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

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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 one-off 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 the one-time tax windfall gains during the TCJA-transition period. 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. The CEO pay associated with the tax windfalls cannot be explained as firms sharing these tax gains with all employees. These results are consistent with rent-extraction by CEOs of less visible firms.

JEL Classification: G30; H25; J33.

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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.

In contrast to the agency-centric theories, an alternative view is that CEO compensation is set to extract as much rent as possible from 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). Although this model predicts that CEOs would be compensated for one-off windfall gains, prior studies have been unable to establish its empirical validity. While supportive evidence had been presented initially for this viewpoint, subsequent theoretical models have explained these findings within the optimal contracting framework. Recent empirical studies have cast further doubt on the claim that CEOs get rewarded for good luck but not penalized for bad luck (e.g., Daniel, Li, Naveen, 2020), shaking the very foundation on which the model rests. These have led researchers to demand a more rigorous evaluation of this alternative rent extraction view. For example, Edmans, Gabaix and Jenter (2017, p 386) note 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.*

We respond to this call by empirically examining the rent extraction view in the context of one-off windfall profits and losses created by the passage of the Tax Cuts and Jobs Act (TCJA) of 2017.[1] If the rent extraction alternative is correct, CEOs of firms with limited investor and external visibility, but not other firms, should be compensated for the one-off windfall tax gains. Also, these CEOs should not be penalized for any one-off tax-related losses.

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 the tax rate would benefit 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 past periods' financial statements but for which taxes have not yet become due through the tax laws permitting these profits to be treated as taxable income in future periods.[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 firms from their deferred tax assets, which represent taxes that firms have paid ahead of their economic recognition in the financial statements. These firms had to take a one-time 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

[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."

[2] The deferral of taxes to the future on profits recognized in the financial statements 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).

markets increased approximately 25% between Trump's election in November 2016 and the legal passage of the TCJA in December 2017.

Unlike the recurring future benefits to all firms from lowering corporate tax rates, the tax gains or losses associated with deferred tax assets and liabilities are one-off, determined only by firms' past activities and idiosyncratic to each firm. Shareholder-value-based theories suggest that such gains and losses should not be considered for executive pay. Thus, examining the impact of TCJA-related windfall tax gains/losses on CEO pay can help more cleanly identify potential violations of these theories compared to evaluating pay for recurring gains and losses.

The tax-rate change under TCJA offers an excellent quasi-natural experiment to test the effect of lucky windfall gains on CEO compensation as these changes were largely unexpected and exogenous to individual firms and their managers' decisions. Although the tax-rate reduction was part of Trump's election manifesto, it was largely unexpected until Trump's surprise victory in 2016, making it highly unlikely that any firm or its managers could have influenced this rule change or acted in anticipation. 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 to predict the final form of this reform law and for firms to suitably adjust their activities.

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 an additional year to recognize these tax effects. As firms would have typically needed at least three or so months to gather all the relevant data and accurately compute the effects on their deferred tax assets and liabilities, we expect these tax effects to have been recognized in fiscal years ending anytime between December 2017 and March 2019. Accordingly,

we focus on these fiscal years (hereafter, the 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, as shown later, our conclusions are robust to marginally shifting the months covered by the TCJA-transition period.

The actual tax gains or losses reported by a firm in its income statement, including deferred tax assets and liability write-offs, are determined not only by tax-rule changes but also by managers' operating and tax planning activities during the year and the overall economic performance for that year. Thus, the actual tax gains or losses in a year would be endogenous to that year's CEO compensation. To tackle this endogeneity problem, 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').[3] This is because these one-off tax gains and losses 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), making these assets/liability values highly correlated with the TCJA tax effects. At the same time, the beginning-of-year values are exogenous to managerial activities and the broader economic performance during the year and so cannot influence executive pay through channels other than the one-off TCJA tax effects. Also, even if CEOs partly anticipated the TCJA rule changes, the reliance on beginning-of-year values mitigates the likelihood that our results reflect compensation for CEOs' activities taken in anticipation of the TCJA.

Employing a difference-in-differences approach, we investigate whether the CEO pay linked to the beginning-of-year deferred tax assets/liabilities differs in the TCJA-transition period

[3] Since the end-of-year values of deferred taxes would reflect the combined effects of (1) managerial actions during the year (including any potential tax-anticipatory actions), (2) firm performance during the year, and (3) the remeasurement of deferred taxes, changes in deferred taxes are also not suitable proxies for the TCJA effects.

relative to the other years. While CEO compensation is significantly positively associated with the beginning-of-year deferred tax liabilities in the TCJA-transition period, there is little evidence of such a relationship in other years.[4] This positive link in the TCJA-transition period is observed only for firms with poor investor visibility, indicating that these firms reward their CEOs for one-off windfall tax gains arising from the passage of TCJA. In contrast, we find no significant relationship between CEO pay and beginning-of-year deferred tax assets for the TCJA-transition period and the other fiscal years. This indicates that the pay of the CEOs of low-visibility firms is not commensurately decreased to reflect TCJA-related tax losses arising from the reduction in the value of their deferred tax assets.

Since the fiscal years affected by the 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 statements for that calendar year differ in their recognition of TCJA tax effects. So, for 2017, we 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 statements 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 statements reflect the TCJA tax effects than firms whose

[4] To corroborate this multivariate evidence, we also read the Compensation Discussion and Analysis (CD&A) section in the definitive proxy statements (DEF 14A) filed by a sub-set of our sample of firms prior to their annual general meeting during the TCJA-transition year. We find that some firms clearly disclose whether the tax windfall gains and losses are included or excluded in the assessment of firm performance for the purposes of executive compensation calculation. We even observe anecdotal evidence consistent with CEOs being rewarded for luck (see Appendix B). However, we find that most CD&As are either unclear about the treatment of the tax windfall gains and losses or there is sufficient discretion for the desired treatment. This is the variation in treatment of tax windfall gains and losses that our difference-in-differences analysis seeks to exploit.

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 only for firms with poor investor visibility. These results are robust to various alternative methodological choices and visibility proxies. Also, consistent with the observed effects being transitory, the results are driven exclusively by the discretionary component of CEO pay.

Our findings that CEOs of poor-visibility firms get paid for one-off tax windfalls arising from past activities but do not get penalized for corresponding tax losses cannot be explained by existing agency-centric explanations for CEO pay. They are, however, consistent with the rent extraction view. But still, we consider an alternative explanation that does not require CEOs to be opportunistic or extract rents from their firms. Specifically, we evaluate the possibility that firms shared their windfall profits with all employees, not just their CEOs. Although this reasoning does not explain why only low-visibility firms share windfall profits, we evaluate it by examining the effect of tax gains on firms' pre-tax operating expenses and margins. Since salaries and bonuses paid to employees are part of a company's operating expenses, sharing windfall profits should increase the cost of goods sold and other operating expenses disproportionately in the TCJA-transition period for firms with high beginning-of-year deferred tax liabilities than for firms with low beginning-of-year deferred tax liabilities. We, however, find no evidence to support this prediction, irrespective of whether we focus on the full sample or on a sub-sample of labor-intensive firms for whom one-off bonus payments would create a more noticeable impact on their operating expenses.

We conclude that rewarding CEOs for one-off windfall tax profits, but not tax losses, in firms with poor investor visibility is supportive of the rent extraction view. Our results indicate

that, on average, a firm in the third quartile of deferred tax liabilities paid its CEO 8.3% more in compensation for the TCJA tax effects than a firm in the first quartile. These figures are, however, only likely to be a lower bound for the TCJA-related benefits drawn by CEOs. This is because our analyses control for the normal bonuses associated with share price growth and ignore the wealth effects from share price gains for CEOs. These share-price-driven gains for CEOs are likely to be significant as well, since the passage of TCJA caused a significant share price run-up.

Our study builds on an influential study by Bertrand and Mullainathan (2001), who provide the first archival evidence supportive of the rent-extraction view. Using industry performance metrics to proxy for luck (i.e., for observable firm performance beyond a CEO's control), they document that weakly-governed CEOs get paid for luck. However, recent theoretical studies counter their conclusions by pointing out that payment for industry performance is consistent with optimal compensation contracts. These studies observe that such payments are needed to retain executive talent and appropriately incentivize CEOs (e.g., Oyer, 2004; Bizjak, Lemmon and Naveen, 2008; Brookman and Thistle, 2013; Gopalan, Milbourn and Song, 2010). However, none of these alternative models of shareholder-value maximization predicts CEOs' pay to reflect windfall gains that are beyond a CEO's control, especially if the gains are one-off, unrelated to future firm profitability and arise from the firm's past transactions.

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 now also need to explain why it is optimal, or in line with shareholders' interests, for low-visibility firms to pay their CEOs for one-off windfall gains.

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 and provide evidence consistent with such asymmetry. However, Daniel, Li, and Naveen (2020) document that the prior evidence on pay-for-luck asymmetry is highly sensitive to methodological choices and conclude that there is no robust evidence to support asymmetry in pay-for-luck. Their conclusions strike at the heart of this explanation for excessive CEO pay by questioning a fundamental premise of the rent-extraction view. But, both Daniel et al. (2020) and Garvey and Milbourn (2006) rely on the market or industry-wide returns to proxy for 'luck', which, due to their correlation with executives' outside opportunities, may not always reflect pay-for-luck asymmetries. These studies also use a two-stage methodology, raising the possibility of errors-in-variables concerns. We provide cleaner evidence on this issue by comparing executive pay response to TCJA effects on deferred tax assets with those on deferred tax liabilities.

This study also builds on prior work that examines the effects of nonrecurring items on executive pay (e.g., Gaver and Gaver, 1998; Potepa, 2020; Jang, Urcan and Yoon, 2020). But these studies do not evaluate whether the pay associated with such non-recurring items (e.g., restructuring charges, M&A charges, gains/losses from discontinued operations and asset sales) is consistent with agency-centric compensation theories or reflects reward-for-luck.

The remainder of the paper is structured as follows. The following section discusses the institutional details of 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.[5]

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 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.[6]

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

[5] While the TCJA bill covers both individual and corporate taxes, our focus is only on the latter.
[6] 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.

tax additionally affects the reported tax expense for multinational firms, our empirical analysis checks for the sensitivity 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). Berkshire Hathaway reported a net income of $24 billion in the previous year compared to this figure. Another financial services firm, Citigroup, reported a one-off tax loss 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.

Koutney and Mills (2018) and Wagner, Zeckhauser and Ziegler (2020) also confirm the hugely heterogeneous effects of TCJA through a systematic analysis of TCJA's immediate impact on S&P 500 firms. They document a roughly equal proportion of firms reporting earnings increases as those reporting decreases. They find that, for firms with positive TCJA tax effects, the aggregate earnings increased from $79 billion in the pre-TCJA-year to $315 billion in the TCJA year—a near quadrupling of profits, mainly due to the TCJA tax effects. For other firms in their sample, TCJA caused the aggregate earnings to fall from a profit of $160 billion in the pre-TCJA-year 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 indicate that the TCJA rule changes have significantly affected companies' financial performance in the transition year. Moreover, these effects varied across firms, implying

that relative performance measures (i.e., benchmarking a firm's financial performance against a set of peer firms) alone would be insufficient to remove the TCJA effects from CEO pay.

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 show that a fifth of the firms had nonrecurring tax benefits or expenses that were over 3% of the total assets. This study focuses exclusively on the nonrecurring tax effects of TCJA. More specifically, it focuses on the 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, the generally accepted accounting principles (GAAP) require firms to recognize the effects of these changes on the book values of deferred tax assets and liabilities in the same quarter as the passage of the law.[7] This means that firms with a fiscal year ending on December 31, 2017, would have had very limited time between the passage of TCJA law on December 22, 2017, and the public release of their 2017 Q4 earnings to gather the necessary information (including, from subsidiaries), analyze it and recognize the appropriate tax effects in the financial statements. Moreover, tax authorities typically issue clarifications and guidance in the weeks subsequent to a tax rule change, and such clarifications can have significant effects on the reported financial numbers. Recognizing these difficulties, the Securities Exchange Commission (SEC) allowed firms flexibility in the timing of when to recognize the TCJA tax effect in their financial statements (Staff Accounting Bulletin No. 118). Specifically, the SEC required firms to make a provisional recognition of the tax effects of TCJA in the first reporting

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

period in which the company could reasonably estimate this figure and allowed these figures to be revised over the next year.

Figure 1 plots the quarterly changes in deferred tax assets and tax liabilities over the period 2017 to 2019. Consistent with the expectation that TCJA led to substantial write-offs of deferred tax assets and liabilities, Figure 1 shows that the average change in deferred tax assets/liabilities across firms is negative in most quarters between 2017 Q4 and 2019 Q1.

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, effectively increasing 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) find 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. Further, Durrant, Gong and Howard (2021) show that firms increased CEO compensation in 2017 in the months before the TCJA tax rules became effective. Unlike these studies, we do not evaluate recurring tax effects on CEO pay. Instead, we 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 to 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 incentive contracts based on outcome metrics can be optimal if they also consider information that identifies output fluctuations unrelated to the agent's 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 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, their proxy for luck in performance. As they

[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 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).

find evidence of pay-for-luck in only weakly governed firms, Bertrand and Mullainathan (2001) conclude that a skimming view of compensation better explains these pay patterns. Under this skimming view, CEOs effectively control 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 (Bertrand and Mullainathan, 2000; 2001).[9]

Extending the rent extraction view 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 remuneration committee members, the CEOs can extract rent in the form of excessive compensation.[10] 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 benefits them. That is, managers would index their performance to a benchmark only when it is

[9] Bertrand and Mullainathan (2000; 2001) observe that, under the skimming view, CEO pay would be related to the non-luck part of a firm performance as well and that this would be done to detract investors' attention from the excessive pay package.

[10] 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.

down, not up. This selective benchmarking predicts that executive pay should be related to benchmark returns, mainly when these returns are positive. Garvey and Milbourn (2006) document supportive evidence using industry and market returns as proxies for benchmark returns.

However, Daniel et al. (2020) caution against relying on the pay-asymmetry evidence in Garvey and Milbourn (2006), claiming that their evidence is not robust to alternative specifications and methodological choices. Trying out 205 different specifications, they find that the evidence supportive of 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 fundamental question about this view: If CEOs truly control their pay-setting process, why are favorable and unfavorable luck factors treated symmetrically? More specifically, why do powerful CEOs willingly accept lower pay due to adverse exogenous shocks to performance?[11]

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

[11] 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.

general market conditions, firms 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 should reward CEOs for sector performance in order to suitably incentivize them to gather information about anticipated sector returns and adapt the firm's exposure to these returns.

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 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 ability of agency-centric models to explain executive compensation decisions and 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, such as industry and market returns. 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 in 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-differences 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.

As discussed in Section 2, the SEC required firms to provisionally recognize the TCJA tax effects in their financial statements as soon as they are able to estimate this figure reasonably and allowed them to revise the estimates for up to a year thereafter. So, a firm with March fiscal year-end could either have provided a provisional estimate in the fiscal year ending March 31, 2018, and revise these figures in the next fiscal year, 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., the 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 initial 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, our empirical model includes 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. However, as shown later, our results are robust to marginal changes in this definition of the TCJA-transition period.

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.

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

Our primary dependent variable is a CEO's total compensation (*Total Comp*) in a given year. To capture the tax losses and gains associated with changes in the deferred tax assets/liabilities each year, we use the beginning-of-year deferred tax assets (*DTA*) and deferred tax liabilities (*DTL*), scaled by the beginning market value of equity, respectively. As discussed earlier, the reliance on beginning-of-year values for *DTA* and *DTL* as instruments for TCJA tax effects mitigates the endogeneity concerns associated with actual tax gains or losses in a year. We interact the *DTA* and *DTL* with the *Tax Shock* indicator variable to capture the compensation effects of one-off tax benefits and expenses arising from the impact of TCJA on deferred taxes.

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 the 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 variation in internal governance metrics, lowering their usefulness in differentiating well-governed firms from poorly governed ones. Consistent with these, several studies have raised concerns about 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 given investors a direct say on the pay awarded to CEOs and senior executives by requiring 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. The SoP voting outcomes have significant reputational and litigation implications for firms, as adverse voting outcomes can lead a firm's management and its board of directors to potentially face political costs, negative public opinion, media backlash and shareholder pressure (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, since the advent of SoP voting, monitoring of CEO compensation by investors has taken on greater importance. We capture this broader monitoring by focusing on the visibility of firms to investors—the more visible a firm, the more attention it attracts from investors. Therefore, in line with the arguments of Bertrand and Mullainathan (2001), we expect that firms with greater visibility are less likely to engage in rent extraction.

We capture the investor visibility with a firm-specific, time-varying variable that summarizes five individual proxies of visibility drawn from prior literature (e.g., Bushee and Miller, 2012; Baker, Powell and Weaver, 1999). Our proxies are measured at the beginning of each fiscal year.[12] Larger firms and those with greater stock liquidity are commonly viewed as

[12] In specific, market value and analyst coverage are measured at the beginning of a fiscal year (i.e., as of month *t*-12, where *t* is the fiscal-year-end month to which the compensation relates), while stock liquidity variables are measured over the prior fiscal year (i.e., over months *t*-24 to *t*-12).

being more visible as they tend to be held by a broader set of investors and attract regular scrutiny from traders. Hence, two of our proxies for investor visibility are the firm's equity value (*Market Value*) and 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 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, as analysts select firms to cover based on demand for information from investors. We define analyst coverage as the number of analysts covering a firm in I/B/E/S (*Analyst Coverage*). We summarize these five proxies into a single variable for firm visibility with investors (*Visibility*) by extracting the first (and only) principal component with an eigenvalue above 1.0. This component explains 78% of the variation in the five individual proxies for visibility. We later check the robustness of our conclusions to the use of individual visibility proxies and to alternative proxies for a firm's external visibility.

Our regression model includes *Visibility* by itself as well as its interaction 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' visibility with investors.

The vector *X* represents a set of time-varying control variables for firm-level and CEO-level economic determinants of executive compensation identified in the prior literature (e.g., Cadman, Klasa and Matsunaga, 2010; Conyon, Core and Guay, 2011; Ellahie, Tahoun and Tuna, 2017). We include the logarithm of total assets (*Size*), return on assets (*Profitability*), size- and industry-adjusted returns over the previous twelve months (*Past Returns*), book-to-price ratio

(*Book-to-Price*), idiosyncratic risk (*Volatility*), and financial leverage (*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. All control variables are measured at the beginning of each year, consistent with the measurement of *DTA* and *DTL*. To control for time-invariant firm effects and common time-period effects, the regressions include firm fixed effects (λ_j), calendar year fixed effects (τ_t), and the interactions of calendar year fixed effects with *DTA* and *DTL*. 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 term, *Tax Shock* × *DTL,* helps us assess whether firms with low visibility incrementally compensate their CEOs for windfall tax profits in the TCJA-transition period relative to the other years, while the term *Tax Shock* × *DTA* assesses the corresponding effects for tax losses. The triple interaction terms enable us to infer whether these relationships differ significantly for firms with high visibility.

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 S&P Capital IQ'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,

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

stock awards, option awards, non-equity incentive plans, long-term incentive plans, and 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 IQ over the period 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.

We retain 17,323 observations with available data for CEO compensation and firm- and CEO-level control variables. We remove 4,862 observations 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 firms.[14]

[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.

Panel B of Table 1 summarizes the sample's composition by calendar year. We observe that the sample increases after 2016, which is due to a larger number of firms being covered by Capital IQ's People Intelligence database 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's total compensation (*Total Comp*), which is based on the total compensation data reported by the firm as collected by Execucomp or Capital IQ. When total compensation is missing, we calculate it using data on the individual components of compensation available in these databases.

The average and median *Total Comp* of CEOs in our sample are $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). In terms of the components of *Total Comp*, we find that the average salary or fixed component (*Fixed Comp*) is $0.8 million, while the average discretionary component (*Discretionary Comp*) is larger at $5.5 million.[15] 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 used in our study. *Tax Shock* is an indicator variable that identifies fiscal years ending in the TCJA-transition period (i.e.,

[15] The discretionary component is defined as total compensation minus the salary.

between December 31, 201, and March 31, 2019). The average of 0.38 for *Tax Shock* indicates that 38% of our sample observations fall during the TCJA-transition period. In our sample, the average *NDTL, DTA and DTL* are 1.7%, 5.6% and 6.9% of the market value of equity, respectively.

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), suggesting that our sample comprises relatively large firms. The table also reports the distribution of our five individual investor visibility proxies (*Market Value*, *Analyst Coverage*, *Trading Volume*, *Liquidity*, and *Non-Zero Return Days*) after normalizing them to be between 0 and 1.[16] Our primary proxy for investor visibility (*Visibility*) is the first principal component extracted from the five individual proxies of investor visibility and normalized to be between 0 and 1. In our sample, the average (median) value of *Visibility* is 0.59 (0.61). We also report the statistics for a media visibility proxy used in our robustness tests (*Media Visibility*). *Media Visibility* is computed as the number of full-sized articles published in media outlets covered by RavenPack during a fiscal year and is normalized to be between 0 and 1. It has an average (median) value of 0.52 (0.51).

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 return on assets of -0.7%, *Past Returns* of 3.2%, a *Book-to-price* multiple of 0.53, 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

[16] 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.

comparable to those reported for CEOs in prior studies (e.g., Table 2 in Cadman et al., 2010; Table 3 in Conyon et al., 2011). Consistent with the 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 2017 Q4. The average deferred tax assets and liabilities declined by 18% to 24% for firms in their fiscal years ending between 2017 Q4 and 2018 Q3, indicating that the TCJA had a significant impact on firms' deferred tax assets and liabilities during this period.

5.2. Compensation Disclosures

One easy to test whether firms pay for tax windfall gains is to check what firms explicitly say in their Compensation Discussion and Analysis (CD&A) section of definitive proxy statements (DEF 14A) about paying executives for TCJA tax effects. Towards this, we randomly selected 100 firms with beginning-of-year DTLs exceeding DTAs for the TCJA-transition period and so, are likely to have a tax windfall gain from the TCJA. We read the CD&A section of their proxy statements for the TCJA-transition period, looking specifically for narrative descriptions of the treatment of tax windfall gains in the assessment of firm performance for CEO compensation.

While 24 firms clearly described their treatment of the TCJA-related tax benefits, 76 firms were silent about the tax reform, which made it difficult to determine whether they included or excluded the TCJA-related tax effects when setting CEO pay. Among the 24 firms that were

explicit about the tax windfall treatment in their CD&A sections, only six admitted to including tax gains when deciding CEO compensation. The remaining 18 firms claimed to exclude the tax gains. The smaller fraction disclosing the inclusion of tax gains is likely due to a self-selection bias, whereby firms are less willing to be transparent when CEOs are compensated for tax gains. In Panel A of Appendix B, we provide examples of the cases where it was clear that the CEOs received higher compensation for tax windfall gains, and in Panel B, we provide examples where it was clear that the tax windfall gains were excluded for CEO compensation. The lack of clarity in the vast majority of the CD&A disclosures on the treatment of TCJA tax effects and the potential self-selection biases in disclosures make it impossible to draw clear conclusions from these disclosures alone.

5.3. Difference-in-Differences Analysis

We start our analysis with the estimation of Equation (1). To set the stage, we first examine whether CEOs are compensated for a firm's overall tax gain or tax loss from TCJA-related write-off of deferred tax liabilities net of the write-off of deferred tax assets. Towards this, we replace *DTL* and *DTA* in Equation (1) with a Net Deferred Tax Liabilities (*NDTL*) variable, defined as *DTL* minus *DTA*. The results from this regression are reported in Table 3.

In Column (1), when we regress the CEO's total compensation on *Tax Shock, NDTL,* and the interaction of *Tax Shock* with *NDTL* alone, the coefficient on *Tax Shock* × *NDTL* is insignificantly different from zero. This indicates that when all firms are treated homogenously, the total compensation of CEOs is not associated with the one-off tax windfall gains and losses based on the *net* deferred tax position 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 term, *Tax Shock* × *NDTL*, to vary with firm visibility among investors (*Visibility)*. The results in Column (2) of Table 3 show that the coefficient on *Tax Shock* × *NDTL* is now positive and significant (0.55 with a *t*-statistic of 2.49) and that the coefficient on the triple interaction variable, *Tax Shock* × *NDTL* × *Visibility* is significantly negative (-0.89 with a t-statistic of -2.32). These indicate that CEOs of low-visibility firms were rewarded for the net tax gains from one-off TCJA-transition adjustments to deferred taxes but that this reward declines for firms with greater visibility to investors. For firms with the highest visibility (*Visibility*=1), the sum of the coefficients on *Tax Shock* × *NDTL* and *Tax Shock* × *NDTL* × *Visibility* is not significantly positive. These results suggest that firms with low visibility, but not those with high visibility, paid their CEOs more in the TCJA-transition period for *NDTL*-associated tax gains relative to other years. Also, the coefficient on standalone *NDTL* is indistinguishable from zero, implying that CEOs do not get rewarded for deferred tax adjustments in years outside the TCJA-transition period.

In terms of economic significance, the coefficient of 0.55 on *Tax Shock* × *NDTL* suggests that the CEO of a firm with *NDTL* that is one standard deviation higher than the mean received approximately 8.3% (= 0.55 × 0.151) more in total compensation for the TCJA-transition period. For a firm with median CEO compensation, the 8.3% greater pay translates to $331,387 (= 8.3% × 4,012.1). These are economically significant rewards for CEOs of firms with low visibility with investors.

The above conclusions remain 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* × *NDTA* and the interaction term *Tax Shock* × *NDTL* × *Visibility*) remain largely unchanged even with the inclusion of these control variables. Further,

the coefficients on the control variables are consistent with those generally reported in prior studies (e.g., Conyon et al., 2011).

While the above regressions inform us whether CEOs get compensated for the net tax gains or losses, they do not evaluate whether CEOs get paid differently for the one-off tax gains relative to one-off tax losses. So, to shed light on the pay-for-luck asymmetry predicted by the rent extraction view, we examine the CEO pay effects separately for *DTA* and *DTL* using the regression specification given in Equation (1).

The results, reported in Column (1) of Table 4, reveal that the coefficients on *Tax Shock* × *DTA* and *Tax Shock* × *DTL* are statistically insignificant. As in the *NDTL* analysis, the insignificant coefficients indicate that when all firms are treated homogenously, CEOs compensation is not associated with the tax windfall gains and losses arising from the remeasurement of deferred tax assets/liabilities in the TCJA-transition period.

In Column (2) of Table 4, when we extend the base regression to allow the coefficients on the interactive terms, *Tax Shock* × *DTA* and *Tax Shock* × *DTL,* to vary with firm visibility (*Visibility),* we find that the coefficient on *Tax Shock* × *DTL* is positive and significant (1.23 with a *t*-statistic of 2.99). The coefficient of 1.23 suggests that a firm in the third *DTL* quartile (0.072) paid its CEOs roughly 8.3% (= 1.23 × [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 8.3% greater pay translates to $330,906 (= 8.3% × 4,012.1).

However, as seen by the significantly negative coefficient on *Tax Shock* × *DTL* × *Visibility*, the additional CEO compensation for *DTL*-related tax gains in the TCJA-transition period declines as investor visibility increases. In contrast, for firms with the highest Visibility (*Visibility=1),* we reject the null hypothesis that the sum of the coefficients on *Tax Shock* × *DTL* and *Tax Shock* ×

DTL × *Visibility* is significantly positive. This indicates that the highest visibility firms did not compensate their CEOs for the one-off TCJA tax gains. Overall, these results are consistent with low visibility firms, but not high visibility ones, paying their CEOs more in the TCJA-transition period for *DTL*-related tax gains relative to other years.

In contrast to the coefficients on *DTL*, the coefficient on *Tax Shock* × *DTA* is insignificantly different from zero, implying that the firms with low investor visibility did not reduce their CEO's compensation for tax losses arising from TCJA-related remeasurement of deferred tax assets. Based on an *F*-test, we also reject the hypothesis (*p*-value of 0.002) 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).

Further, 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 also statistically insignificant. Based on an *F*-test, we can reject the hypothesis (*p*-value of 0.001) that the coefficients on the triple interaction terms with *DTA* and *DTL* are equal in magnitude.

As in Table 3, 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) of Table 4. The coefficients on the control variables (untabulated) are similar to those reported in Table 3.

In Column (4), we include the interactions of *DTA* and *DTL* with the calendar year fixed effects to control for the average association between compensation and the deferred tax variables in each year. The interaction of calendar year fixed effects with *DTA* and *DTL* takes advantage of

the fact that the fiscal years covered by the TCJA-transition period stagger across calendar years. The identifications in these regressions rely on comparing CEO compensation of firms within the same calendar year but across those whose financials are more likely to reflect the TCJA-tax effects and those whose financials are less likely to reflect the tax effects. The results show that the coefficients on *Tax Shock* × *DTL* and *Tax Shock* × *DTL* × *Visibility* increase in magnitude and significance, but there is little change to our earlier inferences. This result highlights 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.[17] For subsequent analyses, we use the specification in Column (4) of Table 4 as the main model.

Finally, in Columns (5) and (6), we split total CEO compensation into the fixed salary and discretionary components and re-estimate the model in Column (4). If our earlier results genuinely reflect the higher CEO pay for one-off tax gains, we should observe these effects mainly in the discretionary, variable pay component rather than in the salary component. Consistent with this expectation, we find that the earlier results are entirely driven by the discretionary pay component. The coefficients of interest are statistically insignificant for the fixed salary component.[18]

5.4. Robustness Analyses

We next conduct a battery of robustness tests to assess the sensitivity of our main result, using the same specification as in Column (4) of Table 4. Tables 5 reports results for alternative

[17] Out of the twelve interaction terms of calendar year fixed effects with *DTA* and *DTL* only one is statistically significant in 2014 (i.e., *2014* × *DTL*), suggesting that the parallel trends assumption underlying our difference-in-differences analysis is not violated. We also repeat our analysis after excluding 2014 and find very similar results.
[18] In untabulated results, we also measure *Discretionary Comp* as the sum of the bonus, non-equity incentive plans, stock awards, and option awards and find similar results.

definitions of the TCJA-transition period and from varying sample selection choices. Table 6 reports results for alternative proxies for firm visibility.

In Column (1) of Table 5, we redefine *Tax Shock* and set 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. Also, in untabulated results, we obtain identical inferences when we exclude observations from January 1, 2016, to December 31, 2017, to reduce potential concerns about TCJA anticipation effects.

We next assess the sensitivity of our results to our sample selection procedures. First, we assess the sensitivity to excluding firm-year observations around CEO turnovers (i.e., the year before, the year of, and the year after a CEO change). This choice reduces the noise in our analysis and eliminates the possibility that the observed results are driven by golden handshakes or compensation changes associated with CEO turnovers. However, this choice also reduces the sample size. 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*.[19]

[19] As in earlier regressions, we require that each firm has at least one observation before and during the TCJA-transition period.

This modification increases our sample size to 15,117 firm-years but otherwise makes little difference to our earlier inferences.

In Column (5), we repeat our analysis after excluding firms with significant non-U.S. income. We do this for two reasons. First, the TCJA changed the federal tax rate for only U.S. income, which could attenuate the effect of this change on multi-national firms' profits. Secondly, the TCJA imposed a one-off transition tax on the profits of foreign subsidiaries, which could affect deferred tax remeasurements and introduce noise in analyses 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 of 4,500 firm-years. 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 IQ's People Intelligence database in the post-2016 period. 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*). We change the scalar for these variables from the market value of equity to either total assets (Column (7)) or book value of equity (Column (8)). Our inferences again remain unchanged.

Columns (1) through (5) to Table 6 report sensitivity checks when the summary measure of a firm's visibility to investors (*Visibility)* is replaced by each of the five individual proxies for investor visibility. Across the five individual proxies for 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.[20]

Recall from our discussions in Section 2 that we focus on firms' investor visibility to test the predictions of the rent extraction model, as the SEC's say-on-pay regulations have increased investors' role in determining the CEO's remuneration.[21] However, we recognize that investors are not the only market participants monitoring executive remuneration. As Core, Guay and Larcker (2008) find that the media also plays a monitoring role, we test the sensitivity of our conclusions to alternatively focusing on the media visibility of firms. Following Bushman, Williams, and Wittenberg-Moerman (2017), we measure media coverage based on the number of firm-specific full-size articles in RavenPack in the prior year.[22] As with our primary investor visibility variable, *Media Visibility* is also normalized to be between 0 and 1. Column (6) of Table 6 confirms that our earlier inferences continue to hold for media visibility.

Overall, our robustness 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.

[20] 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.

[21] We do not use SoP voting outcomes as proxies for investor monitoring, as these outcomes are dependent on CEO compensation and so would be endogenous in the regressions. In untabulated analysis, we checked whether low investor visibility firms that paid CEOs for TCJA tax gains suffered more SoP 'Against' votes. We find no evidence of such a change in SoP votes, consistent with these firms having lower monitoring by investors.

[22] To ensure that the media visibility metric is based only on articles that are focused on the firm, we include only news articles that have a relevance score of at least 80 in RavenPack.

5.5. Alternative Explanation

Our finding that CEOs of poor-visibility firms are paid for one-off tax windfalls but are not penalized for corresponding tax losses is inconsistent with agency-centric models that explain these payments as aligning the incentives of managers and shareholders. These findings are, however, consistent with the predictions of the rent extraction view. But still, in this sub-section, we consider an alternative explanation for our findings that does not rely on CEOs acting opportunistically or extracting rents from their firms.

One explanation for the pay-for-tax-windfalls in low-visibility firms is that firms share their windfall profits with all employees, not just with their CEOs. Although this possibility does not explain why only firms with low investor visibility might share their windfall gains with employees, we test this conjecture by evaluating firms' operating expenses. Since employee payments are part of a firm's cost of goods sold (COGS) and Selling, General and Administration (SG&A) expenses, this alternative explanation predicts that COGS and SG&A (scaled by sales) would increase disproportionately in TCJA-period for firms with higher beginning-of-year *DTL*.[23] To test this, we re-estimate Equation (1) after replacing *Total Comp* in Equation (1) with *COGS*, *SG&A*, and *COGS + SG&A* (all expressed as a percentage of sales). We are careful to exclude CEO pay and bonuses from the *SG&A* expenses so as to avoid these regressions reflecting our earlier results for CEOs. To the extent that low visibility firms shared their windfall tax profits with all employees, we expect a positive and significant coefficient on *Tax Shock* × *DTL*.

The results are reported in Table 7, Panel A. Irrespective of whether we focus on *COGS*, *SG&A* or *COGS+SG&A*, we find no evidence of a significantly positive coefficient for *Tax Shock*

[23] Consistent with analyses of CEO compensation, we rely on the beginning-of-year *DTA* and *DTL* as instruments for the one-off TCJA-related tax gains and losses. We do not use actual tax gains or losses in the analyses as these are likely to be endogenous to the tax gains and losses reported in the income statement.

× *DTL*. Further, the triple interaction terms (*Tax Shock* × *DTL* × *Visibility*) are insignificant in each of the three columns, implying that the effect of TCJA on operating expenses does not vary across high and low investor-visibility firms. These results do not support the view that low-visibility firms shared their tax windfalls with all their employees.[24]

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, namely the rent-extraction view, suggests otherwise. The rent-extraction view (Bertrand and Mullainathan, 2000; 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 empirical evidence to distinctly support this view. We attempt to provide such evidence by examining how CEOs are compensated for one-off windfall profits obtained by their firms from the passage of 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-differences regression model, we find that firms with low visibility, but not those with high visibility, increase their CEOs' pay in

[24] To check whether the lack of significance for the coefficients is to do with low power of the tests, which might occur if employee costs account for only a small part of a firm's operating expenses, we repeat the above regressions after partitioning our sample into labor-intensive and capital-intensive groups based on firms' investments in property, plant, and equipment as a fraction of total assets. For both groups, the coefficients on *Tax Shock* × *DTL* are insignificant in all the regressions.

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. Additional analyses confirm that these results are robust to various sensitivity checks and rule out alternative explanations that are more altruistic than the rent-extraction-view. Specifically, we rule out that the CEO pay for one-off tax gains reflects firms sharing their windfall gains with all employees.

As the tax gains that we examine are one-off, related to past transactions, unrelated to a firm's future opportunities and profitability, and beyond a CEO's effort, the pay-for-tax-windfall that we document is not consistent with optimal compensation contracting theories. It is unclear how compensating managers for such tax windfall profits would help align managers' interests with those of shareholders and help further shareholder-value maximization goals. However, our findings support all the predictions that uniquely arise from 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 penalizing them for tax losses is optimal or in line with shareholders' interests.

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 firms' share prices reflect their TJCA benefits, 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 TCJA critiques have pointed out how this law has benefited CEOs through increased share prices, our study documents an alternative channel through which CEOs have gained from this law, namely through their rewards for the one-off tax windfalls 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 total annual compensation for the firm's CEO in a given fiscal year. The data is collected from ExecuComp and S&P Capital IQ. Total compensation is the sum of salary (salary; ctype1), bonus (bonus; ctype2), non-equity incentive plans (noneq_incent; ctype48), stock awards (stock_awards; ctype4), option awards (option_awards; ctype21), long-term incentive plan payouts (ltip; ctype7), other annual cash compensation (othann; ctype3), and other compensation (othcomp; ctype8).
Fixed Comp	The fixed portion of the CEO's compensation, measured as the natural log of salary (Execucomp item salary; Capital IQ item ctype1).
Discretionary Comp	The discretionary portion of the CEO's compensation, measured as the natural log of total compensation minus salary.
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.
NDTL	Net deferred tax liabilities at the beginning of the fiscal year, scaled by the total market value of equity. Net deferred tax liabilities are calculated as deferred tax liabilities (Compustat item txndbl) minus deferred tax assets (txndba). The market value of equity is calculated using CRSP data for the price (prc) multiplied by the number of shares (shrout).
DTA	Deferred tax assets (txndba) at the beginning of the fiscal year, scaled by the total market value of equity.
DTL	Deferred tax liabilities (txndbl) at the beginning of a given fiscal year, scaled by the total market value of equity.
Visibility Variables	
Market Value	The natural log of the market value of equity at the beginning of the fiscal year, normalized to be between 0 and 1.
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 fiscal year, normalized to be between 0 and 1.
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 fiscal year. The average dollar value of trading is calculated over the prior year using the CRSP daily price (prc) multiplied by the daily volume of shares traded (vol). The variable is normalized to be between 0 and 1.

APPENDIX A (*Continued*)

Variable Definitions

Variable	Definition
Liquidity	The Amihud (2002) illiquidity measure at the beginning of the fiscal year, calculated as follows: $$Illiquidity = \frac{1}{N}\sum_{t=1}^{N}\frac{\lvert RET_t\rvert}{VOL_t}$$ The natural log of absolute daily returns (*RET*) divided by daily trading volume (*VOL*) averaged over the prior year. We multiply the variable by –1 to provide an increasing measure of liquidity and normalize it to be between 0 and 1.
Non-Zero Return Days	The proportion of trading days over the prior year with non-zero returns (i.e., $RET \neq 0$), normalized to be between 0 and 1.
Visibility	The first principal component extracted from the following five individual beginning-of-year proxies for investor visibility: *Market Value*, *Analyst Coverage*, *Trading Volume*, *Liquidity*, and *Non-Zero Return Days*. The variable is normalized to be between 0 and 1.
Media Visibility	The natural log of the number of full-size articles with a relevance score of 80 and above in RavenPack over the prior year. The variable is measured at the beginning of the fiscal year and is normalized to be between 0 and 1.
Control Variables	
Size	The natural log of total assets (at).
Profitability	Net income before extraordinary items (ib) 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 (leftofc – becameceo) in ExecComp and (year – startyear) in CapitalIQ.

APPENDIX B

Example Compensation Disclosures

Examples Supporting CEO Pay for Luck

Perficient Inc. (*Visibility*: 0.52; Tax benefit: $4.9 million)
"*As a result of the one-time benefit the Company received under the Tax Cuts and Jobs Act of 2017 (the "2017 Tax Act") and to facilitate retention of the Company's executive employees, the Compensation Committee approved the payment by March 15, 2018 of supplemental bonuses in the aggregate amount of $2.8 million to employees of the Company that are eligible to participate under the Executive Bonus Plan.*"

Walker & Dunlop, Inc. (*Visibility*: 0.58; Tax benefit: $58.3 million)
"*In evaluating the achievement of the diluted earnings per share metric, the Committee considered the $1.80 per share benefit recorded in the fourth quarter of 2017 associated with the revaluation of our net deferred tax liabilities as a result of the enactment of the 2017 Tax Cuts and Jobs Act.*"

Biglari Holdings, Inc. (*Visibility*: 0.44; Tax benefit: $53.5 million)
"*An incentive fee of $7,353,250 primarily attributable to net earnings of $50,071,000. Net earnings included an income tax benefit of $53,545,000 derived from a reduction in deferred tax liability related to the Corporation's significant unrealized gains on marketable securities.*"

CAI International, Inc. (*Visibility*: 0.42; Tax benefit: $16.9 million)
"*In 2017, actual after-tax income for the Company was approximately $72.1 million, an achievement of over 200% of budget, resulting in the maximum payout of $894,584 to Mr. Garcia under this performance objective.*"

Examples Against CEO Pay for Luck

Covenant Logistics Group, Inc. (*Visibility*: 0.54; Tax benefit: $42.5 million)
"*The Compensation Committee reviewed the 2017 bonus targets and our 2017 year-end results and, based upon such review, determined that the consolidated performance targets were achieved at the 25% level. In making this determination, the Compensation Committee used its negative discretion to exclude the favorable impact of the Tax Cuts and Jobs Act of 2017 (the "TCJA").*"

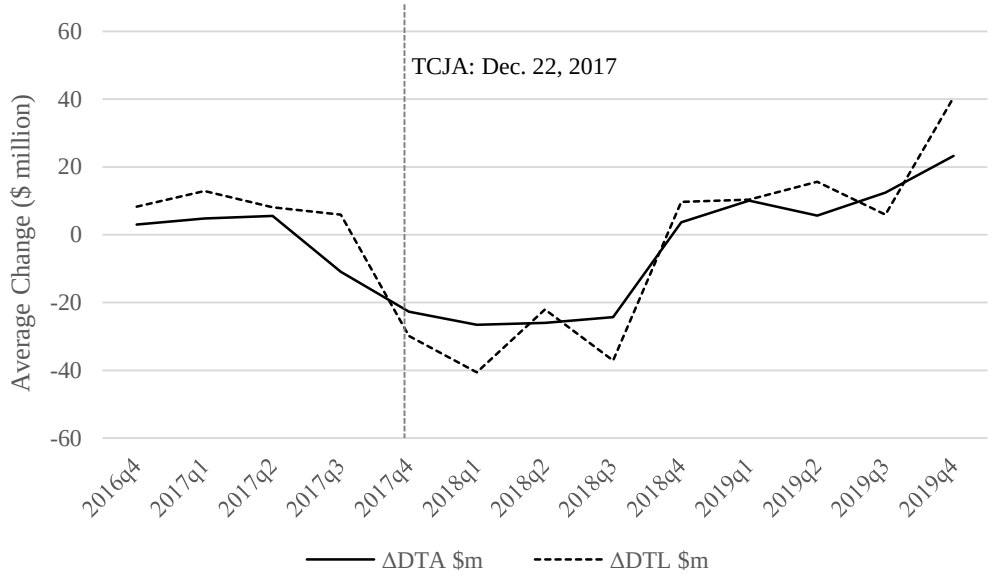
MTS Systems Corp. (*Visibility*: 0.52; Tax benefit: $25.0 million)
"*The Committee determined that the impact of the Tax Cuts and Jobs Act of 2017 was outside of plan performance and therefore utilized downward discretion to reduce the EPS payout to Threshold value. The payout achieved prior to applying downward discretion was 200%.*"

Marten Transport Ltd. (*Visibility*: 0.52; Tax benefit: $56.5 million)
"*The 2017 earnings considered by the committee included the adjustments for the after-tax aggregate value of vesting performance unit awards and cash bonus awards. Additionally, the $56.5 million deferred income taxes benefit related to the federal Tax Cuts and Jobs Act of 2017 was excluded from the calculation of 2017 net income.*"

GATX Corp. (*Visibility*: 0.69; Tax benefit: $371.4 million)
"*GATX net income achievement for 2017 was $185.0 million for incentive payout purposes, or 84.8% of target for our NEOs, resulting in payouts at 81.6% of their target awards. For annual incentive award purposes, net income excludes tax adjustments and other items.*"

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 (%)

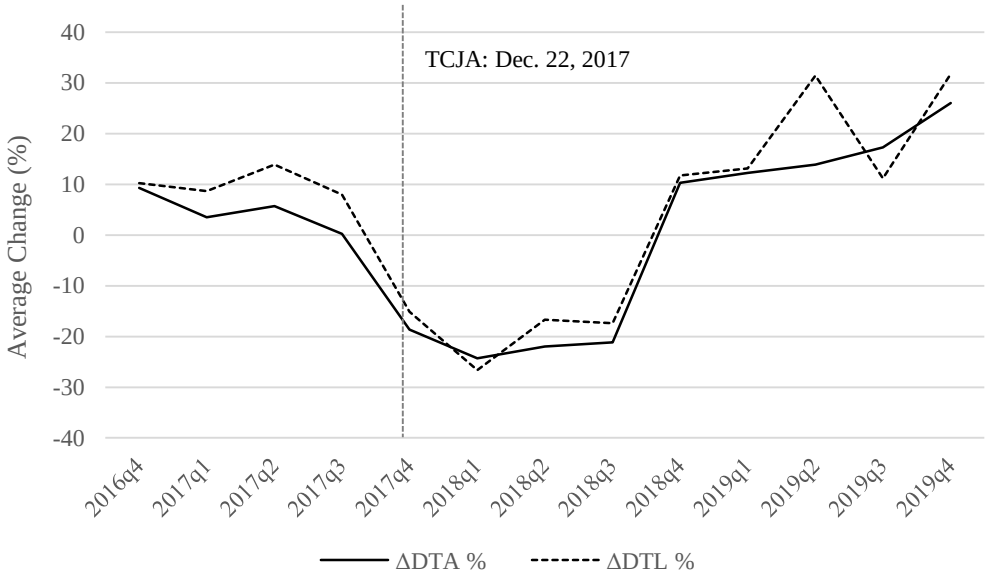


Figure 1 shows the average quarterly change in deferred tax assets (ΔDTA) and liabilities (ΔDTL) in dollar terms (Panel A) and percentage (Panel B) values, around December 20, 2017, i.e., enforcement date of Tax Cuts and Jobs Act (TCJA). See Appendix A for variable definitions.

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
Fixed Comp ($000's)	9,056	778.2	434.3	500.0	739.2	1,000.0
Discretionary Comp	9,056	5,522.1	25,150.9	1,013.9	3,221.8	6,851.2
Total Comp (log)	9,225	8.116	1.237	7.385	8.297	8.960
Fixed Comp (log)	9,056	6.397	1.102	6.217	6.607	6.909
Discretionary Comp (log)	9,056	7.642	1.844	6.923	8.078	8.832
Tax Variables						
Tax Shock	9,225	0.377	0.485	0.000	0.000	1.000
NDTL	9,225	0.017	0.151	-0.013	0.000	0.022
DTA	9,225	0.056	0.091	0.008	0.027	0.062
DTL	9,225	0.069	0.122	0.005	0.023	0.072
Visibility Variables						
Market Value ($m)	9,225	9,153.0	34,712.5	405.9	1,434.2	4,905.8
Market Value	9,225	0.494	0.150	0.400	0.496	0.590
Analyst Coverage	9,225	0.476	0.231	0.346	0.486	0.659
Trading Volume	9,225	0.537	0.155	0.445	0.553	0.651
Liquidity	9,225	0.552	0.152	0.471	0.567	0.655
Non-Zero Return Days	9,225	0.942	0.072	0.928	0.969	0.990
Visibility	9,225	0.592	0.153	0.504	0.606	0.702
Media Visibility	9,069	0.519	0.092	0.462	0.509	0.572
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	-0.007	0.195	0.003	0.029	0.072
Past Returns	9,225	0.032	0.339	-0.151	0.000	0.161
Book-to-price	9,225	0.530	0.445	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	0.556	-1.727	-1.364	-0.974
Leverage	9,225	0.577	0.267	0.379	0.578	0.782
CEO Age	9,225	56.4	7.5	52.0	56.0	61.0
CEO Age (log)	9,225	4.025	0.131	3.951	4.025	4.111
CEO Tenure	9,225	8.9	7.9	3.0	7.0	12.0
CEO Tenure (log)	9,225	2.000	0.783	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 – Net Tax Windfalls

	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*
Tax Shock	0.050**	0.055	0.060
	(2.10)	(1.01)	(1.12)
NDTL	-0.064	-0.064	-0.030
	(-1.62)	(-0.79)	(-0.35)
Tax Shock × NDTL	0.038	0.547**	0.555***
	(0.53)	(2.49)	(2.59)
Visibility		2.095***	1.339***
		(9.48)	(5.47)
Tax Shock × Visibility		-0.005	-0.006
		(-0.06)	(-0.08)
NDTL × Visibility		0.004	-0.074
		(0.01)	(-0.27)
Tax Shock × NDTL × Visibility		-0.889**	-0.896**
		(-2.32)	(-2.38)
Control Variables:			
Size			0.145***
			(4.25)
Profitability			0.012
			(0.16)
Past Returns			0.090***
			(4.77)
Book-to-Price			-0.050
			(-1.59)
Volatility			-0.024
			(-1.30)
Leverage			0.104
			(1.50)
CEO Age			-0.467
			(-1.42)
CEO Tenure			0.045
			(1.47)
Observations	9,225	9,225	9,225
Adjusted R^2	0.860	0.863	0.865
Firm F.E.	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes

Table 3 reports coefficient estimates from panel regressions of CEO total compensation (*Total Comp*) on *Tax Shock*, net deferred tax liabilities (*NDTL*), *Visibility*, their respective interactions, control variables, firm fixed effects, and year fixed effects. 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 – Tax Windfall Gains and Losses

Dependent variable:	(1) *Total Comp*	(2) *Total Comp*	(3) *Total Comp*	(4) *Total Comp*	(5) *Fixed Comp*	(6) *Discretionary Comp*
Tax Shock	0.063**	-0.011	0.000	-0.010	0.109**	0.051
	(2.40)	(-0.17)	(0.00)	(-0.15)	(2.34)	(0.41)
DTA	0.205	0.590	0.436	0.667	0.834*	0.504
	(1.45)	(1.36)	(0.98)	(1.32)	(1.65)	(0.45)
Tax Shock × *DTA* (β_3)	-0.231	0.122	0.001	-0.046	-0.094	-0.922
	(-1.59)	(0.24)	(0.00)	(-0.09)	(-0.19)	(-0.76)
DTL	-0.459***	-0.786*	-0.625	-0.704	-0.607	-1.881*
	(-3.47)	(-1.81)	(-1.48)	(-1.44)	(-1.20)	(-1.84)
Tax Shock × *DTL* (β_5)	0.003	1.231***	1.223***	1.393***	0.308	2.672***
	(0.03)	(2.99)	(3.04)	(3.50)	(1.10)	(2.91)
Visibility		2.060***	1.299***	1.277***	0.897***	1.764***
		(8.39)	(5.02)	(4.93)	(3.62)	(3.69)
Tax Shock × *Visibility*		0.122	0.109	0.116	-0.105	0.047
		(1.18)	(1.09)	(1.15)	(-1.59)	(0.26)
DTA × *Visibility*		-0.338	0.047	0.044	0.037	0.419
		(-0.43)	(0.06)	(0.05)	(0.05)	(0.26)
Tax Shock × *DTA* × *Visibility* (β_9)		-0.677	-0.416	-0.175	-0.434	0.158
		(-0.78)	(-0.49)	(-0.19)	(-0.49)	(0.08)
DTL × *Visibility*		0.607	0.244	-0.009	0.192	1.719
		(0.80)	(0.32)	(-0.01)	(0.29)	(1.17)
Tax Shock × *DTL* × *Visibility* (β_{11})		-1.949***	-1.960***	-2.318***	-0.599	-2.848*
		(-3.03)	(-3.10)	(-3.38)	(-1.24)	(-1.91)
Observations	9,225	9,225	9,225	9,225	9,056	9,056
Adjusted R^2	0.860	0.864	0.865	0.865	0.901	0.818
Control Variables	No	No	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year × *DTA* F.E.	No	No	No	Yes	Yes	Yes
Year × *DTL* F.E.	No	No	No	Yes	Yes	Yes
F-test: $\beta_3 = -\beta_5$ (*p*-value)	0.070	0.002	0.004	0.004	0.651	0.041
F-test: $\beta_9 = -\beta_{11}$ (*p*-value)		0.001	0.002	0.002	0.222	0.056

Table 4 reports coefficient estimates from panel regressions of various CEO compensation measures (*Total Comp*, *Fixed Comp*, and *Discretionary Comp*) on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), *Visibility*, their respective interactions, control variables, and fixed effects. Columns (1) to (4) report results for total CEO compensation (*Total Comp*), Column (5) reports results for the fixed portion of CEO compensation (*Fixed Comp*), and Column (6) reports results for the variable 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. The table also reports *p*-values from *F*-tests of 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	-0.038	-0.085	0.053	0.028	-0.039	-0.028	-0.122	0.017
	(-0.43)	(-0.74)	(0.75)	(0.45)	(-0.56)	(-0.41)	(-1.64)	(0.27)
DTA	0.428	0.337	1.095*	0.399	0.171	0.839	1.352	0.423*
	(0.88)	(0.61)	(1.93)	(0.89)	(0.30)	(1.26)	(1.03)	(1.70)
Tax Shock × DTA	0.288	0.562	-0.305	-0.383	-0.770	0.128	1.631	-0.086
	(0.40)	(0.66)	(-0.50)	(-0.75)	(-1.17)	(0.22)	(1.58)	(-0.29)
DTL	-0.293	-0.298	-0.998*	-0.190	0.042	-1.023	-2.698*	-0.554***
	(-0.63)	(-0.54)	(-1.80)	(-0.45)	(0.07)	(-1.49)	(-1.94)	(-2.68)
Tax Shock × DTL	1.277***	1.434***	1.321***	1.082***	1.839***	1.164**	3.448***	0.619***
	(2.72)	(2.65)	(2.59)	(2.86)	(4.17)	(2.42)	(3.28)	(2.95)
Visibility	1.115***	1.497***	1.268***	1.191***	1.396***	1.227***	1.104***	1.280***
	(4.16)	(4.58)	(4.59)	(5.04)	(4.38)	(4.17)	(4.23)	(5.15)
Tax Shock × Visibility	0.140	0.201	0.057	0.053	0.151	0.152	0.266**	0.050
	(1.14)	(1.35)	(0.62)	(0.55)	(1.29)	(1.43)	(2.33)	(0.58)
DTA × Visibility	0.350	0.735	-0.805	-0.045	0.301	-0.788	1.270	0.035
	(0.44)	(0.78)	(-0.88)	(-0.06)	(0.27)	(-0.70)	(0.61)	(0.09)
Tax Shock × DTA × Visibility	-0.036	-0.314	0.140	0.011	0.830	-0.250	-1.193	0.343
	(-0.03)	(-0.21)	(0.15)	(0.01)	(0.68)	(-0.26)	(-0.70)	(0.84)
DTL × Visibility	-0.709	-0.736	0.858	-0.368	-0.973	1.190	2.284	0.241
	(-0.89)	(-0.78)	(0.99)	(-0.54)	(-0.86)	(1.07)	(1.06)	(0.63)
Tax Shock × DTL × Visibility	-2.566***	-3.598***	-1.918***	-1.640***	-2.461***	-2.160**	-5.983***	-0.976***
	(-2.92)	(-3.45)	(-2.65)	(-2.78)	(-3.24)	(-2.55)	(-3.53)	(-2.96)
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	0.891	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 × *DTA* F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × *DTL* F.E.	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*), *Investor* Visibility, their respective interactions, control variables, and fixed effects as indicated. Each column reports results from additional tests that use the same base specification as reported in Column (4) 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 – Alternative Visibility Proxies

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual proxies included in *Visibility*					
Visibility proxy:	*Market Value*	*Analyst Coverage*	*Trading Volume*	*Liquidity*	*Non-Zero Return Days*	*Media Visibility*
Dependent variable:	*Total Comp*	*Total Comp*	*Total Comp*	*Total Comp*	*Total Comp*	*Total Comp*
Tax Shock	0.033	-0.046	-0.011	-0.004	0.051	-0.084
	(0.58)	(-1.15)	(-0.18)	(-0.07)	(0.26)	(-0.57)
DTA	0.250	0.487	0.738*	0.981**	0.161	0.276
	(0.50)	(1.40)	(1.72)	(2.11)	(0.12)	(0.41)
Tax Shock × *DTA* (β_3)	-0.372	0.200	-0.013	-0.297	0.439	0.383
	(-0.80)	(0.64)	(-0.03)	(-0.56)	(0.24)	(0.53)
DTL	-0.261	-0.825***	-0.811**	-0.935**	0.410	-0.060
	(-0.55)	(-2.68)	(-2.08)	(-2.12)	(0.36)	(-0.09)
Tax Shock × *DTL* (β_5)	1.033***	0.776***	1.209***	1.492***	1.826	1.899***
	(2.82)	(3.12)	(3.20)	(3.78)	(1.38)	(3.02)
Visibility	1.630***	0.242**	0.963***	0.834***	0.450**	0.679**
	(3.88)	(2.25)	(3.79)	(3.30)	(2.31)	(2.12)
Tax Shock × *Visibility*	0.030	0.216***	0.133	0.104	0.001	0.320
	(0.30)	(3.59)	(1.31)	(1.08)	(0.00)	(1.15)
DTA × *Visibility*	1.049	0.373	-0.078	-0.522	0.523	0.683
	(1.02)	(0.66)	(-0.10)	(-0.69)	(0.34)	(0.53)
Tax Shock × *DTA* × *Visibility* (β_9)	0.484	-0.743	-0.238	0.395	-0.605	-0.898
	(0.51)	(-1.25)	(-0.26)	(0.44)	(-0.31)	(-0.65)
DTL × *Visibility*	-0.921	0.249	0.207	0.362	-1.184	-1.092
	(-0.88)	(0.49)	(0.29)	(0.51)	(-0.93)	(-0.88)
Tax Shock × *DTL* × *Visibility* (β_{11})	-1.993***	-1.676***	-2.175***	-2.575***	-1.832	-3.687***
	(-2.71)	(-3.43)	(-3.08)	(-3.78)	(-1.31)	(-2.90)
Observations	9,225	9,225	9,225	9,225	9,225	9,069
Adjusted R^2	0.865	0.865	0.865	0.865	0.864	0.861
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year × *DTA* F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year × *DTL* F.E.	Yes	Yes	Yes	Yes	Yes	Yes
F-test: $\beta_3 = -\beta_5$ (*p*-value)	0.098	0.001	0.006	0.010	0.083	0.012
F-test: $\beta_9 = -\beta_{11}$ (*p*-value)	0.081	0.000	0.003	0.006	0.085	0.010

Table 6 reports coefficient estimates from panel regressions of CEO total compensation (*Total Comp*) on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), *Visibility* proxies, their respective interactions, control variables, and fixed effects as indicated. Each column reports results for additional tests that use the same base specification as is reported in Column (4) 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 differences in magnitudes across indicated coefficients. See Appendix A for variable definitions. The main variables of interest are highlighted in grey.

TABLE 7. Employee Compensation

Dependent variable:	(1) *COGS*	(2) *SG&A*	(3) *COGS + SG&A*
Tax Shock	0.392**	-0.022	0.020
	(2.30)	(-1.31)	(0.85)
DTA	-0.260	-0.050	0.395
	(-0.36)	(-0.53)	(1.51)
Tax Shock × DTA	-1.055	0.302**	0.009
	(-1.37)	(2.22)	(0.04)
DTL	-0.823	-0.247***	-0.799***
	(-1.48)	(-2.95)	(-2.62)
Tax Shock × DTL	-1.019**	-0.046	0.170
	(-2.48)	(-0.57)	(0.92)
Visibility	-1.643*	-0.214***	-0.093
	(-1.79)	(-2.78)	(-0.79)
Tax Shock × Visibility	-0.455*	0.022	-0.053
	(-1.83)	(0.87)	(-1.48)
DTA × Visibility	-0.104	-0.010	-0.830*
	(-0.10)	(-0.06)	(-1.74)
Tax Shock × DTA × Visibility	1.359	-0.485**	-0.017
	(1.17)	(-2.16)	(-0.04)
DTL × Visibility	2.030**	0.334**	1.603***
	(2.33)	(2.57)	(2.93)
Tax Shock × DTL × Visibility	1.058*	0.165	-0.124
	(1.75)	(1.19)	(-0.37)
Observations	9,090	7,709	7,709
Adjusted R^2	0.747	0.929	0.813
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes
Year × *DTA* F.E.	Yes	Yes	Yes
Year × *DTL* F.E.	Yes	Yes	Yes

Table 7 reports coefficient estimates from panel regressions of various firm-level employee compensation expense proxies on *Tax Shock*, deferred tax variables (*DTA* and *DTL*), *Visibility*, their respective interactions, control variables, and fixed effects. The employee expense proxies are Cost of Goods Sold (*COGS)*, Selling, General and Administration expenses (*SG&A)*, and *COGS + SG&A,* as indicated in the table's top row. All proxies are scaled by sales. For each column, we use the same base specification as reported in Column (4) of Table 4.