

NOW YOU SEE ME: HOW STATUS AND CATEGORICAL PROXIMITY SHAPE MISCONDUCT SCANDALIZATION

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Despite the formidable consequences for firms of having their misconduct publicized—and thus scandalized—we know little about why only some misconduct instances become scandals beyond the idea that high-status firms’ transgressions are scandalized more often. Focusing on the media’s essential role in scandalizing misconduct, we take a media routines perspective to theorize how the status of past transgressors inside and outside the focal transgressor’s industry creates different contexts that shape the likelihood of scandalization. We argue that the prevalence of past transgressions by high-status firms within the industry leads journalists to scrutinize the misconduct more, amplifying the effect of the focal firm’s status by highlighting its commonalities with past transgressors. Conversely, the prevalence of transgressions by high-status firms outside the industry attenuates the firm status effect on scandalization by directing media attention outside the industry, limiting the information that can be inferred from firm status. Past transgressors’ status and their categorical proximity to current transgressors serve as boundary conditions for the scandalizing effect of status. Our contribution lies in elucidating contextual factors that influence how status acts as an antecedent of scandals, and explaining how status and categories feed media routines that influence the likelihood of firm misconduct being scandalized.

Research has consistently found that individuals are drawn to high-status actors’ wrongdoing because status imparts salience to their misconduct (e.g., Adut, 2005, 2008; Graffin, Bundy, Porac, Wade, & Quinn, 2013). Apart from the attention high-status actors’ behaviors receive (Sauder, Lynn, & Podolny, 2012), audiences also expect high-status actors to represent the highest values (Pollock, Lashley, Rindova, & Han, 2019). Violating these expectations thus makes high-status actors’ misconduct seem more deviant than lower-status actors’ misconduct—and therefore

more newsworthy (Graffin et al., 2013)—giving birth to scandals, or “publicized transgressions that run counter to established norms” (Piazza & Jourdan, 2018: 165).

Aside from the transgressors’ status, however, scandalization’s antecedents are poorly understood as they have received remarkably little research attention (for exceptions, see Nyhan, 2014, 2017). Rather, most research on the scandalizing effect of status has asserted that an event is a scandal (e.g., Dewan & Jensen, 2020; Graffin et al., 2013; Jonsson, Greve, & Fujiwara-Greve, 2009; Piazza & Jourdan, 2018) and then looked at how differences in status influence the way the scandal affects different actors. This is problematic because scandals originate from misconduct that occurs in private, and that may be tolerated if it remains private (Adut, 2005). Indeed, Adut (2008: 19) defined scandalizable misconduct as “anything that will bring about shame or that will embarrass or provoke when made public.” Misconduct is scandalized only if it is widely publicized by a

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mediating institution, such as the press (Thompson, 2000). Thus, while the misconduct's nature is a critical component of scandalization, we know less about why the media publicizes, and thus scandalizes, some instances of misconduct but not others.

Further, most research on the relationship between status and scandalization has treated the misbehaving actor's status as decontextualized. This is also problematic because the context in which an event or action occurs can differentially shape assessments and responses, sometimes even inverting the relationships (Johns, 2006, 2017). Thus, "both the type and characteristics of a specific context may substantially influence the formation of news media coverage" (Graf-Vlachy, Oliver, Banfield, König, & Bundy, 2020: 55). We have little insight, however, about how context shapes the scandalizing effect of status.

As status perceptions are inherently relational (Hubbard, Pollock, Pfarrer, & Rindova, 2018; Podolny & Phillips, 1996), understanding how the media interprets and reacts to an actor's status requires considering other actors in its environment—such as past transgressors—who, of course, have their own status (Dewan & Jensen, 2020; Han & Pollock, 2021). This is because audiences' evaluations of one actor can influence how they evaluate another actor (Boivie, Graffin, & Gentry, 2016), and high-status actors' behaviors often serve as the basis for evaluating others' behaviors (Rao, Monin, & Durand, 2005). Yet, the influence of past transgressors is complicated when considering the divergent views on how misconduct prevalence affects the attention allocated to a firm's current misconduct (Ahmadjian & Robinson, 2001; Zavyalova, Pfarrer, Reger, & Shapiro, 2012). While increasing prevalence can deprive misconduct of its novelty and salience, reducing the attention paid to each instance and creating "safety in numbers" (Pollock, Rindova, & Maggitti, 2008; Zavyalova et al., 2012), audiences—including journalists (Shoemaker & Reese, 2013)—can also sense a pattern in the misconduct's proliferation (Rindova, Ferrier, & Wiltbank, 2010; Whitson & Galinsky, 2008), leading them to scrutinize new instances more (Desai, 2011; Dewan & Jensen, 2020; Han, Pollock, & Paruchuri, 2023; Naumovska, Zajac, & Lee, 2021). Although seemingly contradictory, both views nonetheless highlight the significant influence past transgressors have on audiences' evaluations.

We disentangle these divergent views and offer a clearer understanding of when and how the prevalence of others' misconduct leads to each dynamic by exploring how high-status past transgressors inside and outside the focal transgressor's industry

(hereafter, high-status "insiders" and "outsiders," for brevity) each shape misconduct scandalization. Past transgressors' status matters because it shapes media routines (Graf-Vlachy et al., 2020). While journalists are biased toward novel cues (Chandler, Polidoro, & Yang, 2020; Zavyalova et al., 2012), they have limited attentional resources and thus seek assurances that a topic is newsworthy (Pollock et al., 2008; Shoemaker & Reese, 2013). High-status firms' repeated misconduct—each instance of which could potentially incur significant publicity (Adut, 2005, 2008; Graffin et al., 2013)—reinforces rather than reduces the misconduct's salience (Paruchuri, Han, & Prakash, 2021), and assures journalists the new instance is newsworthy (Pollock et al., 2008).

This salience-enhancing effect, however, amplifies the scandalizing effect of the focal firm's high status when the past and current transgressors operate in the same industry. Status hierarchies are often bounded by horizontal categories, such as industries; thus, a firm's market identity is defined at the intersection of its vertical status position and horizontal industry membership (Delmestri & Greenwood, 2016; Han & Pollock, 2021; Jensen, Kim, & Kim, 2011; Rao et al., 2005). Because high-status firms are considered exemplars of their industry's norms and practices (Han & Pollock, 2021; Pollock et al., 2019), the perceived commonalities among the past and current high-status transgressors within an industry draw journalists' attention to the industry and its potentially systemic wrongdoing (Han et al., 2023; Paruchuri & Misangyi, 2015), placing the current misconduct within a larger, newsworthy context (Bednar, Boivie, & Prince, 2013; Desai, 2011). For instance, journalists can develop richer storylines when they perceive a firm's market identity as a high-status *software* company by tapping into the firm's stature in the software industry, compared to viewing the firm as just a high-status company.

When there is a high level of misconduct outside the transgressor firm's industry, however, journalists are more likely to ignore the current misconduct, even by a high-status firm, as it does not fit a simple and compelling industry-based narrative. Prior data breaches involving Hilton and Hyatt may cause a data breach by Marriott to seem more newsworthy (Kelleher, 2019), but they would not make a data breach by Walmart more newsworthy, and could even reduce the likelihood it would be covered if journalists were to perceive a hospitality industry-based pattern.

We explored these dynamics using data breaches during 2015–2018 involving publicly traded U.S.

firms. Data breaches, which result in the unauthorized exposure of customers' private information, can damage customers' trust in firms (Culnan & Armstrong, 1999; Martin, Borah, & Palmatier, 2017) and are a serious social issue (Accenture, 2019), topping executives' concerns (Petersen, 2020). Our findings suggest that past transgressors' status and industry membership create important boundary conditions for status's effect on scandalization. Specifically, the positive effect of a focal transgressor's high status on the likelihood of scandalization was amplified by high-status insiders' past transgressions, but was weakened by high-status outsiders' past transgressions.

We contribute to the literatures on status and organizational misconduct by highlighting the importance of the context surrounding a focal firm's transgression (Johns, 2006), identifying past transgressors' status as a key boundary condition for the well-established effect of status on scandalization (Adut, 2005, 2008; Graffin et al., 2013). Johns (2006, 2017) has argued that most theories give contextual factors too little consideration, which can lead to misunderstanding the mechanisms at work and inconsistent results across studies. Graf-Vlachy and colleagues (2020: 57) also called for more research taking "a cross-context perspective that explicitly contrasts the antecedents of media coverage in different contexts." We address these limitations by suggesting that past transgressors' status and categorical proximity to the focal transgressor are important yet overlooked contextual factors that influence whether the transgressor's misconduct is scandalized, explaining when there is safety or scrutiny in numbers (Desai, 2011; Naumovska et al., 2021; Rindova et al., 2010). Further, our theory and findings offer new insights into how audiences combine status and categories in their evaluations (Delmestri & Greenwood, 2016; Han & Pollock, 2021; Jensen et al., 2011).

THEORY AND HYPOTHESES

What Constitutes a Scandal?

Although scholars have offered slightly different definitions of scandal (e.g., Adut, 2005; Esser & Hartung, 2004; Nyhan, 2014; Piazza & Jourdan, 2018; Thompson, 2000), they have all focused on actions that the broad public is likely to find objectionable, and incorporated the three elements Thompson (2000) outlined: (a) some degree of public knowledge about the action or event, (b) the fact that the "public" includes nonparticipating observers, and (c) a process for making the actions or events visible to

others. Thompson (2000: 19) went on to note, "activities that remain invisible to non-participants cannot, *ipso facto*, be scandalous." Thus, scandals arise when actors engage in behaviors that transgress social norms and values in disruptive ways, and their behaviors are brought into the public forum (Dewan & Jensen, 2020; Graffin et al., 2013; Piazza & Jourdan, 2018), typically by the media (Graffin et al., 2013; Nyhan, 2014, 2017; Thompson, 2000).

The media's role is critical because, as Nyhan (2014: 436) noted, scandals are typically defined "as the result of the disclosure of some act of wrongdoing or norm violation" but "whether any specific case meets such a standard is often unclear or contested," and context influences perceptions of whether a scandal has occurred (Nyhan, 2017). Thompson (2000: 16) similarly noted, "scandals are often rather messy affairs, involving the alleged transgression of values and norms which are themselves subject to contestation." Thus, the media must assess whether to publicize, and potentially scandalize, a transgression. In his study of how Oscar Wilde's homosexuality was scandalized during the Victorian Era, Adut (2005) theorized that for a transgression to become a scandal, the benefits of publicizing the transgression must outweigh the costs of attracting and coordinating the public's disapproval. Nyhan (2014, 2017) similarly noted that for political scandals context is important, and whether a president's or governor's transgression became a scandal depended on their approval ratings and the extent to which there were competing news stories.

The need to balance the costs and benefits of scandalizing misconduct is therefore the reason why not all transgressions are scandalized (Adut, 2005; Graffin et al., 2013; Nyhan, 2014, 2017). To understand how the media assesses the costs and benefits of publicizing—and potentially scandalizing—misconduct, it is important to first understand how perceptions of a story's newsworthiness and the media's routines shape its coverage decisions (Graf-Vlachy et al., 2020; Oliver, Campbell, Graffin, & Bundy, 2023; Pollock et al., 2008; Shoemaker & Reese, 2013).

Newsworthiness and Media Routines' Roles in Creating Scandals

Newsworthiness. Summarizing the media literature (e.g., Lee, 2009; Shoemaker, Danielian, & Brendlinger, 1991), Oliver and colleagues (2023) identified two factors that influence a story's potential newsworthiness: deviance and social significance.

Deviance refers to characteristics that set actors apart from others in their community, region, or other categories. (Oliver et al., 2023; Shoemaker et al., 1991), making them stand out relative to others (Lippmann, 1922). Deviance can take a variety of forms, from violating social norms to statistical oddities, but it should be interesting to the broader public. Negative forms of deviance are particularly noteworthy (Rozin & Royzman, 2001; Shoemaker, 1996), since humans are hard-wired to be on the lookout for threats (Shoemaker, 1996), and negative information shapes impression formation and ability assessments (Skowronski & Carlston, 1989).

Social significance refers to the actor's significance to some group or audience based on their cultural, economic, or institutional influence (Oliver, et al., 2023; Shoemaker et al., 1991). The more prominent or influential the actor, the greater their social significance. For example, "large and highly visible firms that are known for significantly impacting many stakeholders are likely to be perceived as more socially significant compared to smaller, less visible firms with a smaller perceived political, economic, or cultural impact" (Oliver et al., 2023: 1218). Socially significant actors, such as high-status firms, are more likely to receive ongoing attention, even for behaviors that are not deviant, because of their social significance.

We argue that data breaches by high-status firms are, all else equal, likely to be newsworthy events because data breaches are a form of "normative" deviance that breaks the firm's social contract with its customers that it will keep their data safe, and can result in significant financial harm if the breached data are used for identity theft or other illegal purposes (Culnan & Armstrong, 1999; Gwebu, Wang, & Wang, 2018; Martin et al., 2017). Moreover, audiences have high expectations about the values high-status actors will uphold (Han & Pollock 2021; Pollock et al., 2019), and violating these expectations can lead audiences to perceive high-status actors' misconduct as more deviant than similar misconduct by lower-status actors (Graffin et al., 2013). Further, high-status firms are socially significant, and thus garner substantial attention, since they occupy the top ranks of a social hierarchy and are typically among the most influential firms in their industries, and sometimes the economy (Hoffman & Ocasio, 2001; Rao et al., 2005; Sauder et al., 2012).

Research has shown that high-status actors' misconduct is more likely to receive wide attention and become scandalized (e.g., Dewan & Jensen, 2020; Graffin et al., 2013). For example, Graffin and

colleagues (2013) found that when a scandal involving the expense accounts of British Members of Parliament (MPs) erupted, high-status MPs were more likely to have their behavior scandalized by the media and to lose their seats than were lower-status MPs. Dewan and Jensen (2020) similarly found that high-status firms were more likely to be the target of the Securities and Exchange Commission's enforcement for securities fraud when they were part of multi-actor scandals. Thus, although established, we treat this as our baseline relationship and hypothesize:

Hypothesis 1. High-status firms are more likely than non-high-status firms to have their misconduct scandalized by the media.

Although prior research may create the impression that high status invariably leads to the transgression's scandalization, given the uncertainties about newsworthiness and the routines employed to manage the costs of routinely producing news, the media will not, and cannot, cover all misconduct—even by high-status firms. Instead, it will lavish significant attention on some misconduct, leading to its scandalization. Hence, we explore how the media's routines for producing news enhance or reduce the likelihood that a particular high-status firm's misconduct is scandalized.

Media routines. Publishing news is a demanding job. McQuail (1985) noted that journalists face pressures to consistently produce articles about newsworthy events on very tight deadlines; thus, they engage in behaviors that simplify and speed up the process of establishing newsworthiness and producing news articles (Pollock et al., 2008). Media routines are "established practices shared across members of the journalistic profession that shape how an individual journalist creates coverage" (Graf-Vlachy et al., 2020: 57). One set of routines has to do with sourcing and producing news. To expedite this process, journalists are often assigned to "beats," where they routinely cover the same government agencies, organizations, or industries (Davies, 2008; Shoemaker & Reese, 2013). This allows them to gain industry expertise and cultivate sources whom they use routinely (Westphal & Deephouse, 2011). It also allows them to build on their prior stories, develop narrative arcs, and reduce the amount of new content they need for each article (Hirsch, 1977; Shoemaker & Reese, 2013).

Because of the uncertainty involved in identifying newsworthy stories, the media bears the costs of assessing the public's potential interest and deciding whether to bring a transgression to light (Dewan &

Jensen, 2020; Graffin et al., 2013; Piazza & Jourdan, 2018). To manage these costs, journalists also consider what stories others are covering, share sources, frame their stories similarly (Rogers, 2002; Sigal, 1973), and try to cater to their readers' tastes by "forgo[ing] complex, technical explanations, and instead build[ing] on their audiences' experiences and schemas" (König, Mammen, Luger, Fehn, & Enders, 2018: 1201). Journalism's reward systems also affect coverage decisions (Pollock et al., 2008; Shudson, 1986). Journalists are not rewarded for being the only ones covering a story; if no one else covers it, the story is likely less newsworthy than anticipated, and the journalist faces a greater risk of backlash (Davies, 2008) and embarrassment (Shoemaker & Reese, 2013) if what they report turns out to be wrong, because they are more easily singled out (Pollock et al., 2008). Rather, journalists are rewarded for getting the "scoop"—that is, being the first to report on a story that others are chasing (Shudson, 1986). In addition, by framing their stories similarly, it is more difficult to single out any one journalist for punishment. These routines and reward structures generate "cascade" effects in media coverage (Pollock et al., 2008) that can result in scandalization.

We explore how the extent to which other high-status firms have previously been involved in similar misconduct, and whether these high-status firms are in the focal firm's industry, affect the coverage and scandalization of the firm's transgression. As status reflects an actor's position within "a socially constructed, intersubjectively agreed-upon and accepted ordering or ranking ... in a social system" (Washington & Zajac, 2005: 284), audiences' reactions are influenced by how others of similar status behave (Han & Pollock, 2021; Podolny & Phillips, 1996; Sauder et al., 2012), but we argue that these reactions will differ based on the context in which they occur.

Two Perspectives on Others' Misconduct

Scholars have found that increasing prevalence changes audiences' evaluations of negative behaviors, where repeated occurrences decrease the salience of later instances, allowing later transgressors to hide behind the accumulating misconduct—that is, providing "safety in numbers" (Ahmadjian & Robinson, 2001; Zavyalova et al., 2012). Because people tend to allocate their attention to subjects that seem novel or unusual relative to others (Jones & McGillis, 1976), a single firm's misconduct attracts attention by sticking

out, but the misconduct's proliferation diminishes its salience and newsworthiness, which decreases the attention paid to each firm's misconduct (Zavyalova et al., 2012). Indeed, a *Wall Street Journal* article suggested that a "string of major data breach disclosures ... may leave consumers numb to future cyberattacks" (Janofsky, 2017).

An alternative psychological tendency also exists, where people look for patterns in repeated cues (Rindova et al., 2010; Whitson & Galinsky, 2008), and the patterns become newsworthy. Observers could thus perceive a pattern of misconduct based on repeated instances and consequently scrutinize current instances more intensely (Desai, 2011; Han et al., 2023; Naumovska et al., 2021). For example, when Capital One disclosed on July 19, 2019, that its customers' personal data had been exposed, the media wrote hundreds of articles about it. Noticeable in the articles publicizing this incident were the numerous mentions of prior data breaches, warning the public that the Capital One breach was no exception, and that they should expect many more breaches (Andriole, 2019; Kiesnoski, 2019; Youn, 2019). Although different factors may have been at play, the safety-in-numbers effect suggests that the Capital One breach should not have received such extensive media coverage, given journalists' awareness of the many prior data breaches, yet these breaches were used as evidence of the Capital One breach's importance, rather than a reason to ignore it.

We argue that whether the safety-in-numbers effect or scrutiny of trends occurs—and thus whether the deviance is deemed newsworthy, and becomes scandalized—depends on the prior perpetrators' social significance and category membership, and the media's routines.

Past Transgressors' Status and Industry Boundaries

High-status insiders' transgressions. Although deviant actions by high-status firms are likely to be newsworthy (Oliver et al., 2023; Shoemaker et al., 1991), their behaviors do not occur in a vacuum, and journalists often look to others' behaviors for guidance when interpreting their behaviors (Pollock et al., 2008; Shudson, 1986)—in particular, other high-status firms (Rao et al., 2005). Dewan and Jensen (2020) argued that information on other transgressors facilitates sanctioning high-status actors because it reduces uncertainty about mislabeling the actors' behavior as misconduct. They stated that it "shifts the

burden of proof from the accuser to the accused,” turning the rule of “innocent until proven guilty” into the rule of “guilty until proven innocent” (Dewan & Jensen, 2020: 1656). We argue that the proliferation of high-status insiders’ misconduct heightens the likelihood that journalists will generalize their culpability to other actors in the same industry (Desai, 2011; Han et al., 2023) making the pattern of misconduct a newsworthy narrative. For example, an article published by *Forbes* concluded that “hotels are not doing enough to protect their guests’ privacy” after seeing “high-volume data breaches at a slew of major hotel brands” (Kelleher, 2019), suggesting that past transgressions by high-status firms facilitate developing industry-based narratives. Similarly, a *Wall Street Journal* article described Vera Bradley’s data breach in October 2016 as “join[ing] a host of other affected retailers [that had had breaches that year]” (Steele, 2016; emphasis added), hinting that journalists identify industry-based patterns and craft their narratives accordingly.

Upon the misconduct’s disclosure, there is likely limited information about its cause and consequences (Bundy & Pfarrer, 2015; Lange & Washburn, 2012), and the media thus face significant uncertainty about the misconduct’s newsworthiness (Shoemaker & Reese, 2013; Shudson, 1986). Journalists therefore rely on heuristics, such as the actor’s social significance—or status—to inform their coverage decisions (Graf-Vlachy et al., 2020; Oliver et al., 2023). The salience of past transgressors’ status, however, depends on whether they belong to the same industry as the current transgressor, because audiences group similar firms and establish comparative referents based on industry categories (Porac, Wade, & Pollock, 1999). Accordingly, research on misconduct spillovers has long identified the industry as the boundary to which customers, investors, and the media extend their scrutiny (Jonsson et al., 2009; Paruchuri & Misangyi, 2015; Zavyalova et al., 2012).

Category boundaries also delineate status hierarchies’ boundaries (Delmestri & Greenwood, 2016; Han & Pollock, 2021; Wang & Jensen, 2019) and reinforce pertinent values and norms that dictate status-conferral rules within the hierarchy (Han & Pollock, 2021; Washington & Zajac, 2005). Thus, status in different categories often conveys different meanings to audiences (Jensen et al., 2011), and high-status actors in each category are regarded as exemplars of the category’s values and norms (Pollock et al., 2019; Rao et al., 2005).

As category prototypes, high-status firms’ misconduct becomes associated with their industry’s core features, regardless of whether the features are the causes of the misconduct (Jonsson et al., 2009). Thus, the media is likely to perceive that high-status insiders are the industry’s prototypical members and assume they share similar values with the current high-status transgressor. These presumed similarities allow journalists to tap into richer materials based on high-status past transgressors’ actions to build dramatic narratives about the current transgressor, using broader industry traits to capture readers’ attention (Shoemaker & Reese, 2013; Shudson, 1986), because the media favors actively shaping the narrative over only reporting on what happened (Bednar et al., 2013; Hersel, 2022; König et al., 2018), and the transgressors’ shared attributes can serve as situational evidence of the current transgressor’s culpability (Dewan & Jensen, 2020; Lange & Washburn, 2012). Further, because repeated transgressions suggest an industry-based pattern and elevate public scrutiny (Desai, 2011; Han et al., 2023; Naumovska, et al., 2021), publishing another story that extends the pattern is much less costly for journalists, because they can build on their audiences’ experiences and schemas (König et al., 2018).

The prevalence of high-status insiders’ misconduct therefore connotes a potential problem endemic to the industry that is worthy of public concern (Dewan & Jensen, 2020; Jonsson et al., 2009; Naumovska, et al., 2021; Roehm & Tybout, 2006), rather than decreasing interest due to familiarity and repetition (Chandler et al., 2020; Zavyalova et al., 2012), increasing the newsworthiness of the current high-status firm’s transgression, and further increasing the likelihood it will be scandalized. Thus, we hypothesize:

Hypothesis 2. The prevalence of high-status insiders’ misconduct strengthens the positive relationship between the focal firm’s high status and the likelihood of its misconduct being scandalized.

High-status outsiders’ transgressions. In contrast, the prevalence of high-status outsiders’ misconduct is likely to diminish the newsworthiness of the current high-status firm’s misconduct. Industry categories each have a status hierarchy (Jensen & Wang, 2018), but observers may not treat the highest-status firms in each hierarchy equivalently (Han & Pollock, 2021). Industry differences between past and current high-status transgressors therefore do not offer opportunities to build the same, rich, industry-based narrative about the current transgressor, and create

starker contrasts between them, even though each is high-status in their respective industry (Förster, Liberman, & Kuschel, 2008). That is, high-status actors' distinct category representativeness (Han & Pollock, 2021) can amplify audiences' tendencies to disregard category combinations of which they cannot make sense (Paolella & Durand, 2016).

Multiple transgressions by high-status outsiders make it harder to ascribe responsibility for the focal high-status transgressor's misconduct to the industry-based characteristics they represent. Observers' tendencies to perceive patterns in simple and predictable cues (Rindova et al., 2010), combined with journalists' limited attentional resources (Pollock et al., 2008; Shoemaker & Reese, 2013; Titus, Parker, & Bass, 2018), their assignment to particular beats, their inability to have contacts and domain expertise in all industries, and their ability and need to build on prior reporting in routinely producing news (Hirsch, 1977; Shoemaker & Reese, 2013) all create pressures to come up with a feasible and easily explained narrative.

Multiple transgressions by high-status firms in a different industry will more likely lead to an industry-based explanation for the transgressions—whether industry characteristics play a material role in the transgressions or not (Jonsson et al., 2009; Paruchuri & Misangyi, 2015)—which they cannot use to explain the focal actor's transgression. This elevates the cost of publicizing the incident, because it limits journalists' abilities to rely on their audiences' schema and prior narratives to ensure persuasiveness (König et al., 2018). Faced with a deviation from its industry-based narrative (Bednar et al., 2013), the media is less likely to pay attention to the focal high-status firm's transgression (Hoffman & Ocasio, 2001), rather than come up with a different explanation. Further, if the transgressions are spread across a variety of industries, rather than clustering in a particular industry, then the prevalence of high-status outsiders' misconduct may dilute the emergence of any patterns, making it harder for journalists to develop an overarching narrative. Either way, the focal high-status firm's misconduct is less likely to be singled out for attention, creating safety in the overall number of transgressors and reducing the likelihood its misconduct will be publicized and turned into a scandal. Thus, we hypothesize:

Hypothesis 3. The prevalence of high-status outsiders' misconduct weakens the positive relationship between the focal firm's high status and the likelihood its misconduct is scandalized.

METHODS

Data and Sample

We tested our hypotheses using corporate data breaches disclosed between 2015 and 2018. Several notable scandals occurred during this period—including the 2016 Yahoo! breach, the 2017 Equifax breach, the 2018 Facebook–Cambridge Analytica scandal, and the 2018 Marriot breach—leading the public to view data breaches as a serious social problem (Accenture 2019), and executives to view them as a top concern (Petersen 2020). Despite many of the breaches occurring through hacker attacks (60% in our sample), governments tend to hold the breached firms accountable, leading to the European Union's enactment of the General Data Protection Regulation, followed by legislative efforts in the United States (Accenture, 2019). More recently, Uber's former chief information security officer was prosecuted for the company's past data breach (Hill & Browning, 2022), indicating that data breaches qualify as misconduct even by the narrowest definition based on legal terms (Greve, Palmer, & Pozner, 2010). This is not surprising, considering that people share their private data “in exchange for some economic or social benefit subject to the ‘privacy calculus,’ an assessment that their personal information will subsequently be used fairly and they will not suffer negative consequences” (Culnan & Armstrong, 1999: 106). Exposing customers' data thus damages their trust in the breached firms (Martin et al., 2017), as the breached firms bear the ultimate responsibility for failing to protect their data—regardless of the cause—and raising questions about why customers should still entrust the firms with their private information.

We identified data breaches using the database provided by the Privacy Rights Clearinghouse, which compiles data from various sources, including government bureaus and other consumer protection organizations (Gwebu et al., 2018; Martin et al., 2017).¹ The database provides an extensive list of breaches involving government institutions, the military, schools, hospitals, other nongovernmental or nonprofit organizations, and companies. We identified 224 breaches involving 157 publicly traded U.S. companies after excluding all the breaches involving

¹ There were some incidents, though only a few, where the database stated media articles as the source. In those cases we tracked the initial disclosure date mentioned in the articles or tracked the original source mentioned in the articles to record the most accurate breach disclosure date.

nonbusiness entities or private firms. We confirmed our data breach list, and data on several breach-related characteristics used to operationalize our control variables, using annual reports published by the Identity Theft Resource Center.

Dependent Variable

Breach media coverage. We collected articles about the data breach incidents published during the two weeks following the incidents' disclosures from all English-language outlets registered with Factiva (Chandler et al., 2020; Graffin et al., 2013). Although two weeks may seem short, particularly given that scandals can last and evolve over months and even years (Dewan & Jensen, 2020; Graffin et al., 2013), misconduct instances' fates are usually determined during the early phase of disclosure. This was the case with data breaches; even within the two weeks, some breaches were featured in hundreds of articles. If the media is going to cover a disclosure, it is highly unlikely that it will wait several weeks before doing so.

We manually identified articles about each breach incident (Bednar, 2012; Chandler et al., 2020), and excluded general articles about the firm that did not mention the breach (to avoid capturing the media attention high-status firms normally expect), resulting in 6,814 articles.² This article count, however, was quite skewed, with 62.5% of the breaches having no coverage and the remaining breaches' coverage ranging from one to 3,073 articles. While analyses using the raw count variable, as well as log-transformed and winsorized counts, displayed similar results to those reported below—in some cases with even stronger statistical significance in support of our hypotheses—because of the skewed data we concluded that we could not confidently interpret the results using the continuous specifications.

Misconduct publicity's extreme skew mirrors a typical characteristic of social evaluations, where the attention and rewards (or, in our case, punishments)

disproportionately accrue to a select few actors (Hoffman & Ocasio, 2001; Sauder et al., 2012). In a sense, scandal is uniquely conferred on certain misconduct incidents, and substantial differences exist between the neglected incidents and publicized incidents, and also between the scandalized incidents and the rest, rather than among incidents with similar levels of publicity. Treating a categorical construct as continuous is thus both theoretically and methodologically problematic, as it adds noise to the measure (Lovelace, Bundy, Pollock, & Hambrick, 2022; Pollock et al., 2019). Indeed, research measuring social evaluations has often employed such categorical coding (e.g., Ertug & Castellucci, 2013; Halebian, Pfarrer, & Kiley, 2017; Lovelace et al., 2022). Hence, we believe our operationalization of misconduct's publicity reasonably captures our outcome of interest while avoiding the problems inherent in the skewed raw data.

As such, we created an ordered variable coded 0 if a breach did not receive any media coverage, 1 if it was covered but the volume of coverage was below the 95th percentile in our sample (40 articles), and 2 if the volume of media coverage was above or equal to the 95th percentile. Thus, we captured three different grades of misconduct publicity: no media coverage (140 breaches), moderate media coverage (71 breaches), and scandalized (13 breaches). Our results are robust to the continuous measure of media coverage, and we discuss these results and alternative scandal category cutoffs in our robustness tests.

Independent Variables

Firm status. We operationalized firm status and past transgressors' status using the measure developed by Dewan and Jensen (2020) and Wang and Jensen (2019), which is, to our knowledge, the only available firm status measure applicable in contexts involving multiple industries. Certification by an authoritative third party is a key status-conferral mechanism (Sauder et al., 2012), and in the business world coverage by analysts with the greatest domain expertise has this certifying effect (Bowers & Prato, 2018). Analysts decide what firms they will cover, and they have incentives to cover more firms (Groysberg, Healy, & Maber, 2011), firms their clients are interested in and that are performing well (McNichols & O'Brien, 1997), and firms that high-status analysts are covering (Rao, Greve, & Davis, 2001).

Our chosen status measure (Dewan & Jensen, 2020; Wang & Jensen, 2019) weights the volume of analyst coverage a firm has received by the analyst's expertise

² We used the search terms "data breach," "hack*," "cyber security (cybersecurity)," "cyber attack (cyberattack)," "privacy issue/concern," "security concern," and "phishing." We used the company search function in Factiva to ensure that the articles were about the breached firms. Then, we manually identified only the articles that were about the focal data breach incident. For instance, general commentaries on firms' cybersecurity measures that would still appear using the search terms above were dropped if they did not mention the focal breach incident.

in the firm's industry, which is conceptually equivalent to eigenvector centrality—the most commonly used measure of status in single-industry settings (Pollock et al., 2019)—by approximating “the centrality of the focal firm in the security analyst coverage network” (Dewan & Jensen, 2020: 1660). Thus, this is “not simply a measure of the popularity” (Wang & Jensen, 2019: 865) and avoids the most common problem of using raw coverage volume (Pollock et al., 2019).

Following Dewan and Jensen (2020) and Wang and Jensen (2019), we counted the number of firms each analyst covered in a 3-digit SIC industry during the previous year. Then, we assigned a value of 1 to the analyst covering the largest number of firms in each industry and used the number of firms they covered to normalize the number of firms all other analysts covered in a given industry, creating analyst expertise scores that ranged from 0 to 1. We then aggregated the analysts' scores by firm-year, and used this aggregated score as our firm status measure.³ We coded the status of firms with no analyst coverage 0.

High-status insider and outsider breaches. To measure *high-status insider breaches*, we first dichotomized the status scores of all firms—within the broader population—with at least one analyst covering them at the 90th percentile to identify high-status firms. Then, we counted the number of data breaches involving high-status firms within the focal firm's 3-digit SIC industry during the 365 days preceding the focal firm's breach disclosure.⁴ *High-status outsider breaches* was operationalized as the number of data breaches involving high-status firms outside the focal firm's industry during the same period. To reflect the media's tendency to base their coverage decisions on their or others' prior coverage (Pollock et al., 2008), and to avoid including breaches the media failed to notice, we used only past breaches that were covered in at least one article. In addition,

to account for diminishing salience over time (Paruchuri et al., 2021), we discounted the past incidents, dividing each incident by the number of weeks from the focal incident. That is, if a data breach involving a high-status insider was disclosed 25 weeks before the focal breach, it was coded as 0.04 before being aggregated with other high-status insiders' breaches to construct our count variable.⁵

Control Variables

We controlled for firm characteristics that are known to influence firms' visibility with audiences, and the likelihood the media covers their data breaches. Because larger and better-performing firms are more likely to receive media coverage, we included *industry-adjusted firm size*, measured as the log of total assets, and *industry-adjusted return on assets* (ROA) (Zavyalova et al., 2012). We subtracted the industry median from both variables to account for our multi-industry setting (Dewan & Jensen, 2020). Because high-status firms are more likely to receive media coverage (Graffin et al., 2013), publicizing their data breaches could reflect the publicity they would have received even without the breaches (Chandler et al., 2020). Although this concern is attenuated by focusing only on articles about breach incidents (Bednar, 2012), rather than those about the firm in general (Zavyalova et al., 2012), we controlled for *firm media visibility* using the volume of media coverage a firm received during the two weeks before the breaches, with a one-week gap (Chandler et al., 2020; Dewan & Jensen, 2020).⁶ As this variable was highly skewed, we transformed it into its natural logarithm, adding a 1 to all values before taking the log, since some firms had no coverage. We also controlled for *firm breach frequency*, measured as the focal firm's number of prior data breaches during the previous 365 days, to account for the potential salience imparted by repeated transgressions (Han et al., 2023).

³ Examples of high-status firms (i.e., firms that belonged to yearly top-50s within the larger population of firms) in our data include Chipotle, Disney, Facebook, FedEx, Google, Time Warner, and Whole Foods. High-status firms not in our data—due to not having been breached during the observation period—include Citi, Costco, Home Depot, Intel, Lockheed Martin, Netflix, Nike, Nvidia, Salesforce, and Visa.

⁴ If a focal firm was breached multiple times during our observation period, we excluded the firm's past breaches in this measure as well as in the measure of *non-high-status insider breaches*, a control variable discussed below.

⁵ We examined the robustness of our results by adopting alternative cutoffs identifying yearly high-status firms; utilizing continuous operationalizations (i.e., sum, average, and maximum status scores of insiders and outsiders); using all past breaches, as opposed to using only previously publicized breaches; and discounting past transgressors' status using the number of months and days. As we discuss below, our findings are robust to these alterations.

⁶ We tried controlling for longer-term media visibility by counting the volume of media coverage a firm received during the previous year, which was correlated at 0.82 with our current variable. Our results remained the same.

The second set of control variables relates to data breach characteristics. First, we used the number of accounts affected by a breach incident to measure *breach severity*, which significantly aggravates reactions to data breaches (Gwebu et al., 2018, Martin et al., 2017). We log-transformed the variable to smooth its distribution. Second, we included a binary variable, *sensitive information breached*, coded 1 if a breach exposed personal identification information, such as Social Security or driver's license numbers; or financial information, such as bank account or credit card numbers (Gwebu et al., 2018). Finally, we controlled for whether a breach occurred through a *hacker attack*, because hacker attacks accounted for more than half of our sample and the media may consider them more newsworthy. We coded this measure 1 if the breach resulted from a hacker attack, and 0 otherwise.

The last set of controls captures situational factors that may affect journalists' evaluative processes. Because the temporal proximity of other data breaches can distract the media's attention (Titus et al., 2018), we controlled for the *number of concurrent breaches* during the surrounding days (i.e., from $t - 1$ to $t + 1$). Similarly, the prevalence of data breaches may affect the salience of the current incident to the media (Han et al., 2023; Zavyalova et al., 2012). Thus, we included *non-high-status insider* and *outsider breaches*, operationalized as the number of publicized data breaches involving non-high-status firms that occurred during the prior 365 days inside and outside the focal firm's industry. Additionally, the data breach's salience as a news topic may vary over time (Desai, 2011). We controlled for this issue using two variables: *Issue salience* and *media scrutiny*. Issue salience was operationalized using the volume of internet searches on the topic "data breach" during the previous two weeks. We obtained this measure from the Google Trends database, which indexes search terms based on topics and stores periodic data on the search volume (Choi & Varian, 2012). Media scrutiny was operationalized as the proportion of publicized data breaches among all disclosed breaches during the previous 365 days, which captures the media's interest in the topic.

Because our sample is not big enough to accommodate using SIC classifications as industry controls, we controlled for industry effects using the Fama–French 12 industry classification.⁷ Only 10 of

the 12 industries were represented in our sample, so we included nine industry dummies, and excluded the "others" category, which includes industries such as mining, construction, building management, and transportation.⁸ Finally, we included year dummies with 2015 as the omitted year.

Analytic Strategy

Given our ordered, categorical dependent variable capturing three levels of misconduct publicity (i.e., no media coverage, moderate media coverage, and scandalized), we used ordered probit regressions to test our hypotheses, with standard errors clustered by firm to account for firms that had multiple data breaches during the observation period (Haleblian et al., 2017; Lovelace et al., 2022). Because our models involve nonlinear specifications with multiple ordinal outcomes, only interpreting the coefficients and their significance can be misleading (Busenbark, Graffin, Campbell, & Lee, 2022; Mize, 2019). Thus, in addition to the sign and significance of the coefficients, we provide interaction plots based on the marginal effects computed at the means and modes of our control variables, and the significance of firm status's average marginal effect across the full range of the moderators (i.e., high-status insider and outsider breaches) (Busenbark, Graffin, et al., 2022; Han & Pollock, 2021), and for each category of our dependent variable (Breen, Karlson, & Holm, 2018; Mize, 2019).

RESULTS

Table 1 presents the descriptive statistics and correlation coefficients. While most of the variables were only moderately correlated with each other, *firm media visibility* was correlated with *industry-adjusted firm size* at 0.58 and with *firm status* at 0.57. Thus, following prior research (e.g., Dewan & Jensen, 2020), we regressed firm media visibility on firm size and status and used the residuals from this regression as the instrument for firm media visibility in our analyses. This alleviated concerns about potential multicollinearity. Computed based on the full model, the mean VIF was 2.60, the maximum VIF was 7.77 for *firm status* \times *high-status outsider breaches*, and the condition number was 24.81, all of which are below the thresholds of 10 for VIFs and

⁷ When we included the 2-digit SIC industry dummies, our results remained virtually the same, but Stata could not compute model-fit statistics for these models.

⁸ The energy (oil, gas, and coal extraction and products) and utilities industries were not represented.

TABLE 1
Descriptive Statistics and Correlations

Variables	Mean	SD	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1. Misconduct publicity	0.43	0.60															
2. Firm status	11.65	7.67	.25														
3. High-status insider breaches	0.08	0.17	.25	-.03													
4. High-status outsider breaches	1.11	0.59	-.13	-.05	-.24												
5. Non-high-status insider breaches	0.06	0.20	.21	-.01	.39	-.13											
6. Non-high-status outsider breaches	0.66	0.50	-.02	.03	-.09	.13	-.11										
7. Firm size (industry-adjusted)	1.29	2.15	.16	.27	.30	-.19	.27	.00									
8. Firm ROA (industry-adjusted)	0.18	0.29	.14	.23	-.06	.03	.08	-.01	.08								
9. Firm media visibility	3.71	1.60	.30	.57	.18	-.11	.15	.09	.58	.18							
10. Firm breach frequency	0.02	0.08	.06	.16	.06	-.09	-.02	.21	.13	-.01	.23						
11. Hacker attack	0.57	0.50	.37	.05	.11	.07	.10	-.05	-.06	.05	-.01	-.16					
12. Breach severity	0.48	1.32	.41	.02	.39	-.01	.05	-.17	.13	-.14	.20	.09	.17				
13. Sensitive information breached	0.55	0.50	-.05	-.20	-.21	.04	-.09	-.06	-.31	.08	-.32	-.15	.17	-.26			
14. Number of concurrent breaches	0.59	0.92	.08	.00	.07	.29	.09	.16	-.06	-.03	-.03	-.01	.04	.08	-.01		
15. Media scrutiny	0.38	0.06	-.04	-.05	-.15	-.03	-.02	.43	.01	.03	-.03	.10	-.03	-.18	-.10	-.16	
16. Issue salience	1.18	0.61	.13	-.03	-.03	.27	-.08	.19	-.12	.14	-.02	.01	.07	.17	.05	.06	-.14

Note: Descriptive statistics and correlation coefficients of year and industry dummies have been omitted to conserve space.

30 for condition numbers (Belsley, Kuh, & Welsch, 2005).

We present our results in Table 2. Model 1 includes the control variables, Model 2 adds the main effect of firm status, Model 3 includes the interaction between firm status and high-status insider breaches, Model 4 adds the interaction between firm status and high-status outsider breaches, and Model 5 includes both interactions.

Hypothesis 1 posited that firm status would positively influence scandalization. The coefficients for firm status are positive and significant at $p \leq .001$ in all models, supporting Hypothesis 1. However, as we argue that this baseline relationship will be moderated by its context (i.e., high-status insider and outsider breaches), interpreting the main effect when there are significant interactions can result in incorrect conclusions, as it implies a mis-specified model (Aguinis, Edwards, & Bradley, 2017; Busenbark, Graffin, et al., 2022; Edwards, 2009). That is, interpreting each component of an interaction term in isolation when the interaction term is present only shows the influence of the main effect of each component at one point—when the other component equals 0 (Busenbark, Graffin, et al., 2022; Edwards, 2009). For instance, *firm status's* coefficient and p -value in Model 5 in Table 2 show its effect on scandalization when *high-status insider* and *outsider breaches* both equal 0 (i.e., there are no prior breaches by high-status insiders and outsiders). Thus, we can truly assess the influence of firm status only when considering differences in the contextual factors we expect to moderate this relationship.

Model 3 in Table 2 tests Hypothesis 2, that the prevalence of high-status insiders' transgressions amplifies the scandalizing effect of the focal firm's high status. The interaction term's coefficient is positive and statistically significant ($p = .000$), and remains so in Model 5 ($p = .004$). We computed the marginal effects based on the fully saturated model to further examine the results. At low levels of *high-status insider breaches* (10th percentile), on average a 1 standard deviation (SD) increase in *firm status* decreases the probability of receiving no media coverage by -9.2% ($p = .000$), and increases the probability of receiving moderate media coverage and being scandalized by 6.8% ($p = .001$) and 2.5% ($p = .003$), respectively. These effects grow to -16.8% , 10% , and 6.9% when the number of *high-status insider breaches* is high (90th percentile) ($p = .000$ for all effects).

The plots in the upper row of Figure 1 show the interaction plots based on the marginal effects

TABLE 2
Ordered Probit Regression Predicting Scandalization

Variables	Model 1	Model 2	Model 3	Model 4	Model 5
Firm size (industry-adjusted)	−0.012 (0.057)	−0.119* (0.055)	−0.141** (0.054)	−0.131* (0.058)	−0.148* (0.058)
Firm ROA (industry-adjusted)	0.913** (0.309)	0.726* (0.313)	0.702* (0.299)	0.756* (0.320)	0.722* (0.308)
Firm media visibility	0.148 [†] (0.090)	0.249* (0.102)	0.252* (0.100)	0.231* (0.095)	0.236* (0.096)
Firm breach frequency	1.325 (1.053)	0.655 (1.002)	0.090 (1.017)	0.471 (1.005)	0.030 (1.005)
Hacker attack	1.160*** (0.236)	1.142*** (0.257)	1.147*** (0.255)	1.185*** (0.257)	1.184*** (0.255)
Breach severity	0.328** (0.102)	0.366*** (0.099)	0.432*** (0.110)	0.382*** (0.103)	0.434*** (0.113)
Sensitive information breached	0.032 (0.222)	0.135 (0.231)	0.170 (0.229)	0.174 (0.229)	0.197 (0.226)
Number of concurrent breaches	0.212 [†] (0.113)	0.219 [†] (0.115)	0.178 (0.121)	0.197 [†] (0.116)	0.168 (0.122)
Media scrutiny	−5.834 [†] (3.542)	−5.911 (3.834)	−5.664 (3.838)	−5.926 (3.731)	−5.693 (3.741)
Issue salience	0.150 (0.184)	0.120 (0.188)	0.124 (0.192)	0.182 (0.189)	0.177 (0.191)
Non-high-status insider breaches	0.427 (0.480)	0.584 (0.448)	0.560 (0.415)	0.851 [†] (0.461)	0.792 [†] (0.429)
Non-high-status outsider breaches	0.119 (0.197)	0.108 (0.210)	0.258 (0.219)	0.194 (0.222)	0.304 (0.229)
High-status insider breaches	0.048 (0.669)	0.844 (0.642)	−1.003 (0.865)	1.104 [†] (0.645)	−0.466 (0.866)
High-status outsider breaches	−0.331 (0.221)	−0.196 (0.232)	−0.233 (0.230)	0.692* (0.342)	0.502 (0.338)
Firm status		0.073*** (0.015)	0.051** (0.016)	0.153*** (0.026)	0.122*** (0.028)
Firm status × High-status insider breaches			0.196*** (0.056)		0.161** (0.056)
Firm status × High-status outsider breaches				−0.074*** (0.021)	−0.061** (0.022)
Year and industry dummies			Included.		
Log pseudolikelihood	−125.57	−116.14	−112.06	−111.70	−109.15

Note: $n = 224$.

[†] $p < .10$

* $p < .05$

** $p < .01$

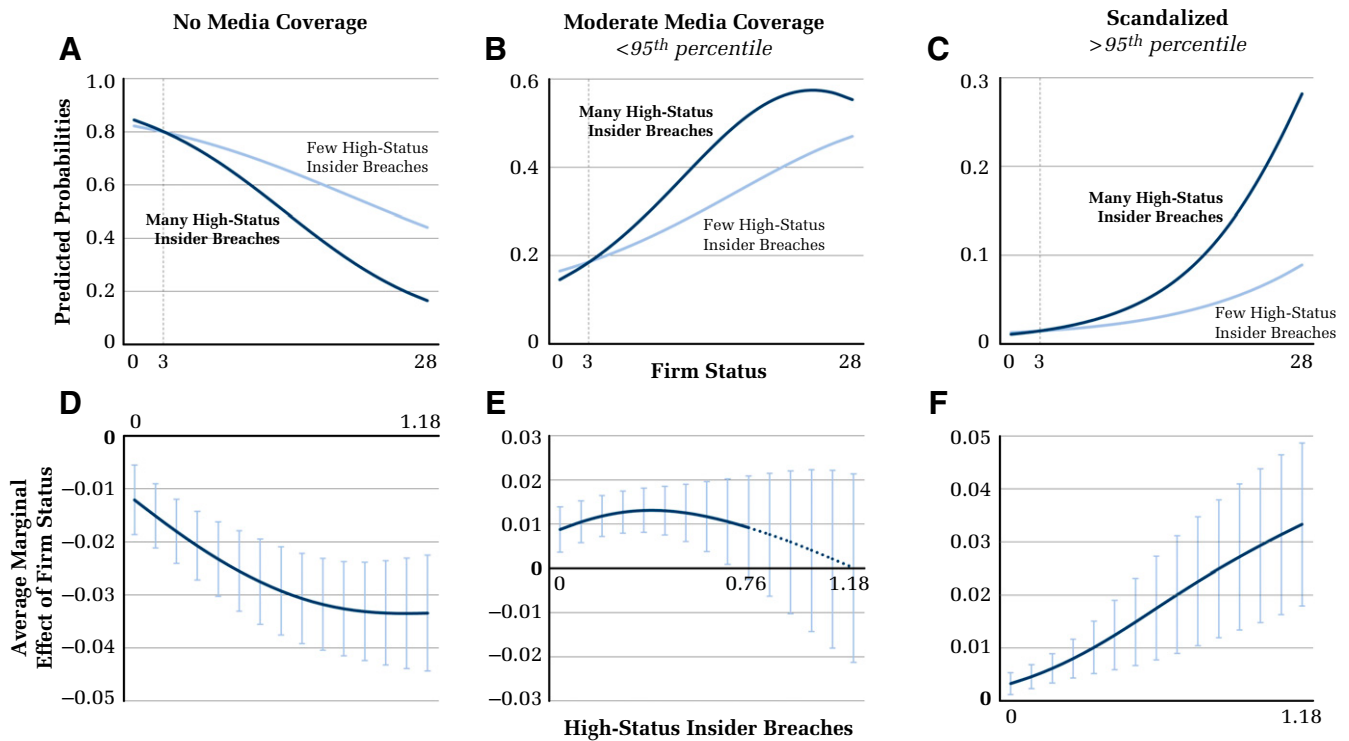
*** $p < .001$

computed while holding the control variables at their means and modes (Han & Pollock, 2021). Plot (a) shows that *firm status* decreases the probability of receiving no media coverage more when *high-status insider breaches* is high (90th percentile) than when it is low (10th percentile), and Plots (b) and (c) show that *firm status*'s positive relationship with the probabilities of receiving moderate media coverage and being scandalized is greater when *high-status insider breaches* is high.

Busenbark, Graffin, Campbell, and Lee (2022: 162) noted that graphing the average marginal effects of the

moderated variable “allows researchers to make more comprehensive inferences about hypothesized relationships” by assessing the range of the moderator variable within which the moderated variable's effect is significant—that is, when 0 is not included in the confidence interval—making it useful for identifying boundary conditions. Accordingly, the lower row in Figure 1 presents the average marginal effects of *firm status* across the full range of *high-status insider breaches*. All three plots confirm the results of the previous analyses. In Plot (d), the average marginal effects of *firm status* on

FIGURE 1
Interaction Between Firm Status and High-Status Insider Breaches



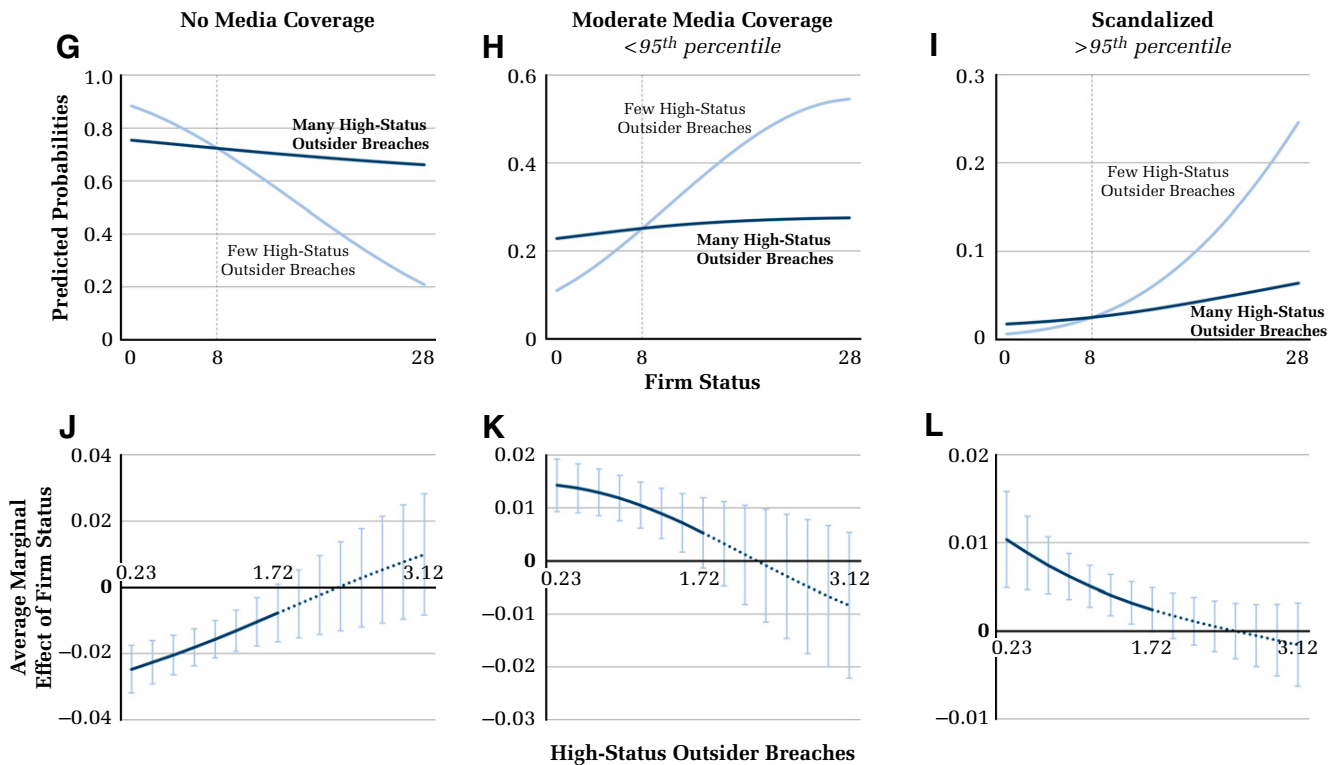
the probability of receiving no media coverage decrease with increases in *high-status insider breaches* and remain significant throughout the entire range. In Plot (f), the effects on the probability of being scandalized increase and remain significant throughout the entire range of *high-status insider breaches*. Although the marginal effect of firm status on the probability of receiving moderate media coverage decreases at the highest level of *high-status insider breaches* in Plot (e), the decreasing effects are not significant (i.e., confidence intervals include 0), in line with what we expected. Thus, Hypothesis 2 is supported.

Model 4 in Table 2 tests Hypothesis 3, which suggested that the prevalence of high-status outsiders' transgressions weakens the scandalizing effect of a focal firm's high status. The interaction between *firm status* and *high-status outsider breaches* is negative and statistically significant ($p = .000$; $p = .005$ in Model 5). Our average marginal effects analysis indicates that when *high-status outsider breaches* is low (10th percentile), a 1 *SD* increase in *firm status* decreases the probability of receiving no media coverage by -16.7% , and increases the probabilities

of receiving moderate media coverage and being scandalized by 10.4% and 6.3% , respectively ($p = .000$ for all effects). These effects become substantially attenuated and statistically nonsignificant, at -2.8% ($p = .513$), 1.8% ($p = .605$), and 1.1% ($p = .297$) when *high-status outsider breaches* is high (90th percentile).

Plots (g), (h), and (i) in the upper row of Figure 2 all show that high *high-status outsider breaches* (90th percentile) flattens out the effects of *firm status* in all three outcomes compared to the effects when *high-status outsider breaches* is low (10th percentile). The results become more interesting when we graph the average marginal effects. Beyond just weakening the effect of *firm status*, the effect becomes nonsignificant at the highest level of *high-status outsider breaches*, suggesting that the prevalence of high-status outsiders' transgressions is a more important boundary condition than we expected. In Plot (j), *firm status* increases, rather than decreases, the probability of receiving no media coverage, and has no effect when *high-status outsider breaches* reaches around $+1$ *SD*. Similarly, the effects of *firm status* on the probabilities of being moderately publicized in

FIGURE 2
Interaction Between Firm Status and High-Status Outsider Breaches



Plot (k) and scandalized in Plot (l) decrease with increasing *high-status outsider breaches* and become nonsignificant at about +1 *SD*. Thus, Hypothesis 3 is supported. We elaborate on the implications of our findings in the Discussion section.

Robustness Tests

Scandal measure validation. Our operationalization of scandal as misconduct publicity is informed by prior studies' definition of scandal as publicly recognizing a transgression that violates established norms (Adut, 2005; Esser & Hartung, 2004; Nyhan, 2014; Piazza & Jourdan, 2018; Thompson, 2000). Thus, we measured misconduct publicity using the volume of media coverage, with scandals defined as data breaches that received a disproportionate amount of media attention. However, given the lack of research measuring and predicting scandals, we conducted a series of analyses to validate our measure. First, to examine whether the media treated data breaches classified as scandals differently from those that were only moderately publicized, we conducted a *t*-test comparing the tenor of articles

covering the breaches in the “scandalized” and “moderately publicized” categories of our dependent variable. Using the positive and negative emotion word dictionaries for the program Linguistic Inquiry Word Count (LIWC), we counted all the positive and negative emotion words in the firms' media coverage, and calculated the proportion of total emotion words that were negative (i.e., we divided negative emotion words by the sum of positive and negative emotion words) for each article and averaged the ratios at the breach level (Hubbard et al., 2018; Lovelace et al., 2022). The results suggested that journalists used significantly more negative language when covering scandalized data breaches than when covering moderately publicized breaches ($p = .002$).

Second, among the very few who measured and predicted scandals was Nyhan (2014, 2017), who focused on the media's “labeling” of misconduct as scandals. Nyhan counted the use of the word “scandal” in articles in addition to calculating the volume of media coverage. Hence, we created a custom dictionary of scandal-related words, which we imported into LIWC and used to content-analyze the

articles covering our sampled breaches.⁹ *T*-tests again revealed that media coverage of the “scandalized” breaches employed significantly more scandal-related words than media coverage of the “moderately publicized” breaches ($p = .010$). When we reran our analyses after redefining the scandalized category to exclude breaches that were publicized without using the scandal-related words, the results remained virtually the same (see Table S-1 in the Additional Materials for detailed results).¹⁰ Hence, we believe our scandal measure captures scandals reasonably well.¹¹

Alternative media coverage cutoffs. We ran several analyses to test our findings’ robustness to alternative media coverage cutoffs. First, we used lower (90th percentile: 22 articles) and higher (99th percentile: 423 articles) cutoff points to define the “scandalized” category. The results stayed virtually the same as the original cutoffs. Second, we ran probit regressions using a dichotomized dependent variable coded 1 if a breach was scandalized (i.e., belonged to the scandalized category in our original dependent variable) and 0 otherwise. Again, our original results held (see Table S-2).

We also ran a series of negative binomial regressions using the raw count of data breach articles, and its log-transformed and winsorized versions (at the 99th percentile). All three continuous specifications yielded virtually the same patterns of results as the original analysis, with some coefficients showing

even stronger statistical significance (see Table S-3). However, none of the variable transformations seemed sufficient to prevent extreme values from driving our results. Further, as we explained in the Methods section, we consider scandal a unique status conferred on incidents that achieve publicity beyond a certain threshold, where the attention and punitive reactions accrue disproportionately to the select few incidents (Graffin et al., 2013; Hoffman & Ocasio, 2001); treating such categorical constructs as continuous is problematic (Pollock et al., 2019). Nonetheless, our results are robust to the continuous specifications.

Alternative specifications of past transgressor status. We tried several different ways to operationalize high-status insider and outsider breaches. First, we applied different cutoff points—at the 95th percentile, and 1 and 2 *SD* above the mean—to determine high-status insiders and outsiders before counting their breaches. The results were almost identical to our original results (see Table S-4). Second, we tried continuous operationalizations by taking the maximum, average, and sum of the insiders’ and outsiders’ status scores. Our original results all held except for the slightly reduced statistical significance of *firm status* \times *outsider status* ($p = .094$) when using the average (see Table S-5). Third, we applied monthly and daily discounts, instead of the original weekly discount, to insiders’ and outsiders’ breaches before counting them. Again, the results confirmed our original findings (see Table S-6).

Finally, our original analyses only counted the publicized cases when measuring the variables concerning past transgressors (i.e., *high-status and non-high-status insider and outsider breaches*) to avoid including prior breaches of which journalists were unaware. However, including the unpublicized cases did not change our results. We also counted only the past breaches that received above-median media coverage because journalists may not recall the breaches that were barely covered. This also did not affect our results (see Table S-7).

Effects of non-high-status insider and outsider breaches. Although our theory highlights the importance of the past transgressions’ salience, reflected in the past transgressors’ status and categorical proximity to the focal transgressor, our primary analysis only tested the moderation effects of high-status insider and outsider breaches. A statistically significant interaction between non-high-status insiders’ and outsiders’ breaches with the focal firm’s status could potentially suggest limits on our theory—particularly if in the same directions as *high-status insider* and *outsider breaches*—or suggest an alternative dynamic

⁹ We used Thesaurus.com to identify synonyms for scandal, and also downloaded a sample of articles—comprised of the top 10 Google news articles on data breaches by different firms—and combined all text from these articles into one corpus. We then content analyzed this corpus for its most frequently used words and identified “scandal-related” words. We combined these words with the synonyms, and imported the dictionary into LIWC. We include the dictionary terms in Table S-9 of the Additional Materials. Using only the word “scandal,” as Nyhan (2014, 2017) did, does not affect our results. Consistent with Nyhan’s findings, we found that journalists were conservative in using this term: Only five out of 84 publicized breaches were covered using the word “scandal,” all of which belonged to the “scandalized” category.

¹⁰ The Additional Materials are available at: <https://doi.org/10.6084/m9.figshare.22728710.v1>

¹¹ In an additional analysis, we also created an alternative measure where we standardized article count, tenor, and scandal-related language; added the minimum values for each component so that all values were positive; and multiplied the three components. Our results are substantively unchanged when we employ this measure.

we did not theorize about. We therefore explored the interaction between *firm status* and *non-high-status insider* and *outsider breaches*. Each variable's interaction with *firm status* was not statistically significant, nor did it influence the significance of *high-status insider* and *outsider breaches*' interaction terms (see Table S-8). Thus, we believe our analyses adequately capture our theoretical framework, while also highlighting the importance of industry membership, as well as status class membership, when assessing insider data breaches.

Endogeneity concerns. We ensured that our dependent variable accurately captured misconduct's publicity by tracking the most accurate disclosure dates of the data breaches and strictly limiting our dependent variable to articles about the breaches. Moreover, controlling for the volume of media attention the breached firms received right before the breach disclosure ensures that we captured the media's response to the breaches, and the effects of firms' status on their response. While we believe these efforts substantially decreased endogeneity concerns, we further conducted a robustness of inference to replacement (RIR) analysis—part of the impact threshold of a confounding variable (ITCV) analysis family (Frank, 2000; Hubbard, Christensen, & Graffin, 2017). Management scholars have increasingly employed these analyses to assess potential endogeneity issues (for a review, see Busenbark, Yoon, Gamache, & Withers, 2022). We conducted our analysis based on Model 2 in Table 2, using the *konfound* command in Stata with the nonlinear option specified (Busenbark, Yoon, et al., 2022; Frank et al., 2021; Xu & Frank, 2021; Xu, Frank, Maroulis, & Rosenberg, 2019).¹² The RIR analysis predicts “how much of a given effect size must be biased in order to overturn an otherwise statistically significant parameter estimate” (Busenbark, Yoon, et al., 2022: 44). To invalidate our current inferences for *firm status*, 64.27% of the estimate (144 cases) would have to be biased. Similarly, 32.55% (73 cases) of *high-status insider breaches* and 57.14% (128 cases) of

high-status outsider breaches would have to be due to bias. Thus, it is highly unlikely that endogeneity is an issue in our study.

Post Hoc Analysis: Main Effects of High-Status Insider and Outsider Breaches

Our theory assumes that past transgressions by high-status insiders and outsiders are salient to journalists, shaping their perceptions of the focal high-status firm's transgression. If so, non-high-status firms may also be affected by high-status firms' past transgressions, because audiences tend to extend their evaluation of high-status actors to lower-status actors (Jonsson et al., 2009; Rao et al., 2005). Thus, we explored the main effects of high-status insider and outsider breaches with a *post hoc* analysis.

As noted above, *high-status insider* and *outsider breaches*' main effects should not be interpreted in isolation, because they are components of interaction terms that turned out to be statistically significant (Aguinis et al., 2017; Busenbark, Graffin, et al., 2022). Even though theory determines which variable is the main effect and which is the moderator when testing hypotheses, mathematically both components of the interaction are affected by the interaction term, and must be interpreted as such when assessing their main effects (Edwards, 2009).

As shown in Table 2, the coefficients of *high-status insider* and *outsider breaches* are largely nonsignificant, except for in Model 4 ($p = .087$ and $p = .043$, respectively). However, the main effect coefficients for either component in a multiplicative interaction only reveal their values when the other component equals 0. How the other component of the moderator is scaled therefore influences the practical and statistical significance of the main effect of the moderator in the interaction term. One way to get a better sense of the component's influence, then, is to center the zero-value cutoff (i.e., rescale the measure) at different points on the distribution for the variable being moderated.

Table 3 presents the results of the fully saturated models (equivalent to Model 5 in Table 2) with *firm status* rescaled by centering at different parts of its distribution beginning at the median and increasing by five percentile increments to the 95th percentile. When *firm status* is centered on its median value, *high-status insider breaches* is positive and significant at $p < .05$, and *high-status outsider breaches* is not significant. At the 60th percentile (i.e., *firm status* takes on a 0 value at the 60th percentile) the main

¹² The RIR analysis essentially serves the same purpose as the ITCV analysis—that is, assessing the potential impact of endogeneity in statistical findings—but is more appropriate for nonlinear models because “for a nonlinear model, the impact of an omitted variable necessary to invalidate an inference [i.e., ITCV] should not be used, because it is correlation based and thus applies only to linear cases. The percent bias to invalidate the inference [i.e., RIR] can still be applied in this case” (Xu et al., 2019: 533; see also Busenbark, Yoon, et al., 2022).

TABLE 3
Results Using Firm Status Centered at Various Levels

Variables	Original Results	Percentiles at which <i>Firm Status</i> is Centered									
		Median	55 th	60 th	65 th	70 th	75 th	80 th	85 th	90 th	95 th
High-status insider breaches	−0.466 (0.866)	1.552* (0.653)	1.737** (0.667)	1.962** (0.693)	2.104** (0.713)	2.199** (0.728)	2.445** (0.772)	2.589** (0.801)	2.697** (0.823)	2.963*** (0.884)	3.283*** (0.964)
High-status outsider breaches	0.502*** (0.338)	−0.267 (0.237)	−0.337 (0.242)	−0.423 [†] (0.251)	−0.477 [†] (0.258)	−0.513 [†] (0.264)	−0.607* (0.281)	−0.662* (0.292)	−0.703* (0.301)	−0.804* (0.324)	−0.926** (0.356)
Firm status	0.128*** (0.028)	<i>Stays the same across all models.</i>									
Firm status × High-status outsider breaches	0.161** (0.056)										
Firm status × High-status insider breaches	−0.061** (0.022)										
Log pseudolikelihood	−109.15	<i>Stays the same across all models.</i>									

Note: $n = 224$. Coefficients and standard errors of control variables are omitted. Their directions, magnitudes, and statistical significances are the same as those reported in Model 5 of Table 2 across all models.

[†] $p < .10$

* $p < .05$

** $p < .01$

*** $p < .001$

effect for *high-status insider breaches* is significant at $p < .01$ and *high-status outsider breaches* is significant at $p < .10$, and both continue to be significant at $p < .05$ or better as the *firm status* variable is centered at the 75th percentile and higher levels in the distribution. That is, the main effect of each of these variables is statistically significant for a firm with status at the median and above for *high-status insider breaches*, and at the 60th percentile and above for *high-status outsider breaches* in our sample.

We also conducted a margins analysis to further assess the range over which these main effects are significant. Table 4 presents the results of our margins analysis based on Model 5 in Table 2, and Figure 3 graphs the main effects. Consistent with our discussion above and the results in Table 3, the average marginal effect of *high-status insider breaches* is statistically significant above the median value of *firm status*. These results also suggest that *high-status outsider breaches*' effect is statistically significant above the 65th percentile of *firm status*. Thus, high-status insider and outsider breaches have direct effects when the focal transgressor is middle or high status, but not when it is low status.

DISCUSSION

Our study sheds new light on status's role in scandalization by focusing on the context in which the transgression occurs; specifically, the prevalence of

past high-status transgressors inside and outside the focal firm's industry. While we replicated the well-known direct effect of a firm's status on the scandalization of its transgressions, we also found that the more prevalent the high-status insiders' misconduct has been, the stronger the influence of the focal firm's high status will be on the likelihood of scandalization. In contrast, the prevalence of high-status outsiders' misconduct largely attenuates the significant effect of the focal firm's high status on scandalization. These findings provide important insights into how audiences use status in their judgments, and why only some instances of misconduct are scandalized. Our findings have several implications for theory and practice.

Theoretical Implications

Our theory provides a more complete and realistic portrayal of when and why status puts firms at a greater risk of being scandalized. Although scholars have recognized that only a select few misconduct instances become scandals (Barnett, 2014; Margolis & Walsh, 2003), our understanding of scandals, and how they differ from more moderate misconduct coverage that does not rise to the level of a scandal, has been limited.

Our findings regarding the differential effects of high-status insiders' and outsiders' past transgressions reveal the important role context (Johns, 2006,

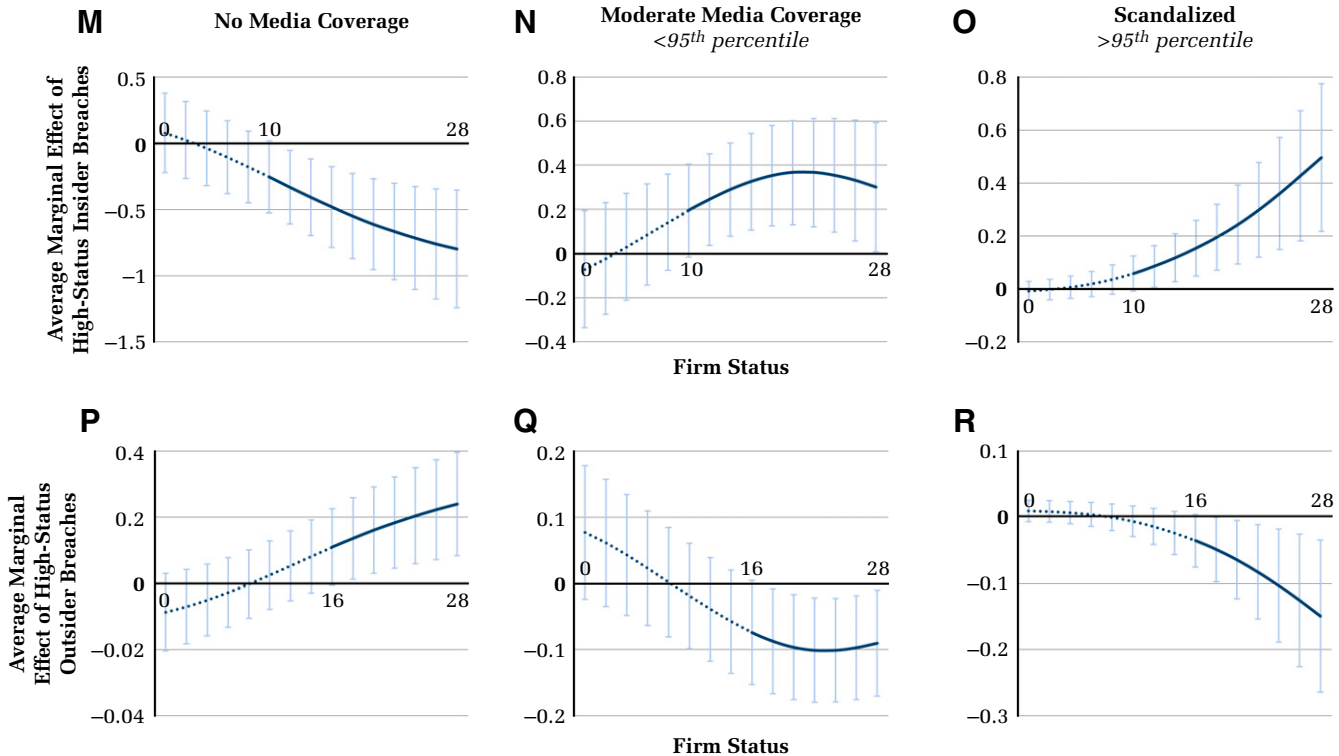
TABLE 4
Effects of High-Status Insider and Outsider Breaches at Different Levels of Firm Status

Outcomes	Average Marginal Effect of <i>High-Status Insider Breaches</i> at Different Percentiles of <i>Firm Status</i>						
	5 th	10 th	25 th	Median	75 th	90 th	95 th
No media coverage	0.081 (0.154)	0.061 (0.152)	−0.039 (0.145)	−0.352* (0.144)	−0.553** (0.166)	−0.651*** (0.183)	−0.805*** (0.230)
Moderate media coverage	−0.072 (0.135)	−0.053 (0.133)	0.034 (0.123)	0.258* (0.106)	0.355** (0.117)	0.370** (0.124)	0.296* (0.151)
Scandalized	−0.010 (0.019)	−0.007 (0.019)	0.006 (0.022)	0.093* (0.042)	0.197** (0.064)	0.281** (0.086)	0.509*** (0.145)

Outcomes	Average Marginal Effect of <i>High-Status Outsider Breaches</i> at Different Percentiles of <i>Firm Status</i>						
	5 th	10 th	25 th	Median	75 th	90 th	95 th
No media coverage	−0.088 (0.060)	−0.081 (0.059)	−0.049 (0.055)	0.060 (0.055)	0.137* (0.063)	0.177* (0.070)	0.243** (0.080)
Moderate media coverage	0.077 (0.052)	0.071 (0.051)	0.042 (0.047)	−0.044 (0.040)	−0.088* (0.041)	−0.100* (0.040)	−0.089* (0.041)
Scandalized	0.010 (0.008)	0.010 (0.008)	0.007 (0.009)	−0.016 (0.015)	−0.049 [†] (0.025)	−0.076* (0.035)	−0.153* (0.060)

[†] $p < .10$
* $p < .05$
** $p < .01$
*** $p < .001$

FIGURE 3
Marginal Effects Analysis Exploring the Main Effects of High-Status Insider and Outsider Breaches



2017) plays in shaping the influence of the focal actor's status on scandalization (Adut, 2005, 2008; Graffin et al., 2013; Jonsson et al., 2009). We thus contribute to the scandalization literature by focusing on contextual factors and media routines, and showing how context can either enhance or largely attenuate the effect of firm status on how journalists decide what stories to cover (Graf-Vlachy et al., 2020; Oliver et al., 2023; Shoemaker & Reese, 2013). While recognizing the act's deviance, we focus on the social significance of other transgressors inside and outside the focal transgressor's industry, and how they create different contexts that alter the social dynamics and cognitive processes that can affect the misconduct's publicity (Adut, 2005; Nyhan, 2014, 2017; Piazza & Jourdan, 2018; Thompson, 2000). Our *post hoc* analyses also showed that not only did scandalized firms' misconduct receive more coverage, the coverage's tenor was also significantly more negative and included more scandal-related words than for firms whose misconduct received moderate coverage. In sum, these results provide opportunities for future research to explore what creates the difference between scandals and merely publicized incidents, or the variance among scandals along these dimensions.

By understanding the routines employed to regularly produce news (Davies, 2008; Pollock et al., 2008; Shoemaker, 1996), we show how the prevalence of high-status insider transgressions can increase the scrutiny of patterns by allowing journalists to craft a more industry-centric story—regardless of whether industry characteristics are relevant to the misconduct (Han et al., 2023; Jonsson et al., 2009; Paruchuri & Misangyi, 2015), enhancing the likelihood of scandalization. In contrast, the prevalence of high-status outsider transgressions can draw attention away from the focal firms' transgression because it does not fit this storyline, or because the prevalence across different industries dilutes journalists' abilities to identify a coherent theme, thus creating safety in numbers and reducing the likelihood of scandalization by shaping the media's attention allocation. Thus, our theory adds new insights that single-industry studies (e.g., Zavyalova et al., 2012) cannot assess, and shows how other findings, such as Dewan and Jensen's (2020) finding that the increasing prevalence of misconduct facilitates the sanctioning of high-status transgressors, can also be further contextualized by bringing in the status and categorical proximity of the past transgressors.

Our study also contributes to the status literature. Although status is a relational construct (Sauder

et al., 2012), the influence of others' status on focal actors' outcomes has primarily been theorized and tested in the context of status leakage—where lower-status actors receive a status boost from higher-status affiliations (Graffin, Wade, Porac, & McNamee, 2008; Podolny & Phillips, 1996). We extend our understanding of status's relational nature by theorizing when and how others' status functions as an evaluative context that influences how an actor's behavior—particularly their misconduct—is processed (Boivie et al., 2016; Hubbard et al., 2018; Rao et al., 2005). In so doing, we also tap into the long-argued but infrequently tested intersection between the vertical (i.e., status) and horizontal (i.e., category) dimensions in social evaluations (Delmestri & Greenwood, 2016; Han & Pollock, 2021). We theorized and found that presumed similarities among high-status actors lead other high-status actors' behaviors to affect how the focal actor's behaviors are interpreted (Han & Pollock, 2021; Pollock et al., 2019; Rao et al., 2005). However, we also theorized and found that high-status actors outside a focal actor's category, who are exemplars of a different hierarchy's values, also exert significant influence on audiences' evaluations, suggesting that conceptualizing status hierarchies as siloed within their local contexts can be misleading. Our theory and findings provide a useful avenue for research on how audiences jointly utilize status and categories in their evaluations.

Our *post hoc* analyses also highlighted important nuances regarding the reach of other actors' status. We found that breaches by insider firms with higher status were statistically and practically influential for firms with status above the median in our sample, but not for firms below this cutoff. This implies that prior breaches within an industry by high-status firms only shape the scandalization process for middle- and high-status firms, but not for lower-status firms. Similarly, transgressions by high-status outsiders were only influential for middle- and high-status firms. Broadly, these findings suggest that prior high-status transgressions are important contextual factors shaping the scandalization process for socially significant firms, but they do not make less socially significant actors' deviance more, or less, newsworthy. This may suggest that past transgressions by high-status firms may matter when audiences are primed to make status-based inferences due to the focal firm's high status (Hubbard et al., 2018). Future research should benefit from exploring this complex web of interdependencies further.

Practical Implications

Despite the long interest in the costs and benefits of possessing high status (Dewan & Jensen, 2020; Graffin et al., 2013; Sauder et al., 2012), scholars have mostly assumed that status is a burden when it comes to scandalization. Our findings show that a firm's status may not necessarily be burdensome when high-status firms outside the firm's industry have been committing similar misconduct. Note, however, that we are not advising high-status firms on how to escape the responsibilities for their misconduct. Rather, our study suggests that accounting for broader misconduct trends can help firms more effectively manage the publicity following their own misconduct, which could enable them to invest in preventive and remedial measures. However, if misconduct is prevalent within their industry, middle- and high-status firms should prepare for the likelihood that their misconduct will receive at least some media attention, if not become scandalized, and may want to proactively take actions that limit its influence, or dissuade the media from aggressively focusing on the firm.

Limitations and Directions for Future Research

Like all research, ours has limitations that suggest future research directions. First, we focus on data breaches as the misconduct of interest. Although we are seeing more and more data breach scandals such as those involving firms like Yahoo!, Meta Platforms (formerly, Facebook), and Marriott, data breaches have indeed become a serious social and business issue (Accenture, 2019; Petersen, 2020), it would be beneficial to replicate our findings using different types of misconduct to see whether there are misconduct characteristics that create interesting boundary conditions. In addition, testing our theory using different audiences, such as analysts, investors, or peer firms may further enrich our understanding of status's role in scandalizing misconduct.

Second, we based our theory and empirics on industry categories, because industries are the most frequently used categorization scheme in business (Porac et al., 1999; Zavyalova et al., 2012). However, there are alternative ways to categorize a firm's in-groups and out-groups; for instance, geographically, by customers served, or by strategic groups (Naumovska et al., 2021; Porac, Thomas, & Baden-Fuller, 1989). Examining whether our theory holds when using other categorization approaches may generate useful insights. Further, some industries may have higher category-level status than others (Han &

Pollock, 2021), or receive more media coverage. Exploring the interplay between firm-level and industry-level status seems promising.

Finally, given that we are using archival data but theorizing about cognitive processes, we cannot empirically measure some of the theoretical mechanisms we argue are at work. Future research could use other methodological approaches, such as experiments or policy capturing, to delve into the micro-foundations of our theory.

CONCLUSION

As social actors, we do not exist in a vacuum, and neither do firms that commit misconduct and the journalists who decide whether to report on them. In this study, we explored how awareness of status hierarchies and their boundaries affect these perceptions and actions. We showed how not just a transgressor's own status, but the status of others who commit the same transgressions, can affect whether a firm's missteps become an embarrassing blip in its history, or are blown up into scandals that can affect its trajectory and image for years to come.

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