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Masculinity, Testosterone, and Financial Misreporting

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ABSTRACT

We examine the relation between a measure of *male* CEOs' facial masculinity and financial misreporting. Facial masculinity is associated with a complex of masculine behaviors (including aggression, egocentrism, riskseeking, and maintenance of social status) in *males*. One possible mechanism for this relation is that the hormone testosterone influences both behavior and the development of the face shape. We document a positive association between CEO facial masculinity and various misreporting proxies in a broad sample of S&P1500 firms during 1996–2010. We complement this evidence by documenting that a CEO's facial masculinity predicts his firm's likelihood of being subject to an SEC enforcement action. We also show that an executive's facial

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masculinity is associated with the likelihood of the SEC naming him as a perpetrator. We find that facial masculinity is not a measure of overconfidence. Finally, we demonstrate that facial masculinity also predicts the incidence of insider trading and option backdating.

JEL codes: M12; M20; M41; M50

Keywords: misreporting; testosterone; facial masculinity; insider trading; option backdating

1. *Introduction*

Interdisciplinary collaborations between social scientists and biologists open up novel avenues for research. In particular, researchers interested in financial decision making have used biological measures to help explain (economic) behavior.[1] Even relatively simple metrics such as a person's height or body mass index appear to have explanatory value (see, e.g., Bodenhorn, Moehling, and Price [2012], Case and Paxson [2008], Korniotis and Kumar [2014]). We focus on a relatively recent line of biological research, which documents that an individual's facial structure is associated with a complex of related "masculine" behaviors including aggression, egocentrism, risk seeking, and a desire to maintain social status (Carré and McCormick [2008], Haselhuhn and Wong [2012], Stirrat and Perrett [2010]).

While in the strict neoclassical model, the personal characteristics of managers do not matter for corporate decision making, an emerging stream of work in accounting and finance recognizes that financial reporting practices vary predictably with specific individual traits of executives (Davidson, Dey, and Smith [2013], Dikolli, Mayew, and Steffen [2012], Schrand and Zechman [2012]). We argue that facial structures meaningfully capture variation in those CEO personal characteristics that are associated with financial reporting. We focus on the risk of materially misstated financial statements and test the hypothesis that it varies systematically with the degree to which the CEO is disposed to masculine behavior.

It is not obvious why the physical traits of individuals should be associated with their behavior, and much ongoing research attempts to unravel the mechanisms involved. The available evidence currently allows only speculations. One conjecture is that testosterone, a steroid hormone, underlies the association between facial structures and behavior (Lefevre et al. [2013]). Testosterone is thought to influence behavior because it shapes an individual's neural circuitry early in life and because the brain responds to changes in current testosterone levels. Testosterone not only affects brain development but is also associated with face shape. Some clinical studies have documented that testosterone administration causes craniofacial growth

[1] See, for example, Harlow and Brown [1990], Kuhnen and Knutson [2005], and Cesarini et al. [2009, 2010].

in adolescents (Verdonck et al. [1999]).[2] Other studies show positive correlations between more "masculine" facial features and testosterone (Lefevre et al. [2013]) but can only speculate about the exact mechanisms involved.

While the mechanisms linking facial features to masculine behavior are not yet fully understood and remain subject to debate in the neurosciences, the literature proposes that an individual's facial features can be used as a biological marker of masculine behavior (Stirrat and Perrett [2012]).[3] Future research might find evidence of a (causal) role of testosterone, but it is also possible that testosterone is not implicated at all. Indeed, behavior is a very complex and "plastic" expression of a multitude of factors, only partly hormonal or even biological.[4]

Nevertheless, the evidence that documents an association between facial "masculinity" and (masculine) behavior is strong, and we rely on it for our empirical strategy to examine the association between a CEO's facial shape and misreporting. Our strategy is related to the approach taken by Wong, Ormiston, and Haselhuhn [2011], who document a positive association between CEO facial masculinity and financial performance.[5]

We collect a sample of pictures of 1,136 CEOs from S&P1500 companies in 2009. We compute a facial structure metric, the facial width-to-height ratio (*fWHR*), by measuring the distance between cheekbones and the height of the upper face of each CEO from his picture. We then trace the full employment history of each CEO to construct a panel data set of 3,909 firm-years during the 1996–2010 period. We use a composite measure of the likelihood of accounting manipulation developed by Dechow et al. [2011] to identify firms that are likely to have materially misstated accounting reports. In our main set of analyses, we examine the relation between a given CEO's *fWHR* and the likelihood of having material accounting misstatements. We find that the likelihood for a substantial risk of misreporting is

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[2] For more evidence from clinical studies that testosterone alters craniofacial morphology, see, for example, Lindberg et al. [2005], Thornhill and Gangestad [1999], Thornhill and Moller [1997], and Vanderschueren and Boullion [1995]. These studies allow one to speculate that there might be a relation between a sexual dimorphism (i.e., difference between men and women) in the face structure and testosterone exposure in adolescence. Lefevre et al. [2013], however, note that the evidence on sexual dimorphism is weak. These authors propose that differences in testosterone may be more directly related to facial bone size within men (while other (hormonal) factors influence the face shape of women).

[3] Our sample comprises male CEOs only. Thus, throughout the paper, we only consider men and use the pronoun he.

[4] In particular, sociocultural practices can reshape behaviors, which highlights the need to include a range of environmental control variables in the empirical tests (Jablonka, Lamb, and Zeligowski [2005]). What's more, hormones are also "social" and are often studied as outcome variables to examine how they respond to social context.

[5] Some other authors have also used facial features in economic and/or corporate settings (Graham, Harvey, and Puri [2010], Rule and Ambady [2008]), but these studies pertain to perceptions of how competent a CEO looks rather than using the face to infer masculine behavior.

up to 98% higher for CEOs with above-median *fWHR*, representing more masculine faces, than for CEOs with below-median *fWHR*.

We conduct several additional analyses to probe whether our main findings indicate *intentional* misreporting *by* the CEO. We collect a sample of 164 CEOs from firms subject to SEC Accounting and Auditing Enforcement Releases (AAERs) between 2004 and 2010 and a sample of 164 CEOs from non-AAER firms matched by industry and size. In the AAER sample, we verify that firms indeed engaged in intentional misreporting. We document that the CEO's *fWHR* is positively associated with being subject to an AAER. In addition, we observe which executives are accused of wrongdoing in the AAERs *by name*. We find that CEOs with more masculine faces are also more likely to be named as a perpetrator. Finally, we collect the pictures of CFOs of the AAER firms and show that the facial structures of the CEO and CFO are associated with the likelihood of being accused by name. An attractive feature of this particular test is that for each CEO–CFO pair we can include firm-fixed effects and thus are able to control for unobserved time-invariant firm-level characteristics that might otherwise complicate inferences. In addition, the test confronts the concern that the effect is due to the firm rather than to the behavior of the executives. If the firm is the driving force, then we should not find differential effects between the facial structures of the CEO and the CFO at a single firm.

We document that the relation between our facial metric *fWHR* and misreporting is robust to including proxies for overconfidence, which prior work has identified as an explanatory factor of financial misreporting (Schrand and Zechman [2012]).

Finally, we examine the association between *fWHR* and two nonreporting corporate decisions. The behaviors associated with individuals who have more masculine faces should not only manifest themselves in financial reporting outcomes, but plausibly also increase the likelihood of executives engaging in other risky decisions. We use an algorithm developed by Cohen, Malloy, and Pomorski [2012] to classify our main (supplemental) sample of CEOs (CFOs) from S&P1500 firms based on their stock market transactions as "routine" and "opportunistic" traders. We show that executives with above-median (below-median) *fWHR* are more likely to be opportunistic (routine) traders. When we control for firm-fixed effects (using data on both the CEO and CFO from the same firm), the positive association remains between *fWHR* and the likelihood of being classified as an opportunistic trader. We also examine the relation between *fWHR* and the incidence of option backdating in the sample of S&P1500 firms. Based on an analysis of unusual stock price patterns around the option grant date in the sample period, we identify firms that backdated stock option grants to executives and report a positive association between *fWHR* and the incidence of backdating.

We make four contributions to the literature. First, prior work has documented the existence of CEO-fixed effects on corporate decision making (Bertrand and Schoar [2003], Fee, Hadlock, and Pierce [2013]). We introduce the idea that facial structure is important in describing the specifics of the "style" of a CEO's decision making. Although the literature has recognized that variations in style exist among CEOs, little progress has been made so far in understanding these different styles. While prior work has linked style to a CEO's military experience, overconfidence, religious beliefs, integrity, or criminal record, the "primitive" of a CEO's style remains uncharacterized. We argue that one productive way of thinking about variations in managerial style is to consider masculine behaviors that might be related to testosterone. Second, we introduce a facial masculinity metric to the accounting literature. Prior biology and psychology studies have documented that this facial metric predicts variation in masculine behaviors across individuals. The metric can be used in broad samples because it only requires a picture of the individual. Third, we show that the *fWHR* of CEOs is positively related to financial misreporting in multiple samples by using a range of misreporting proxies. We also find evidence that a given CEO's *fWHR* is positively associated with opportunistic insider trading and with the incidence of option backdating. Our results are both statistically and economically significant. Finally, we contribute to growing evidence in the neurosciences that facial structure contains cues to an individual's behavioral disposition (Carré, McCormick, and Mondloch [2009a], Geniole et al. [2012], Penton-Voak et al. [2006], Verplaetse, Vanneste, and Braeckman [2007]). This literature examines whether facial structure can be what biologist call an "honest signal" (i.e., a signal that reliably predicts information useful to the receiver). Our results show that the facial width-to-height ratio of senior executives is associated with the likelihood to engage in misreporting, which is consistent with facial structure predicting masculine behavior. This study is one of few to examine the predictive value of facial masculinity (measured from readily available, non–laboratory-standard pictures) in the naturalistic setting of the corporate world. A significant amount of evidence exists on the association between facial structure and behavior under controlled experimental conditions in which individuals conduct relatively simple tasks and from the context of sports. To date, however, it is an open question whether facial structures remain predictive in corporations where individual behavior is constrained by corporate governance structures and other internal controls. Our study shows that it is possible to tease out the effect of variation in facial structure even in an observational study in a "real-life" setting.

2. *Hypothesis Development*

2.1 CEO PERSONAL CHARACTERISTICS AND FINANCIAL REPORTING

While a vast literature has examined the determinants of misreporting (see, e.g., Dechow, Ge, and Schrand [2010], Fields, Lys, and Vincent [2001]), the number of studies that link accounting practices to the personal characteristics of senior management is small. Nevertheless, a growing literature originating in accounting, finance, and economics considers "managerial styles" and their effect on corporate actions (Brunnermeier, Bolton, and Veldkamp [2010], Borghans et al. [2008], Davidson, Dey, and Smith [2013], Fee, Hadlock, and Pierce [2013], Malmendier and Tate [2005]).

Some studies, following Bertrand and Schoar [2003], document the existence of CEO-fixed effects in financial reporting decisions and interpret this finding as evidence of a managerial style (Bamber, Jiang, and Wang [2010], Brochet, Faurel, and McVay [2011], DeJong and Ling [2013], Dyreng, Hanlon, and Maydew [2010], Feng et al. [2011], Ge, Matsumoto, and Zhang [2011], Zhang et al. [2012]). However, others hypothesize that this style is the outcome of the incentives these managers are facing rather than their personal characteristics (Chava and Purnanandam [2010], Jiang, Petroni, and Wang [2010]).

Other studies go a step further and examine a specific managerial style (e.g., "integrity" or "overconfidence") rather than document managerial fixed effects in financial reporting (Dikolli, Mayew, and Steffen [2012], Law and Mills [2014], Schrand and Zechman [2012]). These results are intriguing and suggest that a CEO's personal characteristics are associated with financial reporting (Davidson, Dey, and Smith [2013]). Nevertheless, given the available measures, it proves difficult to determine whether integrity or overconfidence are the *outcome* of corporate governance practices, incentives, and other situational factors or of something that is innate; that is, a stable, nonfakeable individual characteristic of the manager.

Graham, Harvey, and Puri [2013] take a significant step forward in ferreting out whether characteristics hard-wired into the personality of individuals[6] matter in corporate policies by administering psychometric tests of personality to a broad sample of CEOs. This study's method might be difficult for others to follow, however, because researchers need to have access to CEOs, who in turn need to be willing to submit to psychological testing.

In sum, while significant evidence exists that the personal characteristics of senior management are associated with financial reporting practices and other corporate policies, not much is known about the "primitive" forces at work behind those personal characteristics considered in the literature so far.

2.2 FACIAL STRUCTURE, TESTOSTERONE, AND BEHAVIOR

Prior studies have documented an association between the steroid hormone testosterone and a complex of related (masculine) behaviors. Figure 1 depicts a schematic representation of this literature. We cannot do full justice to the richness of this literature, but we do need to point out

[6] Evidence from twin studies suggests a genetic basis for variation in risk preferences (Cesarini et al. [2009]).

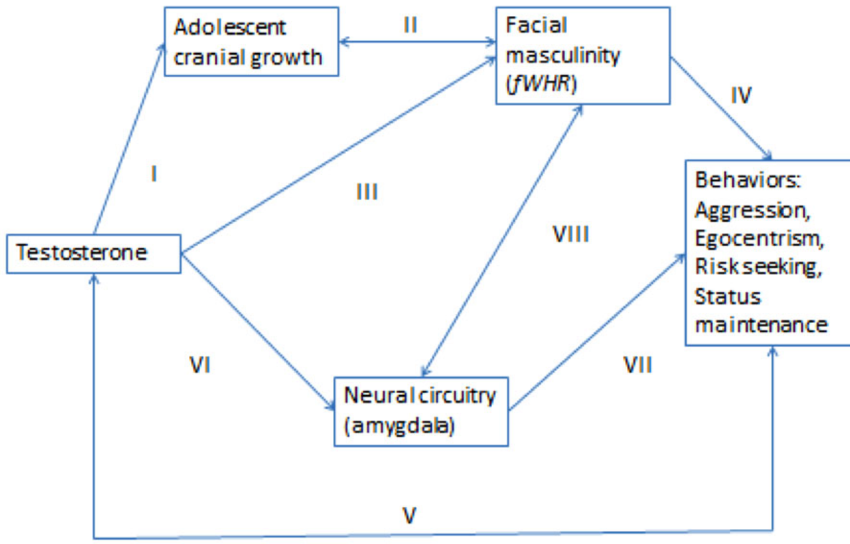

FIG. 1.—The relation between testosterone, craniofacial features, and behavior.
Underlying studies:
Mechanism [I] Testosterone → adolescent cranial growth
Verdonck et al. [1999], Nie [2005], Thornhill and Møller [1997], Thornhill and Gangestad [1999]; Lindberg et al. [2005]
Mechanism [II] Adolescent cranial growth ↔ facial masculinity
Weston, Friday and Liò [2007], Gómez-Valdéz et al. [2013]
Mechanism [III] (Circulating) testosterone → facial structure
Pound, Penton-Voak, and Surridge [2009], Penton-Voak and Chen [2004], Lefevre et al. [2013]
Mechanism [IV] Facial masculinity → behaviors
Carré and McCormick [2008], Haselhuhn and Wong [2012], Stirrat and Perrett [2010], Apicella et al. [2008], Wong, Ormiston, and Haselhuhn [2011], Lewis, Lefevre, and Bates [2012], Campbell et al. [2010]
Mechanism [V] Testosterone ↔ behaviors
Mehta, Jones and Josephs [2008], Pound, Penton-Voak, and Surridge [2009], Eisenegger et al. [2010], Wright et al. [2012],
Mechanism [VI] Testosterone → neural circuitry (amygdala)
Johnson and Breedlove [2010], Sisk, Schulz, and Zehr [2003], Morris, Jordan, and Breedlove [2004], Bos et al. [2012]
Mechanism [VII] Neural circuitry (amygdala) → behaviors
Adolphs, Tranel, and Damasio [1998], Klein, Shepherd, and Platt [2009], Mehta and Beer [2009], Bos et al. [2012]
Mechanism [VIII] Neural circuitry (amygdala) ↔ Facial masculinity
Carré, Murphy, and Hariri [2013]

that the linkages proposed in the figure are topics of ongoing investigation. Importantly, however, no matter what the exact (neuroendocrinological) mechanisms are, our study relies most directly on research demonstrating that the male facial structure can predict behaviors. Based on laboratory evidence as well as on data from naturalistic settings, studies have shown that facial masculinity metrics predict aggressiveness (Carré and McCormick [2008], Christiansen and Winkler [1992]), cheating and deception (Haselhuhn and Wong [2012]), male fitness and military rank (Loehr and O'Hara [2013]), exploitation of others and cooperation with peers (Stirrat and Perrett [2010]), sensation seeking (Campbell et al. [2010]), and a predilection for risktaking (Apicella et al. [2008]).[7] Wong, Ormiston, and Haselhuhn [2011] report a positive association between a firm's financial performance and the CEO's facial width-to-height ratio, while Lewis, Lefevre, and Bates [2012] show that the same measure predicts achievement drive in U.S. presidents.

A prominent conjecture forwarded in this literature is that facial masculinity and behavior are related because both are linked to testosterone. The exact mechanism linking androgens, such as testosterone, with behavior is currently debated in the neurosciences (Carré, McCormick, and Hariri [2011], Zethraeus et al. [2009]).[8] Studies have documented that individuals with higher levels of circulating or baseline testosterone have an enhanced motivation for competition and dominance, display reduced fear, and are more likely to engage in extremely risky behavior such as gambling and alcohol use (Apicella, Dreber, and Mollerstrom [2014], Mehta, Jones, and Josephs [2008], Pound, Penton-Voak, and Surridge [2009], Zuckerman and Kuhlman [2000]). These individuals are also more egocentric and have a higher propensity to cheat as well as a stronger desire to maintain social status (Eisenegger et al. [2010], Haselhuhn and Wong [2012], Wright et al. [2012]).

One possible explanation for the correlation between testosterone and behavior suggests that this hormone exerts organizational effects on the brain both prenatally (i.e., during fetal sexual differentiation) and during puberty (Morris, Jordan, and Breedlove [2004], Sisk, Schulz, and Zehr [2003]). The amygdala, a cluster of nerve cells that play a role in the processing of memory and emotional reactions, is particularly seen as the mediator between testosterone and other brain regions involved in evaluating complex social interactions (Adolphs, Tranel, and Damasio [1998], Bos et al. [2012], Klein, Shepherd, and Platt [2009], Mehta and Beer [2009]). The amygdala has neurons expressing androgen receptors for testosterone (Johnson and Breedlove [2010]). These vasopressinergic neurons in the amygdala may respond to testosterone by reducing communication to the orbitofrontal cortex and increasing communication with brainstem systems (Bos, Terburg, and van Honk [2010]).[9] The amygdala activity activates

[7] Note, however, that the facial metrics in these studies vary and do not always include our facial width-to-height ratio.

[8] Note that Zethraeus et al. [2009] find no evidence of an effect of (administered doses of) testosterone in a double-blind randomized trial on economic behavior in a sample of women.

[9] Another mechanism discussed in the literature sees changes in testosterone during aggressive behavior as a marker of the intrinsic reward value of the kind of behavior in specific situations. If so, then it is not testosterone that *causes* aggressive behavior, but testosterone is

defense responses to interpersonal provocation and status threats, reduces interpersonal trust, and lowers the ability to self-regulate (van Honk, Terburg, and Bos [2011]), which in turn engender the masculine behaviors described earlier.[10]

Testosterone also regulates the adolescent growth spurt in the presence of growth hormone (Johnston et al. [2001]). In particular, testosterone administration causes craniofacial growth in adolescents (Lindberg et al. [2005], Nie [2005], Thornhill and Gangestad [1999], Thornhill and Møller [1997], Vanderschueren and Bouillon [1995], Verdonck et al. [1999]). According to some studies, the growth trajectories of men and women diverge for bizygomatic width, but not for upper facial height (from upper lip to the highest point of the eyelid), at puberty. Weston, Friday, and Liò [2007] propose that this sexual dimorphism in facial width-to-height ratio may reflect sexual selection pressures. This idea is captured in the *immunocompetence handicap hypothesis*, which posits that testosterone is responsible for the development of male secondary sex traits (such as the facial structure) but has a negative impact on the immune system (Folstad and Karter [1992]). Only healthy men can afford to display masculine traits without suffering the costs of parasite loads (due to testosterone's deleterious effect on the immune system). Thus, when choosing a mate, women may prefer men with sexually dimorphic facial characteristics.

An important caveat to this literature should be noted. More recent work tends not to find evidence that *fWHR* is sexually dimorphic.[11] In turn, this casts doubt over the theoretical explanation that links *fWHR* and masculine behavior to the role of testosterone in the development of male secondary sex traits during adolescence. That said, the lack of evidence on whether *fWHR* is sexually dimorphic does not necessarily invalidate a potential role of testosterone in the link between *fWHR* and behavior within males (Alrajih and Ward [2014]). For example, direct evidence about an association between testosterone and the facial width-to-height ratio comes from a recent study by Lefevre et al. [2013]. These authors document a positive association between the facial width-to-height ratio and saliva-assayed testosterone before and after a (speed-dating) event. Specifically, both baseline and (postevent) circulating levels of testosterone are significantly associated with *fWHR*. Lefevre et al. [2013] theorize that, while the facial width-to-height ratio might not be sexually dimorphic, testosterone may possibly have a more direct effect on facial bone size

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elevated when aggression yields intrinsic rewards. And, as aggression yields intrinsic rewards, it is more likely to occur again (Carré et al. [2010a]).

[10] Carré, Murphy, and Hariri [2013], using brain scans, provide direct evidence for a neural link between masculine behaviors and the facial structure.

[11] See, for example, Lefevre et al. [2012] and Özener [2012]. Gómez-Valdés et al. [2013] warn against comparing samples without social-cultural controls. This warning applies less to our setting in which we examine a relatively homogenous sample of U.S. CEOs.

within a sample of men.[12] Pound, Penton-Voak, and Surridge [2009] also report that circulating levels of testosterone are higher (lower) after a successful competitive event for men with more (less) masculine faces.

Thus, the speculation is that testosterone plays a role in the development of both the facial structure and the neural structures, including the neural circuits relevant for the complex of behaviors already described. Nevertheless, the exact causality of the relation is not yet fully understood. Indeed, alternative mechanisms have been proposed to link facial masculinity to masculine behavior and some of these mechanisms do not rely (directly) on testosterone. For example, evidence exists that individuals use facial structures to guide judgments of aggression in others (Carré, McCormick, and Mondloch [2009a], Carré et al. [2010b]). Since this evidence has been documented in both children and adults, humans may be "hard-wired" to rapidly and accurately detect a propensity for threatening masculine behavior in other individuals (Short et al. [2012]). Facial structures may provide what biologists call an *honest signal* (i.e., a signal that reliably predicts information useful to the receiver). To the extent that individuals respond to facial structures, it is also possible that the anticipated behavior becomes self-fulfilling even when the stereotype based on the face is wrong (Eckel [2007]).

2.3 FACIAL MASCULINITY AND FINANCIAL MISREPORTING

Economic theory suggests that the decision to engage in misreporting (and ultimately to commit fraud) is the outcome of cost/benefit analyses (Becker [1968]). The link between masculine behaviors (measured by facial masculinity) and misreporting is intuitive because these behaviors plausibly affect the costs and benefits associated with misreporting.

Perhaps the most directly relevant research is studies that document an association between facial masculinity and the propensity to cheat (Stirrat and Perrett [2010]). Individuals with a higher propensity to cheat are more likely to experience lower emotional costs from misreporting. Other behaviors associated with facial masculinity, such as with riskseeking (Apicella et al. [2008], Zuckerman and Kuhlman [2000]), egocentric behavior [Wright et al. [2012]), and a willingness to exploit others for one's own personal financial gain (Wright et al. [2012]), may increase a given executive's propensity to engage in misreporting by affecting the way in which executives respond to incentives (Baker [2000], Garen [1994], Indjejikian [1999]); incentive pay, in turn, has been linked to misreporting (Dechow, Ge, and Schrand [2010]). Moreover, CEOs with more masculine faces may possibly perform better on the job as an outcome of their masculine behaviors (Wong, Ormiston, and Haselhuhn [2011]). Indeed, being aggressive and risk tolerant might propel the firm forward and push boundaries, with

[12] The effect on women is less straightforward as other (hormonal) factors might obscure the influence of testosterone.

positive outcomes. If so, the desire of these CEOs to maintain the social status (Eisenegger et al. [2010]) associated with high performance could increase incentives to engage in accounting manipulation. Consistent with this conjecture, prior research has shown that "superstar" CEOs are more likely to engage in misreporting *after* they achieve superstar status to maintain their performance record (Malmendier and Tate [2009]). More generally, to the extent that status can be maintained by providing an artificially rosy view in financial statements, CEOs with more masculine faces are more likely to misreport.

The facial width-to-height ratio has been positively associated with a cluster of masculine behaviors, and it is currently not feasible to tease out which specific behavior dominates in influencing the CEO's propensity to misreport. Indeed, more likely than not, the whole cluster of masculine behaviors is involved in many concrete corporate settings. Thus, the described linkages between masculine behaviors and misreporting are best thought of as illustrations of plausible scenarios rather than as separate mechanisms.

Taking all of these arguments together, we formulate the following main hypothesis.

H1: The facial width-to-height ratio of CEOs is positively associated with the risk of materially misstated financial statements of their firm.

3. The Facial Width-to-Height Ratio and the Construction of the Executive Pictorial Database

In this section, we describe how we measure a CEO's propensity to masculine behaviors by using an unfakeable facial feature, and we discuss the validity of this measurement. We then outline how we construct our pictorial database. Finally, we discuss how having a measure of masculine behavior helps to address endogeneity issues in the research design.

3.1 MEASUREMENT OF THE FACIAL WIDTH-TO-HEIGHT RATIO

We measure a given CEO's facial width-to-height ratio (*fWHR*) as the distance between the two zygions (bizygomatic width) relative to the distance between the upper lip and the highest point of the eyelid (height of upper face). Prior studies show that *fWHR* is a valid cue to masculine behaviors in males (Carré, Putnam, and McCormick [2009b], Short et al. [2012], Weston, Friday and Liò [2007], Wong, Ormiston, and Haselhuhn [2011]). Carré and McCormick [2008] show that *fWHR* can be measured from photographs (instead of measuring the skull) and that this ratio predicts aggressive behavior inside and outside the laboratory. Ultimately, we use this cue to masculine behavior to derive our predictions with regard to misreporting.[13]

[13] Deaner et al. [2012] question whether *fWHR* is a predictor of aggression in highly selective samples, such as professional hockey players. Still, while the effect size of *fWHR* is reduced

fWHR is not the only facial masculinity measure used in prior literature. Our choice is driven by two considerations. First, we rely on the evidence in Lefevre et al. [2013], who document correlations between saliva-assayed testosterone and various facial masculinity measures. *fWHR*, in contrast to the alternative measures, has significant positive correlations with testosterone responses. Since we speculate that the masculine behaviors of interest are ultimately linked to testosterone exposure, this evidence is especially pertinent. Second, we conduct a pilot study in which *fWHR*, among the available facial masculinity measures, yields the highest inter-rater reliability scores, which is an especially important methodological criterion when using imperfect photographs.

3.2 COLLECTING THE PHOTOGRAPH OF EXECUTIVES

We first identify the semblance of the executive from the company Web site, the Forbes Web site, or the company's annual report. We then use the executive's family name together with the company name to search for the best available picture of his face on Google Images. If Google Images returns more than one picture for the same executive, we identify the best photograph in terms of resolution, whether the executive is forward facing in the picture, and whether he has a neutral expression. If we find no picture, we use Google video to search for a movie fragment in which the CEO is present. We then obtain the CEO's face from the movie and include this picture in our database.

3.3 OBTAINING THE FACIAL WIDTH-TO-HEIGHT RATIO

Following the procedure outlined in Carré, McCormick, and Mondloch [2009a], we convert each picture into eight-bit images with a standard height of 400 pixels before taking the measurement. We use the .jpg file format, although it has been criticized for not maintaining spatial relationships as well as some other formats. The file format choice may introduce noise in our measurements, but it is unlikely to substantially bias our results. Two research assistants independently measure every picture using the ImageJ software (Rasband [2008]) provided by the National Institutes of Health. If the difference between the two measurements of a CEO's *fWHR* is less than 5%, we use the average of the two measurements as our *fWHR* value. If the measurements diverge by more than 5%, a third research assistant measures the facial structure. If the difference between this third measurement and either of the first two measurements is less than 5%, we code the picture as "good quality" and use the average value of the two closest measurements for *fWHR*. If not, the picture is categorized as "lower quality," and we use the average of the two closest measurements for *fWHR*. We further code pictures as "lower quality" if they have low resolution; if the picture is tilted by more than 45° (which compromises the

after controlling for body size and mass in their study, *fWHR* and aggression continue to be positively correlated with $p = 0.06$.

accuracy of the bizygomatic width measurement); or, if the individual's facial expression is not neutral.[14] Appendix B illustrates the measurement procedure.

To the extent that facial masculinity is largely the outcome of adolescence bone growth, an individual's *fWHR* value should not change after puberty. Thus, for the purpose of our study, it should not matter whether we use a recent or an older picture to obtain our data. We verify this statement for a small sample ($n = 19$) for which we have two pictures for each executive at younger and older ages, respectively. We find no significant differences ($p = 0.443$) in *fWHR* values between the pictures of the same executive at the different ages.

3.4 CORRELATED OMITTED VARIABLES, ENDOGENOUS MATCHING, AND REVERSE CAUSALITY

While we do not claim to demonstrate causality, our tests mitigate potential concerns about biases stemming from correlated (unobservable) omitted variables or from endogenous matching (Ackerberg and Botticini [2002], Prendergast [2002]). It is plausible that firms hire CEOs with certain specific characteristics that make them a good "fit" for the job (Graham, Harvey, and Puri [2013]). In empirical work, accounting for this selection effect is challenging because the selection variables (i.e., the CEO behaviors that a firm uses to hire the right person) are unobservable to the researcher. In our case, we propose that the facial structure of individuals reliably signals a complex of behaviors that together describe salient CEO personal characteristics. Thus, many potentially relevant but previously "unobservable" characteristics should be captured by our measure of facial structure. The econometric issues that arise because a researcher cannot observe the variables that firms use to select their CEOs are reduced to the extent that facial masculinity is related to these selection variables. In addition, by using information on both sides of the match between manager and firm in our tests, our estimates should suffer less from endogenous matching problems (Bandiera et al. [2010]). We control for a comprehensive set of firm-level variables that range from contractual arrangements to corporate governance structures and the firm's competitive environment. We also control for a variety of CEO-level characteristics.[15] To the extent that an individual's facial masculinity is shaped early in life, we are also less vulnerable to reverse causality threats. Finally, in some tests, we use data on both the CEO and the CFO from the same firm, which allows us to use firm-fixed effects. In particular, these tests speak to concerns about the possibility of unobserved heterogeneity driving our results.

[14] Genuine smiles will lower the upper facial height. However, "fake" smiles do not affect the facial structure. We therefore code a fake smile as a neutral expression.

[15] Including a range of environmental controls is also important to provide a more realistic approach to the complexity of the association between physical attributes (such as facial structures) and specific behaviors (see, Gómez-Valdés et al. [2013]).

4. *Sample, Misreporting Measures, and Summary Statistics*

4.1 SAMPLES OF S&P1500, AAERS, AND NON-AAERS

We include in our broad S&P1500 sample all firm-years between 1996 and 2010 for which we have a measurable picture of the CEO in 2009.[16] Table 1, panel A, reports how each step in collecting CEO pictures affects the sample. From the original sample of 1,500 CEOs, 22 cases have missing Execucomp data. We exclude 47 female CEOs, as the facial width-to-height ratio (*fWHR*) is only a valid measure of masculine behaviors in men. We are unable to find (measurable) pictures of 295 CEOs, and we remove them from the sample. The final sample consists of 1,136 (763) CEOs with measurable (good quality) pictures. We draw data from Compustat, CRSP, Risk Metrics, Thomson Reuters (Insiders data), and Execucomp to construct various CEO- and firm-level variables for the broad S&P1500 sample analyses. The number of firm-years with nonmissing values for these variables is 3,909 (2,643) for the sample of measurable (good quality) pictures. Table 1, panel B, details this sample selection process. Table 2, panel A, presents the number of observations by year and by industry for the 3,909 firm-years. The number of firm-years in panel A ranges from a low of 8 (other services industry) to a high of 1,912 (manufacturing).[17]

We derive our sample of firms subject to SEC enforcement action by considering AAERs No. 1937–3222 from the period 2004 to 2010 using a database maintained by the Center for Accounting Research and Education at the University of Notre Dame. We have an initial sample of 509 firms with GVKEYs, which enable us to merge the sample with Compustat. We exclude 207 firms with an enforcement action unrelated to a material misstatement or with misstatements that cannot be clearly linked to a specific violation period. Another 66 firms have missing data on Compustat or CRSP, yielding a final sample of 141 firms. During the accounting violation periods identified in the AAERs, these 141 firms had 194 CEOs. We are unable to find measurable pictures for 22 of these CEOs and another two CEOs are females. For each AAER firm, we select a firm from the same (two-digit SIC) industry and with a similar size (as measured by the total assets at the fiscal year-end preceding the accounting violation period) to form a matched sample. In six cases, we are unable to find a reasonable (size or industry) match that has a male CEO with a measurable picture. Thus, our final matched AAER sample consists of 164 CEOs from AAER firms and 164 CEOs from non-AAER firms. Table 1, panel C, summarizes the sample selection process. Due to the small sample size of AAER firms, we use all available (measureable) pictures in our tests to ensure that we

[16] Thus, individuals can enter the database as the CEO of different firms (provided they were the CEO of one of the S&P1500 firms in 2009 when we took their name from Execucomp).

[17] Our results are not sensitive to excluding the banking and insurance industry from the sample.

TABLE 1
Sample Selection

Panel A: CEO-level sample for the S&P1500 firms in 2009

Sampling Procedure		*N*
Original sample (S&P1500)		1,500
Less:	Observations with missing Execucomp data	22
	Female CEOs	47
	CEOs without measurable pictures	295
Male CEOs with measurable pictures		1,136
Less:	Pictures with bad resolution	72
	Observations with *fWHR* measurement error > 5%	82
	Faces tilted by more than 45°	136
	Nonneutral faces	83
Pictures with good quality		763

Panel B: Firm-year level S&P1500 sample for F-score

Sampling Procedure		*N* Measurable pictures	*N* Good-quality pictures
Total firm-year observations with the *fWHR* data and in Execucomp from 1996 to 2010		6,868	4,611
Less:	Missing data for variables required for the regressions	2,183	1,436
	Missing data for F-score	776	532
	Final S&P1500 sample	3,909	2,643

Panel C: Firm-CEO level AAER sample

Sampling Procedure		*N*
Firms accused in AAER No. 1937–No. 3222 from 2004 to 2010 with GVKEY		509
Less:	Enforcements that are unrelated to earnings misstatement (e.g., bribes, disclosure) or firms with misstatements that cannot be linked to specific reporting periods	207
	Firms with missing data in Compustat or CRSP	66
Earnings misstatement firms		141
Number of firm-CEO observations that overlap with the accounting violation period		194
Less:	AAER CEOs without measurable pictures	22
	Female AAER CEOs	2
	Data unavailability in the first- and second-round matching procedures for non-AAER CEOs: female CEOs or CEOs without measurable pictures in the non-AAER sample	6
Final AAER sample		164

This table reports the sample construction for the CEO-level sample (in panel A), the firm-year level S&P1500 sample for the *F*-score (in panel B), and the firm-CEO level AAER sample (in panel C). The CEO-level sample comprises 1,136 male CEOs with measurable pictures and 763 male CEOs with good-quality pictures. The firm-year level S&P1500 sample comprises 3,909 (2,643) observations for the analyses using all measurable pictures (good-quality pictures). The firm-CEO level AAER sample comprises 164 pairs of CEOs in the AAER and non-AAER firms.

TABLE 2

Composition of Sample by Fiscal Year and Industry

Panel A: Firm-year level S&P1500 sample with measurable pictures ($N = 3{,}909$)

Sample by Fiscal Year			Sample by Industry			
Year	*N*	%	2-Digit NAICS	Industry	*N*	%
1996	27	0.69	11	Agriculture, forestry, fishing, and hunting	9	0.23
1997	53	1.36	21	Mining, quarrying, and oil and gas extraction	207	5.30
1998	61	1.56	22	Utilities	249	6.37
1999	87	2.23	23	Construction	20	0.51
2000	113	2.89	31–33	Manufacturing	1,912	48.91
2001	144	3.68	42	Wholesale trade	173	4.43
2002	173	4.43	44–45	Retail trade	274	7.01
2003	209	5.35	48	Transportation	160	4.09
2004	249	6.37	49	Warehousing	24	0.61
2005	307	7.85	51	Information	259	6.63
2006	371	9.49	52	Finance and insurance	95	2.43
2007	468	11.97	53	Real estate and rental and leasing	28	0.72
2008	538	13.76	54	Professional, scientific, and technical services	174	4.45
2009	621	15.89	56	Admin. and support and waste mgmt. and remed. services	107	2.74
2010	488	12.48	61	Educational services	35	0.90
			62	Health care and social assistance	44	1.13
			71	Arts, entertainment, and recreation	18	0.46
			72	Accommodation and food services	113	2.89
			81, 99	Other services (except public administration)	8	0.20
Total	3,909	100			3,909	100.00

(*Continued*)

TABLE 2—*Continued*

Panel B: Firm-CEO level AAER sample (N = 164)

Sample by the First Misstatement Year			Sample by Industry			
Year	*N*	%	2-Digit NAICS	Industry	*N*	%
1993	2	1.22	21	Mining, quarrying, and oil and gas extraction	11	6.71
1994	1	0.61	22	Utilities	3	1.83
1995	1	0.61	23	Construction	2	1.22
1996	3	1.83	31–33	Manufacturing	58	35.37
1997	13	7.93	42	Wholesale trade	7	4.27
1998	15	9.15	44–45	Retail trade	5	3.05
1999	20	12.20	48	Transportation	4	2.44
2000	33	20.12	51	Information	29	17.68
2001	20	12.20	52	Finance and insurance	21	12.80
2002	15	9.15	53	Real estate and rental and leasing	3	1.83
2003	17	10.37	54	Professional, scientific, and technical services	8	4.88
2004	5	3.05	56	Admin. and support and waste mgmt. and remed. services	1	0.61
2005	10	6.10	62	Health care and social assistance	3	1.83
2006	5	3.05	71	Arts, entertainment, and recreation	2	1.22
2007	3	1.83	72	Accommodation and food services	3	1.83
2009	1	0.61	81, 99	Other services (except public administration)	4	2.44
Total	164	100			164	100

Panel A reports frequency counts by fiscal year and by industry for the 3,909 firm-years in the S&P1500 sample relating to the 1,136 measurable pictures. Panel B reports frequency counts by the first accounting violation year and industry for the 164 CEOs in the AAER firms. Two-digit NAICS codes are used to classify industries.

have sufficient power. Table 2, panel B, reports the distribution of firms by year and by industry for the AAER sample. While we examine AAERs from the period 2004–2010, the earliest violation period associated with these enforcement actions dates back to 1993.

4.2 MISREPORTING MEASURES USED IN THE S&P1500 AND IN THE AAER (NON-AAER) SAMPLES

The *F-score* is a composite measure of the likelihood of accounting manipulation based on the insight that financial statement information beyond accruals is useful for identifying earnings manipulation (Dechow et al. [2011]). We use scores computed based on model 1 in Dechow et al. [2011], which relies on information about accruals quality, performance, and market-related incentives. In constructing our variables, we exploit the guidance in Dechow et al. [2011] about the critical values of the *F-score* in relation to identifying the risk of financial misreporting. We consider the (survival) time until the firm, within the CEO's tenure, first experiences an *F-score* that is greater than 1.85, indicating "substantial risk" of misstatement (*Time until F-score* $>$ *1.85*). We also use an ordinal scale *F-risk*, which starts at zero for *F-scores* smaller than one (i.e., the critical value for "normal risk" of misreporting) and extends to the value three if the *F-score* is greater than 2.45 (i.e., the critical value for "high risk" of misreporting). The intermediate values of *F-risk*, namely one and two, are for *F-scores* that indicate "above normal risk" (greater than one but smaller than or equal to 1.85) and "substantial risk" (greater than 1.85 but smaller than or equal to 2.45), respectively.

We define *D(AAER)* as an indicator variable that equals one if the firm is subject to an AAER and zero for matched non-AAER firms. When reading the enforcement releases, we collect the names of CEOs or CFOs who are accused by name as the perpetrator of the misstatement. We introduce an indicator variable *D(EXECaccused)*, which equals one for those executives (either CEO or CFO) who are named in the AAER, and zero otherwise.

4.3 SUMMARY STATISTICS FOR THE S&P1500 SAMPLE

Table 3, panel A, presents the summary statistics computed at the CEO level for the S&P1500 sample described in table 1. We first consider *fWHR* in both the measurable and good-quality picture samples. The distribution of *fWHR* is very similar in the two samples, with a mean value of 2.01. This finding is comparable to statistics reported in other studies (e.g., Lefevre et al. [2012]).

Table 3, panel B, reports summary statistics for the financial misreporting variables of *Time until F-score* $>$ *1.85* (computed at the CEO level) and *F-risk* (computed at the firm-year level) for CEOs with above-median (below-median) *fWHR* using the sample with all measurable pictures.[18] We include

[18] Note that, as we partition the sample based on the median of *fWHR* at the CEO-level, the number of *firm-year* observations for the above and below median *fWHR* samples is not equal.

TABLE 3

Summary Statistics for the S&P1500 Sample

Panel A: Descriptive statistics for facial width-to-height ratios across the CEO-level sample

	Facial Width-to-Height Ratio (*fWHR*)							
Sample	*N*	Mean	*SD*	P95	Q3	Median	Q1	P5
All measurable pictures	1,136	2.013	0.149	2.267	2.106	2.011	1.906	1.780
Good-quality pictures	763	2.009	0.142	2.258	2.104	2.006	1.901	1.789

Panel B: Analysis of differences in misreporting proxies, firm-level characteristics, and CEO-level characteristics between high- and low-*fWHR* samples

	High *fWHR*			Low *fWHR*			T-test/Log-rank test	
Variable	*N*	Mean	*SD*	*N*	Mean	*SD*	Diff. in Mean	*p*-Value
Time until F-score> 1.85 (years)	374	12.029	6.717	346	12.520	6.320	−0.491	0.066
F-risk	1,996	0.536	0.668	1,913	0.477	0.652	0.059	0.005
Adjusted return	1,996	0.093	0.339	1,913	0.088	0.355	0.004	0.700
Volatility	1,996	0.394	0.166	1,913	0.402	0.171	−0.008	0.149
ROA	1,996	0.066	0.080	1,913	0.063	0.090	0.003	0.295
Book to market	1,996	0.615	0.235	1,913	0.642	0.256	−0.026	0.001
Price to earnings	1,996	17.385	41.597	1,913	18.119	40.550	−0.733	0.577
Loss	1,996	0.121	0.326	1,913	0.137	0.344	−0.016	0.131
Leverage	1,996	0.176	0.142	1,913	0.176	0.150	0.000	0.972
Market value ($ million)	1,996	7.817	1.485	1,913	7.757	1.409	0.060	0.193
Sales growth	1,996	0.101	0.205	1,913	0.109	0.229	−0.008	0.243
Free cash flow	1,996	0.202	0.252	1,913	0.184	0.264	0.018	0.033
RD	1,996	0.047	0.081	1,913	0.039	0.070	0.008	0.001
Firm age	1,996	28.331	17.462	1,913	27.031	15.827	1.299	0.015
CEO age	1,996	54.068	6.896	1,913	54.611	6.679	−0.543	0.013
CEO tenure	1,996	6.801	5.976	1,913	8.884	8.730	−2.083	0.001
CEO power	1,996	0.807	0.835	1,913	0.778	0.803	0.029	0.272
Inside CEO	1,996	0.313	0.464	1,913	0.315	0.465	−0.002	0.888

(*Continued*)

TABLE 3—*Continued*

Panel B: Analysis of differences in misreporting proxies, firm-level characteristics, and CEO-level characteristics between high- and low-*fWHR* samples

	High *fWHR*			Low *fWHR*			T-test/Log-rank test	
Variable	*N*	Mean	*SD*	*N*	Mean	*SD*	Diff. in Mean	*p*-Value
TopExDrct	1,996	1.464	0.695	1,913	1.562	0.791	−0.098	0.001
CEO appoint	1,996	0.352	0.288	1,913	0.395	0.309	−0.043	0.001
Independent director	1,996	0.746	0.147	1,913	0.728	0.153	0.018	0.001
Board Size	1,996	9.336	2.348	1,913	9.094	2.281	0.243	0.001
Salary ($ thousand)	1,996	752	331	1,913	746	319	6	0.545
Bonus ($ thousand)	1,996	1,384	1,268	1,913	1,405	1,354	−21	0.619
New option PPS ($ thousand)	1,996	45.238	80.869	1,913	37.231	67.574	8.007	0.001
New stock PPS ($ thousand)	1,996	14.346	26.276	1,913	14.420	24.889	−0.074	0.928
Equity holdings ($ thousand)	1,996	63,672	201,997	1,913	93,408	272,096	−29,736	0.001

This table presents descriptive statistics for the facial width-to-height ratio across the CEO-level sample (in panel A) and for misreporting proxies and firm-level (CEO-level) characteristics defined in appendix A using the high- and low-*fWHR* samples (in panel B). The high-*fWHR* (low-*fWHR*) sample comprises CEOs with above (below) median *fWHR* in the CEO-level sample. The univariate tests in panel B are based on the firm-years or CEOs with all measurable pictures (i.e., 3,909 firm-years (720 CEOs) for the S&P1500 sample). *Time until F-score> 1.85* measures the number of years within a CEO's tenure until a firm's *F*-score is greater than 1.85 (indicating substantial misreporting risk). The log-rank test examines the difference in restricted mean survival times between the high- and low-*fWHR* samples. The "Diff. in Mean" column in panel B reports differences in the variable means between the high- and low-*fWHR* samples. Two-tailed probability values are reported for log-rank test or for two-sample *t*-test.

a range of firm and CEO characteristics that serve as controls in our later regression models. For each variable, we present *t*-tests or log rank tests of the hypothesis that the difference between high- and low-*fWHR* samples is zero. The log rank test is appropriate for comparing the equality of estimated mean survival times.

We find that CEOs with above-median *fWHR* have a significantly lower restricted mean *Time until F-score* > *1.85*. The restricted mean is the nonparametric estimate of the mean survival time.[19] This pattern is mirrored in the descriptive statistics for the ordinal scale *F-risk*. For this variable, the average value is significantly lower in the below-median *fWHR* sample, which suggests a lower risk of misreporting in this sample.

Firms in the high-*fWHR* sample have lower book-to-market ratios, higher free cash flow, and higher R&D expenses. These firms have significantly fewer directors who are also the top-five highly paid executives and have fewer directors appointed during the CEO's tenure. They have on average more independent directors as well as a larger board size. CEOs with more masculine faces have shorter tenure, higher pay for performance sensitivity in newly granted options but less valuable equity holdings, and tend to be working for older firms.

4.4 SUMMARY STATISTICS FOR THE AAER AND MATCHED NON-AAER SAMPLE

Table 5, panel A, presents paired *t*-tests for the difference in *fWHR* between CEOs from AAER and non-AAER firms. We find that CEOs from AAER firms have significantly higher *fWHR* than those from non-AAER firms ($p < 0.05$). Turning to *t*-tests in a sample restricted to AAER firms, we find that CEOs who are accused by name as the perpetrator of wrongdoing in the enforcement release have higher *fWHR* scores than CEOs who are not accused by name ($p = 0.01$).

5. *Misreporting and the Executive's Facial Width-to-Height Ratio*

5.1 EMPIRICAL MODEL SPECIFICATION

We begin by testing how financial misreporting varies with a given CEO's facial width-to-height ratio (*fWHR*). Our regressions have the following generic specification:

$$\begin{aligned} &\textit{Misreporting}_{it} = f[D(\textit{fWHR} > \textit{median})_{it}, \textit{firm and market controls}_{it},\\ &\textit{CEO controls}_{it}, \textit{corporate governance controls}_{it}, \textit{CEO compensation}\\ &\textit{controls}_{it}, \ \textit{year fixed effects}, \ \textit{industry fixed effects}], \end{aligned} \tag{1}$$

[19] *Time until F-score* > *1.85* is a right-censored variable. Many firms in the sample have never obtained an *F-score* larger than 1.85. The restricted mean reflects that the Kaplan–Meier estimate is not defined beyond the largest observed failure time (Cleves et al. [2008]). The right-censoring of this variable also explains why the average *Time until F-score* > *1.85* is higher than the average CEO tenure measured at the firm-year level.

where i is an index across firms, and t is an index across years. *Misreporting* is either a misreporting proxy based on the Dechow et al. [2011] prediction model or on the AAERs (in section 4.2). When we use the sample of all measurable pictures, we include in equation (1) three variables that capture the potential measurement error associated with these pictures due to nonneutral facial expressions, facial tilt, and picture resolution (*D(GrtExp)*, *D(GrtPrfl)*, and *D(LowRes)*, respectively).[20] In this manner, we can increase the sample size while simultaneously controlling for measurement error in the variable of interest. Our variable of interest is *D(fWHR> median)*, an indicator variable that takes the value of one if a CEO's *fWHR* is above the median and zero otherwise.[21] Inferences are based on standard errors that are clustered at the CEO level throughout.[22]

5.2 FINDINGS FOR *F*-SCORE BASED PROXIES IN THE S&P1500 SAMPLE

We first report the results of a Cox proportional hazard model, which reflects that our interest centers in part on the time elapsed from the start of a CEO's tenure until a "substantial" risk of misreporting occurs (i.e., *Time until F-score* $>$ *1.85*). Since not all CEOs experience a "substantial" misreporting risk during their tenure, our data are right-censored. The dependent variable is the hazard rate, defined as the probability of a firm experiencing the event (i.e., *F-score* $>$ *1.85*) at time t, given that the firm has survived without such event until then. The proportional hazard model allows a nonparametric estimation of the baseline hazard function (i.e., the hazard function in the absence of covariates). The explanatory variables shift the baseline hazard as follows:

$$h(t) = h_0(t)\exp(\beta_1 x_1 + \cdots + \beta_k x_k),$$

where $x_1, \ldots, x_k$ compose the set of explanatory variables described in equation (1), $\beta_1, \ldots, \beta_k$ are the slope parameters to be estimated, and $h_0(t)$ is the nonparametric baseline hazard function. We interpret the estimated parameters by subtracting 1 from the hazard ratios (i.e., the

[20] *D(LowRes)* is equal to one if the resolution of a CEO's picture is low or if the interrater measurement of *fWHR* diverges by more than 5%.

[21] We use the indicator variable transformation of *fWHR* for two reasons. First, Greene [2000, p. 379] shows that this method can be used to reduce measurement error in the variable of interest. Second, the transformation helps to present the economic significance of the estimated effects. We find very similar results (detailed in the online appendix) when using the untransformed *fWHR* variable in the tests. An online appendix to this paper can be downloaded at http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements.

[22] Our annual data yield too few observations to also cluster standard errors by year (see, Angrist and Pischke [2009], Petersen [2009]). Indeed, Petersen [2009] reports that, when the number of clusters in a dimension is below 40, clustering the standard errors increases the bias. On the other hand, our variable of interest, *D(fWHR* $>$ *median)*, is fixed at the CEO level, which makes it imperative to account for potential within CEO correlation (Angrist and Pischke [2009]).

exponentiated coefficients). This transformation yields the percentage change in the hazard rate caused by a unit change in the explanatory variable. We report the hazard ratios in the tables. We also estimate ordered logistic regressions in which the dependent variable is *F-risk*.

Table 4, columns 1 and 2, report the Cox proportional hazard regressions that model the *Time until F-score* > 1.85. Columns 3 and 4 present the ordered logistic regressions using *F-risk*. Results in columns 1 and 3 (2 and 4) are based on the sample of all measurable (good-quality) pictures. We find consistent results across the two *F-score*–based proxies for financial misreporting. The exponentiated coefficient on $D(fWHR > median)$ in the proportional hazard model in column 1 (column 2) is 1.833 (1.984), with $p < 0.01$. Thus, high-*fWHR* CEOs (i.e., CEOs with above-median *fWHR*) face an 83% (98%) higher hazard of experiencing a "substantial" risk of misreporting than low-*fWHR* CEOs. Similarly, the coefficient on the variable of interest in column 3 (column 4) equals 0.434 (0.368) with $p < 0.01$ ($p < 0.05$), suggesting that the odds ratio of high- versus low-*fWHR* CEOs equals 1.54 (1.44). This finding provides further evidence that firms with high-*fWHR* CEOs have substantially higher odds of experiencing above-normal misreporting risk.

In the online appendix (http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements), we report specifications in which we drop the variable of interest to assess how associations change with the inclusion of *fWHR*. In the full specification, we find some evidence for the control variables that the natural log of market value, sales growth, the R&D expense, CEO power, the natural log of the CEO bonus, and the natural log of the CEO equity holdings are associated with financial misreporting. We also find a positive (negative) association between the misreporting proxy in the Cox proportional hazard regressions (ordered logistic regressions) and leverage (volatility of monthly stock returns and an indicator variable for firms that report a negative net income). The variable *%Misreporting industry members* controls for the industry-related incidence of misreporting (Davidson, Dey, and Smith [2013]). As expected, it is positively associated with a given firm's misreporting risk. For many variables, however, including those controlling for differences in corporate governance and operating environment, the results are either insignificant or not stable across the various specifications. This instability likely reflects that financial misreporting, corporate governance, and executive compensation are endogenously determined. This finding reinforces the importance of controlling for these variables when examining the association between stable CEO traits and financial misreporting.[23]

[23] Note that one of the control variables is CEO age. The levels of circulating (baseline) testosterone drop with age, and, for this reason, age itself is associated with testosterone. Thus, by including CEO age in our regressions, we potentially underestimate the effect size of the facial width-to-height ratio.

TABLE 4

Hazard Ratio, Coefficient Estimates, and Summary Statistics for Cox Proportional Hazard Regressions and Ordered Logistic Regressions Relating Misreporting Proxies Based on F-Score to CEO fWHR

	Hazard Rate (F-score > 1.85)		*F-Risk*	
Variable	Measurable Pictures (1)	Good-Quality Pictures (2)	Measurable Pictures (3)	Good-Quality Pictures (4)
Intercept 1			5.393*	20.217***
			(2.908)	(3.552)
Intercept 2			8.580***	23.360***
			(2.926)	(3.717)
Intercept 3			10.067***	24.813***
			(2.936)	(3.651)
D(fWHR > median)	1.833***	1.984***	0.434***	0.368**
	(0.203)	(0.256)	(0.136)	(0.161)
D(GrtExp)	0.452		−0.801**	
	(0.512)		(0.313)	
D(GrtPrfl)	0.403**		0.084	
	(0.382)		(0.197)	
D(LowRes)	0.813		0.033	
	(0.339)		(0.220)	
Adjusted return	1.616*	1.694	0.124	0.217
	(0.270)	(0.346)	(0.120)	(0.147)
Volatility	0.764	0.670	−1.257**	−1.231**
	(0.750)	(0.984)	(0.498)	(0.574)
ROA	3.401	4.759	−1.778	−1.356
	(2.547)	(3.742)	(1.171)	(1.468)
Book to market	2.776	5.254*	0.444	0.819
	(0.677)	(0.892)	(0.421)	(0.524)
Price to earnings	0.998	1.001	−0.001	0.001
	(0.003)	(0.003)	(0.001)	(0.002)
Loss	1.618	1.980	−0.899***	−0.666**
	(0.489)	(0.591)	(0.264)	(0.324)
Leverage	10.837***	16.544***	0.295	0.786
	(0.844)	(1.083)	(0.518)	(0.670)
Ln(Market value)	0.858	0.724*	−0.182**	−0.201**
	(0.143)	(0.183)	(0.080)	(0.085)
Sales growth	11.012***	14.850***	1.947***	2.020***
	(0.496)	(0.800)	(0.271)	(0.374)
Free cash flow	0.532	0.397	−0.240	0.030
	(0.802)	(1.021)	(0.439)	(0.561)
RD	46.155**	580.564***	−2.553**	−1.909
	(1.822)	(2.236)	(1.132)	(1.398)
Ln(Firm age)	0.815	0.962	0.028	0.044
	(0.212)	(0.296)	(0.122)	(0.145)
Ln(CEO age)	0.817	0.391	0.221	−0.332
	(0.989)	(1.374)	(0.642)	(0.756)
Ln(CEO tenure)	0.994	0.974	−0.009	0.056
	(0.080)	(0.102)	(0.038)	(0.042)
CEO power	1.302*	1.423*	−0.126	−0.166*
	(0.137)	(0.181)	(0.079)	(0.092)

(*Continued*)

TABLE 4—*Continued*

	Hazard Rate (F-score > 1.85)		*F-Risk*	
Variable	Measurable Pictures (1)	Good-Quality Pictures (2)	Measurable Pictures (3)	Good-Quality Pictures (4)
Inside CEO	1.292	1.473	0.068	−0.015
	(0.226)	(0.279)	(0.158)	(0.188)
TopExDrct	0.959	0.744	0.091	0.072
	(0.145)	(0.196)	(0.088)	(0.106)
CEO appoint	0.741	0.513	−0.495	−0.840**
	(0.497)	(0.628)	(0.305)	(0.388)
Independent director	0.456	0.189*	0.200	−0.090
	(0.727)	(0.922)	(0.466)	(0.563)
Board size	1.081	1.073	0.021	0.071*
	(0.050)	(0.064)	(0.034)	(0.040)
Ln(Salary)	0.557*	0.647	0.134	0.193
	(0.317)	(0.267)	(0.214)	(0.178)
Ln(Bonus)	1.398**	1.520**	0.293***	0.271***
	(0.147)	(0.198)	(0.081)	(0.091)
Ln(New option PPS)	1.004	0.969	0.006	0.014
	(0.026)	(0.030)	(0.013)	(0.016)
Ln(New stock PPS)	0.997	1.003	0.024	0.016
	(0.030)	(0.040)	(0.016)	(0.019)
Ln(Equity holdings)	1.134	1.467***	0.093*	0.126*
	(0.116)	(0.115)	(0.055)	(0.068)
%Misreporting industry members	950.511***	1,684.123***	5.494***	5.554***
	(0.987)	(1.170)	(0.767)	(0.905)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Clustered std errors	CEO level	CEO level	CEO level	CEO level
Pr > χ^2	0.001	0.001	0.001	0.001
Pseudo-R^2			22.37%	23.99%
Log pseudo likelihood	−577.119	−378.223	−2,707.119	−1,826.657
N	3,417	2,282	3,909	2,643

This table presents an analysis of the association between *D(fWHR > median)* and misreporting proxies measured by (1) *Hazard Rate (F-score > 1.85)*, the probability of a firm's *F-score* being greater than 1.85 (indicating substantial misreporting risk) in a given year, conditional upon the firm having survived to the beginning of the year, and (2) *F-risk*, an ordinal scale classified according to the level of misreporting risk (see appendix A). Columns 1 and 2 (columns 3 and 4) report hazard ratios (coefficient estimates) for Cox proportional hazard regressions (ordered logistic regressions) relating *Hazard Rate (F-score > 1.85)* (*F-risk*) to *D(fWHR > median)* and a vector of control variables. Variable definitions are provided in appendix A. We winsorize the variables at 1% and 99%, with the exception of *RD* and *Leverage* for which only the top percentile observations were winsorized. We run separate regressions for the sample of all measurable pictures (columns 1 and 3) and for the sample of good-quality pictures (columns 2 and 4). The Cox proportional hazard model estimates how soon in the CEO's tenure a substantial risk of financial misreporting occurs (i.e., *F-score > 1.85*). Once this threshold of risk has been passed, we truncate all further observations for the corresponding CEO from the sample. Hence, the number of observations is reduced to 3,417 (2,282) for all measurable pictures (good-quality pictures). Two-tailed probability values are computed using standard errors clustered by CEO. ***, **, and * represent statistical significance at 1%, 5%, and 10%. The standard errors are reported in parentheses.

In sum, the empirical evidence on the link between a given CEO's *fWHR* and financial misreporting is consistent across the *F-score*–based misreporting proxies. As we hypothesized, the financial misreporting probability is higher for those firms with CEOs with more masculine faces who are assumed to exhibit more masculine behaviors.

5.3 FINDINGS FOR THE AAER- AND NON-AAER–MATCHED SAMPLE

So far, we have used a prediction model for misreporting. We cannot rule out the possibility, however, that some firms are falsely classified as a misreporting firm (type-I error). We therefore also consider firms that are subject to SEC AAERs. These firms have unambiguously material accounting misstatements. At the same time, not all misreporting firms will be subject to SEC enforcement actions and by using this measure we likely underidentify the number of misreporting firms (type-II error). Thus, the *F-score*–based and AAER approaches complement each other in the sense that each makes a different tradeoff between type-I and type-II errors. In supplementary tests described in the online appendix (http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements), we use the financial restatements due to intentional misstatements in the S&P1500 sample as an alternative ex post measure of misreporting to address the small sample size issue of AAERs.[24]

In table 5, panel B, we report the results of an ordinary (a conditional) logit regression using *D(AAER)* as the dependent variable in column 1 (column 2). When estimating a conditional logit, we exploit the (within) variation between the AAER firm and its non-AAER match to explain the dependent variable. We find a significant positive coefficient on *D(fWHR >median)* both in the ordinary and conditional logit regressions (both with $p < 0.01$). In terms of economic significance, based on the ordinary (conditional) logit model, the odds that a firm with a high-*fWHR* CEO is involved in an AAER action are about 2.88 (3.15) times higher than the odds for a firm with a low-*fWHR* CEO.

With respect to the control variables in these regressions, we find that the natural log of market value, an indicator variable for firms that report a negative net income, and new stock pay-for-performance sensitivity are positively associated with the probability of an AAER. On the other hand, we find a negative association between the probability of an AAER and free cash flows, the natural log of the CEO's age, the natural log of the CEO's equity holdings, and an indicator variable for whether the CEO was appointed by internal promotion or by an outside hire. These associations are broadly consistent with earlier work on AAER samples (Schrand and Zechman [2012]).

We restrict our sample in table 6 to the AAER firms only and estimate a logit model of the association between the *fWHR* of an executive (CEO or CFO) and his probability of being accused by name in the AAER. The dependent variable is an indicator variable (*D(EXECaccused)*), which takes the value of one if the executive is mentioned by name in the release, and zero otherwise. We find, in column 1, a positive and significant association between the *fWHR* of the CEO and the likelihood that he is named

[24] Results for the restatement-based misreporting measures are consistent with our hypothesis.

TABLE 5

SEC Enforcement Actions and CEO fWHR

Panel A: Paired *t*-test and two sample *t*-tests for difference in CEO facial *fWHR*

Sample	*N*	Mean	*SD*	AAER Sample	*N*	Mean	*SD*
AAER	164	2.098	0.146	CEOs who are accused	45	2.166	0.124
Non-AAER	164	2.059	0.167	CEOs who are not accused	119	2.072	0.147
p-value of *t*-test		0.023		*p*-value of t-test		0.001	

Panel B: Coefficient estimates and summary statistics for logistic regressions relating an intentional misreporting proxy based on the AAER sample to CEO *fWHR*

Variable	Pred. Sign	Ordinary Logistic Regression (1)	Conditional Logistic Regression (2)
Intercept		11.429**	
		(5.032)	
D(fWHR > median)	+	1.059***	1.149***
		(0.325)	(0.341)
D(GrtExp)	−	−0.083	−0.238
		(0.420)	(0.628)
D(GrtPrfl)	?	0.445	0.385
		(0.516)	(0.688)
D(LowRes)	?	0.465	0.716
		(0.373)	(0.476)
Adjusted return		−0.080	−0.288
		(0.215)	(0.280)
Volatility		4.979	4.546
		(4.058)	(4.870)
ROA		2.033	−1.063
		(1.745)	(2.204)
Book to market		−1.163	1.498
		(0.801)	(1.483)
Price to earnings		0.003	0.005
		(0.002)	(0.004)
Loss		1.195**	1.596**
		(0.586)	(0.683)
Leverage		0.626	2.791*
		(1.173)	(1.534)
Ln(Market value)		0.244**	1.872***
		(0.121)	(0.468)
Sales growth		0.005	−0.412
		(0.360)	(0.493)
Free cash flow		−0.970*	−1.938**
		(0.556)	(0.884)
RD		−5.092	−3.125
		(3.190)	(3.506)
Ln(Firm age)		−0.053	−0.023
		(0.035)	(0.041)
Ln(CEO age)		−3.206***	−3.160**
		(1.182)	(1.265)
Ln(CEO tenure)		−0.048	0.137
		(0.224)	(0.220)

(*Continued*)

TABLE 5—*Continued*

Panel B: Coefficient estimates and summary statistics for logistic regressions relating an intentional misreporting proxy based on the AAER sample to CEO *fWHR*

Variable	Pred. Sign	Ordinary Logistic Regression (1)	Conditional Logistic Regression (2)
CEO power		−0.316*	−0.263
		(0.191)	(0.370)
Inside CEO		−0.920***	−0.730**
		(0.287)	(0.349)
TopExDrct		0.265	0.121
		(0.163)	(0.243)
CEO appoint		−0.300	0.127
		(0.629)	(0.627)
Independent director		−1.307	−1.699
		(0.871)	(1.136)
Board size		0.014	0.014
		(0.011)	(0.021)
Ln(Salary)		0.270	0.097
		(0.300)	(0.238)
Ln(Bonus)		−0.013	0.008
		(0.028)	(0.032)
Ln(New option PPS)		0.041	0.039
		(0.034)	(0.039)
Ln(New stock PPS)		0.079**	0.092**
		(0.032)	(0.046)
Ln(Equity holdings)		−0.100*	−0.159***
		(0.054)	(0.059)
Industry fixed effects		Yes	No
Standard errors		Robust	Robust
$\Pr > \chi^2$		0.010	0.001
Pseudo-R^2		20.74%	42.04%
Log pseudo likelihood		−180.197	−65.882
N		328	328

This table presents an analysis of the association between *D(fWHR> median)* and intentional misreporting proxies based on the AAER sample. Panel A presents the paired *t*-test (two-sample *t*-test) for difference in mean of CEO *fWHR* between the *AAER* and non-AAER samples (between the CEOs who are accused and those who are not accused as a perpetrator by the SEC). Two-tailed probability values are reported. Panel B presents coefficient estimates and model summary statistics for an ordinary (conditional) logistic regression relating *D(AAER)* (i.e., an indicator variable coded as one for the AAER firms and zero for the non-AAER firms) to *D(fWHR> median)* and a vector of control variables. Variable definitions are provided in Appendix A. We winsorize the variables at 1% and 99%, with the exception of *RD* and *Leverage* for which only the top percentile observations were winsorized. Two-tailed probability values are computed using robust standard errors. ***, **, and * represent statistical significance at 1%, 5%, and 10%. The standard errors are reported in parentheses.

as the perpetrator ($p < 0.05$). The odds of being accused by name in the AAER are 5.03 times higher for a high-*fWHR* CEO than for a low-*fWHR* CEO. Next, we exploit the fact that both the CEO and the CFO may be accused in the enforcement release, which gives us the opportunity to estimate a firm-fixed effects logit regression. This approach allows us to hold constant all firm characteristics for each CEO–CFO pair. Note that, if both the CEO and CFO have the same value on the dependent variable

TABLE 6

Coefficient Estimates and Summary Statistics for Logistic Regressions Relating Executive (Either CEO or CFO) Being Accused in the AAER to Executive's fWHR

Variable	Pred. Sign	CEO Accused (1)	CEO/CFO Accused (2)	CEO/CFO Accused (3)
Intercept		14.195		
		(11.522)		
D(Higher fWHR)	+			7.262**
				(2.992)
D(fWHR > median)	+	1.615**	2.464***	
		(0.810)	(0.879)	
D(GrtExp)	−	−0.144	−34.062***	−28.874***
		(0.909)	(1.759)	(5.495)
D(GrtPrfl)	?	−1.505	−16.284***	−27.771***
		(1.076)	(1.542)	(6.862)
D(LowRes)	?	−1.258	14.926***	6.472
		(0.833)	(1.859)	(5.491)
DCFO	?		1.946	1.063
			(1.329)	(1.482)
% CEO accused in the industry	+	11.143***		
		(2.773)		
Firm-level controls		Yes	No	No
Managerial level controls		Yes	Yes	Yes
Year fixed effects		Yes	No	No
Firm fixed effects		No	Yes	Yes
Standard errors		Robust	Robust	Robust
Pr > χ^2		0.003	0.001	0.001
Pseudo-R^2		48.57%	64.21%	86.35%
Log pseudo likelihood		−49.558	−7.667	−2.924
N		164	58	58

This table uses the sample of AAER firms only and presents an analysis of the association between an executive's (either CEO's or CFO's) *fWHR* and his being accused by name in the AAERs. Column 1 limits the executives to CEOs only and reports coefficient estimates and model summary statistics for an ordinary logistic regression relating *D(EXECaccused)* (i.e., an indicator variable coded as one for the executive being accused in the AAERs and zero otherwise) to *D(fWHR > median)* and a vector of control variables reported in table 5, panel B. Variable definitions are provided in appendix A. Column 2 (column 3) reports coefficient estimates and model summary statistics for the firm-fixed effect logistic regression relating *D(EXECaccused)* to *D(fWHR > median)* (*D(Higher fWHR)*, an indicator variable coded as one if the executive has the higher *fWHR* within the pair of the firm's CEO and CFO) and a vector of control variables (including executive age, executive tenure, and an indicator variable coded as one if the executive is a CFO and zero otherwise). We winsorize the variables at 1% and 99%, with the exception of *RD* and *Leverage*, for which only the top percentile observations were winsorized. Two-tailed probability values are computed using robust standard errors. ***, **, and * represent statistical significance at 1%, 5%, and 10%. The standard errors are reported in parentheses.

(i.e., *D(EXECaccused)*), the firm-fixed effect will perfectly predict the outcome, and the firm is dropped from the analysis. We find in column 2 that *D(fWHR > median)* significantly increases the likelihood that an executive (CEO or CFO) is accused by name in the firm-fixed effects framework ($p < 0.01$). The odds of a high-*fWHR* executive being accused are 11.75 times higher than the odds for a low-*fWHR* executive. In the same spirit, we ask whether the executive with the higher *fWHR in the pair* is more likely to be named for perpetrating the fraud. Column 3 reports that *D(Higher fWHR)*, an indicator variable that equals one if a given executive has the higher *fWHR* in the pair and zero otherwise, is positively associated with the likelihood of his being accused by name ($p < 0.05$).

5.4 IS MASCULINE BEHAVIOR AS MEASURED BY *fWHR* DIFFERENT FROM OVERCONFIDENCE?

Schrand and Zechman [2012] provide evidence that links financial misreporting to an executive's overconfidence. They show that misreporting often originates in a given CEO's being too optimistic about a firm's future performance. These authors then attempt to link this optimism bias to overconfidence by using proxies for this psychological trait based on compensation data and on investment and financing decisions made by the firm. Their evidence generally supports the relation between overconfidence and misreporting.

It is possible that the underlying masculine behaviors measured by *fWHR* include optimism and/or overconfidence. Perhaps the documented misreporting behavior among high-*fWHR* individuals is partially driven by their overly rosy estimate of how the future will unravel. If so, our findings might just replicate those documented in this prior work.

Before we address this issue, we wish to highlight that the prevalent measures of overconfidence in the prior work are somewhat difficult to interpret. As Schrand and Zechman [2012] observe, these measures are computed at the firm level, not the CEO level. In addition, to the extent that these measures are based on firm-level investing and financing decisions, they are affected by the corporate governance mechanisms present in the firm. Thus, it is difficult to separate overconfidence explanations of misreporting based on these proxies from those stemming from poor corporate governance.

We provide details on the construction of the overconfidence proxies in appendix A. In short, we follow Schrand and Zechman [2012] and use both the extent to which CEOs delay exercising options as a reflection of their overconfidence as well as a composite measure based on the extent to which a firm engages in certain financing and investment activities that prior research has also linked to overconfidence.

We first examine the correlation between these measures of CEO overconfidence and *fWHR*. Correlations are computed at the CEO level (as opposed to at the firm-year level). Details are reported in table 7, panel A. The proxies for overconfidence are not significantly correlated with *D(fWHR >median)* and the magnitude of correlation coefficient is negligible, implying that the overconfidence proxies and *fWHR* are distinct constructs (Johnson et al. [2006]).

We then include these overconfidence measures in our misreporting regressions. The results are summarized in table 7, panels B and C. Regardless of the overconfidence proxy and the misreporting proxy

TABLE 7

CEO fWHR Versus CEO Overconfidence

Panel A: Pearson and Spearman correlation matrix of CEO *fWHR* and CEO overconfidence proxies for the CEO-level sample of all measurable pictures ($N = 1{,}136$)

	D(fWHR > median)	*OC_OPTIONS*	*OC_FIRM4*	*OC_FIRM5*
D(fWHR > median)	1	0.011	0.002	0.018
		(0.721)	(0.944)	(0.536)
OC_OPTIONS	0.009	1	0.053	0.025
	(0.752)		(0.073)	(0.402)
OC_FIRM4	−0.008	0.042	1	0.723
	(0.790)	(0.153)		(0.001)
OC_FIRM5	0.023	0.024	0.729	1
	(0.434)	(0.412)	(0.001)	

Panel B: Hazard ratio, coefficient estimates, and summary statistics for Cox proportional hazard regressions and ordered logistic regressions relating misreporting proxies based on *F-score* to CEO *fWHR* and proxies of CEO overconfidence

	Hazard Rate (F-score > 1.85)		*F-risk*	
Variable	Measurable Pictures (1)	Good-Quality Pictures (2)	Measurable Pictures (3)	Good-Quality Pictures (4)
Intercept 1			5.530**	20.278***
			(2.901)	(2.430)
Intercept 2			8.725***	23.429***
			(2.919)	(2.496)
Intercept 3			10.218***	24.886***
			(2.929)	(2.543)
D(fWHR > median)	1.812***	1.958***	0.433***	0.363**
	(0.203)	(0.252)	(0.136)	(0.161)
OC_OPTIONS	1.991**	2.024*	0.286**	0.278**
	(0.285)	(0.362)	(0.114)	(0.140)
D(GrtExp)	0.465		−0.785**	
	(0.500)		(0.315)	
D(GrtPrfl)	0.384**		0.074	
	(0.391)		(0.197)	
D(LowRes)	0.817		0.030	
	(0.345)		(0.220)	
Controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Clustered std errors	CEO level	CEO level	CEO level	CEO level
Pr > χ^2	0.001	0.001	0.001	0.001
Pseudo-R^2			22.52%	24.11%
Log pseudo likelihood	−573.593	−375.761	−2,702.048	−1,823.600
N	3,417	2,282	3,909	2,643
D(fWHR > median)	1.851***	2.016***	0.441***	0.389**
	(0.202)	(0.257)	(0.135)	(0.160)
OC_FIRM4	4.816***	4.657***	0.832***	0.840***
	(0.319)	(0.371)	(0.115)	(0.141)
D(fWHR > median)	1.915***	2.151***	0.442***	0.387**
	(0.201)	(0.254)	(0.134)	(0.158)
OC_FIRM5	4.509***	4.419***	0.763***	0.717***
	(0.270)	(0.326)	(0.111)	(0.131)

(*Continued*)

TABLE 7—*Continued*

Panel C: Coefficient estimates and summary statistics for logistic regressions relating an intentional misreporting proxy based on the AAER sample to CEO *fWHR* and proxies of CEO overconfidence

Variable	Pred. Sign	Ordinary Logistic Regression (1)	Conditional Logistic Regression (2)
Intercept		11.384**	
		(5.048)	
D(fWHR > median)	+	1.058***	1.151***
		(0.325)	(0.337)
OC_OPTIONS	+	−0.055	0.111
		(0.352)	(0.372)
D(GrtExp)	−	−0.077	−0.278
		(0.425)	(0.637)
D(GrtPrfl)	?	0.449	0.361
		(0.517)	(0.674)
D(LowRes)	?	0.466	0.723
		(0.374)	(0.473)
Controls		Yes	Yes
Industry fixed effects		Yes	No
Standard errors		Robust	Robust
Pr > χ^2		0.013	0.001
Pseudo-R^2		20.75%	42.09%
Log pseudo likelihood		−180.183	−65.834
N		328	328
D(fWHR > median)	+	1.079***	1.202***
		(0.329)	(0.336)
OC_FIRM4	+	0.594*	0.666*
		(0.311)	(0.385)
D(fWHR > median)	+	1.041***	1.304***
		(0.339)	(0.388)
OC_FIRM5	+	0.805***	1.559***
		(0.308)	(0.432)

Panel A of this table presents the Pearson (upper triangle) and Spearman (lower triangle) correlation matrix of CEO *fWHR* (measured by *D(fWHR > median)*) and the means of the CEO overconfidence proxies (measured by *OC_OPTIONS, OC_FIRM4*, and *OC_FIRM5*) during the sample period using the CEO-level sample of all measurable pictures (*N* = 1,136). Similar results are obtained when we use the sample of good quality pictures (*N* = 763). Variable definitions of *D(fWHR > median)* and the CEO overconfidence variables are provided in appendix A. Panel B (panel C) repeats the analyses in table 4 (table 5, panel B) with the CEO overconfidence proxies as additional control variables.

used, we find that *D(fWHR > median)* remains positively and significantly associated with misreporting. We find positive associations at the 5% level or better in both the measurable and good-quality picture samples as well as in the Cox and the (ordered) logistic regressions. Absent the significance levels, the coefficient estimates on *D(fWHR > median)* are very comparable across the models and the samples. Overconfidence is also positively associated with the

F-score proxies, consistent with the findings of prior work that overconfident managers tend to engage more in earnings manipulation (Schrand and Zechman [2012]).[25]

We conclude that the facial width-to-height ratio measures a distinct managerial trait, different from CEO overconfidence. Compared with the overconfidence proxies, a CEO's facial structure is measured at the CEO level and is not the endogenous outcome of corporate governance mechanisms.

6. *Insider Trading, Stock Option Backdating, and the Executive's Facial Width-to-Height Ratio*

The preceding analyses provide important evidence but are limited to one particular managerial action, namely misreporting. A valid question is whether our findings regarding the *fWHR* effect are limited to accounting choices only. To address this question, we examine the relation between the likelihood that a CEO engages in opportunistic insider trading (stock option backdating) and his *fWHR* to validate our facial masculinity measure further.

6.1 INSIDER TRADING

Our tests rely on an algorithm developed by Cohen, Malloy, and Pomorski [2012], which separates "routine" from "opportunistic" trades by insiders. Routine traders are insiders who placed a trade in the same calendar month over the past three consecutive years. Opportunistic traders are those who have traded in the same years as the routine traders, but without a clear pattern in the past timing of the trades.[26] Cohen et al. document that trades by opportunistic traders (but not those by routine traders) are informative. In particular, they show that opportunistic trading significantly increases the likelihood of future SEC enforcement action (linked to illegal insider trading). Based on this prior work, we predict that the *fWHR* of an executive should be positively associated with the likelihood of him being classified as an opportunistic trader. The insider trading setting is appealing for two reasons. First, insider trading is an intentional *individual* action *by* the executive, and thus the setting facilitates better validating our *fWHR* measure by directly linking the effect of the individual characteristic on behavior. Second, and related, the CEO is not the only insider who trades; we use a sample of male CFOs from the same firm, to allow the inclusion of firm-fixed effects. The tests based on this sample address concerns that correlated omitted firm-level factors complicate inferences.

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[25] Our findings for the relation between the alternative overconfidence proxies and misreporting are not sensitive to the exclusion of *D(fWHR > median)* from the regressions.

[26] Specifically, we employ the rolling window algorithm described in footnote 10 in Cohen, Malloy, and Pomorski [2012]. This approach allows each executive to switch back and forth between being an opportunistic and routine trader.

Our tests are based on the broad S&P1500 firm-year sample described in section 4.1. We establish the identity of 1,399 CFOs in this sample. We then drop the observations corresponding to 124 female CFOs and are left with a sample of measurable (good) quality pictures for 757 (467) CFOs.

Following the Cohen, Malloy, and Pomorski [2012] algorithm, 304 (199) CEOs and 244 (142) CFOs with measurable (good quality) pictures were classified as either routine or opportunistic traders in our sample. The remaining executives were "nonclassifiable" because they either never placed a trade or did not have three consecutive years of trading. Following Cohen, Malloy, and Pomorski [2012], we eliminate those traders from our sample.

Table 8, panel A, reports the distribution of *fWHR* for the full sample that combines the CEOs and CFOs and for each subsample separately. Panel B tests the difference in means when the sample is partitioned on the median *fWHR* for two variables: *Time until opportunistic trader* (i.e., the number of years until the executive is first classified as an opportunistic trader) and *D(Opportunistic trader)* (i.e., an indicator variable equal to one if the executive is classified as an opportunistic trader during the year, or zero otherwise). We find that executives with high-*fWHR* (low-*fWHR*) are classified significantly sooner (later) as opportunistic traders. We also find that the incidence of opportunistic trading is higher (lower) for those CEOs. While this result is also obtained in the full sample, we do not find evidence of a difference in the average incidence of opportunistic trading in the CFO sample.

In panel C, columns 1 and 2, we examine the Cox proportional hazard regression using the full sample. We report all regression results controlling for variables suggested in Cohen, Malloy, and Pomorski [2012]. We find that the hazard rate of being an opportunistic trader is positively and significantly ($p < 0.01$) associated with *D(fWHR > median)*. A high-*fWHR* executive has a 37% (43%) higher hazard of opportunistic trading behavior than a low-*fWHR* executive in the measurable picture (good-quality picture) sample. Using *D(Opportunistic trader)* as the dependent variable in the logistic regression, we find, in columns 3 and 4, a significant positive effect of *fWHR* on the likelihood that the executive is classified as an opportunistic trader. The odds that a high-*fWHR* CEO is classified as an opportunistic trader in any given year are 1.71 (1.64) times higher than the odds for a low-*fWHR* CEO (using the measureable (good-quality) pictures sample). Columns 5 and 6 report the conditional logistic regression that includes a fixed effect for each CEO–CFO pair from the same firm. Note that we have a relatively small subset of observations because this specification puts heavy demands on the data.[27] Despite this, in the measurable picture sample,

[27] Indeed, we need the CEO and CFO from the same firm to be both classifiable (as either routine or opportunistic trader) in the same year. In addition, if both the CEO and CFO have the same value on the dependent variable (i.e., both are either routine traders or opportunistic traders), the firm-fixed effect perfectly predicts the outcome, and the pair is dropped from the analysis.

TABLE 8

Executive's (Either CEO's or CFO's) fWHR and Opportunistic Insider Trading

Panel A: Descriptive statistics for facial width-to-height ratio (*fWHR*) for classifiable inside traders at the individual level

Sample	*N*	Mean	*SD*	P95	Q3	Median	Q1	P5
Full sample of CEOs and CFOs								
All measurable pictures	548	2.059	0.157	2.330	2.166	2.050	1.950	1.810
Good-quality pictures	341	2.052	0.154	2.308	2.163	2.039	1.939	1.808
CEO subsample								
All measurable pictures	304	2.008	0.146	2.262	2.108	2.006	1.901	1.789
Good-quality pictures	199	2.006	0.143	2.262	2.103	2.008	1.889	1.791
CFO subsample								
All measurable pictures	244	2.123	0.146	2.379	2.222	2.117	2.006	1.910
Good-quality pictures	142	2.116	0.145	2.359	2.216	2.121	1.997	1.896

Panel B: Descriptive analysis of differences in proxies for opportunistic insider trading between high- and low-*fWHR* sample

	High-*fWHR*			Low-*fWHR*			*t*-test/Log-rank test	
Variable	*N*	Mean	*SD/SE*	*N*	Mean	*SD/SE*	Diff in Mean	*p*-Value
Full sample of CEO and CFOs								
Time until opportunistic trader (years)	289	4.163	0.269	267	5.110	0.324	−0.947	0.006
D(Opportunistic trade)	485	0.800	0.400	450	0.716	0.452	0.085	0.003
CEO subsample								
Time until opportunistic trader (years)	117	3.902	0.303	188	4.874	0.350	−0.971	0.064
D(Opportunistic trader)	223	0.785	0.412	362	0.707	0.456	0.077	0.039
CFO subsample								
Time until opportunistic trader (years)	172	4.193	0.351	79	5.153	0.515	−0.960	0.016
D(Opportunistic trader)	262	0.813	0.391	88	0.750	0.436	0.063	0.205

(*Continued*)

TABLE 8—*Continued*

Panel C: Hazard ratio, coefficient estimates, and summary statistics for Cox proportional hazard regressions and logistic regressions relating opportunistic insider trading behavior to executive's (either CEO's or CFO's) *fWHR*

	Hazard Rate (Opportunistic Trader)		*D(Opportunistic trader)*			
	Cox Regression		Ordinary Logistic Regression		Conditional Logistic (Paired)	
Variable	Measurable Pictures (1)	Good-Quality Pictures (2)	Measurable Pictures (3)	Good-Quality Pictures (4)	Measurable Pictures (5)	Good-Quality Pictures (6)
D(fWHR > median)	1.373***	1.428***	0.535**	0.496*	1.156**	0.983
	(0.152)	(0.189)	(0.233)	(0.280)	(0.584)	(0.787)
D(GrtExp)	0.766		−0.606*		1.029	
	(0.127)		(0.325)		(0.733)	
D(GrtPrfl)	0.741*		0.013		0.081	
	(0.117)		(0.361)		(1.065)	
D(LowRes)	1.283*		0.398		0.659	
	(0.190)		(0.387)		(0.771)	
Ln(Tenure)	1.000	1.008	−0.054	−0.126	−0.887**	−1.104
	(0.031)	(0.037)	(0.086)	(0.120)	(0.445)	(0.939)
Nr. Trades	1.000*	1.000*	−0.002***	−0.001**	0.001	−0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)
CFO	0.874	0.811	−0.112	−0.313		
	(0.091)	(0.107)	(0.232)	(0.279)		
Geo dispersion: Herfindahl	1.046	1.032	0.218**	0.180		
	(0.046)	(0.049)	(0.109)	(0.121)		
% sales in home state	1.536	2.127**	1.320	1.649		
	(0.494)	(0.766)	(1.027)	(1.013)		
Nr. Products	0.989	0.996	0.015	0.045*		
	(0.009)	(0.011)	(0.022)	(0.025)		
Ln(Market value)	1.058	1.070	−0.049	−0.187		
	(0.048)	(0.062)	(0.131)	(0.146)		
Book to market	0.669	0.678	0.853	−0.186		
	(0.188)	(0.235)	(0.678)	(0.817)		
Volatility	1.026	0.935	−1.670**	−1.665*		
	(0.407)	(0.461)	(0.715)	(0.874)		

(*Continued*)

TABLE 8—*Continued*

Panel C: Hazard ratio, coefficient estimates, and summary statistics for Cox proportional hazard regressions and logistic regressions relating opportunistic insider trading behavior to executive's (either CEO's or CFO's) *fWHR*

	Hazard Rate (Opportunistic Trader)		*D(Opportunistic trader)*			
	Cox Regression		Ordinary Logistic Regression		Conditional Logistic (Paired)	
Variable	Measurable Pictures (1)	Good-Quality Pictures (2)	Measurable Pictures (3)	Good-Quality Pictures (4)	Measurable Pictures (5)	Good-Quality Pictures (6)
Stock return	0.880	0.808	0.416*	0.192		
	(0.127)	(0.146)	(0.225)	(0.258)		
TopExDrct	1.011	0.960	−0.269*	−0.349*		
	(0.086)	(0.103)	(0.160)	(0.185)		
Board size	0.955	0.933*	−0.065	0.052		
	(0.030)	(0.037)	(0.076)	(0.090)		
Independent director	2.642**	4.589***	−1.192	−0.588		
	(1.137)	(2.419)	(1.043)	(1.184)		
Intercept			3.035**	3.311**		
			(1.348)	(1.582)		
Clustered std errors	Firm level	Firm level	Firm level	Firm level	Firm-year level	Firm-year level
Firm fixed effects	No	No	No	No	Yes	Yes
$\Pr > \chi^2$	0.000	0.005	0.000	0.002	0.066	0.269
Pseudo-R^2			9.10%	8.00%	18.50%	16.70%
Log (pseudo) likelihood	−1,779.232	−1,066.954	−468.779	−314.535	−25.981	−9.818
N	1587	1007	935	624	92	34

This table presents an analysis of the association between an executive's facial width-to-height ratio and opportunistic insider trading behavior measured by (1) *Hazard Rate (Opportunistic trader)*, the probability that an executive (either CEO or CFO) is classified as an opportunistic trader in a given year, conditional upon the firm having survived to the beginning of the year, and (2) *D (Opportunistic trader)*, an indicator variable equal to one if an executive is classified as an opportunistic trader in a given year, and zero otherwise. For each firm-year-executive, we define opportunistic inside trader based on an algorithm developed by Cohen, Malloy, and Pomorski [2012] (see appendix A). Following Cohen, Malloy, and Pomorski [2012], we eliminate nonclassifiable traders from our analysis. Panel A presents descriptive statistics for the *fWHR* of classifiable inside traders (either CEOs or CFOs) at the executive level. Panel B presents univariate tests for the firm-year-executives in the broad S&P1500 sample with all measurable pictures. The variable *Time until opportunistic trader* (*D(opportunistic trader)*) of the full sample has 556 firm-executive observations (935 firm-year observations). The "Diff. in Mean" column in panel B reports differences in the variable means between the high- and low-*fWHR* samples. Two-tailed probability values are reported for log-rank test or two-sample *t*-test. Panel C, columns 1 and 2 (3–6), report hazard ratios (coefficient estimates) for Cox proportional hazard regressions (ordinary and conditional logistic regressions) relating *Hazard Rate (Opportunistic trader) (D(Opportunistic trader))* to *D(fWHR > median)* and a vector of control variables. Variable definitions are provided in appendix A. We winsorize the continuous variables at 1% and 99%. We run separate regressions for the sample of all measurable pictures (columns 1, 3, and 5) and for the sample of good-quality pictures (columns 2, 4, and 6). The Cox proportional hazard model estimates how soon in the executive's tenure he is classified as an opportunistic trader. Once classified, we truncate all further observations for the corresponding executive from the sample. For Cox regressions, we use the full tenure of managers who are classifiable because we intend to predict the probability conditional upon the firm having survived to the beginning of the year. For logistic regressions, we only take firm-years in which the executive is a classifiable trader. Columns 5 and 6 show the results for a paired CEO and CFO for a given firm-year. Two-tailed probability values are computed using standard errors clustered by firm or firm-year. ***, **, and * represent statistical significance at 1%, 5%, and 10%. The standard errors are reported in parentheses.

we find a positive and significant ($p < 0.05$) association between *D(fWHR >median)* and being classified as an opportunistic trader. In terms of economic significance, the odds that a high-*fWHR* executive is classified as an opportunistic trader in a year are 3.17 times higher than the odds for a low-*fWHR* executive. While in the good-quality picture sample the estimate on *D(fWHR >median)* is similar in magnitude, it is not significant at the conventional levels, most likely due to a lack of power given the small sample ($n = 34$). Altogether, our findings are strongly supportive of the prediction that *fWHR* is positively associated with opportunistic insider trading.

6.2 STOCK OPTION BACKDATING

When a firm backdates its executive options, it selects favorable past dates (i.e., when the stock price is particularly low) to be the grant date to increase the value of the award. Unlike insider trading, option backdating is not as clearly attributable to an action by the CEO himself. However, this setting allows providing further evidence on the impact of managerial characteristics on corporate decision making.

We follow the procedure outlined in Collins, Gong, and Li [2009] to identify firms that are likely to have engaged in backdating. These authors consider an option backdated if the stock price at the option grant date ranks in the bottom decile of the firm's stock price distribution over a 240-day window surrounding the option grant date. Whenever at least one CEO stock option award within a given year satisfies the backdating criterion, we define *D(Backdating)*, an indicator variable, to be equal to one. We identify that backdating is likely to have taken place in 8.15% of our sample of 4,685 observations.[28] Untabulated findings reveal that the estimated mean survival time corresponding to the number of years in a CEO's tenure until his first option backdating is significantly lower for high-*fWHR* CEOs ($p < 0.05$ in a log rank test). The mean for the indicator variable *D(Backdating)* is significantly higher for CEOs with above-median *fWHR*, implying a higher incidence of backdating in this group.

Table 9 summarizes the results of regressions that link option backdating to CEO facial masculinity. These regressions are specified similarly to those we used to examine misreporting, but include control variables specific to the backdating setting. The full model specification is reported in the online appendix (http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements). Columns 1 and 2 present a positive and significant association between *D(fWHR >median)* and the hazard rate of option backdating, irrespective of the sample that we use. We also find a positive and significant association between *D(fWHR >median)* and the probability of backdating in the logit regressions in column 3. In sum, these results document that the CEO's facial width-to-height ratio is significantly associated with the firm's backdating of options.

[28] The backdating sample is larger than the sample used for the analysis of the *F-score* because the data requirements for the latter are substantially more stringent.

TABLE 9

Hazard Ratio, Coefficient Estimates, and Summary Statistics for Cox Proportional Hazard Regressions and Logistic Regressions Relating CEO Stock Option Backdating to CEO fWHR

		Hazard Rate (Backdating)		*D(Backdating)*	
Variable	Pred. Sign	Measurable Pictures (1)	Good-Quality Pictures (2)	Measurable Pictures (3)	Good-Quality Pictures (4)
Intercept				−9.140***	−5.852
				(2.846)	(3.603)
D(fWHR > median)	+	1.482***	1.381*	0.349**	0.226
		(0.139)	(0.180)	(0.141)	(0.176)
D(GrtExp)	−	0.769		−0.205	
		(0.284)		(0.280)	
D(GrtPrfl)	?	0.870		0.077	
		(0.226)		(0.214)	
D(LowRes)	?	1.645**		0.345*	
		(0.198)		(0.197)	
Controls		Yes	Yes	Yes	Yes
Year fixed effects		Yes	Yes	Yes	Yes
Industry fixed effects		Yes	Yes	Yes	Yes
Clustered std errors		CEO level	CEO level	CEO level	CEO level
Pr > χ^2		0.001	0.001	0.001	0.001
Pseudo- R^2				27.79%	34.00%
Log pseudo likelihood		−1,336.207	−805.261	−941.878	−580.919
% correctly classified				86.70	90.00
N		3,559	2,410	4,631	3,145

This table presents an analysis of the association between *D(fWHR > median)* and CEO stock option backdating measured by (1) *Hazard Rate (Backdating)*, the probability of CEO stock option backdating in a given year, conditional upon the firm having survived to the beginning of the year, and (2) *D(Backdating)*, an indicator variable coded as one if a CEO engages in stock option grant backdating and zero otherwise. The event of option backdating and all other variables are defined in appendix A. The backdating sample is based on the S&P1500 sample and has 4,631 (3,145) firm-years for all measurable pictures (good-quality pictures). Columns 1 and 2 (columns 3 and 4) report coefficient estimates and model summary statistics for Cox proportional hazard regressions (logistic regressions) relating *Hazard Rate (Backdating)* (*D(Backdating)*) to *D(fWHR > median)* and a vector of control variables. We winsorize the variables at 1% and 99%, with the exception of *RD* and *Leverage* for which only the top percentile observations were winsorized. We run separate regressions for the sample of all measurable pictures (columns 1 and 3) and for the sample of good-quality pictures (columns 2 and 4). The Cox proportional hazard model estimates how soon in the CEO's tenure a CEO option backdating occurs. Once option backdating occurs, we truncate all further observations for the corresponding CEO from the sample. Hence, the number of observations is reduced to 3,559 (2,410) for all measurable pictures (good-quality pictures). Two-tailed probability values are computed using standard errors clustered by CEO. ***, **, and * represent statistical significance at 1%, 5%, and 10%. The standard errors are reported in parentheses.

7. *Additional Analyses*

Our measurement of *fWHR* relies on the manual collection of pictures. To conserve on data collection costs, we use the year 2009 as our starting point, identify the CEO in each S&P1500 firm, and collect his picture. We then gather data for the complete employment record of the CEO with the firm from (potentially) 1996 to 2010. Doing so, the number of observations we have for each CEO in the sample varies. One concern with this procedure could be that the period over which a CEO enters the sample varies with some unobserved characteristics that are also associated with misreporting. We further address this issue in three ways.[29] First, we compare the low- and high-*fWHR* groups with respect to the distribution of the total number of observations for each CEO in the sample. Results of a Kolmogorov–Smirnov test show that there are no significant differences in the distributions between these two groups (corrected p-value $= 0.681$). Second, we only use the 2009 data and run the multivariate regressions of misreporting using this single cross-section to account for the potential concern that our variable of interest is fixed at the CEO level. We continue to find a positive and significant association between *D(fWHR > median)* and misreporting. Third, for the misreporting regressions based on the restatement proxy, we implement a one-to-one matching procedure. For each restatement firm, we keep the first "misreporting year" in the sample. Then, for each first misreporting year, we identify an industry size–matched non-misreporting firm. We continue to find a positive and significant association between *D(fWHR > median)* and intentional misreporting in a logit regression.

In a second set of additional tests, we examine whether controlling for the *fWHR* of the *CFO* changes our inferences. Since the CFO is intimately involved in the process of drawing up the financial statements, it is reasonable to conjecture that his propensity for masculine behavior is associated with misreporting as well. We show in the online appendix (http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements) that including the CFO's *fWHR* in our regressions does not materially affect the inferences with regard to the CEO's *fWHR*. Indeed, the coefficient on the CFO's *fWHR* never attains significance (even when we drop the CEO's *fWHR* from the analysis).

8. *Conclusions*

Regulators, investors, academics, and managers do not need to be persuaded that financial misreporting can pose a very significant threat to the proper working of capital markets. Prior studies have examined in depth many firm-level and market factors that are associated with financial misreporting. The most recent work considers the role of senior managers in the financial reporting process and recognizes that these senior managers have "style." A continuing challenge is to describe the relevant managerial styles and show their respective relation with reporting decisions. We draw attention to work in behavioral endocrinology and neuropsychology that considers the possibility of a link between the facial masculinity and a complex of related behaviors. While this work is still in development and the understanding of the exact mechanisms that are responsible for the association between facial masculinity and these behaviors is less than complete, some results are available. To us, the most important of these results

[29] Some of those analyses are reported in the online appendix (http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements).

are the following three: (1) testosterone is implicated in a complex of human behaviors that include aggression, egocentric behavior, risk seeking, and an individual's propensity to cheat as well as the desire to maintain status, (2) the facial width-to-height measure appears to predict variation in these "masculine" behaviors across individuals, and (3) the correlation between facial masculinity and masculine behaviors is strong notwithstanding the ongoing debate on whether this correlation is caused by the effect of testosterone. We use these findings to conjecture a relation between the facial structure of CEOs and their firms' financial misreporting. Our results support that firms with CEOs with more (less) masculine faces have a higher (lower) incidence of misreporting. This result is obtained by using proxies for misreporting based on prediction models (which might misclassify some firms as misreporting) as well as using AAER-based proxies (which might underestimate the number of misreporting firms).

We document that masculine behaviors (as measured by facial width-to-height ratio) are different from overconfidence, a personality trait that has been examined in the context of misreporting as well, and, in particular, that our results are not sensitive to including proxies for overconfidence taken from prior work. Finally, we extend our analysis to a nonaccounting setting and show that a given CEO's facial structure is also associated with the incidence of opportunistic insider trading and option backdating.

Our results are subject to several caveats. First, we do not have a measure of CEO testosterone exposure, but only a facial shape measure that predicts variation in masculine behaviors. For inferences about the role of testosterone, one would ideally draw saliva and serum samples from subjects to examine their biochemical composition (and (base) testosterone content) in a laboratory. Practical considerations make it unlikely that this will ever be possible for a broad cross-sectional sample of CEOs from listed U.S. companies. Second, the understanding of the mechanism that links hormones to human behavior is not complete. It is also not clear whether testosterone in particular is directly responsible for the documented correlation between behavior and the male face. Hence, while our tests support the idea that we are documenting higher *intentional* misreporting *by* CEOs with more masculine faces, drawing inferences about the determinacy of the relation between facial structure and financial misreporting is not advisable. Further, to the extent that there is endogenous matching of CEOs with firms on the basis of some unobserved characteristics (which are not captured by the facial structure variable or our broad set of controls), this reduces the plausibility of a causal interpretation of our results and biases the estimates we report. Finally, the literature has documented significant associations between testosterone and body mass index and between testosterone and intelligence (Azurmendi et al. [2005]). Obesity and testosterone might have a common link with facial shape (Mayew [2013], Osuna et al. [2006]). Earlier studies in economics have used measures of obesity and intelligence to explain variation in financial decision making. We cannot exclude the possibility, therefore, that our facial shape measure partially includes the effect of obesity and intelligence. An interesting avenue to explore in future research is how facial structure and "CEO ability" are associated. Regrettably, even the state-of-the-art proxy for managerial ability available for broad-based empirical work (Demerjian et al. [2012]) measures this construct at the level of the top management team rather than at the individual CEO level. Exploring the influence of managerial ability on the relations documented in this study will have to be postponed until an (exogenous) individual measure of ability becomes available.

APPENDIX A

Variable Definitions

Facial structure variables

1. *fWHR* is the CEO's facial width-to-height ratio. It is measured as the distance between the left and right zygion (bizygomatic width) relative to the distance between the upper lip and the highest point of the eyelids (upper facial height).
2. *D(fWHR > median)* is an indicator variable coded as one if a CEO's facial *fWHR* is above the median in the CEO-level sample and zero otherwise.
3. *D(GrtExp)* is an indicator variable that takes the value of one if a CEO's face is nonneutral in the picture (i.e., the 83 pictures reported in table 1) and zero otherwise.
4. *D(GrtPrfl)* is an indicator variable that takes the value of one if a CEO's face is tilted by more than 45° in the picture (i.e., the 136 pictures reported in table 1) and zero otherwise.
5. *D(LowRes)* is an indicator variable that takes the value of one if the resolution of a CEO's picture is low (i.e., the 72 pictures reported in table 1) or if the *fWHR* measurement error generated by the two research assistants is greater than 5% (i.e., the 82 pictures reported in table 1) and zero otherwise.

Proxies for misreporting

1. *F-score* is a scaled logistic probability derived from a misstatement-prediction model developed by Dechow et al. [2011] for the purpose of predicting accounting manipulations disclosed in the SEC AAERs. We use prediction model 1 from Dechow et al. [2011] to compute the variable based on data from Compustat, CRSP, I/B/E/S, and Execucomp as needed. The Compustat variables are from the unrestated, as-first-reported Compustat database to avoid data backfilling that occurs in the Compustat fundamentals database in the event of a restatement.
2. *F-risk* is an ordinal scale classified according to the level of misreporting risk. We follow the critical values of the *F-score* documented in Dechow et al. [2011] to identify the misreporting risk level as below:

"Normal or low" risk: if *F-score* < 1 then *F*-risk $= 0$; "Above normal" risk: if $1 <$ *F-score* ≤ 1.85 then *F*-risk $= 1$; "Substantial" risk: if $1.85 <$ *F-score* ≤ 2.45 then *F*-risk $= 2$; "High" risk: if *F-score* > 2.45 then *F*-risk $= 3$ (source: Compustat).

3. *D(AAER)* is an indicator variable that equals one if the firm is subject to an AAER and zero for matched non-AAER firms.
4. *D(EXECaccused)* is an indicator variable that equals one for an executive (either CEO or CFO) accused by name as a perpetrator in the AAER and zero otherwise.

Firm-level control variables

1. *Adjusted return* is the market-adjusted buy-and-hold stock return defined as the annual buy-and-hold return inclusive of delisting returns minus the annual buy-and-hold value-weighted market return (source: CRSP).
2. *Volatility* is the standard deviation of monthly stock returns computed over the past five years (source: CRSP).
3. *ROA* is defined as net income over beginning-of-period total assets (source: Compustat).
4. *Book to market* is the book value of total assets relative to the sum of the market value of equity and the book value of liabilities (source: Compustat).
5. *Price to earnings* is the market value of the company at the end of the fiscal year divided by net income of the fiscal year (source: Compustat).
6. *Loss* is an indicator variable that is equal to one when net income is negative and zero otherwise (source: Compustat).
7. *Leverage* is the ratio of long-term debt to total assets at the end of the fiscal year (source: Compustat).
8. *Market value* is the market value of the company at the end of the fiscal year (in millions of dollars; source: Compustat).
9. *Sales growth* is the one-year percentage change in sales for the year prior to the current fiscal year (source: Compustat).
10. *Free cash flow* is the net cash flow from operating activities at fiscal year t less average capital expenditures of the three years prior to year t, scaled by current assets at $t-1$ (source: Compustat).
11. *RD* is defined as total research and development expenses scaled by sales (source: Compustat).
12. *Firm age* is measured as the fiscal year of the observation minus the year the firm first appeared on CRSP.

CEO level control variables

1. *CEO age* is the CEO age reported in Execucomp.
2. *CEO tenure* is the CEO tenure in years computed based on the start of employment as reported in Execucomp.

Governance variables

1. *CEO power* is an ordinal scale variable that is equal to one if the CEO is also the chair of the board and is equal to two if the CEO in addition to being the chair is also the president of the company; in all other cases *CEO power* equals zero (source: Execucomp).
2. *Inside CEO* equals one if the CEO is promoted from inside the company (source: Execucomp).
3. *TopExDrct* is the number of top-five highly paid executives who are also directors (source: Execucomp).
4. *CEO appoint* is the percentage of board members appointed during the CEO's tenure (source: Riskmetrics).
5. *Independent director* is the percentage of board members who are independent (source: Riskmetrics).
6. *Board size* is the number of directors sitting on the board (source: Execucomp).

Compensation variables

1. *Salary* is the annual salary in thousands of dollars (source: Execucomp).
2. *Bonus* is the annual bonus in thousands of dollars for observations before December 15, 2005. After this date, following Hayes, Lemmon, and Qiu [2012], *Bonus* is the sum of the annual bonus and nonequity incentives in thousands of dollars (source: Execucomp).
3. *New option PPS* is the sensitivity of the newly issued total value of options held by the CEO to a 1% change in stock price and is measured at fiscal-year end. The measure is based on Core and Guay [1999].
4. *New stock PPS* is the sensitivity of the total value of newly issued stocks held by the CEO to a 1% change in stock price, and is measured at fiscal-year end. The measure is based on Core and Guay [1999].
5. *Equity holdings* is equal to the sum of in-the-money unexercised options (opt_unex_exer_est_val + opt_unex_unexer_est_val) and shares owned (shrown_excl_opts × prcc_f) in thousands (sources: Execucomp).

Industry control variable

1. *%Misreporting industry members* is the number of misreporting firms (defined by the corresponding misreporting proxy) in the firm's two-digit SIC code divided by the total number of firms in that two-digit SIC code that year.

Proxies for overconfidence (Schrand and Zechman, [2012])

The overconfidence proxies (i.e., *OC_OPTIONS*, *OC_FIRM4*, and *OC_FIRM5*) are constructed based on the following variables:

1. *OPTIONDELAY* is the natural logarithm of in-the-money unexercised exercisable options held by the CEO, equal to opt_unex_exer_est_val + 0.01 (source: Execucomp).
2. *XSINVEST_INDADJ* is the residual from a regression of total asset growth on sales growth, adjusted for the industry median (source: Compustat).
3. *ACQUIRE_INDADJ* is defined as net acquisitions from the statement of cash flows, adjusted for the industry median (source: Compustat).
4. *DERATIO* is the debt-to-equity ratio defined as long-term debt plus short-term debt, scaled by the total market value of the firm. The total market value equals the sum of the market value of equity plus the book values of long-term debt and preferred stock (source: Compustat).
5. *DERATIO_INDADJ* is equal to *DERATIO*, adjusted for the industry median.
6. *RISKYDT* is an indicator variable coded as one if either convertible debt or preferred stock is greater than zero; and zero otherwise (source: Compustat).
7. *DIVYLD* is the dividend yield that is equal to dividends per share divided by share price for the firms that pay dividends and zero otherwise (source: Compustat).

The overconfidence proxies are defined as below:

1. *OC_OPTIONS* is an indicator variable coded as one if *OPTIONDELAY* is greater than the industry median and zero otherwise.
2. *OC_FIRM4* is an indicator variable coded as one if the firm meets the requirements of at least two of the four criteria following and zero otherwise:
   a) *XSINVEST_INDADJ* is greater than zero;
   b) *ACQUIRE_INDADJ* is greater than zero;
   c) *DERATIO_INDADJ* is greater than zero;
   d) *RISKYDT* is equal to one.

3. *OC_FIRM5* is an indicator variable coded as one if the firm meets the requirements of at least three of the five criteria following and zero otherwise: (a) – (d) are the same as for *OC_FIRM4* and (e) *DIVYLD* is equal to zero.

Insider trading variables and the additional control variables for insider trading

1. *D(Opportunistic trader)* is an indicator variable equal to one if an executive (either CEO or CFO) is identified as an opportunistic trader in a given year, and zero otherwise. Opportunistic traders are identified using an algorithm described in Cohen, Malloy, and Pomorski [2012]. A routine trader is an insider who placed a trade in the same calendar month for at least three consecutive years. Opportunistic traders are

insiders who also traded in the past three consecutive years but without an obvious discernible pattern. The individuals are identified at the end of the third year. Specifically, we use the rolling window algorithm described in footnote 10 in Cohen et al. This approach allows each executive to switch back and forth between being an opportunistic and routine trader (source: CRSP and Insider Trading).

2. *Nr. Trades* is the number of trades a given insider has made so far (source: Insider Trading).
3. *Geo dispersion: Herfindahl* is a Herfindahl index of the firm's operations (sales) across all states in which it operates (source: Compustat).
4. *% sales in home state* is the percentage of the firm's operations located in the state where the firm has its headquarters (source: Compustat).
5. *Nr. Products* is the number of products that the firm reports to sell (source: Compustat).

Backdating variables

1. *D(Backdating)* is an indicator variable equal to one if a CEO engaged in stock option grant backdating, and zero otherwise. Backdating is identified using an algorithm described in Collins, Gong, and Li [2009]. An option award is defined as backdated (nonbackdated) if the stock price at the option grant date ranks in (above) the bottom decile of the firm's stock price distribution over a 240-day window surrounding the option grant date. The backdating firm-years consist of firm-years that have at least one backdated CEO stock option granted (source: CRSP and Insider Trading).

APPENDIX B

Illustration of Measuring the Dimensions of the Face in Pictures

Measure of *fWHR*: the horizontal lines represent the distance between the upper lip and the highest point of the eyelids (upper face height). The vertical lines represent the maximum distance between the left and right cheekbones (bizygomatic width). *fWHR* is calculated as width divided by height.
Source: Lefevre et al. [2013]

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