

**ORIGINAL ARTICLE**

# Workplace discrimination: A meta-analytic extension, critique, and future research agenda

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**Abstract**

Despite a large and growing literature on workplace discrimination, there has been a myopic focus on the direct relationships between discrimination and a common set of outcomes. The aim of this meta-analytic review was both to challenge and advance current understanding of workplace discrimination and its associations with outcomes by identifying the pathways through which discrimination affects outcomes, examining boundary conditions to explain when discrimination is most harmful for employees, and exploring a potential third variable explanation for discrimination–outcome relationships. Mediation tests indicated that workplace discrimination is associated with employee outcomes through both job stress and justice. Moderator analyses showed that discrimination appears to be most detrimental when it is observed rather than personally experienced, interpersonal rather than formal, and measured broadly rather than specifically. We also found that discrimination–outcome relationships differ across work and nonwork contexts and as a function of the social identity targeted by discrimination. Discrimination generally explained meaningful incremental variance in outcomes after controlling for the effects of negative affectivity, but the relationships between discrimination and health were substantially decreased. We conclude by offering a constructive critique of the empirical discrimination literature and by detailing an agenda for future research.

In recent decades, positive strides have been made toward fostering more egalitarian workplaces. Although evidence suggests that legal and organizational efforts have achieved some degree of success in this endeavor (Colella, Hebl, & King, 2017), workplace discrimination has hardly been eradicated. A vast literature has confirmed the negative consequences of discrimination, with several meta-analytic summaries linking discrimination to reduced job attitudes and impaired mental and physical health (Jones, Peddie, Gilrane, King, & Gray, 2016; Lee & Ahn, 2011, 2012; Pascoe & Richman, 2009; Schmitt, Branscombe, Postmes, & Garcia, 2014; Triana, Jayasinghe, & Pieper, 2015). Although understanding the consequences of workplace discrimination is critical, the discrimination literature has reached a point of relative stagnation. Beyond the confirmation of direct discrimination–outcome relationships, there is much

we still do not know about this pervasive and costly workplace phenomenon. For instance, what mediating mechanisms explain discrimination's established links with workplace outcomes? Are there unknown boundary conditions for discrimination–outcome relationships that can indicate when discrimination might be more detrimental for individuals? Are there alternative explanations for the relationships between discrimination and outcomes? We believe that, for the discrimination literature to move beyond its current point, questions such as these need to be addressed to challenge assumptions and advance the literature.

In service of this effort, this meta-analysis contributes to the workplace discrimination literature in several ways. First, we consider how discrimination affects outcomes by examining job stress and perceived justice as two theoretically relevant mechanisms to explain discrimination–outcome relationships. Second, we explore several untested boundary conditions that may alter the impact of discrimination. That is, we consider the formality, type of exposure, target, and context of discrimination as possible substantive moderators of discrimination–outcome relationships. Third, we test the extent to which discrimination–outcome relationships may be accounted for by a potential confounding variable. Specifically, because researchers have pinpointed negative affectivity as a particularly likely confound (Shen & Dhanani, 2018), we consider the extent to which this variable may be responsible for assumed discrimination–outcome relationships. Finally, based on our cumulative findings, we highlight those results that challenge existing knowledge of workplace discrimination and use that as the basis for proposing an agenda for future discrimination research. In combination, this paper provides a comprehensive meta-analytic summary of workplace discrimination that shifts past the prevailing consideration of its direct relationships with employee outcomes toward a deeper understanding of the nature and process of discrimination and an increased awareness of the questions that still need to be answered.

## 1 | PERCEIVED WORKPLACE DISCRIMINATION

We define perceived workplace discrimination as an employee or job applicant's perception of unfair or negative treatment based on membership in a particular social group (Chung, 2001). We focus solely on *perceived* workplace discrimination because discrimination arguably only has the power to influence individual outcomes such as employee attitudes, behaviors, or health when the individual is aware of the discriminatory action or event (Ragins & Cornwell, 2001; Swanson & Wotike, 1997). We note that this conceptualization of discrimination is broader than the legal definition of discrimination in two ways. First, our definition includes discrimination that targets social identities that are not currently afforded legal protection (e.g., weight discrimination). Second, our definition is not limited to legally actionable behaviors as it is possible for employees to perceive behaviors as discriminatory that would not be considered illegal (e.g., exclusion from informal social events).

Several published meta-analyses have reported population estimates of the direct relationships between discrimination and individual outcomes (e.g., Jones et al., 2016; Pascoe & Richman, 2009; Triana et al., 2015). Consequently, our focus in this meta-analysis is not on the direct relationships between perceived discrimination and employee outcomes—though we report updated estimates for completeness—but rather on a number of factors that will shed light on the meaning and limits of these relationships. Below, we begin by discussing two mechanisms that provide explanations for the associations between perceived discrimination and its most commonly considered and focal outcomes: job attitudes, work behavior, and employee health.

### 1.1 | Mechanisms for discrimination–outcome relationships

#### 1.1.1 | Job stress as a mechanism

Perceived workplace discrimination has often been conceptualized as a social stressor, or the perception of having (or observing) negative, personally depleting social interactions (Sonnetag & Frese, 2003). Although a number of stressor-strain models exist with subtle differences (e.g., Keashly & Harvey, 2005; Pascoe & Richman, 2009; Podsakoff, LePine, & LePine, 2007), they are each linked by the underlying assumption that experiencing a workplace stressor

can cause a variety of proximal and distal stressor reactions (Keashly & Harvey, 2005; Lazarus, DeLongis, Folkman, & Gruen, 1985; Sonnentag & Frese, 2003). We posit that the most proximal outcome of perceived workplace discrimination is job stress, which we define as an employee's subjective perception of strain associated with his/her job (Stanton, Balzer, Smith, Parra, & Ironson, 2001). When an individual experiences or witnesses mistreatment based on a meaningful component of their social identity (e.g., a person identifies as African American and then experiences race discrimination), a stress response is induced (e.g., Branscombe, Schmitt, & Harvey, 1999; Chrobot-Mason, Ragins, & Linnehan, 2013; Cruwys, Haslam, Dingle, Haslam, & Jetten, 2014). Theoretically, this occurs because identity-based mistreatment threatens a person's sense of self, causing feelings of marginalization (Tajfel & Turner, 1986). Following from this, we maintain that when discrimination is perceived at work, this social stressor leads to the psychological experience of job stress (Barling, 1996; Sonnentag & Frese, 2003). As discrimination-based job stress compounds, it creates strain reactions that are manifested in negative job attitudes, decreased positive and increased negative workplace behaviors, and reduced mental and physical health (Gross, 1970; Hershcovis & Barling, 2010; Sonnentag & Frese, 2003). Therefore, echoing existing stressor-strain frameworks, we posit that job stress is a mechanism by which discrimination relates to job attitudes, job behaviors, and employee health.

**Hypothesis 1:** The relationships between perceived discrimination and (a) job attitudes, (b) work behaviors, and (c) employee health are mediated by job stress.

### 1.1.2 | Perceived justice as a mechanism

Although the predominant approach in the discrimination literature is to conceptualize discrimination as a stressor, a recent review noted that discrimination research would benefit from a wider variety of viewpoints that move beyond this approach (Colella et al., 2017). We contend that, in addition to stress, an individual's perception of justice is a mechanism by which discrimination relates to attitudinal, behavior, and health outcomes. For the purpose of this paper, we define justice broadly to include perceptions of distributive, procedural, and interactional justice (Colquitt, 2001; Colquitt & Shaw, 2005). A broad definition of justice was adopted because discriminatory treatment is manifested in a range of behaviors that can violate perceptions of multiples forms of justice. That is, behaviors that comprise workplace discrimination may affect perceptions of distributive justice (e.g., rewards not being allocated equally to minority and majority employees), procedural justice (e.g., bias against certain individuals being present in decision-making processes), interactional justice (e.g., minority employees being treated disrespectfully by decision makers), or global perceptions of justice (Ambrose & Schminke, 2009). Social exchange theory posits that employment relationships are characterized by interdependent exchange rules, many of which revolve around norms of reciprocity or mutual obligation (Cropanzano & Mitchell, 2005). Perceived discrimination contradicts the expected exchange relationship and, by violating norms of social exchange, should result in perceived injustice. Perceived injustice should, in turn, precipitate negative job attitudes that reflect employees' dissatisfaction with the exchange relationship and lead to reductions in positive—and increases in negative—employee work behaviors to reciprocate the negative treatment (Adams, 1965; Colquitt et al., 2013; Robinson & Morrison, 2000). Furthermore, a substantial body of evidence indicates that perceptions of injustice are associated with decreased mental and physical functioning (Robbins, Ford, & Tetrick, 2012). Consequently, we posit the following:

**Hypothesis 2:** The relationships between perceived discrimination and (a) job attitudes, (b) work behaviors, and (c) employee health are mediated by justice perceptions.

## 1.2 | Conceptual moderators in workplace discrimination

Though it is clear that perceived discrimination is negatively associated with outcomes such as job attitudes and health (Jones et al., 2016; Lee & Ahn, 2011, 2012; Pascoe & Richman, 2009; Schmitt et al., 2014; Triana et al., 2015), what is less clear are the factors that cause these meta-analytic estimates to meaningfully vary in magnitude from one discrimination study to the next. To explore substantive sources of this variability, we examine differences in the type of

discriminatory behaviors (i.e., formal vs. interpersonal), the exposure to discrimination (i.e., experienced vs. observed), the target of discrimination (e.g., race, sex, age), and the context of discrimination (i.e., work vs. nonwork settings) as moderators of otherwise-accepted discrimination–outcome relationships. We believe that the consideration of these possible moderators not only clarifies and deepens current understanding of existing findings but also provides a guide for future discrimination research, which we elaborate on in the following subsections.

### 1.2.1 | Formality of workplace discrimination

Despite an extensive body of research on the relationships between discrimination and outcomes, it remains unknown if the *formality* of discriminatory behaviors, or the degree to which they are connected to job decisions, changes their impact on individual outcomes. Thus, we examine the extent to which the formal versus interpersonal nature of discriminatory behaviors affects discrimination–outcome relationships. Formal discrimination is manifested in job-related decisions such as hiring, promotion, or compensation (Hebl, Foster, Mannix, & Dovidio, 2002), whereas interpersonal discrimination—which does not directly involve job-related decisions—is manifested in negative verbal and nonverbal behaviors that occur in everyday workplace social interactions. These behaviors can include acts of avoidance, refusal to make eye contact, an unwillingness to provide assistance, or unfriendly communication (Pettigrew & Martin, 1987; Solorzano, Ceja, & Yosso, 2000).

Although formal discrimination is coupled with arguably more concrete and high-profile consequences (e.g., failing to be hired or promoted) than interpersonal discrimination, there are reasons to expect interpersonal forms of discrimination to be even more pernicious than formal forms. First, interpersonal discrimination is likely to occur with far greater frequency than formal discrimination (Kessler, Mickelson, & Williams, 1999). Interpersonal discrimination can occur in any social interaction with one's coworkers and supervisors (Hebl et al., 2002), whereas formal discrimination is limited to specific job-related decisions, which occur comparatively infrequently. Employees may, therefore, appraise themselves as being at a near-constant risk for interpersonal discrimination. Second, formal discrimination is more likely to be prohibited by laws, regulations, and formal organizational policies (Hebl et al., 2002; Jones, Arena, Nitttrouer, Alonson, & Lindsey, 2017) which can give employees confidence in seeking recourse for such discriminatory decisions. Interpersonal discrimination, on the other hand, tends to be more difficult to litigate against (and report) as the outcomes of such discrimination tend to be less identifiable. Employees with the inclination to behave in discriminatory ways may thus feel less restricted from engaging in interpersonal discrimination (Guyl, Matthews, & Bromberger, 2001) and the recipients of such discrimination may feel particularly powerless to stop these behaviors. Thus, despite formal discrimination restricting access to vital job resources, we hypothesize that interpersonal discrimination is more strongly related to employee outcomes than formal discrimination.

**Hypothesis 3:** Interpersonal discrimination is more strongly related to outcomes than formal discrimination.

### 1.2.2 | Exposure to workplace discrimination

Another conceptual distinction that has received insufficient research attention is the possible difference between *experienced* and *observed* workplace discrimination. Experienced discrimination is defined as “discrimination directed at the personal self as a function of group membership” (Schmitt et al., 2014, p. 924), whereas observed discrimination (also referred to as bystander or ambient discrimination; Chrobot-Mason et al., 2013; Schneider, 1996) refers to perceiving the general presence of discrimination in one's workplace. Understanding whether and to what degree these two forms of discrimination differentially affect individual outcomes has important workplace implications as the majority of workers are likely to fit into one of these categories.

We expect experienced discrimination to have a stronger relationship with individual outcomes than observed discrimination. In support of this expectation, previous theoretical and empirical work has suggested that personally experienced discrimination poses a greater threat to one's identity, resulting in a stronger impact on health outcomes than observed discrimination (Bourguignon, Seron, Yzerbyt, & Herman, 2006; Hafer & Olson, 1993; Walker & Mann, 1987). Work in the area of organizational justice further supports the expectation that experienced discrimination

has stronger effects on individual outcomes than observed discrimination. This work suggests that when observing the mistreatment of third parties, the mistreatment can be attributed to multiple causes, including blaming the victim for his/her mistreatment (Kray & Lind, 2002; Lind, Kray, & Thompson, 1998; Skarlicki & Kulik, 2005). This allows the observer to perceive the behavior as fair, which reduces its negative impact on the attitudes, behaviors, and health of the observer. In comparison, although self-blame can also occur after personally experiencing mistreatment, it is less pronounced than the victim blaming that can weaken the effects of observing mistreatment (Lind et al., 1998). As a result, personally experienced mistreatment is more likely to lead to negative outcomes than observed mistreatment because experienced mistreatment is more likely to be perceived as unfair. We expect this same pattern of relationships to occur when considering workplace discrimination as a specific form of mistreatment. Thus, we posit the following:

**Hypothesis 4:** Experienced workplace discrimination is more strongly related to outcomes than observed workplace discrimination.

### 1.2.3 | Target of workplace discrimination

Discrimination's impact may also differ based on the social identity (e.g., race, sex, age) that is targeted by the discrimination. Because existing meta-analytic work has either focused narrowly on discrete types of discrimination (e.g., Lee & Ahn [2011, 2011, 2012] and Triana et al. [2015] exclusively examined race discrimination) or combined all forms into overarching estimates (e.g., Jones et al., 2016), it is currently unclear if racism, sexism, ageism, and other specific forms of discrimination differentially relate to job and health outcomes. Mistreatment scholars have called for the development of general models of discrimination that broadly capture the dynamics that are shared across multiple types of discrimination—an advancement that has been described as the biggest improvement needed in the discrimination literature (Dipboye & Colella, 2005). We attempt to answer these calls by identifying whether discrete types of discrimination are comparably linked to a common set of outcomes. In the absence of theoretical and empirical work comparing different types of discrimination, these analyses remain exploratory.

**Research Question 1:** Does the target of discrimination impact the magnitude of discrimination-outcome relationships?

### 1.2.4 | Context of discrimination

In addition to considering differences in discriminatory behaviors (formal vs. interpersonal), exposure to discrimination (experienced vs. observed), and the target of discrimination (e.g., race, sex, age, etc.), another step in advancing our theoretical understanding of discrimination is to consider the impact of the context in which the discrimination occurs on discrimination–outcome relationships. In particular, there are conceptually meaningful differences between work and nonwork discrimination that may produce different relationships with outcomes. We posit that discrimination at work is likely to be more serious and detrimental for individuals than discrimination that occurs in other life domains. We expect this for a number of reasons. First, relative to discrimination in other life contexts, workplace discrimination is more frequently perpetrated by higher-power individuals who have the authority to make impactful decisions such as those regarding access to work resources. In addition, whereas victims of perceived discrimination in nonwork settings may be able to more easily avoid the perpetrator of the mistreatment (e.g., avoiding a particular restaurant), victims of discrimination at work are much less capable of avoiding the situation. Relatedly, because the amount of time individuals spend at work exceeds the amount of time spent in almost any other context, individuals who perceive discrimination at work can be at a nearly constant risk of mistreatment. These conceptual differences suggest that discrimination occurring at work may be more damaging for those who experience (or witness) it in comparison to discrimination occurring in other life domains.

**Hypothesis 5:** Workplace discrimination is more strongly related to outcomes than non-work discrimination.

### 1.3 | Measurement distinctions in workplace discrimination

Measurement approaches may also impact the magnitude of discrimination–outcome relationships, and one important distinction among discrimination measures is how broadly discrimination is conceptualized. Specific measures of discrimination focus on a single form of discrimination (e.g., race discrimination, sex discrimination) and explicitly label this form of discrimination in the scale items (example: “The people I work with treat me less favorably *because of my age*” [italics added]; Redman & Snape, 2006). In contrast, broad measures of discrimination more globally assess the perception of discrimination without limiting responses to discriminatory perceptions based on membership in a single group (e.g., “Do you feel you have been discriminated against in your workplace in the past 12 months?”; Jones, Ni, & Wilson, 2009). Given that specifying a particular reason for discrimination limits responses to a single target, this may fail to capture the full spectrum of discriminatory behaviors that might otherwise be reported. For example, an employee’s perception of race discrimination would not be captured if only sex discrimination was assessed. For this reason, we posit that broad measures of discrimination that encompass all possible forms of discrimination will lead to stronger discrimination–outcome relationships than specific measures.

**Hypothesis 6:** Workplace discrimination is more strongly related to outcomes when discrimination is measured broadly than when a specific type of discrimination is measured.

### 1.4 | An alternative explanation for workplace discrimination–outcome relationships

One important criticism that has been levied against the workplace discrimination literature is that no known attempts have been made to address potential confounding variables (Shen & Dhanani, 2018). Thus, despite the large body of research linking discrimination to important outcomes, it is currently unknown if, and to what degree, these relationships might be inflated by third variables. To address this possibility and critically appraise existing meta-analytic estimates of discrimination–outcome relationships, we consider negative affectivity as a potential third variable explanation for these relationships. We selected negative affectivity because it has a well-documented relationship with perceptions of mistreatment (Aquino, Grover, Bradfield, & Allen, 1999; Bowling & Beehr, 2006) and has been implicated as a potentially meaningful third variable explanation for several relationships in the stress literature (Brief, Burke, George, Robinson, & Webster, 1988; Watson & Pennebaker, 1989). Research has revealed that both work stress and health outcomes share nontrivial relationships with negative affectivity, which may artificially inflate the observed relationships between these variables (Brief et al., 1988; Watson & Pennebaker, 1989). In effect, individuals who are high in negative affectivity may have “mud-colored glasses,” wherein they are more likely to perceive the existence of stress and health problems relative to individuals who are low in negative affectivity.

Negative affectivity may similarly affect the relationships between workplace discrimination and employee outcomes. Researchers have posited that employees high in negative affectivity may be more likely to perceive the presence of mistreatment as a result of inherent perceptual and behavioral tendencies (Aquino & Bradfield, 2000; Bolger & Zuckerman, 1995). That is, individuals high in negative affectivity are not only more likely to interpret behaviors as mistreatment, but they may also be more likely to engage in disrespectful or counternormative behaviors that precipitate negative treatment in return. Further, those who are higher in negative affectivity are also more likely to have negative attitudes toward their job, have greater desires to leave their jobs, engage in more negative workplace behaviors, and perceive themselves to be in poorer health (Kaplan, Bradley, Luchman, & Haynes, 2009; Ng & Sorensen, 2009). Consequently, the associations between negative affectivity, discrimination, and outcomes raise the question of whether discrimination maintains substantive associations with individual work outcomes after controlling for negative affectivity.

**Research Question 2:** Does workplace discrimination display incremental validity over negative affectivity in predicting employee outcomes?

## 2 | METHOD

### 2.1 | Workplace discrimination literature search

We located discrimination studies by searching PsycINFO, PubMed, ProQuest, ProQuest Dissertations and Theses, and Google Scholar using the following keywords: *workplace discrimination, ageism, sexism, heterosexism, sexual orientation discrimination, weight discrimination, religious discrimination, sex discrimination, accent discrimination, and racism*. In addition, we used the following outcome-related keywords in combination with the keywords listed above: *job stress, justice, negative affect, negative affectivity, job attitudes, job satisfaction, organizational commitment, turnover intentions, organizational citizenship behavior, counterproductive work behavior, job performance, anxiety, depression, well-being, distress, psychological health, physical health, physical symptoms, and health*. We located unpublished studies by searching for dissertations and theses, contacting researchers in the field, searching the reference sections of included articles, and searching conference proceedings from the Society for Industrial and Organizational Psychology and the Academy of Management. To be as inclusive as possible, we did not restrict the publication date and the earliest study included in this paper was published in 1978.

### 2.2 | Workplace discrimination inclusion criteria

Studies were eligible for inclusion if they reported a correlation or sufficient information to calculate a correlation between perceived workplace discrimination and job stress, justice, negative affectivity, job satisfaction, organizational commitment, turnover intentions, organizational citizenship behavior, counterproductive work behavior, mental health, or physical health. We note that the search yielded too few correlations between discrimination and task performance to estimate this relationship. Because we sought to capture employees' perceptions of discrimination in real workplaces, we excluded experimental studies. Consistent with previous meta-analyses (e.g., Schmitt et al., 2014), we only included studies that explicitly labeled their discrimination measures as discrimination or used such terms as racism, sexism, ageism, heterosexism, prejudice, or stigma. To exclusively capture the appraisal of job-specific stress, we operationalized job stress as any measure assessing how stressful one views his/her job. As such, we did not include measures of stress in nonwork domains (i.e., life stress), outcomes of stress (e.g., mental health, physical health, burnout), and stressors (e.g., role conflict, role ambiguity). We included studies measuring organizational justice if they assessed any of the specific justice dimensions (i.e., distributive, procedural, or interactional justice) or used a global measure that broadly assessed perceived justice. Although separately estimating the relationships between discrimination and the three dimensions of justice would be informative, the available data did not allow us to report relationships at this level of specificity.

We included studies reporting the relationship between workplace discrimination and job satisfaction if the study assessed satisfaction with the job in general or with specific facets of the job. We restricted our operationalization of organizational commitment to studies that assessed affective commitment as this is arguably more akin to job attitudes than normative or continuance commitment (Klein, Molloy, & Brinsfield, 2012). In addition, we only estimated the relationship between discrimination and turnover intentions because too few studies were available to estimate the relationships between discrimination and behavioral measures of withdrawal (e.g., absenteeism, lateness). We operationalized organizational citizenship behaviors (OCB) and counterproductive work behaviors (CWB) to include global measures as well as measures of specific facets of OCB (e.g., altruism, conscientiousness) and CWB (e.g., production deviance, theft). Moreover, OCB and CWB measures were eligible for inclusion regardless of whether the behaviors were directed at the organization or one's coworkers. We note that the majority of studies produced by the literature search employed global measures of OCB ( $k = 6$  of 9) and CWB ( $k = 6$  of 6). We categorized studies measuring mental health symptomology, psychological distress, depression, and anxiety as mental health and studies measuring physical health symptomology or appraisals of physical functioning as physical health. Last, because we were interested in general levels of negative affectivity, we only included studies that measured trait, rather than state, negative affectivity.

These inclusion criteria resulted in a final sample of 99 studies with 110 independent samples and a total sample size of 238,951 (references for included studies are available in Appendix A). Race discrimination was the most commonly measured target of discrimination ( $k = 39$ ), followed by discrimination based on sex ( $k = 25$ ), sexual orientation ( $k = 16$ ), age ( $k = 14$ ), disability ( $k = 7$ ), religion ( $k = 2$ ), weight ( $k = 1$ ), and accent ( $k = 1$ ). In addition, 17 studies assessed discrimination without referencing a specific target group. The included studies also varied in response format (e.g., frequency,  $k = 25$ ; agreement,  $k = 44$ ; yes/no,  $k = 21$ ) and scale length (i.e., single item measures,  $k = 15$ ; multiple item measures;  $k = 94$ ).<sup>1</sup> In total, we note that our meta-analytic database represents a 92% expansion over the largest existing meta-analysis on workplace discrimination ( $k = 51$ ; Lee & Ahn, 2012).

### 2.3 | Workplace discrimination data coding

We report the sample-specific information that was coded to conduct our meta-analyses (e.g., sample size, measure reliability, effect size) in Appendix B. We also coded the included samples for information relevant to our moderator analyses. We coded discrimination as *formal* if the discriminatory behaviors involved job-related outcomes, including hiring, firing, promotions, performance evaluations, and access to training and developmental opportunities (example item: "Gender played a role in the last performance evaluation I received"; Shaffer, Joplin, Bell, Lau, & Oguz, 2000). We coded discrimination as *interpersonal* if it was not related to job outcomes. These behaviors included verbal and nonverbal behaviors that occur in interpersonal interactions, such as being excluded, derogated, or receiving unfavorable interpersonal treatment (example item: "Someone at work makes derogatory comments about your ethnicity"; Schneider, Hitlan, & Radhakrishnan, 2000). We coded discrimination as *experienced* if the measures specifically referred to the individual, as demonstrated in the following item, "At work, I sometimes feel that my ethnicity is a limitation" (Sanchez & Brock, 1996). We coded discrimination as *observed* if the measure assessed the occurrence of discrimination without specifying the respondent as the target of those actions (e.g., "During the past 24 months in your workplace, have you been in a situation where any of your supervisors or coworkers told offensive jokes about lesbians, gay men, or bisexual people?"; Waldo, 1999).

We coded scales as *specific* measures of discrimination if the items identified the reason for discrimination (e.g., "The people I work with treat me less favorably *because of my age*" [italics added]; Redman & Snape, 2006) while coding *broad* measures as those that assessed discrimination in general without referring to a specific type of discrimination (e.g., "Do you feel you have been discriminated against in your workplace in the past 12 months?"; Jones et al., 2009). For specific measures, we also coded studies for the target of discrimination (i.e., race discrimination, sex discrimination, sexual orientation discrimination, age discrimination) to assess whether the discrimination–outcome relationships varied based on the social identity targeted by discrimination.

If a single sample reported multiple effect sizes for one relationship (i.e., discrimination and job satisfaction), we computed a composite correlation (Nunnally, 1978) when possible. All articles included for our meta-analyses were coded by two independent coders and any discrepancies were reconciled by discussion until consensus was reached. Average preconsensus agreement between coders was 93% across all coded variables.

### 2.4 | Nonwork discrimination meta-analyses

To compare workplace discrimination effect sizes with effect sizes involving discrimination in nonwork contexts, we also conducted a meta-analysis of nonwork discrimination. We limited our comparison of work and nonwork discrimination effect sizes to the relationships between discrimination and mental/physical health because all other outcomes considered in this paper are specific to the work context. To locate primary studies, we obtained those included in previous meta-analyses that estimated the relationship between discrimination and health (i.e., Jones et al., 2016; Pascoe & Richman, 2009; Schmitt et al., 2014). We then conducted an updated literature search to find studies not included in the previous meta-analyses. We searched using PsycINFO starting from 1 year prior to the publication of the largest available meta-analysis (2008–2016 for physical health, 2013–2016 for mental health). The keywords used for this



**TABLE 1** Mean sample-based reliability estimated used for artifact distributions

Construct	k	N	Mean reliability estimate
Discrimination	233	297,452	.88
Negative affectivity	91	26,164	.84
Job stress	21	15,154	.84
Justice	12	7,799	.93
Job satisfaction	67	207,658	.87
Affective commitment	31	25,741	.85
Turnover intentions	44	38,130	.84
Organizational citizenship behavior	9	4,914	.84
Counterproductive work behavior	10	5,547	.84
Mental health	143	95,543	.88
Physical health	47	50,731	.85

Note: *k* = the number of independent samples; *N* = sample size.

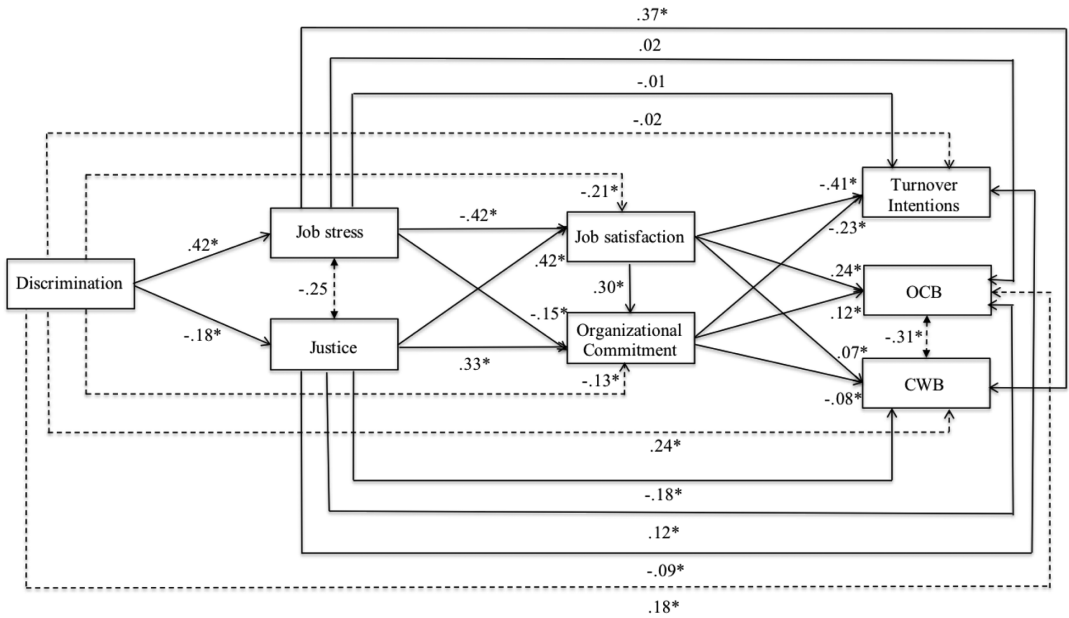
search were: *discrimination, anxiety, depression, well-being, distress, psychological health, physical health, physical symptoms, and health*. Studies were eligible for inclusion if discrimination was measured in any context other than work (e.g., school, healthcare, restaurants). All other inclusion criteria and procedures were identical to those used for the workplace discrimination analyses. There were a total of 117 studies and 122 independent samples measuring non-work discrimination with a total sample size of 57,901 (initial agreement between the two coders was 90% across all coded variables).

## 2.5 | Meta-analytic procedures

Using Hunter and Schmidt's (2004) meta-analytic approach, we corrected meta-analytic correlations for sampling error and unreliability in both the predictor and criterion measures using artifact distributions. We only used internal consistency estimates in creating the artifact distributions, which we report in Table 1. In addition to reporting point estimates for corrected correlations, we also report 80% credibility intervals and 95% confidence intervals to describe the variability in these estimates. Credibility intervals indicate the variability in correlations across studies with wider credibility intervals suggesting the possibility of moderating effects (Hunter & Schmidt, 2004). We also estimated 95% confidence intervals around mean corrected effect sizes to assess the accuracy of these population estimates given sampling error. We tested moderation by calculating separate meta-analyses for each moderator condition and computing a *z*-statistic to identify statistically significant differences in mean effect sizes using formulas taken from Raju and Brand (2003).

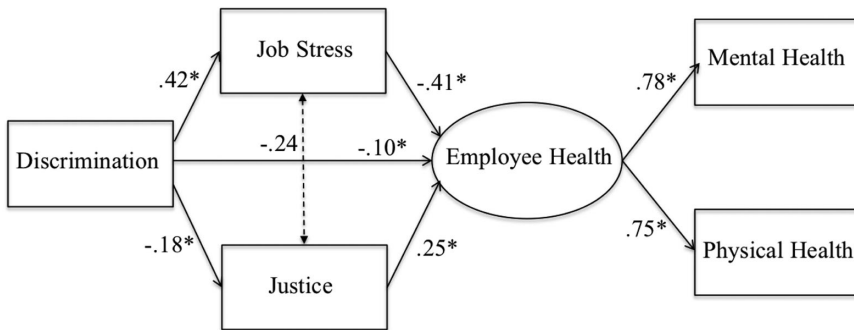
## 2.6 | Mediation analyses

To test job stress and justice as mediators of the relationships between workplace discrimination and outcomes, we estimated two meta-analytic path models in LISREL 8.8 (Jöreskog & Sörbom, 2006). The models are displayed in Figures 1 and 2, respectively. As displayed in Figure 1, the first model includes job satisfaction, affective commitment, turnover intentions, OCB, and CWB. Drawing from Podsakoff et al. (2007), we included paths from job satisfaction to affective commitment and from both satisfaction and commitment to turnover intentions. Further, previous work supports the inclusion of paths from job satisfaction and commitment to OCB and CWB (Dalal, 2005; Harrison, Newman, & Roth, 2006). Figure 2 displays job stress and justice as mediators of the relationship between discrimination and employee health. We treated mental and physical health as indicators of overall employee health given that these variables are strongly related both conceptually and empirically (Darr & Johns, 2008), and



**FIGURE 1** Discrimination predicting job attitudes, turnover intentions, OCB, and CWB through job stress and justice

Note: \* $p < .05$ ; Standardized estimates. To achieve empirical identifications, the job satisfaction and organizational commitment factor loadings were constrained to equality. Correlations were allowed between the errors for job stress and justice and between the errors for OCB and CWB.  $N = 3,746$ ;  $\chi^2_{[2]} = 18.76, p < .05$ ; CFI = 1.00; TLI = .98; RMSEA = .05, SRMR = .009.



**FIGURE 2** Discrimination predicting employee health through job stress and justice

Note: \* $p < .05$ ; Standardized estimates. To achieve empirical identifications, the mental health and physical health factor loadings were constrained to equality. Correlations were allowed between the errors for job stress and justice.  $N = 16,235$ ;  $\chi^2_{[3]} = 404.86, p < .05$ ; CFI = .98; TLI = .93; RMSEA = .09, SRMR = .04.

have been combined in past research (Cole, Walter, Bedeian, & O'Boyle, 2012; Jones et al., 2016; Podsakoff et al., 2007).

In order to analyze the proposed path models, we constructed meta-analytic correlation matrices including all study variables. The intercorrelations between the outcome variables were drawn from the most recent and/or largest available meta-analyses; we report these correlations and their sources in Tables 2 and 3; all are corrected for unreliability in the predictor and criterion. The correlations taken from Dowden and Tellier (2004) and Cohen-Charash and Spector (2001) were not originally corrected for unreliability and were therefore corrected using the artifact distributions from this study. The meta-analytic correlation between job stress and CWB was not available in the literature, so we

**TABLE 2** Meta-analytic intercorrelations among discrimination, job stress, job attitudes, and turnover intentions

	1	2	3	4	5	6	7	8
1. Workplace discrimination								
2. Job satisfaction	-.46							
<i>k</i> studies	66							
<i>N</i> total observations	73,067							
3. Affective commitment	-.39	.60 <sup>a</sup>						
<i>k</i> studies	30	112						
<i>N</i> total observations	25,371	39,187						
4. Turnover intentions	.27	-.58 <sup>b</sup>	-.51 <sup>c</sup>					
<i>k</i> studies	44	88	24					
<i>N</i> total observations	38,130	35,494	8,724					
5. OCB	.01	.28 <sup>a</sup>	.25 <sup>a</sup>	-.22 <sup>d</sup>				
<i>k</i> studies	9	32	42	90				
<i>N</i> total observations	4,914	16,348	10,747	26,510				
6. CWB	.42	-.37 <sup>e</sup>	-.36 <sup>e</sup>	.29 <sup>f</sup>	-.39 <sup>e</sup>			
<i>k</i> studies	6	25	22	17	49			
<i>N</i> total observations	4,781	6,106	5,582	8,049	16,721			
7. Job stress	.42	-.54 <sup>g</sup>	-.39 <sup>g</sup>	.30 <sup>g</sup>	-.09 <sup>h</sup>	.48		
<i>k</i> studies	17	11	6	7	—	4		
<i>N</i> total observations	14,388	5,294	5,655	1,880	1,888	766		
8. Justice	-.18	.49 <sup>i</sup>	.51 <sup>i</sup>	-.40 <sup>j</sup>	.27 <sup>i</sup>	-.25 <sup>e</sup>	-.32 <sup>j</sup>	
<i>k</i> studies	13	8	13	4	7	11	13	
<i>N</i> total observations	8,023	2,205	2,355	1,067	1,688	2,130	—	
9. Negative affectivity	.29	-.37 <sup>j</sup>	-.31 <sup>k</sup>	.29 <sup>k</sup>	-.14 <sup>k</sup>	.41 <sup>e</sup>	.52 <sup>k</sup>	-.20 <sup>k</sup>
<i>k</i> studies	8	145	24	36	13	23	11	22
<i>N</i> total observations	4,547	52,120	10,287	10,563	2,930	4,101	2,059	9,382

Note: All correlations are corrected for unreliability in both the predictor and the criterion variables. The superscripts indicate the source of the meta-analytic correlations as follows:

<sup>a</sup>Harrison et al. (2006)

<sup>b</sup>Tett and Meyer (1993)

<sup>c</sup>Meyer, Stanley, Herscovitch, and Topolnysky (2002)

<sup>d</sup>Podsakoff, Whiting, Podsakoff, and Blume (2009)

<sup>e</sup>Dalal (2005)

<sup>f</sup>Carpenter and Berry (2017)

<sup>g</sup>Dowden and Tellier (2004)

<sup>h</sup>Kaplan, Bradley, Luchman, and Haynes (2009)

<sup>i</sup>Cohen-Charash and Spector (2001)

<sup>j</sup>Robbins et al. (2012)

<sup>k</sup>Ng and Sorensen (2009)

All meta-analytic estimates that appear without a superscript are original analyses.

calculated an original meta-analytic effect size using the same meta-analytic procedures described earlier (results are reported in Table 2).

Path analysis typically assumes a single sample size for all correlations included in the matrix (Landis, 2013). However, given that the correlations used in the current analyses were derived from various sources, every cell in our matrix had a different sample size. Following best practice recommendations, we used the harmonic mean sample size of each meta-matrix as the sample size for the corresponding path analysis to limit the influence of extreme sample sizes on

**TABLE 3** Meta-analytic intercorrelations among discrimination, job stress, and employee health

	1	2	3	4	5
1. Workplace discrimination					
2. Mental health	-.29				
<i>k</i> studies	30				
<i>N</i> total observations	42,819				
3. Physical health	-.19	.58 <sup>a</sup>			
<i>k</i> studies	21	—			
<i>N</i> total observations	39,511	—			
4. Job stress	.42	-.40 <sup>b</sup>	-.32 <sup>b</sup>		
<i>k</i> studies	17	51	43		
<i>N</i> total observations	14,388	—	—		
5. Justice	-.18	.28 <sup>c</sup>	.16 <sup>c</sup>	-.32 <sup>c</sup>	
<i>k</i> studies	13	8	3	13	
<i>N</i> total observations	8,023	—	—	—	
6. Negative affectivity	.29	-.69	-.40	.52 <sup>d</sup>	-.20 <sup>e</sup>
<i>k</i> studies	8	65	27	11	22
<i>N</i> total observations	4,547	17,521	6,452	2,059	9,382

Note: All correlations are corrected for unreliability in both the predictor and the criterion variables. The superscripts in the table indicate the source of the meta-analytic correlations as follows:

<sup>a</sup>Darr and Johns (sample size not provided; 2008)

<sup>b</sup>Yu, Lin, Chen, Wang, and Chiu (sample size not provided; 2007)

<sup>c</sup>Robbins et al. (2012)

<sup>d</sup>Ng and Sorensen (2009)

<sup>e</sup>Kaplan et al. (2009).

All meta-analytic estimates that appear without a superscript are original analyses.

estimates of model fit and parameter standard errors (Landis, 2013; Viswesvaran & Ones, 1995). We evaluated model fit using the chi-square index ( $\chi^2$ ), root mean square error of approximation (RMSEA), comparative fit index (CFI), Tucker-Lewis index (TLI), and standardized root mean square residual (SRMR).

## 2.7 | Incremental validity

We conducted incremental validity analyses to estimate the unique effect of workplace discrimination on job and health outcomes after controlling for trait negative affectivity. We constructed the correlation matrices—shown in Tables 2 and 3—using meta-analytic estimates from this paper in combination with previously published meta-analytic estimates. For intercorrelations between trait negative affectivity and the outcome variables, we obtained estimates from the largest available published meta-analyses (i.e., Kaplan et al., 2009; Ng & Sorensen, 2009). Because there were no available meta-analytic estimates for the relationships between negative affectivity and both mental health and physical health, we conducted original meta-analyses to estimate these relationships. To locate studies, we searched PsycINFO using the following keywords: *negative affect*, *negative affectivity*, *PANAS*, *anxiety*, *depression*, *well-being*, *distress*, *psychological health*, *physical health*, *physical symptoms*, and *health*. We excluded studies that measured state negative affect (e.g., using timeframes such as “in the present moment” and “in the past week”), as opposed to trait negative affect. The searches yielded a total of 82 independent samples drawn from 68 studies ( $N = 21,617$ ). All studies were double-coded, and initial agreement was 93% across all coded variables. We conducted regression analyses using the minimum meta-analytic  $N$  to be conservative in estimating the significance of our effects.

**TABLE 4** Meta-analytic results for the outcomes of perceived workplace discrimination

Variable	<i>k</i>	<i>N</i>	<i>r</i>	$\hat{\rho}$	<i>SD<sub>ρ</sub></i>	95% CI		80% CV		% variance
						Lower	Upper	Lower	Upper	
Job stress	17	14,388	.36	.42	.17	.35	.49	.21	.64	4.07
Justice	13	8,023	-.16	-.18	.17	-.26	-.10	-.39	.03	6.44
Negative affectivity	8	4,547	.25	.29	.11	.22	.36	.15	.43	14.31
Job satisfaction	67	207,658	-.23	-.26	.20	-.30	-.22	-.51	.00	0.96
Outlier removed	66	73,067	-.40	-.46	.22	-.51	-.41	-.74	-.18	1.68
Affective commitment	30	25,371	-.34	-.39	.12	-.43	-.35	-.54	-.24	8.40
Turnover intentions	44	38,130	.23	.27	.12	.23	.30	.11	.42	8.68
Organizational citizenship behavior	9	4,914	.01	.01	.14	-.07	.09	-.17	.19	11.35
Counterproductive work behavior	6	4,781	.36	.42	.21	.27	.56	.15	.68	2.88
Mental health	30	42,819	-.26	-.29	.13	-.33	-.25	-.46	-.12	4.45
Physical health	21	39,511	-.17	-.19	.10	-.23	-.15	-.32	-.06	6.18

Note: *k* = the number of independent samples; *N* = sample size; *r* = sample size-weighted mean uncorrected correlation;  $\hat{\rho}$  = mean corrected correlation (corrected for unreliability in the predictor and the criterion); *SD<sub>ρ</sub>* = standard deviation of the corrected correlation; 95% CI = 95% confidence interval constructed around  $\hat{\rho}$ ; 95% CV = 95% credibility interval constructed around  $\hat{\rho}$ ; % variance = percent of variance accounted for by sampling error and corrected artifacts.

### 3 | RESULTS

#### 3.1 | Direct relationships between workplace discrimination and employee outcomes

We report the meta-analytic relationships between perceived workplace discrimination and outcomes in Table 4. Results show that discrimination is positively related to job stress ( $\hat{\rho} = .42$ ) and negatively related to justice perceptions ( $\hat{\rho} = -.18$ ). Although workplace discrimination was negatively related to job satisfaction ( $\hat{\rho} = -.26$ ), it is noteworthy that this estimate includes one unusually large sample (*N* = 134,591; King, Dawson, Kravitz, & Gulick, 2012). With this study excluded, the discrimination–job satisfaction relationship increases from  $\hat{\rho} = -.26$  to  $\hat{\rho} = -.46$ . Further, results showed that perceived workplace discrimination was negatively related to affective commitment ( $\hat{\rho} = -.39$ ), positively related to turnover intentions ( $\hat{\rho} = .27$ ), and positively related to CWB ( $\hat{\rho} = .42$ ). However, contrary to expectations, perceived discrimination was not meaningfully related to OCB ( $\hat{\rho} = .01$ ). Last, discrimination was significantly related to both mental health ( $\hat{\rho} = -.29$ ) and physical health ( $\hat{\rho} = -.19$ ).

#### 3.2 | Publication bias

One concern when conducting meta-analyses is the influence that publication bias has on estimates. To test for publication bias, we conducted trim and fill analyses using Comprehensive Meta-Analysis 2.0 (Borenstein, Hedges, Higgins, & Rothstein, 2005). Trim and fill uses an iterative process in which extreme effect sizes are removed from an asymmetric distribution and then reimputed into the distribution until symmetry is achieved (Duval & Tweedie, 2000). Meta-analytic estimates are then recalculated including the imputed effect sizes. No publication bias is indicated when adjusted meta-analytic effect sizes show minimal change, moderate publication bias is indicated when adjusted effect sizes show a change in magnitude but do not change the conclusion of the research, and severe publication bias is indicated when the change in magnitude is sufficient enough to change the conclusion of the research (Kepes, Banks, McDaniel, & Whetzel, 2012).

Results indicate no publication bias for the reported relationships between discrimination and job stress, negative affectivity, job satisfaction, affective commitment, counterproductive work behavior, mental health, and physical health (i.e., no studies were imputed). However, the results showed evidence of publication bias for the

relationships between discrimination and justice (four studies were imputed;  $\Delta\hat{\rho} = .042$ ), turnover intentions (four studies were imputed;  $\Delta\hat{\rho} = .008$ ), and OCB (four studies were imputed;  $\Delta\hat{\rho} = .058$ ). We note that the adjusted meta-analytic estimates do not change the interpretations of the estimated relationships between discrimination and both justice and turnover intentions; thus, the publication bias appears to be moderate. OCB demonstrated a slightly larger difference; however, the recalculated estimate still suggests a modest relationship between discrimination and OCB.

### 3.3 | Mediation results for workplace discrimination

To assess whether job stress and justice perceptions mediate the relationships between workplace discrimination and outcomes, we tested two mediated path models. Our first mediated model examined whether discrimination is associated with job satisfaction, affective commitment, turnover intentions, OCB, and CWB through job stress and justice perceptions (see Fig. 1). Given the impact of the large sample outlier on the job discrimination–job satisfaction relationship, these analyses were run excluding the outlier. This model demonstrated adequate fit to the data ( $\chi^2_{[2]} = 18.76$ ,  $p < .05$ ; CFI = 1.00; TLI = .98; RMSEA = .05, SRMR = .009). An examination of the path estimates, shown in Figure 1, indicates that discrimination is positively related to job stress, and job stress is negatively related to job satisfaction, affective commitment, and CWB. However, job stress was not significantly related to turnover intentions or OCB. We tested the significance of the indirect effects for the outcomes (i.e., job satisfaction, commitment, turnover intentions, OCB, and CWB) using the Monte Carlo method for testing mediation (Preacher & Hayes, 2008). Results (20,000 iterations) indicate that, with job stress as the mediating mechanism, the indirect effects for job satisfaction ( $-.18$ ; 95% CI  $[-.19, -.16]$ ), affective commitment ( $-.06$ ; 95% CI  $[-.08, -.05]$ ), and CWB (.15; 95% CI  $[.14, .17]$ ) were significant. However, the indirect effect of discrimination through job stress was not significant for turnover intentions (.00; 95% CI  $[-.02, .02]$ ) or OCB (.01; 95% CI  $[-.01, .02]$ ).

Examining the paths involving justice as a mediator shows that discrimination was negatively related to justice perceptions and justice perceptions were positively related to both job satisfaction and affective commitment. Justice perceptions also demonstrated a positive relationship with OCB and negative relationships with turnover intentions and CWB. With justice perceptions as the mediating mechanism, the indirect effects were significant for job satisfaction ( $-.08$ ; 95% CI  $[-.09, -.06]$ ), affective commitment ( $-.06$ ; 95% CI  $[-.07, -.05]$ ), turnover intentions (.01; 95% CI  $[.01, .02]$ ), OCB ( $-.02$ ; 95% CI  $[-.03, -.01]$ ), and CWB (.03; 95% CI  $[.02, .04]$ ). Further, the direct paths between discrimination and job satisfaction, affective commitment, OCB, and CWB were significant, suggesting partial mediation of these relationships. Interestingly, and contrary to expectations, the direct effect of discrimination on OCB was positive (we discuss this in more detail in Section 4).

The second model, displayed in Figure 2, involved discrimination predicting employee health through job stress and justice perceptions. This model demonstrated adequate fit to the data ( $\chi^2_{[3]} = 404.86$ ,  $p < .05$ ; CFI = .98; TLI = .93; RMSEA = .09, SRMR = .04). The indirect effects via job stress ( $-.17$ ; 95% CI  $[-.18, -.16]$ ) and justice were both significant ( $-.05$ ; 95% CI  $[-.05, -.04]$ ). The direct effect between workplace discrimination and employee health was also significant, suggesting that job stress and justice partially mediate the relationship between workplace discrimination and employee health. Taken together, these findings support our expectation that job stress and justice perceptions serve as mediators of the relationships between workplace discrimination and job attitudes, work behaviors, and employee health (Hypotheses 1 and 2). Given that the indirect effects through job stress were stronger than those through justice for several outcomes, the mediation analyses also suggest that job stress may play a more significant role in the relationship between discrimination and health outcomes than justice perceptions.

### 3.4 | Moderation results for workplace discrimination

#### 3.4.1 | Formality of workplace discrimination

As shown in Table 5—and consistent with Hypothesis 3—interpersonal discrimination displayed a stronger relationship ( $z = -49.76$ ,  $p < .05$ ) with job satisfaction ( $\hat{\rho} = -.66$ ) than formal discrimination ( $\hat{\rho} = -.35$ ). Interpersonal discrimination

**TABLE 5** Moderation results for interpersonal and formal discrimination

Variable	k	N	r	$\hat{\rho}$	SD $\rho$	95% CI		80% CV		% variance	z
						Lower	Upper	Lower	Upper		
Job stress											
Formal	2	2,044	.34	.39	.13	.23	.55	.22	.56	5.66	-8.21*
Interpersonal	4	5,014	.46	.53	.16	.40	.67	.33	.74	2.59	
Job satisfaction											
Formal	12	16,483	-.30	-.35	.13	-.41	-.28	-.52	-.18	4.33	-49.76*
Interpersonal	24	23,960	-.58	-.66	.24	-.75	-.58	-.97	-.36	0.99	
Affective commitment											
Formal	2	1,044	-.15	-.17	.00	-.23	-.11	-.17	-.17	100.00	0.00
Interpersonal	6	2,067	-.15	-.17	.00	-.21	-.13	-.17	-.17	100.00	
Turnover intentions											
Formal	5	9,483	.24	.28	.00	.26	.30	.28	.28	100.00	0.00
Interpersonal	13	5,135	.24	.28	.09	.23	.33	.17	.39	28.82	
Mental health											
Formal	4	10,346	-.25	-.29	.10	-.38	-.20	-.42	-.16	3.94	-12.09*
Interpersonal	11	16,539	-.33	-.38	.08	-.42	-.34	-.48	-.28	10.37	
Physical health											
Formal	6	11,101	-.15	-.17	.04	-.20	-.14	-.22	-.12	30.15	-11.96*
Interpersonal	10	19,992	-.21	-.24	.11	-.30	-.18	-.38	-.10	4.84	

Note: k = the number of independent samples; N = sample size; r = sample size-weighted mean uncorrected correlation;  $\hat{\rho}$  = mean corrected correlation (corrected for unreliability in the predictor and the criterion); SD $\rho$  = standard deviation of the corrected correlation; 95% CI = 95% confidence interval constructed around  $\hat{\rho}$ ; 95% CV = 95% credibility interval constructed around  $\hat{\rho}$ ; % var = percent of variance accounted for by sampling error and corrected artifacts; z = z-statistic calculated using formulas from Raju and Brand (2003); \*p < .05.

also exhibited stronger relationships with job stress (interpersonal:  $\hat{\rho} = .53$ ; formal:  $\hat{\rho} = .39$ ;  $z = -8.21$ ,  $p < .05$ ), mental health (interpersonal:  $\hat{\rho} = -.38$ ; formal:  $\hat{\rho} = -.29$ ;  $z = -12.09$ ,  $p < .05$ ), and physical health (interpersonal:  $\hat{\rho} = -.24$ ; formal:  $\hat{\rho} = -.17$ ;  $z = -11.96$ ,  $p < .05$ ) than formal discrimination. However, interpersonal and formal discrimination were equally related to both affective commitment (interpersonal:  $\hat{\rho} = -.17$ ; formal:  $\hat{\rho} = -.17$ ;  $z = 0.00$ , n.s.) and turnover intentions (interpersonal:  $\hat{\rho} = .28$ ; formal:  $\hat{\rho} = .28$ ;  $z = 0.00$ , n.s.). Thus, these findings suggest that interpersonal discrimination is equally or more detrimental than formal discrimination.

### 3.4.2 | Exposure to workplace discrimination

As shown in Table 6, our results were contrary to Hypothesis 4; affective commitment was more strongly related ( $z = -14.61$ ,  $p < .05$ ) to observed discrimination ( $\hat{\rho} = -.45$ ) than experienced discrimination ( $\hat{\rho} = -.32$ ). Turnover intentions (observed:  $\hat{\rho} = .39$ ; experienced:  $\hat{\rho} = .23$ ;  $z = -18.97$ ,  $p < .05$ ) and mental health (observed:  $\hat{\rho} = -.48$ ; experienced:  $\hat{\rho} = -.29$ ;  $z = -9.00$ ,  $p < .05$ ) were also more strongly related to observed discrimination than experienced discrimination. Interestingly, the OCB-discrimination relationships were in opposing directions across the types of exposure; OCB was negatively related to observed discrimination ( $\hat{\rho} = -.15$ ), as hypothesized, but positively related to experienced discrimination ( $\hat{\rho} = .07$ ). In addition, the z-statistic indicates that these relationships were significantly different from each other ( $z = -5.07$ ,  $p < .05$ ).

Observed discrimination ( $\hat{\rho} = -.36$ ) also displayed a stronger relationship with job satisfaction than experienced discrimination ( $\hat{\rho} = -.25$ ;  $z = -20.10$ ,  $p < .05$ ). However, this relationship was reversed when the large sample size study (King et al., 2012) was removed (experienced discrimination:  $\hat{\rho} = -.50$ ;  $z = 27.26$ ,  $p < .05$ ). Job stress

**TABLE 6** Moderation results for experienced and observed discrimination

Variable	k	N	r	$\hat{\rho}$	SD $\rho$	95% CI		80% CV		% variance	z
						Lower	Upper	Lower	Upper		
Job stress											
Experienced	10	11,540	.38	.44	.16	.36	.53	.24	.65	3.19	13.14*
Observed	2	857	.18	.20	.09	.08	.33	.09	.32	28.17	
Job satisfaction											
Experienced	41	185,062	-.22	-.25	.20	-.30	-.19	-.51	.01	0.63	-20.10**
Outlier removed	40	50,471	-.44	-.50	.24	-.57	-.44	-.82	-.19	1.12	27.26*
Observed	9	15,729	-.32	-.36	.08	-.41	-.31	-.47	-.26	8.07	
Affective commitment											
Experienced	17	7,346	-.28	-.32	.12	-.37	-.27	-.47	-.17	16.43	-14.61*
Observed	5	14,555	-.39	-.45	.06	-.50	-.40	-.53	-.37	7.88	
Turnover intentions											
Experienced	29	27,244	.20	.23	.12	.19	.27	.08	.38	9.12	-18.97*
Observed	5	6,806	.34	.39	.08	.32	.46	.29	.50	10.39	
Organizational citizenship behavior											
Experienced	6	3,305	.06	.07	.14	-.03	.17	-.11	.24	11.53	-5.07*
Observed	2	890	-.13	-.15	.00	-.20	-.11	-.15	-.15	100.00	
Mental health											
Experienced	22	38,647	-.25	-.29	.13	-.34	-.24	-.45	-.12	3.64	-9.00*
Observed	3	963	-.42	-.48	.20	-.69	-.27	-.74	-.22	6.16	

Note: k = the number of independent samples; N = sample size; r = sample size-weighted mean uncorrected correlation;  $\hat{\rho}$  = mean corrected correlation (corrected for unreliability in the predictor and the criterion); SD $\rho$  = standard deviation of the corrected correlation; 95% CI = 95% confidence interval constructed around  $\hat{\rho}$ ; 95% CV = 95% credibility interval constructed around  $\hat{\rho}$ ; % var = percent of variance accounted for by sampling error and corrected artifacts; z = z statistic calculated using formulas from Raju and Brand (2003); \*p < .05.

displayed the opposite result, demonstrating a stronger relationship with experienced discrimination ( $\hat{\rho} = .44$ ) than observed discrimination ( $\hat{\rho} = .20$ ;  $z = 13.14$ ,  $p < .05$ ). Taken together, Hypothesis 4 was not supported. However, we note that some of our analyses are based on a limited number of studies and should be interpreted with caution.

### 3.4.3 | Target of workplace discrimination

To address our first research question, we ran analyses to determine if discrimination–outcome relationships differ based on the target of discrimination (i.e., race, sex, sexual orientation, or age). As reported in Table 7, results show that sexual orientation discrimination exhibited the strongest relationships with job stress ( $\hat{\rho} = .27$ ), affective commitment ( $\hat{\rho} = -.46$ ), and physical health ( $\hat{\rho} = -.32$ ) relative to the other examined discrimination targets. Similarly, sexual orientation discrimination ( $\hat{\rho} = -.32$ ) and race discrimination ( $\hat{\rho} = .31$ ) were more strongly related to mental health than discrimination targeted at sex ( $\hat{\rho} = -.24$ ) or age ( $\hat{\rho} = -.23$ ), and turnover intention was more strongly related to sexual orientation ( $\hat{\rho} = .27$ ) and sex discrimination ( $\hat{\rho} = .28$ ) than either race ( $\hat{\rho} = .23$ ) or age discrimination ( $\hat{\rho} = .14$ ). Last, results showed that race discrimination had the strongest relationship with job satisfaction ( $\hat{\rho} = -.55$ ) and race discrimination ( $\hat{\rho} = .17$ ) was more strongly related to justice perceptions than sex discrimination ( $\hat{\rho} = .14$ ). Taken together, these results suggest several differences in discrimination–outcome relationships based on the target of discrimination, with one of the more consistent differences being that sexual orientation discrimination was more strongly related to several outcomes relative to other targets.



**TABLE 7** Moderation results for target of discrimination

Variable	k	N	r	$\hat{\rho}$	$SD_{\hat{\rho}}$	95% CI		80% CV		% variance	z		
						Lower	Upper	Lower	Upper		Sex	LGBT	Age
Job stress													
Race	4	2,749	.09	.10	.12	-.01	.21	-.06	.26	11.36	-6.88*	-10.18*	
Sex	2	2,681	.15	.18	.00	.17	.19	.18	.18	100.00		-5.05*	
LGBT	4	1,423	.23	.27	.13	.15	.39	.10	.43	17.05			
Justice													
Race	3	2,030	-.15	-.17	.16	-.34	.00	-.37	.04	6.30	2.39*		
Sex	5	2,765	-.13	-.14	.17	-.28	.00	-.35	.07	7.19			
Job satisfaction													
Race	20	166,304	-.20	-.23	.20	-.30	-.15	-.48	.03	0.37	-25.49*	-11.00*	-20.89*
Outlier removed	19	31,713	-.48	-.55	.27	-.66	-.45	-.90	-.20	0.61	27.81*	17.49*	26.75*
Sex	12	13,668	-.32	-.37	.12	-.43	-.31	-.53	-.21	5.76		1.66	0.82
LGBT	9	3,185	-.31	-.35	.10	-.42	-.29	-.48	-.23	24.07			-0.65
Age	9	10,228	-.32	-.36	.04	-.39	-.33	-.42	-.31	32.50			
Affective commitment													
Race	8	3,336	-.30	-.35	.13	-.43	-.27	-.51	-.19	14.38	7.44*	-4.45*	1.88
Sex	3	1,973	-.20	-.23	.10	-.34	-.12	-.36	-.09	14.91		-9.10*	-2.58*
LGBT	3	878	-.40	-.46	.09	-.56	-.35	-.57	-.34	28.65			4.85*
Age	2	590	-.26	-.30	.22	-.57	-.02	-.58	-.02	7.48			
Turnover intentions													
Race	10	5,992	.20	.23	.05	.19	.27	.16	.30	41.65	-5.85*	-2.55*	9.63*
Sex	13	13,378	.24	.28	.08	.24	.32	.18	.38	15.35		0.67	17.41*
LGBT	5	1,658	.23	.27	.05	.21	.33	.20	.34	55.31			8.44*
Age	6	4,659	.12	.14	.16	.03	.25	-.06	.34	6.43			
Mental health													
Race	12	21,594	-.27	-.31	.14	-.38	-.24	-.49	-.13	2.88	11.50*	0.60	7.00*
Sex	9	13,417	-.21	-.24	.14	-.32	-.16	-.42	-.06	3.83		-4.73*	0.86
LGBT	6	1,308	-.29	-.32	.11	-.41	-.23	-.46	-.19	30.73			4.62*
Age	4	2,386	-.20	-.23	.00	-.26	-.19	-.23	-.23	100.00			
Physical health													
Race	8	19,051	-.22	-.26	.07	-.30	-.21	-.35	-.17	9.41	12.36*	-2.63*	
Sex	4	9,062	-.15	-.18	.05	-.23	-.13	-.25	-.11	16.54		-6.09*	
LGBT	3	732	-.27	-.32	.09	-.43	-.21	-.43	-.20	36.47			

Note: LGBT refers to measures of discrimination on the basis of sexual orientation; k = the number of independent samples; N = sample size; r = sample size-weighted mean uncorrected correlation;  $\hat{\rho}$  = mean corrected correlation (corrected for unreliability in the predictor and the criterion);  $SD_{\hat{\rho}}$  = standard deviation of the corrected correlation; 95% CI = 95% confidence interval constructed around  $\hat{\rho}$ ; 95% CV = 95% credibility interval constructed around  $\hat{\rho}$ ; % var = percent of variance accounted for by sampling error and corrected artifacts; z = z statistic calculated using formulas from Raju and Brand (2003); \*p < .05.

### 3.4.4 | Context of discrimination

Our tests of Hypothesis 5 are reported in Table 8. Results showed that the relationships between discrimination and both mental health (work:  $\hat{\rho}$  = -.29; nonwork:  $\hat{\rho}$  = -.27; z = 5.45, p < .05) and physical health (work:  $\hat{\rho}$  = -.19; nonwork:  $\hat{\rho}$  = -.15; z = 8.26, p < .05) were significantly different across work and nonwork contexts. However, despite statistically

**TABLE 8** Meta-analytic results for work discrimination versus nonwork discrimination

Variable	k	N	r	$\hat{\rho}$	SD $\rho$	95% CI		80% CV		% variance	z
						Lower	Upper	Lower	Upper		
Mental health											
Workplace discrimination	30	42,819	-.26	-.29	.13	-.33	-.25	-.46	-.12	4.45	5.45*
Nonwork discrimination	115	53,194	-.24	-.27	.13	-.29	-.25	-.44	-.11	13.11	
Experienced	86	32,438	-.27	-.30	.11	-.33	-.28	-.45	-.16	18.38	19.48*
Observed	11	4,798	-.14	-.16	.11	-.23	-.10	-.31	-.02	17.72	
Physical health											
Workplace discrimination	21	39,511	-.17	-.19	.10	-.23	-.15	-.32	-.06	6.18	8.26*
Nonwork discrimination	25	11,131	-.13	-.15	.14	-.20	-.10	-.34	.03	12.40	

Note: k = the number of independent samples; N = sample size; r = sample size-weighted mean uncorrected correlation;  $\hat{\rho}$  = mean corrected correlation (corrected for unreliability in the predictor and the criterion); SD $\rho$  = standard deviation of the corrected correlation; 95% CI = 95% confidence interval constructed around  $\hat{\rho}$ ; 95% CV = 95% credibility interval constructed around  $\hat{\rho}$ ; % var = percent of variance accounted for by sampling error and corrected artifacts; z = z statistic calculated using formulas from Raju and Brand (2003); \*  $p < .05$ .

significant differences, it is noteworthy that the examined work and nonwork discrimination–health relationships are very similar in magnitude. Thus, contrary to our hypothesis, there does not appear to be an appreciable difference in the magnitude of discrimination–outcome relationships in work versus nonwork domains.

To explore this unexpected finding further, we tested to see if there are differences in observed versus experienced discrimination in work versus nonwork settings. Although too few studies were available to test these differences for physical health as an outcome, we were able to do so with mental health. Interestingly, results regarding mental health showed the opposite pattern in nonwork settings relative to work settings. Specifically, results revealed that mental health was more strongly related ( $z = 19.48, p < .05$ ) to experienced discrimination ( $\hat{\rho} = -.30$ ) than observed discrimination ( $\hat{\rho} = -.16$ ) in nonwork settings, whereas the opposite was true in work settings. Though limited to a single outcome (i.e., mental health), this suggests that the relative impact of experienced and observed discrimination differs across work and nonwork contexts. We revisit this finding in Section 4.

### 3.4.5 | Measurement of workplace discrimination

To test Hypothesis 6, we next compared discrimination–outcome relationships based on the use of broad versus specific measures of discrimination. As reported in Table 9, results showed that workplace discrimination displayed a stronger relationship with job satisfaction when measured broadly ( $\hat{\rho} = -.59$ ) than when specific measures were used ( $\hat{\rho} = -.20; z = -101.76, p < .05$ ). This pattern of relationships held when the outlier (King et al., 2012) was removed from the specific measure condition ( $z = -46.16, p < .05$ ). This was also true for turnover intentions (broad measures:  $\hat{\rho} = .38$ ;

**TABLE 9** Moderation results for specific measures and broad measures of discrimination

Variable	k	N	r	$\hat{\rho}$	SD $\rho$	95% CI		80% CV		% variance	z
						Lower	Upper	Lower	Upper		
Negative affectivity											
Specific measures	4	1,437	.18	.20	.00	.17	.24	.20	.20	100.00	-10.26*
Broad measures	3	2,481	.34	.39	.01	.36	.43	.38	.40	92.24	
Job satisfaction											
Specific measures	51	175,946	-.18	-.20	.11	-.23	-.17	-.34	-.06	3.02	-101.76*
Outlier removed	50	41,355	-.32	-.37	.11	-.40	-.34	-.51	-.23	9.76	-46.16*
Broad measures	15	30,851	-.52	-.59	.26	-.71	-.48	-.92	-.26	0.51	
Affective commitment											
Specific measures	26	18,301	-.35	-.40	.14	-.45	-.35	-.58	-.22	7.25	4.56*
Broad measures	4	7,203	-.31	-.36	.03	-.39	-.33	-.39	-.32	43.55	
Turnover intentions											
Specific measures	37	28,926	.20	.24	.12	.20	.27	.09	.39	10.36	-18.07*
Broad measures	6	8,343	.32	.38	.06	.33	.42	.29	.46	16.16	
Mental health											
Specific measures	23	25,902	-.22	-.25	.14	-.30	-.20	-.42	-.07	5.39	-20.24*
Broad measures	3	15,893	-.33	-.37	.08	-.45	-.29	-.47	-.27	3.21	
Physical health											
Specific measures	16	20,824	-.14	-.16	.07	-.19	-.12	-.25	-.06	15.19	-13.88*
Broad measures	4	18,196	-.20	-.23	.11	-.33	-.14	-.38	-.09	2.15	

Note: k = the number of independent samples; N = sample size; r = sample size-weighted mean uncorrected correlation;  $\hat{\rho}$  = mean corrected correlation (corrected for unreliability in the predictor and the criterion); SD $\rho$  = standard deviation of the corrected correlation; 95% CI = 95% confidence interval constructed around  $\hat{\rho}$ ; 95% CV = 95% credibility interval constructed around  $\hat{\rho}$ ; % var = percent of variance accounted for by sampling error and corrected artifacts; z = z statistic calculated using formulas from Raju and Brand (2003); \* $p < .05$ .

**TABLE 10** Incremental validity of discrimination predicting outcomes over negative affectivity

Outcome	Model 1		Model 2			
	$\beta$	$R^2$	$\beta_1$	$\beta_2$	$R^2$	$\Delta R^2$
Job stress	.52*	.270*	.43*	.29*	.350*	.079*
Justice	-.20	.040*	-.16*	-.13*	.056*	.016*
Job satisfaction	-.37*	.137*	-.26*	-.39*	.273*	.136*
Affective commitment	-.31*	.096*	-.21*	-.33*	.194*	.098*
Turnover intentions	.29*	.084*	.23*	.20*	.122**	.038*
OCB	-.14*	.020*	-.16*	.06*	.022**	.003*
CWB	.41*	.168*	.31*	.33*	.267*	.099*
Mental health	-.69*	.476*	-.66*	-.10*	.485*	.009*
Physical health	-.40	.160*	-.38*	-.08*	.166*	.006*

Note: Standardized regression coefficients. Model 1 includes negative affectivity as a predictor. Model 2 includes negative affectivity ( $\beta_1$ ) and discrimination ( $\beta_2$ ) as predictors. Minimum  $N$  for outcomes: job stress = 2,059; justice = 4,547; job satisfaction = 4,547; affective commitment = 4,547; turnover intentions = 4,547; OCB = 2,930; CWB = 4,101; mental health = 4,547; physical health = 4,547; \* $p < .05$ .

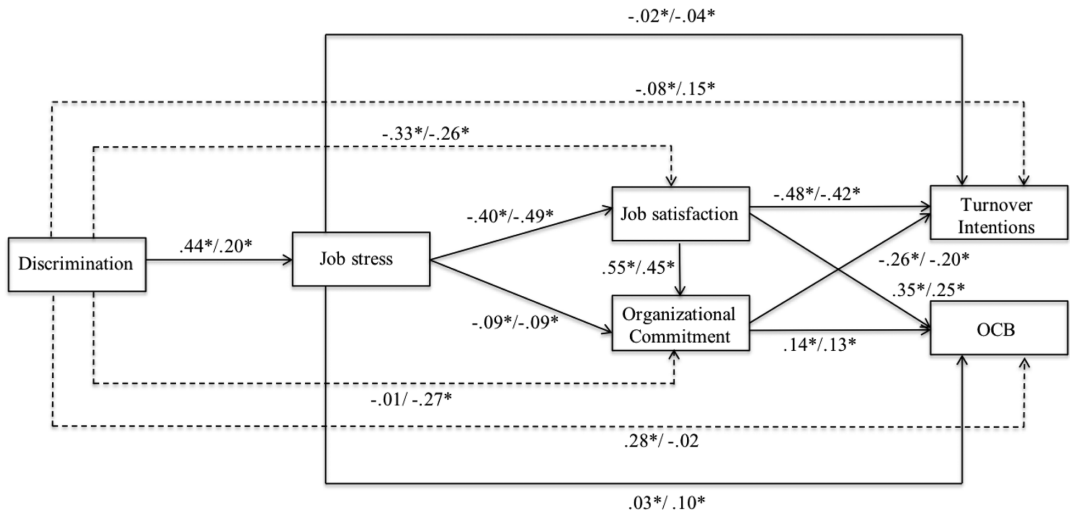
specific measures:  $\hat{\rho} = .24$ ;  $z = -18.07$ ,  $p < .05$ ), mental health (broad measures:  $\hat{\rho} = -.37$ ; specific measures:  $\hat{\rho} = -.25$ ;  $z = -20.24$ ,  $p < .05$ ), physical health (broad measures:  $\hat{\rho} = -.23$ ; specific measures:  $\hat{\rho} = -.16$ ;  $z = -13.88$ ,  $p < .05$ ), and negative affectivity (broad measures:  $\hat{\rho} = .39$ ; specific measures:  $\hat{\rho} = .20$ ;  $z = -10.26$ ,  $p < .05$ ). However, affective commitment was more strongly related to discrimination when specific measures were used ( $\hat{\rho} = .40$ ) in comparison to when broad measures were used ( $\hat{\rho} = .36$ ;  $z = 4.56$ ,  $p < .05$ ). Taken together, our results appear to be largely consistent with Hypothesis 6, though it is noteworthy that comparatively few studies used broad discrimination measures relative to specific.

### 3.5 | Incremental validity results for workplace discrimination

To test our second research question, we examined the incremental validity of workplace discrimination over negative affectivity in predicting individual outcomes. Results, displayed in Table 10, show that negative affectivity explained a statistically significant percentage of variance in each of the examined outcomes of discrimination, particularly for job stress ( $R^2 = .27$ ) and mental health ( $R^2 = .48$ ), confirming the importance of testing the incremental validity of discrimination beyond the effects of negative affectivity. However, workplace discrimination explained a statistically significant percentage of unique variance above and beyond negative affectivity when predicting job stress ( $\Delta R^2 = .079$ ,  $p < .05$ ), justice perceptions ( $\Delta R^2 = .016$ ,  $p < .05$ ), job satisfaction ( $\Delta R^2 = .136$ ,  $p < .05$ ), affective commitment ( $\Delta R^2 = .098$ ,  $p < .05$ ), turnover intentions ( $\Delta R^2 = .038$ ,  $p < .05$ ), and CWB ( $\Delta R^2 = .099$ ,  $p < .05$ ). Although workplace discrimination explained statistically significant, unique variance in OCB ( $\Delta R^2 = .003$ ,  $p < .05$ ), mental health ( $\Delta R^2 = .009$ ,  $p < .05$ ), and physical health ( $\Delta R^2 = .006$ ,  $p < .05$ ) after accounting for negative affectivity, the increases in explained variance for these outcomes were minimal by any practical standard. Thus, in answer to our research question, results appear to show that workplace discrimination predicts above and beyond negative affectivity for most outcomes but predicts only minimally beyond negative affectivity for OCB, mental health, and physical health. Important, we note that negative affectivity accounts for between 49% and 98% of the total explained variance in the discrimination–outcome relationships reported here.

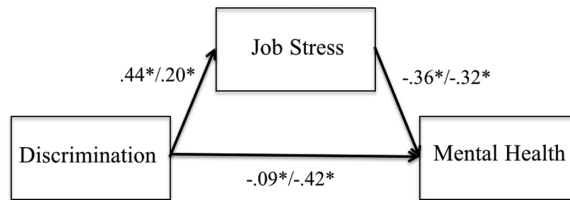
### 3.6 | Supplemental mediation analyses

As stated previously, observed discrimination unexpectedly displayed stronger relationships with several outcomes than experienced discrimination. To further explore potential differences between these forms of discrimination, we conducted separate mediation analyses for experienced and observed discrimination. We excluded perceived justice,



**FIGURE 3** Experienced and observed discrimination predicting job attitudes, turnover intentions, and OCB through job stress

Note:  $p < .05$ ; Standardized estimates. Experienced discrimination estimates are shown before the slash and observed discrimination estimates are shown after the slash. To achieve empirical identifications, the job satisfaction and organizational commitment factor loadings were constrained to equality. Experienced discrimination:  $N = 6,331$ ;  $3.86, p < .05$ ; CFI = 1.00; TLI = 1.00; RMSEA = .02, SRMR = .004. Observed discrimination:  $N = 3,443$ ;  $6.15, p < .05$ ; CFI = 1.00; TLI = .99; RMSEA = .04, SRMR = .007.



**FIGURE 4** Experienced and observed discrimination predicting mental health through job stress

Note:  $p < .05$ ; Standardized estimates. Experienced discrimination estimates are shown before the slash and observed discrimination estimates are shown after the slash. Experienced discrimination:  $N = 17,773$ . Observed discrimination:  $N = 907$ . Models were fully saturated.

CWB, and physical health from the models because separate correlations for experienced and observed discrimination were unavailable. As shown in Figure 3, the model that included job attitudes and behaviors fit adequately for both experienced discrimination ( $\chi^2_{[1]} = 3.86, p < .05$ ; CFI = 1.00; TLI = 1.00; RMSEA = .02, SRMR = .004) and observed discrimination ( $\chi^2_{[1]} = 6.15, p < .05$ ; CFI = 1.00; TLI = .99; RMSEA = .04, SRMR = .007). Experienced discrimination exhibited a significant indirect effect through job stress on job satisfaction ( $-.18$ ; 95% CI  $[-.19, -.16]$ ) and affective commitment ( $-.04$ ; 95% CI  $[-.05, -.03]$ ) but not on turnover intentions ( $.002$ ; 95% CI  $[-.02, .00]$ ) or OCB ( $.00$ ; 95% CI  $[-.02, .02]$ ). Observed discrimination similarly had a significant indirect effect through job stress on job satisfaction ( $-.10$ ; 95% CI  $[-.12, -.08]$ ), affective commitment ( $-.02$ ; 95% CI  $[-.03, -.01]$ ), and OCB ( $.02$ ; 95% CI  $[-.01, .03]$ ) but not on turnover intentions ( $-.01$ ; 95% CI  $[-.02, .00]$ ).

The second model, shown in Figure 4, included mental health as an outcome. The model was fully saturated, which meant that model fit was perfect by definition. There was a significant indirect effect through job stress for both experienced discrimination ( $-.16$ ; 95% CI  $[-.16, -.15]$ ) and observed discrimination ( $-.06$ ; 95% CI  $[-.09, -.04]$ ). When examining the magnitude of the indirect effects of experienced and observed discrimination, the mediation models demonstrated that experienced discrimination displayed a stronger indirect effect through job stress on job satisfaction,

affective commitment, and mental health compared to observed discrimination. This provides preliminary evidence to suggest that experienced and observed discrimination may affect employee outcomes through different mechanisms.

## 4 | DISCUSSION

An expansive and growing body of research has made it clear that workplace discrimination is associated with a host of negative outcomes for individuals. Several published meta-analyses have converged on this general conclusion, providing population estimates of discrimination's associations with outcomes such as individual attitudes and health (Jones et al., 2016; Lee & Ahn, 2011, 2012; Pascoe & Richman, 2009; Schmitt et al., 2014; Triana et al., 2015). However, we sought to advance understanding by focusing on the nature and nuances of discrimination's connections with these outcomes. As such, the purpose of this meta-analysis was to expand understanding of discrimination–outcome relationships and challenge prevailing assumptions by examining the mediating mechanisms, boundary conditions, and a plausible alternative explanation for these relationships. We divide our discussion of this study's findings and their corresponding implications for research and practice into two sections. First, we offer a brief discussion of the results that ultimately confirmed current knowledge regarding workplace discrimination. Second, and more important, we discuss the findings that we believe challenge current knowledge and which provide a platform for future discrimination research to build upon.

### 4.1 | Confirming current knowledge

Although not the focus of our hypotheses and research questions, a necessary part of this paper was to meta-analyze discrimination–outcome relationships that have been reported elsewhere (Jones et al., 2016; Lee & Ahn, 2011, 2012; Pascoe & Richman, 2009; Schmitt et al., 2014; Triana et al., 2015). Though we incorporated a larger and more up-to-date meta-analytic database for testing these relationships, our results largely substantiate what past meta-analyses have already reported. That is, discrimination maintained the expected associations with job stress, perceived justice, job satisfaction, affective commitment, counterproductive work behaviors, and psychological and physical health. Our mediation analyses also confirmed the predominant conceptualization of workplace discrimination as a social stressor that elicits stress responses that subsequently lead to strain reactions (Raver & Nishii, 2010; Sonnentag & Frese, 2003). However, it is noteworthy that our examination of justice as a mediator answers recent calls to move beyond the consideration of stress as an explanatory mechanism for discrimination–outcome relationships (Colella et al., 2017). This extends the existing theory on workplace mistreatment by providing evidence to suggest that both job stress *and* perceived injustice can explain the associations between discrimination and negative work and health-related outcomes.

### 4.2 | Challenging current knowledge

Beyond confirming the direction and magnitude of known discrimination–outcome relationships, our meta-analyses revealed several findings that challenge current understanding of workplace discrimination. First, contrary to our hypothesis, our findings suggest that reports of observed discrimination are often more strongly associated with employee outcomes than reports of experienced discrimination. This challenges the prevailing conceptualization of *who* is adversely affected by discrimination. Organizations may consider discrimination to be an issue affecting a relatively small proportion of their employees; however, the evidence presented here suggests that *all* employees can be negatively impacted by the existence of workplace discrimination as a result of witnessing discrimination against others. Although this finding is contrary to our original expectation, we offer three explanations as to why the second-hand experience of workplace discrimination may be more detrimental in some cases than the first-hand experience. First, it has been suggested that the indirect effects of deviant behaviors (such as discrimination) may be deceptively impactful because employees who were not personally targeted are unaware of how or to what degree they are being affected by the observed events and therefore may be less cognizant of the need to engage in coping responses (Robinson, Wang, &

Kiewitz, 2014). Whereas a person who directly experiences discrimination may mitigate its negative effects by actively engaging in coping mechanisms (Utsey, Ponterotto, Reynolds, & Cancelli, 2000) or increasing the perceived status of the threatened portion of their social identity (Tajfel & Turner, 1986), a person who observes discrimination may not recognize or feel that they have access to the same support sources, which may exacerbate discrimination's effects.

The differences in effect sizes for observed versus experienced discrimination may also be a function of different mediational processes. Although both forms of discrimination should elicit stress as just described, they may do so to differing degrees. As reported in Section 3, we conducted supplemental analyses to assess whether experienced and observed discrimination appear to relate to outcomes via the same mediating mechanisms. Although both experienced and observed discrimination related to job and health outcomes through job stress, the indirect associations appeared to be stronger for experienced discrimination relative to observed. We were unable to test justice in these models, but prior work suggests that personal experiences have a substantially stronger impact on justice perceptions than the experiences of others (Lind et al., 1998). Together, this suggests that the mechanisms through which observed discrimination impacts employees may differ somewhat from the mechanisms that explain the impact of experienced discrimination. These supplemental analyses highlight the need for future work to generate a theory that considers how witnessing discrimination may foment negative outcomes in a different manner than experiencing discrimination.

Alternatively, employees may be more likely to report discrimination when they do not have to identify themselves as a specific target of discrimination (i.e., employees may be more likely to endorse items on observed discrimination measures). Some have argued that the thought of implicating oneself as a target of discrimination may activate a defense mechanism that prevents individuals from wanting to be viewed as a victim (Magley, Hulin, Fitzgerald, & DeNardo, 1999). Supporting this idea, there is a large body of work showing that substantially fewer respondents endorse measures that require self-identification as a victim of mistreatment in comparison to behavioral measures of mistreatment that do not require self-identification (i.e., measures that ask respondents if they have experienced behaviors thought to represent mistreatment without labeling the experiences as such; Ilies, Hauserman, Schwochau, & Stibal, 2003; Nielsen, Matthiesen, & Einarsen, 2010).<sup>2</sup> Our findings regarding observed discrimination may reflect a similar phenomenon wherein respondents are less likely to endorse experienced measures in an attempt to avoid viewing themselves as a victim. This could have the effect of creating range restriction in the meta-analytic correlations involving experienced workplace discrimination given that individuals may feel uncomfortable reporting their personal experience of what is already a low base-rate phenomenon. In any case, it is clear that future research is needed to better understand whether the stronger connections between observed discrimination and some individual outcomes relative to experienced discrimination is a substantive issue or the result of a methodological artifact.

A second key finding that challenges current understanding of discrimination is that interpersonal discrimination was at least equally harmful, and in many cases more harmful, than formal discrimination. The comparable association of interpersonal discrimination with outcomes relative to formal discrimination runs counter to the tendency for organizational and legal interventions to focus their attention on formal discrimination. Even though formal discrimination tends to be higher in severity (i.e., it affects one's access to critical job resources), the prevalence and lack of legal restrictions for interpersonal discrimination may make it more insidious. This finding advances understanding of discrimination by showing that the degree of formality has a meaningful impact on discrimination–outcome relationships and should justify changes to organization discrimination policies and interventions to be more expansive in defining and protecting against discrimination.

Third, examining the context of discrimination revealed that work and nonwork discrimination had similar impacts on employee health, despite the conceptual expectation that workplace discrimination would be more damaging. One reason for this finding may be the difficulty of teasing apart the negative effects of discrimination in one life domain from another. The primary studies that contributed to our meta-analytic comparisons from nonwork settings generally did not provide clear instructions to participants demarcating *where* discrimination was experienced. As such, our results may not be an accurate reflection of the comparative impact of discrimination in work versus nonwork contexts. However, we note that whereas experiencing discrimination was equally harmful in work and nonwork settings, witnessing discrimination had a differential impact across contexts. Witnessing discrimination at work ( $\hat{\rho} = -.48$ ) seems to be more detrimental for an individual's mental health in comparison to witnessing discrimination in other

contexts ( $\hat{\rho} = -.14$ ;  $z = 14.63$ ,  $p < .05$ ). Consistent with the group-value model (Lind & Tyler, 1988), observing discrimination at work may be more harmful because it diminishes an individual's identification with the organization's values. The group-value model takes a social identity perspective to propose that groups provide members with an identity through which they derive personal value (Lind & Tyler, 1988). When organizations are seen as endorsing negative treatment of others, it may, by extension, damage one's self-worth through their identification with the organization. Observing discrimination in nonwork contexts may not engender the same responses because nonwork contexts are less likely to be linked to one's identity.

Fourth, our incremental validity analyses showed that the relationships between discrimination and mental and physical health are substantially reduced after accounting for negative affectivity. This implies that previous analyses may have overestimated the relationships between discrimination and health and calls into question the seemingly unanimous evidence that has been presented by prior meta-analyses. However, the literature cannot currently conclude whether negative affectivity is a confounding variable or if negative affectivity plays a substantive role in the discrimination process—a point which we return to later.

Finally, despite the expectation that workplace discrimination would diminish individual helping behaviors, discrimination was unrelated to OCB. We propose two possible explanations for this unexpected finding. It is possible that the null relationship between discrimination and OCB is due to the fact that the discrimination literature has relied so heavily on self-report measures of OCB (i.e., all of the primary studies included in our meta-analytic database utilized self-report measures of OCB). Employees may inflate their reports of OCB to present themselves more favorably, hampering the ability to detect true decrements that may exist as a result of discrimination. However, it is also possible that the direction of the discrimination-OCB relationship is contingent upon other factors; we detail potentially explanatory moderating variables in the following section. Interestingly, the contradictory finding for OCB did not extend to CWB—the other behavioral outcome included in this study. This suggests that those who perceive discrimination may reciprocate with negative behaviors while maintaining existing levels of certain positive work behaviors.

### 4.3 | A critique of the literature and future research agenda

Although a substantial body of literature on workplace discrimination has amassed in the last few decades, our findings highlight some areas of needed growth. Below, we describe the primary issues we believe future discrimination research should consider.

#### 4.3.1 | Broadening the outcomes of discrimination

With the preponderance of existing discrimination research focusing on linking discrimination to job attitudes and employee health outcomes, we know comparatively little about how discrimination affects behaviors. For example, our literature search revealed a dearth of empirical work considering the impact of discrimination on task performance. Thus, the magnitude of, and boundary conditions affecting the relationship between discrimination and task performance are currently unknown. Our results additionally suggest that the relationship between discrimination and OCB is more complex than accounted for in current theory. Not only did the near-zero relationship found in this study counter theoretical expectations and previous meta-analytic findings (Triana et al., 2015), primary studies have reported both positive and negative relationships between discrimination and OCB, which suggests that there are potential boundary conditions of this relationship that need to be explored empirically. Perceived discrimination's connection with OCBs may be dependent on the perceived perpetrator. To test this, a study could be designed in which respondents not only report perceiving discrimination but also the source (or sources) of the discrimination. Such responses could then be empirically linked with OCB (preferably nonself-reported) that are germane to the perceived perpetrator. For example, discrimination perceived to come from the organization (e.g., formal discrimination) would be expected to decrease OCB directed at the organization but not necessarily have any effect on OCB directed at coworkers. Similarly, discrimination perpetrated by a coworker should reduce OCB directed at individuals but not OCB directed at the organization.



It could additionally be the case that victims of discrimination fear additional mistreatment if they were to withhold certain types of OCB and thus choose not to do so, even if that decision is contrary to their natural inclinations. In particular, mistreated employees may leverage affiliative citizenship behaviors, or behaviors that enhance interpersonal relationships (Van Dyne, Cummings, & McLean Parks, 1995), as a reparative action to bolster their social standing. Future study designs should distinguish between affiliative and other types citizenship behaviors (e.g., change-oriented OCB; Bettencourt, 2004) when linking discrimination to behavioral outcomes. Qualitative research techniques (e.g., interviews) may be needed to uncover other possible explanations for engaging (or not engaging) in OCB following perceived discrimination.

We also encourage future work that extends understanding of the relationships between discrimination and organizational justice. Specifically, we propose that discrimination theory would benefit from exploring the linkages between specific manifestations of workplace discrimination and individual justice dimensions. For example, interpersonal and formal discrimination likely violate different dimensions of justice, such that interpersonal discrimination may have the strongest impact on perceptions of interactional justice, whereas formal discrimination may have the strongest impact on perceptions of distributive and procedural justice. In light of the differential relationships revealed between the various justice dimensions and other employee outcomes (Colquitt et al., 2013), we believe that organizational justice offers a unique vantage point through which the theoretical distinctiveness of formal and interpersonal discrimination can be better understood. Laboratory designs may be best suited for studying this phenomenon for two reasons: first, it is important to capture justice perceptions specific to the discriminatory event, and second, justice perceptions are likely formulated quickly after discrimination occurs.

#### 4.3.2 | Considering alternative explanations

Empirical work examining the consequences of workplace discrimination has not accounted for potential confounding variables. Our incremental validity findings demonstrated substantial decreases in the relationships between workplace discrimination and employee health when accounting for the simultaneous effects of negative affectivity. This calls into question prevailing estimates of these relationships. However, we note that we cannot necessarily conclude that negative affectivity is a confound of discrimination–health relationships as negative affectivity may play a substantive role in the discrimination process. As Spector, Zapf, Chen, and Frese (2000) note, a variable cannot be considered a confound if it is causally linked—as cause or effect—with the underlying construct. It could be the case that employees high on negative affectivity are more likely to view even ambiguous behaviors as mistreatment and, as such, are more likely to *perceive* discrimination at work (Aquino & Bradfield, 2000). Conversely, it has also been argued that employees high in negative affectivity may be more likely to engage in discourteous behaviors that elicit negative treatment (Aquino & Bradfield, 2000; Bolger & Zuckerman, 1995). Thus, it could be the case that employees high on negative affectivity are more likely than others to appraise certain behaviors as discrimination or these employees could truly be more frequently targeted by workplace discrimination. Bolger and Zuckerman (1995) also suggest that traits such as negative affectivity are associated with stronger *reactions* to stressors and, thus, can strengthen the relationships between stressors and outcomes. Therefore, negative affectivity may not only increase one's perceived or actual exposure to discrimination but also the strength of the reaction to discrimination. We recommend future work that disentangles whether negative affectivity is a confound of discrimination–outcome relationships or if it plays a more substantive role in the discrimination process. In addition to clarifying the relationships between negative affectivity, discrimination, and outcomes, we also encourage future work to consider the impact of other potential omitted third variable explanations. For example, it is possible that environmental variables, such as role stressors or destructive leadership styles, similarly inflate the relationships between discrimination and outcomes.

#### 4.3.3 | Improved research designs

Another prevalent limitation of discrimination studies is that the majority are cross-sectional, hampering our ability to establish temporal precedence for the relationships tested here (Shadish, Cook, & Campbell, 2002). In fact, only 4 of

the 99 included studies reported time-lagged relationships between discrimination and at least one of the examined outcome variables. It is conceivable that some of the posited relationships may be in the opposite direction of what we hypothesized. For example, although the tendency is to interpret the relationship between discrimination and CWB as evidence of employees engaging in deviance to retaliate against their mistreatment, it is also possible that an employee who engages in CWB may elicit mistreatment-in-kind from their colleagues.

The lack of longitudinal discrimination studies likewise restricts the conclusions we can draw from our mediation tests. Despite the dominance of the stress perspective in the discrimination literature, we cannot rule out the possibility that job stress precedes workplace discrimination as it could be argued that mistreatment occurs in reaction to stressful working environments. The conceptual relationship between justice and discrimination is similarly unclear; occurrences of workplace discrimination may violate perceptions of justice—as our mediation model assumes—but it is also possible that a preceding unjust event can cause employees to interpret behaviors as discriminatory (i.e., perceived injustice → perceived discrimination). Consequently, we believe that one of the most beneficial avenues for future research is to utilize time-lagged and longitudinal study designs that are more appropriate for establishing temporal precedence.

In addition to longitudinal between-person studies, the discrimination literature would benefit from the inclusion of within-person study designs. Existing discrimination theory predominantly posits within-person relationships (i.e., as a person's level of perceived discrimination changes, so too do their attitudes, behaviors, and health), but current study designs do not reflect the intraindividual nature of these predictions. Employing within-person designs would allow researchers to gain a more fine-grained understanding of the processes through which discrimination affects employee outcomes, the various coping mechanisms that may (or may not) buffer against the adverse consequences of discrimination, and whether discrimination has differential effects across domains (e.g., at work, school, or in the community). In addition, using a within-person methodology such as experience sampling would allow researchers to better explore the potentially varied causal mechanisms associated with experienced and observed discrimination. Such a study could address whether experienced and witnessed discrimination elicit different initial stress and justice responses or if reactions diverge later in the appraisal process. Although the meta-analytic estimates reported here suggest that experienced discrimination is more strongly related to job stress than observed discrimination (from a between-person perspective), it is possible that both experienced and observed discrimination provoke a similar stress response immediately following discrimination but that the stress response dissipates at a different rate. Experience sampling, or other such within-person methodologies, would also allow researchers to test our expectation that employees may be more likely to engage in coping behaviors following personal discriminatory events in comparison to witnessed discriminatory events.

An important consideration when employing within-person study designs is timing; sampling moments should be selected based on the amount of time needed for the state or behavior to reasonably change (Fisher & To, 2012). Given that different forms of discrimination have different base rates, there may not be one ideal timing for all discrimination research questions. For example, formal discrimination occurs in discrete organizational decisions (e.g., promotion decisions, performance ratings) that occur relatively infrequently (Hebl et al., 2002). Conversely, microaggressions and other subtle, interpersonal forms of discrimination can occur in any interaction and should have a much higher base rate (Jones et al., 2016). Whereas a daily approach may be advantageous for understanding reactions to microaggressions, this time frame would be inappropriate for research questions focused on formal discrimination. Research questions such as the effectiveness of adopting a new diversity policy would also warrant longer time frames than research questions regarding the attribution process of workplace discrimination. We recommend that researchers carefully match their measurement time frames to the specific research questions they seek to address.

Moreover, the tendency for many of the included primary studies to use single-source data may have resulted in the overestimation of some of the meta-analyzed relationships between discrimination and the outcomes of interest. The preponderance of cross-sectional studies utilizing solely self-reported measures creates natural concerns about inflation due to common method variance (Podsakoff, MacKenzie, & Podsakoff, 2012; Podsakoff, MacKenzie, Lee, & Podsakoff, 2003). Future work would benefit from the use of longitudinal or time-lagged study designs in combination with obtaining data from multiple relevant sources when assessing discrimination and outcome variables. This may be

of particular importance for variables that are likely to be influenced by social desirability, such as measures of OCB and task performance.

#### 4.3.4 | Theoretical advancements in experienced and observed discrimination

Discrimination theories predominantly focus on experienced discrimination and, as such, little is known about observed discrimination. We thus advocate for theoretical and empirical expansion in three areas. First, our supplemental mediation results suggest that observed discrimination may relate to outcomes through different mechanisms than experienced discrimination, and we believe that future work would benefit from generating theories that elucidate the process through which observed discrimination operates. Second, we advocate for future research that examines the combined effects of both experienced and observed discrimination. Previous research in this area has been mixed. For example, Hitlan, Schneider, and Walsh (2006) found that women were more upset by personal experiences of sexual harassment when they were also bystanders to the sexual harassment of others. Conversely, other findings have shown that observing the mistreatment of others suppresses the negative impact of experienced mistreatment because it conveys that one is not alone in their negative treatment (Bourguignon et al., 2006; Schlipzand, Leavitt, & Lim, 2016). Additional work is needed to reconcile these findings. Third, we were unable to test the influence of shared group membership on the impact of witnessed discrimination. It is possible that witnessing discrimination aimed at an in-group member is more detrimental for employees than witnessing discrimination aimed at an out-group member because actions that devalue one's group are also personally devaluing (Tajfel & Turner, 1986). Future work should explore this possibility.

#### 4.3.5 | Improving measurement

Existing work on discrimination may also be limited by current discrimination measures. As previously noted, the dominant approach has been to use specific measures of discrimination that focus on a particular target (e.g., race discrimination, sex discrimination). Although we do not advocate for the unanimous use of broad measures, these measures do appear to be advantageous when one's goal is to maximize the prediction of employee outcomes. However, our finding that broad discrimination measures increase predictive power relative to specific measures needs to be interpreted in light of our results indicating that discrimination–outcome relationships varied meaningfully based on discrimination targets. Most notably, discrimination targeting sexual orientation appeared to be the most detrimental in several cases. Sexual orientation discrimination may be particularly impactful because of its concealability. Previous work has asserted that people with concealable identities have the added challenge of continuously managing their identity and deciding if, when, and to whom they should reveal their identity (Goffman, 1963).

Taken together, although broad discrimination measures may be most advantageous in explaining variance in individual outcomes, there are potentially important differences based on the target of discrimination that should not be discounted. We encourage discrimination researchers to carefully consider their goals when measuring discrimination and to justify their decisions in light of these findings.

#### 4.3.6 | Discrimination interventions

The last area we draw attention to is the scarcity of research developing and testing specific interventions aimed at reducing the occurrence and impact of workplace discrimination. The consequences sustained by employees who experience and observe discrimination make clear the benefits of such work. In the absence of rigorous research testing specific intervention programs, organizations are urged to follow the best practice guidelines for implementing diversity training programs offered by King, Gulick, and Avery (2010). Organizations should also be aware that employing a diverse workforce is not enough to foster positive intergroup relationships; organizations must also take purposeful actions to integrate diverse employees into the workgroup (Gonzalez & Denisi, 2009).

Although we recommend organizations adopt primary interventions where possible (i.e., work to eliminate discrimination), secondary interventions aimed at the mediational processes identified in this study—stress and justice—may also be a fruitful approach to reducing the impact of discrimination. One path toward mitigating stress responses may

be to train employees to utilize engagement coping strategies wherein employees actively confront the stressor and the associated emotions (Varni, Miller, McCuin, & Solomon, 2012). Discussing the benefits of engagement coping and identifying strategies to effectively use engagement coping can be integrated into existing employee assistance programs as one possible intervention to reduce stress. In addition, the mediational role of justice suggests that recovery attempts may be another promising avenue for reducing the impact of discrimination. Recovery attempts, or actions intended to atone for injustice, have been shown to reduce retaliation following an unjust event (Christian, Christian, Garza, & Ellis, 2012). Studies could be designed that test the relative effectiveness of specific recovery actions (e.g., the perpetrator offering an apology, the organization offering an apology) in buffering against the consequences of discrimination. Mindfulness interventions may also be particularly effective as mindfulness has been linked to reduced stress responses and a lower likelihood of retaliation after unjust events (Long & Christian, 2015). Subsequent work is urged to explore these and other possible interventions.

## 5 | CONCLUSION

Despite the existence of several workplace discrimination meta-analyses that have confirmed expected connections between discrimination and individual outcomes, comparatively little work has focused on the nuances of this detrimental and costly workplace phenomenon and its associations with individual attitudes, behaviors, and health. Consequently, the purpose of this study was to challenge and advance current understanding of workplace discrimination by examining potential mediators of discrimination–outcome relationships, testing a battery of substantive moderators regarding the conceptualization of discrimination, and considering the impact of negative affectivity as a possible third-variable explanation for discrimination–outcome relationships. Results of these analyses challenged a number of prevailing expectations regarding discrimination. In total, we extend the work of previous meta-analyses by moving beyond the estimation of direct relationships to the consideration of a set of factors that deepen and expand theoretical knowledge of workplace discrimination. We encourage researchers and practitioners to build from these findings to further advance our understanding of discrimination and our subsequent ability to prevent or mitigate its negative effects.

Studies included in our meta-analyses are reported in Appendix A.

## NOTES

- <sup>1</sup> Analyses were conducted to determine whether response scale and scale length affected the magnitude of the estimated discrimination–outcome relationships. Multiple-item measures of discrimination displayed stronger relationships with all outcomes in comparison to single-item measures. Results were largely consistent across response scale formats.
- <sup>2</sup> A supplemental moderator analysis was conducted to compare direct and behavioral measures of discrimination. Echoing findings from previous work, discrimination had weaker relationships with most employee outcomes when direct measures were used as opposed to behavioral measures.

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## SUPPORTING INFORMATION

Additional Supporting Information may be found online in the supporting information tab for this article.

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