Distal and Historical Constraints on Leadership: Loneliness and Leniency from the Leader's Perspective

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Abstract

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Contextual factors – environmental stimuli external to individuals – are historically understudied loci of leadership. This dissertation comprises two chapters exploring characteristics of distal and historical context that impose constraints on leaders. In the first chapter, I examine distal context in terms of stereotypes associated with leader and gender roles. I propose a gender-contingent effect of leader role occupancy on loneliness, such that the effect is positive only for women but not men, and that experienced authenticity mediates this effect. Across three studies using diverse methods and data, I find support for the hypothesized model. In the second chapter, I examine historical context in terms of leaders' past misconduct prior to disciplinary decision-making. I propose two mechanisms – hypocrisy avoidance and moral compensation – that provide potentially competing explanations for how leaders' past misconduct influences their

leniency towards subordinates, and suggest social consensus determines when either mechanism occurs. Across three further studies using diverse methods, results suggest hypocrisy avoidance occurs regardless of social consensus, while support for moral compensation is mixed and depends on research methodology. This dissertation furthers understanding of distal and historical contextual factors that shape leaders' emotions and behavior.

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Dreams can’t be buy.

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INTRODUCTION

Leadership is deeply rooted in the study of organizational behavior, and broadly refers to a process whereby leaders influence others to achieve common goals (DeRue, 2011; Hollander & Julian, 1969; Yukl, 2002). Accordingly, leadership research has traditionally focused on attributes of individuals that make them effective wielders of influence – the "Great Man" approach to leadership (Lord et al., 2017). This approach has unveiled important distal (e.g., cognitive abilities, personality) and proximal (e.g., tacit knowledge, social appraisal skills) attributes that affect important leadership outcomes such as leader effectiveness and emergence (Zaccaro et al., 2004). Also building on this perspective are more behaviorally oriented leadership theories. The Ohio State research program highlighted that initiating structure and consideration were two primary dimensions of leader behavior (Fleishman, 1953). This approach has since evolved to further examine behaviors such as transformational and charismatic leadership (Bass, 1985; Burns, 1978; Judge & Piccolo, 2004), suggesting that exceptional performance arises from leaders motivating followers towards higher goals, and helping them constantly learn and improve through critical thinking (Lord et al., 2017).

While these literatures have revealed generative insights about the effects leaders have on other people and the broader context, a key overlooked consideration has been the effect that context has on leaders themselves (Avolio, 2007). That is, most leadership theories are "largely context free" (Zaccaro & Klimoski, 2001), and underestimate the extent to which leaders themselves are constrained by the contexts in which they operate. In conceptual terms, poor understanding of these constraints may lead concomitantly to poor understanding of the true reasons underlying leaders' cognitions and behaviors, as well as their effects on the context (Vroom & Jago, 2007). In statistical terms, this is problematic because it creates room for

endogeneity; the observed attributes and behaviors of leaders are situated within contexts that may constrain (or facilitate) those same attributes and behaviors (Antonakis et al., 2010). Ignoring context effects can thus severely bias estimates of leadership effects on outcomes (Liden & Antonakis, 2009).

Proximal, Distal, and Historical Constraints

This points to the importance of greater consideration of contextual constraints in leadership research (Oc, 2018). Indeed, context features increasingly in emerging leadership approaches, though it is still relatively understudied (Dinh et al., 2014). Dinh and colleagues' (2014) review placed contextual approaches to leadership as the third most prolific emerging leadership perspectives, behind strategic and team leadership. Avolio (2007) suggested that three particular aspects of context play a role in leadership processes. The proximal context refers to the immediate characteristics of a leader's group or tasks that affect leaders themselves and their relationships with others. The distal context refers to characteristics of the broader sociocultural environment (e.g., organizational and national culture, nature of competitors) that affect how leaders make meaning of the events they encounter. Finally, the historical context refers to events that transpire before a leader's emergence, and can affect the types of leadership and followership that are deemed more or less effective. Of these three aspects of context, existing theories have arguably focused most on the proximal context. Fiedler's (1964) contingency model first articulated how leaders' motivations affected their effectiveness depending on task structure (though empirical support for the model has been limited, Jago & Ragan, 1986; Peters et al., 1985). Task structure thus constituted a proximal contextual factor that could constrain leaders. Path-goal theory (House, 1971) suggested that leader behaviors are effective to the extent that they match with subordinate and environmental characteristics. For example,

followers who face ambiguous performance demands, another proximal contextual factor, may prefer directive leader behavior (House, 1996). The substitutes for leadership perspective (Kerr & Jermier, 1978) also suggested that leadership affects outcomes less strongly in the presence of contextual factors that may substitute for or neutralize leadership. Substitutes affect the types of leadership which are influential as well as the consequences of leadership, while neutralizers reduce leadership effects. Both substitutes and neutralizers comprise a variety of proximal contextual characteristics such as task ambiguity, feedback, and organizational formalization (Podsakoff et al., 1996), which may function as either loci or mechanisms of leadership (Dionne et al., 2002). These theories explain how the effects of leadership may be more or less constrained by proximal characteristics of the immediate work context, particularly those that relate to subordinates and tasks, and thus constitute the primary consideration of context in leadership theories.

Less well understood are constraints associated with the distal and historical context. Theories that explain general norms associated with leadership constitute perhaps the central research around the distal context, which encompasses broader sociocultural characteristics. For example, social role and role congruity theories (Eagly & Karau, 2002; Eagly & Wood, 2012) delineate the expectations associated with leader and gender roles along the broad dimensions of agency and communion. This literature examines expectations and perceptions third parties have of leaders by virtue of the agentic stereotypes associated with leader and male gender roles, and communal stereotypes associated with female gender roles. A general finding is that leader roles are strongly associated with masculinity (Koenig et al., 2011), and that this often results in prejudice against female leaders (Phelan & Rudman, 2010). But considering the many contingencies that have instead been found to create a female leadership advantage (e.g., gender-

role identity, Kark et al., 2012; type of leader behavior, Lemoine & Blum, 2021; top management position, Rosette & Tost, 2010), it is clear that even such a widely used paradigm of distal context remains lacking in general, cohesive theory.

Relatedly, the GLOBE model (House et al., 2002) examines nine cultural dimensions on which individuals hold implicit expectations of leaders, gender egalitarianism being one of them. Uncertainty avoidance, power distance, societal collectivism, in-group collectivism, assertiveness, future-orientation, performance orientation, and humane orientation comprise the other eight, drawing greatly from the cultural dimensions originally identified by (Hofstede, 2001). The model suggests that societal culture, an example of distal context, affects leader attributes and behavior, and the types of leadership which are more likely to be endorsed. This research, along with the literature on leader's social roles, also examines very broad dimensions of cultural norms that affect how leaders are perceived. And given the substantial variation in whether leadership behaviors are perceived similarly or differently across cultures (Dorfman et al., 2012), there is much more work that can be done to elucidate specific cultural norms associated with distinct leadership behaviors. Beyond this, there is also a wide range of distal characteristics that remain understudied. For example, research on intersectional norms (Liu, 2019) may advance understanding of how combinations of social roles beyond the leader and gender role may affect leadership. Research on economic contexts can also explain how leaders are constrained by macroeconomic (e.g., unemployment), industry-level (e.g., market structure), and organization-level (e.g., firm access to resources) factors (Hiller et al., 2020). These are research areas that have burgeoned in their respective disciplines, but less so in management. Nonetheless, they highlight the importance of moving beyond traditional considerations of distal context, which may be overly broad, to consider other contextual constraints on leaders.

Finally, historical contextual effects on leaders are arguably the least well understood among the three types of context. From a more "distal historical" perspective (Avolio, 2007), a life span perspective highlights that an individual's life experiences beginning from childhood affect their subsequent leadership development. Economic conditions (Barling & Weatherhead, 2016) and parenting styles (Liu et al., 2019) during childhood can affect leader emergence. Similar effects of family environment on leadership continue into adolescence (Oliver et al., 2011). Indeed, because early stages of life are particularly sensitive developmental periods, they are also expected to be the most crucial for subsequent leader development in adulthood (Murphy & Johnson, 2011). From a more proximal historical perspective, leaders engage in dynamic and reciprocal processes over time where they influence others and the context (Shamir, 2011). They construct narratives based on past events, which are used to interpret the meaning of future events (Combe & Carrington, 2015). Research in this literature has thus explored how leaders' ongoing experiences shape their subsequent cognitions and behaviors. This can affect their moral opinions (Kreps et al., 2017), relationships with others (Kelley & Bisel, 2014), and even how they respond to crises (Mumford et al., 2007).

For example, Kreps and colleagues (2017) highlight that leaders who undergo transformative experiences may undergo concomitant changes in moral judgment, though they examine how this affects third parties' evaluations of hypocrisy instead of leaders themselves. Notably, foundational theories of moral development (Kohlberg, 1971; Rest, 1986) suggest the way individuals reason about moral issues changes as they acquire new experiences. This has important implications because leaders' moral reasoning also affects their subordinates' perceptions of transformational and transactional leadership (Turner et al., 2002). In terms of relationships, qualitative research finds that a key concern for leaders is figuring out who they

can trust. They do so using narrative sensemaking, integrating their past experiences with others into storylines characterized as "predictably good, unpredictable, and predictably bad" (Kelley & Bisel, 2014, p. 438). More broadly, Mumford and colleagues (2007) suggest leaders form prescriptive mental models to deal with problems, and these cognitions arise primarily from knowledge abstracted from their past experiences. This can include complex information including causes, outcomes, and contingencies for certain events (Hammond, 1990). This burgeoning line of research thus highlights that events in a leader's past, as part of the historical context, can influence how they process information when they encounter new circumstances, and motivate them towards different action tendencies. Where there is room for leaders to dynamically regulate their behavior in relation to past events, consideration of the historical context is likely to be particularly important. However, in between the distal historical context, which examines events starting from birth, and the more commonly studied proximal historical context, which includes events in the immediate history of a leader's ongoing relationships with others, there lies a large time period within which many events and experiences may accumulate and subsequently influence a leader's cognitions and behaviors. This is the premise of emerging literatures that explain how leadership effects spillover onto other individuals in the future (Chen et al., 2019; Tu et al., 2018), and highlights the potential for more research on the effects of leaders' past experiences from a longer timeframe in the "past".

Distal Contextual Effects on Loneliness and Historical Contextual Effects on Leniency

Accordingly, this program of research draws from Avolio's (2007) broad framework of contextual factors affecting leadership to focus on constraints associated with the distal and historical context. I focus on two specific constraints and further extend the contextual perspective by examining how these constraints affect leaders themselves. This departs from the

traditional focus on how leaders affect outcomes external to themselves (Eberly et al., 2013). For example, recent research has highlighted the effects of ethical leadership (Lin et al., 2016), abusive supervision (Liao et al., 2018), and transformational leadership (Lin et al., 2019) on leaders themselves, though the loci of leadership (Hernandez et al., 2011) in these studies remains within the leader instead of the context. First, to examine effects of the distal context, I draw from role congruity theory (Eagly & Karau, 2002) to explore gender-contingent effects of leader role occupancy on loneliness. Role congruity theory suggests male leaders are evaluated more favorably than female leaders because the leader role is strongly associated with agentic and masculine stereotypes (Koenig et al., 2011). This literature has thus primarily examined third parties' evaluations of leaders as focal outcomes. Importantly however, the interaction between leader roles and gender roles may also impose actual constraints on leaders themselves independent of third parties' perceptions (Eagly, 2005). Constraints on relationships are a particularly salient concern for leaders (Lindorff, 2001). This research thus explores how the leader role interacts with gender roles to affect leaders' feelings of loneliness, a negative emotion experienced when one perceives deficiencies in their relationships (Peplau & Perlman, 1982). Across one archival study, one lab experiment, and one two-wave survey, I found support for the hypotheses that for women but not men, occupying leader roles is associated with more loneliness, and this is explained by reduced experiences of authenticity.

In the second chapter, I examine effects of historical context in the domain of disciplining misconduct, which is generally a responsibility undertaken by organizational leaders (Arvey & Jones, 1985). Taking a historical perspective entails moving beyond examining how the immediate characteristics of a misconduct event and its perpetrator affect a disciplinary decision, to examining how seemingly independent, past events also affect it. This presents a theoretical

puzzle because two potentially competing mechanisms in particular may determine how leaders' past experiences affect their use of discipline. Theories of hypocrisy suggest that, to avoid hypocrisy and the attendant psychological discomfort (Stone et al., 1997), leaders who have engaged in more misconduct in the past may be compelled to show more leniency to a transgressing subordinate. On the other hand, moral self-regulation theory (Mullen & Monin, 2016; Zhong et al., 2009) suggests individuals dynamically regulate their moral behavior by evaluating how their past behavior matches up to their ideal moral selves. Because individuals tend to seek a positive level of moral self-regard, and showing leniency may be considered unjust behavior (Trevino & Ball, 1992), leaders who have engaged in more misconduct in the past may *compensate* for it by showing less leniency to a transgressing subordinate. This research thus aims to delineate when and why either of these mechanisms may take precedence. To integrate these two theoretical perspectives, I suggest the social consensus around a misconduct event determines whether hypocrisy avoidance or moral compensation occurs. When there is high social consensus that a subordinate's misconduct is unethical, a leader can use less lenient (i.e., more severe) punishment to regain moral credits and compensate for their own past misconduct. When social consensus is low however, the leader's personal preference to avoid hypocrisy by showing leniency can take hold. Across two experiments and one critical incident study, I found mixed support for the hypocrisy avoidance and moral compensation mechanisms. Past misconduct was generally associated with anticipated guilt regardless of social consensus, indicative of the hypocrisy avoidance mechanism. For the moral compensation mechanism, although past misconduct was associated with a loss of moral credits (except in Study 3), moral credits interacted with social consensus to affect leniency only in Study 2.

These two chapters separately focus on how a distal constraint (sociocultural norms of agency and communion) and a historical constraint (leaders' past misconduct) affects leaders' own cognitions and behaviors. Because leaders operate in complex environments and consider myriad aspects of the context in daily events, this program of research takes the approach of considering the key contextual constraints that may affect leaders in the domains of loneliness and leniency, instead of a more general approach to context that may underspecify the relevant constraints in a specific domain (Avolio, 2007; Marion & Uhl-Bien, 2001). This general program of research thus aims to make two main theoretical contributions. First, various leadership theories share in common the specification of a locus and mechanism of leadership (Hernandez et al., 2011). The locus is the source from which leadership originates, and has traditionally been studied in terms of a designated leader role. Correspondingly, the mechanism refers to how leadership is transmitted, and may refer to affect, motivation, or values promoted by the leader. Extending this perspective, I examine leadership processes where context instead is the locus of leadership, and leaders' own experiences are the mechanisms and outcomes. In particular, the first paper depicts interactive cultural stereotypes associated with leader and gender roles as the loci of leadership, and leaders' own affective experience (instead of third parties' perceptions) as the mechanism and outcome. The second paper depicts the leader's past behavior (i.e., historical context) as the locus of leadership, and the leader's cognitions around hypocrisy and moral self-regulation as the mechanisms. Together, these studies move the literature towards better understanding of the lived experience of leadership as it is determined by the leader's context.

The second, related contribution is in exploring how contextual constraints can affect both intrapersonal and interpersonal mechanisms of leadership. Eberly and colleagues (2013) highlight the distinction between the two. Traits, affect, and cognition are intrapersonal, while

behaviors are interpersonal mechanisms. Yet these mechanisms also simultaneously and dynamically affect each other. Under a traditional paradigm that depicts leaders themselves as the loci of leadership, it is clear that intrapersonal factors as well as leader behaviors affect outcomes of interest. But because context is an understudied locus of leadership, it is less clear to what extent it affects leader processes via intrapersonal or interpersonal mechanisms. The two papers provide distinct answers to this question in their respective domains. The first paper argues that the context represented by leader and gender role expectations influences leaders' subjective experiences of loneliness via the similarly subjective experience of authenticity. In this regard, intrapersonal mechanisms specifically transmit the effects of context. The second paper alternately argues that the context created by a leader's past behavior affects an interpersonal behavior, leniency, via the intrapersonal mechanisms of hypocrisy avoidance and moral compensation. This highlights a more conventional way that the context may constrain leadership – through intrapersonal followed by interpersonal mechanisms. These studies thus explore specific intrapersonal and interpersonal mechanisms that transmit the effects of different aspects of context to leaders.

CHAPTER 1: GENDER-CONTINGENT EFFECTS OF LEADERSHIP ON LONELINESS

Abstract

This article builds from role congruity theory to develop and test a theoretical model about the gender-contingent experiences of loneliness for individuals taking on leadership roles. Across three complementary studies using diverse methods, occupying a leader role was associated with greater loneliness for women, but not for men. This effect was mediated by authenticity such that women experienced less authenticity when they occupied leader roles, but men did not. Study 1 applied a propensity score matching method to a longitudinal, archival dataset. Study 2 replicated and extended Study 1 by examining the mediating mechanism of authenticity using a laboratory experiment. Study 3 adopted a two-wave survey design to address the limitations of the previous studies and replicate their results. The findings contribute to theory on how occupying leader roles affects incumbents' affective experiences.

Taking on leadership roles is an important career goal for many individuals because such roles confer both material (e.g., salary, working conditions) and nonmaterial benefits (e.g., power, status). Indeed, current research suggests that being a leader is likely to *feel* good. For example, an elevated sense of power increases positive affect and reduces negative affect (Keltner et al., 2003; Li et al., 2016). Achieving high levels of status is itself also a fundamental desire and leads to reduced stress and increased subjective well-being (Anderson et al., 2012, 2015). Given that being a leader is often associated with greater power and status (Yukl, 1989), these aspects of leader roles may generally be expected to result in positive affective outcomes.

Yet these findings do not account for the real difficulties associated with occupying leader roles, many of which stem from dissatisfactory relationships. Leaders often struggle to manage relationships with their subordinates (Benjamin & O'Reilly, 2011), and feel that support, understanding, or genuine relationships are hard to come by (McCauley et al., 1994; Wright, 2012). This suggests that loneliness – the negative affect that arises from deficient relationships (Peplau & Perlman, 1982) – may be a costly outcome of leader role occupancy, as also evidenced by the common belief that it is "lonely at the top". Indeed, while loneliness has been generally described as an epidemic in the workplace (Murthy, 2017), recent evidence highlights that those occupying leader roles (e.g., managers, business owners) may be particularly susceptible (Cigna, 2020; Fernet et al., 2016). Given that loneliness is harmful for key organizational outcomes such turnover, commitment, and in-role and extra-role performance (Chen et al., 2016; Lam & Lau, 2012; Ozcelik & Barsade, 2018), and the possibility that some leaders may be more at risk for loneliness, it is important to understand when and why being a leader may actually feel lonely.

Despite the widespread beliefs and recent evidence that leaders may feel particularly lonely, some related research also finds negligible relationships between hierarchical rank and loneliness (Reinking & Bell, 1991; Wright, 2012; Yip, 2015). These mixed findings for leadership-loneliness relationships may be due to the neglect of important individual differences affecting the experience of leader roles. Individuals do not occupy leader roles in isolation, but in addition to other existing social roles. Role congruity theory, which broadly explains prejudice towards female leaders, suggests the most salient social role in the leadership context is gender (Eagly & Karau, 2002). Because leader roles and female (but not male) gender roles often contain conflicting expectations, men and women are evaluated differently when they take on leader roles (Koenig et al., 2011), which may lead to differential constraints on their behavior (Eagly, 2007). Accordingly, this article suggests the effect of leader role occupancy depends on the incumbent's gender. Drawing from role congruity theory, I suggest that for women, occupying a leader role leads to reduced experiences of authenticity due to the behavioral constraints imposed by their conflicting leader and gender roles. This subsequently leads to increased loneliness. However, this effect is not present for men, who do not experience such conflicting role-expectations and associated behavioral constraints.

The present research aims to make three contributions to research on role congruity, loneliness, and gender in organizations. First, role congruity theory emphasizes the reactions of observers to female compared to male leaders (Eagly & Karau, 2002). I shift the focus of this theorizing to the experiences of leaders themselves, suggesting that loneliness is also a key outcome of role incongruity for female leaders compared to female non-leaders, but not necessarily compared to male leaders. This also adds to understanding of how external expectations affect leaders (e.g. Hoyt & Blascovich, 2010; Latu et al., 2013). Second, I extend

the literature on workplace loneliness beyond examining its consequences (e.g., Lam & Lau, 2012; Ozcelik & Barsade, 2018) by explaining why leader roles specifically may be an antecedent of loneliness. This extends the literature by answering the open question of why employees feel lonely in the first place, while focusing on an antecedent that is of both theoretical and practical relevance. It also highlights why occupying a leader role may not be as positive an experience as previously thought. Finally, I contribute to the ongoing conversation on how gender influences individuals' work experiences. This research has revealed valuable insights about how women's experiences differ from men's in many domains (e.g., developmental job experiences, Ohlott et al., 1994; mentoring, Ragins & Scandura, 1994; work-family interfaces, Shockley et al., 2017). I highlight that beyond cross-gender comparisons (i.e., men vs. women), it is also useful to make within-gender comparisons across roles. This is in line with emerging perspectives that examine gender as a moderator of various organizational experiences (Kacmar et al., 2011; Zhan et al., 2015).

Theory and Hypotheses

Loneliness is a subjective, negative affect that arises from an attribution of deficient social relationships (Heinrich & Gullone, 2006; Peplau & Perlman, 1982). Loneliness may be a chronic tendency for some, but is commonly studied as a discrete emotion because most individuals experience it to varying degrees from time to time (Asher & Paquette, 2003). Loneliness is also domain specific as individuals may experience different levels of loneliness with respect to relationships in different domains such as with friends, family, and at work (Ozcelik & Barsade, 2018; Weiss, 1973). As an affective experience, loneliness is distinct from behaviors that may cause it, such as ostracism and discrimination (Williams, 2007).

Role Congruity and Loneliness

Several theoretical perspectives provide initial countervailing intuitions for potential leadership-loneliness relationships. Occupying a leader role refers to whether an individual enacts a leader role at work, which most commonly occurs in formally assigned positions where individuals are responsible for other employees (Li et al., 2011; Mintzberg, 1971). This means leader roles often come with higher levels of power and status (Yukl, 1989). Past results suggest individuals in positions of power are less interested in maintaining positive interpersonal relationships (Magee & Smith, 2013). For example, Waytz et al. (2015) find a high-power manipulation decreases loneliness by reducing the need to belong. High levels of status have also been associated with fewer negative emotions (Keltner & Haidt, 1999). For example, higher-status individuals experience fewer negative emotions, particularly when they work in groups of mostly lower-status individuals where they can avoid competing for status (Chattopadhyay et al., 2010). Thus, if leadership is equated with only power or status, occupying leader roles may appear to have primarily positive affective outcomes.

Yet this does not account for leadership being associated with many real negative experiences ranging from strain, to exhaustion, to sleep deprivation (Cavanaugh et al., 2000; Courtright et al., 2014; Svetieva et al., 2017). Due to the pressure and expectations of leader roles, leaders face many difficulties managing relationships with and satisfying multiple stakeholders (McCauley et al., 1994). This may even create anxiety (Mannor et al., 2016), which can also limit their ability to have fulfilling relationships (Russell et al., 1980). Indeed, leaders may feel lonely because they are unable to develop meaningful or genuine relationships at work (Cooper & Quick, 2003; Wright, 2012). To more fully understand why leader roles may increase loneliness, a key first consideration is that roles (e.g., the leader role) cannot only be described

by their associated resources (e.g., power, status). Traditional role theory suggests such resources are also accompanied by expectations for behavior, and that individuals occupying similar roles may further be subject to different expectations depending on other personal characteristics (Biddle, 1986). Acknowledging this complexity, role congruity theory highlights that the leader role is indeed associated with power and status, which may be categorized under the broader construct of agency. But role incumbents themselves also have preexisting social roles which impose constraints on their behavior. It is thus necessary to examine how the combination of the leader and social role, instead of either one alone, affects an incumbent's experience of leadership. And since gender is the most salient social role used to evaluate behavior (Fiske et al., 1991), leader role effects are likely to depend on gender, instead of manifesting as main effects on subjective experiences.

Individuals hold beliefs about gender roles with respect to agency and communion (Bakan, 1966). Agency involves mastering the environment and enacting competence and achievement (e.g., dominance, ambition), and is mostly ascribed to men (Eagly & Karau, 2002). Communion involves concern for others instead of the self (e.g., affection, social sensitivity) and is mostly ascribed to women. Because leader roles are associated with masculinity (Koenig et al., 2011) and agentic traits (Schein, 1973), female gender roles conflict with leader roles (but see Rosette & Tost, 2010 for an exception). The communal traits ascribed to women conflict with the agentic traits ascribed to leaders. Thus, women who occupy leader roles are viewed unfavorably because they seem to violate both descriptive and prescriptive norms – the leader category is more descriptive of men than women, and leader behaviors are less desirable in women than men (Eagly & Karau, 2002; Madera et al., 2009). For example, women who display

agency face incivility (Gabriel et al., 2018) and backlash discrimination (Rudman & Glick, 1999).

The existence of role incongruity for women suggests that when they take on leader roles, their ability to have open and honest relationships with others is constrained because they face conflicting expectations for whether they should be agentic and strive for competence, or be communal and develop warm relationships. This means that for women, taking on a leader role imposes limits on the range of acceptable interpersonal behaviors they can enact with others, which likely leads to less-than-satisfactory relationships. Female leaders should thus feel lonelier than female non-leaders as female non-leaders occupy the same gender role without the additional constraints of the leader role.

In contrast, men experience little change in role expectations whether they are leaders or not. Male non-leaders occupy only their gender roles. Male leaders occupy both a leader and gender role, but both similarly prescribe agency and its attendant expectations. Thus, leader roles should have minimal effects on men's interpersonal behavior and relationships because they do not add anything different to the already agentic male gender role (Eagly & Wood, 2012), which also includes power and status (Eagly, 1983; Ridgeway & Correll, 2004). The alternative possibility is that the leader roles make men feel *less* lonely. But this neglects that the leader role is also associated with challenging expectations in terms of managing relationships and potential conflict among different stakeholders ranging from subordinates, to superiors, to clients or other external parties (McCauley et al., 1994). Separately, male gender roles also place little value on close relationships to begin with (Baumeister & Sommer, 1997). The first aim of this research is thus to establish the overall moderating role of gender on the effect of leader role occupancy on loneliness, such that the effect should be positive only for women.

Hypothesis 1: The effect of occupying a leader role on loneliness is moderated by gender such that the effect is positive for women but not men.

Gender-Contingent Experiences of Authenticity

The moderating role of gender can be further explained by authenticity. Authenticity broadly refers to the "unobstructed operation" of one's true self (Kernis, 2003). In particular, the subjective experience of authenticity involves perceiving that one can achieve truthfulness in individual and interpersonal behavior with respect to one's values and beliefs (Kernis, 2003; Schmader & Sedikides, 2018). Being free to interact with others as one desires is necessary for experiencing authenticity (Schmader & Sedikides, 2018). Individuals who experience more constraints on their behavior are thus less able to relate to others in ways that reflect their true cognitions and emotions. This is very relevant for leaders, whose roles inherently require influencing and interacting with followers or other coworkers (Hollander & Julian, 1969). Since leader roles implicate prescriptive norms that impose boundaries on acceptable behavior (Eagly, 2007), they effectively impose behavioral constraints individuals try to stay within to avoid disapproval (Cialdini et al., 1991). If these constraints also compel individuals to change how they would otherwise behave, they are likely to reduce authenticity experiences.

From a role congruity perspective, individuals in non-leader roles are those who have existing social roles of which gender is the most salient. If they assume a leader role, they face a new set of expectations which overlap to varying degrees with their original gender role. For women, there is little overlap between their gender role that encourages communion and the leader role that encourages agency. This is a difficult circumstance – displaying agency leads to being penalized for lacking warmth, but displaying communion leads to devaluations of competence (Eagly, 2005; Rudman & Glick, 1999). This reflects increased behavioral constraints

for female leaders, who experience a smaller range of acceptable behaviors compared to female non-leaders. Thus, women likely have to suppress how they would usually interact with others when they take on leader roles, which limits their authenticity experiences. For example, female physicians feel they must alter their behavior after taking on leader roles in physician teams, but male physicians do not (Kolehmainen et al., 2014). This is also known as a compliance strategy where women feel they must conform to a narrow set of behavioral expectations (often set by men, Chattopadhyay et al., 2004). I thus suggest that because female leaders face conflicting leader- and gender-role expectations, they experience less authenticity than female non-leaders, who occupy only gender-roles.

For men however, being a leader does not lead to conflicting expectations around agency and communion. Instead, men face a similar range of acceptable behaviors whether they occupy leader roles or not. This means they experience negligible changes in behavioral constraints upon occupying leader roles, and should thus experience no change in authenticity. Importantly, these arguments are based on the extent to which the leader role imposes constraints in addition to one's existing gender role. Because men do not experience fewer constraints when they become leaders, I do not propose that they experience more authenticity when they become leaders.

Hypothesis 2: The effect of occupying a leader role on experienced authenticity is moderated by gender, such that the effect is negative for women but not men.

I further theorize that experienced authenticity influences loneliness. As loneliness is a subjective affective experience, effects of objective structural positions (i.e., leader roles) on loneliness are likely mediated by more subjective appraisals of them. Indeed, research has documented that objective relationship features are only weakly related to loneliness because the same objective relationship can be experienced in different ways (Cacioppo et al., 2014). Current

evidence suggests authentic relationships involve attributes such as trust, honesty, and support (Hinojosa et al., 2014; Ilies et al., 2005), all of which lead to satisfying relationships (Gillath et al., 2010; Rempel et al., 1985) and are thus likely to reduce loneliness. Experiencing authenticity is also associated with relationship-enhancing behaviors (e.g. reduced deception, Gillath et al., 2010; self-disclosure, Schmader & Sedikides, 2018), which should similarly limit feelings of loneliness.

Accordingly, I suggest individuals who experience more authenticity are more satisfied with their relationships and thus feel less lonely. Integrating the overall theorizing, I propose the moderating role of gender on the effect of leader role occupancy on loneliness is mediated by experienced authenticity. For women, occupying a leader role increases loneliness because they experience less authenticity. In contrast, men's loneliness and authenticity experiences are similar whether they occupy leader roles or not. The conceptual model is presented in Figure 1.

Hypothesis 3: Experienced authenticity is negatively related to loneliness.

Hypothesis 4: The effect of occupying a leader role on loneliness as moderated by gender is mediated by experienced authenticity, such that the indirect effect of occupying a leader role on loneliness via experienced authenticity is positive for women but not men.

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Overview of Studies

The classic tradeoff between external and internal validity is exacerbated when estimating leader role effects; although field observation bolsters external validity, there are likely many unobservable factors that influence both whether individuals become leaders or not, and the relationships they experience. This means there are likely omitted variable or selection biases that severely limit causal inference. However, focusing on internal validity by estimating

effects only experimentally risks sacrificing generalizability, as it may be difficult to fully inhabit leader roles in a controlled experimental setting. To balance such concerns, I conducted three complementary studies that alternately focus on external and internal validity.

Study 1 examines H1 with longitudinal archival data. This establishes the overall moderating effect of gender on the relationship between occupying a leader role and workplace loneliness. I used propensity score matching, a quasi-experimental method, to correct for pretreatment differences between treatment (leader) and control (non-leader) groups (Caliendo & Kopeinig, 2008). Study 2 replicates and extends Study 1 by examining all hypotheses using a laboratory experiment with random assignment of participants to leader and non-leader roles. Finally, Study 3 tests all hypotheses using a sample of full-time employees, thus improving the external validity of conclusions drawn from Study 2.

Study 1

Sample

Study 1 data comes from the Midlife in the United States study (MIDUS, Brim et al., 2004). This study collects responses from a national random sample of noninstitutionalized adults living in the 48 contiguous states. The study launched in 1995 and continues to sample existing and new respondents at intervals of several years. Participants respond to a battery of psychosocial surveys and demographic questions each time, though the actual questions change between intervals. Only two waves of the MIDUS contained data on loneliness – MIDUS 2 (2004-2006) and MIDUS 3 (2013-2014). I thus matched responses across these two waves according to participants' unique identification numbers, which provided longitudinal data to test Hypothesis 1. I selected participants who had complete responses, were under 65 years old and not in a supervisory position at Time 1 (see Analytic Method), and employed in wage-earning

work at both Times 1 and 2. This resulted in an initial sample of 442 individuals, of which 102 became leaders before Time 2.

Analytic Method

Propensity score matching aims to quasi-experimentally contrast non-manipulated treatment groups to control groups by matching them on their initial propensity to be treated, based on observed covariates. This is particularly useful for naturally occurring treatment groups in field settings such as those occupying leader roles. In most real organizations, it is not feasible to randomly assign individuals to leader roles. Instead, this method matches each Time 2 leader with a Time 2 non-leader, where both individuals at Time 1 had similar propensities to become leaders. The sample must therefore only include those who are not leaders at Time 1. In other words, in a sample of non-leaders at Time 1, some number become leaders at Time 2. The sample is matched by comparing someone who did become a leader with someone who had a similar propensity to become a leader but did not actually do so. The difference between these two individuals is thus considered a pseudorandom assignment to a leader role. Propensity score matching allows for relatively unbiased estimates of treatment effects and results can approximate those of true randomized experiments due to greatly reducing selection bias (Hong & Raudenbush, 2005; Luellen et al., 2005).

The first step in this method is to estimate the propensity of receiving treatment (i.e., to become a leader) for each individual. This was done following current convention where a logistic regression model is used to predict the probability of occupying a leader role at Time 2 from relevant covariates at Time 1 (Ho et al., 2011). In this step it is recommended to select a large number of these covariates based on theoretical knowledge of their influence on treatment

assignment and the dependent variable (Ho et al., 2007). This allows for more accurate estimates of propensity scores.

Next, a matching algorithm is used to match individuals based on these propensity scores. I used nearest neighbor matching without replacement, the most straightforward and commonly used algorithm in applied research (Austin, 2011, 2014). This method matches each treated individual with the control individual who has the most similar propensity score, and was conducted using the R package MatchIt (Ho et al., 2011). In the supplemental analyses, I report results using three other algorithms to verify that they are robust to the choice of algorithm. A final matched sample of N = 204 was obtained (M_{age} = 45.0, SD = 6.53, 50.5% female, 52.0% of leaders female). Power analysis using G*Power showed that testing the model with this sample would yield 80% power to detect a small to moderate interaction effect (Cohen's f^2 = .039). 55.9% of the sample held a Bachelor's degree or higher qualification, and 52.5% of the sample earned an annual income between $25,000 and $65,000. OLS regression was run on this final sample to predict the dependent variable from the relevant predictors, controlling for the same covariates used to estimate the propensity scores (Morgan et al., 2010).

Measures

Leader Role. Leader role was measured at Times 1 and 2 using a single item, binary question indicating whether or not participants "supervise anyone on this job" (1 = yes, 0 = no). Leader role at Time 2 was used as the independent variable, indicating whether the individual became a leader at some time between Times 1 and 2. Although this measure may not necessarily include all leaders, employees in formal supervisory positions are generally considered prototypical leaders (Mintzberg, 1971).

Gender. Participants indicated whether they were female (1) or male (0).

Loneliness (Times 1 and 2). Participants responded to three items indicating the extent to which they felt "lonely", "close to others", and "like you belong" during the past 30 days. Responses were made on a five-point scale (1 = none of the time, 5 = all the time). These items were written as reverse-worded items from the established UCLA-R loneliness scale (Russell et al., 1980), which includes "I do not feel alone", "There are people I feel close to", and "I feel left out". Alphas were .74 (Time 1) and .76 (Time 2). Time 2 loneliness was the dependent variable while Time 1 loneliness was a control variable (see below). Controlling for Time 1 loneliness is particularly appropriate as there may be some dispositional component to loneliness (Cacioppo et al., 2006), in which case it is important to estimate effects of leadership on loneliness over and above an individual's existing level of loneliness (Keele & Kelly, 2006).

Control Variables. Propensity score matching requires an extensive set of control variables from Time 1 to accurately predict the propensity to become a leader at Time 2 (Morgan et al., 2010). These variables were selected for being theoretically relevant for both leader selection and loneliness. Following existing recommendations (Ho et al., 2011; Morgan et al., 2010), I used these variables to first predict propensity scores, and then as control variables in the final OLS regression predicting loneliness from the interaction of leader role and gender. The full list of 22 control variables is briefly described next.

Demographics. I controlled for *age* in years because it is suggested to relate negatively to loneliness (Barreto et al., 2020), and older individuals are more likely to occupy leader roles. *Marital status* was controlled using a dummy indicator (1 = married, 0 = not married) because marriage is associated with reduced loneliness (Pinquart, 2003) and may be disproportionately represented in leadership roles. Participants indicated their *education* on a 12-point ordinal scale (1 = no school/some grade school, 12 = Ph.D. or equivalent professional degree). Pre-tax *income*

over the last calendar year was measured on a 40-point ordinal scale (1 = less than $2,000, 40 = $200,000 or more). Education and income were included because they are related to career success (Seibert et al., 1999) and loneliness (Pinquart & Sorensen, 2001). *Hours worked per week* was measured using participants' self-reported estimate of their average weekly hours. *Industry* of work was controlled using a dummy indicator for each participant's industry. There were 10 industries in total (e.g., manufacturing, construction, finance) and thus 9 dummy indicators. Finally, I measured *ethnicity* by including two dummy indicators for whether the participant identified as White (1 = White, 0 = not White) or Black and/or African-American (1 = Black, 0 = not Black). The only other category of ethnicity represented was "Other". Minority ethnic groups tend to report greater loneliness (Visser & El Fakiri, 2016), and are also less likely to be categorized as leaders (Rosette et al., 2008).

Loneliness (Time 1). The same loneliness scale was used at Times 1 and 2.

Physical Health. Physical health may influence leader selection because leaders need to handle substantial job demands (Lovelace et al., 2007), and is also negatively related to loneliness (Hawkley & Cacioppo, 2010). I used Mechanic and Hansell's (1987) single-item measure to control for general physical health (1 = poor, 5 = excellent).

Discrimination (Times 1 and 2). I controlled for discrimination at Time 1 as it may lead to a reduced probability of becoming a leader (Rudman & Glick, 1999) as well as more loneliness (Juang & Alvarez, 2010). Participants responded to the nine-item discrimination scale from Williams et al. (1997) at both times. Sample items are "You are treated with less courtesy than other people", and "You are treated with less respect than other people" (1 = never, 4 = often; Time 1 alpha = .91, Time 2 alpha = .93). Time 2 discrimination was also used in the supplemental analysis to rule out discrimination as an alternative mediator.

Family and Friend Strain. Variance in loneliness may partly be driven by non-work sources. Outside the workplace, the two key sources of loneliness are poor relationships with friends or intimate connections such as one's spouse and children (Weiss, 1973). I thus controlled for social strain from friends and family (separate questions for each domain) using the four-item scale from Walen and Lachman (2000). A sample item is "How often do your [family/friends] make too many demands on you?" (1 = never, 4 = often). Cronbach's alpha was .79 for family and .77 for friends.

Negative Affect (Times 1 and 2). I controlled for Time 1 negative affect to ensure results were specific to loneliness and not general negative affect. Participants indicated how much they felt nervous, afraid, jittery, irritable, ashamed, and upset during the past 30 days (1 = none of the time, 5 = all the time; Time 1 alpha = .78, Time 2 alpha = .80). These six items are from the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988). Negative affect was measured in the same way at Time 2 and used as an outcome in the supplemental analysis to verify that unlike loneliness, there is no interactive effect of gender and leader role on general negative affect.

Social Support. Employees who receive more social support at work may be more likely to emerge as leaders, and feel less lonely. This was measured using five items from Karasek & Theorell (1990). A sample item is "How often do you get help and support from your coworkers?" (1 = never, 5 = all of the time; alpha = .82).

Job Demands. As job demands may affect leader selection, and employees experiencing high levels of demands also feel more lonely (Fernet et al., 2004, 2016), I controlled for job demands using the five-item scale from Grzywacz and Marks (1999). A sample item is "How often do you have too many demands made on you?" (1 = never, 5 = all of the time; alpha = .73).

Results

Descriptive statistics and correlations are presented in Table 1. Figure 2 graphically depicts the improved balance of propensity scores due to matching. Before matching, the non-leader group understandably had lower propensities to become leaders. Matching reduced the difference in mean propensity scores between leader and non-leader groups by 91.5%.

--- Insert Table 1 About Here ---

--- Insert Figure 2 About Here ---

Hypothesis 1 predicted that the effect of occupying a leader role on loneliness would be positive only for women. To test this, I conducted moderated OLS regression on the matched sample to examine the interactive effect of leader role and gender on loneliness. Because a large number of control variables was used, I show results both including and excluding control variables by adding them in the last step, as presented in Table 2 (and in all subsequent tables). The interaction between leader role and gender was significant ($B = 0.40$, $p = .025$). Decomposing the interaction by gender revealed that for women, the simple effect of occupying a leader role on loneliness was positive (Hedge's $g = .41$, simple effect = 0.26, $SE = 0.12$, $p = .034$). For men, the estimated effect was negative but not significant (Hedge's $g = -.25$, simple effect = -0.14, $SE = 0.12$, $p = .257$). The form of the interaction thus supported Hypothesis 1. Figure 3 presents the associated simple effects plot.

--- Insert Table 2 About Here ---

--- Insert Figure 3 About Here ---

Supplemental Analysis. To verify that results were robust to the choice of matching algorithm, I conducted the same analyses using three other algorithms. First, I used the same nearest neighbor algorithm but allowed matching with replacement (instead of without

replacement). This involves a bias-variance tradeoff such that bias is lower but variance higher when matching with replacement (see Caliendo & Kopeinig, 2008 for a discussion). Second, a common alternative to nearest neighbor matching is caliper matching, which avoid poor matches from the nearest neighbor method by discarding matches outside some specified caliper (recommended to be .2 standard deviations of the propensity score; Austin, 2014). I thus used caliper matching with and without replacement as well. All three algorithms provided similar estimates and significance levels for the interaction effect of leader role and gender on loneliness. Additionally, the simple effect of leader role on loneliness for women was positive and significant for all algorithms except caliper matching without replacement, for which the simple effect was marginally significant (Hedge's g = .40, simple effect = 0.23, SE = 0.12, p = .056). Overall, this provides evidence that the results are relatively robust to the matching algorithm.

I conducted several further analyses to evaluate alternative explanations. A plausible alternative outcome of inauthenticity is general negative affect instead of loneliness specifically because inauthenticity may lead to general unpleasant feelings instead of discrete negative emotions (Schmader & Sedikides, 2018). To verify that role congruity theory's predictions for leader role effects are specific to loneliness, I aimed to provide evidence against this more general affective outcome. I thus repeated the analysis using Time 2 negative affect instead of loneliness as the outcome. The leader role × gender interaction was not significant (B = .17, p = .151), which suggests the findings are specific to loneliness.

Another alternative explanation is that leader role occupancy is related to loneliness through more explicit disapproving behavior by others instead of subjectively experienced authenticity. This would represent a more extreme view of role incongruity, in that although it

predicts negative reactions to female leaders, related findings often reflect more distal forms of disapproval such as evaluations of effectiveness, wages, and promotions (Eagly & Karau, 2002). To test this possibility, I repeated the analysis using Time 2 discrimination (i.e., overt discriminatory behavior by others) as the outcome. The interaction was again not significant (B = 0.05, p = .676), ruling out discrimination as an alternative mediator. This suggests women who take on leader roles are lonelier not because of increased discrimination.

Finally, although the present research makes within-gender, cross-role comparisons (e.g., female leaders vs. female non-leaders), another way to examine the proposed interaction is to make within-role, cross-gender comparisons (e.g., female leaders vs. male leaders). This is the more common approach in role congruity research. In this case, among leaders, women did not differ from men in loneliness (simple effect = -0.09, SE = 0.14, p = .520). Among non-leaders, women were less lonely than men (simple effect = -0.30, SE = 0.13, p = .023). This highlights that female leaders are lonelier than female non-leaders, but not male leaders.

Discussion

Study 1 provides initial evidence that feeling lonely at the top is a phenomenon experienced by women who occupy leader roles (vs. non-leader roles), but not men. I found support for the hypothesized interaction between leader role and gender predicting loneliness, such that the effect of occupying a leader role was positive only for women. This effect was specific to loneliness and not general negative affect, and no similar interaction effect was found for the outcome of discrimination. Beyond significance levels, the simple effects of leader role occupancy on loneliness were of different signs for men and women. This may explain why prior studies which collapsed across gender have found null relationships between hierarchical position and loneliness (e.g., Reinking & Bell, 1991; Wright, 2012; Yip, 2015). Despite using a

matching design, Study 1 was ultimately quasi-experimental and unable to establish true causal effects of occupying leader roles. Two further limitations of the data were that individuals in the leader "condition" did not just occupy leader roles but became leaders over time, and that authenticity was not measured. Thus, to replicate Study 1's results and extend them by testing the full model, I next conducted an experimental study to determine the causal mechanism underlying why women feel lonelier when they take on leader roles.

Study 2

Sample

To obtain a similar sample size to Study 1 while allowing for attrition, 215 undergraduate students at a large public university in the Northwest were recruited to participate in this study for credit. Participants were enrolled across a variety of management classes in the business school. The study was posted as a "Group Decision Making Study" on an online subject pool website where participants voluntarily signed up for studies. I removed 9 participants who failed a comprehension check (see Procedures). The final sample thus included 206 participants (M_{age} = 20.79, SD = 2.56, 55.8% female). Power analysis indicated that this sample would yield 80% power to detect a small to moderate interaction effect (Cohen's f^2 = .038).

Procedures

The experimental task was adapted from Hewlin and colleagues (2017). The study adopted a 2 (leader or non-leader) × 2 (male or female) design. Each participant was randomly assigned to groups of three or four participants. Within each group, one participant was randomly assigned to be a leader. This resulted in 58 leaders, of which 32 were female.

At the beginning of the session, participants were seated at individual computer terminals. They read instructions indicating that the university was considering implementing a

drug testing policy, the Campus Drug and Accountability Act (CADA), and wanted to solicit students' opinions about the proposed policy. The purpose of the session was to collect feedback from groups of students regarding the extent to which they found the policy favorable. The description of this policy can be found in Appendix A. Participants then adjourned to team rooms for a group discussion. Each group comprised one leader (randomly assigned) and two or three contributors. The groups were given 20 minutes to discuss the policy. Their final objectives were to a) provide a recommendation either for or against CADA, b) provide reasons for the recommendation, and c) address potential concerns about the recommendation. This was to be written on a worksheet provided to each group.

Leader Role Manipulation. All participants were told that "the leader will direct the discussion to get a representative range of opinions from other members, but only the leader can make the final recommendation for or against CADA". This leader role thus encompassed the three roles classically described by Mintzberg (1971) – interpersonal, informational, and decisional. The other group members, referred to as "contributors", were instructed to contribute their opinions where necessary to help the leader and group come to a final recommendation. After the group discussion, all participants returned to their computer terminals to complete survey questions of dependent measures. Participants also indicated whether they played the role of the leader or contributor during the discussion. Because understanding this manipulation was critical, the 9 participants whose answers did not match their condition were removed.

Measures

Manipulation Check. I conducted a separate study to validate the manipulation. 100 participants were recruited from Prolific (M_{age} = 33.83, SD = 8.72, 38.00% female). Participants were shown the same policy and similarly randomly assigned to either the contributor or leader

role. Contributors were asked to explain whether and why they were in favor or opposed to the policy. Leaders were shown responses from three ostensibly real contributors (e.g., "I agree with this policy, it could reduce problems with drug abuse", "I don't like this idea, universities don't have the right to do this") and asked to make a final recommendation based on the group's preference. After this task, participants responded to a two-item manipulation check asking whether they felt like a leader during the task, and acted like a leader during the task (1 = strongly disagree, 5 = strongly agree; alpha = .85). Supporting the validity of the manipulation, an independent samples *t*-test showed that participants in the leader condition ($M = 3.78$, $SD = 0.83$) scored higher on the manipulation check than those in the contributor condition ($M = 2.94$, $SD = 1.13$; $t = 4.10$, $df = 98$, $p < .001$).

Gender. Gender was measured as a discrete variable with three options. Individuals indicated if they identified as female, male, or other (with the option to specify alternatives). Because no participants identified with nonbinary gender labels, I dichotomized the data (1 = female, 0 = male).

Experienced Authenticity. I adapted five items from Wood and colleagues' (2008) authenticity scale for the experimental task. Participants responded based on how they felt during the group discussion. Sample items are "I felt out of touch with the real me" and "I felt I needed to do what others expected me to do" (1 = strongly disagree, 5 = strongly agree; alpha = .72).

Loneliness. I adapted four items from the UCLA-R loneliness scale (Russell et al., 1980) for the experimental setting. To measure context-specific loneliness (Ozcelik & Barsade, 2018), the items were written in reference to the group. Sample items are "People in the group were around me but not with me" and "I felt alone in my group" (1 = strongly disagree, 5 = strongly agree; alpha = .71).

Negative Affect. To verify that findings were specific to the outcome of loneliness instead of general affect, I measured negative affect using the three-item measure from Bono et al. (2007). Participants indicated the extent to which they felt anxiety, anger, and irritation during the discussion (1 = very slightly or not at all, 5 = extremely; alpha = .80).

Communion Striving. A potential alternative explanation for the increased loneliness felt by women in leader roles is that they avoid communal interpersonal behaviors, knowing that agentic behaviors are expected in the leader role. For example, this may manifest if, in situations of disagreement, female leaders feel pressured to reach an optimal group decision at the expense of amicable consensus-creation. However, role congruity theory is agnostic as to whether women actually suppress communal behaviors after taking on leader roles. To rule out this alternative mediator, I measured communion striving during the group discussion using the four-item measure from Foulk et al. (2019). Items were adapted to refer to the group instead of coworkers. A sample item is "I focused my attention on getting along with others in the group" (1 = strongly disagree, 5 = agree; alpha = .74).

Control Variables. Similar to Study 1, I controlled for *age* and *ethnicity*. Despite the random assignment, participants may also have ended up in groups where they already knew the members, which could affect authenticity and loneliness. Participants thus reported how many *prior acquaintances* in the group they were already familiar with before the experiment. These control variables were included to reduce error variance in the dependent variables.

Analytic Method

Even though there was random assignment to groups and leader roles, it is possible that observations within groups are nested and not independent due to the common group discussion. However, there was no evidence of nesting on either authenticity (ICC1 < .01) or loneliness

(ICC1 < .05). For each variable, I conducted a likelihood ratio test to compare a fixed intercept-only model to a random intercept-only model with intercepts varying across groups. There were no significant differences for authenticity (χ^2 < 0.01, df = 1, p > .999) or loneliness (χ^2 = 0.37, df = 1, p = .543), suggesting that group membership did not explain significant variance in either outcome. I thus analyzed the data using OLS regression and estimated indirect and conditional indirect effects with bootstrapped confidence intervals (Preacher et al., 2007). I used a bias-corrected bootstrap, which provides more accurate estimates of confidence intervals (Preacher & Hayes, 2008).

Results and Discussion

Descriptive statistics and correlations are presented in Table 3. Confirmatory factor analyses (CFAs) presented in Table 4 showed that a four-factor model of the focal constructs fit acceptably and better than an alternative three-factor model where loneliness and experienced authenticity loaded on the same factor.

--- Insert Table 3 About Here ---

--- Insert Table 4 About Here ---

Regression results are presented in Tables 5 and 6. Hypothesis 1 predicted an overall moderating effect of gender on the relationship between leader role occupancy and loneliness such that the relationship would be positive, but only for women. Indeed, a significant interaction between leader role occupancy and gender was found to predict loneliness (B = 0.46, p = .012). Simple effects analysis showed a significant positive effect of leader role on loneliness for women (Hedge's g = .37, simple effect = 0.24, SE = 0.12, p = .048), and a non-significant effect for men (Hedge's g = -.38, simple effect = -0.22, SE = 0.14, p = .112). The form of the interaction thus supported Hypothesis 1; Figure 4 presents the simple effects plot. Hypothesis 2

predicted that the effect of occupying a leader role on experienced authenticity would be negative, but only for women. Again using moderated regression, results showed that the interaction of leader role and gender predicting experienced authenticity was significant (B = -0.46, p = .015). Probing the interaction revealed that the simple effect was negative and significant for women (Hedge's g = -.38, simple effect = -0.27, SE = 0.13, p = .032). For men, the estimated effect was not significant (Hedge's g = .28, simple effect = 0.19, SE = 0.14, p = .183). Hypothesis 2 was thus supported; Figure 5 presents the associated simple effects plot.

--- Insert Table 5 About Here ---

--- Insert Table 6 About Here ---

--- Insert Figure 4 About Here ---

--- Insert Figure 5 About Here ---

Hypothesis 3 predicted that experienced authenticity would be negatively related to loneliness. Controlling for the experimental and control variables, the relationship between authenticity and loneliness was negative (B = -0.56, SE = 0.06, p < .001). Hypothesis 3 was thus also supported. Finally, Hypothesis 4 suggested a mediated moderation effect (Muller et al., 2005) such that that overall moderating effect of gender on the relationship between leader role occupancy and loneliness would be mediated by experienced authenticity. I used Hayes' (2015) path analytic method to test the entire model simultaneously. The model was estimated with the lavaan package in R (Rosseel, 2012). 10,000 bootstrapped resamples were used to estimate the difference between men and women in the conditional indirect effects of leader role on loneliness via authenticity. For women, the indirect effect was positive (indirect effect = 0.13, 95% CI = [0.009, 0.278]). For men, the indirect effect was negative but the 95% CI included zero (indirect effect = -0.10, 95% CI = [-0.263, 0.050]). Additionally, the 95% CI for the difference in

indirect effects between women and men was above zero (difference = 0.24, 95% CI = [0.039, 0.464]), suggesting that the effect of leader role on loneliness through authenticity was indeed moderated by gender, such that the indirect effect was positive only for women. This supported Hypothesis 4.

Supplemental Analysis. To rule out communion striving as a possible alternative mediator, I tested for the interactive effect of leader role and gender on communion striving instead of authenticity while controlling for the same covariates. The interaction was not significant (B = -0.01, SE = 0.20, p = .950), suggesting that women and men do not actually enact different communal behaviors when occupying leader roles compared to non-leader roles. To show again that the effect was specific to loneliness, I tested for the interactive effect of leader role and gender on negative affect as well. Again, the interaction was not significant (B = 0.16, SE = 0.16, p = .326), which suggests that occupying a leader role does not have differential effects on general negative affect for men and women.

Like Study 1, I further conducted within-role, cross-gender comparisons as an alternative decomposition of the interaction effects. Among leaders, women experienced less authenticity than men (simple effect = -0.37, SE = 0.16, p = .021), but did not differ from men in loneliness (simple effect = 0.23, SE = 0.15, p = .147). Among non-leaders, women did not differ from men in experienced authenticity (simple effect = 0.09, SE = 0.10, p =.364), but were less lonely than men (simple effect = -0.24, SE = 0.10, p = .016).

Discussion

Study 2 replicated the overall moderating role of gender on the effect of occupying a leader role on loneliness. Since random assignment was possible in Study 2, I further delineated the causal mechanism underlying this effect - women subjectively experience less authenticity

when they assume leader roles, but men do not. Tests of mediated moderation showed a positive indirect effect of leader role on loneliness via experienced authenticity, but only for women. The difference in indirect effects between women and men was also significant. By ruling out differential effects of the leader role on communion striving and negative affect between women and men, this study highlights that the mechanisms producing loneliness are inherently due to subjective experiences instead of overt behaviors, and that the discrete emotion of loneliness is the key outcome instead of general affective experiences. This further emphasizes the importance of considering leaders' own experiences of enacting leadership.

Study 2 is not without limitations however. Although random assignment to roles allowed for causal inference about the effect of leader role occupancy on experienced authenticity, the relationship between authenticity and loneliness may have been affected by common method variance as both measures were self-reported in the same session. The generalizability of the results is also questionable as leader role effects found in a temporary laboratory setting may differ from those experienced by full-time employees. This is a concern particularly for experienced authenticity, which was not measured in Study 1. I thus conducted a third study to replicate all findings in a more externally valid setting.

Study 3

Sample and Procedure

To obtain a sample at least as large as Studies 1 and 2 while allowing for attrition across two time points, 400 participants were initially recruited from Prolific Academic, an online subject pool specializing in academic research which produces data quality on par with or better than alternative online platforms (Peer et al., 2017). Participants were full-time employees in the United States. Upon signing up for the study, participants completed a survey with measures of

leader role occupancy, experienced authenticity, and control variables. One week later, participants were sent a follow-up survey containing measures of loneliness and demographics. Authenticity and loneliness were time-separated to reduce concerns of common method bias (Podsakoff et al., 2003), which was a potential limitation of Study 2. However, because leader role occupancy is a relatively objective structural construct, and because Study 2's experimental design mitigated common method bias concerns between leader role occupancy and authenticity, the latter two measures were not time-separated. After accounting for attrition between the first and second surveys, a final complete sample of 269 was obtained (M_{age} = 33.23, *SD* = 9.13, 41.64% female). This was larger than the previous two samples; power analysis indicated that this sample would yield 80% power to detect a small to moderate interaction effect (Cohen's f^2 = .030).

Measures

Leader Role Occupancy. Similar to Study 1, participants indicated whether they supervise anyone at their job (1 = yes, 0 = no).

Gender. Similar to Study 2, gender was measured with three options (female, male, other). However, I dichotomized the data again (1 = female, 0 = male) because no participants identified with nonbinary gender labels.

Experienced Authenticity. The same five-item scale as Study 2 was used with items adapted to refer to the participants' workplaces (e.g., "At work, I feel out of touch with the real me"; alpha = .72).

Loneliness. The same four-item scale as Study 2 was used, again with items adapted to refer to the participants' workplaces (e.g., "People in this organization are around me but not with me"; alpha = .80).

Control Variables. Because loneliness can be both dispositional and domain-specific (Heinrich & Gullone, 2006), it is important to control for individuals' dispositional loneliness before examining the effect of leader role occupancy on loneliness in the work domain. I controlled for *dispositional loneliness* using the 20-item UCLA-R scale (Russell et al., 1980). Sample items are "I feel isolated from others" and "I lack companionship" (1 = strongly disagree, 5 = strongly agree; alpha = .94). Similar to Study 1, I also controlled for trait *negative affectivity* affect to ensure results were specific to loneliness and not general negative affect, and that negative affect did not account for any association between inauthenticity and loneliness. All 10 items from the PANAS (Watson et al., 1988) were used (alpha = .91). *Social support* (alpha = .85) and *physical health* (single item) were controlled using the same measures as Study 1. Finally, I controlled for the demographics of *age* and *ethnicity*, similar to Studies 1 and 2. Ethnicity was again measured using dummy indicators for each ethnicity while an "Other" category was used as a reference (see Table 7 for details).

Analytic Method

I used OLS regression to test Hypotheses 1-3. To test for mediated moderation in Hypothesis 4, I used Hayes' (2015) path analytic method to estimate all coefficients simultaneously. Indirect and conditional indirect effects were estimated with 10,000 bias-corrected bootstrapped confidence intervals (Preacher & Hayes, 2008; Preacher et al., 2007). These analyses were conducted with the lavaan package in R (Rosseel, 2012).

Results and Discussion

Descriptive statistics and correlations are presented in Table 7. Similar to Study 2, a set of CFAs showed that the hypothesized four-factor model displayed significantly better fit than an

alternative three-factor model where authenticity and loneliness items loaded on the same factor (see Table 4).

--- Insert Table 7 About Here ---

Moderated regression results are presented in Tables 8 and 9. Hypothesis 1 predicted that gender would moderate the relationship between leader role occupancy and loneliness such that the relationship would be positive only for women. Moderated regression revealed a marginally significant interaction between leader role occupancy and gender on loneliness (B = 0.28, p = .078). Decomposing the interaction by gender, the simple effect of leader role occupancy on loneliness was positive and significant for women (Hedge's g = .22, simple effect = 0.26, SE = 0.12, p = .035). The effect was negative but not significant for men (Hedge's g = -.21, simple effect = -0.02, SE = 0.10, p = .854). The form of the interaction thus provided marginal support for Hypothesis 1; the associated simple effects are presented in Figure 6.

--- Insert Table 8 About Here ---

--- Insert Table 9 About Here ---

--- Insert Figure 6 About Here ---

Hypothesis 2 predicted that the effect of occupying a leader role on experienced authenticity would be negative only for women. Indeed, a significant interaction between leader role occupancy and gender was found to predict authenticity (B = -0.32, p = .024). For women, the simple effect of leader role occupancy on authenticity was negative (Hedge's g = -.32, simple effect = -0.29, SE = 0.11, p = .008). For men however, there was no significant simple effect (Hedge's g = .20, simple effect = 0.03, SE = 0.09, p = .782). These effects are shown in Figure 7. Hypothesis 2 was thus supported.

--- Insert Figure 7 About Here ---

Hypothesis 3 predicted a negative relationship between experienced authenticity and loneliness. Results showed that this relationship was indeed negative (B = -0.40, SE = 0.07, p < .001), thus supporting Hypothesis 3. Finally, Hypothesis 4 predicted that the moderating effect of gender on the relationship between leader role occupancy and loneliness would be mediated by experienced authenticity. Path analysis revealed that for women, the indirect effect of leader role occupancy on loneliness via experienced authenticity was positive (indirect effect = 0.12, 95% CI = [0.021, 0.247]). For men, the indirect effect was not significantly different from zero (indirect effect = -0.01, 95% CI = [-0.086, 0.057]). Further, the difference between the indirect effects was positive (difference = 0.13, 95% CI = [0.019, 0.279]). Thus, Hypothesis 4 was supported.

Supplemental Analysis. Following Studies 1 and 2, I probed the interaction effects by alternatively examining gender differences within leader roles and non-leader roles. Among leaders, women were lonelier than men (simple effect = 0.26, SE = 0.12, p = .034), and experienced less authenticity than men (simple effect = 0.24, SE = 0.11, p = .024). Among non-leaders, women and men did not differ significantly in loneliness (simple effect = -0.02, SE = 0.11, p = .827) and authenticity (simple effect = 0.07, SE = 0.109, p = .421). Thus, across the studies, cross-gender comparisons within roles produced variable findings for loneliness and more consistent findings for authenticity. However, cross-role, within-gender comparisons consistent findings for both outcomes. This further highlights the difficulties associated with comparing male and female leaders (or non-leaders) on their subjective experiences when baseline gender differences in these experiences are unclear.

Discussion

Study 3 replicated the results of Studies 1 and 2 using a more externally valid survey design. The time separation between the measures of experienced authenticity and loneliness also addressed a weakness of Study 2 that both constructs were measured at the same time, which could have contributed to common method variance. Time-separation was used instead of alternative methods such as obtaining data from different sources because experienced authenticity and loneliness are subjective psychological constructs and third parties are unlikely to be able to rate them accurately. The findings suggest that the hypothesized gender-contingent effects of leader role occupancy on both authenticity and loneliness generalize beyond a laboratory setting to working individuals.

General Discussion

This research contributes to understanding of how leaders themselves experience leadership. While the link between leadership and valued resources such as power and status has led to leader roles being depicted as involving mostly positive experiences, I draw from role congruity theory to highlight that that loneliness is an important, gender-contingent outcome of leader role occupancy. Across one archival, one experimental, and one longitudinal survey study, I found that for women, occupying a leader role was associated with greater loneliness. For men however, there was no effect of leader role occupancy on loneliness. Results also show that this is because women subjectively experience a reduced sense of authenticity when they assume leader roles. The hypothesized overall mediated moderation model was thus supported, whereby gender moderates the effect of leader role occupancy on loneliness, and this moderating effect is mediated by experienced authenticity. As a result, this study adopts role congruity theory to explain why men and women differentially feel “lonely at the top”.

The results also rule out two alternative explanations for this phenomenon. First, from a role congruity perspective, one may argue that occupying a leader role leads to generally negative affective experiences for women. In this case, effects on loneliness would simply be indicative of broader effects on negative affect. However, neither Study 1 nor 2 find this result. Study 3 also accounts for this alternative explanation by controlling for negative affectivity. This highlights the importance and utility of theorizing about specific discrete emotions instead of general affect. Second, it is possible that women who take on leader roles suppress their communal behavior with others because communality may conflict with expectations of leadership (Eagly & Karau, 2002). To the extent that communality facilitates relationship satisfaction (Helgeson, 1994), suppressing it should induce more loneliness. However, the experimental design in Study 2 revealed no interactive effect of gender and leader role on communion striving. Taken together, these results suggest that differential effects of leader role occupancy on loneliness for men and women are due to the subjective experiences associated with the leader role, instead of general social exclusion or the suppression of communal behavior.

Theoretical Implications

The key outcomes of interest in the leadership literature have conventionally been categorized in terms of leader emergence or leader effectiveness (Foti & Hauenstein, 2007; Zaccaro, 2007). These outcomes represent effects of leadership on external loci such as followers or the context (Eberly et al., 2013). More recent research has begun to examine consequences of leader behaviors for leaders themselves (e.g., Lanaj et al., 2016; Lin et al., 2019). I build on and enrich this perspective by showing that occupying a leader role is associated with the specific discrete emotion of loneliness. In particular, taking on a leader role carries asymmetric affective

costs for men and women – women feel lonelier when they take on these roles, but men do not. More generally, the current approach highlights the importance of comparisons between leaders and non-leaders, instead of the more common examinations of effects of leadership in samples comprising only leaders.

This is particularly important from a role congruity perspective. Role congruity theory is often used to predict observers' differential perceptions of male leaders compared to female leaders. But such comparisons can be misleading when focal actors' subjective experiences (e.g., loneliness) are of interest, because they disregard potential baseline differences between men and women. For example, if there is a positive leader role effect on loneliness for women, but men are generally lonelier than women to a similar degree in the first place, then it is possible that male and female leaders do not differ in loneliness. In reality, the literature on baseline gender differences in loneliness is inconclusive. Women are theorized to be less lonely because they spend more time with important relationship partners (Koenig & Abrams, 1999) and have higher quality relationships (Borys & Perlman, 1985). But empirical evidence has found slightly less loneliness in women as well as no gender differences in loneliness (Borys & Perlman, 1985; Cramer & Neyedley, 1998; Koenig et al., 1994; Pinquart, 2003). The same is true for authenticity. Research suggests women engage in authentic interpersonal communication and emotion expression more than men (Lopez & Rice, 2006; Neff & Suizzo, 2006). Yet widespread and subjectively perceived discrimination by sex also devalues the female gender role (Dambrun, 2007; Schmitt et al., 2014), which may limit experiences of authenticity. Without clear evidence for such gender differences, it is difficult to predict or interpret loneliness and authenticity differences between male and female leaders. This lends to the within-gender, cross-role comparisons this research examines.

The affective perspective also provides an alternative lens to examine the underrepresentation of women in leader roles (Cotter et al., 2001; Glass & Cook, 2016). Existing role- and network-based perspectives have revealed important reasons why third parties are biased in favor of male leaders compared to female leaders (Eagly & Karau, 2002; Ibarra, 1993). This explains why women may be underrepresented for reasons external to them. Complementing these perspectives, I show one reason why men and women may have different inclinations to take on leadership in the first place. That is, the behavioral constraints experienced by female leaders lead to more subjective relationship deficiencies after they take on leader roles, and this is not experienced by men.

This study further contributes to theory on the antecedents of loneliness at work, a domain of study which to date has focused on the harmful consequences of loneliness (e.g., Ozcelik & Barsade, 2018; Peng et al., 2017; see Waytz et al., 2015 for an exception). Specifically, I show that structural positions like the leader role can have important effects on loneliness, though these effects are likely mediated by subjective appraisals or experiences. I also suggest that the inherent subjectivity of loneliness makes it a complex phenomenon because it is not always easy to ascertain why individuals appraise their relationships as being deficient. In this light, it is important to consider relevant moderators that explain why organizational factors differentially affect relationship appraisals across individuals. Within the current study, I highlight that gender is one such moderator for leader role effects, in line with role congruity theory's suggestion that the leader roles and gender roles are not equally compatible for men and women.

Finally, this research contributes to an emerging perspective that examines gender as a moderator instead of main effect on individuals' experiences in organizations. In recent

examples, gender moderated the relationship with between ethical leadership and citizenship behavior (Kacmar et al., 2011), and the relationship between status striving and bridge employment (Zhan et al., 2015). I highlight that such explorations are especially relevant when the outcomes of interest are subjective experiences. For example, across Studies 2 and 3, female leaders experienced less authenticity than male leaders. However, across all three studies, female leaders were not consistently lonelier than male leaders. Consistent results were only found for gender as a moderator of the effect of leader role occupancy on these outcomes. Thus, when underlying gender differences in subjective focal constructs are unclear, it may be useful to consider gender as a moderator of effects on those constructs instead.

Limitations and Future Directions

Although this research makes several valuable contributions, I also acknowledge its limitations. In terms of study design, Study 1 consisted entirely of observational self-report measures, and there is potential for common method bias from such data (Podsakoff et al., 2003). This may have been somewhat mitigated by the relatively objective measures of participants' gender and supervisory roles, compared to more common psychosocial measures. Further, although propensity score matching strengthens causal inference, it is not a replacement for true experimental design with random assignment. Study 2 was conducted to address these internal validity concerns. But while the random assignment to groups and leader roles allows for stronger causal inference, the social processes involved in experimental teams may not fully generalize to the experiences of employees because short-term leader roles may be experienced differently from longer-term positions in organizations. The mediator and dependent variable were also measured at the same time, which could also produce common method bias. Thus, Study 3 was conducted to replicate the results in a more externally valid setting, and mitigate

concerns of common method bias between experienced authenticity and loneliness by time-separating their measurement. The observational nature of Study 3 does limit the internal validity of the findings. However, the three studies in combination were designed to compensate for each other's specific limitations of internal and external validity.

This research also highlights possibilities for future study. The authenticity-loneliness relationship may be particularly noteworthy because loneliness and authenticity have been described as originating from different sources. Whereas loneliness often results from feeling alienated from others (Heinrich & Gullone, 2006), experiencing inauthenticity results from feeling alienated from oneself (Sloan, 2007). This suggests the mechanism linking authenticity to loneliness may lie in more complex microprocesses related to self-identity and construal (Brewer & Gardner, 1996), which can produce changes how social information is appraised (Suh et al., 2008) as well as actual social behavior (Cross et al., 2000; van Baaren et al., 2003).

Additionally, individuals with nonbinary gender identities were not represented in the data. There was no option to self-report as such in Study 1, and while this option was present in Studies 2 and 3, no participants selected it. Thus, future research may examine whether results can be generalized to such individuals, who are increasingly recognized as some of the groups against which much discrimination occurs (Richards et al., 2016). Even for women, there may be important boundary conditions on leader role effects because leader roles do not always conflict with the female gender role. For example, female leaders may be evaluated more favorably in top management positions (Rosette & Tost, 2010). Thus, while the leader roles of interest here still exist at lower levels of hierarchy and remain male-dominated (Eagly, 2007), certain contextual factors may reduce role incongruity for women as well as their experiences of inauthenticity and loneliness when taking on leader roles. Context effects may further arise by

reducing discrimination (e.g., at higher levels of hierarchy), or because different types of discrimination occur in different contexts or hierarchical levels (Rosette & Tost, 2010).

Finally, the independent variables in both studies captured individuals formally assigned to leader roles because they tend to be prototypical leaders in organizations. However, leadership may also occur in the absence of a designated formal, hierarchical position (e.g., shared leadership, Carson et al., 2007; identity-based leadership, DeRue & Ashford, 2010). It would be useful to examine whether the effects found in this research generalize to settings where leader roles are informally instead of formally designated, and if they do not, whether the difference between the two situations lies in different prescriptive expectations contained in the respective leader roles. Even within the context of formal leader roles, there is the open question of how the effects of leader role occupancy may change over time. Study 1 explored this to some extent by virtue of the six-year separation between the two waves of data. But only loneliness, not authenticity, was included in Study 1. There may thus be room to further investigate leader role effects over time for authenticity in particular.

Practical Implications and Conclusion

The findings suggest that organizations should consider the affective consequences of leadership for individuals themselves. It is certainly important to continue championing diverse representation in leader roles. At the same time, it is also worthwhile to consider why taking on these roles may take a toll on individuals. The results indicate a key problem is that for women, becoming a leader leads to greater subjectively experienced behavioral constraints, and these constraints arise from conflicting role expectations. In this case, organizations may clarify expectations so that such conflict is less likely to be experienced. Organizations can also monitor new leaders for signs of adjustment and provide support to those who have difficulty doing so.

At an organizational level, it may also be helpful to cultivate "looser" cultures (Gelfand, 2012), where norms are weaker and experiences of conflicting norms are thus less likely to be harmful.

In conclusion, this research shows that it is lonely at the top after all, but only for women. Across two studies, I find that taking on a leader role increases feelings of loneliness in women, but not in men. At a time when there is strong and admirable advocacy for increased representation of women at all levels of hierarchy, this highlights the importance of simultaneously considering how leaders themselves feel, and hopefully in the process help improve their well-being.

CHAPTER 2: HOW LEADERS' PAST MISCONDUCT AFFECTS THEIR DISPLAYS OF LENIENCY

Abstract

Leaders' leniency decisions towards their subordinates' misconduct carry strong ethical implications and are known to be driven by immediate situational characteristics. However, less is known about how these decisions fit into leaders' ongoing moral self-regulatory processes based on their past behavior. This article presents two potentially competing explanations for how leaders' own past misconduct affects their leniency decisions. Hypocrisy avoidance may lead to previously deviant leaders showing more leniency, but moral compensation may lead to them showing less leniency. I suggest social consensus determines when hypocrisy avoidance or moral compensation occurs. Across one vignette-based experiment, one critical incident experiment, and one experiment using behavioral ethics paradigms, results suggest moral compensation may be more likely when social consensus is high, while hypocrisy avoidance does not depend on social consensus. The findings contribute to theory on leaders' moral self-regulation in the historical context of their past behavior.

Employee misconduct has serious consequences for organizational performance. Employee theft alone costs businesses $50 billion a year (Pofeldt, 2017). As a result, disciplining employees who engage in misconduct is a key responsibility for leaders in organizations. Leaders often have considerable discretion in determining the consequences employees face for misconduct. Their positions of power allow them to punish wrongdoing harshly (Mooijman et al., 2015), which in extreme cases may lead to enacting unjust punishment (Mooijman & Graham, 2018). However, such severe action is difficult because it also produces strain for leaders (Butterfield et al., 1996). Alternatively, leaders may show leniency, which refers to lessening the consequences of misconduct (Zipay et al., 2020). The increasing reports of widespread lenient treatment for police misconduct are infamous contemporary examples (Harris & Worden, 2014).

Extant research has explored various reasons why leaders may be more or less lenient to a subordinate who has engaged in misconduct. Perhaps the most important consideration is the nature of the misconduct itself. Individuals are strongly motivated by just deserts – that punishment should be proportionate to wrongdoing (Carlsmith et al., 2002). Accordingly, less leniency should be shown when misconduct is more severe. This fulfills desires for retribution (Carlsmith, 2006) and deters serious offenses with the prospect of severe consequences (Warr et al., 1983). At the same time, leaders' decisions are also driven by characteristics of the offender that signal the extent to which they deserve a prescribed punishment. A common finding is that high-performing subordinates receive more lenient treatment (Bellizzi & Hasty, 2003; Podsakoff, 1982), potentially because their performance earns them the right to a reprieve (Butterfield et al., 1996). Other extenuating circumstances that push leaders towards leniency are also taken into account. For example, subordinates who express remorse for their actions signal a low

probability of reoffending (Eisenberg et al., 1998; Gold & Weiner, 2000). When subordinates' reasons for misconduct are externally instead of internally attributed, leaders are also less likely to hold them responsible and will thus show more leniency (Butterfield et al., 1996; Fragale et al., 2009).

These key insights have revealed how leniency (or the lack thereof) is driven by the immediate characteristics around an instance of misconduct, whether they pertain to the behavior itself or the perpetrator. Yet this neglects that leaders' decisions are likely also influenced by events in the past. Disciplinary decisions are ethically charged (Trevino & Ball, 1992), and correspondingly fit into a leader's ongoing temporal trajectory of other ethically relevant behaviors. This is important because individuals regulate their ethical behavior in relation to their past behavior (Monin & Jordan, 2009; Zhong et al., 2009). Two mechanisms in particular may explain how leaders' disciplinary decisions are influenced by their past behavior. On one hand, theories of hypocrisy suggest individuals are motivated to avoid hypocritical behavior (Aronson et al., 1991; Stone et al., 1997). This suggests that, to avoid hypocrisy, leaders who have engaged in misconduct in the past may feel compelled to be lenient to subordinates who also engage in misconduct. On the other hand, moral self-regulation theory suggests individuals compensate for past unethical behavior by behaving more ethically in the future (Zhong et al., 2009). Because showing leniency may be interpreted as unjust behavior (Trevino & Ball, 1992), this suggests leaders who have engaged in misconduct in the past would in fact avoid showing leniency. These two mechanisms thus have opposite implications for whether leaders' past misconduct facilitates or deters leniency, and it is unclear which of them will take precedence in a given situation. Leaders' disciplinary decisions have important consequences for broader group-level ethical outcomes (Mayer et al., 2009). Given the discretion they have in these decisions, it is important

to understand how their own past, underappreciated because it is often known only to leaders themselves, affects how they wield their discretion.

The purpose of this research is thus to integrate these theoretical perspectives by examining why leaders may alternately be motivated by hypocrisy avoidance or moral compensation in their disciplinary decisions. I suggest that the mechanism at play depends on the social consensus around the unethicality of a misconduct event (Jones, 1991). When there is high social consensus that the misconduct is unethical, leaders are likely to compensate for their own past misconduct by avoiding leniency. However, when there is low social consensus, leaders are able to avoid hypocritical behavior by showing leniency. This research makes several theoretical contributions. First, I integrate potentially conflicting theoretical perspectives on how leaders' past misconduct affects their use of discipline in response to subordinates' misconduct. While a hypocrisy lens suggests historically errant leaders may be compelled to be lenient, a moral compensation lens suggests these leaders may avoid leniency to compensate for their past behavior. This research aims to demonstrate which of these mechanisms occur, depending on how much agreement there is around the unethicality of the subordinate's misconduct. Second, although the punishment literature has explained how the immediate characteristics of a misconduct event, its perpetrator, and the organizational context affect disciplinary decisions (Arvey & Jones, 1985; Butterfield et al., 1996; Carlsmith et al., 2002), I shift the focus temporally towards events that have occurred in the decision-maker's own past. This highlights how events that seem independent and temporally distant from a given disciplinary incident may still affect the attendant decision. Finally, an emerging literature suggests that ethical judgments in organizations are much less clear than previously thought (Warren & Smith-Crowe, 2008).

Extending this perspective, I show that variation in social consensus has important implications for how leaders regulate their disciplinary behavior in relation to their past behavior.

Theory and Hypotheses

Ethical Implications of Leniency

Organizations implement a variety of control systems to manage employee misconduct. Behaviors such as dishonesty, insubordination, and miscellaneous rule violations historically comprise some of the most common types of misconduct, and are highly detrimental to organizational functioning (Wheeler, 1976). As a result, when misconduct does occur, leaders across all levels are responsible for making specific disciplinary decisions to stop and deter future misconduct (Treviño et al., 2000). Leaders often have considerable discretion over the actual disciplinary action they take, and consider a variety of characteristics of the transgressor and transgression before making decisions (Arvey & Jones, 1985).

Dismissing an employee is perhaps the most severe possible action in many organizations, and is correspondingly used only in rare, serious cases of misconduct (O'Reilly & Weitz, 1980). Such decisions are only undertaken after complex considerations involving organizational, social, and personal costs (Laulié et al., 2020). Indeed, across all sorts of punishments, leaders experience overwhelmingly negative emotions when they administer punishment (e.g., frustration, embarrassment, guilt; Butterfield et al., 1996). Leaders are also cognizant of and concerned about subordinates' specific circumstances, including the harmful consequences that punishment can have on their well-being (Butterfield et al., 1996, 2005). Leaders can thus display leniency by using their discretion to reduce the negative consequences for misconduct (Zipay et al., 2020). Such behavior can help transgressors internalize desired behavioral norms (Bertels et al., 2014) and repair relationships with transgressors (Aquino et al.,

2003). Leniency can also have beneficial effects for leaders, who are likely to feel more pride and engagement as a result (Zipay et al., 2020). Further, although leniency is clearly related to punishment, it is conceptually distinct from a low level of punishment, which may not be lenient if it is considered appropriate for a transgression. That is, leniency occurs specifically when a decision-maker punishes less than what is expected or possible given the misconduct and organizational norms surrounding it (Zipay et al., 2020). For example, in 2009, the Securities and Exchange Commission brought insider trading charges against several executives involved with the Galleon Group. One of these executives, Anil Kumar, received a reduced fine and no jail time because of his immediate cooperation with attorneys. His co-conspirators received full sentences of varying fines and jail time commensurate with their crimes (Hurtado, 2012). This reduction of the full extent of punishment for Anil Kumar thus characterizes leniency.

Underlying the diverse considerations and consequences around a leniency decision is its ethical nature. When misconduct has occurred, individuals evaluate how it is dealt with in terms of justice (Trevino & Ball, 1992). The primary concerns are often distributive and retributive justice, such that a perpetrator is believed to deserve a punishment that "fits the crime" (Carlsmith et al., 2002; Hogan & Emler, 1981). Showing leniency would be viewed unfavorably as it trivializes valued standards of behavior (Trevino, 1992). Procedural justice is also important as individuals think about the consistency of procedures used to come to final disciplinary decisions (Tyler, 2006). Further, the equity of such decisions is considered in terms of perpetrators being treated similarly to others who have also engaged in misconduct (Adams, 1965; Arvey & Jones, 1985). A general finding, however, is that less lenient decisions are perceived as more fair (Trevino & Ball, 1992). Leaders themselves are highly aware of these fairness-related implications of disciplinary decisions. They consider a perpetrator's specific

circumstances to determine appropriate punishments, and are careful not to ignore prescribed procedures for making judgments (Butterfield et al., 1996).

These justice-related concerns have primarily manifested in considerations of the immediate circumstances surrounding a misconduct event and its perpetrator. However, because leniency decisions are inherently ethical, they cannot be divorced from the decision-maker's own history of ethically relevant behaviors. This is the premise of moral self-regulation theory, which seeks to explain how individuals regulate their moral behaviors over time (Mullen & Monin, 2016; Zhong et al., 2009). Importantly, leaders not only manage others who engage in misconduct, but are organizational members who may also have engaged in misconduct themselves. This presents potentially competing theoretical implications for how leaders regulate their disciplinary behavior based on their own past behavior. On one hand, from the perspective of moral compensation (Zhong et al., 2009), leaders who have engaged in misconduct may reaffirm their moral selves by enacting moral behaviors. Because showing leniency may be considered unjust behavior, this view suggests leaders who have engaged in more misconduct in the past would avoid showing leniency when disciplining a transgressing subordinate. At the same time, avoiding leniency may be considered hypocritical. A leader who has engaged in misconduct in the past would experience psychological discomfort from punishing a subordinate for also engaging in misconduct (Effron & Miller, 2015; Stone et al., 1997). Because individuals generally undertake behaviors to directly reduce or avoid such hypocrisy (Aronson et al., 1991; Stone et al., 1997), this view alternately suggests leaders who have engaged in misconduct would prefer to show leniency and thus avoid the discomfort of behaving hypocritically. However, it is not immediately clear when a moral compensation or hypocrisy avoidance mechanism will take precedence in a given disciplinary situation.

Varying Social Consensus Around Misconduct

To integrate these two theoretical perspectives, I suggest that both moral compensation and hypocrisy avoidance occur together, but that hypocrisy avoidance is only possible when the social consensus around a misconduct event does not constrain their disciplinary decisions. Moral compensation occurs because individuals enact behaviors to reaffirm their own core values (Tetlock et al., 2000). Indeed, across different individuals and contexts, a wide variety of behaviors has been found to achieve compensatory effects (Zhong et al., 2009). The implicit premise is that for a behavior to be compensatory, the individual must perceive that it is ethical. This is a key assumption and it is important to understand why individuals may perceive a behavior to be more or less ethical – ethical judgments in organizations are often ambiguous because they require complex consideration of myriad situational factors (Warren & Smith-Crowe, 2008). This is consistent with prevailing models of ethical decision-making where ethical judgments are driven by multiple aspects of an ethical issue (Jones, 1991; Rest, 1986), while also being affected by the decision-maker's characteristics such as cognitive moral development (Kohlberg, 1971) and emotions (Haidt, 2001). Indicative of this is the fact that leaders feel compelled to consider unique individual circumstances surrounding a misconduct event; it is rarely immediately clear what the best course of action might be (Butterfield et al., 1996).

This line of reasoning suggests that moral compensation can only occur insofar as there is a reasonable level of social consensus around the unethicality of misconduct. Social consensus refers to "the degree of social agreement that a proposed act is evil (or good)" (Jones, 1991; p. 375), and is one of six dimensions of moral intensity – the underlying characteristics of an issue that affect ethical decision-making (Jones, 1991). It is one of the most important (Frey, 2000; Yam et al., 2014) and is also the only normative, as opposed to descriptive, dimension of moral

intensity (Weaver & Trevino, 1994). Though importantly, it is not orthogonal to the other dimensions, but all six dimensions tend to be positively correlated (McMahon & Harvey, 2006). Misconduct may vary in social consensus such that when social consensus is high, there is widespread agreement that a behavior should judged as unethical; when social consensus is low, there is little such agreement (Reynolds & Ceranic, 2007). In an organizational context for example, there is likely high social consensus that assaulting a coworker is wrong, but much lower social consensus about opening mail when the intended recipient cannot be ascertained (Warren & Smith-Crowe, 2008). The construct is thus highly relevant to real-world contexts, and has correspondingly been measured with surveys (e.g., McMahon & Harvey, 2006) as well as experimentally manipulated in vignettes (e.g., Yam et al., 2014).

Social consensus is important in a leadership context because leaders take the ethical perceptions of the general work group into account when making disciplinary decisions. Indeed, their decisions also affect the emotions and attitudes of their work groups (Trevino, 1992). Leaders are thus likely to be aware of how much social consensus there is in their contexts that a particular misconduct event is unethical. When social consensus is high, it is easy to determine the right course of action without individual judgment (Reynolds & Ceranic, 2007). In particularly, high social consensus is likely associated with faces strong expectations to administer punishment to deter other transgressions, and prevent negative emotions (Butterfield et al., 1996). A leader facing such a situation thus has clear motives to fulfill the responsibility of punishment. A baseline expectation is thus that misconduct that is high in social consensus receives less leniency. Such a finding would validate the assumption that showing less leniency is viewed as more ethical or just (Trevino & Ball, 1992).

Hypothesis 1: Social consensus is negatively related to leniency.

Beyond the potential main effect of social consensus, the first effect of interest is how it may also facilitate the first key mechanism – moral compensation. Because misconduct high in social consensus is accompanied by strong expectations of punishment, failure to do so may be considered unfair behavior by the leader (Schaubroeck et al., 2012), while harsh punishment is considered just (Trevino & Ball, 1992). Compensation can thus occur such that leaders who have engaged in more misconduct in the past can compensate by avoiding leniency. If there is low social consensus around the misconduct however, there are likely very different opinions about the right way to enforce discipline. It then becomes is unclear to what extent administering punishment allows leaders to reaffirm their morality. They may believe a transgression deserves minimal punishment while still being responsible to the organization to enforce standards of behavior. They may also be caught in a mercy-justice dilemma because both showing leniency and avoiding it may be considered moral (i.e., being either merciful or just; Kidder, 1996; Wang & Murnighan, 2017) When there is no single decision that has the clear best moral implications, it is likely difficult for the leader to use the decision to compensate for their own past misconduct. In summary, a moral compensation mechanism is likely to occur only if there is relatively high social consensus around the unethicality of the subordinate's misconduct. This motivates the leader to compensate for their own past misconduct by showing less leniency.

Unlike moral compensation, I suggest that hypocrisy avoidance is unlikely to occur when there is high social consensus around misconduct. The act of punishing is itself a very clear signal of disapproval by the leader, and will similarly be clearly hypocritical and evaluated negatively if the leader has also engaged in misconduct in the past (Effron & Monin, 2010). Such unambiguous hypocrisy leads to cognitive dissonance and negative emotions (Aronson, 1999; Batson et al., 1997), which suggests individuals generally strive to avoid it. Yet this may not be

possible when social consensus around the misconduct is high. In such situations, there is little ambiguity about the extent to which the misconduct deserves punishment (Effron & Monin, 2010), which effectively characterizes a strong situation (Meyer et al., 2010; Mischel, 1977) with explicit cues for appropriate behavior (i.e., punishment). Disciplinary decisions are thus likely to revolve around the more conventionally considered immediate characteristics of the misconduct event and leaders' personal histories should be of limited relevance. The strong expectations to punish unethical behavior (Butterfield et al., 1996) are thus constraints that likely outweigh any potential, personal discomfort of hypocrisy for leader themselves (though not necessarily precluding a moral compensation mechanism, as argued above). This is consistent with findings that in situations of high social consensus, individuals' personal moral judgments have minimal influence on their behavior (Reynolds & Ceranic, 2007). Yet these constraints should be less salient when there is low social consensus regarding the misconduct, in which case there likely no strong expectations for specific punishment behavior. It is thus possible for leaders to consider the discomfort of behaving hypocritically and adjust their decisions accordingly, which implies being less explicitly disapproving of the misconduct. This can be achieved by showing leniency to the transgressing subordinate. In other words, leaders who have engaged in misconduct in the past can avoid hypocrisy by showing leniency only when there is low social consensus that the misconduct is unethical.

> *Hypothesis 2: The relationship between a leader's past misconduct and leniency shown to a subordinate is moderated by the social consensus of the subordinate's behavior, such that the relationship is negative when social consensus is high, and positive when social consensus is low.*

Losing Moral Credits from Past Misconduct

To further delineate moral compensation from hypocrisy avoidance, I distinguish their respective explanatory mechanisms. Individuals value moral traits as part of their self-concepts (Aquino & Reed, 2002). Moral compensation suggests that moral (immoral) deeds trigger increases (decreases) in moral credits (Liao et al., 2018; Miller & Effron, 2010). An individual's past misconduct thus causes a loss of moral credits which, to maintain positive moral self-regard, motivates behavior to restore them (Sachdeva et al., 2009). Just as a various type of misconduct ranging from lying (Gino et al., 2015) to abusive behavior (Liao et al., 2018) can reduce one's moral credits, so behaviors ranging from ethical leadership (Lin et al., 2016) to corporate social responsibility (Ormiston & Wong, 2013) can increase them. Thus, the more a leader has engaged in misconduct in the past, the more they will have lost moral credits, and potentially use punishment as a way to restore these credits if punishment can be construed as moral behavior.

It is also important to note that at any given time, an individual's moral credits correspond to their current accumulated ethical and unethical behaviors at that time, and are not contingent on future behavior. In other words, misconduct causes a loss of moral credits independent of whatever opportunities there may be for compensation in the future. Thus, while moral compensation was argued to occur only when there is high social consensus around the subordinate's misconduct, it is possible to specify that this moderating effect occurs after, not before, moral credits are lost. That is, a leader's past misconduct leads to a loss of moral credits independent of social consensus of the subordinate's misconduct. Having lost moral credits however, punishing misconduct without leniency allows leaders to restore credits, but only when social consensus around the misconduct is high.

Relatedly, moral credentials have increasingly been suggested to be a potential alternative mechanism to moral credits, though primarily in moral licensing instead of moral compensation research. While a moral credits model depicts individuals as balancing their good and bad deeds to maintain an equilibrium over time, a moral credentials model suggests past behavior changes the lens through which subsequent behavior is interpreted (Mullen & Monin, 2016). Moral credentials present a potential alternative explanation for licensing effects because of the possibility that past moral behavior makes subsequent immoral behavior seem less immoral (Monin & Miller, 2001). But it is unlikely that the same is true for moral compensation – if past immoral behavior made subsequent behavior seem less moral, that would limit instead of facilitate moral compensation, assuming individuals still strive to achieve positive levels of moral self-regard. For these reasons and in line with the preceding theorizing, I propose the loss of moral credits as the mediating mechanism for how moral compensation motivates leaders to avoid showing leniency to transgressing subordinates.

Hypothesis 3: A leader's past misconduct is positively related to the loss of moral credits.

Hypothesis 4: The loss of moral credits partially mediates the interactive effect of a leader's past misconduct and social consensus of the subordinate's behavior on leniency shown to the subordinate, such that the indirect effect is negative when social consensus is high, and null when social consensus is low.

Anticipating Guilt from Punishing

The hypocrisy avoidance motive is characterized by the desire to reduce psychological discomfort, a phenomenon characterized by negative affect. Individuals who advocate for certain behavioral standards while being aware of times when their past behavior did not live up to these standards experience cognitive dissonance (Fried & Aronson, 1995). This results in feelings of

discomfort, guilt being the specific affective outcome that drives subsequent action tendencies aimed at alleviating dissonance (Devine et al., 1991; Kenworthy et al., 2011). Indeed, guilt arises when individuals evaluate their own behavior negatively (Tangney et al., 2007), consistent with how hypocrisy involves actual behavior conflicting with an accepted behavioral standard (Devine et al., 1991). This is in contrast to the related self-conscious emotion of shame, which arises when individuals evaluate their global selves (as opposed to specific behaviors) negatively (Tangney et al., 2007). Thus, for example, behaving in ways that support standards such as racial equality can lead to guilt if an individual knows they have engaged in racial discrimination in the past (Hing et al., 2002). Similarly, leaders who punish transgressing subordinates when they themselves have engaged in misconduct are likely to feel guilty.

Although guilt motivates behavior to reduce dissonance (Kenworthy et al., 2011), individuals do not only act after they experience it. Individuals are capable of anticipating their emotional responses to situations and act pre-emptively to avoid negative emotions in particular (Baumeister et al., 2007). Guilt is a prime candidate for such anticipatory reactions, as individuals often learn from past situations where their behavior led to feelings of guilt and update their future behaviors accordingly (Baumeister et al., 1995). For example, anticipating guilt predicts piracy attitudes (Wang & McClung, 2012), criminal deviance (Svensson et al., 2013), and bone marrow donation (Lindsey, 2005). Because hypocrisy is associated with guilt, and individuals who experience hypocrisy enact behaviors to directly reduce it (Stone et al., 1997), it is likely that individuals can anticipate the guilt that would arise from hypocritical behavior and avoid that behavior as much as possible. This suggests that leaders making disciplinary decisions would consider their own history of misconduct. The more misconduct they have engaged in previously, the more they are likely to anticipate feeling guilty if they were

to punish a subordinate who for also engaging in misconduct. Per the previous discussion, this mechanism would occur only when there is low social consensus around the subordinate's misconduct, failing which there would be excessively strong situational expectations for punishing. Upon anticipating guilt, the leader can then directly mitigate hypocrisy by showing leniency to the subordinate. The more leniency is shown, the less disapproval the leader is signaling towards the subordinate's behavior, and the less hypocritical the leader's decision becomes. I thus propose the following hypotheses, as well as the overall conceptual model in Figure 8:

Hypothesis 5: The relationship between a leader's past misconduct and anticipated guilt is moderated by social consensus of the subordinate's behavior, such that the relationship is positive when social consensus is low, and null when social consensus is high.

Hypothesis 6: Anticipated guilt partially mediates the interactive effect of a leader's past misconduct and social consensus of the subordinate's behavior on leniency shown to the subordinate, such that the indirect effect is positive when social consensus is low, and null when social consensus is high.

--- Insert Figure 8 About Here ---

Overview of Studies

I first conducted two pilot studies to validate an experimental manipulation of social consensus. Then, I conducted three main studies to test the theoretical model. In Study 1, I conducted a vignette-based experiment with an online sample. The scenario allowed for the manipulation of both social consensus and past misconduct, the latter being particularly difficult to manipulate because it results from endogenous choice by individuals. In Study 2, I conducted

a critical incident study with a sample of managers. I recruited managers because they are prototypical leaders in organizations (Mintzberg, 1971) who are likely to have observed misconduct from their employees. The critical incident technique allows for sampling from ecologically valid situations where leaders have been aware of their subordinate's misconduct, and report their actual disciplinary decisions. Finally in Study 3, I conducted a final experiment with an online sample and manipulated social consensus using an in-basket task for improved internal validity. This design also improved on the hypothetical nature of the dependent variable in Study 1 by providing a behavioral measure of leniency.

Pilot Study 1A

Sample and Procedures

To validate the social consensus manipulation, I recruited 400 participants from Prolific Academic (Peer et al., 2017), screening for full-time employed adults residing in the US. After removing six participants who failed an attention check, the final sample comprised 394 participants (M_{age} = 34.99, SD_{age} = 8.80, 41.6% female, 77.2% White). Participants were presented with Akaah's (1992) 17-item measure of ethics behavior in random order. Sample items are "using company services for personal use" and "concealing one's errors". Participants reported the social consensus of each item (1 = most people have a different opinion regarding the ethicality of this, 7 = everyone thinks this is unethical; Yam et al., 2014). The six highest and lowest scoring behaviors were averaged to form high- and low-social consensus measures respectively.

Results and Discussion

I conducted a paired-samples *t*-test to examine whether the selected high-social consensus behaviors were indeed higher on social consensus compared to the low-social

consensus behaviors. Results showed a significant difference between the two ($M_{difference}$ = 1.95, $t(393) = 31.39$, $p < .001$). This validated the manipulation to be used in Study 2, in which participants were randomly assigned to recall either a high- or low-social consensus behavior.

Pilot Study 1B

I conducted a second pilot study to further establish the construct validity of the social consensus manipulation. Social consensus is one of six components of moral intensity (Jones, 1991). Exploratory factor analyses have found a three-factor structure of moral intensity where social consensus loads on one factor, proximity on another factor, and magnitude of consequences, probability of effect, and temporal immediacy on the third factor (McMahon & Harvey, 2006). Thus, beyond determining whether the manipulation affects social consensus as expected, I conducted a study to check for discriminant validity, examining if the manipulation also affected proximity and magnitude of consequences. Magnitude of consequences in particular is one of the most important dimensions for ethical judgment alongside social consensus (Morris & McDonald, 1995).

Sample and Procedures

I recruited 400 participants from Prolific Academic using the same screening criteria as Pilot Study 1a. Participants were shown the six high-consensus and six low-consensus behaviors from Pilot Study 1a in random order. Each behavior was rated on the same single-item measure of social consensus (Yam et al., 2014), as well as proximity ("the harmful effects of the behavior will affect people that are close to the decision maker") and magnitude of consequences ("the negative consequences of the behavior will be very serious"). Items were adapted from McMahon and Harvey (2006).

Results and Discussion

A paired-samples *t*-test showed again that the high-social consensus behaviors were indeed higher on social consensus compared to the low-social consensus behaviors ($M_{difference}$ = 1.62, $t(399) = 24.38$, $p < .001$). However, the high-social consensus behaviors were also higher on magnitude of consequences ($M_{difference} = 0.83$, $t(399) = 22.51$, $p < .001$) and proximity ($M_{difference} = .65$, $t(399) = 18.21$, $p < .001$), though the magnitudes of these differences were smaller. These findings are consistent with prior research that shows positive correlations between these dimensions of moral intensity (McMahon & Harvey, 2006), and suggest it is important to control for both magnitude of consequences and proximity when manipulating social consensus as such. This was implemented in Study 2.

Study 1

Sample and Procedures

I recruited 400 participants from Prolific Academic (Peer et al., 2017) to participate in a vignette study. One participant was removed for not providing a complete response, leaving a final sample of 399 ($M_{age} = 38.08$, $SD_{age} = 9.50$, 35.1% female, 82.5% White). The study design was a 2 (leader's past misconduct: high or low) × 2 (social consensus: high or low) experiment. Participants read one of four vignettes adapted from Longenecker et al., (2006). The vignettes are presented in Appendix B. Each vignette depicted the participant as a corporate director who had inside information about a stock split and either engaged in insider trading for profit (high past misconduct), or did not act on this information (low past misconduct). The director later had to discipline an executive who had been found padding their expense account. In the high social consensus condition, everyone in the organization considered this behavior unethical. In the low social consensus condition, there was little agreement on whether it was unethical. After reading the vignette, participants completed measures of the mediators and dependent variable.

Measures

Manipulation Checks. I used three items in total to check the past misconduct and social consensus manipulations. For past misconduct, participants were asked to what extent they agreed that they had "engaged in misconduct" and "engaged in inappropriate behavior" (1 = strongly disagree, 5 = strongly agree). For social consensus, the same single-item measure as Pilot Study 1A was used.

Loss of Moral Credits. I used Liao and colleagues' (2018) measure of loss of moral credits. Sample items are "I lost moral credits for performing immoral behavior" and "My bad deeds forfeited me credits as a moral person" (1 = strongly disagree, 5 = strongly agree; alpha = .97).

Anticipated Guilt. Participants were asked how they would feel if they punished the subordinate based on four items adapted from Grant and Wrzesniewski (2010) and Steenhaut and Van Kenhove (2006). The items are "I would feel guilty", "I would feel tension", "I would think that I did something wrong", and "I would think that I should not have done what I did" (1 = strongly disagree, 5 = strongly agree; alpha = .89).

Leniency. To measure leniency, participants were told they had the authority to fire the executive for their misconduct, but could also choose a different punishment. Six punishment options from Fukami and Hopkins (1993) were presented (0 = fire the executive, 5 = ignore the incident and do nothing). Higher scores indicate more leniency because they reflect the lessening of consequences beyond the maximum potential punishment of termination.

Results

Descriptive statistics are presented in Table 10 and regression results in Tables 11 and 12. Compared to participants in the low past misconduct condition (M = 2.00, SD = 1.28),

participants in the high past misconduct condition (M = 3.71, SD = 1.25) scored higher on the past misconduct manipulation check (t(397) = 13.47, p < .001). Similarly for social consensus, participants in the high social consensus condition (M = 6.00, SD = 1.17) scored higher on the social consensus manipulation check than those in the low social consensus condition (M = 4.47, SD = 1.79, t(397) = 10.12, p < .001). I thus proceeded to test the hypotheses.

--- Insert Table 10 About Here ---

Hypothesis 1 predicted a negative relationship between social consensus and leniency. Supporting the hypothesis, there was less leniency in the high social consensus condition (M = 2.76, SD = 1.72) than the low social consensus condition (M = 3.47, SD = 1.69 t(397) = -4.17, p < .001). Hypothesis 2 predicted an interaction between past misconduct and social consensus predicting leniency. Hierarchical moderated regression (Aiken & West, 1991) revealed that the interaction was marginally significant (B = .56, SE = .33, p = .093). Simple effects analysis further revealed that when social consensus was low, the effect of past misconduct was marginally positive (B = .43, SE = .24, p = .068). This effect was in the same direction as the hypothesis. However, when social consensus was high, the effect of past misconduct was positive (B = .99, SE = .24, p < .001) and thus in the opposite direction from the prediction. Hypothesis 2 was thus partially supported.

--- Insert Table 11 About Here ---

Hypothesis 3 suggested that past misconduct would be positively related to the loss of moral credits. Indeed, moral credit loss was greater in the high past misconduct (M = 3.62, SD = 1.40) compared to low past misconduct condition (M = 2.17, SD = 1.36, t(397) = 12.40, p < .001). Hypothesis 4 predicted a second-stage moderated mediation effect, such that there would be a negative conditional indirect effect of past misconduct on leniency via the loss of moral

credits when social consensus was high, and a null effect when social consensus was low. Because the required second stage moderating effect of social consensus on the relationship between the loss of moral credits and leniency was not significant (B = .18, SE = .12, p = .137), Hypothesis 4 was not supported and I did not proceed to test for conditional indirect effects.

Hypothesis 5 suggested that past misconduct and social consensus would interact to predict anticipated guilt. This was not supported by the moderated hierarchical regression as the interaction term did not reach significance (B = .01, SE = .07, p = .851). As a result, I also did not proceed to test Hypothesis 6, which predicted a conditional indirect effect of past misconduct on leniency via anticipated guilt depending on social consensus. Hypothesis 6 was thus not supported.

--- Insert Table 12 About Here ---

Supplemental Analyses. I conducted path analysis to examine if anticipated guilt mediated the relationship between the leader's past misconduct and leniency (without moderation by social consensus). Confidence intervals were estimated using 1,000 bootstrapped resamples (Preacher et al., 2007). Results showed a positive indirect effect of past misconduct on leniency via anticipated guilt (indirect effect = .18, 95% CI = [.062, .327]), suggesting the hypocrisy avoidance mechanism did occur, but that it did not depend on social consensus.

Discussion

Study 1 found support only for the effect of social consensus on leniency, and the effect of past misconduct on the loss of moral credits. The primary moderating role of social consensus on the effect of past misconduct on leniency was supported when social consensus was low, but the effect remained in the same positive direction (instead of negative as predicted) when social consensus was high. In addition, due to the absence of the relevant interaction effects (moral

credits × social consensus and past misconduct × social consensus) there was limited support for conditional indirect effects via the specific mediating mechanisms of the loss of moral credits and anticipated guilt. These findings are thus unable to shed light on the theoretical mechanisms surrounding potential moral compensation and hypocrisy avoidance effects. A potential methodological explanation for these findings may be that the manipulation of past misconduct using a vignette lacks the requisite ecological validity. Simply imagining that one has engaged in misconduct in the past may not be enough to simulate actual behavior and the relevant intrapsychic processes such as moral reasoning. Similarly, administering a hypothetical punishment may not produce hypocrisy avoidance because of the absence of real consequences. This may explain why the interactive effect of past misconduct and social consensus on anticipated guilt was not found. To overcome this limitation, I conducted a second study using a critical incident method, which enabled the measurement of participants' actual behavior.

Study 2

Sample and Procedures

To obtain a sample of managers in diverse organizations, I recruited from alumni organizations (e.g., via newsletters, social media platforms) of a large public university in the Pacific Northwest. Such a sample likely encompasses a variety of organizations and occupations, thus allowing the results to be generalizable beyond narrow types of misconduct that may be unique to any one specific context. This is also important to allow for variance in social consensus of misconduct events. 222 participants provided complete responses (M_{age} = 38.19, SD_{age} = 10.02, 38.7% female, 72.1% White). This excludes 29 participants who were unable to recall any incident involving a subordinate's misconduct.

Participants were first asked to write about an incident where they became aware of a subordinate's misconduct (restricted to events in which the subordinate acted in their self-interest, Moore et al., 2019). I manipulated social consensus such that in the high-consensus condition, participants were asked to recall an incident corresponding to one of the six high-consensus behaviors from the pilot studies. In the low-consensus condition, participants recalled one of the six low-consensus behaviors. Participants wrote a short paragraph about this incident (50-100 words). They then responded to a manipulation check, as well as reported their moral credits at the time of this incident, their anticipated guilt if they were to punish the subordinate, and the amount of leniency they ultimately showed. Finally, participants reported how much misconduct they had engaged in prior to this incident, and several measures for control variables.

Measures

Manipulation Check. After recalling the misconduct incident, participants responded to the same single-item measure of social consensus with respect to the specific incident.

Leader's Past Misconduct. Participants responded to the 17-item scale from Akaah (1992; alpha = .93). The past misconduct variable was measured as the same item for which they recalled an incident from a subordinate. For example, if the participant recalled an incident where a subordinate padded their expenses, their past misconduct was measured as the extent to which they padded their own expenses in the past. The other 16 items in the scale were used as the instrument (see Analytic Approach).

Loss of Moral Credits. I used the same measure as Study 1, yoked to the specific behavior selected by the participant (alpha = .97).

Anticipated Guilt. I used the same measure as Study 1 (alpha = .86).

Leniency. I adapted Zipay and colleagues' (2020) three-item scale. The items are "I was lenient to this subordinate", "I gave this subordinate a lighter punishment for their misconduct than I could have", and "I was lenient in response to this subordinate's misconduct" (1 = strongly disagree, 5 = strongly agree; alpha = .87).

Control Variables. Research on cheating suggests the nature of one's relationship with a perpetrator of misconduct can shape ethical judgments (Jendrek, 1992). I thus controlled for the leader's *liking* for the subordinate, which may bias their evaluation of the unethicality of the subordinate's behavior, as well as compel the leader to show more leniency. I adapted three items from Wayne and Ferris's (1990) liking scale. A sample item is "I get along well with this subordinate" (1 = strongly disagree, 5 = strongly agree; alpha = .81). Moral identity can also influence both moral judgment (Reed et al., 2007) and behavior (Aquino & Reed, 2002), and may thus affect both perceptions of social consensus and leniency. I thus controlled for *moral identity* using Aquino and Reed's (2002) five-item measure. A sample item is "It would make me feel good to be a person who has these characteristics" (1 = strongly disagree, 5 = strongly agree; alpha = .88). Beyond moral identity, individuals' *ethical dispositions* affect their ethical perceptions and behavior, and attitudes towards justice. I controlled for this using Brady and Wheeler's (1996) Measure of Ethical Viewpoints. The measure lists 20 traits denoting either utilitarian (e.g., "a winner") or formalist (e.g., "principled") dispositions. Participants rated each one on a seven-point scale (1 = not important to me, 7 = very important to me). Alpha was .84 for the utilitarianism subscale, and .87 for the formalism subscale. Finally, as found in Pilot Study 1B, social consensus may covary with magnitude of consequences and proximity. I thus controlled for them using the same single-item measures as Pilot Study 1B. I also included the

interactions between all covariates and the manipulated social consensus variable as additional covariates (Yzerbyt et al., 2004).

Analytic Approach

I used a series of two-stage least squares (2SLS) regressions to overcome the potential endogeneity in the leader's past misconduct variable. This method requires an instrumental variable that predicts the leader's past misconduct and is related to leniency only via the leader's past misconduct (Antonakis et al., 2010). I used the other 16 items in Akaah's (1992) measure of ethics behavior that the participant did not select as the instrument. This instrument was used in all regressions involving the leader's past misconduct as an independent variable (see also Supplemental Analyses). This means that in each first stage regression, the original leader's past misconduct variable was regressed on the instrument and covariates. In the second stage regressions, the *predicted* values of leader's past misconduct (based on the first stage) were used as the independent variables. The instrumented 2SLS coefficient estimates and standard errors were subsequently used to estimate conditional indirect effects using the product-of-coefficients approach, as recommended by (Dippel et al., 2020). Confidence intervals were estimated using 1,000 bootstrapped resamples (Preacher et al., 2007).

Results

Descriptive statistics are presented in Table 13 and regression results in Tables 14 and 15. The manipulation check was rated higher by participants in the high social consensus condition ($M = 5.69$, $SD = 1.39$) than those in the low social consensus condition ($M = 4.44$, $SD = 1.90$, $t(220) = 5.35$, $p < .001$), thus validating the manipulation.

--- Insert Table 13 About Here ---

Hypothesis 1 predicted a negative relationship between social consensus and leniency. Supporting this hypothesis, being in the high (compared to low) social consensus condition had a negative effect on leniency (B = -.29, SE = .13, p = .027). Hypothesis 2 predicted a past misconduct × social consensus interaction effect on leniency. The F-statistic for the first stage regression was 22.88, well above the threshold of 10 required for strong instruments (Staiger & Stock, 1997). In the second stage, the hypothesized interaction was not significant (B = -.02, SE = .17, p = .887). Hypothesis 2 was thus not supported, suggesting no overall moderating effect of social consensus on the relationship between past misconduct and leniency.

--- Insert Table 14 About Here ---

Hypothesis 3 predicted that past misconduct would be positively related to the loss of moral credits. This positive relationship was indeed found (B = .46, SE = .10, p < .001). Hypothesis 4 further predicted conditional indirect effects of past misconduct on leniency via the loss of moral credits, depending on social consensus. As predicted, the indirect effect was negative when social consensus was high (indirect effect = -.04, 95% CI = [-.058, -.030]). However, the indirect was positive (instead of null) when social consensus was low (indirect effect = .11, 95% CI = [.072, .139]). Thus, Hypothesis 4 was partially supported.

--- Insert Table 15 About Here ---

Hypothesis 5 predicted a past misconduct × social consensus interactive effect on anticipated guilt. This interaction was not significant (B = .03, SE = .16, p = .869); Hypothesis 5 was thus not supported. Hypothesis 6 further predicted conditional indirect effects of past misconduct on leniency via anticipated guilt, depending on social consensus. Because the first-stage moderation predicted by Hypothesis 5 was required for this hypothesis, and that interaction was not significant, I did not proceed to test Hypothesis 6.

Supplemental Analyses. In keeping with recent calls to include control variables only where there is clear theoretical justification (Carlson & Wu, 2012), I tested the hypotheses while excluding proximity and magnitude of consequences. The results were unchanged. Next, similar to Study 1, I examined if anticipated guilt mediated the relationship between the leader's past misconduct and leniency. The indirect effect was again positive (indirect effect = .04, 95% CI = [.001, .089]), suggesting that hypocrisy avoidance occurred but was not contingent on social consensus.

Finally, the validity of 2SLS regression depends on the instrumental variable used. A strong assumption of this method is that the instrument itself is exogenous (Antonakis et al., 2010). Using measured variables as instruments, as the preceding analyses has done, thus requires great caution as there can be no guarantee of their exogeneity – it is not a testable assumption (Antonakis et al., 2010). To address this potential limitation, I repeated the analyses which used 2SLS estimates by replacing them with conventional OLS regression and path analysis (i.e., Hypotheses 2 through 5). The same control variables were retained. The results were substantively unchanged, including the magnitudes and standard errors of the tested coefficients, and patterns of interaction effects where relevant. Thus, whether instrumental variable or OLS regression was used, Hypotheses 2 and 3 were supported, Hypothesis 4 was partially supported, and Hypothesis 5 was not supported.

Discussion

Study 2 improved on ecological and external validity compared to Study 1, as well as adopted 2SLS regression for more robust causal inference for the effect of past misconduct. Accordingly, although the overall moderating effect of social consensus on the relationship between leaders' past misconduct and leniency was again not fully supported, Study 2 revealed

more nuanced findings for the mediating mechanisms. The moral compensation mechanism was supported when social consensus was high (i.e., negative conditional indirect effect of past misconduct on leniency via the loss of moral credits). This is consistent with theorizing that suggests a deficit of moral credits can be compensated for by administering punishment to the full extent possible for misconduct that is widely agreed to be unethical, and thus that losing moral credits would be associated with less leniency. However, when social consensus was low, this mechanism was not supported (i.e., positive instead of null conditional indirect effect of past misconduct on leniency via the loss of moral credits). This suggests that instead of having neutral moral implications, showing leniency in response to misconduct that is low in social consensus may in fact be construed as a moral behavior which can compensate for a deficit in moral credits. Finally, the hypocrisy avoidance mechanism was not supported, as evidenced by the absence of the past misconduct × social consensus interactive effect on anticipated guilt. This result replicates Study 1.

A weakness of Study 2 lies in the validity of the social consensus manipulation. The pilot studies highlighted that in the critical incident method used here, social consensus may covary with magnitude of consequences and proximity. Although these two aspects of moral intensity were controlled for, it is possible that social consensus also covaries with other unmeasured situational characteristics, which could be potential sources of omitted variable bias. I thus conducted Study 3 using a manipulation specific to social consensus while retaining a behavioral measure of leaders' past misconduct.

Study 3

Sample and Procedures

I recruited 330 full-time employed participants from the US on Prolific Academic (Peer et al., 2017). I adapted the anagram and matrix tasks which are commonly used to assess unethical behavior (Mazar et al., 2008; Schweitzer et al., 2004). The final task comprised two time-separated parts – the first part took place in the morning and the second in the evening of the same day. After excluding 122 participants who did not complete the second part, the final sample was 208 (M_{age} = 36.88, SD_{age} = 9.47, 49.5% female, 77.9% White).

In the first part, participants were asked to solve eight matrix puzzles of which three were unsolvable, though this was not known to the participants. Each matrix comprised 12 three-digit numbers, the objective was to find two of these numbers that summed exactly to 10. Participants self-reported whether they solved each matrix; each solution earned them a monetary bonus of $0.05. There was thus an incentive to lie by reporting an anagram "solved" even if it was not.

In the second part, the social consensus manipulation was first introduced. Participants were presented with fictitious research findings about how much social consensus there is that cheating for a small reward is unethical. In the high social consensus condition, participants were told that 89.2% of people surveyed consider this unacceptable; in the low social consensus condition, they were told that 39.2% of people surveyed consider this unacceptable (Yam et al., 2014). As a manipulation check, participants were then presented with a series of unethical behaviors, one of which was "cheating for a small reward", and asked to estimate what society in general thinks of these behaviors (1 = most people have a different opinion regarding the ethicality of this, 7 = everyone thinks this is unethical; Yam et al., 2014). After the manipulation, participants were ostensibly paired with a random participant from a previous study who completed an anagram task. This task similarly involved self-reporting whether one had "solved" an anagram, with each solution earning the participant a monetary bonus. Participants were

asked to act as a supervisor by checking for cheating, where the ostensible partner claimed a greater bonus than was appropriate given the number of anagrams they solved. In reality, all participants were shown the same set of anagram solutions, which included three anagrams reported as "solved" even though they were not. Participants are asked to deduct a bonus amount from this partner, up to a maximum of $0.20. After deciding how much to deduct, participants answered survey questions about their decision, including measures of the mediators and moderator.

Measures

Leader's Past Misconduct. The number of unsolvable matrices that a participant reported having solved in the first part was used as the measure of past misconduct.

Loss of Moral Credits. I used the same measure as Studies 1 and 2, yoked to the specific day (e.g., "Today, I lost moral credits for performing immoral behavior; alpha = .96).

Anticipated Guilt. I used the same measure as Studies 1 and 2 (alpha = .91).

Leniency. Leniency was measured using the same scale as Study 2 (alpha = .87).

Control Variables. Unlike Study 2, participants in this study had no preexisting knowledge of or relationship with the individual to be punished. I thus did not control for liking. However, because moral identity and predisposition could still affect participants' ethical judgments and behavior, I controlled for them using the same measures as Study 2. Alphas were .82 for moral identity, .82 for utilitarianism, and .80 for formalism. I also controlled for other past unethical behaviors using the same measure from Akaah (1992; alpha = .96), because they may also produce variance in the outcomes independent of the cheating measure of interest. Finally, a similar analytic approach was used where covariates were entered as main effects and interactions with the social consensus manipulation in the regression models.

Results

Descriptive statistics are presented in Table 16 and regression results in Tables 17 and 18. The manipulation check was validated as social consensus was rated higher in the high social consensus condition ($M = 4.61$, $SD = 1.59$) than the low social consensus condition ($M = 4.08$, $SD = 1.38$, $t(206) = 2.57$, $p = .011$).

--- Insert Table 16 About Here ---

Hypothesis 1 predicted a negative relationship between social consensus and leniency. Again supporting the hypothesis, this negative relationship was found ($B = -.32$, $SE = .13$, $p = .014$). Hypothesis 2 further predicted a moderating effect of social consensus on the relationship between past misconduct and leniency. Moderated regression did not reveal such an interactive effect ($B = .04$, $SE = .13$, $p = .742$). Hypothesis 2 was thus not supported.

--- Insert Table 17 About Here ---

Hypothesis 3 predicted that past misconduct would be positively associated with the loss of moral credits. This relationship was not significant ($B = .06$, $SE = .05$, $p = .262$). Hypothesis 4 predicted conditional indirect effects of past misconduct on leniency via the loss of moral credits, depending on social consensus. Because the first-stage relationship between past misconduct and moral credit loss (Hypothesis 3) was not supported, I did not proceed to test Hypothesis 4.

Hypothesis 5 predicted a moderating effect of social consensus on the relationship between past misconduct and anticipated guilt. This interaction was not significant ($B = .02$, $SE = .14$, $p = .886$). Because this was the first-stage interaction required for the conditional indirect effects suggested in Hypothesis 6, I did not proceed to test Hypothesis 6.

--- Insert Table 18 About Here ---

Supplemental Analyses. I ran all analyses without control variables. The only changed result was for Hypothesis 3 – past misconduct had the expected positive effect on the loss of moral credits (B = .25, SE = .06, p < .001), instead of a null effect. The results for Hypotheses 1, 2, 4, 5, and 6 were unchanged.

Finally, I tested for the simple mediating role of anticipated guilt in the relationship between the leader's past misconduct and leniency. Similar to Studies 1 and 2, the indirect effect was positive (indirect effect = .09, 95% CI = [.043, .153]). Thus, across three studies, the hypocrisy avoidance mechanism was supported but did not depend on social consensus.

Discussion

Study 3 found support only for Hypothesis 1, but not Hypotheses 2 through 6. Similar to Study 1, these results highlight the difficulty in testing for moral self-regulation effects in a controlled experimental context. For Study 3 in particular, it is likely that the effects of any misconduct in the experiment were dwarfed by the effects of real-life misconduct that participants had engaged in. This would explain why the control variable measuring other forms of misconduct had significant effects in the expected directions on both mediators and the dependent variable, while the independent variable which measured the participant's behavior within the experimental paradigm did not. It is also possible that the dependent variable which involved punishing an unnamed participant with a small amount of bonus deduction was, like Study 1, an environment with stakes that were too low for the relevant moral compensation and hypocrisy avoidance mechanisms to occur. Taken together, these findings suggest it is important to measure moral self-regulation processes in more ecologically valid settings with real stakes, as was the case in Study 2.

General Discussion

Across three studies using diverse methods, results showed mixed support for the moral compensation and hypocrisy avoidance mechanisms theorized to be relevant self-regulatory processes after leaders engage in misconduct, and that are likely to affect the leniency they subsequently show to their own subordinates. Leaders consistently showed less leniency to misconduct that was higher in social consensus. However, there was limited support for the overall moderating effect of social consensus on the relationship between past misconduct and leniency, and the mediating mechanisms of the loss of moral credits and anticipated guilt. Only in Study 2 was there partial support for the loss of moral credits as a mediating mechanism. This was evidenced by the negative conditional indirect effect of past misconduct on leniency via the loss of moral credits for misconduct that was high in social consensus, suggesting that leaders can morally compensate for past transgressions by showing less leniency towards these types of misconduct.

The primary aim of this research was to disentangle competing predictions of moral compensation and hypocrisy avoidance theories around when a leader's past misconduct would lead to more or less leniency when dealing with their subordinates' misconduct. Although the mixed findings across three studies make it difficult to draw clear conclusions, they still reveal several insights. First, the only supported mediating mechanism was the loss of moral credits when social consensus was high (i.e., moral compensation) in Study 2, which arguably utilized the most ecologically and externally valid critical incident method. In contrast, the anticipated guilt mechanism was not supported in any of the three studies. It is possible that either the construct or measure of anticipated guilt did not adequately reflect hypocrisy avoidance in the context of these studies. Alternatively, past misconduct may simply lead to anticipated guilt

regardless of social consensus, which is generally consistent with results across the three studies. Given the strong theoretical and empirical precedent for hypocrisy-guilt associations (Du et al., 2019; Effron et al., 2018; Schaumberg & Flynn, 2017), the latter explanation is arguably more likely. Second, even within Study 2, the moral compensation mechanism was not fully supported as results suggested a *positive* (instead of null) indirect effect of past misconduct on leniency when social consensus was low. This suggests leniency may be viewed not as unjust treatment (Trevino & Ball, 1992), but as a moral behavior possibly more akin to forgiveness or mercy (Aquino et al., 2006; L. Wang & Murnighan, 2017). From the perspective of the decision-making leader, showing more (instead of less) leniency could then also be one way of compensating for their past misconduct. Taken together, these results paint a complex picture of the theorized moral compensation and hypocrisy avoidance processes. Both processes may co-occur when social consensus is either high or low, but moral compensation takes different forms in either case – compensation occurs by showing less leniency when social consensus is high, and more leniency when social consensus is low. Additionally, the null results in Studies 1 and 3 suggest a further precondition for either of these processes to occur may be that the punishment stakes must be sufficiently high and realistic.

These findings also shed light on the second contribution of this research, which was to examine how beyond the immediate characteristics of a misconduct event, events in the past also affect leniency decisions. That is, leaders are likely to use both hypocrisy avoidance and moral compensation to regulate their leniency decisions based on their past behavior. Hypocrisy avoidance in particular appears to occur regardless of situational characteristics such as social consensus, though future work may also examine other potential moderators of this effect. This makes sense in light of the literature on hypocrisy and more broadly cognitive dissonance, which

suggests that these are aversive experiences which individuals try to minimize (Aronson et al., 1991; Bruneau et al., 2020; Hing et al., 2002). It is perhaps not surprising, then, that leaders may proactively preempt hypocrisy by showing leniency to others when they are aware of their own past transgressions. Relatedly, while research has highlighted various situational factors that moderate interpersonal reactions to hypocrisy (Effron et al., 2018), it is possible that intrapersonal reactions to hypocrisy are less likely to be similarly moderated.

The final contribution was to examine how variation in social consensus affects ethical decision-making. Warren and Smith-Crowe (2008) suggest scholars have neglected the potential for (un)ethical conduct to be ambiguous, and that actual moral judgments vary more than previously thought because they rely on moral rules and beliefs that vary widely across individuals (Gert, 2004). In other words, social consensus around the unethicality of misconduct events is likely to vary. More recent research also highlights different forms of moral ambiguity which affect the decision-making processes. Notably, (Hirsh et al., 2018) suggest moral uncertainty may arise from being in an unfamiliar situation, when there are conflicting goals or ethical principles, or when there are unforeseen consequences of certain actions. In line with this theorizing, I show specifically that high ambiguity (or equivalently low social consensus) is likely associated with more leniency, and facilitates a moral compensation mechanism whereby leaders compensate for their past conduct by being more lenient.

Limitations and Future Directions

As previously mentioned, the null findings in Studies 1 and 3 may reflect important limitations in external and ecological validity in these studies. While this may be less surprising for the vignette-based Study 1, which was hypothetical in nature, it is more surprising for Study 3, which used common paradigms in behavioral ethics. It is often argued that the cheating

measures in these paradigms represent clearly unethical behavior with "direct organizational analogs" (Schweitzer et al., 2004). Although few scholars would disagree that these behavioral measures reflect cheating strictly in terms of claiming an undeserved reward, it is much less clear to what extent these behaviors are actually judged as unethical. In Study 3 for example, while other temporally distal misconduct events were significantly associated with the loss of moral credits, the main cheating measure was not. Other recent research also highlights that the explicit risk of getting caught does not affect cheating in the matrix task (Gamliel & Peer, 2013), which runs counter to the extensive literature on deterrence theory (Nagin, 2013). Future research may thus examine similar moral self-regulation mechanisms using more ecologically and externally valid methods. For example, Pierce and Balasubramanian (2015) call for more behavioral field data, which can range from field experiments (e.g., Jin & Kato, 2006; Nagin et al., 2002) to archival data (e.g., Jacob & Levitt, 2003; Pierce & Snyder, 2015). This would also greatly enhance the generalizability of studies in behavioral ethics more broadly.

A further limitation is the implicit assumption that leaders compensating for their past transgressions would show less leniency where possible. The results from Study 2 suggest an alternative possibility, that showing more leniency may also be construed as a moral behavior that allows one to compensate for past transgressions. Future research should thus examine when and why leniency is perceived as more or less moral. Consistent with this notion, Zipay et al. (2020) examine both positive and negative consequences of leniency by showing its associations with both pride and guilt for decision-makers themselves. Related individual differences such as guilt-proneness and moral identity may also moderate these effects, and these constructs may also play a stronger moderating role in hypocrisy avoidance mechanisms than what was found in this research for social consensus. It is likely that situations where leniency is perceived as more

moral would also result in pride for the lenient decision-maker, and situations where leniency is perceived as less moral would instead result in guilt. Examining moderators of such relationships would extend our understanding of when either outcome is more likely. Potential insights here may be gleaned from the literature on the moral nature of forgiveness and mercy. For example, Enright et al. (1991) suggest that individuals who possess higher levels of cognitive moral development may also view leniency as a way to increase overall social harmony and reduce conflict. Leniency may also be more likely to be viewed positively when a transgression is justifiable (McCullough & Witvliet, 2002).

Finally, although the present research examined moral self-regulation based on past misconduct of a similar nature to the misconduct subject to leniency decisions, it is possible that leniency may also be affected by qualitatively different types of past misbehavior. This may apply more readily to the moral compensation mechanism, as individuals may lose moral credits for any number or type of misbehaviors. How different misbehaviors may affect hypocrisy avoidance is less clear, and depends on the extent to which punishing misconduct is perceived as hypocritical when the decision-maker has previously engaged in a different type of misconduct. Effron et al. (2018) suggest several reasons why word-deed misalignment in general may be more likely to be perceived as hypocrisy, including when an audience is motivated to condemn the decision-maker, when the audience is worried about unfair treatment, when the audience's culture interprets words more literally, and when the decision-maker's words espouse more benevolence.

In conclusion, this research provides mixed findings for leaders regulating their leniency decisions based on their past misconduct. The first mechanism for such self-regulation – hypocrisy avoidance – likely occurs regardless of the social consensus of a subordinate's

transgression. The second mechanism – moral compensation – may occur via leaders showing either more or less leniency to make up for their past misconduct. Both mechanisms highlight the importance of considering how events in a leader's own past affect their disciplinary decisions.

TABLES AND FIGURES

Table 1

Study 1 Descriptive Statistics and Correlations

	M	*SD*	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
1. Leader role (Time 2)	0.50	0.50																	
2. Loneliness (Time 2)	2.08	0.73	.04	(.75)															
3. Loneliness (Time 1)	2.21	0.73	-.02	.50***	(.75)														
4. Friend strain	3.12	0.45	.02	.09	.28***	(.77)													
5. Family strain	2.86	0.57	.00	.22**	.29***	.43***	(.79)												
6. Negative affect	1.64	0.46	-.00	.42***	.54***	-.10	-.31***	(.78)											
7. Job demands	3.07	0.59	-.08	.11	.15*	-.18**	-.25***	.15*	(.73)										
8. Discrimination	1.43	0.49	.03	.36***	.31***	-.25***	-.42***	.27***	.20**	(.91)									
9. Social support	3.52	0.72	.00	-.23***	-.20**	.16*	.20**	-.14*	-.30***	-.25***	(.82)								
10. Physical health	3.80	0.81	-.03	-.31***	-.24***	.05	.18*	-.20**	-.01	-.20**	.18**								
11. Income	22.22	8.80	-.01	-.07	-.02	.04	.08	-.19**	.10	.02	-.11	.02							
12. Weekly hours	40.08	12.24	.02	-.02	-.05	-.17*	-.15*	-.11	.18*	.14*	-.11	-.08	.33***						
13. Marital status	0.79	0.41	-.02	-.09	-.26***	.12	.06	-.19**	-.20**	-.07	.05	.04	-.01	-.11					
14. Age	45.00	6.53	-.00	-.17*	-.07	.07	-.02	-.09	-.11	-.05	-.13	-.10	.19**	.05	.06				
15. Education	8.13	2.24	-.04	-.05	-.03	.06	.11	.04	.01	-.07	-.02	.18*	.23***	-.09	-.04	-.02			
16. Ethnicity (White)	0.96	0.19	-.10	-.04	-.03	.04	.12	-.04	-.01	-.18*	-.07	.14*	.01	-.09	.02	-.01	.09		
17. Ethnicity (Black)	0.01	0.12	.04	-.01	.00	-.05	-.08	.05	-.06	.21**	.10	-.12	-.05	.07	-.14*	-.03	-.01	-.60***	
18. Gender (Female)	0.50	0.50	.03	-.03	.08	-.00	-.13	.19**	.04	.09	.19**	.03	-.36***	-.27***	-.07	-.02	-.03	.00	.04

Note. $N = 204$. Alphas presented on diagonal in parentheses where applicable. The following variables are dummy coded: Leader role (1 = leader, 0 = non-leader), Marital status (1 = married, 0 = not married), Ethnicity (White; 1 = White, 0 = not White), Ethnicity (Black; 1 = Black, 0 = not Black), Gender (Female; 1 = female, 0 = male). All variables measured at Time 1 unless otherwise stated.

*$p < .05$. **$p < .01$. ***$p < .001$.

Table 2

Study 1 Regression of Loneliness (Time 2) on Leader Role and Gender

	Model 1		Model 2		Model 3	
	B	*SE*	*B*	*SE*	*B*	*SE*
Controls						
Age					-.02**	.01
Ethnicity (Black)					-.58	.45
Ethnicity (White)					-.13	.28
Education					.04†	.02
Income					-.00	.01
Weekly hours					-.00	.00
Marital status					.04	.11
Loneliness (Time 1)					.31***	.07
Negative affect					.25*	.12
Physical health					-.19*	.06
Discrimination					.31**	.11
Friend strain					.14	.11
Family strain					-.04	.09
Social support					-.07	.07
Job demands					-.07	.08
Industry					Included	Included
Predictors						
Leader role	.06	.10	-.18	.15	-.14	.12
Gender (Female)	-.04	.10	-.28†	.14	-.30*	.13
Leader role × Gender			.48*	.20	.40*	.17
R^2 (ΔR^2)	.00		.03 (.03)		.45 (.42)	

Note. $N = 204$. Coefficients are unstandardized. The following variables are dummy coded: Ethnicity (Black; 1 = Black, 0 = not Black), Ethnicity (White; 1 = White, 0 = not White), Marital status (1 = married, 0 = not married), Leader role (1 = leader, 0 = non-leader), Gender (1 = female, 0 = male). †$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

Table 3

Study 2 Descriptive Statistics and Correlations

	M	*SD*	1	2	3	4	5	6	7	8	9	10	11	12	13
1. Gender (Female)	0.56	0.50													
2. Leader role	0.28	0.45	-.01												
3. Experienced authenticity	3.98	0.61	-.04	-.04	(.72)										
4. Loneliness	1.81	0.59	-.10	.00	-.60***	(.71)									
5. Negative affect	1.28	0.53	-.10	-.06	-.26***	.26***	(.80)								
6. Communion striving	3.51	0.65	.00	.09	-.10	-.07	-.08	(.74)							
7. Prior acquaintances	0.26	0.62	.01	-.02	-.04	.04	.04	.18**							
8. Age	20.79	2.56	-.03	-.06	.06	-.01	.01	-.21**	.00						
9. Ethnicity (Asian)	0.41	0.49	-.01	-.04	-.25***	.14*	-.06	.05	.08	-.11					
10. Ethnicity (Black)	0.02	0.14	-.02	.07	.08	-.05	-.07	-.15*	-.06	-.03	-.12				
11. Ethnicity (Hispanic)	0.04	0.20	-.05	-.08	.16*	-.06	-.05	-.02	-.09	.07	-.18**	-.03			
12. Ethnicity (Other)	0.02	0.14	-.09	-.09	-.13	.18**	.33***	-.06	.00	-.03	-.12	-.02	-.03		
13. Ethnicity (Pacific Islander)	0.02	0.14	-.02	-.09	.03	-.02	-.05	.04	.00	.18*	-.12	-.02	-.03	-.02	
14. Ethnicity (White)	0.47	0.5	.06	.12	.19**	-.14*	.00	.02	-.03	.05	-.79***	-.13	-.20**	-.13	-.13

Note. $N = 206$. Alphas presented on diagonal in parentheses where applicable. The following variables are dummy coded: Gender (Female; 1 = female, 0 = male), Leader role (1 = leader, 0 = non-leader), Ethnicity (Asian; 1 = Asian, 0 = not Asian), Ethnicity (Black; 1 = Black, 0 = not Black), Ethnicity (Hispanic; 1 = Hispanic, 0 = not Hispanic), Ethnicity (Other; 1 = Other, 0 = not Other), Ethnicity (Pacific Islander; 1 = Pacific Islander, 0 = not Pacific Islander), Ethnicity (White; 1 = White, 0 = not White). *$p < .05$. **$p < .01$. ***$p < .001$.

Table 4

Confirmatory Factor Analyses for Studies 2 and 3

Study	Model	χ^2	df	$\Delta\chi^2$	CFI	RMSEA	SRMR
2	M0: Four-factor model	56.52	40		.973	.045	.045
	M1: Three-factor model combining experienced authenticity and loneliness into one factor	116.76	43	60.24	.881	.091	.060
3	M0: Four-factor model	68.82	40		.960	.052	.040
	M1: Three-factor model combining experienced authenticity and loneliness into one factor	124.74	43	55.93	.886	.084	.056

Note. M0 is the hypothesized model with leader role, gender, experienced authenticity, and loneliness loading on separate factors. For both studies, the chi-square difference test was significant at $p < .001$. CFI = comparative fit index; RMSEA = root mean square error of approximation; SRMR = standardized root mean square residual.

Table 5

Study 2 Regressions of Loneliness on Leader Role and Gender

	Model 1		Model 2		Model 3		Model 4	
	B	*SE*	*B*	*SE*	*B*	*SE*	*B*	*SE*
Controls								
Ethnicity					Included	Included	Included	Included
Age					.00	.02	.01	.01
Prior acquaintances					.02	.07	.02	.05
Predictors								
Leader role	.01	.09	-.24†	.13	-.22	.14	-.12	11
Gender (Female)	-.12	.08	-.25*	.10	-.24*	.10	-.20*	.08
Leader role × Gender			.44*	.18	.46*	.18	.21	.15
Experienced authenticity							-.56***	.06
R^2 (ΔR^2)	.01		.04 (.03)		.09 (.05)		.39 (.30)	

Note. $N = 206$. Coefficients are unstandardized**.** Five dummy variables for ethnicity are represented: Asian, Black, Hispanic, Pacific Islander, and White; Other is the reference category. Leader role (Leader role: 1 = leader, 0 = non-leader) and gender (Gender: 1 = female, 0 = male) variables are dummy coded. †$p < .10$. *$p < .05$. ***$p < .001$.

Table 6

Study 2 Regressions of Experienced Authenticity on Leader Role and Gender

	Model 1		Model 2		Model 3	
	B	*SE*	*B*	*SE*	*B*	*SE*
Controls						
Ethnicity					Included	Included
Age					.00	.02
Prior acquaintances					-.01	.07
Predictors						
Leader role	-.05	.10	.18	.14	.17	.14
Gender (Female)	-.04	.09	.07	.10	.08	.10
Leader role × Gender			-.41*	.19	-.45*	.18
R^2 (ΔR^2)	.00		.02 (.02)		.13 (.11)	

Note. $N = 206$. Coefficients are unstandardized**.** Five dummy variables for ethnicity are represented: Asian, Black, Hispanic, Pacific Islander, and White; Other is the reference category. Leader role (Leader role: 1 = leader, 0 = non-leader) and gender (Gender: 1 = female, 0 = male) variables are dummy coded. *$p < .05$.

Table 7

Study 3 Descriptive Statistics and Correlations

	M	*SD*	1	2	3	4	5	6	7	8	9	10	11	12	13
1. Gender (Female)	0.42	0.49													
2. Leader role	0.44	0.50	-.08												
3. Experienced authenticity	3.42	0.68	-.09	.00	(.72)										
4. Loneliness	2.46	0.79	.12	-.03	-.57***	(.80)									
5. Social support	3.63	0.79	.00	.02	.24***	-.38***	(.85)								
6. Dispositional loneliness	2.40	0.74	.09	-.14*	-.53***	.57***	-.33***	(.94)							
7. Negative affectivity	1.73	0.69	.20**	-.05	-.41***	.34***	-.16**	.44***	(.91)						
8. Physical health	3.72	0.79	-.09	.14*	.26***	-.26***	.19**	-.32***	-.21***						
9. Age	33.23	9.13	.13*	.10	.24***	-.09	-.01	-.16**	-.17**	.03					
1. Ethnicity (Asian)	0.20	0.40	-.08	.13*	-.13*	.05	.04	.01	.01	-.01	-.17**				
11. Ethnicity (Black)	0.08	0.27	.05	-.13*	.08	-.01	.05	-.07	-.06	-.07	-.10	-.15*			
12. Ethnicity (Hispanic)	0.07	0.26	.00	-.16*	.06	-.02	.05	-.04	-.09	.08	-.10	-.14*	-.08		
13. Ethnicity (Other)	0.04	0.21	.00	.10	.00	.02	-.12*	-.06	-.01	-.01	-.01	-.11	-.06	-.06	
14. Ethnicity (White)	0.61	0.49	.03	.01	.03	-.03	-.03	.08	.08	.01	.25***	-.61***	-.37***	-.34***	-.27***

Note. N = 269. Alphas presented on diagonal in parentheses where applicable. The following variables are dummy coded: Gender (Female; 1 = female, 0 = male), Leader role (1 = leader, 0 = non-leader), Ethnicity (Asian; 1 = Asian, 0 = not Asian), Ethnicity (Black; 1 = Black, 0 = not Black), Ethnicity (Hispanic; 1 = Hispanic, 0 = not Hispanic), Ethnicity (Other; 1 = Other, 0 = not Other), Ethnicity (White; 1 = White, 0 = not White). *$p < .05$. **$p < .01$. ***$p < .001$.

Table 8

Study 3 Regressions of Loneliness on Leader Role and Gender

	Model 1		Model 2		Model 3		Model 4	
	B	*SE*	*B*	*SE*	*B*	*SE*	*B*	*SE*
Controls								
Ethnicity					Included	Included	Included	Included
Age					.00	.00	.00	.00
Physical health					-.05	.05	-.02	.05
Dispositional loneliness					.48***	.06	.34***	.06
Negative affectivity					.12†	.06	.05	.06
Social support					-.21***	.05	-.19***	.05
Predictors								
Leader role	-.03	.10	-.16	.13	-.02	.10	-.01	.10
Gender (Female)	.19†	.10	.05	.13	-.02	.11	.01	.10
Leader role × Gender			.33†	.20	.28†	.16	.16	.15
Experienced authenticity							-.40***	.07
R^2 (ΔR^2)	.01		.02 (.01)		.40 (.38)		.47 (.07)	

Note. $N = 269$. Coefficients are unstandardized**.** Four dummy variables for ethnicity are represented: Asian, Black, Hispanic, White; Other is the reference category. Leader role (Leader role: 1 = leader, 0 = non-leader) and gender (Gender: 1 = female, 0 = male) variables are dummy coded. †$p < .10$. ***$p < .001$.

Table 9

Study 3 Regressions of Experienced Authenticity on Leader Role and Gender

	Model 1		Model 2		Model 3	
	B	*SE*	*B*	*SE*	*B*	*SE*
Controls						
Ethnicity					Included	Included
Age					.01**	.00
Physical health					.08†	.05
Dispositional loneliness					-.35***	.06
Negative affectivity					-.18**	.06
Social support					.06	.05
Predictors						
Leader role	-.01	.08	.13	.11	.03	.09
Gender (Female)	-.12	.08	.03	.11	.07	.09
Leader role × Gender			-.34*	.17	-.32*	.14
R^2 (ΔR^2)	.01		.02 (.01)		.38 (.36)	

Note. $N = 269$. Coefficients are unstandardized**.** Four dummy variables for ethnicity are represented: Asian, Black, Hispanic, White; Other is the reference category. Leader role (Leader role: 1 = leader, 0 = non-leader) and gender (Gender: 1 = female, 0 = male) variables are dummy coded. †$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

Table 10

Study 1 Descriptive Statistics and Correlations

	M	***SD***	**1**	**2**	**3**	**4**
1. Social consensus	.50	.50				
2. Past misconduct	.51	.50	.00			
3. Loss of moral credits	2.90	1.38	-.01	.53***		
4. Anticipated guilt	2.91	1.11	-.07	.34***	.60***	
5. Leniency	3.12	1.74	-.20***	.20***	.19***	.22***

Note. $N = 399$. The following variables are dummy coded: Social consensus (1 = high social consensus, 0 = low social consensus), Past misconduct (1 = high past misconduct, 0 = low past misconduct). *$p < .05$. **$p < .01$. ***$p < .001$.

Table 11

Study 1 Effects of Social Consensus and Past Misconduct on Leniency

	Model 1		Model 2	
Predictor	***B***	***SE***	***B***	***SE***
Intercept	3.11***	.15	3.25***	.17
Social consensus	-.71***	.17	-1.00***	.24
Past misconduct	.71***	.17	.43†	.24
Loss of moral credits				
Past misconduct × Social consensus			.56†	.33
R^2	.08		.09	
ΔR^2			.01	

Note. $N = 399$. Coefficients are unstandardized**.** The following variables are dummy coded: Social consensus (1 = high social consensus, 0 = low social consensus), Past misconduct (1 = high past misconduct, 0 = low past misconduct). †$p < .10$. **$p < .01$. ***$p < .001$.

Table 12

Study 1 Effects of Social Consensus and Past Misconduct on Anticipated Guilt

	Model 1		Model 2	
Predictor	***B***	***SE***	***B***	***SE***
Intercept	2.49***	.12	2.51***	.15
Social consensus	-.13	.11	-.16	.21
Past misconduct	.18***	.03	.17***	.05
Past misconduct × Social consensus			.01	.07
R^2	.07		.07	
ΔR^2			.00	

Note. $N = 399$. Coefficients are unstandardized**.** The following variables are dummy coded: Social consensus (1 = high social consensus, 0 = low social consensus), Past misconduct (1 = high past misconduct, 0 = low past misconduct). ***$p < .001$.

Table 13

Study 2 Descriptive Statistics and Correlations

	M	***SD***	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**
1. Social consensus	.41	.49											
2. Past misconduct	2.10	1.39	-.41***										
3. Past misconduct (instrument)	1.78	.76	.08	.56***									
4. Loss of moral credits	2.26	1.16	-.02	.36***	.32***								
5. Anticipated guilt	2.58	1.00	-.14*	.30***	.20**	.25***							
6. Leniency	3.18	1.02	-.18**	.20**	.14*	.10	.48***						
7. Liking	3.38	.83	-.11	.10	.12	.05	.33***	.32***					
8. Moral identity	4.24	.83	-.20**	-.01	-.21**	-.02	-.17*	.05	.03				
9. Utilitarianism	3.99	.61	-.06	-.15*	-.17**	-.02	-.19**	-.13	.15*	.20**			
10. Formalism	4.36	.61	-.10	-.18**	-.30***	-.10	-.21**	-.10	.09	.38***	.61***		
11. Magnitude of consequences	3.21	.98	.14*	-.24***	-.10	-.02	-.43***	-.40***	-.25***	.03	.23***	.16*	
12. Proximity	3.31	.91	.01	-.23***	-.15*	-.13	-.22**	-.20**	-.10	.14*	.22**	.18**	.31***

Note. $N = 222$. Social consensus was dummy coded (1 = high social consensus, 0 = low social consensus). *$p < .05$. **$p < .01$. ***$p < .001$.

Table 14

Study 2 Effects of Social Consensus and Past Misconduct on Leniency

	Model 1		Model 2		Model 3		Model 4	
Predictor	***B***	***SE***	***B***	***SE***	***B***	***SE***	***B***	***SE***
Control variables								
Intercept	4.80	.28	4.86***	.28	3.25***	.94	3.21***	.96
Magnitude of consequences	-.40***	.07	-.38***	.07	-.10	.09	-.10	.09
Proximity	-.10	.07	-.11	.07	-.04	.10	-.04	.10
Moral identity					.20	.14	.20	.1.4
Utilitarianism					-.25	.18	-.25	.18
Formalism					-.11	.19	-.10	.19
Liking					.30**	.10	.30**	.10
Magnitude of consequences × Social consensus					-.50***	.14	-.50***	.14
Proximity × Social consensus					-.18	.15	-.19	.15
Moral identity × Social consensus					-.17	.17	-.17	.17
Utilitarianism × Social consensus					.35	.27	.35	.27
Formalism × Social consensus					.02	.27	.01	.28
Liking × Social consensus					-.05	.16	-.05	.16
Independent variables								
Social consensus			-.29*	.13	1.46	1.17	1.56	1.37
Past misconduct					.05	.07	.06	.08
Past misconduct × Social consensus							-.02	.17
R^2	.18		.20		.32		.32	
ΔR^2			.02				.00	

Note. $N = 222$. Coefficients are unstandardized**.** Social consensus was dummy coded (1 = high social consensus, 0 = low social consensus). *$p < .05$. **$p < .01$. ***$p < .001$.

Table 15

Study 2 Effects of Social Consensus and Past Misconduct on Mediators

	DV = Loss of Moral Credits				DV = Anticipated Guilt			
	Model 1		Model 2		Model 1		Model 2	
Predictor	***B***	***SE***	***B***	***SE***	***B***	***SE***	***B***	***SE***
Control variables								
Intercept	2.90***	.75	.42	.88	4.28***	.90	4.33***	.92
Magnitude of consequences	.06	.09	.12	.08	-.35***	.09	-.35***	.09
Proximity	-.17†	.09	-.06	.09	-.01	.09	-.01	.09
Moral identity	.04	.10	.04	.10	-.23	.14	-.23	.14
Utilitarianism	.14	.17	.16	.16	-.14	.17	-.14	.17
Formalism	-.27	.17	-.11	.16	-.15	.18	-.16	.18
Liking	.07	.10	.05	.09	.42***	.10	.42	.10
Magnitude of consequences × Social consensus					.12	.14	.12	.14
Proximity × Social consensus					.02	.14	.03	.14
Moral identity × Social consensus					.10	.17	.10	.17
Utilitarianism × Social consensus					-.07	.26	-.07	.26
Formalism × Social consensus					.17	.26	.19	.27
Liking × Social consensus					-.25†	.15	-.26†	.15
Independent variables								
Social consensus	-.06	.16	.47*	.19	-.59	1.12	-.70	1.32
Past misconduct			.46***	.10	.13†	.07	.13	.08
Past misconduct × Social consensus							.03	.16
R^2	.03		.15		.34		.34	
ΔR^2			.12				.00	

Note. $N = 222$. Coefficients are unstandardized**.** Social consensus was dummy coded (1 = high social consensus, 0 = low social consensus). †$p < .10$. *$p < .05$. ***$p < .001$.

Table 16

Study 3 Descriptive Statistics and Correlations

	M	SD	1	2	3	4	5	6	7	8
1. Social consensus	.50	.50								
2. Past misconduct	.60	1.04	.05							
3. Loss of moral credits	1.86	.95	-.02	.27***						
4. Anticipated guilt	2.64	1.04	-.09	.23***	.41***					
5. Leniency	2.85	1.01	-.15*	.19**	.35***	.42***				
6. Moral identity	4.22	.70	.00	-.23***	-.42***	-.14*	-.08			
7. Utilitarianism	3.62	.66	.06	.08	.12	.07	.14***	.18*		
8. Formalism	4.09	.60	.02	-.22**	-.26***	-.18***	-.05	.51***	.46***	
9. Other past misconduct	2.26	1.19	.01	.27***	.67***	.27***	.33***	-.46***	.11	-.26***

Note. $N = 208$. Social consensus was dummy coded (1 = high social consensus, 0 = low social consensus). *$p < .05$. **$p < .01$. ***$p < .001$.

Table 17

Study 3 Effects of Social Consensus and Past Misconduct on Leniency

	Model 1		Model 2	
Predictor	***B***	***SE***	***B***	***SE***
Control variables				
Intercept	-.18	.90	-.13	.92
Moral identity	.45**	.17	.45**	.16
Utilitarianism	.33†	.17	.33†	.17
Formalism	-.15	.20	-.16	.20
Other misconduct	.29**	.09	.29**	.09
Moral identity × Social consensus	-.61*	.23	-.60*	.24
Utilitarianism × Social consensus	-.22	.23	-.23	.24
Formalism × Social consensus	.14	.28	.15	.28
Other misconduct × Social consensus	-.04	.13	-.04	.13
Independent variables				
Social consensus	2.55*	1.22	2.48*	1.24
Past misconduct	.12†	.07	.10	.10
Past misconduct × Social consensus			.04	.13
R^2	.20		.20	
ΔR^2			.00	

Note. $N = 208$. Coefficients are unstandardized**.** Social consensus was dummy coded (1 = high social consensus, 0 = low social consensus).). †$p < .10$. *$p < .05$. **$p < .01$.

Table 18

Study 3 Effects of Social Consensus and Past Misconduct on Mediators

	DV = Loss of Moral Credits				DV = Anticipated Guilt			
	Model 1		Model 2		Model 1		Model 2	
Predictor	***B***	***SE***	***B***	***SE***	***B***	***SE***	***B***	***SE***
Control variables								
Intercept	1.62***	.46	1.55***	.46	.73	.95	.75	.97
Moral identity	-.16†	.09	-.16†	.09	.35†	.18	.35†	.18
Utilitarianism	.18*	.09	.16†	.09	.43*	.18	.43*	.18
Formalism	-.18†	.10	-.16	.11	-.39†	.21	-.39†	.21
Other misconduct	.46***	.05	.45***	.05	.21*	.10	.21*	.10
Moral identity × Social consensus					-.47†	.25	-.47†	.25
Utilitarianism × Social consensus					-.41†	.25	-.42†	.25
Formalism × Social consensus					.12	.29	.13	.30
Other misconduct × Social consensus					-.06	.13	-.06	.14
Independent variables								
Social consensus	-.07	.10	-.07	.10	2.86*	1.29	2.83*	1.31
Past misconduct			.06	.05	.17*	.07	.16	.10
Past misconduct × Social consensus							.02	.14
R^2	.48		.48		.17		.17	
ΔR^2			.00				.00	

Note. $N = 208$. Coefficients are unstandardized**.** Social consensus was dummy coded (1 = high social consensus, 0 = low social consensus).). †$p < .10$. *$p < .05$. ***$p < .001$

Figure 1

Conceptual Model

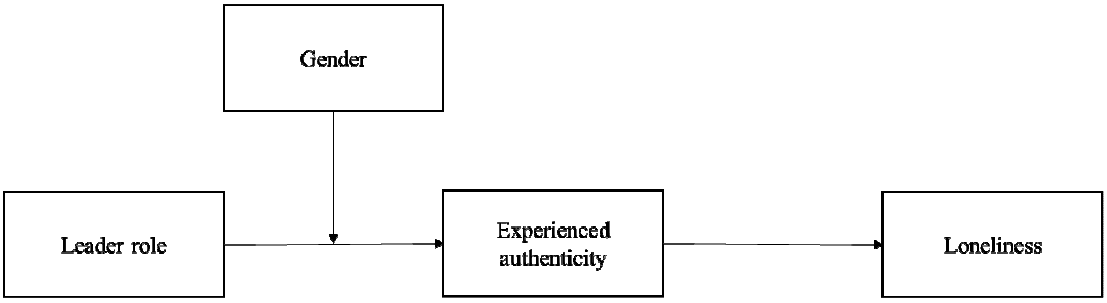


Figure 2

Study 1 Distribution of Propensity Scores Before and After Matching

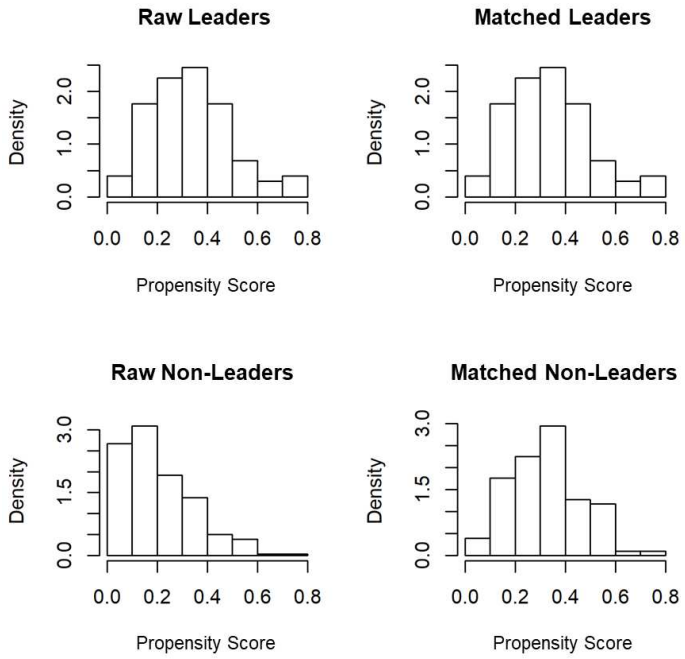


Figure 3

Study 1 Simple Effects of Leader Role on Loneliness Moderated by Gender

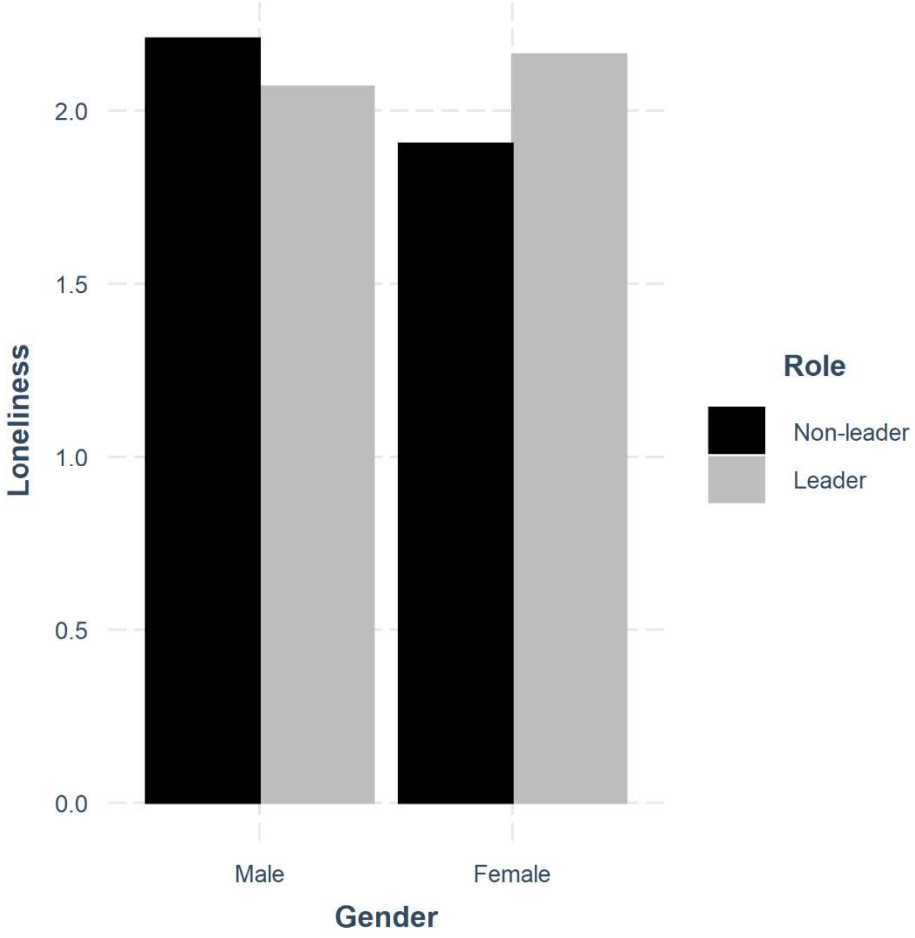


Figure 4

Study 2 Simple Effects of Leader Role on Loneliness Moderated by Gender

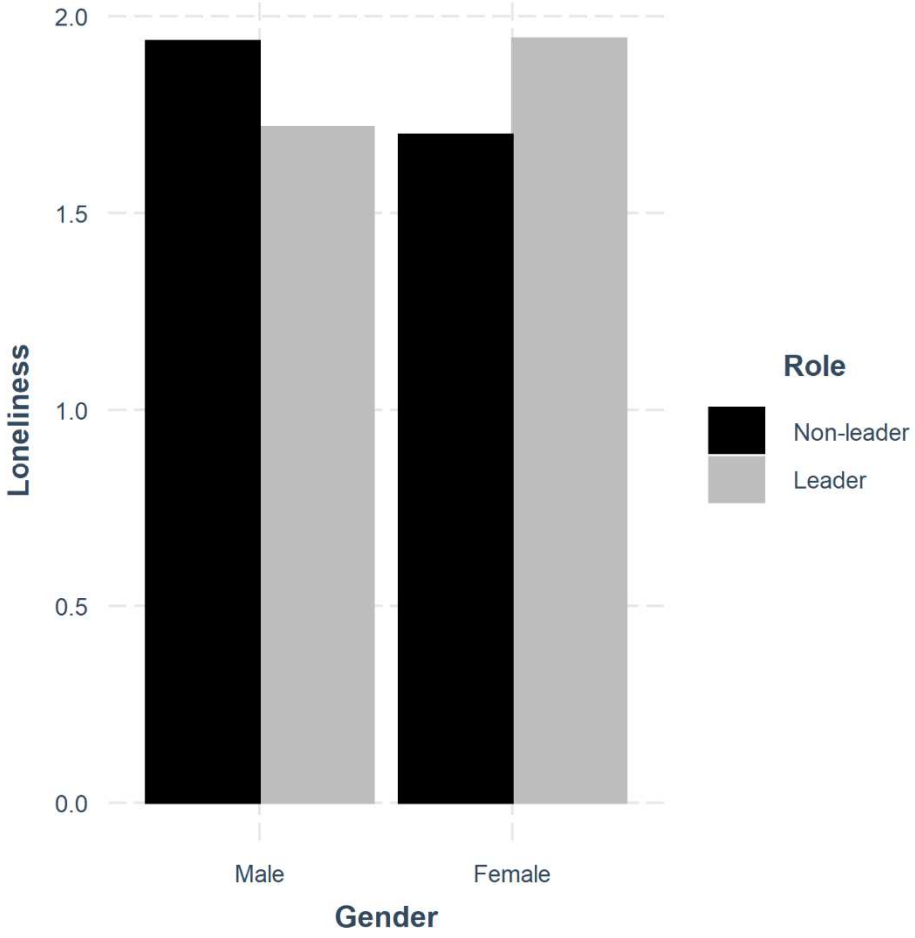


Figure 5

Study 2 Simple Effects of Leader Role on Experienced Authenticity Moderated by Gender

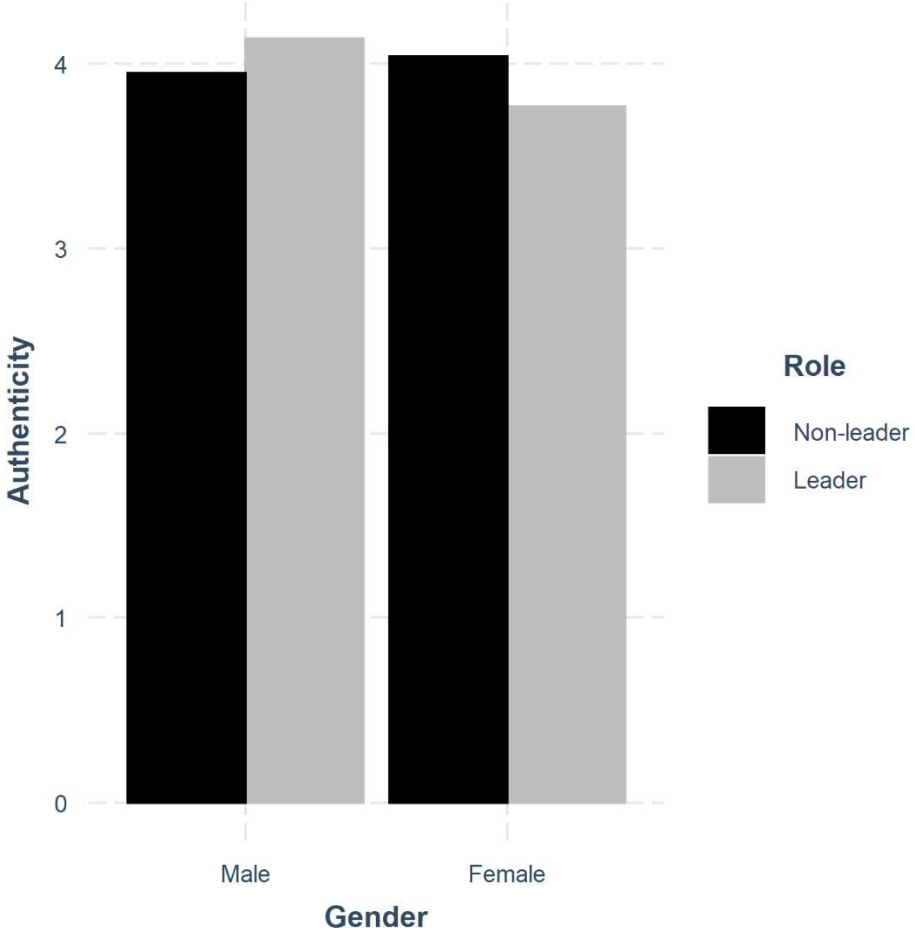


Figure 6

Study 3 Simple Effects of Leader Role on Loneliness Moderated by Gender

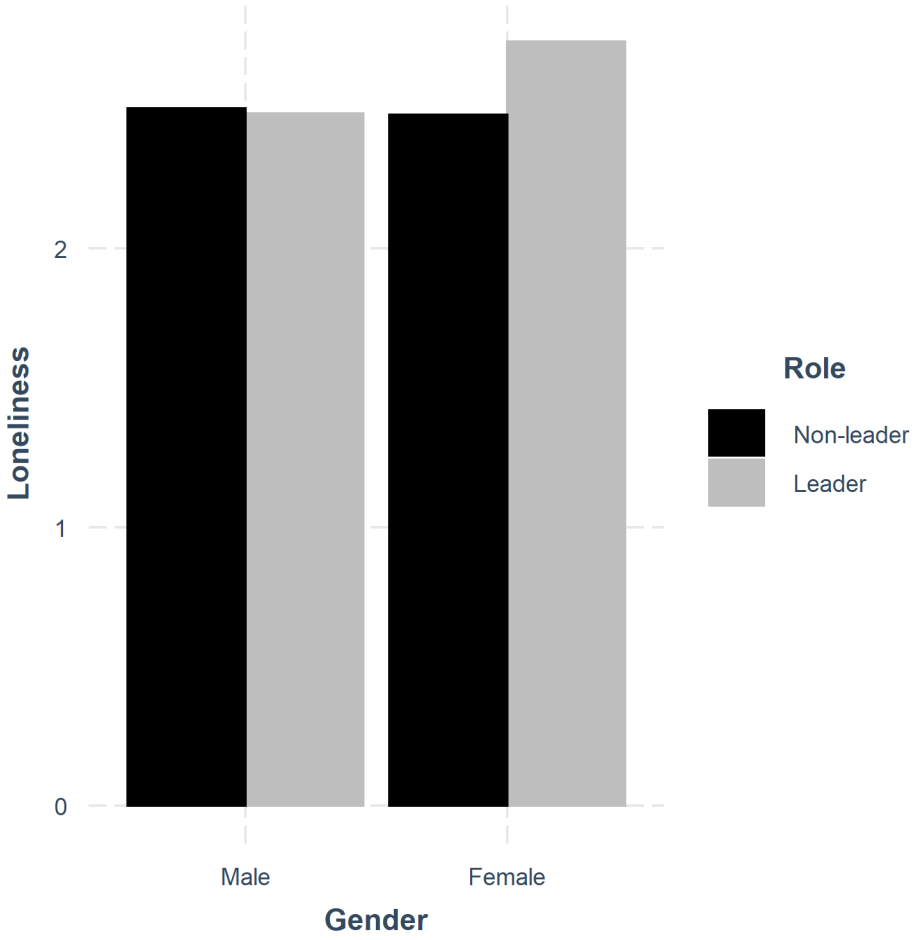


Figure 7

Study 3 Simple Effects of Leader Role on Experienced Authenticity Moderated by Gender

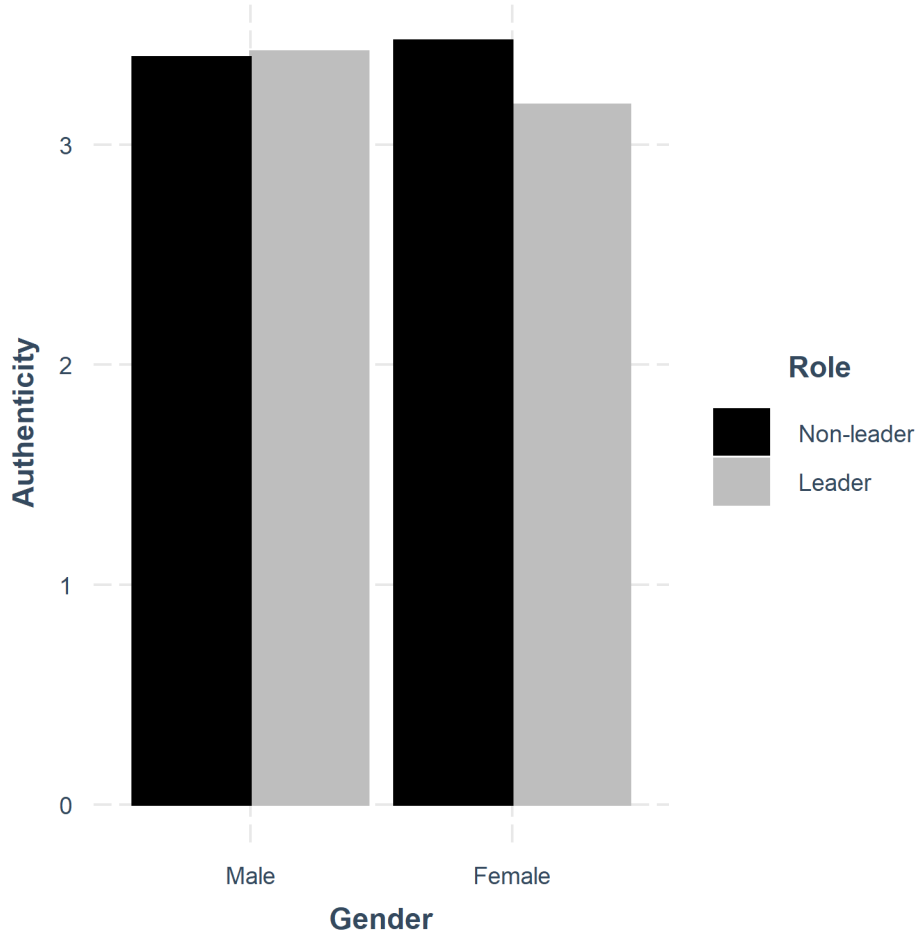
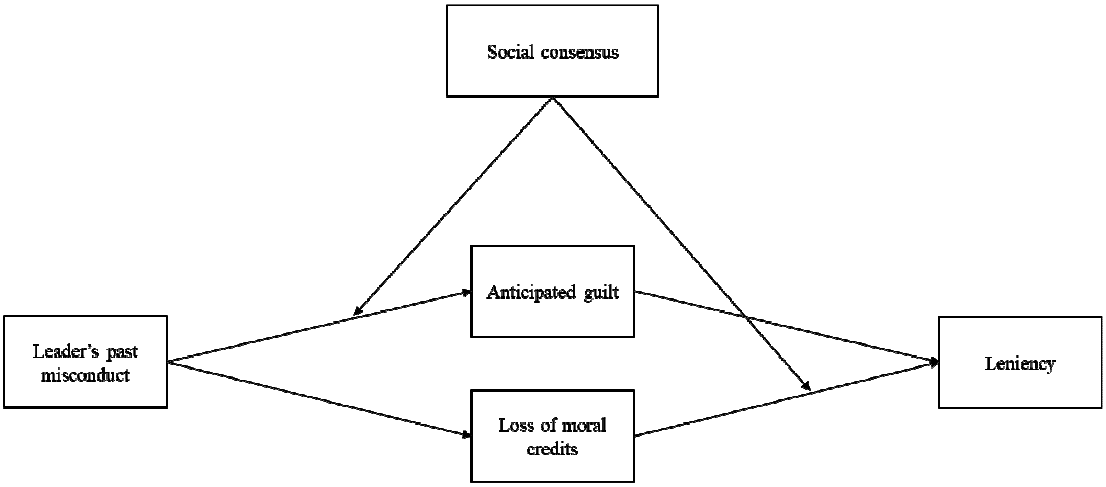


Figure 8

Conceptual Model



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APPENDICES

Appendix A

The Campus Alcohol and Drug Accountability Act (CADA) is a proposed bill to help universities identify hazardous and harmful patterns of alcohol and substance consumption and dependence on campus. The proposed bill requires universities to implement alcohol and drug screening tools, counseling, and abuse prevention classes for all undergraduate students. A core feature of this bill is to require universities to conduct random breath and blood tests among selected undergraduate students at least three times during an academic year. Every year, 5% of the undergraduate population will be randomly selected to undergo these breath and blood tests. This bill is in response to data showing that 19% of college students between the ages of 18 and 24 meet the criteria for alcohol abuse and dependence, and up to 50% of college students are abusing prescription and non-prescription drugs. Unfortunately, this substance abuse often contributes to violence or unsafe conditions for all students.

Appendix B

You are a corporate director who learns that your company intends to announce a stock split and increase its dividend. [You buy additional shares and sell them at a gain following the announcement / You do not use this information to make new financial decisions].

Later, you find out that one of your executives has been padding their expense accounts by $2,000 every year for the last three years. [Everyone in the organization considers this unethical / There is little agreement in the organization about whether this is unethical]. Still, company policy allows you to terminate this executive's employment, though you may also choose a different course of action.