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RESEARCH ARTICLE

Revisiting the gender gap in CEO compensation: Replication and extension of Hill, Upadhyay, and Beekun's (2015) work on CEO gender pay gap

Vishal K. Gupta[1] | Sandra C. Mortal[2] | Xiaohu Guo[2]

[1]Department of Management, Culverhouse College of Business, University of Alabama, Tuscaloosa, Alabama

[2]Department of Economics, Finance, and Legal Studies (EFLS), Culverhouse College of Business, University of Alabama, Tuscaloosa, Alabama

Correspondence
Vishal K. Gupta, 171 Alston Hall, Culverhouse College of Business, The University of Alabama, Tuscaloosa, AL 35487.
Email: vkgupta@cba.ua.edu

Research Summary: The increasing number of women chief executives motivates considerable interest in examining possible gender differences in CEO compensation. Recently, Hill, Upadhyay and Beekun reported that female CEOs receive greater compensation than male CEOs, which runs counter to common wisdom that the gender pay gap in the labor market favors men over women. With the goal of contributing to cumulative knowledge development in this area, we seek to reexamine Hill et al.'s finding about gender differences in CEO compensation by extending the analyses further in time, using a larger sample of firms and more rigorous empirical analyses. Our findings, which are robust to different statistical procedures and econometric specifications, do not reveal reliable evidence for differences in compensation paid to male and female CEOs.

Managerial Summary: For years, a lively debate has centered on the issue of gender pay gap. The ubiquity of the pay gap between men and women has recently been questioned by Hill et al. who identify the chief executive officer (CEO) role as a workplace position where women receive greater compensation than men. Our investigation examines whether women CEOs are indeed compensated substantively more than male CEOs. We seek to replicate earlier work by Hill and colleagues, using an expanded dataset over a longer period of time and with more rigorous analytical tools. We do not find reliable evidence for a difference in compensation paid to male and female CEOs, suggesting that claims about gender gap in CEO compensation favoring women over men may be premature.

KEYWORDS

CEO compensation, gender pay gap, pay equity, replication, women

Although women remain underrepresented in chief executive officer (CEO) positions, the number of women CEOs in public firms has gradually increased over time (Donovan, 2015). The growing numbers of women in the CEO role has stimulated considerable interest in understanding the similarities and differences in compensation paid to male and female CEOs. Recently, Hill, Upadhyay, and Beekun (2015; henceforth HUB) theorized and found that women CEOs receive substantively higher pay than male CEOs. HUB's intriguing claim has motivated discussions of a "female premium" in upper-echelon compensation (Leslie, Manchester, & Dahm, 2017, p. 402), gaining significant momentum in the academic community within a short time. Yet, some have raised concerns that by terminating their sample in 2005, HUB missed the significant uptick in women making it to the CEO level in subsequent years (Elsaid & Ursel, 2018). The present study reexamines HUB's finding of gender differences in CEO compensation by extending the analysis further in time (that is, up to 2014), using a larger sample and more rigorous empirical analyses. Our reexamination embraces what Bettis, Helfat, and Shaver (2016) describe as a "staged approach"—that is, closely imitating the research design described in HUB with the original sample (1996–2005) and an extended sample (1996–2014) with more rigorous methods. Following Goldfarb and King's (2016) advice to use subsamples to ensure the robustness of results, we also conduct separate analyses for the post-HUB (2006–2014) period.[1]

Our reexamination of HUB's finding on gender differences in CEO compensation favoring women makes several contributions to the literature. First, conducting a direct replication and extension of the original work provides additional empirical evidence to elucidate the gender dynamics of CEO compensation. This is important because, in contrast to the large literature on the gender pay gap in the labor market (Blau & Kahn, 2017), few systematic efforts have examined possible gender gaps in CEO compensation. Second, expanding the sampling period from 10 to 20 years introduces temporal contextualization to HUB's findings of pay bias favoring women CEOs. Considering Whetten's (1989, p. 492) iconic observation that researchers "should be encouraged to think about whether their theoretical effects vary over time," extending HUB's research on male–female CEO compensation over a longer time period may reveal novel insights with regard to the temporal context of CEO compensation. Finally, there is increasing recognition that "secure and established fields allow—and encourage—a constant regimen of replications, extensions and minor refinements" (Hambrick, 2004, pp. 94–95). We contribute to growing efforts in this direction, demonstrating the knowledge gained so far about how replication can help advance research and implementing Bettis et al.'s (2016) advice on conducting sound replication inquiry. Our replication study, both with the original sample and the expanded sample, provides a contrast to HUB's findings, suggesting that claims about gender gap in CEO compensation favoring women over men may be premature.

1 | PRIOR RESEARCH ON GENDER WAGE GAP

Studies conducted over the past few decades consistently reveal that women tend to be paid less than men in the general workforce (Blau & Kahn, 2017). The magnitude of this gap is debatable, but estimates suggest that on average women workers now earn about 75–90% of men's earnings in the United States. Notably, the gender pay gap has decreased over time (Gayle & Golan, 2012), perhaps in part because of social and legislative actions to encourage gender parity in organizational remuneration. Yet, women workers continue to be underpaid relative to their male counterparts. A recent meta-analysis of 30 years of research on the gender pay gap revealed that, when averaged over time, working women receive significantly lower rewards than men in comparable jobs (effect size = .56;

[1]We thank an anonymous reviewer for pointing us in this direction.

TABLE 1 Summary statistics for HUB and our two samples

PANEL A: Descriptive statistics for HUB and Our Samples

	HUB		Restricted sample (1996–2005)		Post-HUB sample (2006–2014)		Expanded sample (1996–2014)	
	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*
Log (CEO compensation)	7.88	1.07	7.85	1.08	8.27	0.95	8.04	1.03
Female CEO	0.02	0.12	0.02	0.12	0.04	0.18	0.02	0.15
Board size	9.05	2.53	9.09	2.47	8.99	2.06	9.03	2.26
Board independence	0.63	0.18	0.64	0.18	0.78	0.12	0.70	0.17
Officer and board ownership	0.10	0.17	0.10	0.14	0.07	0.12	0.09	0.13
CEO age	55.76	7.53	57.50	10.30	56.35	7.23	57.10	8.88
CEO tenure	7.21	7.60	7.49	7.39	8.59	7.25	8.29	7.36
Firm size (*x 1 Mill.*)	5,432	23,656	4,675	10,555	7,527	16,791	6,127	14,646
ROA	0.11	0.11	0.10	0.09	0.10	0.08	0.10	0.09
Equity return	0.16	0.46	0.15	0.50	0.13	0.42	0.14	0.45
Firm risk	0.44	0.19	0.45	0.20	0.40	0.15	0.42	0.18
SOX dummy	0.36	0.48	0.30	0.46	–	–	0.63	0.48

PANEL B: Descriptive Statistics for Both Samples by Gender

	Restricted sample (1996–2005)			Post-HUB sample (2006–2014)			Expanded sample (1996–2014)		
	Male CEO	Female CEO	Difference (*p*-value)	Male CEO	Female CEO	Difference	Male CEO	Female CEO	Difference (*p*-value)
Log (CEO compensation)	7.85	7.83	0.02 (0.73)	8.26	8.37	−0.11 (0.04)	8.04	8.19	−0.15 (0.00)
Board size	9.11	8.16	0.95 (0.00)	8.99	9.04	−0.05 (0.66)	9.03	8.76	0.27 (0.01)
Board independence	0.64	0.65	−0.01 (0.31)	0.78	0.80	−0.02 (0.00)	0.70	0.75	−0.05 (0.00)
Officer & Board Ownership	0.10	0.10	0.00 (0.55)	0.07	0.07	0.00 (0.21)	0.09	0.08	0.01 (0.15)
CEO age	57.57	53.17	4.40 (0.00)	56.43	54.23	2.20 (0.00)	57.18	53.95	3.23 (0.00)
CEO tenure	7.51	5.39	2.12 (0.00)	8.69	6.02	2.67 (0.00)	8.35	6.01	2.34 (0.00)
Log (firm size)	7.28	6.74	0.54 (0.00)	7.69	7.76	−0.07 (0.38)	7.47	7.44	0.03 (0.61)
ROA	0.10	0.09	0.01 (0.67)	0.10	0.10	0.00 (0.82)	0.10	0.10	0.00 (0.71)
Equity return	0.15	0.07	0.08 (0.04)	0.13	0.13	0.00 (0.88)	0.14	0.12	0.02 (0.23)
Firm risk	0.44	0.50	−0.06 (0.00)	0.40	0.40	0.00 (0.96)	0.42	0.43	−0.01 (0.46)
SOX dummy	0.30	0.39	−0.09 (0.01)	–	–	–	0.63	0.81	−0.18 (0.00)

Note. Columns shaded grey in Panel A are from HUB's Table 1. For the restricted sample: N = 10,940 observations with 1,853 unique firms of which 169 are female-firm-year observations and 47 are unique female led-firms. Officer and board ownership has 8,994 firm-year observations due to missing values for that variable. For Post-HUB sample: N = 9,043 observations with 1,507 unique firms of which 319 are female-firm-year observations and 79 unique female-led firms; For the expanded sample: N = 19,170 firm-year observations with 2,282 unique firms of which 469 are female-firm-year observation and 105 unique female-led firms. All variables are winsorized at the 1 and 99%. *p*-values in parentheses. ROA = return on assets; SOX = Sarbanes-Oxley act.

Joshi, Son, & Roh, 2015, p. 1529), leading the authors to question whether women can ever close the "gap in rewards in the workplace."

There is also a vibrant literature on gender pay gap in managerial roles, albeit with mixed evidence. Some studies reveal that male managers receive more pay than female managers (specifically, 33% higher; Bertrand & Hallock, 2001, p. 5), and that this wage gap is greater than the gender pay gap in the general labor force (Jacobs, 1992). Others, however, find that after controlling for human capital and organizational characteristics, the pay gap in managerial compensation favors women ("female premium"; Gayle, Golan, & Miller, 2012, p. 829), presumably because of the relatively stronger demand for (and lower supply of) high-potential women managers compared to male managers. Most recently, Leslie et al. (2017: Study 3) showed that the female premium in managerial compensation exists only in industries that place a greater emphasis on diversity (e.g., consumer goods), but not in industries that have less emphasis on diversity goals (e.g., manufacturing).

Little research has examined the gender pay gap at the CEO level so far, perhaps because until recently, few women actually made it to the top of the organizational hierarchy. Some work in this area simply compared the mean compensation of male and female CEOs without appropriate consideration to control variables. For example, Bertrand and Hallock (2001, p. 10) found that female CEOs are paid more than male CEOs (specifically, female/male pay ratio = 1.75), while Gayle et al. (2012) found no difference between male and female CEO compensation at traditional levels of significance (specifically, M_{men} = 3,795 and M_{women} = 2,992, both in 000 's). Researchers using regression-based approaches to control for relevant factors revealed nonsignificant differences in male and female CEO compensation, whether in a broad sample of large publicly traded firms (Adams, Gupta, Haughton, & Leeth, 2007) or in a matched sample of male- and female-led firms (Bugeja, Matolcsy, & Spiropoulos, 2012). Outside the United States, researchers found that male CEOs were paid more than female CEOs in China (Lam, McGuinness, & Vieito, 2013), but the two groups were paid similarly in the United Kingdom (Geiler & Renneboog, 2015). It was in this zeitgeist that HUB argued for (and found) a pay gap favoring women CEOs in the United States. Given the limited previous work in this area, HUB's finding is gaining traction in the literature (as reflected in the 30 Google Scholar citations it has received as of March 15, 2018, that is, within about 30 months of publication). Thus, it is to HUB's work that we now turn our attention.

1.1 | HUB summary

HUB recognized two divergent perspectives on gender differences in CEO compensation (namely, stereotyping logic and resource-based logic), both of which accept that women have traditionally not occupied leadership roles (Eagly & Carli, 2007). From a stereotyping perspective, the dearth of women leaders will cause people to hold biased beliefs about women who do become leaders ("lack-of-fit" effect; Heilman, 2001), so that compensation provided to women CEOs will not be commensurate with their true ability for the job. Resource-based logic, on the other hand, posits that the historical absence of women from leadership roles makes women CEOs a rare and inimitable resource for their firm,[2] which enables them to extract greater compensation for their services. The

[2]HUB offer a number of reasons why women CEOs may be a unique resource for the firm (p. 1119): (a) women bring to the CEO job different decision-making skills and ethical perspectives than men, assisting "the firm not only in understanding the needs of a diverse target market, but also in broadening decision-making"; (b) women surmount more barriers than men on their way up, so women who make it to the CEO position may be "particularly gifted and/or especially good at learning and/or dealing with adversity", (c) women CEOs serve as a highly visible and encouraging signal to employees about equity in human resource practices at the firm. Finally, HUB do not mention this, but it is also possible that women CEOs are seen as more valuable to the firm than male CEOs "because a feminized approach to managing others is increasingly viewed as a strength" (Rosette & Tost, 2010, p. 222).

TABLE 2 The effect of gender on CEO compensation for HUB, restricted, and expanded samples

		Restricted sample (1996–2005)			Post-HUB sample (2006–2014)		Expanded sample (1996–2014)	
	HUB	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female CEO	0.06	0.06	0.02	0.06	−0.05	0.03	0.01	0.04
	($p < .05$)	(0.56)	(0.84)	(0.57)	(0.50)	(0.66)	(0.83)	(0.49)
Log (firm size)	0.54	0.49	0.42	0.48	0.32	0.40	0.36	0.45
	($p < .001$)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
ROA	0.84	1.67	1.74	1.39	1.55	1.06	1.84	1.39
	($p < .001$)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Equity returns	0.01	0.06	0.09	0.14	0.09	0.14	0.11	0.15
	($p < .001$)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Firm risk	0.35	0.37	0.13	0.73	−0.04	0.20	0.13	0.58
	($p < .001$)	(0.00)	(0.36)	(0.00)	(0.68)	(0.02)	(0.11)	(0.00)
SOX dummy	−0.02	−0.00						
	($p > .10$)	(0.90)						
CEO tenure	−0.01	−0.01	−0.01	−0.01	0.00	−0.01	−0.00	−0.01
	($p < .01$)	(0.00)	(0.02)	(0.00)	(0.38)	(0.05)	(0.38)	(0.00)
CEO-chair duality	0.04	−0.00	0.04	0.10	0.06	0.09	0.05	0.12
	($p < .1$)	(0.96)	(0.22)	(0.00)	(0.05)	(0.00)	(0.04)	(0.00)
Board size	−0.14	−0.00	−0.00	0.01	−0.00	0.03	−0.01	0.01
	($p < .05$)	(0.52)	(0.59)	(0.33)	(0.56)	(0.00)	(0.22)	(0.04)
Board independence	0.17	0.26	0.09	0.47	0.24	0.85	0.34	0.57
	($p < .05$)	(0.00)	(0.30)	(0.00)	(0.02)	(0.00)	(0.00)	(0.00)
Officer & Board Ownership	−0.25	−0.41						
	($p < .05$)	(0.00)						
Fixed effects	Firm	Firm	Firm/Yr	Ind/Yr	Firm/yr	Ind/Yr	Firm/Yr	Ind/Yr
Cluster (firm)	No	No	Yes	Yes	Yes	Yes	Yes	Yes
N	10,060	8,994	10,940	10,940	9,043	9,043	19,170	19,170
Adj. R^2	0.45	0.68	0.68	0.51	0.79	0.58	0.72	0.55

Note.The first column presents results from HUB (specifically Table 2, column 2, p. 1123). In HUB's regression: N = 10,060 firm-year observations with 2,255 unique firms. Regressions (1)–(3) present results from our sample built following HUB's description. In regression (1), N = 8,994 firm-year observations with 1,715 unique firms of which 151 are female-firm-year observations and 45 unique female-led firms; in regressions (2)–(3): N = 10,940 observations with 1,853 unique firms of which 169 are female-firm-year observations and 47 are unique female-led firms. Regressions (4)–(5) present results for the post-HUB sample: N = 9,043 firm-year observations with 1,507 unique firms of which 319 are female firm-year observations and 79 are unique female-led firms. Regressions (6)–(7) present results for the expanded sample: N = 19,107 firm-year observations with 2,282 unique firms of which 469 are female firm-year observations and 105 are unique female-led firms. *p*-values are in parentheses. At the bottom of the table, we indicate whether the regression includes fixed-effects and what kind (firm, industry and/or year fixed-effects). We also indicate whether standard errors are adjusted for firm-level clustering. Intercept is included, but untabulated. ROA = return on assets; SOX = Sarbanes-Oxley act.

two competing predictions—both grounded in established theory—were examined by HUB using a 10-year sample of ExecuComp firms (from 1996 to 2005), revealing that women CEOs "indeed benefit from their minority status" (approximately 6% higher compensation, Table 2 in HUB). In discussing these findings, HUB view higher compensation for female CEOs as a "symbolic gesture" that captures goodwill and attention of "various stakeholder groups" exerting pressure on firms to have women in senior management (p. 1119).

1.2 | Reexamining HUB

HUB's finding of female CEOs being paid more than male CEOs is not only consistent with resource-based logic (as discussed earlier), it is also "interesting" in that it challenges "something that is generally believed to be the case" (Davis, 2015, p. 182). Yet, there are several reasons to be skeptical of HUB's revelations about pay gap favoring women CEOs. First, given that the CEO position represents the "pinnacle of power, influence and leadership" in Corporate America and has traditionally been considered a "male bastion" (Ryan et al., 2016, p. 447), one would expect bias against women in such positions. Indeed, some research finds that investors react more favorably to appointment of male CEOs than female CEOs (Lee & James, 2007; but also see Cook & Glass, 2011), presumably because the former are perceived to be more competent at their jobs. Evidence indicates that the mere appointment of a female CEO can have a significant negative impact on stock prices of female-led firms in general (Dixon-Fowler, Ellstrand, & Johnson, 2013). Second, as discussed earlier, years of scholarship spanning diverse disciplines converges on the conclusion that, in general, women receive lower pay relative to men for the same kind of work (Blau & Kahn, 2017). O'Neill (2003) argued that as one moves up in the organizational hierarchy, there are increasing returns to skills like work experience, which women managers tend to have less of than their male counterparts. Third, Kanter's (1977) well-known tokenism theory argues that when a group represents less than 15% of the profession, its members will be subject to discriminatory and prejudicial behaviors. Women constitute about 5% of CEOs in US public firms (Donovan, 2015), and so tokenism theory would suggest discrimination and prejudice against women CEOs.

Given the academic and practical import of HUB's finding, we believe it is important to reexamine and extend their revelation about gender difference in CEO compensation. Our reexamination also provides an opportunity to confirm whether the findings on gender dynamics in CEO compensation are robust to different regression specifications. If women's rarity in the CEO role is a competitive advantage as HUB conjectured, then an increase in the number of women in the post-2005 era may at least partly erode the advantage they enjoy. Based on our discussion of the relevant literature on gender pay gap, including HUB's theorizing and inferences on male–female difference in CEO compensation, we pose the following research question to be examined in various sample periods:

> When it comes to CEO compensation, is there a premium or a penalty for women chief executives compared to male chief executives or are both male and female chief executives paid equally?

2 | METHODS

Motivated by Bettis et al. (2016), our investigation takes a sequential approach: In the first step, we focus on the same time period as HUB (that is, 1996–2005), generate a sample from the period after HUB (2006–2014), and create a combined sample to cover a longer time period (1996–2014). In our investigations, we sought to follow the original work as closely as possible with respect to various assumptions and aspects of data structure, thereby adhering closely to the description in HUB's method section. Yet, we also move beyond the constraints of HUB's approach, introducing data refinements and analytical enhancements that should enhance confidence in our findings.[3]

[3]Following the advice of Editor Helfat, we make our SAS and STATA codes available to the interested reader (see Appendix S1 online). Furthermore, those who wish to reexamine our findings are welcome to reach out to us for any help they may need with replicating the sample, analyses, and/or results.

2.1 | Sample

Following HUB, we start by drawing a sample of firms over a 10 year-period, from 1996 to 2005—we call it the "restricted sample." Data were obtained from four different datasets: ExecuComp for compensation data, RiskMetrics for data on board characteristics, Compustat for accounting data and equity market value, and CRSP for return information. Our sample contains 10,940 firm-year observations from 1,853 unique firms, of which there are 169 female-firm-year observations and 47 unique female-led firms. The number of firm-year observations is slightly higher than HUB's sample of 10,060 firm year observations. The number of unique firms in our sample is somewhat smaller than HUB's 2,255 unique firms. The number of female-firm-year observations is comparable to HUB's 176 female-CEO years. Information about number of unique firms with women CEOs is absent from HUB, but Elsaid and Ursel (2018, p. 157) estimate it to be "less than 50 female CEOs", which is consistent with our sample. The Appendix details how we obtain our sample.

Next, following Goldfarb and King's (2016) advice, and based on an anonymous reviewer's recommendation, we also generate a sample of firms from the 2006 to 2014 time period—we call it the "post-HUB sample." This sample contains 9,043 firm-year observations covering 1,507 unique firms with 319 firm-year female observations and 79 unique female-led firms.

We also create a combined sample to include data from 1996 to 2014, which increased both the total number of firm-year observations and the number of female CEOs—we call it the "expanded sample." Expanding the sample time-frame is desirable because we are able to more than double both the number of firm-year observations where a female is CEO and the number of unique firms with female CEOs. This should boost the power of our tests, and therefore increase the chances of confirming whether an association exists between CEO gender and compensation. All procedures for forming the post-HUB and expanded samples are identical to those in the restricted sample, with one caveat: The expanded and post-HUB samples (unlike the restricted sample) exclude observations where the CEO has been in office less than a year. This is because CEO pay in years when there is turnover tends to only reflect compensation for part of the year (results remain unchanged whether we include or exclude part-year compensation data). The final dataset for 1996 to 2014 time-period now contains 19,170 firm year observations covering 2,282 unique firms with 469 firm-year female observations and 105 unique female-CEO firms.[4]

2.2 | Measures

We follow HUB in constructing our variables. Total CEO compensation (data item TDC1)—salary, bonus, total value of restricted stocks granted, total value of stock options granted, long-term incentive payouts and all other annual compensation—is obtained from ExecuComp. The natural log of this variable is used as the dependent variable. The main independent variable of interest is CEO gender. *Female* is an indicator variable that takes value "1" if CEO is female and "0" if male. Data on CEO gender is from ExecuComp (data item GENDER).[5]

Following HUB, we controlled for various determinants of CEO pay. Two variables were obtained from Compustat: *Firm Size* as total assets (data item AT) and return on assets (*ROA*) for firm performance as Earnings before interest and tax (data item EBIT) scaled by total assets. One variable was from

[4]We note that the number of firm-year observations for the restricted (1996–2005) and post-HUB (2006–2014) samples does not add up to the number of observations for the expanded sample (1996–2014). This is because the latter two samples exclude observations where the CEO has been in office less than one year, but the restricted sample does not, as we followed HUB in generating the restricted sample.

[5]CEO gender data can also be obtained from RiskMetrics. Our results are qualitatively similar whether CEO gender data is from RiskMetrics or ExecuComp.

CRSP: *Firm Risk* is the standard deviation of monthly returns using the past 60 months of data (we multiply standard deviation by the square root of 12 to express it in yearly returns).[6] Three variables were obtained from ExecuComp: *CEO Age* (data item AGE); *CEO Tenure*, the number of days between fiscal year end date (obtained from data items FYEAR and FYR) and the date the CEO took office (data item BECAMECEO), scaled by 365 to express the variable in years[7]; and *Equity Return*, the 1-year total return to shareholders (data item TRS1YR). Four variables were obtained from RiskMetrics: *CEO-Chair Duality* is an indicator variable that takes the value of 1 if the CEO is also the board chairperson[8]; *Board Size* is the number of members on the board; *Board Independence* is the proportion of independent directors on the board; and *Officer & Board Ownership* is computed as the ratio of shares held by officers and directors to common shares outstanding. *SOX Dummy* is an indicator variable for the years after 2002, the years after Sarbanes-Oxley act (SOX) took effect. Following HUB, we also winsorized all continuous variables at the top and bottom 1%.[9]

2.3 | Analyses

Table 1 presents summary statistics. Panel A presents means and standard deviations for the HUB sample as well as our three samples (restricted, post-HUB, and expanded). Although the HUB sample and our restricted sample are similar, we do note some differences in firm size mean and standard deviation. We also observe substantive similarities between the various samples we draw. Panel B presents summary statistics for male and female CEOs for the restricted, post-HUB, and expanded samples. For the restricted sample, we observe that female-led firms are smaller and riskier than male-led firms, and have poorer equity returns and smaller board sizes. Female CEOs have shorter tenures and are younger than male CEOs. The SOX dummy is larger for female-led firms, suggesting more female CEOs after 2002 when SOX was introduced. In the post-HUB and expanded samples, we find that mean for total compensation is higher for female CEOs, who are typically younger and have shorter tenure. Female-led firms also have greater board independence than male-led firms. Once again, in the expanded sample female-led firms have smaller board sizes, and the SOX dummy is significantly larger for female CEOs.

Table 2 presents regression analysis to investigate the effect of CEO gender on compensation, keeping all other firm characteristics constant. All regressions in our tables include intercepts (albeit unreported). The first table column (shaded grey) presents relevant results from HUB for ready comparison. Regressions (1)–(3) contain results for our restricted sample, regressions (4)–(5) for our post-HUB sample and regressions (6)–(7) for our expanded sample. As in HUB, regression (1) includes firm-fixed effects to account for time-invariant unobservable firm characteristics that might affect CEO compensation.[10] Firm-fixed effects leverages the explanatory power of within-firm variation in explanatory variables, which in the present context means exploiting changes in CEOs (that is, CEO gender). In our sample, there are a total of 1,642 CEO changes for 1,075 unique firms, of which 63 are male-to-female changes and 17 are female-to-male changes.

[6]Our results remain unchanged regardless of whether we obtain 1-year return and standard deviation of returns from ExecuComp or CRSP. In robustness tests we require a minimum of 25 observations during the 60-month period for which we compute standard deviation of returns, and results remain unchanged.

[7]We replace missing values for data item BECAMECEO with the fiscal year end date the CEO first appeared in the dataset.

[8]We complete missing values of this variable with data from ExecuComp. We check the variable TITLEANN: we consider the CEO to also be the board chairperson if the variable contains the word "chmn," "chairman" or "co-chairman,, and does not contain key words "vice chairman" or "v-chmn" (or any other similar combination of words).

[9]Instead of dropping the outliers as one would do in trimming, winsorizing involves taking those same values that would otherwise have been trimmed and substituting them with the values that remain at the ends of the sorted, trimmed array.

[10]Readers will notice that our presentation of results is somewhat unconventional as regression 1 presents the full model (with controls and independent variable) without first presenting the controls-only model. We do so to follow HUB's reporting. Results for the regression with only control variables are available from us.

Regression (1) included officer and board ownership (proportion of equity held by directors and officers in the firm) but we excluded this variable in all other regressions because this data is only available for a limited time-period.[11] The coefficient on *Female CEOs* is identical for HUB and our regression 1 (that is, $\beta = 0.06$), suggesting that female CEOs earn 6% higher compensation. However, the *p*-values are very different: less than .05 for HUB and .56 for regression 1 in our sample. The remainder of the coefficients and *p*-values are comparable, though *p*-values were larger in our sample for CEO-Chair Duality, and Board Size.[12] Regression 2 includes year-fixed-effects to account for time variation in compensation (Murphy & Zabojnik 2004; we also drop the indicator variable for *SOX* to alleviate multicollinearity, though results remain identical if we include *SOX*).

HUB did not control for time, but we think it is important to do so because of simultaneous growth trends in CEO compensation and increasing ascent of women in CEO positions over the years. In addition, in regression 2 (and in all subsequent regressions), we report *p*-values based on standard errors adjusted for firm-level clustering. We find that the coefficient on compensation drops considerably and *p*-value increases to .84. Regression (3) presents results for industry and year-fixed effects.[13] While the coefficients on *Female CEO* now increase to 0.06, indicating that female CEOs earn 6% higher compensation, the associated *p*-value continues to be high, .57, suggesting that we cannot confidently infer that compensation of male and female CEOs differ.

Regressions (4)–(5) mirror regressions (2)–(3), but for the post-HUB (2006–2014) sample. In regressions (4) and (5), the magnitude of the coefficients for female CEOs drops to - 0.05 and 0.03, respectively, and corresponding *p*-values continue to be high (.50 and .66, respectively). Results for the expanded sample, regressions (6)–(7), are qualitatively similar to those presented for the HUB and post-HUB samples, regressions (2)–(5).

To provide greater confidence in our findings, we conduct additional analyses, not reported here for parsimony, but available from us. We include additional control variables believed to impact CEO compensation in previous research (e.g., Bugeja et al., 2012; Core, Guay, & Larcker, 2008). Specifically, we consider lagged value of *Equity Returns*, book-to-market ratio, and CEO ownership as additional controls in our regressions.[14] Results remain unchanged when these additional controls are included.

In the results we present, all continuous variables are winsorized at the top and bottom 1% levels. Results remain unchanged if we do not winsorize data. In addition, our results remain unchanged when we perform quantile regressions (25th, 50th, 75th, and 95th percentile), and our results are

[11]This variable is computed using data from RiskMetrics as the ratio of shares held by officers and directors to common shares outstanding. However, shares held is not available before 1998. We do not believe omitting this variable affects our results: Our results remain unchanged when we include this variable for the time-period for which we have data.

[12]Regarding *R*-square in Table 2, regression (1), the reason it is different from HUB is that the *R*-square we report includes the variation that is explained by firm-fixed effects, in addition to variation explained by the independent variables. When we compute *R*-square that only includes variation explained by independent variables, we obtain an *R*-square very similar to HUB's. We chose the *R*-square we present for comparability with the *R*-squares of other regressions that include industry and time dummies and time dummies only.

[13]Industry is defined as 4-digit SIC codes. We obtain SIC codes from Compustat. We use historical SIC code (data item HSIC), and the most recent SIC code (data item SIC) in the few instances when historical SIC code is unavailable. Many of the firms in our sample operate in multiple industries, and it is difficult to assign them to a single industry. For this reason, SIC codes in CRSP and Compustat often diverge, and there is some evidence that SIC codes from Compustat are more representative of industry groupings (Guenther & Rosman, 1994).

[14]Book to Market Ratio is computed as defined in Davis, Fama, and French (2000) and is the book value divided by market value of equity from Compustat. Book value of equity is stockholders' equity (data item SEQ) minus preferred stock plus deferred taxes and investment tax credit (data item TXDITC). Preferred stock is computed in the following order, depending on data availability: redemption value (data item PSTKRV), liquidation value (data item PSTKL) or stock capital (data item PSTK). Market value of equity is stock price at the end of fiscal year (data item PRCC_F) multiplied by shares outstanding (data item CSHO). *CEO Ownership* is from RiskMetrics.

robust to outlier sensitivity analysis using Cook's D.[15] Finally, we present inferences based on standard errors robust to firm-level clustering; results are identical if instead we cluster at the CEO level. Taken together, our analyses suggest that the absence of meaningful gender difference in CEO compensation is robust across the sample distribution and is not sensitive to outliers.

3 | DISCUSSION

Contending that women CEOs are a rare resource that creates unique value for the firm, HUB predicted and found that women CEOs in large publicly traded American firms receive greater pay than male CEOs. Using three samples constructed based on HUB's methodological description, we reexamine their results about the gender gap in CEO pay favoring women over men. Our sequenced reexamination fails to find reliable evidence to support HUB's conclusion that women CEOs receive higher compensation than male CEOs. More specifically, while we find that our replication of the main regression in HUB yields an identical coefficient on *Female CEO*, and the coefficients we obtain in the various other specifications are mostly positive, the *p*-values associated with those coefficients are very high, making it difficult to infer substantive pay differences between male and female CEOs. Notably, the lack of evidence for significant differences between male and female CEO compensation is consistent across three time periods: the original 1996–2005 period sampled by HUB, the 2006–2014 time period after HUB for which we were able to obtain data, and the combined total extended time period from 1996 to 2014. Thus, we are unable to reject the null hypothesis of no difference between male and female compensation, and do not find that time is an important contextual influence on gender dynamics in CEO compensation.

What explains the difference in findings between HUB and us? If one were to accept the position that methodological description in a journal paper is akin to a "recipe" so that with "sufficient information someone could replicate the study and get the same results, if they used exactly the same procedure and data" (Zhang & Shaw, 2012, p. 9), the differences between the sample we were able to obtain and the sample HUB report are disconcerting. The reality, however, is that method sections (no matter how well-crafted) are rarely, if ever, exhaustive and almost always have some key pieces of information missing (Schmidt, 2009). Seen in this light, we speculate that there may be some unstated aspect(s) of HUB's data collection and analyses not readily obvious from their write-up. Consequently, it is possible that our results are different from HUB because, despite adhering to their methodological description, we were not able to obtain the exact same dataset as they report. Given the observation that replication of significant results would likely fail in about half of the published strategy studies (Goldfarb & King, 2016), our inability to replicate HUB's finding about gender differences in CEO compensation should not be very surprising (though it is concerning).

Whatever its cause, the discrepancy between HUB and us raises questions about the robustness of the so-called "female premium" in CEO compensation. Our finding about the lack of gender differences in CEO compensation also runs counter to the common wisdom that "women earn substantially less than similarly qualified men working in the same organizational positions" (Abraham, 2017, p. 29). Illuminating the circumstances in which there is gender parity in compensation is an important issue because of the widely shared concern that the "gender gap in earnings has proven both persistent and universal" (Lips, 2013, p. 169). Our research therefore contributes to empirical

[15]Cook's D is a statistic commonly used to detected influential outliers. It is common to drop observations with Cook's D above 1, however none of the observations in our sample have a Cook's D value above one. Another common rule is to drop observations with values above 4/*N*, where *N* is total number of observations: this rule resulted in dropping 793 observations. Our results remain qualitatively similar.

research on the gender pay gap in general, and CEO compensation in particular. In doing so, we were able to put into practice Bettis et al.'s (2016) advice about "staged replication" as the appropriate way to reexamine prior findings. As such, we also contribute to the "ongoing stream of methodological inquiry in strategy research" (Wiersema & Bowen, 2009, p. 688) and benefit strategic management researchers in their discussions about the place of replication in pursuing answers to questions important in their field (Goldfarb & King, 2016). We hope our research will stimulate greater interest and efforts to reexamine published articles based on archival data.

Our research also has some limitations that need to be acknowledged. First, we focus only on gender differences in CEO compensation, and do not delve into the internal and external forces that influence the remuneration CEOs receive. The possible role of internal factors such as hiring committees and external factors like compensation consultants in facilitating gender parity in CEO remuneration may be interesting avenues for future scholarship. Second, reexamination efforts such as ours provide additional evidence about the validity of the findings derived in the original investigation, but do not speak directly to the theoretical tenets that inform the predictions in the first place. Thus, the present study casts light on HUB's contention of a "female premium" in CEO compensation, yet does not address their underlying logic for expecting the pattern of results they reported. Finally, the applicability of our findings to male–female compensation in other upper-echelon positions (e.g., CFO, COO) or in other firms (e.g., non-S&P 1500 firms) or in other societies (e.g., less gender egalitarian societies like Spain and Mexico) is a matter for further inquiry, and cannot simply be assumed.

The issue of gender pay gap generates intense discussions in academic and everyday life. Our research suggests that, at least in public firms in the United States, there may not be a meaningful difference in male and female CEO compensation. This finding of a nonsignificant gender pay gap in CEO compensation, robust across various regression specifications and over different time periods, gains additional salience when one considers the common perception that "despite 50 years of political mobilization and dedicated public policy, the gender wage gap remains one of the most persistent form of workplace inequality" (Abraham, 2017, p. 29). Future research aimed at expanding our research to other executive roles (e.g., CFO or COO), and to other samples, types of firms or societies, will further extend the knowledge frontier about a gender pay gap in the C-suite.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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A. APPENDIX—DETAILS ON BUILDING OUR SAMPLE

A.1 RESTRICTED SAMPLE (1996–2005)

Because HUB do not discuss how they merge ExecuComp, RiskMetrics, Compustat and CRSP, we follow the algorithm described in Coles, Daniel, and Naveen (2014: Section A.2.1) for merging the datasets. Table A1 presents the number of observations we obtain at the various steps of the merging algorithm. We match firms in RiskMetrics using CUSIPs, tickers and names. These identifiers are susceptible to change over time. CRSP contains historical information on these identifiers, and thus we start by matching RiskMetrics with CRSP. We are able to match all firms in RiskMetrics with CRSP's security unique identifier (PERMNO). We have 15,996 firm-year observations and 2,974 unique firms.

Next, we match CRSP with Compustat using the PERMNO/GVKEY link file compiled by WRDS (GVKEY is Compustat's unique firm identifier). We are able to find PERMNO/GVKEY combinations for 15,660 firm-year observations and 2,850 unique firms. Both ExecuComp and Compustat have a common unique firm identifier, GVKEY, which we use to match both datasets. The resulting dataset contains 13,884 firm-year observations and 2,368 unique firms.[16] Utilities and financial firms (SIC codes starting with 49 and 6) were excluded from consideration.[17] For a firm-year observation to be included in the sample, it was required that data be available for all variables in regressions. The final dataset contained 10,940 firm-year observations covering 1,853 unique firms. The HUB's sample contains 10,060 firm-year observations and 2,255 unique firms.

ExecuComp alone contains 2,105 unique firms with data on CEO compensation that are not in the financial or utility industries, about 6% fewer than the number of unique observations in HUB's sample of 2,255 unique firms. Note that our numbers are before we merge ExecuComp with the other three datasets, which naturally resulted in loss of some observations, whereas HUB's sample is after the datasets have been merged. In our case, after we merge the four datasets together and after excluding observations with no data for study variables (the variables included in regressions) we have 1,853 unique firm observations.

[16]One of the authors of Coles et al. (2014)—Lalitha Naveen—posted on her website a file linking risk-metrics with CRSP PERMNOS and Compustat GVKEYS. Our matching algorithm produces a combined dataset similar to theirs. See https://sites.temple.edu/lnaveen/data/.

[17]In excluding utilities and financial firms from their sample, HUB cite "differences in regulations and policies within these firms" (p. 1120), so we also excluded these firms from the sample.

TABLE A1 Sample size for HUB and our restricted sample (1996–2005)

	Firm-year Obs.	Unique firms	Female Firm-year Obs.	Unique female firms
ExecuComp				
Whole sample	19,198	2,694		
Financial and utility firms excluded	15,341	2,109		
With data on CEO Total compensation	14,695	2,105	250	62
RiskMetrics				
Whole sample	15,996	2,974		
Matched with PERMNO	15,996	2,974		
Matched with GVKEY	15,660	2,850		
Matched with ExecuComp	13,884	2,368	183	51
Financial and utility firms excluded	11,003	1,860	169	47
Final sample				
Our sample (with data on key variables)	10,940	1,853	169	47
HUB final sample	10,060	2,255	176	N/A
Percentage difference	−0.08	0.22	0.04	

Note. Sample size at various stages of merging four different datasets: ExecuComp, RiskMetrics, CRSP, and Compustat. PERMNO is CRSP's unique identifier, and GVKEY is the unique identifier for Compustat and ExecuComp. We compare our final sample with HUB in the last three rows.

A.2 EXPANDED SAMPLE (1996–2014)

Table A2 presents the number of observations obtained at each step for building our final sample. RiskMetrics changed the methodology used to collect data in 2007, and as a result data on directors is provided in two datasets in WRDS: a "Directors Legacy" dataset that provides data from 1996 to 2006 and a "Directors" dataset with data after 2007 (including). The first three rows present the number of observations in each of these two datasets and their sum. We start with 29,238 firm-year observations in RiskMetrics. Merging RiskMetrics with CRSP results in 29,235 firm-year observations with matching PERMNOs. We are able to obtain GVKEYS for 28,647 firm-year observations. The

TABLE A2 Sample size for our expanded sample (1996–2014)

	Firm-year observations	Unique firms	Female firm-year Obs.	Unique female firms
RiskMetrics				
Sample 1996–2006	17,409	3,033		
Sample 2007–2014	11,829	1,947		
Whole sample	29,238	N/A		
Merged with CRSP (PERMNO)	29,235	3,594		
Merged with Compustat (GVKEY)	28,647	3,416		
Merged with ExecuComp	26,691	2,996	636	137
Financial and utility firms excluded	20,652	2,296	522	111
Final sample				
Our sample (with data on key variables)	20,566	2,282	470	105
With tenure ≥1	19,170	2,282	469	105

Note. Sample size at various stages of merging four different datasets: ExecuComp, Risk Metrics, CRSP, and Compustat. Sample size is reported separately for 1996 to 2006 and 2007 to 2014 because RiskMetrics has two parts corresponding to these years.

TABLE A3 Correlation table

	1	2	3	4	5	6	7	8	9	10	11	12
1.Log (CEO compensation)		0.01	0.30	0.19	−0.20	−0.04	−0.11	0.25	0.08	0.09	−0.08	0.10
2.Female CEO	−0.01		−0.05	0.00	0.01	−0.08	−0.04	−0.01	−0.01	−0.01	0.03	0.03
	(0.02)											
3.Board size	0.32	−0.04		0.07	−0.05	0.09	−0.08	0.26	0.07	−0.04	−0.40	−0.07
	(0.38)	(−0.01)										
4.Board independence	0.19	0.02	0.08		−0.35	−0.03	−0.18	0.04	−0.05	−0.01	−0.02	0.20
	(0.27)	(0.05)	(0.10)									
5.Officer and board ownership	−0.24	0.01	−0.05	−0.41		0.06	0.22	−0.07	−0.03	0.02	0.05	−0.05
	(−0.26)	(−0.01)	(−0.06)	(−0.40)								
6.CEO age	−0.04	−0.06	0.09	−0.02	0.06		0.41	0.01	0.05	−0.02	−0.20	−0.03
	(−0.02)	(−0.06)	(0.09)	(−0.07)	(0.08)							
7.CEO tenure	−0.10	−0.04	−0.07	−0.16	0.19	0.26		−0.04	0.05	0.02	0.01	−0.02
	(−0.09)	(−0.05)	(−0.01)	(−0.14)	(0.20)	(0.33)						
8.Log (firm size)	0.42	−0.01	0.43	0.09	−0.12	0.05	−0.06		−0.02	−0.02	−0.12	0.03
	(0.67)	(0.00)	(0.58)	(0.21)	(−0.21)	(0.05)	(−0.09)					
9.ROA	0.17	−0.01	0.11	−0.01	0.02	0.00	0.05	0.02		0.16	−0.43	0.02
	(0.16)	(0.00)	(0.08)	(0.00)	(0.00)	(−0.01)	(0.02)	(0.07)				
10.Equity return	0.09	−0.02	−0.05	0.01	−0.02	−0.06	0.02	−0.02	0.18		−0.02	0.14
	(0.09)	(−0.01)	(−0.02)	(0.00)	(−0.01)	(−0.03)	(0.01)	(0.01)	(0.16)			
11.Firm risk	−0.11	0.03	−0.42	−0.03	−0.01	−0.15	0.01	−0.22	−0.35	0.04		0.13
	(−0.21)	(0.00)	(−0.36)	(−0.12)	(0.04)	(−0.11)	(0.01)	(−0.38)	(−0.30)	(0.06)		
12.SOX dummy	0.08	0.02	−0.02	0.21	−0.06	−0.02	0.02	0.03	0.05	0.18	0.12	
	(0.17)	(0.05)	(0.00)	(0.39)	(−0.10)	(−0.06)	(0.03)	(0.12)	(0.06)	(0.10)	(−0.07)	

Note. Correlation coefficients for the HUB sample (shaded grey in the upper diagonal), the restricted sample (in the lower diagonal), and the expanded sample (in the lower diagonal in parentheses). ROA = return on assets; SOX = Sarbanes-Oxley act.

sample merged with ExecuComp results in 26,691 firm-year observations. As before, we drop firms in financial and utility industries (6,039 firm-year observations), and exclude observations with no data available on the variables of interest (the variables included in the regressions). In the final stage, we drop observations with CEO tenure less than 1 year. We do so for two reasons: (a) CEO pay in a turnover year is likely to reflect part-year compensation only, and (b) First-year CEOs may get higher than average stock compensation (to better align incentives) and higher bonuses (including signing bonuses). Our final expanded sample contains 19,170 firm-year observations, 2,282 unique firms, 469 female-firm-year observations, and 105 unique female led firms.

Table A3 presents correlation coefficients, both for our restricted and expanded samples (below diagonal, with coefficients in parentheses belonging to the expanded sample) and HUB (above diagonal). We note that, as expected, correlation coefficients from our restricted sample are comparable to HUB, and those from our expanded sample are also quite close to HUB.