTA	9,225	0.056	0.094	0.008	0.027	0.062
DTL	9,225	0.071	0.128	0.005	0.023	0.072
Visibility Variables						
Market Value ($m)	9,225	9,153.0	34,712.5	405.9	1,434.2	4,905.8
Market Value	9,225	0.513	0.205	0.385	0.516	0.644
Analyst Coverage	9,225	0.537	0.260	0.390	0.547	0.742
Trading Volume	9,225	0.575	0.222	0.443	0.598	0.738
Liquidity	9,225	0.589	0.209	0.478	0.610	0.731
Non-Zero Return Days	9,225	0.851	0.187	0.816	0.921	0.974
Visibility	9,225	0.599	0.197	0.486	0.618	0.741
Control Variables						
Size ($m)	9,225	17,177.1	113,198.5	414.7	1,610.9	6,298.0
Size	9,225	7.381	2.105	6.028	7.385	8.748
Profitability	9,225	0.062	0.226	0.025	0.097	0.157
Past Returns	9,225	0.035	0.362	-0.151	0.000	0.161
Book-to-price	9,225	0.541	0.557	0.226	0.425	0.711
Volatility	9,225	0.317	0.220	0.178	0.256	0.398
Volatility (log)	9,225	-1.329	0.563	-1.727	-1.364	-0.974
Leverage	9,225	0.579	0.278	0.379	0.578	0.782
CEO Age (years)	9,225	56.4	7.5	52.0	56.0	61.0
CEO Age	9,225	4.024	0.132	3.951	4.025	4.111
CEO Tenure (years)	9,225	8.9	7.9	3.0	7.0	12.0
CEO Tenure	9,225	2.001	0.785	1.386	1.946	2.565

Table 2 reports descriptive statistics for the variables used in this study. All variables are winsorized at the top and bottom 1% to reduce the influence of potential outliers due to data errors. See Appendix A for variable definitions.

TABLE 3. CEO Compensation Around TCJA

	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*	(4) *S.T. Comp*	(5) *L.T. Comp*
Tax Shock	0.061**	0.008	0.024	0.113**	0.369**
	(2.36)	(0.15)	(0.47)	(2.33)	(2.22)
DTA	0.182	0.539	0.369	0.418	-0.555
	(1.36)	(1.61)	(1.08)	(1.30)	(-0.55)
Tax Shock × *DTA* (β_3)	-0.224	0.054	-0.031	-0.154	1.326
	(-1.58)	(0.14)	(-0.08)	(-0.35)	(0.87)
DTL	-0.409***	-0.602**	-0.512*	-0.408	-1.806**
	(-3.24)	(-1.99)	(-1.74)	(-1.46)	(-2.00)
Tax Shock × *DTL* (β_5)	0.017	0.916***	0.903***	0.845***	0.324
	(0.17)	(2.80)	(2.80)	(3.05)	(0.30)
Visibility		1.572***	1.073***	0.185	1.428***
		(8.35)	(5.45)	(0.71)	(3.01)
Tax Shock × *Visibility*		0.090	0.072	-0.058	-0.356
		(1.17)	(0.97)	(-1.06)	(-1.62)
DTA × *Visibility*		-0.310	0.014	-0.085	1.328
		(-0.52)	(0.02)	(-0.13)	(0.89)
Tax Shock × *DTA* × *Visibility* (β_9)		-0.535	-0.340	-0.071	-2.331
		(-0.82)	(-0.53)	(-0.10)	(-1.07)
DTL × *Visibility*		0.371	0.075	0.235	1.927
		(0.70)	(0.14)	(0.50)	(1.44)
Tax Shock × *DTL* × *Visibility* (β_{11})		-1.419***	-1.407***	-1.173***	-0.481
		(-2.91)	(-2.92)	(-3.10)	(-0.33)
Control Variables:					
Size			0.146***	0.087***	0.164**
			(4.37)	(2.95)	(2.30)
Profitability			0.020	0.021	-0.084
			(0.33)	(0.45)	(-0.42)
Past Returns			0.089***	0.026**	0.101**
			(5.14)	(2.03)	(2.13)
Book-to-Price			0.001	-0.019	0.013
			(0.03)	(-1.27)	(0.15)
Volatility			-0.019	-0.002	-0.079*
			(-1.10)	(-0.14)	(-1.67)

TABLE 3. CEO Compensation Around TCJA (*Continued*)

	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*	(4) *S.T. Comp*	(5) *L.T. Comp*
Leverage			0.154**	0.152**	0.294*
			(2.45)	(2.36)	(1.70)
CEO Age			-0.406	-0.797	-0.383
			(-1.25)	(-1.50)	(-0.71)
CEO Tenure			0.039	0.149***	-0.034
			(1.28)	(3.14)	(-0.61)
Observations	9,225	9,225	9,225	9,225	9,225
Adjusted R^2	0.860	0.863	0.865	0.866	0.781
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
F-test: $\beta_3 = -\beta_5$ (*p*-value)	0.087	0.003	0.006	0.186	0.227
F-test: $\beta_9 = -\beta_{11}$ (*p*-value)		0.001	0.002	0.091	0.178

Table 3 reports coefficient estimates from panel regressions of CEO compensation measures (*Total Comp*, *S.T. Comp*, and *L.T. Comp*) on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), *Visibility* and their respective interactions. The regressions also include control variables, firm fixed effects, and year fixed effects. Columns (1) to (3) reports results for total CEO compensation (*Total Comp*), Column (4) reports results for the short-term component of CEO compensation (*S.T. Comp*), and Column (5) reports results for the long-term portion of CEO compensation (*L.T. Comp*). The *t*-statistics are based on standard errors clustered by firm. The asterisks *, **, and *** indicate statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively. See Appendix A for variable definitions. The main variables of interest are highlighted in grey.

TABLE 4. CEO Compensation Around TCJA Controlling for Parallel Trends

	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*	(4) *S.T. Comp*	(5) *L.T. Comp*
Tax Shock	0.042	-0.001	0.015	0.112**	0.358**
	(1.41)	(-0.02)	(0.29)	(2.11)	(2.11)
DTA	0.492*	0.842**	0.597	0.649	-0.180
	(1.89)	(2.02)	(1.45)	(1.35)	(-0.14)
Tax Shock × DTA (β_3)	-0.032	0.056	-0.103	-0.211	0.332
	(-0.16)	(0.13)	(-0.24)	(-0.41)	(0.20)
DTL	-0.573***	-0.703*	-0.623*	-0.481	-1.845*
	(-2.85)	(-1.85)	(-1.73)	(-1.23)	(-1.91)
Tax Shock × DTL (β_5)	0.099	1.029***	1.056***	0.850***	1.214
	(0.44)	(3.36)	(3.39)	(2.87)	(1.15)
Visibility		1.547***	1.055***	0.176	1.398***
		(8.19)	(5.33)	(0.67)	(2.94)
Tax Shock × Visibility		0.092	0.078	-0.048	-0.355
		(1.18)	(1.03)	(-0.81)	(-1.61)
DTA × Visibility		-0.307	0.023	0.064	1.583
		(-0.49)	(0.04)	(0.10)	(1.06)
Tax Shock × DTA × Visibility (β_9)		-0.316	-0.111	-0.084	-1.288
		(-0.45)	(-0.16)	(-0.11)	(-0.58)
DTL × Visibility		0.165	-0.121	0.143	1.304
		(0.28)	(-0.21)	(0.28)	(0.97)
Tax Shock × DTL × Visibility (β_{11})		-1.689***	-1.708***	-1.303***	-1.402
		(-3.26)	(-3.30)	(-3.01)	(-0.91)
2014 × DTA	-0.170	-0.243	-0.254	-0.147	-0.497
	(-0.60)	(-0.86)	(-0.92)	(-0.45)	(-0.73)
2015 × DTA	0.062	-0.033	0.021	-0.588	0.223
	(0.19)	(-0.10)	(0.07)	(-1.05)	(0.31)
2016 × DTA	-0.334	-0.340	-0.234	-0.344	-0.766
	(-1.26)	(-1.33)	(-0.95)	(-0.90)	(-1.11)
2017 × DTA	-0.514*	-0.542*	-0.424	-0.430	-0.738
	(-1.73)	(-1.89)	(-1.53)	(-1.01)	(-0.81)
2018 × DTA	-0.454	-0.313	-0.113	-0.104	0.763
	(-1.19)	(-0.84)	(-0.31)	(-0.21)	(0.68)
2019 × DTA	-0.587*	-0.517	-0.390	-0.557	-0.294
	(-1.68)	(-1.51)	(-1.17)	(-1.24)	(-0.38)

TABLE 4. CEO Compensation Around TCJA Controlling for Parallel Trends (*Continued*)

	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*	(4) *S.T. Comp*	(5) *L.T. Comp*
2014 × DTL	0.400**	0.402**	0.395**	0.059	0.730**
	(2.12)	(2.20)	(2.21)	(0.28)	(2.19)
2015 × DTL	0.076	0.121	0.109	0.003	0.166
	(0.35)	(0.58)	(0.52)	(0.01)	(0.38)
2016 × DTL	0.167	0.243	0.271	0.217	0.568
	(0.87)	(1.31)	(1.54)	(0.91)	(1.51)
2017 × DTL	0.174	0.392	0.385	0.275	0.496
	(0.57)	(1.34)	(1.39)	(0.85)	(0.73)
2018 × DTL	-0.106	0.029	-0.008	0.098	-0.793
	(-0.29)	(0.08)	(-0.02)	(0.25)	(-0.95)
2019 × DTL	0.045	0.123	0.115	0.182	-0.255
	(0.17)	(0.44)	(0.43)	(0.64)	(-0.52)
Observations	9,225	9,225	9,225	9,225	9,225
Adjusted R^2	0.861	0.863	0.865	0.866	0.781
Control Variables	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
F-test: $\beta_3 = -\beta_5$ (*p*-value)	0.760	0.002	0.007	0.248	0.273
F-test: $\beta_9 = -\beta_{11}$ (*p*-value)		0.001	0.002	0.111	0.197

Table 4 reports coefficient estimates from panel regressions of CEO compensation measures (*Total Comp*, *S.T. Comp*, and *L.T. Comp*) on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), *Visibility* and their respective interactions. The regressions include control variables, firm fixed effects, and year fixed effects. All columns also include the interactions of year fixed effects with *DTA* and *DTL.* Columns (1) to (3) reports results for total CEO compensation (*Total Comp*), Column (4) reports results for the short-term component of CEO compensation (*S.T. Comp*), and Column (5) reports results for the long-term portion of CEO compensation (*L.T. Comp*). The reported *t*-statistics are based on standard errors clustered by firm. The asterisks *, **, and *** indicate statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively. The table also reports *p*-values from *F*-tests of significant differences in magnitudes across the indicated coefficients. See Appendix A for variable definitions. The main variables of interest are highlighted in grey.

TABLE 5. CEO Total Compensation Around TCJA – Additional Tests

	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*	(4) *Total Comp*	(5) *Total Comp*	(6) *Total Comp*	(7) *Total Comp*	(8) *Total Comp*
Tax Shock	-0.015	-0.050	0.070	0.036	-0.006	0.007	-0.081	0.028
	(-0.19)	(-0.51)	(1.19)	(0.71)	(-0.10)	(0.13)	(-1.31)	(0.55)
DTA	0.453	0.441	0.843*	0.319	0.074	0.735	1.425	0.379*
	(1.11)	(1.01)	(1.83)	(0.88)	(0.17)	(1.45)	(1.36)	(1.94)
Tax Shock × DTA	0.316	0.553	-0.403	-0.337	-0.696	0.120	1.503*	0.000
	(0.56)	(0.79)	(-0.81)	(-0.78)	(-1.39)	(0.26)	(1.79)	(0.00)
DTL	-0.323	-0.357	-0.725*	-0.201	-0.074	-0.859*	-2.380**	-0.510***
	(-0.92)	(-0.89)	(-1.77)	(-0.57)	(-0.18)	(-1.70)	(-2.10)	(-3.24)
Tax Shock × DTL	0.869**	0.828*	1.177***	0.870***	1.493***	0.789**	2.695***	0.481***
	(2.43)	(1.90)	(2.83)	(2.68)	(4.48)	(2.15)	(2.99)	(2.69)
Visibility	0.897***	1.153***	1.094***	0.968***	1.111***	1.009***	0.933***	1.067***
	(4.40)	(4.72)	(5.14)	(5.36)	(4.70)	(4.38)	(4.72)	(5.68)
Tax Shock × Visibility	0.101	0.147	0.037	0.041	0.110	0.098	0.204**	0.037
	(1.10)	(1.32)	(0.52)	(0.55)	(1.23)	(1.22)	(2.35)	(0.56)
DTA × Visibility	0.209	0.461	-0.536	0.028	0.261	-0.738	1.027	0.047
	(0.35)	(0.65)	(-0.78)	(0.05)	(0.33)	(-0.89)	(0.64)	(0.16)
Tax Shock × DTA × Visibility	-0.056	-0.272	0.183	-0.083	0.634	-0.226	-1.043	0.191
	(-0.06)	(-0.24)	(0.26)	(-0.13)	(0.69)	(-0.31)	(-0.79)	(0.64)
DTL × Visibility	-0.639	-0.593	0.374	-0.345	-0.816	0.938	1.708	0.207
	(-1.13)	(-0.90)	(0.61)	(-0.64)	(-1.01)	(1.20)	(1.03)	(0.74)
Tax Shock × DTL × Visibility	-1.876***	-2.647***	-1.521***	-1.259***	-1.890***	-1.547**	-4.647***	-0.732***
	(-2.89)	(-3.58)	(-2.77)	(-2.66)	(-3.31)	(-2.44)	(-3.51)	(-2.97)
Observations	8,923	7,467	7,597	15,117	4,500	6,490	9,225	9,225
Adjusted R^2	0.859	0.857	0.880	0.821	0.890	0.874	0.865	0.865

TABLE 5. CEO Total Compensation Around TCJA – Additional Tests (*Continued*)

	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*	(4) *Total Comp*	(5) *Total Comp*	(6) *Total Comp*	(7) *Total Comp*	(8) *Total Comp*
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Turnover Controls	–	–	–	Yes	–	–	–	–
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E. × *DTA*	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E. × *DTL*	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 5 reports coefficient estimates from panel regressions of CEO total compensation (*Total Comp*) on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), *Visibility* and their respective interactions. The regressions also include control variables, firm fixed effects, year fixed effects, and the interactions of year fixed effects with *DTA* and *DTL*. Each column reports results from additional tests that use the same base specification as reported in Column (3) of Table 4. In Column (1), *Tax Shock* is set equal to 1 for all fiscal years ending between March 31, 2018, and March 31, 2019, and 0 otherwise. In Columns (2) to (8), *Tax Shock* is as defined in the base specification. In Column (2), we exclude all observations between December 31, 2017, and June 30, 2018 (both inclusive). In Column (3), we exclude all observations between December 31, 2018, and March 31, 2019 (both inclusive). In Column (4), we retain firm-years surrounding CEO turnover events (i.e., years –1, 0, and +1) and instead include a set of turnover controls. The set of turnover controls includes an indicator variable for firm-years (i.e., years –1, 0, and +1) affected by CEO turnover and interactions of this indicator with *Tax Shock, DTA, DTL,* and *Visibility*. In Column (5), we exclude all observations with foreign pre-tax income (Compustat item: PIFO). In Column (6), we exclude all observations before January 1, 2016. In Columns (7) and (8), we scale the deferred tax variables by total assets and common equity, respectively. The reported *t*-statistics are based on standard errors clustered by firm. The asterisks *, **, and *** indicate statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively. See Appendix A for variable definitions. The main variables of interest are highlighted in grey.

TABLE 6. CEO Total Compensation Around TCJA – *Low Visibility* Indicator

	(1) *Total Comp*	(2) *S.T. Comp*	(3) *L.T. Comp*
Tax Shock	0.078**	0.071**	0.088
	(2.29)	(2.47)	(0.94)
DTA	0.656**	0.681	0.937
	(2.27)	(1.22)	(1.49)
Tax Shock × DTA	-0.225	-0.320	-0.414
	(-0.81)	(-1.08)	(-0.57)
DTL	-0.699***	-0.327	-0.969**
	(-3.40)	(-1.24)	(-2.58)
Tax Shock × DTL	-0.188	-0.069	0.290
	(-0.70)	(-0.30)	(0.46)
Low Visibility	-0.039	-0.000	-0.089
	(-1.22)	(-0.00)	(-1.64)
Tax Shock × Low Visibility	-0.045	0.017	0.091
	(-1.60)	(0.62)	(1.28)
DTA × Low Visibility	-0.177	-0.028	-0.585
	(-0.76)	(-0.08)	(-1.23)
Tax Shock × DTA × Low Visibility	0.164	0.183	0.322
	(0.53)	(0.49)	(0.39)
DTL × Low Visibility	-0.043	-0.124	-0.298
	(-0.22)	(-0.55)	(-0.80)
Tax Shock × DTL × Low Visibility	0.563**	0.407*	0.431
	(2.45)	(1.79)	(0.69)
Observations	9,225	9,225	9,225
Adjusted R^2	0.864	0.866	0.780
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes
Year F.E. × *DTA*	Yes	Yes	Yes
Year F.E. × *DTL*	Yes	Yes	Yes

Table 6 reports coefficient estimates from panel regressions of CEO compensation measures (*Total Comp*, *S.T. Comp*, and *L.T. Comp*) on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), an indicator variable for below-median values of *Visibility* (i.e., *Low Visibility*) and their respective interactions. The regressions also include control variables, firm fixed effects, year fixed effects, and the interactions of year fixed effects with *DTA* and *DTL*. Column (1) reports results for total CEO compensation (*Total Comp*), Column (2) reports results for the short-term component of CEO compensation (*S.T. Comp*), and Column (3) reports results for the long-term portion of CEO compensation (*L.T. Comp*). The reported *t*-statistics are based on standard errors clustered by firm. The asterisks *, **, and *** indicate statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively. See Appendix A for variable definitions. The main variables of interest are highlighted in grey.

TABLE 7. CEO Total Compensation Around TCJA – Alternative *Visibility* Proxies

	(1)	(2)	(3)	(4)	(5)
Visibility proxy:	*Market Value*	*Analyst Coverage*	*Trading Volume*	*Liquidity*	*Non-Zero Return Days*
Dependent variable:	*Total Comp*	*Total Comp*	*Total Comp*	*Total Comp*	*Total Comp*
Tax Shock	0.043	-0.039	0.015	0.017	0.066
	(0.92)	(-0.97)	(0.29)	(0.33)	(0.88)
DTA	0.291	0.382	0.648*	0.809**	0.396
	(0.74)	(1.11)	(1.85)	(2.06)	(0.83)
Tax Shock × *DTA* (β_3)	-0.348	0.170	-0.087	-0.287	-0.071
	(-0.96)	(0.55)	(-0.22)	(-0.69)	(-0.11)
DTL	-0.308	-0.804***	-0.746***	-0.892***	-0.334
	(-0.91)	(-2.85)	(-2.59)	(-2.60)	(-0.93)
Tax Shock × *DTL* (β_5)	0.765***	0.729***	0.880***	1.126***	0.693
	(2.67)	(3.13)	(3.08)	(3.66)	(1.47)
Visibility	1.317***	0.240**	0.740***	0.643***	0.197***
	(4.83)	(2.52)	(4.29)	(3.52)	(2.62)
Tax Shock × *Visibility*	0.010	0.185***	0.085	0.069	-0.012
	(0.13)	(3.49)	(1.23)	(1.01)	(-0.15)
DTA × *Visibility*	0.840	0.324	-0.068	-0.375	0.169
	(1.15)	(0.68)	(-0.13)	(-0.69)	(0.30)
Tax Shock × *DTA* × *Visibility* (β_9)	0.361	-0.637	-0.132	0.299	-0.096
	(0.55)	(-1.24)	(-0.22)	(0.47)	(-0.13)
DTL × *Visibility*	-0.738	0.186	0.094	0.262	-0.445
	(-1.06)	(0.45)	(0.20)	(0.54)	(-1.01)
Tax Shock × *DTL* × *Visibility* (β_{11})	-1.355**	-1.368***	-1.441***	-1.773***	-0.667
	(-2.54)	(-3.28)	(-3.00)	(-3.71)	(-1.34)
Observations	9,225	9,225	9,225	9,225	9,225
Adjusted R^2	0.865	0.864	0.865	0.864	0.864
Control Variables	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E. × *DTA*	Yes	Yes	Yes	Yes	Yes
Year F.E. × *DTL*	Yes	Yes	Yes	Yes	Yes
F-test: $\beta_3 = -\beta_5$ (*p*-value)	0.169	0.001	0.016	0.019	0.173
F-test: $\beta_9 = -\beta_{11}$ (*p*-value)	0.098	0.000	0.003	0.005	0.140

Table 7 reports coefficient estimates from panel regressions of CEO total compensation (*Total Comp*) on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), *Visibility* and their respective interactions. The regressions also include control variables, firm fixed effects, year fixed effects, and the interactions of year fixed effects with *DTA* and *DTL*. Each column reports results for additional tests that use the same base specification as is reported in Column (3) of Table 4 but employs an alternative *Visibility* proxy (indicated at the top of each column). The reported *t*-statistics are based on standard errors clustered by firm. The asterisks *, **, and *** indicate statistical significance at the 10%, 5% and 1% levels (two-tailed), respectively. The table also reports *p*-values from *F*-tests of significant differences in magnitudes across indicated coefficients. See Appendix A for variable definitions. The main variables of interest are highlighted in grey.