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ABSTRACT

Do Big Inequalities in Executive Pay Hurt Firm Performance?*

Research Question/Issue: Do large, within-firm executive pay differences hurt firm performance? Prior literature shows mixed results concerning the sign of the relationship between executive pay disparity and firm performance. This study evaluates that literature, clarifies what tournament theory predicts about the relationship, identifies methodological pitfalls and how to address them, and guides future scholarship in this area of considerable importance to firms and policy makers. *Research Findings/Insights*: We estimate the relationship using improved methodology and find evidence of an inverted-U shaped relationship between the executive pay spread and firm performance. However, the peak of this inverted U occurs at such a high level of the executive pay spread that it is practically irrelevant in most firms. The inverted U is found using a market-based measure of firm performance, but not a returns-based measure (i.e., ROA). *Theoretical/Academic Implications*: This study addresses the theoretical and empirical limitations of the prior literature, thereby providing more credible estimates of the relationship between pay disparity and firm performance. Tournament theory offers a unified framework that can explain an inverted-U-shaped relationship between the executive pay spread and firm performance. *Practitioner/Policy Implications*: Our results should reduce public concerns that CEOs increase their own compensation to exorbitant levels, to the detriment of firm performance.

JEL Classification: G32, G39, J31, M12

Keywords: executive compensation, vertical pay disparity, firm performance, tournament theory, market structure

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I. Introduction

The incentives and effort of a firm's top executives, which derive from the pay raise they can expect if promoted to Chief Executive Officer (CEO), have important implications for firm performance. The prospect of a bigger monetary prize from promotion to CEO creates powerful incentives for executives to invest effort, thereby fueling higher levels of firm performance. But can prizes become too big, to the detriment of firm performance? Answering that question is particularly important given well-known concerns that CEOs have undue influence over their own compensation and may effectively award themselves exorbitant pay packages that are not in shareholders' interests (Bebchuk and Fried 2006).

To date, however, the answer remains elusive. A literature, predominantly in accounting and finance, has investigated the relationship between firm performance and vertical pay disparity, specifically the "executive pay gap", which compares the CEO's pay to the average pay of the firm's other top executives.[1] The evidence from that literature, as summarized in Table I, is mixed. Some studies find a positive relationship, others a negative or no relationship, and still others a curvilinear relationship or one that flips signs under certain conditions.[2]

The accounting and finance literature almost universally uses tournament theory (Lazear and Rosen 1981) to explain a positive relationship between executive pay disparity and firm performance, based on the following logic. A large difference between the CEO's pay and that of the firm's other executives is a "prize" that the other executives will win if promoted to CEO. The

[1] A closely related literature compares the CEO's pay not to that of the firm's other executives but to the average pay of the firm's other workers. The present study focuses on the executive pay gap, that is, the comparison of the CEO's pay to that of the firm's other executives. Tournament theory is the main theoretical foundation for both lines of inquiry, though it is more appropriate for studying the executive pay gap, given that a firm's (non-CEO) executives have the most realistic chance of being promoted to CEO.

[2] To facilitate comparisons with the existing literature, the present study is included in Table I's first row. Throughout this study, frequent reference is made to the "studies of Table I", the "evidence from Table I", etc. Such references mean all studies in Table I excluding the present study.

prospect of receiving this prize motivates the executives to work hard, with their greater effort translating into higher firm performance. To explain the negative relationships in Table I, researchers invoke alternative theories such as social comparison and equity theory, or what some have collectively called behavioral theory. The basic theme in these alternative theories is that large inequities in pay discourage those who are paid less, depressing their effort levels.

We have three goals: (1) evaluating the literature in Table I to better understand how to interpret the mixed results; (2) producing new estimates of the relationship between executive pay disparity and firm performance using improved empirical methods, thereby providing more credible estimates of the relationship of interest; (3) empirically distinguishing the roles of executive incentives (i.e., effort provision) and ability-based executive selection into tournaments, by incorporating CEO-firm fixed effects into the empirical model.

The forthcoming theoretical and empirical observations may create the impression that our study is a critique of the literature in Table I. To some extent that impression is unavoidable in any discussion of methodological improvements. Nonetheless, we see our contribution not as a critique but rather as clarifying what we have learned from existing work and building on it to guide future scholarship in this area of considerable importance to firms and policy makers. Our study, including its insights concerning methodological improvements, would not be possible were it not for the foundational work of the previous literature.

As a first step in evaluating the literature, we examine the underlying theory. Tournament theory is the most invoked theory in this literature and is, as noted above, almost universally used in this literature to explain a positive relationship between executive pay disparity and firm performance. We identify a misinterpretation of tournament theory in this literature, which has significant implications for interpreting the empirical evidence in Table I. The literature routinely claims that tournament theory predicts a positive relationship between firm performance and vertical pay disparity, interpreting an estimated positive relationship as supportive of the theory and a negative or null relationship as unsupportive. However, tournament theory in fact predicts a non-monotonic, inverted-*U*-shaped relationship.

As a second step in evaluating the literature, we examine the empirical methods used to estimate the relationship of interest. We identify four methodological pitfalls that affect the studies in Table I:[3] (1) improperly incorporating fixed effects from the standpoint of tournament theory; (2) winsorizing the pay ratio; (3) ignoring or inadequately accounting for endogeneity of the vertical pay disparity in the firm performance equation; and (4) including lagged dependent variables and/or alternative measures of the dependent variable (lagged or contemporaneous) on the empirical model's right-hand side.

Concerning the first pitfall, the type of fixed effects incorporated into the empirical model should derive from the underlying theory. As noted, tournament theory is the most invoked theory in this literature. For testing tournament theory's predictions, which are based on the non-CEO executives' incentives, CEO-firm fixed effects are superior to firm fixed effects. The reason is that in empirical models that include firm fixed effects, some of the variation in the pay gap that identifies the relationship of interest comes from the firm switching CEOs, which is problematic because changes in a firm's leadership are accompanied by a host of other changes that can affect firm value that are unrelated to the pay gap and to non-executives' behavioral responses to it. None of the studies in Table I use CEO-firm fixed effects.

Concerning the second pitfall, the practice of winsorizing all continuous variables on both ends, typically at 1% and 99% but sometimes even at 5% and 95%, thwarts the identification of the relationship of interest by throwing away some of the most valuable variation in the extremes

[3] All four limitations do not necessarily afflict each study in Table I.

of the pay distributions, particularly the right tail of the executive pay distribution that is needed to identify an inverted-*U*-shaped relationship based on data on either side of its peak. Winsorizing at the top of the distributions introduces significant biases, as we show in a Monte Carlo analysis in the online appendix and then in the actual data.

Concerning the third and fourth pitfalls, endogeneity of vertical pay disparity poses a challenge for measuring the relationship of interest.[4] Some studies in Table I ignore the problem altogether. Others acknowledge the problem but implement methodologies to mitigate it that are known to worsen the problem. Others use instrument-based methods which, though in principle can mitigate the problem, are based on invalid instruments that researchers do not properly motivate. Specifically, when motivating their chosen instruments, these studies explain only the relevance of the instruments and typically remain silent concerning their validity.

As mentioned, our second main goal is to produce new estimates based on improved empirical methods that address the preceding methodological issues. We find an inverted-*U*-shaped relationship, consistent with tournament theory, i.e., firm performance increases with executive pay disparity up to a point and then decreases. Nonetheless, the inflection point is so high as to be practically irrelevant for most firms, i.e., the relationship is positive over most of its range.

Our empirical estimates include CEO-firm fixed effects, as appropriate for testing tournament theory. In addition to allowing us to avoid the first methodological pitfall, CEO-firm fixed effects allow us, for the first time in this literature, to separately identify executive incentives

[4] Endogeneity in this setting can be categorized into two types: "natural" (i.e., due to the nature of the problem) and "researcher induced". Of the four methodological pitfalls mentioned in this study, only the pitfall numbered (3) is naturally related to endogeneity, while those numbered (1), (2), and (4) lead to researcher-induced endogeneity. Researcher induced endogeneity is widespread in Table I and, paradoxically, the very econometric methods that induce endogeneity are frequently adopted in the name of mitigating endogeneity. For example, Table I reveals that the practices in (4) are commonly adopted in the name of endogeneity mitigation. But, in fact, they introduce endogeneity for econometric reasons that are well known and reviewed in the online appendix.

and ability-based CEO sorting across firms as alternative mechanisms generating the inverted *U*. Note that a change in the pay gap can affect firm performance in two ways. One is via tournament incentives, operating through changes in the effort levels of the executives who compete to become CEO. The second is via selection, by attracting higher-ability CEOs to the firm, at least to the extent that higher CEO pay levels tend to be associated with larger executive pay gaps. Both effects are simultaneously present in empirical models that include firm fixed effects, whereas only the first is present in models that include CEO-firm fixed effects. We compare estimates from both empirical models to reveal whether executive incentives and CEO selection both contribute to generating the inverted *U*. Our estimates are consistent with the presence of both mechanisms.

Our empirical estimates do not winsorize the executive pay variables, thereby avoiding the second methodological pitfall. In addition to avoiding researcher induced endogeneity via afflicting the independent variable with avoidable measurement errors, we introduce an instrument for vertical pay disparity that is less prone to the limitations of the instruments in prior work. Our instrument is a "counterfactual pay ratio" that compares the CEO's pay in firm *A* to the average pay of firm *B*'s executives, where the two firms are "connected" in a particular manner that would facilitate information exchange about their compensation policies, such as if the CEO in one firm sits or sat on the other firm's board of directors. This instrument plausibly satisfies the validity criterion that most studies in this literature (that attempt to address endogeneity) do not mention. Although we see this instrument as an improvement over prior attempts, it too has limitations that we will discuss.

Our study is not the first to identify an empirical inverted *U*. For example, Burns et al. (2017) find it in the U.S., the U.K., Switzerland, South Africa, China, and Australia, but not in Sweden, Norway, the Netherlands, France, Germany, India, Hong Kong, and Canada. Thus, the present study's contribution is in interpreting the inverted *U* correctly within the context of tournament theory, showing that it is driven by executive incentives as opposed to entirely by ability-based selection, and measuring it using improved methods (which change the result quantitatively).

This study is organized as follows. Section II reviews the theoretical arguments underlying the literature in Table I. Section III highlights common methodological pitfalls to avoid in future research. Section IV presents the empirical analysis measuring the relationship between firm performance and the executive pay gap, applying the insights from sections II and III. Section V includes robustness tests and further analysis. Section VI discusses the broader implications of the empirical results. Section VII summarizes the main results and concludes. The online appendix details the methodological pitfalls to avoid in future research, e.g., section A1 reports a Monte Carlo analysis (see Table V) illustrating that winsorizing the pay ratio introduces a form of measurement error that is destructive to the main result.

II. Theory and Hypotheses: Relationship Between the CEO Pay Gap and Firm Performance

Different theories are invoked in the literature of Table I, depending on the hypothesized sign of the relationship between the executive pay gap and firm performance. Tournament theory is invoked nearly exclusively to explain a positive relationship. Shin et al. (2015), for example, only discuss tournament theory when reviewing the literature explaining a positive relationship. An exception is Cheng et al. (2019), which instead relies on ability-based CEO selection into firms. The idea there is that larger pay spreads are more appealing to external executive talent and enable boards of directors to hire more able CEOs, thereby improving firm performance. An assumption underlying this argument, i.e., the external applicant pool is likely to contain more executive talent than the internal pool, should usually hold, given the relative sizes of these pools in most settings.[5]

[5] In fact, if the external pool has sufficiently higher executive ability than the internal pool, Tsoulouhas, Knoeber, and Agrawal (2007) show theoretically in a tournament setting that it may be profitable to handicap internal candidates to give outsiders a better chance to become CEO.

Various theoretical explanations for a negative relationship have been posited. One is CEO "rent extraction" theory (see, e.g., Bebchuk, Fried, and Walker 2002). The idea is that boards of directors do not set CEO compensation at arm's length from the CEO. Rather, the CEO has considerable influence over her compensation and can inflate it to exorbitant levels, to the detriment of firm performance.

Other theoretical rationales for a negative relationship are based on executives' perceptions of justice and fairness. Shin et al. (2015) refer to these (closely related) fairness-based theories collectively as "behavioral theory". They include equity theory (Adams 1965), relative deprivation theory (Martin 1981; Cowherd and Levine 1992; Siegel and Hambrick 1996; Henderson and Fredrickson 2001; Siegel and Hambrick 2005; Fredrickson, Davis-Blake, and Sanders 2010), and social comparison theory (Crosby 1976; Festinger 1954).[6] The basic idea underlying these theories is that executives are demotivated by the perceived unfairness of large pay gaps between themselves and the CEO, which depress their performance and, ultimately, firm performance.

To elaborate, equity theory (Adams 1965) argues that executives' perceptions of fairness in rewards between peer groups affects their motivation to work, which ultimately affects firm performance. Relatedly, social comparison theory argues that individuals compare themselves to others and are sensitive to observed differences in rewards. Given that executives' pay packages are more visible than those of other employees, such social comparisons are relatively easily made. Relative deprivation theory says that individuals feel deprived if they observe large differences in rewards between themselves (receiving a small reward) and those in a comparison group (receiving a large reward). A large executive pay gap would be one such example. These theories argue that such perceived inequities reduce individuals' performance in various ways that harm firm

[6] See also Akerlof and Yellen (1990) and Wade et al. (2006).

performance (e.g., less commitment to the organization, less willingness to cooperate and collaborate with peers).

An extensive discussion of behavioral theories as applied to research on vertical pay disparity and firm performance appears in Ridge et al. (2015). Those authors argue that social comparison theory and relative deprivation theory, as just summarized, have been misapplied in the literature connecting vertical pay disparity to firm performance. We describe their argument and its implications shortly.

Research that endeavors to explain the mixed empirical evidence of Table I (e.g., Rouen 2020, Balsam et al. 2020, Shin et al. 2015, Ridge et al. 2015) requires explaining both a positive and a negative sign on the key relationship. One approach is to simultaneously consider multiple measures of the independent variable, i.e., the pay gap, perhaps by decomposing it into two components whose partial correlations with firm performance exhibit opposite signs (e.g., Rouen 2020). Another approach allows for nonlinearity in the key relationship because an inverted *U* (or a *U*) exhibits both upward-sloping and downward-sloping portions, corresponding to the positive and negative relationships found in the literature.[7]

In either approach, researchers have appealed to different theories to explain a positive relationship versus a negative one. For example, each of Rouen (2020), Balsam et al. (2020), Ridge et al. (2015), and Shin et al. (2015) invoke one theory to explain a positive relationship and another to explain a negative relationship. Rouen (2020) decomposes the pay ratio into two components (one that relates to economic factors and another that does not). The one that relates to economic factors exhibits a positive relationship with firm performance, and the other exhibits a negative relationship. Rouen interprets the positive correlation as supportive of tournament theory and the

[7] In addition to the present study, examples of the latter approach are Ridge et al. (2015), which hypothesizes and finds a *U*-shaped relationship, and Shin et al. (2015), Burns et al. (2017), and Balsam et al. (2020), which find an inverted *U*. Of the 14 countries analyzed in Burns et al. (2017), 5 (including the U.S.) exhibit an inverted *U*.

negative correlation as supportive of equity theory (Adams 1965). Ridge et al. (2015) hypothesize a *U*-shaped relationship, with the downward-sloping portion of the *U* (i.e., the negative sign in the left part of the graph) explained by social comparison theory and the upward-sloping portion of the *U* (i.e., the positive sign in the right part of the graph) explained by tournament theory.

Balsam et al. (2020) hypothesize an inverted-*U*-shaped relationship, with the upward-sloping portion of the curve (i.e., the positive sign) explained by tournament theory and the downward-sloping portion (i.e., the negative sign) explained by social comparison and equity theory.[8] Shin et al. (2015) find a negative main effect and zero quadratic effect, interpreting this as evidence inconsistent with tournament theory and supportive of behavioral theory. They prefer an additional estimated specification showing that deviations in either direction from a measure of "expected firm performance" tend to hurt firm performance, interpreting that result as consistent with an inverted *U* that supports both tournament theory (because of the upward-sloping portion of the curve) and behavioral theory (because of the downward-sloping portion).

The claim pervading Table I's studies is that tournament theory can only explain a *positive* relationship between vertical pay disparity and firm performance (see, e.g., Rouen 2020, Cheng et al. 2019, Burns et al. 2017, Connelly et al. 2016, Ridge et al. 2015, and Kale et al. 2009). We now evaluate that claim by reexamining tournament theory. In the classic tournament model of Lazear and Rosen (1981), a large pay difference between the CEO and a lower-ranked executive position serves as a monetary prize, or "spread", that motivates the executives to work hard, in hopes of winning the promotion to the CEO's position. The larger the prize, the greater the effort that the executives expend, and therefore the higher is their performance.

[8] Cheng et al. (2019) also allow for a nonlinear relationship and find an inverted *U* for ROA; it is nearly a monotonic, concave relationship given that it is downward sloping only beyond the 99th percentile of the sample distribution. For comparison, the pay ratio at the peak of the inverted *U* in Balsam et al. (2020) has values ranging from 310 to 453, all of which are beyond the 75th percentile for the pay ratio but not as high as in Cheng et al. (2019).

Tournament theory does not, however, predict that increases in the pay gap cause increases in *firm performance*. Rather, it predicts that increases in the pay gap cause increases in *executive effort*. Higher effort does not always translate into higher firm performance because higher effort also comes with costs (such as higher compensation costs) that reduce firm performance (Lazear and Rosen, 1981). Up to a point, the greater effort resulting from a larger pay gap increases revenue to a degree that overwhelms the higher compensation costs, so firm performance increases. But raising the pay gap beyond that point implies an incremental increase in firm revenues that is outweighed by the increase in compensation costs, so firm performance *decreases*.[9] Thus, tournament theory implies an inverted-*U*-shaped relationship between the pay gap and firm performance.

One implication is that a negative sign should not automatically be interpreted, as it has been to date, as inconsistent with tournament theory and favoring social comparison and equity theory. Moreover, despite the custom in the literature, separate theories are not needed to explain the different signs of the relationship. It is not necessary to switch to a different theory when moving from the upward-sloping portion of an inverted *U* to the downward-sloping portion, because tournament theory itself predicts an inverted *U*, as stated in H1.

H1 [Tournament Incentives]: *The relationship between firm performance and vertical pay disparity exhibits an inverted U.*

[9] Intuitively, executives can be incentivized to work harder by increasing the size of the compensation prize they would win from a promotion. But the harder they work, the more exhausted they become, and the more expensive it becomes (in terms of increased compensation costs and prize value) to extract even more effort from them. Other mechanisms can also be present. For example, the CEO's hubris may be increasing in the levels of CEO compensation and pay inequality, and the CEO may behave unethically, reducing firm performance to the shareholders' detriment.

Empirical evidence consistent with H1 does not prove that tournament theory alone is the mechanism generating this result. Another interpretation, consistent with the arguments in the prior literature, is that tournament theory explains the positive relationship (on the left of the inverted *U*) and behavioral theories explain the negative relationship (on the right of the inverted *U*).[10] Distinguishing empirically between those different interpretations of the inverted *U* is beyond the scope of this study. However, two considerations make the interpretation based only on tournament theory appealing relative to the interpretation that switches from tournament theory to behavioral theory when the inverted *U* changes slope.

The first consideration is parsimony, i.e., when there exists a pattern of evidence consisting of multiple pieces, it is appealing to have one unified theory that can simultaneously explain all pieces, as opposed to invoking different theories to explain each individual piece.[11] The second consideration is an argument presented in Ridge et al. (2015) that questions the application of behavioral theory to explain the negative relationship on the right side of the inverted *U*. Those authors argue that the literature of Table I has "overlooked fundamental theoretical aspects" when applying behavioral theory to explain the negative sign. They note that, "the core proposition in Festinger's (1954) social comparison theory is that individuals will compare themselves with similar others (Wood, 1989). This "similarity hypothesis" is not acknowledged by applications of social comparison theory to the relationship between pay disparity and firm performance. (p. 620)"

According to that argument, the pay ratio influences executives' perceptions of similarity between themselves and the CEO. The larger the ratio, the less similar to the CEO the executives perceive themselves to be, and the less they expect pay equity and resent pay disparity. Executives

[10] Alternatively, tournament theory explains the inverted *U*, but behavioral theories are simultaneously present and reinforce the downward sloping part to the right of the inflection point.

[11] The point about parsimony should not be over-interpreted, i.e., it is not a claim that behavioral theories are irrelevant for explaining the downward-sloping part of the inverted *U*.

perceive a large pay gap as reflecting differences in power, status, and performance, all of which justify a large pay gap in executives' minds and undermine the relevance of the behavioral theories in explaining the negative correlation. On this basis, Ridge et al. (2015) argue that behavioral theories are most relevant in explaining the relationship of interest when the pay gap is *low*. Empirically, they find a *U*-shaped relationship in which social comparison theory is used to explain the downward-sloping "left portion" of the curve that applies when pay disparity is low. In contrast, other studies from Table I, like Shin (2015) and Balsam et al. (2020), appeal to behavioral theories to explain the downward-sloping "right portion" of an inverted *U*, that applies precisely when the pay gap is *high*, meaning that these theories are *least likely to be relevant* according to the argument in Ridge et al. (2015). One limitation of this argument is that the factors that contribute to "similarity" are not clearly identified.

The tournament mechanism for the relationship between the pay gap and firm performance is based on the incentives of the executives who aspire to become CEO. Firm performance changes with the pay gap because changes in the pay gap alter the incentives of those executives to exert effort. An alternative mechanism is ability-based CEO selection across firms. Firms interested in "upgrading" to hire a higher-ability CEO may set higher CEO pay, which tends to increase the executive pay gap. Then, when those high-ability CEOs assume leadership, firm performance increases because of their ability rather than because executives' stronger incentives.

The importance of ability-based CEO selection for firm profitability is highlighted in the theoretical analysis of Tsoulouhas, Knoeber, and Agrawal (2007), which focuses on the following tradeoff. Handicapping tournaments in favor of insiders yields strong internal executive incentives at the expense of selecting the most able CEO.[12] Reducing or eliminating the handicap improves

[12] This point is also central to Agrawal, Knoeber, and Tsoulouhas (2006). For other studies on ability-based selection of executive talent, see Goel and Thakor (2008), Campbell et al. (2014), Cragun, Nyberg, and Wright (2016), Kaplan and Sorensen (2016), Quigley et al. (2018), Abernethy et al. (2019), and Li and Tong (2022).

the expected ability of the new CEO (because the broader pool of external candidates may be expected to yield a more able CEO) but at the expense of weakening internal incentives. Conditions are identified under which certain handicapping schemes are more profitable. When inside candidates are nearly as good as outsiders (or maybe even better) it is optimal to handicap outsiders, thereby strongly incentivizing internal executives. But when external candidates are sufficiently more able than insiders, it is optimal to handicap insiders to reap the full benefit of ability-based CEO selection. Cziraki and Jenter (2022) find evidence that externally hired CEOs are more expensive than internally promoted CEOs. One interpretation they offer is that external CEOs have more transferable human capital, which is consistent with them having higher ability levels.

These mechanisms (i.e., tournament incentives, and ability-based selection) are not mutually exclusive and can co-exist with differing relative magnitudes. They yield the following hypothesis, which conditions on H1 being satisfied:

> **H2 [Tournament Incentives + Ability-based Selection]**: *Ability-based selection of CEOs into firms shifts the location and peak of the inverted-U-shaped relationship between firm performance and vertical pay disparity.*

The selection mechanism requires explanation because there are two ways to increase the pay spread (i.e., by increasing the CEO's pay or by lowering the pay of the non-CEO executives). If the latter approach is used, the CEO's pay might not be particularly high, in which case firms' "upgrading" strategy will not work. Two considerations mitigate this concern. First, boards of directors are limited in their ability to reduce the pay of non-CEO executives because those workers would quit to join other firms that offer market competitive pay. That market-imposed constraint only pertains to lowering non-CEO executives' pay and not to raising CEO pay. This suggests that bigger pay spreads will most often be achieved by raising CEO pay. Second, there is evidence that workers care about relative comparisons (e.g., Frey et al. 2013 and the behavioral theory citations provided in section II). That is, workers care not just about their absolute reward but also about their "rank". Thus, workers like being the top-paid person in the organization and may prefer it to being the fifth highest paid in another organization, even if their absolute pay is higher in the latter case. This consideration encourages CEOs to select into organizations with large executive pay gaps even if the CEO pay itself is not extraordinary.

III. Methodological Pitfalls

The literature in Table I has not yielded a conclusive answer to the research question. When confronted with mixed results on an important empirical question, it is appropriate to consider methodological issues with an eye towards improving future estimation attempts so that a clearer picture may emerge. We now highlight some methodological pitfalls to be avoided in future research and that we avoid or mitigate in our forthcoming empirical analysis.

The biggest methodological challenge is endogeneity of the executive pay gap in an empirical model of firm performance. Further amplifying this challenge is that endogeneity can be expected from three distinct sources: omitted variables, measurement errors in the measure of executive pay disparity, and reverse causality. Some of Table I's studies ignore endogeneity altogether. Others acknowledge the endogeneity problem but attempt to address it using methods that do not address it (e.g., by adopting instrument-based methods using invalid instruments) and that, in some cases, worsen it. Our approach mitigates endogeneity to a greater extent than the prior literature. We also highlight common mistakes to avoid that can worsen endogeneity problems in future research.

Endogeneity in this setting is of two types: "natural" (i.e., inherently present due to the nature of the problem) and "researcher induced". The latter is considerably easier to address because the solution is simply to avoid the methodological practices that induce endogeneity. We discuss this type of endogeneity first, highlighting three types of researcher-induced endogeneity that affect Table I. Natural endogeneity, which we discuss second, is harder to mitigate in this setting, due in part to the considerable breadth of the dependent variable.

A. Researcher-Induced Endogeneity

One source of researcher-induced endogeneity that afflicts some of the studies in Table I involves including lagged dependent variables and/or alternative measures of the dependent variable (lagged or contemporaneous) on the empirical model's right-hand side. Table I reveals that these practices are commonly adopted by researchers in the name of endogeneity mitigation even though they worsen, rather than mitigate, endogeneity. The reason is that the regression's composite error term is correlated with the lagged dependent variable or with the alternative measures of the dependent variable (lagged or contemporaneous). Section A3 of the online appendix elaborates. Fortunately, this type of endogeneity is easily avoided simply by refraining from those methodological practices, as we do in the forthcoming empirical work.

A second source of researcher-induced endogeneity arises from the widespread practice of winsorizing the pay gap. Measurement error in the independent variable is a well-known source of endogeneity bias, and winsorizing the pay gap amounts to researcher-induced measurement error.

The common practice of winsorizing all continuous variables on both ends, typically at 1% and 99% but sometimes even at 5% and 95%, thwarts the identification of the relationship of interest by throwing away some of the most valuable variation in the extremes of the pay distributions, particularly the right tail of the executive pay distribution that is needed to identify an inverted-*U*-shaped relationship based on data on either side of its peak. The biases introduced by this practice can be severe, as we show in a Monte Carlo analysis in section A1 of the online appendix; winsorizing at the top of the distributions is revealed to be particularly damaging. This is a rare instance in which the researcher knows the exact form of the measurement error (and can therefore measure its adverse consequences exactly) because the errors are induced by the researchers themselves.

In lieu of blanket winsorizing, alternative means of assuring data quality are recommended. Ideally, this would involve close examination of the tails of the executive pay distribution, when sample sizes are not prohibitively large. We take this approach in section A6 ("Analysis of Outliers") of the online appendix, which reports the top 100 observations of the executive pay ratio. We manually check the cases of the extreme (highest) values of compensation against the original source to ensure accuracy, finding close alignment. Online appendix section A6 elaborates.

A third form of researcher-induced endogeneity concerns omitted variable bias from improperly incorporating (or omitting) fixed effects. The correct treatment of fixed effects in the empirical specification must derive from the underlying theoretical framework. In the present study and most others in Table I, that framework is tournament theory, which requires the inclusion of CEO-firm fixed effects (not firm fixed effects). The problem with firm fixed effects is that some of the variation in the pay gap that identifies the relationship of interest comes from the firm switching CEOs. This is problematic because changes in a firm's leadership are accompanied by a host of changes (reflected in the regression's error term) that can affect firm value directly rather than indirectly via affecting the pay gap. Correlation between the composite error term and the pay gap then creates endogeneity bias.

Including CEO-firm fixed effects eliminates this problem. None of the studies in Table I incorporates CEO-firm fixed effects. Some include firm fixed effects, and others include no fixed effects. The result is researcher-induced endogeneity in the form of omitted variable bias. The present study's use of CEO-firm fixed effects eliminates that problem.

While inappropriate for testing the predictions of tournament theory, models with firm fixed effects are still useful. Specifically, comparing the results of models that have CEO-firm fixed effects with those that have firm fixed effects reveals whether ability-based CEO selection across firms drives the relationship of interest or whether executive incentives also play a role. That comparison yields this study's result that executive incentives indeed matter.

B. Natural Endogeneity

The sources of endogeneity just described are potentially serious but permit straightforward solutions. Given that they are researcher induced, simply avoiding the relevant methodological practices solves the problem. Natural endogeneity, in contrast, is not researcher induced and is considerably harder to fix. In section IV.D, we introduce an instrument for vertical pay disparity that is less prone to the limitations of the instruments in prior work, though still not immune to criticism.

IV. Empirical Analysis

This section empirically investigates the theoretical hypotheses in section II, using improved measures and empirical models that address the limitations briefly mentioned in section III and discussed in detail in the online appendix.

A. Empirical Specifications

Letting i, j, and t index firms, CEO-firm matches, and years, respectively, two main empirical specifications (differing notationally in their indexes) are estimated to address H1, H2a, and H2b:

$$y_{it} = \delta_1 ExecPayRatio_{i,t\text{-}1} + \delta_2 (ExecPayRatio_{i,t\text{-}1})^2 + \boldsymbol{\beta}'\mathbf{x}_{it} + \alpha_i + \varepsilon_{it} \quad (4.1)$$

$$y_{jt} = \delta_1 ExecPayRatio_{j,t\text{-}1} + \delta_2 (ExecPayRatio_{j,t\text{-}1})^2 + \boldsymbol{\beta}'\mathbf{x}_{jt} + \alpha_j + \varepsilon_{jt} \quad (4.2)$$

where y is industry-adjusted firm performance (either Tobin's q or *ROA*),[13] and *ExecPayRatio* is the ratio of CEO pay to the average pay of the firm's other top executives. The index t runs from 1 to T, the index i runs from 1 to N, and the index j runs from 1 to N^*, where $N^* \geq N$.

Equation (4.1) differs from the typical specification in Table I in that it omits the lagged dependent variable and alternative measures of the dependent variable (both contemporaneous and lagged) from the right-hand side, for reasons noted in section III and discussed in greater detail in the online appendix (section A3). Equation (4.2) differs from equation (4.1) in that the subscript i (indexing the N firms) is replaced by j (indexing the N^* CEO-firm matches).[14] Thus, α_i in (4.1) is a firm fixed effect, whereas α_j in (4.2) is a CEO-firm fixed effect. The vector of controls, $\boldsymbol{x}$, appearing in both models includes year dummies, leverage (*lev*), firm age (*firmage*), firm size (*firmsize*), capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D), expense-to-sales ratio (*rdsale*), a dummy variable indicating missing observations on R&D (*rddum*), and industry concentration, as measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the industry classifications based on the Standard Industrial Classification (SIC) 2-digit code.[15]

These control variables are included for the following reasons. Firm performance is related to a firm's fundamental characteristics, such as firm size (see, e.g., Bebchuk et al. 2011) and firm maturity (or firm age) as well as industry structure (e.g., industry concentration). Firm performance

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[13] Our reading of the empirical literatures across multiple disciplines is that industry-adjusting the dependent variable of firm performance is widely seen as a best practice. See, for example, the following studies from the accounting, economics, and finance literatures that use industry-adjusted Tobin's Q and/or industry-adjusted ROA: Vafeas (1999), Klein (2001), Engel, Hayes, and Wang (2003), Rajgopal, Shevlin, and Zamora (2006), and Bebchuk, Cremers, and Peyer (2011), among others. We industry-adjust our dependent variables of firm performance because we do not believe it is harmful to our analysis, and we lack a strong and compelling reason for deviating from the literature on this dimension. Unreported analysis reveals that our main results concerning the "inverted *U*" hold without the industry adjustment.

[14] $N^* \geq N$ because there are more CEO-firm matches than firms. The inequality is strict if at least one firm has at least two different CEOs over the T years in the sample.

[15] The model includes industry dummies (based on the Fama and French 12-industry classifications) when it is not estimated with firm fixed effects or CEO-firm fixed effects.

also reflects major corporate and financial decisions. Corporate financing decisions, such as financial leverage, have implications for firm value to shareholders (see, e.g., Campello, 2006). Similarly, corporate investment decisions, such as capital expenditure and R&D, are associated with changes in firm value or performance (see, e.g., McConnell and Muscarella, 1985, Maxwell and Siddique, 2004, 2008).

The results on control variables are highlighted and discussed in section IV.C. The parameters of interest are δ_1 and δ_2. An inverted *U* implies $\delta_1 > 0$ and $\delta_2 < 0$ in equations like (4.1) and (4.2), but the type of fixed effect included in the specification determines the interpretation. Tournament theory (H1) concerns incentive effects, which is captured in equation (4.2) because the CEO-firm fixed effect holds the CEO-firm match constant and eliminates sorting of CEOs across firms. Thus, evidence of $\delta_1 > 0$ and $\delta_2 < 0$ in (4.2) would support H1. Equation (4.1) includes firm fixed effects, thereby allowing for CEO sorting across firms in addition to tournament incentives. Evaluating H2 requires comparing the results from equations (4.1) and (4.2). A larger quantitative effect is expected in (4.1) than in (4.2) if sorting matters in addition to incentives. Estimates of δ_1 and δ_2 in equation (4.1) that satisfy $\delta_1 > 0$ and $\delta_2 < 0$, and that differ significantly from the corresponding coefficients in (4.2), would support H2. In contrast, evidence that the estimates of δ_1 and δ_2 are similar between equations (4.1) and (4.2) would be inconsistent with H2. The conclusion in that case would be that the same quantitative result is obtained with or without CEO sorting across firms, which implies that the result is due to incentives and not sorting.

Statistical power must be considered, however, when interpreting H2. If the statistical test of the null hypothesis that δ_1 and δ_2 in equation (4.1) are equal to their counterparts in equation (4.1) has low power, then the null will be difficult to reject even if false. The test of H2 should therefore be interpreted with some degree of caution.

B. Data, Measures, and Sample Selection

Compensation data for CEOs and other executives in both current and previous S&P1500 firms are obtained from the ExecuComp database, which started in 1992. All variables used in the analysis are defined in Table II. CEO pay and the average pay of the firm's other executives are denoted *CEOpay* and *AvExecPay,* respectively. As Table I reveals, the most common measure of vertical pay disparity is a pay ratio. Following the literature, we use *CEOpay*/*AvExecPay*, which is denoted by *ExecPayRatio*, to measure vertical pay disparity among executives.[16] The studies in Table I (see column 6) typically use a pay ratio that winsorizes the pay ratio at both ends, whereas we use the unwinsorized ratio. Winsorizing introduces endogeneity by corrupting the independent variables with measurement error, as shown in section A1 of the online appendix.

Two measures of firm performance are used. One is the firm's market valuation (i.e., industry-adjusted Tobin's *q*), and the other is operating performance (i.e., industry-adjusted return on assets), denoted *ROA*. Tobin's *q* and *ROA* capture different dimensions of firm performance. Tobin's *q* is a measure of stock market valuation, i.e., the market performance of the firm, which is used in the finance literature as a proxy for firm value (market value). It is a forward-looking measure of firm performance given that it is computed using forward-looking stock market value. *ROA* measures operating performance and firm profitability. Both Tobin's *q* and *ROA* are industry-adjusted to measure the firm's performance relative to the industry. The industry-adjusted Tobin's *q* is the firm's Tobin's *q* ratio minus the industry median based on the Fama and French 12-industry classifications. The industry median is computed for each year in the sample. Similarly, industry-adjusted return on assets (*ROA*) is the firm's ROA minus the industry median computed for each year.

[16] Connelly et al. (2016) note that "Pay dispersion has become the cornerstone of tournament theory and is frequently operationalized as a ratio (Gupta et al., 2012)." Section A7 of the online appendix considers an alternative measure of the executive pay gap, namely the pay difference rather than the pay ratio.

Along with the firm performance measures, other firm characteristics such as total assets, net sales, and leverage are obtained from the merged CRSP/COMPUSTAT database. Firm and industry characteristics that appear as controls in the empirical models are: leverage, firm age, firm size, capital expenditure, research and development expenditure, and industry concentration. The financial and utility industries are excluded from the sample because these industries are often subject to unique regulations and characteristics, as well as different governance and compensation structures.[17]

Some studies in Table I (see column 3) "industry adjust" the dependent and independent variables, a decision that is made in the name of mitigating endogeneity from omitted (industry) variables (see, e.g., Cheng et al. 2019). Our analysis includes CEO-firm fixed effects, which hold the CEO-firm match (which includes industry) constant, so omitted industry effects do not contaminate the estimated relationship of interest, and neither do any other unobserved variables that are time-invariant throughout the duration of a CEO-firm employment relationship. Nonetheless, for consistency with both the literature in Table I and the broader literatures in accounting, economics, and finance, we industry-adjust the dependent variables.[18]

The analysis sample, which is derived in section A6 of the online appendix, contains 37,241 firm-year observations for 2918 unique firms from 1992 to 2019.[19] Table III displays descriptive statistics for all variables in the analysis. The average annual CEO compensation is $5.48 million,

[17] See, for example, Joskow, Rose, and Shepard (1993) for regulatory oversight and political constraints on the structure and level of CEO compensation in the utility industry, and Fama and French (1992) and Foerster and Sapp (2005) for the unique characteristics (e.g., greater leverage and sensitivity to financial risks) of the financial industry. See also Yermack (1996), whose study excludes financial and utility firms due to concerns that government regulation leads to different, more limited roles of the boards.
[18] Footnote 13 gives reasons using industry-adjusted dependent variables. In the panel regression model with fixed effects, the industry fixed effects are dropped.
[19] The final sample sizes reported in the tables are smaller as they require non-missing values in both the dependent and control variables.

that for non-CEO executive compensation is \$1.80 million, and the mean *ExecPayRatio* is 3.07-to-1.

C. Results

Table IV displays estimation results from empirical models (4.1) and (4.2). Panel A displays the preferred results based on the unwinsorized pay ratio, Panel B displays results that winsorize both tails of the pay ratio, and Panel C displays results that winsorize only the lower tail of the pay ratio. Given that H1 is a directional hypothesis, one-tailed hypothesis tests are used.[20] In Table IV, models 3 and 6 of Panel A provide the test of H1. An inverted *U* is identified in model 3 of Panel A for Tobin's *q*, with $\delta_1 > 0$ and $\delta_2 < 0$, supporting H1. Figure 1 shows these results graphically, indicating the peak of the inverted *U* with a vertical line at 70.46.

This support for H1 vanishes, however, in model 3 of Panel B. The coefficients become statistically insignificant when the pay ratio is winsorized. The results in model 3 of Panel B illustrate the destructive effect of winsorizing the right tail of the pay ratio. When winsorization is only applied to the left tail, as in Mueller et al. (2017), the results are very similar, qualitatively and quantitatively, to the unwinsorized results in Panel A.

Turning to *ROA*, the point estimates in Panel A's model 6 do not reveal an inverted *U* because the coefficient on the quadratic term is positive and statistically insignificant. Panel B's model 6, however, does reveal an inverted *U*. Although this result ostensibly supports H1, this is an artifact of discarding legitimate and valuable information from the extremes of the pay distributions. The results from Table IV for both dependent variables reveal that the damage to the preferred results from Panel A is driven by winsorization of the right tail of the pay ratio (which

[20] The conclusions drawn concerning H1 are virtually identical at conventional significance levels if two-tailed tests are used for inferences, given that in most cases the parameters of interest are estimated with high precision.

provides relevant information concerning extremely high CEO pay), given that the results are very similar between Panels A and C.

To summarize the results for H1, in models 3 and 6 of Table IV's Panel A the results are consistent with tournament theory but only for Tobin's *q* (which is a proxy for firm value and market-based measure of firm performance) and not for *ROA* (which is a returns-based measure of firm performance). The results in models 1 and 4 (no fixed effects) and in models 2 and 5 (firm fixed effects) suffer from endogeneity induced by omitted variable bias. Nonetheless, for a given dependent variable, comparing the estimated inflection points across the three models, and comparing Panels A and B, reveals that this endogeneity problem from omitted variables is less destructive to the preferred results than is the endogeneity from measurement error induced by routine winsorization of the pay variables. Finally, even though an inverted *U* is empirically detected, its inflection point lies so far to the right that the downward-sloping portion of the relationship lacks practical relevance.

Evaluating H2 for Tobin's *q* requires comparing models 2 and 3 in Table IV's Panel A. Three chi-square statistical tests are performed on the differences in coefficients between the models with firm fixed effects and the models with CEO-firm fixed effects. The first is a test of equality of the coefficients of *ExecPayRatio*, i.e., δ_1, between models 2 and 3. The second is a test of equality of the coefficients of $ExecPayRatio^2$, i.e., δ_2, between models 2 and 3. The third is a joint test of the preceding two null hypotheses, i.e., that the *ExecPayRatio* coefficients are equal and that the $ExecPayRatio^2$ coefficients are equal. The results in Table IV's Panel A reveal that the null hypothesis of equal coefficients between models 2 and 3 cannot be rejected at conventional levels. That is true for the coefficient of *ExecPayRatio*, for the coefficient of $ExecPayRatio^2$, and for the two coefficients considered jointly. This leads us to reject H2 and conclude that the inverted *U* is driven by executive incentives rather than by CEO sorting across firms, though as previously noted, the possibility of low statistical power suggests caution in concluding that sorting does not matter.

Evaluating H2 in the case of *ROA* follows the same approach and compares models 5 and 6. The results in Table IV's Panel B (based on standard winsorization) reveal that the null hypothesis of equal coefficients of *ExecPayRatio* between equations (4.1) and (4.2) is rejected at the 5% level, whether the comparison is between models 2 and 3 or between models 5 and 6. The null hypothesis of equal coefficients of $ExecPayRatio^2$ between the two equations is rejected at the 10% level, whether the comparison is between models 2 and 3 or between models 5 and 6. Thus, when statistical tests are performed on the difference in coefficients individually, the winsorized results in Panel B lead to different qualitative inferences being drawn than when using the preferred results in Panel A. When the joint hypothesis is considered, equality of the *ExecPayRatio* coefficients and of the $ExecPayRatio^2$ coefficients cannot be rejected at conventional levels. Although in this case the correct qualitative inference would be drawn as in Panel A, the *p*-values for this joint test are smaller in Panel B than in Panel A (i.e., 0.146 and 0.135, versus 0.486 and 0.562).

Some of the control variables in the models reported in Table IV have statistically significant effects that are worthy of comment. First, firm size has a negative coefficient, suggesting that larger firms do not have a high firm valuation or performance. This result is consistent with prior work. For example, Table 3 of Bebchuk et al. (2011) shows that firm size (measured as the log of book value of assets) has a negative coefficient in a model of industry-adjusted Tobin's Q. Second, our regressions reveal a negative effect of leverage. While Campello (2006) shows that debt can both enhance or hinder a firm's performance, our result suggests that overall, higher debt is associated with underperformance. Our finding of a negative relationship between leverage and Tobin's Q (a proxy for shareholder value) is consistent with the finding by

D'Mello, Gruskin, and Kulchania (2018) that on average, shareholders view firms to be overleveraged and increasing debt has a negative impact on shareholders' wealth. Third, the positive coefficient on capital expenditure is consistent with the signaling theory by Trueman (1986), wherein firms use the level of investment in a project to signal their private favorable information about the project to other firms. Our result is also consistent with the empirical finding by McConnell and Muscarella (1985) that increases in planned capital expenditures are associated with a higher market value of the firm. Fourth, the positive R&D coefficient (see models 1 to 3 in Panel A) is consistent with Eberhart, Maxwell, and Siddique (2004, 2008), which show that R&D increases are associated with improvements in firm value or market valuation. Nevertheless, the R&D coefficient becomes negative when using industry-adjusted ROA as the dependent variable (see also Section V for further analysis of missing R&D values).

D. Endogeneity of the Pay Ratio

A potential concern in this literature is endogeneity of the vertical pay gap, i.e., correlation between the measure of vertical pay disparity and the regression's disturbance term. Instrumental variables estimation, which we undertake, has the potential to address the natural endogeneity that occurs in this problem. But prior attempts at this approach have used instrumental variables that are generally invalid (see section A4 of the online appendix). A credible instrument must meet two criteria (relevance and validity), but most studies in Table I that use instruments only discuss and establish relevance without even mentioning validity. The most credible attempt to date uses instruments proposed in Kale et al. (2009) and repeated in Burns et al. (2017). Those studies instrument for a firm's pay ratio using the median pay ratio in that firm's industry (and quartile of the firm size distribution).

We propose the "counterfactual pay ratio" as an instrument for *ExecPayRatio* that improves on that of Kale et al. (2009). Consider two firms (*A* and *B*) and the problem of finding a credible instrument for firm *A*'s *ExecPayRatio*. Firms *A* and *B* are "connected" in the sense that there is reason to believe that the pay of firm *A*'s CEO is influenced by the executive compensation policies at firm *B*, and vice versa, as might happen if either CEO sits (or has sat) on the board of directors at the other firm. Such cross pollination can be expected to stimulate learning and information sharing between the two firms concerning CEO pay practices.

Define the counterfactual pay ratio for firm *A* as the ratio of the CEO's pay in firm *B* to the average pay of firm *A*'s non-CEO executives. It is called "counterfactual" because it is not an actual measure of vertical pay dispersion in either firm. But it should be positively correlated with firm *A*'s actual pay ratio for two reasons. First, firm *A*'s actual and counterfactual pay ratios both share the same denominator; an increase in that common denominator would cause both ratios to shrink, inducing a positive correlation between them. A second reason to expect correlation is the aforementioned "cross pollination" between the two firms. The preceding arguments establish the instrument's relevance. Validity is satisfied because there is little reason to expect a measure of vertical pay disparity that spans two firms (i.e., that compares the pay of one firm's CEO to the average pay of another firm's non-CEO executives) to have an important direct effect on the first firm's performance. One challenge to the validity criterion, however, is discussed below.

The instrument in Kale et al. (2009) and Burns et al. (2017) is also "counterfactual" in the sense that it instruments for a particular firm's pay ratio using a pay ratio derived from other firms (specifically the median in that firm's industry and size quartile).[21] However, our proposed instrument has some appealing features that the earlier ones lack. Rather than just coarsely

[21] Section A4 of the online appendix discusses the instruments in the previous literature. For example, several studies in Table I attempt to address the endogeneity of the pay gap using instrument-based approaches like two-stage least squares (2SLS). These instruments are generally invalid because they do not satisfy the crucial exclusion restriction (i.e., no explanation is given for why the instrument should not appear in the equation for firm performance). Kale et al. (2009) and Burns et al. (2017) use more defensible (i.e., valid) instruments than those used in the other studies in Table I.

matching a firm to the median of an entire broad cell based on industry and size quartile, it matches each firm in the data to a particular companion firm that is directly "connected" to the first firm in a way that would facilitate information exchange relevant to the determination of executive compensation. This connection can be expected to enhance the relevance of the instrument. A further implication is that the earlier instrument exhibits limited variation (across, but not within, large cells defined by two-digit industry and firm size quartile) whereas our instrument yields firm-level variation both within and across those cells.

Our instrument is defined as follows:

$$\textit{Counterfactual pay ratio} = \frac{\text{CEO total pay in firm } B}{\text{average non}-\text{CEO executive pay in firm } A}$$

Constructing the instrument based on the closest industry match accentuates the cross pollination (in terms of information sharing) between firm *A* and firm(s) *B*. The challenge in constructing this ratio is choosing an appropriate "firm *B*" to match to firm *A*. To make this selection and, therefore, define the numerator in the preceding ratio, we apply the following criteria:

I. If exactly one CEO sits on firm *A*'s board of directors who leads a firm that is in the same SIC code as firm *A*, that CEO's total pay is used as the numerator. If there are multiple CEOs that sit on firm *A*'s board of directors and lead firms that are in the same SIC code as firm *A*, the average total pay of these CEOs is used as the numerator.

II. Suppose that criterion I fails. If exactly one CEO sits on firm *A*'s board of directors and leads a firm that matches firm *A*'s SIC code to the first three digits, that CEO's total pay is used as the numerator. If there are multiple CEOs that sit on firm *A*'s board of directors and lead firms that match firm *A*'s SIC code to the first three digits, the average total pay of these CEOs is used as the numerator.

III. Suppose that criteria I and II fail. If exactly one CEO sits on firm *A*'s board of directors and leads a firm that matches firm *A*'s SIC code to the first two digits, that CEO's total pay is used as the numerator. If there are multiple CEOs that sit on firm *A*'s board of directors and lead firms that match firm *A*'s SIC code to the first two digits, the average total pay of these CEOs is used as the numerator.

IV. Suppose that criteria I, II, and III fail, meaning that no CEO who sits on firm *A*'s board leads a firm that can be matched even to just the first two digits with firm *A*'s SIC code. The average total pay of all CEOs who sit on firm *A*'s board is then used as the numerator.

As a robustness check, we also use as the numerator the equal weighted average CEO total pay across all members of firm *A*'s board of directors who are CEOs in other firms, regardless of the industry.[22] The (unreported) analysis based on this alternative measure shows similar results and the same conclusion.

A subsample of firms is used for which there is no CEO duality problem – i.e., these firms do not have CEOs who are also the chairperson of the board, a scenario which may allow the CEO to have undue influence over his/her own pay (Bebchuk and Fried 2006). Furthermore, the model is overidentified given that other related instruments are also included in the instrument set.[23]

Although our proposed instrument improves over the earlier one, it has three limitations. First, not every "firm *A*" in the data can be matched to another "firm *B*", so there is a reduction in the sample size. Second, the correlation between the actual and counterfactual pay ratios, while positive, is not as strong as desired. However, the results from the first-stage regressions in the generalized method of moments (GMM) estimation reveal that the instruments are statistically

[22] This alternative measure is identical to criterion IV for cases in which firm *A* fails criteria I through III.

[23] Murray (2006) argues for enhancing the credibility of instrumental variable estimates by including multiple (alternative) instruments in an analysis.

significant in predicting the instrumented variables of *ExecPayRatio* and its square. The first-stage regressions, reported in Table A5 of the online appendix, reveal a statistically significant coefficient of the instruments on predicting *ExecPayRatio* and its square. Third, a challenge to the instrument's validity is that a "connection" between two firms could mean that a firm's non-CEO executives are aware of the executive compensation structure within the matched firm. This knowledge of cross-company pay disparity could result in demotivation and lower performance for non-CEO executives even if the channel is not through pay disparity within their company.

Two alternative instruments are included that may stimulate learning and information sharing (concerning CEO pay practices) between firm *A* and the other firm(s) matched to it in the counterfactual pay ratio. "CEO-board of director" denotes a board member in firm *A* who is also the CEO in the firm that is matched to firm *A*. Additional instrumental variables are included that proxy for the board of director's strength of influence as a CEO in her/his own company and the board of director's experience in the sample industry. The first additional instrumental variable, *%Chairman*, with results reported in Table A4 of the online appendix, is computed as follows:

$$\%Chairman = \frac{\text{Number of members of firm A's board of directors who are both CEOs and the Chairman of the board}}{\text{Number of members of firm A's board of directors who are also CEOs}}$$

The second additional instrumental variable, *%Same_Industry*, with results reported in Table A5 of the online appendix, is computed as follows:

$$\%Same\_Industry = \frac{\text{Number of members of firm A's board of directors who are also CEOs in the same or similary industry}}{\text{Number of members of firm A's board of directors who are also CEOs}}$$

where the numerator is the number of members of firm *A*'s board of directors who are also CEOs in an industry that matches firm *A*'s SIC code to at least the first three digits.

GMM estimation results for the executive pay ratio, analogous to those in Table IV, are displayed in Table VI. Panel A of Table VI provides a summary of the first-stage regression results, which overall support the significance of the instrumental variables. In addition, the Sargan-Hansen test statistic supports the validity of the instruments. In Panel A of Table VI, model 3 reveals that the model for Tobin's *q* survives the instrumental variable analysis, in that an inverted *U* is revealed, but only when fixed effects are incorporated in the theoretically preferred manner (i.e., CEO-firm fixed effects). Models 4 to 6 reveal that the models for *ROA* do not survive the instrumental variables analysis, in that the coefficients of interest are not statistically significant at conventional levels, even though the signs of the point estimates are as theoretically predicted.

Similar qualitative conclusions are found with respect to the additional (alternative) configurations of instruments in the online appendix Tables A4 and A5. Regardless of the configuration of instruments across the three panels, the *ROA* results are never statistically significant at conventional levels, despite always having the theoretically predicted signs. In the Tobin's *q* models, an inverted *U* is found and is statistically significant at conventional levels regardless of the configuration of instruments, when the preferred CEO-firm fixed effects are used. Collectively, the results suggest that the inverted-*U*-shaped relationship between executive pay gap and firm performance is observed mainly through Tobin's *q* (a proxy for firm value or market-based performance) rather than returns-based measure of firm performance such as ROA. Furthermore, the estimated inflection point – while considerably lower in the instrumental variables analysis (see, e.g., model 3 in Panels B of Table VI and Tables A4 and A5) than in Table IV – is still high (at 16).

V. Robustness Tests and Further Analysis

We perform several robustness tests and further analyses to ensure that our conclusions remain stable. First, given that we do not winsorize the data, we perform a close inspection of the data to detect any outliers that should be discarded. Outliers (before applying the selection criteria in Table A2) are at the bottom 1% (i.e., the lowest percentiles of the executive pay gap distribution). These outliers mainly reflect CEO pay that is either low (perhaps even zero) or less than that of the firm's non-executives. A close examination of every observation in the top 1% or bottom 1% of the pay gap distribution – after applying the selection criteria in Table A2 – raises no cause for concern. Regardless, we perform further robustness tests. First, we trim observations where the average executive pay is below $100,000. This trimming criterion drops 13 observations in our final sample (including the observations of COCA-COLA EUROPEAN PARTNERS and EBIX INC in Table A1). The regression analysis based on this trimmed sample produce similar results and the same conclusion (see Panel A of Table A6). Second, we perform an additional check by dropping any CEO-firm pair that has at least one observation violating one of the selection criteria in Table A2. We note that this trimming also eliminates some outliers of firms (e.g., Alphabet and Apple in Table A1) that appear in both the top 1% and bottom 1% executive pay gap distribution. Although this trimming has reduced our sample size by almost 20%, regression analysis based on this trimmed sample produce similar results and the same conclusion (see Panel B of Table A6).

Second, we address the concern that our inclusion of CEO-firm fixed effects, while holding constant the CEO-firm match, still allows for turnover among the non-executives, which could also change the nature of the tournament. The presence of multiple non-CEO executives within a firm makes it difficult to completely account for such a turnover, but we are taking a step in that direction by replacing CEO-firm fixed effects with CEO-COO-firm fixed effects.[24] The rationale is one of the most likely candidates to ascend to CEO when a vacancy arises, so a COO separation from the firm would be a consequential event for the tournament.[25] Table A7 shows that using CEO-COO-firm fixed effects yields results and conclusions matching our main analysis.[26]

Third, our sample contains missing R&D values. As a robustness check, we follow a hybrid approach recommended by Koh and Reeb (2015). This hybrid approach deals with the missing R&D by combining the use of a dummy variable for missing R&D with: (i) replacing missing R&D values with the industry average of reported R&D values; and (ii) using patent data to identify pseudo-blank firms (i.e., a dummy for blank firms with no patent filings).[27] Table A8 reveals that our findings remain qualitatively unchanged using the preceding approach for handling missing R&D values.

VI. Discussion and Implications

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[24] We identify the Chief Operating Officer (COO) based on the job titles provided in the ExecuComp database. Given that there are observations of multiple COOs for a given firm in a given fiscal year, we exclude those observations when computing the CEO-COO firm fixed effects. In an unreported robustness check, we include one of the multiple COOs to compute CEO-COO firm fixed effects, obtaining similar results and same conclusions.

[25] Examples: Cook's role immediately prior to becoming Apple's CEO was COO. Mark Fields' role immediately prior to becoming Ford Motor Company's CEO was COO. Randall Stephenson's role immediately prior to becoming AT&T's CEO was COO. Bernard Tyson's role immediately prior to becoming Kaiser Permanente's CEO was COO (and president). A McKinsey study analyzed about 600 CEOs, finding that about 85 percent of individuals who were promoted to CEO "had been operators—CEOs at other companies, leaders of major operating divisions, or chief operating officers." (Birshan, Meakin, and Strovink 2017)

[26] When companies promote functional (as opposed to operational) executives to the role of CEO, the individual is most commonly the CFO. An example is Safra Catz, who was CFO immediately before becoming Oracle's CEO. The aforementioned McKinsey study finds that although fewer than 15 percent of CEOs were promoted into that role from a functional executive position, on those occasions the individual was the CFO about two thirds of the time. The study notes that "companies undertaking a growth plan based on M&A or a major cost-reduction effort often look to CFOs [to fill CEO vacancies]. As a further robustness check, we included CEO-CFO-firm fixed effects using the ExecuComp database, which provides an explicit identifier of executives who served as Chief Financial Officer (CFO) in a given fiscal year. Our results are largely unchanged and are available upon request.

[27] We follow the recommendation from Koh and Reeb (2015) and add a new control variable of missing R&D using patents (using patent data from the WRDS US Patents databases) and replacing missing R&D values with the industry mean. The argument is that if a firm does not report R&D but their industry peers do, then the industry median R&D can pick up the missing information. Although the patent data from WRDS are mostly observed in the years after 2011, we use patent filings (instead of patent grants) to identify pseudo-blank firms (i.e., firms with no patent filings).

The empirical result that the relationship between firm performance and the executive pay gap is an inverted *U* holds whether the fixed effects are for the firm or (more appropriately in the case of tournament theory) for the CEO-firm. That picture changes, however, when the model accounts for endogeneity. Specifically, in the instrumental variables analysis, the inverted *U* only emerges in the presence of CEO-firm fixed effects, which are preferred on theoretical grounds. When firm performance is measured as ROA, the evidence of an inverted *U* is weaker, emerging in some specifications but not in others. Specifically, it emerges when vertical pay disparity is measured (as it is in section A7 and Table A3 of the online appendix) as a difference rather than as a ratio. In the instrumental variables analysis, however, it becomes statistically insignificant. Results that include CEO-firm fixed effects are relevant to tournament theory because the incentive effects implied by that theory can be identified in isolation when the sorting effects associated with ability-based CEO selection across firms are eliminated. In the models with CEO-firm fixed effects, identification comes from variation in the pay gap over time within a CEO-firm match. How should such temporal variation be interpreted? What is its nature?

One interpretation is that such variation represents trial-and-error by the firm's board. Tournament theory implicitly assumes that the firm's board has all relevant information for choosing the optimal pay spread. In practice, however, boards of directors have limited information that constrains their ability to perfectly choose the optimal spread every time. The board makes the best decision it can each year, based on all available relevant information, but sometimes the informational deficiencies will lead to mistakes.[28] The result is a reduction in firm performance, which contributes to the aforementioned identifying variation in the data.

[28] Suppose, for example, that in one year the firm is located to the right of the peak of the inverted *U*. If perfect information were available, the board would reduce *ExecPayRatio*. Instead, because of imperfect information, the board incorrectly believes that the firm lies to the left of the peak, so it increases *ExecPayRatio* to raise firm performance.

A second interpretation, also related to the board's information, is that the multi-year nature of most CEO compensation contracts creates a lag between the time when the board receives information and the time when the board can act on it by adjusting *ExecPayRatio*.[29] Both the first and second interpretations concern information, but they differ. In the first the board makes mistakes because of a lack of information, whereas in the second the board always has perfect information but is forced to make suboptimal decisions due to the constraints imposed by its prior contractual obligations.

A third interpretation does not concern the board's information but rather the influence that CEOs have over their own pay, i.e., CEO duality or CEO power. It has been argued that for various reasons, CEOs exercise undue influence over their own compensation, which can lead to exorbitant pay packages that do not align with shareholders' interests (e.g., see Bebchuk and Fried 2006). The CEO's ability to extract greater compensation from the board may vary over time, creating the aforementioned identifying variation. It is easier to envision this mechanism in the empirical models that include firm (rather than CEO-firm) fixed effects, because in those cases switches in the firm's CEO contribute to identification, and a new CEO could have more (or less) success than the predecessor extracting compensation from the board.

The preceding three interpretations, which are neither mutually exclusive nor exhaustive, all involve constraints on CEO pay that are faced by the board. The constraint is imperfect information in the first interpretation, rigidity implied by multi-year contracts in the second

[29] Such lags contributed to the public outcry during the 2008-2009 global financial crisis, when CEOs collected large bonuses in failing firms that received taxpayer-financed bailouts (e.g., see DeVaro and Fung 2014). Schwab and Thomas (2006) find that in the U.S., the most common length of CEO contracts is three years, and the second most common length is five years. Suppose that in one year the firm's board correctly realizes that it is located to the left of the peak of the inverted *U*. It signs a three-year contract that increases the CEO's pay in each of the next three years, with the aim of climbing to a higher performance level on the inverted *U*. Suppose that two years into the contract, conditions suddenly change, and the firm finds itself to the right of the peak. The board would like to reduce CEO pay to shrink *ExecPayRatio*, but it is instead contractually obligated to increase the CEO's pay, which in turn reduces firm performance.

interpretation, and undue influence of the CEOs on their own pay in the third interpretation. The policy implications of the results differ across the three interpretations. For example, the final part of Bebchuk and Fried (2006), entitled "Going forward", includes two chapters on improving future executive compensation and corporate governance via regulations and reforms (e.g., increasing direct shareholder input on executive pay via voting) that are designed to limit CEOs' abilities to set their own pay. From the perspective of improving shareholder value, such solutions would be helpful under the third interpretation but not under either of the first two.

Another takeaway is that the widespread practice of "rote winsorization" of the pay gap is quantitatively quite destructive to the result, particularly when the winsorization is done at the top of the distribution. Whereas the inflection points in Panel C of Table IV (with winsorization of the bottom 1%) are of comparable magnitudes to those of Panel A (with no winsorization), the inflection points in Panel B (with 1% winsorization at both the top and bottom) are severely underestimated, which would lead researchers to the erroneous conclusion that a negative relationship (i.e., the downward sloping portion of the inverted) is more practically relevant than it actually is.

Finally, our findings provide new evidence to answer our research question: *Do big inequalities in executive pay hurt firm performance?* Overall, the empirical results support the inverted-*U*-shaped relationship predicted by tournament theory. The estimated inflection point suggests that firm performance, measured by Tobin's *q*, does not start to decrease until *ExecPayRatio* exceeds about 70, as seen in Figure 1. However, how practically relevant is the downward-sloping portion of this relationship? Our results reveal that the inflection point occurs far enough to the right that the downward-sloping portion is practically irrelevant. In other words, the relationship between the executive pay gap and firm performance is essentially positive. The mean of the pay gap is about 3 in the descriptive statistics, whereas even the lowest inflection point we identify in the data is substantially above that. While there are concerns that CEO pay has become exorbitant, our empirical results reveal that the inflection point beyond which firm performance suffers is extremely high. It should be remembered, however, that the analysis focuses on firm performance and does not address the social welfare implications of vertical pay disparity. While CEO pay increases with the firm's financial performance and may benefit investors/shareholders *at the firm level*, the economic effects on other firm outcomes (and other stakeholders, such as employees) and rising pay inequality are beyond the scope of this analysis.

VII. Conclusion

The subject of this study is topical, particularly given the new records in executive compensation being set by CEOs in the U.S. and around the world. We have taken stock of the growing literature on the subject and have clarified what tournament theory can say about that literature. Although most studies in the accounting and finance literature invoke tournament theory to explain a positive sign of the relationship of interest, that theory instead predicts an inverted *U*. It is unnecessary to appeal to behavioral theory, like social comparison and equity theory, to explain a negative relationship and tournament theory to explain a positive relationship. Tournament theory offers a unified theoretical framework that can explain both results consistently.

In addition to providing a theoretical foundation for examining the relationship of interest, this study highlights some methodological pitfalls with the goal of alerting scholars to some issues that, if avoided, can enhance the credibility of their estimates. This study demonstrates how to avoid the three types of researcher-induced endogeneity (i.e., uses of lagged dependent variables, winsorization of the pay gap, and omitted variable bias from the omission of CEO-firm fixed effects), and it develops a new instrument that is less vulnerable to criticism than earlier instruments. For example, the inclusion of CEO-firm fixed effects in the empirical specification avoids the estimated relationship of interest being corrupted by CEO turnover or other effects on firm performance – unrelated to vertical pay disparity – that arise from new organizational leadership. The main empirical result concerning the inverted *U* in Tobin's *q* survives the instrumental-variables analysis when fixed effects are incorporated in the theoretically preferred way (i.e., CEO-firm fixed effects) that tournament theory requires.

Furthermore, a Monte Carlo analysis reveals that "blanket" winsorization throws away valuable information in the extremes of the pay distributions, which are of particular interest for this research question. The common practice of winsorization induces measurement error in the independent variable, which creates an endogeneity bias that this study avoids by analyzing unwinsorized pay data that were carefully vetted, observation by observation, to discard rogue outliers. Our methodological point on winsorization has implications for branches of the accounting and finance literatures beyond our study.

When all the preceding measurement and econometric problems are addressed, the empirical specification delivers the inverted-*U*-shaped relationship predicted by tournament theory, albeit with an inflection point that is sufficiently high as to have little practical relevance. Thus, the answer to our research question is that even extremely high levels of executive pay disparity tend not to hurt firm performance.

Finally, a contribution of this study is bridging a gap between the literatures of labor economics (where tournament theory originated and continues to develop) and accounting and finance. Our study brings these disciplines and their literatures into closer alignment. We hope that this study helps to advance future work by encouraging scholars to: (a) recognize tournament theory's true empirical implications; (b) avoid inducing endogeneity problems via winsorizing the pay data and by other means; and (c) employ the fixed effects that are appropriate given the underlying theory (e.g., CEO-firm fixed effects in the case of tournament theory).

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Figure 1. ***ExecPayRatio*** vs. Industry-adjusted Tobin's *q*

This figure displays the relationship between *ExecPayRatio* and industry-adjusted Tobin's *q*, which is based on the regression results reported in Panel A's model 3 of Table IV. *** indicates statistical significance of the regression coefficients of *ExecPayRatio*(t) and *ExecPayRatio*^{2}(t), at the 1% level based on one-tailed tests. The inflection point is indicated by a vertical line.

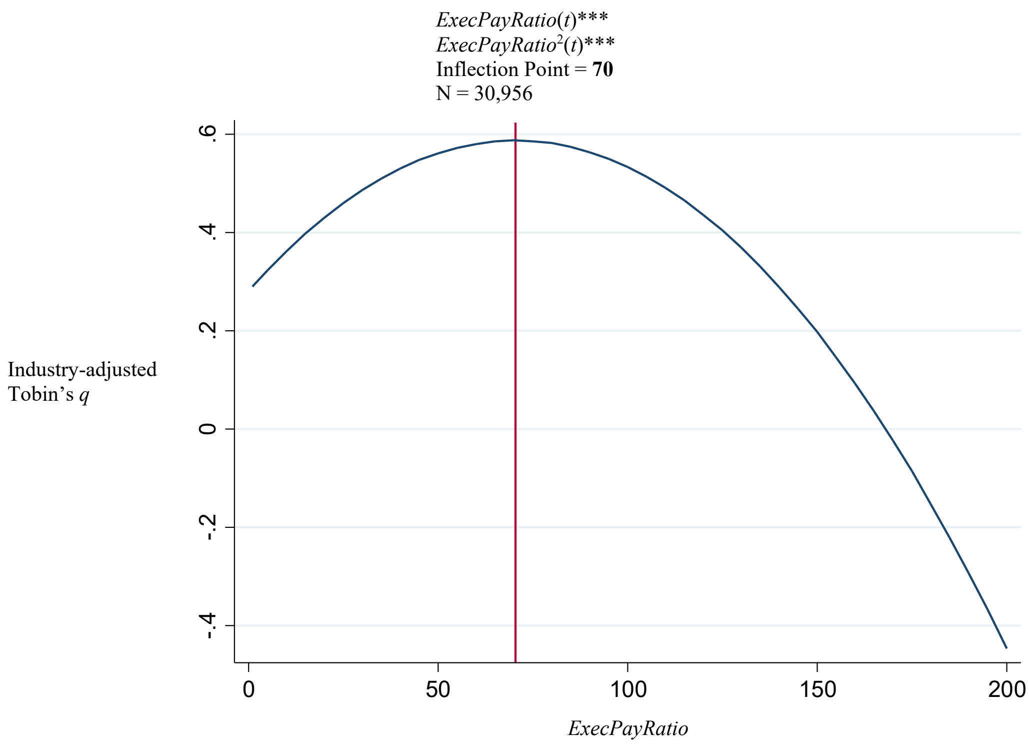

Table I: Empirical studies testing pay disparity and firm performance

(1) **Study and Sample (* indicates endogeneity of pay disparity is acknowledged; see column 8 for how it is addressed)**	(2) **Type of pay disparity ratio**	(3) **Dependent variable(s): Performance measure(s)**	(4) **Linear or quadratic**	(5) **Relation between pay disparity and performance**	(6) **Winsorizing**	(7) **Firm Fixed Effects**	(8) **Empirical methods (* indicates that the, or a, stated purpose of the method is to address endogeneity)**
* Present study	Vertical, (i) CEO versus all others (average worker pay); and (ii) CEO versus other executives	Tobin's q; ROA (both are industry adjusted)	quadratic	inverted *U*	Three cases are examined: no winsorzing; winsorized at 99%; winsorized at 1% and 99%.	Yes, including (i) firm fixed effects; and (ii) firm-CEO fixed effects)	* (i) industry adjusting the dependent and/or independent variables: Yes, in some specifications (ii) lagged dependent variable: No * (iii) contemporaneous and/or lagged alternative measure of dependent variable: No * (iv) instrumental variables estimation: Yes, 2SLS (instruments: see section IV.D of this study) (v) Arellano-Bond estimation: No
* Balsam et al. (2020) sample: US firms; years 1997-2014	Vertical, CEO versus all others (industry-level average worker pay)	Tobin's q; ROA (both are industry adjusted)	quadratic	inverted *U* with (far to the right) peak	Yes. All continuous variables, at 1% and 99%.	Yes	(i) industry adjusting the dependent and/or independent variables: Yes, in some specifications (ii) lagged dependent variable: Yes (iii) contemporaneous and/or lagged alternative measure of dependent variable: Yes * (iv) instrumental variables estimation: Yes, 2SLS (instruments: dummy for being headquartered in a blue state, unionization, and state minimum wage) (v) Arellano-Bond estimation: No
* Rouen (2020) sample: US firms; years 2006-2013	Vertical, CEO versus all others (employees' pay aggregated to the firm level)	industry-adjusted return on net operating assets, and 'cumulative returns' as an alternative measure	linear	positive if economic pay ratio (EPR) is used, consistent with tournament theory; negative if unexplained pay ratio (UPR) is used, consistent with equity theory	Yes. All continuous variables, at 1% and 99%.	Yes	* (i) industry adjusting the dependent and/or independent variables: Yes, industry-adjusted pay ratio * (ii) lagged dependent variable: Yes * (iii) contemporaneous and/or lagged alternative measure of dependent variable: Yes * (iv) instrumental variables estimation: Yes, 2SLS (instruments: firm's R&D growth and competitive environment). Also states that endogeneity is addressed by firm fixed effects and propensity score matching (v) Arellano-Bond estimation: No

(1) Study and Sample (* indicates endogeneity of pay disparity is acknowledged; see column 8 for how it is addressed)	(2) Type of pay disparity ratio	(3) Dependent variable(s): Performance measure(s)	(4) Linear or quadratic	(5) Relation between pay disparity and performance	(6) Winsorizing	(7) Firm Fixed Effects	(8) Empirical methods (* indicates that the, or a, stated purpose of the method is to address endogeneity)
* Cheng et al. (2019) sample: US firms; year 2011 only	Vertical, CEO versus all others (employee's pay from PayScale.com)	Tobin's q; ROA (both are industry adjusted)	linear; quadratic (in robustness check)	Positive	Yes. All variables, at 1% and 99%.	No	* (i) industry adjusting the dependent and/or independent variables: Yes, industry-adjust both the CEO pay ratio and measures of firm value/performance * (ii) lagged dependent variable: Yes * (iii) contemporaneous and/or lagged alternative measure of dependent variable: Yes (iv) instrumental variables estimation: No (v) Arellano-Bond estimation: No
* Burns et al. (2017) sample: Capital IQ cross-country data from 14 countries; years 2006-2010	Vertical, CEO versus next 3 workers (both the ratio and the difference); also Bebchuk's CEO pay slice	Tobin's q; ROA	linear plus interactions with culture; quadratic in robustness check	inverted *U* (for the U.S. and several other countries)	Yes. Compensation variables and firm characteristics at 5% and 95%.	No	(i) industry adjusting the dependent and/or independent variables: No (ii) lagged dependent variable: No (iii) contemporaneous and/or lagged alternative measure of dependent variable: No * (iv) instrumental variables estimation: Yes, 2SLS (instruments: median values of the pay gap (or pay difference) for firms in the same industry and in the same size quartile as the firm) (v) Arellano-Bond estimation: No
Mueller et al. (2017) sample: UK firms; years 2004-2013	Vertical, using 9 different hierarchical levels and comparing 2 at a time	ROA; industry-adjusted ROA; Tobin's q, industry-adjusted Tobin's q	linear	Positive	Yes. Only firm size and wage are winsorized at the bottom with various cutoffs.	No	(i) industry adjusting the dependent and/or independent variables: Yes (ii) lagged dependent variable: Yes (iii) contemporaneous and/or lagged alternative measure of dependent variable: No (iv) instrumental variables estimation: No (v) Arellano-Bond estimation: No
* Banker et al. (2016) sample: Chinese firms; years 2000-2009	Vertical, CEO versus all others	ROA; Margin; Growth (industry adjustments to all 3 are done as robustness check)	linear	positive, supporting "economic theories" as opposed to sociological ones	Yes. All variables winsorized at 1% and 99%, with similar results if variables are truncated at 1% and 99%.	No	(i) industry adjusting the dependent and/or independent variables: Yes, as robustness check (ii) lagged dependent variable: Yes (iii) contemporaneous and/or lagged alternative measure of dependent variable: No * (iv) instrumental variables estimation: Yes, 2SLS (instruments: two-period lagged values of pay gap) (v) Arellano-Bond estimation: No

(1) **Study and Sample** **(* indicates endogeneity of pay disparity is acknowledged; see column 8 for how it is addressed)**	(2) **Type of pay disparity ratio**	(3) **Dependent variable(s): Performance measure(s)**	(4) **Linear or quadratic**	(5) **Relation between pay disparity and performance**	(6) **Winsorizing**	(7) **Firm Fixed Effects**	(8) **Empirical methods** **(* indicates that the, or a, stated purpose of the method is to address endogeneity)**
* Ridge et al. (2015) sample: North American Fortune 500 firms; years 2003-2006	Vertical, CEO versus other executives	ROA	nonlinear, quadratic	*U*-shaped relationship, with negative coefficient on pay disparity and positive coefficient on its square	No.	Yes	(i) industry adjusting the dependent and/or independent variables: No (ii) lagged dependent variable: Yes (iii) contemporaneous and/or lagged alternative measure of dependent variable: No (iv) instrumental variables estimation: No * (v) Arellano-Bond estimation: Yes
* Shin et al. (2015) sample: Korean firms; years 2000-2009	Vertical, executives versus all employees	ROA; annual stock return	quadratic	negative main effect and zero quadratic effect in standard model; in preferred model, deviations from "expected" firm performance suggest an inverted-*U* relationship	Yes. All continuous variables, at 1% and 99%.	Yes	(i) industry adjusting the dependent and/or independent variables: Yes (ii) lagged dependent variable: Yes (iii) contemporaneous and/or lagged alternative measure of dependent variable: No * (iv) instrumental variables estimation: Yes, IV estimation (instruments: industry average pay ratio, fraction of unionized employees, and promotion probability of prospective executives) (v) Arellano-Bond estimation: No
* Connelly et al. (2016) sample: S&P500 firms; years 1996-2006	Vertical, executives versus all employees	ROA (the measure of short-term performance)	linear	Positive	Yes. All continuous variables, at 1% and 99%.	Yes	(i) industry adjusting the dependent and/or independent variables: No (ii) lagged dependent variable: Yes (iii) contemporaneous and/or lagged alternative measure of dependent variable: No (iv) instrumental variables estimation: No * (v) Arellano-Bond estimation: Yes
* Faleye et al. (2013) sample: US firms; years 1993-2006	Vertical, CEO versus non-executives	employee productivity is the main measure. TFP is also used. ROA and Tobin's q are also examined	linear	zero when employee productivity is the dependent variable, positive when ROA or Tobin's q are the dependent variables	Yes. All continuous variables, at 1% and 99%.	Yes	(i) industry adjusting the dependent and/or independent variables: Yes (ii) lagged dependent variable: Yes, in some models (iii) contemporaneous and/or lagged alternative measure of dependent variable: Yes (iv) instrumental variables estimation: No * (v) Arellano-Bond estimation: Yes
* Kale et al. (2009) sample: US firms; years 1993-2004	Vertical, log of the difference between CEO pay and the median pay of the other executives	Tobin’s q; ROA	linear	Positive	Yes. All variables at 1% and 99%.	Yes, CEO-fixed effects as robustness	(i) industry adjusting the dependent and/or independent variables: No (ii) lagged dependent variable: No (iii) contemporaneous and/or lagged alternative measure of dependent variable: No * (iv) instrumental variables estimation: Yes, 2SLS (instruments: median values of both tournament variables for firms in the same industry and in the same size quartile as the firm) (v) Arellano-Bond estimation: No

Table II: Variable Definitions

Variable	Definition
CEO and executive pay variables	
CEO total pay	CEO total compensation = *tdc1* obtained from the ExecuComp database that includes compensation such as salary, bonus, restricted stock grants, stock option grants, long-term incentive plan payout, and others.
Average non-CEO executive pay	Average of non-CEO executives' total compensation = average(*tdc1*)
Vertical pay disparity measures	
Executive pay ratio (*ExecPayRatio*)	CEO total pay / average non-CEO executive pay = *tdc1* / average(*tdc1*)
Executive pay difference (*ExecPayDiff*)	CEO total pay – average non-CEO executive pay = *tdc1* – average(*tdc1*)
Dependent variables	
Industry-adjusted Tobin's q (*q*)	Tobin's q = one year ahead market value of assets / book value of assets = (market value of equity + book value of assets – book value of equity – balance sheet deferred taxes) / book value of assets = (*at* + *csho*×*prc_f* – *ceq* – *txdb*) / *at*. Industry-adjusted Tobin's q is the firm's Tobin's q minus the industry median Tobin's q (based on Fama-French 12 industry classifications). The industry median Tobin's q is computed for each year in the sample.
Industry-adjusted ROA (*ROA*)	ROA = *ib*/*at* = one year ahead income before extraordinary items/ total assets. Industry-adjusted return on assets (ROA) is the firm's ROA minus the industry median ROA (based on Fama-French 12 industry classifications). The industry median ROA is computed for each year in the sample.
Other variables	
Leverage (*lev*)	Long-term debt/total assets = *dltt*/*at*
Firm age (*firmage*)	Years since listing
Firm size (*firmsize*)	Log(total assets) = log(*at*)
Capital Expenditure (*capxat*)	Capital expenditure/total assets = *capx*/*at*
R&D expenditure (*rdsale*)	Research and development expense/ net sales = *xrd*/*sale*
Missing R&D dummy (*rddum*)	Dummy variable indicating missing observations on R&D
Industry Concentration (*HHI*)	The net sales-based Herfindahl index is computed over all COMPUSTAT firms in the same industry using the Standard Industrial Classification (SIC) 2-digit code

Table III: Descriptive Statistics

This table reports descriptive statistics of the key variables used in the regression analysis. Industry-adjusted Tobin's q (*q*) and Industry-adjusted ROA (*ROA*) refer to one-year-ahead market value of assets (market value of equity, book value of assets net of book value of equity and deferred taxes) to book value of assets and one-year-ahead income before extraordinary items divided by total assets and, net of their industry medians based on Fama-French 12 industry classifications. *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). *ExecPayDiff* is the difference between CEO total pay (*CEOpay*) and the average pay for the non-CEO executives (*AvPayExec*). CEO total pay (*CEOpay*) and average pay for the non-CEO executives (*AvPayExec*) are measured in millions of U.S. dollars. Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code.

	N	Mean	Standard Deviation	Minimum	Maximum	Median
q	30,956	0.307	1.158	-1.298	5.560	-0.016
ROA	32,034	-0.013	0.105	-0.551	0.199	0.000
ExecPayRatio	34,754	3.068	5.377	1.000	847.214	2.604
ExecPayDiff	34,754	3.683	8.276	0.000	593.498	1.909
CEOpay	34,754	5.481	9.698	0.122	655.448	3.232
AvPayExec	34,754	1.797	2.277	0.027	92.293	1.156
capxat	34,754	0.055	0.053	0.001	0.298	0.039
rdsale	34,754	0.051	0.121	0.000	0.913	0.002
rddum	34,754	0.363	0.481	0.000	1.000	0.000
lev	34,754	0.204	0.182	0.000	0.828	0.182
firmage	34,754	20.489	13.723	0.000	54.000	18.000
firmsize	34,754	7.340	1.596	3.978	11.463	7.227
HHI	34,754	0.201	0.175	0.034	1.000	0.145

Table IV: Relationship Between Firm Performance and *ExecPayRatio*

This table reports regression estimation results from equations (4.1) and (4.2). *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). In the regression models, *ExecPayRatio* is scaled by dividing by 10; hence, *ExecPayRatio*^{2} is scaled by dividing by 100. Panel A displays results of the unwinsorized pay ratio, Panel B displays results that winsorize both tails of the pay ratio, and Panel C displays results that winsorize only the lower tail of the pay ratio. The dependent variables are industry-adjusted Tobin's *q* (reported in Models (1) to (3)) and 1-year-ahead industry-adjusted *ROA* (reported in Models (4) to (6)) at time *t*+1 (1-year ahead). Industry-adjusted Tobin's *q* and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). The control variables include year dummies, industry dummies (based on the Fama and French 12-industry classifications), Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The inflection point of the inverted *U* is computed as $-\delta_1/(2\delta_2)$ and multiplied by 100 (as the dependent variables are scaled by multiplying by 100); δ_1 is the coefficient of *ExecPayRatio*(*t*) and δ_2 is the coefficient of *ExecPayRatio*^{2}(*t*). "N/A" is reported if there is no inverted-*U*-shaped relationship and, hence, no inflection point of that inverted *U* to be found. The OLS regressions (Models (1) and (4)) are estimated with standard errors corrected for clustering at the firm level. The panel regressions (Models (2) and (5)) are estimated with firm fixed effects. The panel regressions (Models (3) and (6)) are estimated with CEO-firm fixed effects. Adjusted R-squared is reported for the OLS regressions (Models (1) and (4)) and within R-squared is reported for the panel regressions (Models (2), (3), (5), and (6)). Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on one-tailed tests for the regression results and two-tailed tests for the χ^2 tests of differences in coefficients across equations.

Panel A. No winsorization

	Model (1) OLS	Model (2) firm FE	Model (3) CEO-firm FE	Model (4) OLS	Model (5) firm FE	Model (6) CEO-firm FE
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	10.494	9.979	8.681	0.331	0.422	0.248
	(5.323)**	(2.661)***	(2.595)***	(0.409)	(0.279)*	(0.289)
ExecPayRatio^2(*t*)	-0.835	-0.774	-0.616	-0.015	-0.014	0.001
	(0.418)**	(0.258)***	(0.246)***	(0.036)	(0.027)	(0.028)
Lev	-142.622	-55.245	-32.737	-8.779	-4.358	-1.914
	(10.344)***	(4.639)***	(5.032)***	(0.739)***	(0.443)***	(0.523)***
firmage	-0.411	0.209	0.199	0.007	-0.013	0.040
	(0.134)***	(0.139)*	(0.245)	(0.008)	(0.014)	(0.027)*
firmsize	-0.719	-40.745	-45.437	0.733	-1.769	-2.735
	(1.290)	(0.981)***	(1.268)***	(0.099)***	(0.100)***	(0.139)***
Capxat	190.044	117.344	48.688	8.325	7.765	5.799
	(28.775)***	(14.694)***	(14.936)***	(1.934)***	(1.538)***	(1.656)***
Rdsale	147.786	9.318	29.347	-34.689	-35.329	-32.111
	(21.002)***	(9.782)	(10.098)***	(1.768)***	(0.983)***	(1.107)***
Rddum	-5.755	-7.415	-6.656	-0.963	0.291	0.461
	(3.986)*	(2.896)***	(3.561)**	(0.239)***	(0.302)	(0.391)
HHI	7.120	-20.049	-13.287	-1.940	-5.466	-4.669
	(11.243)	(6.970)***	(8.545)*	(0.681)***	(0.729)***	(0.947)***
constant	43.328	358.471	404.001	(5.017)	16.180	21.209
	(14.700)***	(9.576)***	(13.873)***	(1.439)***	(0.971)***	(1.512)***
Industry dummies	Yes	No	No	Yes	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.10	0.07	0.17	0.07	0.06
Number of Observations	30,956	30,956	30,956	32,034	32,034	32,034
Inflection Point	63	64	70	110	151	N/A
Hypothesis A:			$\chi^2(1) = 0.20$			$\chi^2(1) = 0.58$
δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			*p*-value = 0.651			*p*-value = 0.445
Hypothesis B:			$\chi^2(1) = 0.92$			$\chi^2(1) = 1.13$
δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			*p*-value = 0.337			*p*-value = 0.288
Joint Test of			$\chi^2(2) = 1.45$			$\chi^2(2) = 1.15$
Hypothesis A and Hypothesis B			*p*-value = 0.486			*p*-value = 0.562

Panel B. Winsorization at bottom 1% and top 1%

	Model (1) OLS	Model (2) firm FE	Model (3) CEO-firm FE	Model (4) OLS	Model (5) firm FE	Model (6) CEO-firm FE
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	-10.937	24.452	12.729	3.387	6.499	5.337
	(21.110)	(9.882)***	(9.956)	(1.473)**	(1.031)***	(1.098)***
ExecPayRatio^2(*t*)	22.858	-11.934	-2.954	-2.947	-5.573	-4.653
	(19.198)	(9.121)*	(8.970)	(1.436)**	(0.953)***	(0.991)***
lev	-142.450	-55.198	-32.756	-8.820	-4.346	-1.879
	(10.322)***	(4.639)***	(5.0323)***	(0.738)***	(0.443)***	(0.522)***
firmage	-0.407	0.206	0.197	0.006	-0.014	0.039
	(0.134)***	(0.139)*	(0.245)	(0.008)	(0.014)	(0.027)*
firmsize	-0.658	-40.800	-45.447	0.721	-1.785	-2.741
	(1.289)	(0.982)***	(1.268)***	(0.099)***	(0.100)***	(0.139)***
capxat	189.759	117.332	48.698	8.392	7.769	5.792
	(28.790)***	(14.693)***	(14.937)***	(1.934)***	(1.537)***	(1.656)***
rdsale	147.728	9.409	-29.432	-34.666	-35.269	-32.074
	(20.990)***	(9.782)	(10.099)***	(1.772)***	(0.983)***	(1.107)***
rddum	-5.899	-7.295	-6.562	-0.935	0.313	0.476
	(3.991)*	(2.896)***	(3.561)**	(0.240)***	(0.301)	(0.391)
HHI	7.280	-20.275	-13.371	-1.966	-5.563	-4.749
	(11.246)	(6.971)***	(8.546)*	(0.681)***	(0.729)***	(0.947)***
constant	46.026	355.730	403.078	(5.400)	15.078	20.203
	(14.985)***	(9.722)***	(13.996)***	(1.457)***	(0.987)***	(1.526)***
Industry dummies	Yes	No	No	Yes	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.10	0.07	0.17	0.07	0.06
Number of Observations	30,956	30,956	30,956	32,034	32,034	32,034
Inflection Point	N/A	10	22	6	6	6
Hypothesis A:			$\chi^2(1) = 3.85$*			$\chi^2(1) = 3.99$**
δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			*p*-value = 0.050			*p*-value = 0.046
Hypothesis B:			$\chi^2(1) = 3.44$*			$\chi^2(1) = 3.74$*
δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			*p*-value = 0.064			*p*-value = 0.053
Joint Test of			$\chi^2(2) = 3.85$			$\chi^2(2) = 4.01$
Hypothesis A and Hypothesis B			*p*-value = 0.146			*p*-value = 0.135

Panel C. Winsorization at bottom 1%

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
	OLS	firm FE	CEO-firm FE	OLS	firm FE	CEO-firm FE
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	10.508	9.985	8.681	0.332	0.421	0.247
	(5.324)**	(2.662)***	(2.595)***	(0.409)	(0.279)*	(0.289)
$ExecPayRatio^2(t)$	-0.836	-0.775	-0.616	-0.015	-0.014	0.001
	(0.418)**	(0.258)***	(0.247)***	(0.036)	(0.027)	(0.028)
lev	-142.622	-55.245	-32.737	-8.779	-4.358	-1.914
	(10.345)***	(4.639)***	(5.032)***	(0.739)***	(0.443)***	(0.523)***
firmage	-0.411	0.209	0.199	0.007	-0.013	0.040
	(0.134)***	(0.139)*	(0.245)	(0.008)	(0.014)	(0.027)*
firmsize	-0.719	-40.745	-45.437	0.733	-1.769	-2.735
	(1.290)	(0.981)***	(1.268)***	(0.099)***	(0.100)***	(0.139)***
capxat	190.046	117.344	48.687	8.325	7.765	5.799
	(28.775)***	(14.694)***	(14.936)***	(1.934)***	(1.538)***	(1.656)***
rdsale	147.786	9.318	-29.347	-34.689	-35.329	-32.111
	(21.002)***	(9.782)	(10.098)***	(1.768)***	(0.983)***	(1.107)***
rddum	-5.755	-7.415	-6.657	-0.963	0.291	0.461
	(3.986)*	(2.896)***	(3.561)**	(0.239)***	(0.302)	(0.391)
HHI	7.120	-20.048	-13.286	-1.940	-5.466	-4.669
	(11.243)	(6.970)***	(8.545)*	(0.681)***	(0.729)***	(0.947)***
constant	43.325	358.468	403.999	(5.017)	16.180	21.210
	(14.700)***	(9.576)***	(13.873)***	(1.439)***	(0.971)***	(1.512)***
Industry dummies	Yes	No	No	Yes	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.10	0.07	0.17	0.07	0.06
Number of Observations	30,956	30,956	30,956	32,034	32,034	32,034
Inflection Point	63	64	70	111	150	N/A
Hypothesis A:			$\chi^2(1) = 0.20$			$\chi^2(1) = 0.58$
δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			*p*-value = 0.652			*p*-value = 0.446
Hypothesis B:			$\chi^2(1) = 0.92$			$\chi^2(1) = 1.13$
δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			*p*-value = 0.337			*p*-value = 0.288
Joint Test of			$\chi^2(2) = 1.44$			$\chi^2(2) = 1.15$
Hypothesis A and Hypothesis B			*p*-value = 0.486			*p*-value = 0.563

Table V: Monte Carlo Simulations Showing Effect of Winsorizing Pay Variables

This table reports Monte Carlo analysis demonstrating the effect of measurement error induced by winsorization of the pay variables. Section A1 of the online appendix details the analysis. Column 1 assigns values $\delta_0 = 4.04$, $\delta_1 = 0.00868$, $\delta_2 = -0.0000616$, $\sigma = 1.361459$ in the regression $y_i = \delta_0 + \delta_1 ExecPayRatio_i + \delta_2 (ExecPayRatio_i)^2 + \varepsilon_i$, where $\varepsilon_i \sim N(0,\sigma^2)$. *ExecPayRatio*'s numerator is generated from the lognormal distribution with mean 5.481 and standard deviation 9.698. *ExecPayRatio*'s denominator is generated from the lognormal distribution with mean 1.797 and standard deviation 2.277. The sample size is 30,000. Column 2 reports averages (over *S* samples of size 30,000, where *S* = 100,000) of regression coefficients and the inflection point. Column 3 does the same thing as Column 2, using the same *S* samples of size 30,000, but winsorizing the top and bottom 1% of observations in *ExecPayRatio*.

	True Values	Unwinsorized	Winsorized
δ_0	4.040	mean = 4.040 min = 4.001 max = 4.076	mean = 4.008 min = 0.591 max = 4.069
δ_1	0.00868	mean = 0.00868 min = 0.00645 max = 0.01116	mean = 0.0206 min = 0.0089 max = 1.246
δ_2	-0.0000616	mean = -0.0000616 min = -0.0000685 max = -0.0000537	mean = -0.00038 min = -0.0314 max = -0.000125
Inflection point: $ExecPayRatio^* = -\delta_1/(2\delta_2)$	70.459	mean = 70.443 min = 55.775 max = 87.502	mean = 28.300 min = 17.84 max = 42.06

Table VI: Instrumental Variable (IV) Regressions - Firm Performance and *ExecPayRatio*

This table reports instrumental variable (IV) regression estimation (using GMM) results from equations (4.1) and (4.2). The instruments include *Counterfactual ExecPayRatio* (based on CEO-BOD(s)) and the squared and third-order terms of this ratio. *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). The dependent variables of the first-stage models include *ExecPayRatio*, which is scaled by dividing by 10, and $ExecPayRatio^2$, which is scaled by dividing by 100. Panel A reports summary results of the first-stage models. Panel B reports summary results of the second-stage results of firm performance on *ExecPayRatio* with instrumental variables including *Counterfactual ExecPayRatio* (based on CEO-BOD(s)) and the squared and third-order terms of this ratio. The dependent variables in the second-stage models are industry-adjusted Tobin's *q* (reported in Models (1) to (3)) and 1-year-ahead industry-adjusted *ROA* (reported in Models (4) to (6)) at time *t*+1 (1-year ahead). Industry-adjusted Tobin's *q* and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). In Panel B, a test of overidentifying restrictions is performed using the Sargan-Hansen test statistics (with the *p*-value reported in square brackets). The control variables include the following. Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The estimated coefficients of the control variables are not reported but are available upon request. Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on 1-tailed tests for the regression results.

Panel A. First-stage regressions with instrumental variables including *Counterfactual ExecPayRatio* and the squared and third-order terms

Instrumental variable: *ExecPayRatio*	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Counterfactual ExecPayRatio	0.108	0.167	0.142	0.103	0.187	0.172
	(0.031)***	(0.052)***	(0.055)***	(0.030)***	(0.050)***	(0.054)***
Counterfactual $ExecPayRatio^2$	-0.0469	-0.106	-0.075	-0.047	-0.111	-0.089
	(0.012)***	(0.036)***	(0.037)**	(0.011)***	(0.034)***	(0.037)***
Counterfactual $ExecPayRatio^3$	0.005	0.014	0.009	0.005	0.014	0.011
	(0.001)***	(0.005)***	(0.005)*	(0.001)***	(0.005)***	(0.005)**
Number of Observations	2079	1917	1782	2181	2016	1869

Instrumental variable: $ExecPayRatio^2$	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Counterfactual ExecPayRatio	0.243	0.461	0.448	0.236	0.502	0.555
	(0.116)**	(0.171)***	(0.184)***	(0.113)**	(0.162)***	(0.181)***
Counterfactual $ExecPayRatio^2$	-0.097	-0.261	-0.204	-0.098	-0.267	-0.260
	(0.037)***	(0.117)**	(0.125)*	(0.036)***	(0.110)***	(0.121)**
Counterfactual $ExecPayRatio^3$	0.010	0.0331	0.024	0.010	0.033	0.031
	(0.003)***	(0.017)**	(0.018)*	(0.003)***	(0.016)**	(0.018)**
Number of Observations	2079	1917	1782	2181	2016	1869

Panel B. Second-stage regressions with instrumental variables including *Counterfactual ExecPayRatio* and the squared and third-order terms

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	389.138	383.190	614.961	59.829	9.080	27.498
	(685.942)	(350.861)	(476.718)*	(77.830)	(35.921)	(44.472)
$ExecPayRatio^2(t)$	-230.530	-146.717	-192.526	-33.713	-0.513	-8.673
	(377.797)	(127.077)	(138.054)*	(42.805)	(12.972)	(12.924)
Number of Observations	2079	1917	1782	2181	2016	1869
Sargan-Hansen test statistics [*p*-value]	0.499 [*p* = 0.480]	0.305 [*p* = 0.581]	0.141 [*p* = 0.707]	0.001 [*p* = 0.981]	0.125 [*p* = 0.724]	0.243 [*p* = 0.622]
Inflection point	8	13	16	9	89	16

Online Appendix to

Do Big Inequalities in Executive Pay Hurt Firm Performance?

All tables reported in this online appendix are preceded by "A", as in "Table A1". Tables not preceded by "A" appear in the main text. This naming convention is also used for section numbers.

A1. Measurement Error Induced by Winsorizing the Pay Ratio

Table I reveals that previous work winsorizes the pay variables, following the usual custom in the finance and accounting literatures of winsorizing all continuous variables at 1% and 99%.[30] Such "blanket winsorization" is inappropriate for the present research question because it discards valuable information in the extremes of the pay distributions. Winsorization is customarily applied to minimize the influence of outliers. Here, however, the outliers themselves are of considerable interest. Winsorization, particularly at the top, throws away information in the most interesting part of the pay distribution for the question at hand. When the outliers themselves are of considerable interest for the question at hand, censoring them at a particular value induces bias (via measurement error in the censored variable) that renders the sample unrepresentative of the population of interest.[31]

We conduct Monte Carlo experiments to identify the potential effect of measurement error in the pay ratio (induced by winsorization at 1% and 99%) on the estimated relationship of interest.

[30] The only exception to this practice in Table I is Mueller et al. (2017), where the pay variables at the top of the distribution are not winsorized. Burns et al. (2017) employ unusually aggressive winsorization, i.e., 5% at both the top and bottom.

[31] Our concerns about winsorizing extreme pay data in a study of the effects of extreme pay disparity on firm performance were foreshadowed 60 years ago in the words of the statistician John Tukey (1960, p. 457), who writes the following about outliers: "we are likely to think of them as 'strays' [or] 'wild shots' ... and to focus our attention on how normally distributed the rest of the distribution appears to be. One who does this commits two oversights, forgetting Winsor's principle that 'all distributions are normal in the middle,' and forgetting that the distribution relevant to statistical practice is that of the values actually provided and not of the values which ought to have been provided." A page later he further warns that, "Sets of observations which have been de-tailed by over-vigorous use of a rule for rejecting outliers are inappropriate, since they are not samples." (Tukey 1960, p. 458).

We assume the following simple model for the data generating process, suppressing a time subscript:

$y_i = \delta_0 + \delta_1 ExecPayRatio_i + \delta_2(ExecPayRatio_i)^2 + \varepsilon_i$

where y_i is a measure of firm i's performance in a given year, $ExecPayRatio_i$ is the previous year's ratio of the CEO's pay to the average pay that the firm's other workers received, and $\varepsilon_i \sim N(0,\sigma^2)$, where ε_i is assumed to satisfy the standard assumptions, most importantly being uncorrelated with *ExecPayRatio* or its square. *ExecPayRatio* is unwinsorized.

Although population parameters in the preceding regression could be assigned arbitrarily for the purpose of generating Monte Carlo samples, these assignments are made from the data. Specifically, the values for δ_0, δ_1, δ_2, and σ are set equal to the estimated values of their counterparts in Model (3) of Table IV's Panel A. Although those values from Table IV are estimates from a data sample, in the present exercise they are treated as population parameters for the data generating process. Given those parameter values, and letting S denote the number of simulations, the following 6 steps are performed S times:

Step 1. Generate an N-vector for *ExecPayRatio* that represents the pay ratio in one simulated data set of size N. Specifically, the numerator and denominator of *ExecPayRatio* are generated as realizations of independent, lognormal random variables with means and variances matching those moments in the actual data. Given those estimated moments from the data, take N random draws from both lognormal distributions, and compute their ratios for each observation to generate N values of simulated data on *ExecPayRatio* and $ExecPayRatio^2$.

Step 2. Generate an N-vector for ε that represents the disturbance term in one simulated data set. Do this by taking N draws from $N(0,\sigma^2)$, given the population parameter σ.

Step 3. Generate an *N*-vector for *y* that represents the dependent variable in one simulated data set. Do this by substituting the population parameters (δ_0, δ_1, δ_2) and the values for *ExecPayRatio* and $ExecPayRatio^2$ (from step 1) and ε (from step 2) into the preceding population regression equation.

Step 4. Given the generated dependent and independent variables from steps 1 and 3, estimate the preceding regression using OLS to generate estimates of δ_0, δ_1, δ_2. Store those.

Step 5. Generate $ExecPayRatio^*$ by taking *ExecPayRatio* that was generated in step 1 and corrupting it with measurement error by winsorizing the ratio at 1% and 99%.[32]

Step 6. Given the generated variables from steps 3 and 5, estimate the preceding regression model via OLS, where *ExecPayRatio* and $ExecPayRatio^2$ are replaced by $ExecPayRatio^*$ and $(ExecPayRatio^*)^2$. Store the resulting estimates of δ_0, δ_1, δ_2.

Finally, compare the averages (over the *S* simulated samples) of the estimated parameters δ_0, δ_1, δ_2 from step 3 to the corresponding corrupted averages from step 6. The difference between those two sets of averages reflects the bias induced by winsorizing the pay variables.

Table V reveals a bias of considerable magnitude in all three estimated coefficients, with the intercept flipping sign because of winsorization. With unwinsorized pay variables, an inverted *U* is correctly and precisely identified in all *S* samples. Its peak, which occurs at *ExecPayRatio* = 70.459, is nearly exactly replicated in the unwinsorized analysis. In contrast, when the pay

[32] Specifically, *N* was chosen to be divisible by 100. Then $k = 0.01N$ was computed. *ExecPayRatio* was sorted from lowest to highest, with observation 1 being the lowest value and observation *N* the highest. Finally, the (sorted) ratio's smallest *k* values were replaced by its value $k+1$, and its largest *k* values were replaced by its value $N - k$.

variables are winsorized, the peak is dramatically underestimated at about 28.3, and even its maximum value (42.06) across all *S* samples falls far short of the true value of 70.459. Winsorization leads to an extreme mislocation of the peak of the inverted *U*. The fact that such extreme bias is induced even with only 1% winsorization at the top and bottom is noteworthy, given that some researchers winsorize even more aggressively. For example, Burns et al. (2017) winsorize both the top and bottom by 5%.

The bottom line from the Monte Carlo analysis is that winsorization of the pay variables is destructive to the identification of the relationship of interest. No claim is made that the directions of the biases revealed in Table V hold in general. Echoing the Monte Carlo analysis, the empirical analysis using real data shows that winsorizing (particularly at the top of the pay distributions) would lead to seriously misleading inferences. The claim here is not that winsorization is the only source of measurement error in *ExecPayRatio* or that avoiding winsorization completely solves the measurement error problem. Rather, it is that for this research question, winsorizing the pay variables introduces an avoidable source of measurement error that can bias all estimated parameters, potentially severely. Moreover, the bias cannot be signed. Roberts and Whited (2013, p. 560) note that finding better measures of an error-laden independent variable is an "obvious, but often costly approach" to the problem of measurement error. In the present case, because the measurement error is of a known form and introduced by the researcher, the solution is unusually simple, i.e., refrain from winsorizing the pay variables.

A2. CEO-Firm Fixed Effects Versus Firm Fixed Effects

The studies in Table I (see column 7) are roughly evenly split between those that incorporate firm fixed effects and those that include no fixed effects. Researchers' stated rationale for including firm fixed effects is to mitigate endogeneity caused by common unobserved determinants of the dependent and independent variables. Including firm fixed effects is better than excluding them, because the research question concerns the relationship between vertical pay disparity and firm performance within firms. An alternative to firm fixed effects, however, is CEO-firm fixed effects, which yield results that hold the CEO-firm match constant, even though none of the studies in this literature has used them. The choice between firm fixed effects and CEO-firm effects must be based on the underlying theoretical framework that motivates the empirical analysis. The difference between them is that firm fixed effects produce estimated coefficients that measure the effect of a change in the CEO pay ratio over time within a given firm, where at least part of that change might have occurred because the firm switches CEOs, whereas CEO-firm fixed effects produce estimated coefficients that measure the effect of a change in the CEO pay ratio over time for a given CEO-firm pair.

CEO-firm fixed effects are appropriate for testing tournament theory, which is the theoretical basis for most studies in Table I. Tournament theory concerns incentives, i.e., how the effort of the firm's non-CEO executives changes when the pay spread between them and the CEO changes, holding all else (and particularly the CEO-firm match) constant. Firm fixed effects are insufficient to identify the effect of interest. The reason is that when firm fixed effects are included, there are two types of within-firm variation that identify the relationship of interest. The first type is within-firm variation over time in the pay ratio of the existing workers and CEO. The second type is within-firm variation over time in the pay ratio that is caused by the firm switching CEOs. Only the first type of variation is desired, and including CEO-firm fixed effects exploits that type only, whereas including firm fixed effects allows both types simultaneously.

The reason that variation in vertical pay disparity that results from CEO turnover is undesirable from the standpoint of testing incentives-based tournament theory is that a new CEO is a major shock to an organization that often comes with organizational changes other than just a difference in vertical pay disparity. Proper identification of the relationship of interest, however, requires that *only* the vertical pay disparity changes while all else remains constant. Bennedsen et al. (2020) apply similar logic to address a different research question:

"However, firm fixed effects specifications do not allow us to determine whether the results are driven by CEO turnover events, which compare outgoing and incoming CEOs, or by within-CEO variation in CEO exposure that results from hospitalizations … we seek to estimate the effect of changing CEO productivity on performance, holding firm-CEO matches constant, which is an economically relevant relation that is unexplored in the literature. To this end, we rely on firm-CEO fixed effects specifications."

Bennedsen et al. (2020) cite multiple studies showing changes in performance around CEO turnover events (e.g., Johnson et al. 1985, Denis and Denis 1995, Huson et al. 2004, Pérez-González 2006, and Bennedsen et al. 2007). Such turnover-related changes are exactly what must be avoided in the present tournament context that is focused on internal incentives within the firm. If the organizational changes from new CEO leadership cause firm performance to change for reasons unrelated to vertical pay disparity, then the estimated relationship of interest is biased. This concern applies to most of the literature summarized in Table I, which uses incentives-based tournament theory as a theoretical framework but uses either firm fixed effects or (even worse) no fixed effects. The present study avoids the concern, holding the CEO-firm match constant by including CEO-firm fixed effects.

If CEO selection, rather than tournament incentives, is the motivating theoretical framework, then firm fixed effects are appropriate.[33] The argument is that a high CEO pay ratio may be an indication of the firm's ability to attract superior CEO talent in the labor market. This, rather than tournament-style incentives, is the argument advanced in Cheng et al. (2019) to explain

[33] See Tsoulouhas et al. (2007) for a theoretical analysis capturing CEO selection based on ability and the implications for firm performance.

a positive relationship between the pay ratio and firm performance. Under this argument, the value of a higher pay spread is that it attracts higher-quality CEOs, whose presence and actions lead to higher firm performance. The "presence and actions" of a new CEO following a turnover event are precisely the considerations that, as noted in the preceding paragraph, must be avoided to identify an uncorrupted measure of internal tournament incentives. But for the selection mechanism, which is based on CEO switches, firm fixed effects are desirable.

In summary, for testing tournament-style incentives, CEO-firm fixed effects are preferable, and for testing selection based on CEO ability, firm fixed effects are preferable. Most of the literature in Table I uses incentives-based tournament theory as a motivator, but none of the studies uses CEO-firm fixed effects. They instead use firm fixed effects or no fixed effects. Cheng et al. (2019) focus on CEO selection based on ability but are unable to use firm fixed effects because their data are cross sectional. Given that tournament theory is our motivating framework, we use CEO-firm fixed effects but also compare those results to those with firm fixed effects, to shed light on whether selection plays a role in generating the inverted *U*.

A3. Lagged Dependent Variable and Firm Performance Measure on the Right-Hand Side

In the literature of Table I (see column 3), the dependent variable measuring firm performance is typically either or both of Tobin's *q* and ROA, sometimes with an industry adjustment.

Many studies in Table I include the lagged dependent variable on the empirical model's right-hand side.[34] Researchers' justification for this decision is to mitigate endogeneity caused by simultaneity or omitted variables. Unfortunately, when estimation is by OLS, including the lagged dependent variable as a regressor creates, rather than solves, endogeneity problems. This is a

[34] Exceptions are Kale et al. (2009), Burns et al. (2017), and Mueller et al. (2017).

standard result in econometrics textbooks. For example, consider the following dynamic panel regression model reproduced from Cameron and Trivedi (2005, p. 763) with minor notation changes:

$$y_{it} = \gamma y_{i,t\text{-}1} + \delta ExecPayRatio_{i,t\text{-}1} + \boldsymbol{\beta}'\boldsymbol{x}_{it} + \alpha_i + \varepsilon_{it}, \qquad i = 1, \ldots, N, \qquad t = 1,\ldots,T$$

This specification is used in the studies in Table I, where y_{it} is a measure of firm i's performance in year t, such as Tobin's q, $ExecPayRatio_{i,t\text{-}1}$ is the year-t-1 ratio of the pay of firm i's CEO to the average pay of other executives in firm i, α_i is a firm fixed effect that is omitted in some studies in Table I, $\boldsymbol{x}_{it}$ is a column vector of control variables that includes a constant, ε_{it} is a serially uncorrelated stochastic term, and δ is the parameter of interest. The quadratic term in the pay ratio is omitted for simplicity.

Cameron and Trivedi (2005, p. 763) note that "An important result is that even if α_i is a random effect, OLS estimation of [the preceding model] leads to inconsistent estimation of [δ,] γ and β. This is because the regressor $y_{i,t\text{-}1}$ is correlated with α_i and hence with the composite error term ($\alpha_i + \varepsilon_{it}$)."

Multiple studies in Table I also include alternative measures of firm performance on the right-hand side. Since most studies use either Tobin's q or ROA as a dependent variable, this means that whichever one is not used appears on the right-hand side (either contemporaneously, lagged, or both). For example, Rouen (2020) includes both a contemporaneous and lagged value of an alternative performance measure on the right-hand side, in addition to the lagged dependent variable. The justification provided in that study and others is to address endogeneity arising from the fact that firm performance and compensation decisions are jointly determined.

Again, this practice creates, rather than solves, endogeneity. To see why, consider an alternative measure of the dependent variable, denoted z. Per Table I, let y denote Tobin's q, and let z denote ROA. Substituting z for y in the preceding dynamic panel model yields:

$$z_{it} = \gamma z_{i,t-1} + \delta ExecPayRatio_{i,t-1} + \boldsymbol{\beta}'\mathbf{x}_{it} + \alpha_i + \varepsilon_{it}, \quad i = 1, \ldots, N, \quad t = 1,\ldots,T$$

Lagging this equation once yields:

$$z_{i,t-1} = \gamma z_{i,t-2} + \delta ExecPayRatio_{i,t-2} + \boldsymbol{\beta}'\mathbf{x}_{i,t-1} + \alpha_i + \varepsilon_{i,t-1}, \quad i = 1, \ldots, N, \quad t = 1,\ldots,T$$

The studies in Table I (see column 8) regularly include either or both of z_{it} and $z_{i,t-1}$ on the right-hand side of the original y_{it} equation. When that equation includes $z_{i,t-1}$ on its right-hand side, it becomes:[35]

$$y_{it} = \gamma y_{i,t-1} + \delta ExecPayRatio_{i,t-1} + \lambda z_{i,t-1} + \boldsymbol{\beta}'\mathbf{x}_{it} + \alpha_i + \varepsilon_{it}, \quad i = 1, \ldots, N, \quad t = 1,\ldots,T$$

Now the control variable $z_{i,t-1}$ (which contains α_i, as seen in the equation for $z_{i,t-1}$) is correlated with the composite error term ($\alpha_i + \varepsilon_{it}$) in the y_{it} equation. This creates an endogeneity problem that yields inconsistent estimates of all parameters, including δ. Consistent estimation requires using a dynamic panel estimator like the Arellano-Bond estimator (see section A5).

A4. Instruments in Previous Literature

Several studies in Table I attempt to address the endogeneity of the pay gap using instrument-based approaches like two-stage least squares (2SLS). The instrument used in Kale et al. (2009) and Burns et al. (2017) is more defensible than the other instruments used in Table I. The main text describes how our instrument improves on that of Kale et al. (2009) and Burns et al. (2017). The instruments used in Table I's other studies are subject to the full force of the critiques discussed in Roberts and Whited (2013), particularly their observation on page 519 that "Another common mistake in the implementation of IV estimators is more careful attention to the relevance of the instruments than to their validity … Indeed, many papers in corporate

[35] The same problem arises if z_{it} is included, or both z_{it} and $z_{i,t-1}$ simultaneously.

finance discuss only the relevance of the instrument and ignore any exclusion restrictions." This problem is particularly daunting in the literature summarized in Table I, because of the breadth of the dependent variable. With as broad a concept as firm performance, that is subject to such a multitude of influences (direct and indirect), it is difficult to conceive of a perfectly valid instrument that could influence firm performance only via its effect on vertical pay disparity.

The validity criterion is typically what undermines confidence in this literature's instruments. Most studies in this literature do not even mention the criterion. The following discussion evaluates the validity criteria in the instruments used in Shin et al. (2015), Banker et al. (2016), Balsam et al. (2020), and Rouen (2020). The discussion corroborates the overall conclusion in Roberts and Whited (2013, p. 519) that "truly exogenous instruments are extremely difficult to find."

Shin et al. (2015) discuss endogeneity in one paragraph of their paper, on page 70, where they state that they estimate an instrumental variable model. Their quantitative results are unreported in tables or text. They state that they use three instruments (*Industry_Pay_Multiple*, *Union*, and *Promotion_Prob*) for *Pay_Multiple*, which is the independent variable, defined as the ratio of average annual pay for the executive director divided by that of the employees.

The first instrument, *Industry_Pay_Multiple*, is the industry average of *Pay_Multiple* but excluding the firm of interest. This resembles the instrument in Kale et al. (2009), which is the median pay ratio in that firm's industry and is the least problematic of three instruments in Shin et al. (2015), though the authors provide no justification for it.

Union measures the fraction of unionized employees at the firm. This is invalid because unionization (like regulation) can be expected to affect firm performance through numerous channels other than through the pay multiple (e.g., by rules governing hiring, layoffs, and promotions, and by affecting the composition of compensation packages in ways that do not affect the ratio *Pay_Multiple*).[36]

Promotion_Prob , which is designed to capture the promotion prospects of vice presidents (VPs), is the ratio $x/(x+y)$, where x is the number of inside executive directors, and y is the number of VPs. This ratio, which is a measure of the shape of the job hierarchy and specifically how bottom heavy it is (i.e., more VPs implies a more bottom-heavy job hierarchy). Bottom heaviness of the job hierarchy can be expected to have a direct effect on firm performance that operates independently of the pay ratio, as shown both theoretically and empirically in DeVaro and Morita (2013). Therefore, *Promotion_Prob* is an invalid instrument.

Banker et al. (2016) mention instruments only once in their paper, on page 525 in the following footnote 23: "In additional sensitivity tests, we check that our results hold when using a 2SLS approach. In the first stage, we instrument for pay gap using two-period lagged values of pay gap. Untabulated regression results indicate that our main results are robust to this alternate specification to address potential endogenity [sic] concerns." The validity criterion fails here because serial correlation in the regression's disturbance term implies that the period-(t-2) pay gap directly affects period-t firm performance, independent of the period-t pay gap.

Balsam et al. (2020) attempt to address endogeneity from omitted variables by using two-stage least squares (2SLS) with three instruments for vertical pay disparity, namely an indicator for whether the U.S. corporate headquarters is in a red state or a blue one, unionization in the industry, and the state minimum wage. The authors argue that these instruments are relevant because they are correlated with vertical pay disparity but uncorrelated with the disturbance term in the firm performance regression. No defense of that claim is offered for the latter two instruments, but the first is justified as follows: "For example, location of corporate headquarters

[36] See also our critique of unionization in the forthcoming discussion of Balsam et al. (2020).

(in blue or red state), which is likely where top executives also reside, can impact corporate culture as well as the tolerance for high pay and pay disparities."

None of these instruments is valid, because all three can directly influence firm performance via numerous plausible channels other than via the pay ratio. In the case of blue and red states, location of the headquarters in a blue state (setting aside the fact that even most "blue states" have a sizeable fraction of the population comprised of "red workers", and vice versa) likely affects the regulatory environment facing the organization. There are a host of regulations that can be expected to affect firm performance other than via vertical pay disparity. Similarly, although unionization in the industry can certainly influence vertical pay disparity, it can also influence firm performance via channels that are unrelated to pay disparity (e.g., by raising the costs of hiring and firing workers). Finally, echoing the red-states-blue-states argument, states with high minimum wages are likely to have a heavier regulatory hand in general than states with low minimum wages, and more heavily regulated environments can be expected to affect firm performance in ways that are unrelated to vertical pay disparity.

Rouen (2020, p. 345) decomposes the pay ratio into two separate measures (i.e., the "economic pay ratio" and the "unexplained pay ratio"). He instruments for the economic pay ratio using a shock to firms' growth opportunities, measured as firm-level R&D growth during the prior four years. Consistent with his focus on tournament theory as a motivating theoretical framework, he establishes the instrument's relevance (i.e., its correlation with the economic pay ratio) via the following incentives-based argument on page 367: "As firms grow, they require and attract more talented managers, and [the natural logarithm of the economic pay ratio] arguably becomes more important as it allows top performers to differentiate themselves and induces effort (Gabaix and Landier 2008; Trevor et al. 2012)." The problem with the exclusion is that a firm's recent R&D growth can be expected to directly affect its performance in a multitude of ways that have nothing to do with worker incentives or vertical pay disparity.[37]

Rouen (2020) instruments for the unexplained pay ratio using a shock to firms' competitive environment, which he argues should alter employees' perceptions of pay fairness. He presents an argument to establish the instrument's relevance,[38] but again, is silent on the crucial exclusion restriction. The problem with the exclusion is that if one were to survey managers and ask them whether a shock to the firm's competitive environment would affect firm performance, they would surely say yes, and if one were to further ask them about the mechanisms for this effect, they would surely provide multiple reasons that have nothing to with employees' perceptions of pay fairness.[39]

In summary, the instruments used in the preceding studies are invalid[40] because they do not satisfy the crucial exclusion restrictions, which in fact go unmentioned in all four studies.

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[37] Indeed, these other channels of influence are likely to be even more important than the channel via worker incentives and vertical pay disparity. For example, Griliches (1981) argues that R&D spending creates intangible capital for a firm and hence increases firm value. Johnson and Pazderka (1993) show that the stock market places a positive value on the reported R&D spending of the firm, as an indicator of expected profitability and growth. Their findings suggest that R&D reflects a rational allocation of resources. Eberthart, Maxwell, and Siddique (2004) show that R&D can improve a firm's stock market returns and long-term operating performance. Chen, Chen, Liang, and Wang (2013) find that R&D spillover effects (i.e., reaping benefits from R&D investment made by other firms) can affect a firm's long-run performance. Bøler, Moxnes, and Ulltveit-Moe (2015) find that R&D and international sourcing have a joint impact on firm performance.

[38] From pages 367-69: "For Ln UPR, I use as an instrument a shock to firms' competitive environment, that should alter employees' perception of pay fairness, large reductions in industry-level U.S. import tariffs, as defined by Huang, Jennings, and Yu (2017). As product market competition increases, competition for talent also increases, and Ln UPR is associated with higher turnover. In addition, Guadalupe and Wulf (2010) find that increased competition through trade liberalization results in domestic firms reducing the number of positions between the CEO and divisional managers, as well as increasing the number of employees reporting directly to the CEO, meaning that CEO compensation becomes a more salient measure of comparison for lower-level employees. This suggests the need to reduce pay unfairness (i.e., Ln UPR) as a result of tariff reductions in order to improve performance."

[39] The exclusion restriction is not credible if shocks to the competitive environment have a direct effect on firm performance, i.e., in ways that do not operate through the channel of workers' perceptions of pay fairness. For example, Vickers and Yarrow (1991) argue that product-market competition is important for performance through mechanisms such as allocative efficiency, as well as productive efficiency (e.g., through monitoring and performance comparisons). Isik and Hassan (2003) show that financial deregulation, which fosters a competitive environment, improved the performance of banks through productivity gains driven mostly by improved resource management. Yang and Zhao (2014) show that an exogenous shock to a competitive environment can improve firm performance through the channel of the CEO's dual leadership, with information cost savings and speedy decision making.

[40] The least problematic instrument is *Industry_Pay_Multiple* in Shin et al. (2015), which resembles that of Kale et al. (2009) discussed in the main text.

Instruments that fail to meet this validity condition should not be pursued, as they do not lead to meaningful inferences from instrument-based identification strategies like 2SLS.

A5. Arellano-Bond Dynamic Panel Estimator

Table I (see column 8) reveals that it is customary in this literature to include the lagged dependent variable on the regression's right-hand side, creating a dynamic panel model. Although researchers adopt this practice in the name of addressing endogeneity from omitted variables and simultaneity, in fact the practice creates an endogeneity problem. Thus, OLS estimation yields biased and inconsistent parameter estimates. The Arellano-Bond (1991) estimator is an instrument-based generalized method of moments estimator that can generate consistent parameter estimates in a dynamic panel model. The method starts by first differencing the dynamic panel regression to remove the firm fixed effect. The first difference of the lagged dependent variable that appears on the right-hand side of the differenced equation is endogenous, and further lags of the dependent variable are used to instrument for it. Other endogenous regressors (e.g., vertical pay disparity in the present context) can also be accommodated and are instrumented by their own lags.

The Arellano-Bond method instruments for endogenous regressors but differs from the instrument-based methods in section A4 in a few ways. One is that the method is designed for dynamic panel models with lagged dependent variables and fixed effects. It also generates its instruments "internally" (i.e., based on lagged values of the variables that are already in the regression) whereas the examples of the 2SLS method of section A4 involve instruments that are new variables that are not present in the original regression. Thus, multiple instruments are generated with greater ease with the Arellano-Bond method than with 2SLS, where it can be difficult to find even one valid instrument. An implication of the large number of instruments relative to the number of parameters to be estimated is that overidentifying restrictions can be tested via the Sargan-Hansen test. The null hypothesis for the test is that the overidentifying restrictions are valid, and a small *p*-value would lead to the rejection of the null. Unfortunately, the test's power weakens as the number of instruments expands. This problem of overfitting is discussed in Roodman (2009).

In addition to such practical concerns of implementation over matters like overfitting and proliferation of instruments, there are fundamental concerns about applying the method in this literature. Roberts and Whited (2013, section 3.6) discuss the inherent problems with lagged instruments. They note that original applications of dynamic panel methods in the finance literature were based on estimation of investment Euler equations (e.g., Whited 1992, Bond and Meghir 1994). The fundamental concern is that most financial variables exhibit considerable persistence, which makes it hard for researchers to "escape" undesired correlations among variables simply by lagging them. These problems do not apply to the present analysis, which omits a lagged dependent variable from the right-hand side.

A6. Analysis of Outliers

When estimating using an unwinsorized independent variable, care is needed to ensure that the tails of the distribution of the pay ratio contain no obviously errant observations. Table A1 displays the top-100 observations of *ExecPayRatio* for which the values of *CEOpay* and *AvExecPay* are both non-missing. All observations listed in Table A1 would be winsorized based on the standard practice adopted in Table I. Table A1 reveals that the tails of the pay variables contain some well-known companies that should legitimately be allowed to contribute (with uncensored pay data) to identification of the relationship between vertical pay disparity and firm performance.[41]

Table A2 provides the sample selection criteria. In the sample, 136 observations were deleted because the total CEO compensation is less than the average of the other executives'

[41] Examples include Alphabet, Apple, Disney, Helen of Troy, Intuit, and Broadcom, among others.

compensation (excluding the CEO's). After deleting the observations at the bottom that require trimming, the analysis sample contains 37,241 firm-year observations for 2918 unique firms from 1992 to 2019.[42] None of the top 100 observations were trimmed because those observation all fit the selection criteria in Table A2.

To ensure data quality, we also manually check the observations with the highest values of compensation in the ExecuComp database by comparing them to the original source documents such as the filing of SCHEDULE 14A (SEC Form DEF 14A). Overall, we found a high degree of correspondence for these observations of extreme values. One example that we manually reviewed was Alphabet in 2019 (the first observation in Table A1), which shows CEO pay of 280 million U.S. dollars. This observation is consistently reported in the source document of SCHEDULE 14A, obtained from the SEC EDGAR.[43] This observation is also covered by multiple news outlets, such as the articles from CNBC, stating that "SEC filings showed Pichai was paid a total of $226 million last year, mostly through a $218 million stock award, which he receives every three years. The last time he received the award, in 2019, it was for $276.6 million and total compensation was $280.6 million."[44] We also discuss other notable examples, such as those with CEO pay near or exceeding $100 million. For example, in the second and eighth observations of Table A1, we manually checked the data for CEO

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[42] The reported sample sizes in the regression results are smaller because they require non-missing values in both the dependent and control variables.

[43] Source: https://www.sec.gov/Archives/edgar/data/1652044/000130817920000203/lgoog2020_def14a.htm

[44] See (https://www.cnbc.com/2023/05/03/google-employees-complain-about-ceo-sundar-pichais-pay-raise.html#:~:text=SEC%20filings%20showed%20Pichai%20was,total%20compensation%20was%20%24280.6%20million.). See also the article from Bloomberg (https://www.bloomberg.com/news/articles/2020-04-24/alphabet-ceo-pichai-s-2019-compensation-worth-281-million), which reports that "Alphabet CEO Pichai's 2019 Compensation Worth $281 Million". Furthermore, see S&P Global (https://www.spglobal.com/marketintelligence/en/news-insights/latest-news-headlines/at-280m-alphabet-s-sundar-pichai-is-2019-s-highest-paid-us-info-tech-ceo-59872469), which reports that "At $280M, Alphabet's Sundar Pichai is 2019's highest-paid US info tech CEO."

compensation values, verifying and confirming them using the source document (actual filing of SCHEDULE 14A) of Apple from the SEC EDGAR.[45]

[45] See (https://www.sec.gov/Archives/edgar/data/320193/000104746903009893/a2106068zdef14a.htm) and (https://www.sec.gov/Archives/edgar/data/320193/000091205701007512/a2040764zdef14a.txt). In addition to the source documents, these observations are also consistent with the data reported in the literature. For example, the study by Guthrie, Sokolowsky, and Wan (2012) reports the same observations for the pay of Apple's CEO (see Table II of Guthrie, Sokolowsky, and Wan (2012)), which are identical to the values reported in our sample. Furthermore, Apple's largest compensation value in fiscal year 2000 is also covered by the news outlets; see, e.g., (https://www.forbes.com/2000/01/20/mu2.html).

Table A1. The top 100 observations of *ExecPayRatio*

Table A1 displays the observations in the top 100 observations of *ExecPayRatio* for which the values of *CEOpay* and *AvPayExec* (both measured in millions of U.S. dollars) are both non-missing and greater than zero.

Fiscal Year	Company Name	*CEOpay*	*AvPayExec*	*ExecPayRatio*	*ExecPayDiff*
2019	ALPHABET INC	280.622	0.331	847.214	280.29
2002	APPLE INC	93.016	0.551	168.741	92.465
2013	NABORS INDUSTRIES LTD	95.246	0.647	147.278	94.599
2016	COCA-COLA EUROPEAN PARTNERS	3.89	0.027	145.859	3.863
2015	MASIMO CORP	119.223	0.886	134.514	118.336
2002	CARMIKE CINEMAS INC	16.832	0.14	120.373	16.693
2003	APPLE INC	74.75	0.764	97.842	73.986
2000	APPLE INC	600.347	6.85	87.644	593.498
1996	DISNEY (WALT) CO	202.185	2.661	75.967	199.524
1998	ETHAN ALLEN INTERIORS INC	17.729	0.248	71.345	17.481
2012	HELEN OF TROY LTD	41.639	0.697	59.768	40.943
2000	INTUIT INC	58.287	0.984	59.214	57.303
2003	EPICOR SOFTWARE CORP -OLD	39.732	0.677	58.671	39.054
2001	PRIME HOSPITALITY CORP	16.285	0.287	56.716	15.998
2004	BROADCOM CORP	11.056	0.196	56.503	10.86
2013	FLEETCOR TECHNOLOGIES INC	32.858	0.641	51.272	32.217
2008	FEDERAL-MOGUL HOLDINGS CORP	38.466	0.826	46.59	37.64
2005	BROADCOM CORP	53.402	1.189	44.909	52.213
2009	URBAN OUTFITTERS INC	29.944	0.691	43.336	29.253
2010	UNIFIRST CORP	21.173	0.498	42.502	20.675
1993	BORLAND SOFTWARE CORP	8.549	0.216	39.57	8.333
1998	SAFESKIN CORP	30.773	0.788	39.05	29.985
1995	DSC COMMUNICATIONS CORP	54.218	1.393	38.925	52.825
2005	WATSCO INC	18.571	0.478	38.875	18.093
1998	WARNACO GROUP INC	77.99	2.071	37.66	75.919
1993	CEC ENTERTAINMENT INC	12.836	0.353	36.35	12.483
1999	ABERCROMBIE & FITCH -CL A	124.514	3.441	36.182	121.072
2002	EDWARDS J D & CO	21.139	0.616	34.313	20.523
2008	EBIX INC	2.849	0.084	34.074	2.766
2012	NABORS INDUSTRIES LTD	19.735	0.586	33.676	19.149
1998	AUTHENTIC FITNESS	16.032	0.49	32.718	15.542
2013	CHARTER COMMUNICATIONS INC	49.912	1.614	30.918	48.297
2001	CDW CORP	45.118	1.46	30.907	43.658
2014	DISCOVERY INC	156.078	5.066	30.808	151.012
2003	ETHAN ALLEN INTERIORS INC	9.772	0.318	30.697	9.454
2007	MASSEY ENERGY CO	35.93	1.176	30.56	34.755
2000	NYFIX INC	8.287	0.284	29.206	8.003

1999	BROADVISION INC	22.126	0.763	29.008	21.363
1995	CHAMPION ENTERPRISES INC	7.963	0.28	28.414	7.683
2016	UNIFIRST CORP	17.686	0.628	28.15	17.058
1998	HILTON WORLDWIDE HOLDINGS	31.55	1.133	27.848	30.417
2009	SUPER MICRO COMPUTER INC	12.236	0.439	27.842	11.796
2004	INTEGRA LIFESCIENCES HOLDNGS	34.005	1.226	27.747	32.779
2008	TICKETMASTER ENTERTNMNT INC	61.017	2.233	27.331	58.784
2013	HELEN OF TROY LTD	31.332	1.166	26.882	30.166
2009	EBIX INC	2.46	0.092	26.878	2.368
2018	J2 GLOBAL INC	45.062	1.751	25.736	43.311
2013	MOBILE MINI INC	24.073	0.946	25.458	23.128
2000	STEEL CONNECT INC	119.012	4.686	25.396	114.326
2015	EBIX INC	5.61	0.222	25.311	5.388
1994	GEOTEK COMMUNICATIONS	6.269	0.249	25.173	6.02
2017	WEIGHT WATCHERS INTL INC	33.372	1.329	25.104	32.043
2002	MASSEY ENERGY CO	13.249	0.534	24.827	12.715
2000	SIEBEL SYSTEMS INC	293.097	11.849	24.736	281.249
1998	PICTURETEL CORP	7.44	0.301	24.698	7.139
2019	VIACOMCBS INC	124.94	5.101	24.495	119.839
2017	VARIAN MEDICAL SYSTEMS INC	40.974	1.673	24.495	39.301
2017	EBIX INC	5.708	0.234	24.415	5.474
1999	ANADARKO PETROLEUM CORP	21.53	0.883	24.384	20.647
2008	CONSOLIDATED GRAPHICS INC	12.781	0.525	24.347	12.256
2004	MIDWAY GAMES INC	7.102	0.296	23.992	6.806
2004	GATEWAY INC	47.944	2.005	23.916	45.939
2000	INFORMATION HOLDINGS INC	5.847	0.245	23.915	5.603
1996	HILTON WORLDWIDE HOLDINGS	31.574	1.326	23.813	30.248
2006	WEBSENSE INC	25.624	1.079	23.753	24.545
2000	POWERWAVE TECHNOLOGIES INC	16.501	0.706	23.358	15.794
2007	EBIX INC	1.945	0.084	23.15	1.861
2005	ACI WORLDWIDE INC	15.244	0.659	23.121	14.585
2012	BIGLARI HOLDINGS INC	10.918	0.473	23.094	10.445
2008	ROPER TECHNOLOGIES INC	33.318	1.451	22.961	31.867
2001	CONOCOPHILLIPS	48.509	2.133	22.741	46.376
1998	SAFEWAY INC	20.645	0.914	22.599	19.731
2000	IDEX CORP	14.264	0.635	22.459	13.629
2006	MASSEY ENERGY CO	24.942	1.111	22.447	23.831
2016	NVR INC	20.825	0.929	22.425	19.896
2002	MIDGARDXXI INC	6.659	0.298	22.319	6.361
1997	DONNELLEY (R R) & SONS CO	21.387	0.974	21.951	20.413
2001	RETEK INC	25.091	1.147	21.873	23.944
2004	ETHAN ALLEN INTERIORS INC	6.362	0.292	21.801	6.07
2008	ETHAN ALLEN INTERIORS INC	5.372	0.247	21.746	5.125

2002	SUN MICROSYSTEMS INC	25.709	1.193	21.545	24.516
2015	IMS HEALTH HOLDINGS INC	34.53	1.612	21.421	32.918
2005	MASSEY ENERGY CO	32.64	1.529	21.344	31.111
2007	GAP INC	39.069	1.846	21.167	37.223
2014	MICROSTRATEGY INC	24.102	1.145	21.042	22.956
2009	HELEN OF TROY LTD	8.906	0.426	20.893	8.479
1999	CALPINE CORP	20.096	0.974	20.642	19.122
1997	HONEYWELL INTERNATIONAL INC	21.688	1.052	20.613	20.636
1996	SOUTHWEST AIRLINES	9.783	0.476	20.542	9.307
2000	FRONTIER COMMUNICATIONS CORP	21.672	1.058	20.479	20.614
2013	SPRINT CORP	26.211	1.285	20.392	24.926
1995	OFFICEMAX INC-OLD	6.362	0.313	20.316	6.049
1995	SEARS HOLDINGS CORP	19.833	0.977	20.289	18.855
2004	ALTABA INC	120.061	5.924	20.266	114.137
2008	LANDRYS RESTAURANTS INC	6.687	0.33	20.248	6.357
2008	HARMAN INTERNATIONAL INDS	30.387	1.51	20.121	28.877
2009	ABERCROMBIE & FITCH -CL A	36.336	1.81	20.072	34.525
2001	INTERNET SECURITY SYSTEMS	15.63	0.78	20.049	14.85
2018	EBIX INC	4.41	0.221	19.931	4.189
1992	B/E AEROSPACE INC	6.446	0.324	19.89	6.122

Table A2: Sample Selection Criteria

Table A2 displays the sample selection criteria.

Criteria	Sample – CEO vs. other executives
#1	Drop if gap is missing (non-missing for either numerator or denominator)
#2	CEO compensation pay = 0 or < 0
#3	Average Executive Pay < 0 or = 0
#4	Ratio < 1

A7. Alternative Measure of Executive Pay Gap

An alternative measure of vertical pay disparity is the difference between the numerator and denominator, rather than their ratio. Although the ratio is more commonly analyzed in the literature of Table I, there is a theoretical rationale for preferring the difference. The theoretical literature on tournament theory, starting with Lazear and Rosen (1981), has used the wage difference (i.e., the "spread") between job levels, not the ratio of the wage between levels.[46] Ratios and differences are not the same and can yield different results in regression analysis. The ratio is studied in the main analysis for comparability with the literature in Table I, most of which uses the ratio.

For executives, consider *ExecPayDiff*, which is defined as *CEOpay* – *AvExecPay*. Table A3 reports estimation results that replicate Table IV but using *ExecPayDiff* instead of *ExecPayRatio*. As revealed in models 3 and 6 of Panel A, evidence of an inverted *U* is found for both measures of firm performance. The approximate values of *ExecPayDiff* at which the peak occurs are 181 for Tobin's *q* and about 230 for *ROA*. These numbers drop substantially to 20 and 15, respectively, when standard winsorization of the pay variables is applied, as shown in Panel B. As in Table IV, this damage to the preferred quantitative results from Panel A is driven by winsorization of the right tails of the pay variables, given that the results are very similar between Panels A and C.

In Table A3, for Tobin's *q*, Panel A reveals that the joint test of equality of coefficients is rejected at the 10% level. This result is driven by the difference in coefficients of *ExecPayDiff* between models 2 and 3, given that in the individual test the equality of those two coefficients can be rejected even at significance levels below 3%. Panel B reveals that winsorization of the pay

[46] The empirical tests of tournament theory in DeVaro (2006a,b) and DeVaro and Kauhanen (2016) also are based on the wage difference rather than on the ratio.

variables induces an exaggeration of the differences in coefficients between the models with firm effects and those with CEO-firm fixed effects. The differences in coefficients between the equations, whether tested individually or jointly, are all highly statistically significant.

Turning to *ROA*, in the preferred results of Panel A, all three statistical tests fail to reject the null hypothesis of equality of coefficients between the models with firm fixed effects and those with CEO-firm fixed effects. The inferences are radically different in the winsorized results of Panel B, where all three null hypotheses in the chi-square tests are definitively rejected. The winsorized results in Panel B (which support H2c) paint a different qualitative picture for *ROA* than the unwinsorized results (which support H2a) in Panel A.

Table A3: Relationship Between Firm Performance and *ExecPayDiff*

This table reports regression estimation results from equations (4.1) and (4.2). *ExecPayDiff* is the difference between CEO total pay (*CEOpay*) and the average pay for the non-CEO executives (*AvPayExec*). In the regression models, *ExecPayDiff* is scaled by dividing by 10; hence, $ExecPayDiff^2$ is scaled by dividing by 100. Panel A displays results using the unwinsorized pay ratio, Panel B displays results that winsorize both tails of the pay ratio, and Panel C displays results that winsorize only the lower tail of the pay ratio. The dependent variables are industry-adjusted Tobin's *q* (reported in Models 1 to 3) and 1-year-ahead industry-adjusted *ROA* (reported in Models 4 to 6) at time *t*+1 (1-year ahead). Industry-adjusted Tobin's *q* and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). The control variables include the following. Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The estimated coefficients of the control variables are not reported but are available upon request. The inflection point of the inverted *U* is computed as $-\delta_1/(2\delta_2)$ and multiplied by 100 (as the dependent variables are scaled by multiplying by 100); δ_1 is the coefficient of *ExecPayRatio*(*t*) and δ_2 is the coefficient of $ExecPayRatio^2(t)$. The OLS regressions (Models 1 and 4) are estimated with standard errors corrected for clustering at the firm level. The panel regressions (Models 2 and 5) are estimated with firm fixed effects. The panel regressions (Models 3 and 6) are estimated with CEO-firm fixed effects. Adjusted R-squared is reported for the OLS regressions (Models 1 and 4) and within R-squared is reported for the panel regressions (Models 2, 3, 5, and 6). Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on 1-tailed tests for the regression results and 2-tailed tests for the χ^2 tests of differences in coefficients across equations.

Panel A. No winsorization

	Model (1) OLS	Model (2) firm FE	Model (3) CEO-firm FE	Model (4) OLS	Model (5) firm FE	Model (6) CEO-firm FE
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayDiff(*t*)	21.283	9.111	6.545	0.581	0.432	0.420
	(2.603)***	(0.990)***	(0.976)***	(0.153)***	(0.104)***	(0.109)***
$ExecPayDiff^2(t)$	-0.455	-0.216	-0.181	-0.013	-0.009	-0.009
	(0.069)***	(0.025)***	(0.024)***	(0.003)***	(0.003)***	(0.003)***
R-squared	0.10	0.10	0.08	0.17	0.07	0.06
Number of Observations	30956	30956	30956	32034	32034	32034
Inflection point	233	211	181	230	229	230
Hypothesis A: δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			$\chi^2(1) = 5.14$** *p*-value = 0.023		$\chi^2(1) = 0.01$ *p*-value = 0.935	
Hypothesis B: δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			$\chi^2(1) = 2.62$ *p*-value = 0.106		$\chi^2(1) = 0.04$ *p*-value = 0.845	
Joint Test of Hypothesis A and Hypothesis B			$\chi^2(2) = 5.36$* *p*-value = 0.069		$\chi^2(2) = 0.20$ *p*-value = 0.907	

Panel B. Winsorization at bottom 1% and top 1%

	Model (1) OLS	Model (2) firm FE	Model (3) CEO-firm FE	Model (4) OLS	Model (5) firm FE	Model (6) CEO-firm FE
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayDiff(*t*)	94.855	52.283	33.076	4.639	3.846	3.019
	(8.432)***	(3.736)***	(3.742)***	(0.543)***	(0.390)***	(0.413)***
$ExecPayDiff^2(t)$	-27.412	-15.347	-8.107	-1.660	-1.338	-0.979
	(3.489)***	(1.658)***	(1.635)***	(0.242)***	(0.173)***	(0.181)***
R-squared	0.11	0.10	0.08	0.17	0.07	0.06
Number of Observations	30956	30956	30956	32034	32034	32034
Inflection point	17	17	20	14	14	15
Hypothesis A: δ_1 (firm FE) – δ_1 (CEO-firm) FE = 0			$\chi^2(1) = 39.23$*** *p*-value = 0.000			$\chi^2(1) = 7.89$*** *p*-value = 0.005
Hypothesis B: δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			$\chi^2(1) = 27.00$*** *p*-value = 0.000			$\chi^2(1) = 8.78$*** *p*-value = 0.003
Joint Test of Hypothesis A and Hypothesis B			$\chi^2(2) = 40.03$*** *p*-value = 0.000			$\chi^2(2) = 8.84$** *p*-value = 0.012

Panel C. Winsorization at bottom 1%

	Model (1) OLS	Model (2) firm FE	Model (3) CEO-firm FE	Model (4) OLS	Model (5) firm FE	Model (6) CEO-firm FE
Dependent variable	Tobin's *q*(t+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayDiff(*t*)	21.284	9.111	6.545	0.581	0.432	0.420
	(2.603)***	(0.990)***	(0.976)***	(0.153)***	(0.104)***	(0.109)***
ExecPayDiff^2(*t*)	-0.455	-0.216	-0.181	-0.013	-0.009	-0.009
	(0.069)***	(0.025)***	(0.024)***	(0.003)***	(0.003)***	(0.003)***
R-squared	0.10	0.10	0.08	0.17	0.07	0.06
Number of Observations	30956	30956	30956	32034	32034	32034
Inflection point	234	211	181	230	229	230
Hypothesis A: δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			$\chi^2(1)$ = 5.14** *p*-value = 0.023			$\chi^2(1)$ = 0.01 *p*-value = 0.934
Hypothesis B: δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			$\chi^2(1)$ = 2.62 *p*-value =			$\chi^2(1)$ = 0.04 *p*-value = 0.846
Joint Test of Hypothesis A and Hypothesis B			$\chi^2(2)$ = 5.36* *p*-value = 0.069			$\chi^2(2)$ = 0.20 *p*-value = 0.907

Table A4: Instrumental Variable (IV) Regressions - Firm Performance and *ExecPayRatio* (Alternative Configurations of Instruments Including Directors' Influence)

This table reports instrumental variable (IV) regression estimation results, using GMM, from equations (4.1) and (4.2). The instruments include *Counterfactual ExecPayRatio* (based on CEO-BOD(s)), the squared and third-order terms of this ratio, and the percentage of CEO-BOD(s) who is also the CEO and Chairman of her/his own company. *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). The dependent variables of the first-stage models include *ExecPayRatio*, which is scaled by dividing by 10, and *ExecPayRatio*^{2}, which is scaled by dividing by 100. Panel A reports summary results of the first-stage models. Panel B reports summary results of the second-stage results of firm performance on *ExecPayRatio* with instrumental variables including *Counterfactual ExecPayRatio* (based on CEO-BOD(s)) and the squared and third-order terms of this ratio. The dependent variables in the second-stage models are industry-adjusted Tobin's *q* (reported in Models 1 to 3) and 1-year-ahead industry-adjusted *ROA* (reported in Models 4 to 6) at time *t*+1 (1-year ahead). Industry-adjusted Tobin's *q* and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). In Panel B, a test of overidentifying restrictions is performed using the Sargan-Hansen test statistics (with the *p*-value reported in square brackets). The control variables include the following. Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The estimated coefficients of the control variables are not reported but are available upon request. Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on 1-tailed tests for the regression results.

Panel A. First-stage regressions with instrumental variables including *Counterfactual ExecPayRatio*, the squared and third-order terms of this ratio, and the percentage of CEO-BOD(s) who is also CEOs and Chairman of her/his own company

Instrumental variable: *ExecPayRatio*	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Counterfactual ExecPayRatio	0.108	0.167	0.142	0.104	0.187	0.172
	(0.031)**	(0.052)**	(0.055)**	(0.030)**	(0.050)**	(0.054)**
Counterfactual ExecPayRatio^2	-0.047	-0.106	-0.075	-0.048	-0.111	-0.089
	(0.012)**	(0.036)**	(0.037)**	(0.012)**	(0.034)**	(0.036)**
Counterfactual ExecPayRatio^3	0.005	0.014	0.009	0.005	0.014	0.011
	(0.001)**	(0.005)**	(0.005)**	(0.001)**	(0.005)**	(0.005)**
% of CEO-BOD(s) who is CEOs and	-0.017	-0.020	-0.029	-0.016	-0.018	-0.026
	(0.008)**	(0.015)	(0.017)**	(0.007)**	(0.0150)	(0.018)*
Number of Observations	2079	1917	1782	2181	2016	1869

Instrumental variable: *ExecPayRatio*^{2}	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Counterfactual ExecPayRatio	0.245	0.463	0.448	0.237	0.503	0.555
	(0.116)**	(0.171)**	(0.184)**	(0.112)**	(0.162)**	(0.181)***
Counterfactual ExecPayRatio^2	-0.098	-0.261	-0.204	-0.098	-0.267	-0.259
	(0.037)**	(0.117)**	(0.125)*	(0.036)**	(0.110)**	(0.121)**
Counterfactual ExecPayRatio^3	0.010	0.033	0.023	0.010	0.033	0.031
	(0.003)**	(0.017)**	(0.018)*	(0.003)**	(0.016)**	(0.018)**
% of CEO-BOD(s) who is CEOs and	-0.012	-0.035	-0.054	-0.011	-0.033	-0.051
	(0.015)	(0.050)	(0.058)	(0.015)	(0.048)	(0.058)
Number of Observations	2079	1917	1782	2181	2016	1869

Panel B. Second-stage regressions with instrumental variables including *Counterfactual ExecPayRatio*, the squared and third-order terms of this ratio, and the percentage of CEO-BOD(s) who is also CEOs and Chairman of her/his own company

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	56.411	479.738	580.246	29.288	8.251	2.401
	(305.476)	(347.879)*	(344.381)**	(35.132)	(33.012)	(32.680)
$ExecPayRatio^2(t)$	-58.792	-174.528	-183.994	-17.066	-0.260	-2.104
	(165.671)	(130.896)*	(110.317)**	(19.294)	(12.230)	(10.027)
Sargan-Hansen test statistics [*p*-value]	1.599 [p = 0.450]	0.612 [p = 0.736]	0.163 [p = 0.922]	0.390 [p = 0.823]	0.129 [p = 0.938]	1.250 [p = 0.535]
Number of Observations	2079	1917	1782	2181	2016	1869
Inflection point	5	14	16	9	158	6

Table A5: Instrumental Variable (IV) Regressions - Firm Performance and *ExecPayRatio* (Alternative Configurations of Instruments Including Directors' Industry Experience)

This table reports instrumental variable (IV) regression estimation results, using GMM, from equations (4.1) and (4.2). The instruments include *Counterfactual ExecPayRatio* (based on CEO-BOD(s)), the squared and third-order terms of this ratio, and the percentage of CEO-BOD(s) in the same/similar industry (based on the first three-digit SIC codes). *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). The dependent variables of the first-stage models include *ExecPayRatio*, which is scaled by dividing by 10, and *ExecPayRatio*^{2}, which is scaled by dividing by 100. Panel A reports summary results of the first-stage models. Panel B reports summary results of the second-stage results of firm performance on *ExecPayRatio* with instrumental variables including *Counterfactual ExecPayRatio* (based on CEO-BOD(s)) and the squared and third-order terms of this ratio. The dependent variables in the second-stage models are industry-adjusted Tobin's *q* (reported in Models 1 to 3) and 1-year-ahead industry-adjusted *ROA* (reported in Models 4 to 6) at time *t*+1 (1-year ahead). Industry-adjusted Tobin's *q* and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). In Panel B, a test of overidentifying restrictions is performed using the Sargan-Hansen test statistics (with the *p*-value reported in square brackets). The control variables include the following. Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The estimated coefficients of the control variables are not reported but are available upon request. Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on 1-tailed tests for the regression results.

Panel A. First-stage regressions with instrumental variables including *Counterfactual ExecPayRatio*, the squared and third-order terms of this ratio, and the percentage of CEO-BOD(s) in the same/similar industry

Instrumental variable: *ExecPayRatio*	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Counterfactual ExecPayRatio	0.108	0.166	0.142	0.103	0.186	0.171
	(0.031)**	(0.052)**	(0.055)**	(0.031)**	(0.050)**	(0.054)**
Counterfactual ExecPayRatio^2	-0.047	-0.106	-0.076	-0.048	-0.110	-0.090
	(0.012)**	(0.036)**	(0.037)**	(0.012)**	(0.034)**	(0.036)**
Counterfactual ExecPayRatio^3	0.005	0.014	0.009	0.005	0.014	0.011
	(0.001)**	(0.005)**	(0.005)**	(0.001)**	(0.005)**	(0.005)**
% of CEO-BOD(s) in the same/similar	0.011	-0.046	-0.077	0.012	-0.036	-0.077
	(0.015)	(0.041)	(0.048)*	(0.014)	(0.040)	(0.049)*
Number of Observations	2079	1917	1782	2181	2016	1869

Instrumental variable: *ExecPayRatio*^{2}	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Counterfactual ExecPayRatio	0.245	0.460	0.448	0.237	0.501	0.553
	(0.117)**	(0.171)***	(0.184)***	(0.113)**	(0.162)***	(0.181)***
Counterfactual ExecPayRatio^2	-0.098	-0.260	-0.205	-0.098	-0.266	-0.260
	(0.037)***	(0.117)**	(0.125) *	(0.036) ***	(0.110)***	(0.121)**
Counterfactual ExecPayRatio^3	0.010	0.033	0.024	0.010	0.033	0.031
	(0.003)***	(0.017)**	(0.018)*	(0.003) ***	(0.163)**	(0.018)**
% of CEO-BOD(s) in the same/similar industry	0.016	-0.066	-0.122	0.013	-0.048	-0.108
	(0.020)	(0.137)	(0.163)	(0.019)	(0.130)	(0.164)
Number of Observations	2079	1917	1782	2181	2016	1869

Panel B. Second-stage regressions with instrumental variables including *Counterfactual ExecPayRatio*, the squared and third-order terms of this ratio, and the percentage of CEO-BOD(s) in the same/similar industry

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	424.593	495.356	593.829	66.561	11.911	16.991
	(690.308)	(347.592)*	(333.749)**	(76.416)	(32.739)	(28.187)
$ExecPayRatio^2(t)$	-249.995	-181.406	-187.172	-37.414	-1.437	-5.822
	(380.270)	(130.534)*	(107.003)**	(42.051)	(12.051)	(8.830)
Sargan-Hansen test statistics	0.519	0.619	0.151	0.045	0.159	0.376
[*p*-value]	[*p* = 0.771]	[*p* = 0.734]	[*p* = 0.927]	[*p* = 0.978]	[*p* = 0.924]	[*p* = 0.829]
Number of Observations	2079	1917	1782	2181	2016	1869
Inflection point	8	14	16	9	41	15

Table A6: Sensitivity (to Outliers) of the Relationship Between Firm Performance and *ExecPayRatio*

This table reports regression estimation results from equations (4.1) and (4.2). *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). In the regression models, *ExecPayRatio* is scaled by dividing by 10; hence, *ExecPayRatio*^{2} is scaled by dividing by 100. Panel A displays results based on the sample that trims observations with very low executive pay. Panel B displays results based on the sample that trims observations with very low (or high) executive pay. The dependent variables are industry-adjusted Tobin's q (reported in Models 1 to 3) and 1-year-ahead industry-adjusted *ROA* (reported in Models 4 to 6) at time t+1 (1-year ahead). Industry-adjusted Tobin's q and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). The control variables include year dummies, industry dummies (based on the Fama and French 12-industry classifications), Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The inflection point of the inverted *U* is computed as $-\delta_1/(2\delta_2)$ and multiplied by 100 (as the dependent variables are scaled by multiplying by 100); δ_1 is the coefficient of *ExecPayRatio*(t) and δ_2 is the coefficient of *ExecPayRatio*^{2}(t). "N/A" is reported if there is no inverted-*U*-shaped relationship and, hence, no inflection point of that inverted *U* to be found. The OLS regressions (Models 1 and 4) are estimated with standard errors corrected for clustering at the firm level. The panel regressions (Models 2 and 5) are estimated with firm fixed effects. The panel regressions (Models 3 and 6) are estimated with CEO-firm fixed effects. Adjusted R-squared is reported for the OLS regressions (Models 1 and 4) and within R-squared is reported for the panel regressions (Models 2, 3, 5, and 6). Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on one-tailed tests for the regression results and two-tailed tests for the χ^2 tests of differences in coefficients across equations.

Panel A. Sample that trims observations with very low executive pay

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
	OLS	firm FE	CEO-firm FE	OLS	firm FE	CEO-firm FE
Dependent variable	Tobin's $q(t+1)$	Tobin's $q(t+1)$	Tobin's $q(t+1)$	*ROA*(t+1)	*ROA*(t+1)	*ROA*(t+1)
ExecPayRatio(t)	10.065	9.787	8.358	0.223	0.394	0.219
	(5.392)**	(2.670)***	(2.603)***	(0.394)	(0.281)*	(0.291)
ExecPayRatio^2(t)	-0.736	-0.734	(0.538)	-0.002	-0.009	0.007
	(0.446)**	(0.274)***	(0.264)**	(0.040)	(0.029)	(0.030)
Lev	(142.342)	-54.980	-32.315	-8.783	-4.361	-1.911
	(10.329)***	(4.632)***	(5.023)***	(0.740)***	(0.443)***	(0.523)***
Firmage	-0.409	0.209	0.199	0.007	-0.013	0.040
	(0.134)***	(0.139)*	(0.245)	(0.008)	(0.014)	(0.027)*
firmsize	-0.690	-40.666	-45.366	0.738	-1.765	-2.729
	(1.290)	(0.980)***	(1.267)***	(0.099)***	(0.100)***	(0.140)***
capxat	189.308	117.121	48.648	8.313	7.730	5.790
	(28.578)***	(14.677)***	(14.912)***	(1.932)***	(1.539)***	(1.657)***
rdsale	147.756	6.171	-34.281	-34.904	(35.631)	-32.387
	(21.076)***	(9.850)	(10.177)***	(1.781)***	(0.992)***	(1.119)***
rddum	-5.717	-7.443	-6.693	-0.968	0.289	0.459
	(3.977)*	(2.892)***	(3.554)**	(0.240)***	(0.302)	(0.391)
HHI	6.648	-20.219	-13.309	-1.957	-5.460	-4.669
	(11.117)	(6.962)***	(8.527)*	(0.680)***	(0.730)***	(0.947)***
constant	43.775	358.071	403.807	-5.028	16.168	21.184
	(14.700)***	(9.563)***	(13.856)***	(1.447)***	(0.972)***	(1.514)***
Industry dummies	Yes	No	No	Yes	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.090	0.10	0.07	0.170	0.07	0.06
Number of Observations	30,943	30,943	30,943	32,021	32,021	32,021
Inflection Point	68	67	78	692	211	N/A
Hypothesis A:			$\chi^2(1) = 0.59$			$\chi^2(1) = 0.82$
δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			p-value = 0.443			p-value = 0.366
Hypothesis B:			$\chi^2(1) = 1.47$			$\chi^2(1) = 0.66$
δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			p-value = 0.226			p-value = 0.416
Joint Test of			$\chi^2(2) = 1.60$			$\chi^2(2) = 0.86$
Hypothesis A and Hypothesis B			p-value = 0.450			p-value = 0.651

Panel B. Sample that trims observations with very low (or high) executive pay

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)
	OLS	firm FE	CEO-firm FE	OLS	firm FE	CEO-firm FE
Dependent variable	Tobin's $q(t+1)$	Tobin's $q(t+1)$	Tobin's $q(t+1)$	$ROA(t+1)$	$ROA(t+1)$	$ROA(t+1)$
ExecPayRatio(*t*)	12.905	12.941	10.643	0.727	0.735	0.315
	(6.672)**	(3.026)***	(2.921)***	(0.520)*	(0.342)**	(0.353)
$ExecPayRatio^2(t)$	-0.848	-1.024	-0.790	-0.031	-0.037	0.004
	(0.554)*	(0.296)***	(0.277)***	(0.052)	(0.034)	(0.034)
lev	-132.548	-58.565	-32.795	-8.408	-4.122	-1.985
	(11.322)***	(4.788)***	(5.127)***	(0.816)***	(0.493)***	(0.575)***
firmage	-0.232	0.215	0.240	0.007	-0.026	0.018
	(0.138)**	(0.143)*	(0.243)	(0.009)	(0.015)**	(0.029)
firmsize	-0.046	-38.589	-41.945	0.766	-1.809	-2.744
	(1.383)	(1.064)***	(1.346)***	(0.106)***	(0.118)***	(0.161)***
capxat	175.462	86.479	25.324	8.200	7.534	5.204
	(28.950)***	(15.287)***	(15.317)**	(2.142)***	(1.730)***	(1.854)***
rdsale	172.270	-4.196	-26.418	-38.310	-39.523	-35.041
	(29.722)***	(13.194)	(13.102)**	(2.792)***	(1.466)***	(1.587)***
rddum	-2.370	-4.021	-4.111	-1.107	0.596	0.502
	(4.400)	(3.074)*	(3.761)	(0.273)***	(0.346)**	(0.449)
HHI	12.714	-29.624	-15.899	-1.459	-4.866	-4.013
	(11.687)	(7.137)***	(8.600)**	(0.714)**	(0.806)***	(1.037)***
constant	33.207	340.510	371.335	-6.241	16.649	21.915
	(16.795)**	(10.236)***	(14.507)***	(1.707)***	(1.126)***	(1.722)***
Industry dummies	Yes	No	No	Yes	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.08	0.09	0.07	0.14	0.06	0.05
Number of Observations	24,638	24,638	24,638	25,558	25,558	25,558
Inflection Point	76	63	67	116	100	N/A
Hypothesis A:			$\chi^2(1) = 1.17$			$\chi^2(1) = 3.59$*
δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			*p*-value = 0.279			*p*-value = 0.058
Hypothesis B:			$\chi^2(1) = 2.27$			$\chi^2(1) = 5.76$**
δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			*p*-value = 0.132			*p*-value = 0.016
Joint Test of			$\chi^2(2) = 3.83$			$\chi^2(2) = 7.08$**
Hypothesis A and Hypothesis B			*p*-value = 0.147			*p*-value = 0.029

Table A7: Relationship Between Firm Performance and *ExecPayRatio* (Including Firm-CEO-COO Fixed Effects)

This table reports regression estimation results using CEO-COO-firm fixed effects. *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). In the regression models, *ExecPayRatio* is scaled by dividing by 10; hence, *ExecPayRatio*^{2} is scaled by dividing by 100. This table displays results of the unwinsorized pay ratio. The dependent variables are industry-adjusted Tobin's *q* (reported in Models 1 to 3) and 1-year-ahead industry-adjusted *ROA* (reported in Models 4 to 6) at time *t*+1 (1-year ahead). Industry-adjusted Tobin's *q* and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). The control variables include year dummies, industry dummies (based on the Fama and French 12-industry classifications), Leverage (*lev*) is long-term debt divided by total assets. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale*), the dummy variable *rddum* indicates missing observations on R&D, and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The inflection point of the inverted *U* is computed as $-\delta_1/(2\delta_2)$ and multiplied by 100 (as the dependent variables are scaled by multiplying by 100); δ_1 is the coefficient of *ExecPayRatio*(*t*) and δ_2 is the coefficient of *ExecPayRatio*^{2}(*t*). The panel regressions (Models 1 and 2) are estimated with CEO-COO-firm fixed effects. Within R-squared is reported for the panel regressions. Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on one-tailed tests for the regression results.

	Model (1) CEO-COO-firm FE	Model (2) CEO-COO-firm FE
Dependent variable	Tobin's *q*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	8.216	0.478
	(2.681)***	(0.301)*
ExecPayRatio^2(*t*)	-0.503	-0.011
	(0.253)**	(0.029)
lev	-25.526	-1.122
	(5.385)***	(0.566)**
firmage	0.073	0.022
	(0.269)	(0.029)
firmsize	-47.00632	-2.854
	(1.428)***	(0.158)***
capxat	30.723	5.178
	(15.885)**	(1.780)***
rdsale	-11.248	-27.884
	(10.767)	(1.187)***
rddum	-6.003	0.714
	(3.885)*	(0.431)**
HHI	-12.156	-4.280
	(9.368)*	(1.049)***
constant	419.423	21.881
	(15.464)***	(1.701)***
Industry dummies	No	No
Year dummies	Yes	Yes
R-squared	0.07	0.05
Number of Observations	30,956	32,034
Inflection Point	82	218

Table A8: Relationship Between Firm Performance and *ExecPayRatio* (Accounting for Missing R&D Values)

This table reports regression estimation results from equations (4.1) and (4.2) using the unwinsorized pay ratio. *ExecPayRatio* is the ratio of CEO total pay (*CEOpay*) to the average pay for the non-CEO executives (*AvPayExec*). In the regression models, *ExecPayRatio* is scaled by dividing by 10; hence, *ExecPayRatio*^{2} is scaled by dividing by 100. The dependent variables are industry-adjusted Tobin's *q* (reported in Models 1 to 3) and 1-year-ahead industry-adjusted *ROA* (reported in Models 4 to 6) at time *t*+1 (1-year ahead). Industry-adjusted Tobin's *q* and industry-adjusted *ROA* are scaled by multiplying by 100 (i.e., expressed as percentages). The control variables include year dummies, industry dummies (based on the Fama and French 12-industry classifications), Leverage (*lev*) is long-term debt divided by total assets. Research and development (R&D) expenditure is the R&D-development-expense-to-net-sales ratio (*rdsale**), with missing R&D values replaced with the industry average of reported R&D values. The dummy variable *rddum* indicates missing observations on R&D. The dummy variable *patent_dum* indicate pseudo-blank firms with no patent filings where patent data are obtained from the WRDS US Patents databases. Firm age (*firmage*) is the number of years since listing. Firm size (*firmsize*) is the natural log of total assets. Capital expenditure is the capital-expenditure-to-total-asset ratio (*capxat*), and industry concentration is measured by the net sales-based Herfindahl-Hirschman Index (*HHI*) for each year and each industry, using the Standard Industrial Classification (SIC) 2-digit code. The inflection point of the inverted *U* is computed as $-\delta_1/(2\delta_2)$ and multiplied by 100 (as the dependent variables are scaled by multiplying by 100); δ_1 is the coefficient of *ExecPayRatio*(*t*) and δ_2 is the coefficient of *ExecPayRatio*^{2}(*t*). "N/A" is reported if there is no inverted-*U*-shaped relationship and, hence, no inflection point of that inverted *U* to be found. The OLS regressions (Models 1 and 4) are estimated with standard errors corrected for clustering at the firm level. The panel regressions (Models 2 and 5) are estimated with firm fixed effects. The panel regressions (Models 3 and 6) are estimated with CEO-firm fixed effects. Adjusted R-squared is reported for the OLS regressions (Models 1 and 4) and within R-squared is reported for the panel regressions (Models 2, 3, 5, and 6). Standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively, based on one-tailed tests for the regression results and two-tailed tests for the χ^2 tests of differences in coefficients across equations.

	Model (1) OLS	Model (2) firm FE	Model (3) CEO-firm FE	Model (4) OLS	Model (5) firm FE	Model (6) CEO-firm FE
Dependent variable	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	Tobin's *q*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)	*ROA*(*t*+1)
ExecPayRatio(*t*)	10.223	9.957	8.665	0.294	0.410	0.248
	(5.326)**	(2.662)***	(2.595)***	(0.411)	(0.279)*	(0.289)
ExecPayRatio^2(*t*)	-0.811	-0.773	-0.617	-0.014	-0.013	0.002
	(0.419)**	(0.258)***	(0.246)***	(0.036)	(0.027)	(0.028)
lev	-141.746	-55.141	-32.735	-8.541	-4.352	-1.899
	(10.323)***	(4.639)***	(5.032)***	(0.743)***	(0.444)***	(0.523)***
firmage	-0.437	0.210	0.202	0.004	-0.014	0.041
	(0.134)***	(0.139)*	(0.245)	(0.008)	(0.014)	(0.027)*
firmsize	-1.727	-40.770	-45.393	0.652	-1.78851	-2.739
	(1.284)*	(0.982)***	(1.268)***	(0.099)***	(0.100)***	(0.139)***
capxat	198.604	117.325	48.968	8.122	7.769	5.912
	(28.668)***	(14.695)***	(14.936)***	(1.941)***	(1.538)***	(1.657)***
*rdsale**	146.608	2.858	-28.386	-35.655	-34.776	-31.344
	(21.172)***	(9.696)	(10.002)***	(1.819)***	(0.978)***	(1.098)***
rddum	-8.550	-7.629	-5.721	0.637	1.453	1.545
	(3.937)**	(2.895)***	(3.563)*	(0.236)***	(0.301)***	(0.392)***
patent_dum	15.293	-0.857	-2.220	1.371	0.491	0.253
	(3.497)***	(1.532)	(1.561)*	(0.248)***	(0.160)***	(0.173)*
HHI	5.565	-20.196	-13.535	-1.326	-5.283	-4.583
	(11.201)	(6.973)***	(8.547)*	(0.686)**	(0.730)***	(0.948)***
constant	46.966	359.410	404.456	-4.063	15.976	20.914
	(14.755)***	(9.587)***	(13.875)***	(1.421)***	(0.973)***	(1.513)***
Industry dummies	Yes	No	No	Yes	No	No
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.10	0.07	0.17	0.07	0.06
Number of Observations	30,956	30,956	30,956	32,034	32,034	32,034
Inflection Point	63	64	70	103	164	N/A
Hypothesis A:			$\chi^2(1) = 0.48$			$\chi^2(1) = 0.71$
δ_1 (firm FE) – δ_1 (CEO-firm FE) = 0			*p*-value = 0.486			*p*-value = 0.400
Hypothesis B:			$\chi^2(1) = 1.12$			$\chi^2(1) = 0.69$
δ_2 (firm FE) – δ_2 (CEO-firm FE) = 0			*p*-value = 0.290			*p*-value = 0.408
Joint Test of			$\chi^2(2) = 1.28$			$\chi^2(2) = 0.78$
Hypothesis A and Hypothesis B			*p*-value = 0.527			*p*-value = 0.679

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