

# The Consequences of Perceived Discrimination for Psychological Well-Being: A Meta-Analytic Review

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In 2 meta-analyses, we examined the relationship between perceived discrimination and psychological well-being and tested a number of moderators of that relationship. In Meta-Analysis 1 (328 independent effect sizes,  $N = 144,246$ ), we examined correlational data measuring both perceived discrimination and psychological well-being (e.g., self-esteem, depression, anxiety, psychological distress, life satisfaction). Using a random-effects model, the mean weighted effect size was significantly negative, indicating harm ( $r = -.23$ ). Effect sizes were larger for disadvantaged groups ( $r = -.24$ ) compared to advantaged groups ( $r = -.10$ ), larger for children compared to adults, larger for perceptions of personal discrimination compared to group discrimination, and weaker for racism and sexism compared to other stigmas. The negative relationship was significant across different operationalizations of well-being but was somewhat weaker for positive outcomes (e.g., self-esteem, positive affect) than for negative outcomes (e.g., depression, anxiety, negative affect). Importantly, the effect size was significantly negative even in longitudinal studies that controlled for prior levels of well-being ( $r = -.15$ ). In Meta-Analysis 2 (54 independent effect sizes,  $N = 2,640$ ), we examined experimental data from studies manipulating perceptions of discrimination and measuring well-being. We found that the effect of discrimination on well-being was significantly negative for studies that manipulated general perceptions of discrimination ( $d = -.25$ ), but effects did not differ from 0 when attributions to discrimination for a specific negative event were compared to personal attributions ( $d = .06$ ). Overall, results support the idea that the pervasiveness of perceived discrimination is fundamental to its harmful effects on psychological well-being.

**Keywords:** discrimination, stigma, attributions to prejudice, well-being, depression

Members of stigmatized groups experience relatively poor outcomes across a wide variety of contexts. African Americans face discrimination across key life domains including education, housing, employment, and the criminal justice system (Sidanius & Pratto, 1999). Women continue to face a host of barriers in the workplace (Barreto, Ryan, & Schmitt, 2008), and gay men and lesbians face violence and legal discrimination in a number of contexts (Herek,

Gillis, & Cogan, 1999). The mistreatment and relatively poor life events that result from discrimination represent threats to psychological well-being. In other words, discrimination can harm psychological well-being by making it hard to find a good job or apartment, undermining learning in educational settings, or depriving people of adequate health care. In general, being treated relatively poorly and having worse life outcomes than others can harm psychological well-being; however, there may be additional consequences for psychological well-being when mistreatment and disadvantage are subjectively understood as discrimination. As an explanation for negative outcomes, perceived discrimination implies something about one's place in society and thus can have consequences for well-being that go beyond the consequences of the negative treatment itself (Schmitt & Branscombe, 2002b). As noted by Paradies (2006a), it is important to distinguish the *objective* encounters with discrimination from the *subjective* interpretation of discrimination, as both encountering discrimination and perceiving the self as a target of discrimination might have unique consequences for well-being. In the research reported here, we have focused on the consequences of the subjective perception that one faces discrimination—what we call *perceived discrimination*.

A number of theoretical perspectives suggest that perceived discrimination represents a threat to psychological well-being.

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Early symbolic interactionist approaches to stigma and discrimination argued that the self-concept emerges through social interactions, such that others' views of the self are often internalized (Goffman, 1963). Because discrimination communicates that the perpetrator (or perpetrator group) believes you and others like you are unworthy, the symbolic interactionist perspective would suggest that perceiving the self as a target of discrimination would threaten the self-concept. In Goffman's (1963) terms, the stigmatized individual is saddled with a *spoiled identity*—spoiled by the appraisals of others. Perceived discrimination implies a rejection or exclusion of the targeted group and its members and thus can undermine psychological well-being by threatening the fulfillment of needs for inclusion and acceptance (Baumeister & Leary, 1995; Wirth & Williams, 2009). Beyond individual acceptance, humans are also motivated to have their *group* identities accepted and valued within society at large (Tajfel & Turner, 1979). Discrimination can create a sense of powerlessness, as it implies a lack of control over important life outcomes (Verkuyten, 1998) and exclusion from positions of power (Sidanius & Pratto, 1999). Given the devaluation, exclusion, and lack of control suggested by discrimination, perceiving discrimination against an ingroup membership would, in general, be expected to have negative effects on well-being.

Empirically, the link between perceptions of discrimination and poor psychological well-being has been found in many studies, and all recent reviews conclude that there is a negative relationship between perceived discrimination and well-being (e.g., Paradies, 2006b; Williams, Neighbors, & Jackson, 2003). In a meta-analytic review, Pascoe and Smart Richman (2009) found that perceived discrimination was negatively correlated with both psychological and physical health. In addition, Pascoe and Smart Richman found that discrimination was related to two plausible pathways to poorer health—increased stress and unhealthy behaviors. Of particular relevance to the current article is Pascoe and Smart Richman's analysis of 110 studies that examined the relationship between perceived discrimination (based on race, gender, sexual orientation) and psychological well-being. Perceived discrimination was negatively related to psychological well-being for both women and men, across ethnicities, and for depressive symptoms, psychiatric distress, and a composite variable of general well-being.

The present investigation extends the work of Pascoe and Smart Richman (2009) by including a greater number of studies and effect sizes (about 3 times as many), allowing us to examine how generalizable the relationship between perceived discrimination and well-being is across a wider range of different stigmatized identities, including discrimination based on race, ethnicity, gender, sexual orientation, mental illness, and physical disability. We conceptualize psychological well-being broadly and consider the effects of perceived discrimination on a number of indicators of psychological well-being, including relatively high levels of self-esteem, life satisfaction, positive affect, and a sense of control, as well as relatively low levels of depression, anxiety, other forms of psychological distress, and negative affect.

One limitation of most studies of perceived discrimination and well-being is that they are correlational, thus limiting the ability to make strong causal inferences. To address the issue of causality, we conducted a separate analysis of experimental studies that manipulated perceptions of discrimination. Pascoe and Smart Richman (2009) meta-analyzed 12 experimental studies and found

a significant negative effect of discrimination. However, their analysis focused exclusively on stress responses and contained too few studies to meaningfully test moderators. To adequately assess the accumulated evidence for perceived discrimination's causal effect on well-being, a more comprehensive synthesis of the experimental literature was needed. To fill this gap, our meta-analysis of experimental studies considered discrimination's effect on psychological well-being more broadly, included a larger number of effect sizes, and tested moderators of perceived discrimination's effect on well-being.

### Moderators of Perceived Discrimination's Effect on Psychological Well-Being

Recent theorizing on perceptions of discrimination and well-being has shifted toward an emphasis on moderation—considering how personal and contextual factors might influence the degree to which perceptions of discrimination are harmful or self-protective (Major, Quinton, & McCoy, 2002). Researchers have found a number of moderators of the effects of perceived discrimination, with perceived discrimination sometimes having opposite effects at different levels of those moderators (e.g., Kaiser, Major, & McCoy, 2004; McCoy & Major, 2003; B. Miller, Rote, & Keith, 2013; Schmitt & Branscombe, 2002a). We took a similar approach in our meta-analyses and tested a number of theoretically relevant moderators. Indeed, a major contribution of our research compared to Pascoe and Smart Richman (2009) is that we test a number of moderators of the relationship between perceived discrimination and psychological well-being that have not previously been tested meta-analytically.

### Perceiving Pervasive Versus Isolated Discrimination

Schmitt and Branscombe (2002b) argued that the consequences of perceptions of discrimination depend on the degree to which discrimination is perceived to be isolated and idiosyncratic versus *pervasive*—meaning that discrimination is systemic and thus occurs frequently and across multiple contexts. Because discrimination is less avoidable if more pervasive across contexts, perceptions of pervasive discrimination should undermine feelings of control. In addition, pervasive discrimination, compared to discrimination that is seen as isolated, is more likely to be experienced as rejection and exclusion by dominant society. For these reasons, discrimination appraised as pervasive is especially likely to have a negative effect on well-being compared to discrimination that is seen as isolated (e.g., Schmitt, Branscombe, & Postmes, 2003).

**Ingroup status.** For groups of relatively disadvantaged status, discrimination is by definition more pervasive than it is for groups of relatively advantaged status. In addition, discriminatory events are likely to be more severe for members of disadvantaged groups than discrimination encountered by members of advantaged groups (Branscombe, 1998). Because perceptions of discrimination are likely to reflect greater pervasiveness and severity for members of disadvantaged groups, Schmitt and Branscombe (2002b) predicted that perceptions of discrimination would be more harmful for members of disadvantaged groups than for advantaged groups.

A number of studies support the idea that perceived discrimination has different consequences for advantaged versus disadvantaged

groups (Schmitt & Branscombe, 2002a; Schmitt, Branscombe, Kobrynowicz, & Owen, 2002; Wang, Leu, & Shoda, 2011). Indeed, Kessler, Mickelson, and Williams (1999) found that the relationship between perceived discrimination and poor well-being was stronger for women than men, Black Americans than White Americans, low social class than high social class, and those with less education than those with more education. Some other studies, such as Major, Kaiser, and McCoy (2003), have not found that group status moderates the effects of perceived discrimination. Paradies (2006b) concluded that there is a need to “clarify the differing associations between self-reported racism and health for dominant and minority groups” (p. 896). We examined group status as a moderator in our meta-analyses by comparing the effect of perceived discrimination on well-being for groups that are relatively disadvantaged with the effect for groups that are relatively advantaged.

**General perceptions of discrimination versus attributions for specific events.** Perceptions of discrimination can be conceptualized in different ways. One critical distinction is between perceptions of discrimination in general—which tend to reflect how frequent discrimination is across time and contexts (i.e., pervasiveness)—and discrimination *attributions* for a specific negative event (Major et al., 2002). Attributions for single events do not necessarily reflect the perception that discrimination is pervasive, and thus, as suggested by Branscombe, Schmitt, and Harvey (1999), such attributions to discrimination would not necessarily be as psychologically costly as perceptions of pervasive discrimination. Our analysis of experimental studies provides an opportunity to compare effect sizes for studies that manipulate perceptions of the pervasiveness of discrimination against the ingroup with studies that manipulate attributions to discrimination for a single negative event. We hypothesized that the effects for manipulations of the pervasiveness of discrimination would be more harmful than attributions to discrimination for single events.

The *attributional ambiguity perspective* (Crocker & Major, 1989; Major, Kaiser, & McCoy, 2003) makes an even stronger claim, suggesting that for single negative events, attributions to discrimination can be self-protective in comparison to attributions to personal deservingness. The attributional ambiguity perspective argues that explaining a negative outcome in terms of discrimination externalizes *blame* and thus serves to discount personal deservingness as an explanation for a negative outcome. As a consequence, self-directed affect (self-esteem and depressed affect) is hypothesized to be protected compared to when an attribution of personal deservingness is made for the same negative outcome. Crocker and Major (1989) further speculated that the more members of disadvantaged groups perceived discrimination as pervasive, the higher their self-esteem would be. This speculation has received practically no empirical support and has been explicitly disavowed in more recent theoretical treatments of the attributional ambiguity perspective (Major et al., 2002).

Initial tests of the attributional ambiguity hypothesis by Crocker, Voelkl, Testa, and Major (1991) produced only weak support, but some more recent tests have been more supportive (e.g., Major, Kaiser, & McCoy, 2003; Major, Quinton, & Schmader, 2003). Other studies, however, have not found that attributions to discrimination significantly protect self-esteem relative to attributions to personal deservingness—a null effect (e.g., Adams, Garcia, Purdie-Vaughns, & Steele, 2006, Study 1; Pintel, 2004).

Recent work on the attributional ambiguity hypothesis has moved away from the idea that attributions to discrimination are self-protective per se, suggesting instead that the effect of attributions to discrimination depends on personal and contextual factors (Major et al., 2002). Researchers have found that the psychological consequences of attributions to discrimination are moderated by minority group identification (e.g., McCoy & Major, 2003), optimism (Kaiser et al., 2004), and ideological worldviews (Major, Kaiser, O’Brien, & McCoy, 2007), among other variables. However, it remains of practical and theoretical importance to examine the strength of evidence for a self-protective main effect of attributions to discrimination relative to attributions to personal deservingness. Our examination of studies that manipulated attributions to discrimination for a negative event provided the opportunity to conduct the first meta-analytic test of the attributional ambiguity hypothesis.

### Perceived Discrimination and Well-Being in Different Intergroup Contexts

One issue that has received little empirical attention is whether perceived discrimination has different consequences depending on the kind of group that is the target of discrimination (but see Paradies, 2006b). In other words, do perceived racism, sexism, heterosexism, and so forth have different relationships with well-being? In our analyses, we compare the effects of perceived discrimination across a variety of contexts, including racism, sexism, heterosexism, and prejudice against illness or disability. There are two general reasons why effects could differ between groups: The groups differ in the kind of discrimination they encounter and perceive (its pervasiveness, severity, legitimacy), and they might differ in terms of their access to or use of successful strategies for coping with discrimination. These analyses are necessarily exploratory, however, because these intergroup contexts differ from one another in multiple ways. Nonetheless, examining the relationship between perceived discrimination and well-being across different intergroup contexts is an important test of the generalizability of that relationship and may point toward further theoretical development if differences do emerge.

In addition to comparing different types of stigma, we also coded and compared the effects of discrimination based on stigma dimensions identified by prior theorizing and research that might make people more vulnerable (or resilient) to perceived discrimination. Although multiple dimensions of stigma have been suggested (Jones et al., 1984), Crocker, Major, and Steele (1998) argued for the particular importance of two dimensions: stigma visibility and perceived controllability of the stigma.

The degree to which the stigma is visible or concealable refers to the conspicuousness of stigma. When concealability is high, observers will typically not know that a person possesses the stigma unless they are told that information directly (cf. discreditable stigma; Goffman, 1963). For nonconcealable or visible stigmas, observers can detect the presence of the stigmatizing attribute directly. In a recent narrative review, Chaudoir, Earnshaw, and Andel (2013) concluded that people with concealable stigmas face worse mental and physical health outcomes than people with visible stigmas. People with concealable stigmas are less likely to have social networks that contain similarly stigmatized others (Frible, Platt, & Hoey, 1998), are therefore less able

to take advantage of social support as a coping strategy, and are more likely to internalize negative attitudes and beliefs about their stigma (Chaudoir et al., 2013). Adding to the challenge of lacking easy access to ingroup social support, people with concealable stigmas must contend with the dilemma of when, where, and to whom they will reveal their stigma (Goffman, 1963). Such a dilemma might have even more deleterious consequences when expectations of discrimination are high, as that increases the risks of coming out. Keeping stigma secret might allow people to avoid negative treatment and embarrassment and protect valued interpersonal relationships, but over the long term, concealment leads to anxiety, requires active suppression of the stigmatized identity, and deprives the stigmatized of opportunities for social support (Chaudoir & Fisher, 2010; Major & Gramzow, 1999). Because of the added stress of managing other people's awareness of one's stigmatized status and because people with concealable stigmas have less access to coping with stigma through social support from similarly stigmatized others, we hypothesized that the effect of perceived discrimination would be stronger when it is directed at concealable stigmas compared to more visible stigmas.

The controllability of the stigma refers to the degree to which the stigmatized are, or are perceived to be, in control of having the stigma (onset controllability) or in control of removing the stigma. In one of the few studies to examine the consequences of control beliefs for the well-being of stigmatized individuals, Quinn and Crocker (1999) found that women who saw themselves as overweight had poorer psychological well-being than those who perceived themselves as normal or underweight. However, the effect of perceived weight stigma was most evident among participants who believed that their weight was highly controllable.

There are a number of reasons why perceived discrimination against a controllable stigma could have a more harmful effect than discrimination directed against an uncontrollable stigma. Beliefs about control over removing or maintaining a stigma encourage the stigmatized to pursue individual mobility by attempting to remove the stigma rather than developing an attachment to a collective ingroup identity (Fernández, Branscombe, Gómez, & Morales, 2012; Garstka, Schmitt, Branscombe, & Hummert, 2004; Tajfel & Turner, 1979). To the extent that individual mobility attempts fail, stigmatized people are potentially left isolated—having turned away from their ingroup while not having gained full acceptance in the outgroup.

Negative group-based treatment is more likely to be appraised as legitimate (by both targets and perpetrators) if directed at people with controllable stigmas compared to people with uncontrollable stigmas (Rodin, Price, Sanchez, & McElligot, 1989; Weiner, Perry, & Magnusson, 1988). When discrimination against a stigmatized identity is appraised as legitimate, it discourages people from developing a sense of collective identity with similarly stigmatized others, depriving them of social support and social-psychological resources for coping with discrimination (Jetten, Schmitt, Branscombe, Garza, & Mewse, 2011). From the attributional ambiguity perspective, seeing group-based treatment as deserved undermines the possibility of protecting the self from negative outcomes by discounting self-blame, as attributions to discrimination themselves would essentially be attributions of self-blame (Crocker, Cornwell, & Major, 1993).

Thus, we predicted that perceived control over stigma would make the effect of perceptions of discrimination more harmful. In

our meta-analyses, we were able to compare effect sizes for stigmas that vary in their concealability and controllability. We predicted that both concealability and controllability would exacerbate the costs of perceived discrimination on psychological well-being relative to conspicuous and uncontrollable stigmas.

### Perceived Discrimination and Different Aspects of Psychological Well-Being

Past research on the effects of perceived discrimination has examined well-being in a variety of ways, with some studies focusing on mental health symptoms (anxiety, depression, and other forms of psychological distress) and others on mood, self-esteem, perceptions of control, and life satisfaction. Although there are good theoretical and empirical reasons for predicting that perceived discrimination harms psychological well-being in general (Williams et al., 2003), particular aspects of well-being might be differentially affected by perceived discrimination. Paradies's (2006b) review reported twice as many significant harmful effects for negative outcomes (depression, anxiety) than for positive outcomes (e.g., self-esteem). Similarly, the attributional ambiguity perspective suggests that the consequences of perceived discrimination differ for self-directed affect (self-esteem and depressed affect) compared to other kinds of emotional and psychological outcomes. Major et al. (2002) argued that perceptions of discrimination, and attributions to discrimination in particular, deflect blame for negative events away from the *self*, thus protecting self-directed affect in particular, while other aspects of well-being might not be protected (e.g., anxiety). Although the attributional ambiguity perspective predictions are specific to attributions for single events, it is also possible that general perceptions of discrimination have a less harmful effect on self-directed emotions than other aspects of well-being. To examine whether the effect of perceived discrimination generalizes across different operationalizations of well-being, we meta-analyzed the relationship between perceived discrimination and well-being separately for a variety of indicators, such as self-esteem, depression, anxiety, and life satisfaction.

### Personal Versus Group Discrimination and Psychological Well-Being

In the literature on discrimination and psychological well-being, an important distinction has been made between discrimination directed at the personal self as a function of group membership and discrimination directed at the ingroup as a whole. Crosby (1984) argued that members of disadvantaged groups deny personal discrimination to a greater degree than group discrimination because personal discrimination is more threatening (cf. Postmes, Branscombe, Spears, & Young, 1999). More recently, Bourguignon, Seron, Yzerbyt, and Herman (2006) investigated the distinct effects of perceived personal and group discrimination and found that perceptions of personal discrimination were negatively related to self-esteem. However, when controlling for personal discrimination, perceptions of group discrimination positively predicted self-esteem. Bourguignon et al. argued that perceptions of group discrimination suppress the negative consequence of perceived personal discrimination because group discrimination conveys that one is not alone in one's rejection and exclusion and thus contrib-

utes to successful coping (cf. Armenta & Hunt, 2009). Too few studies included the necessary statistical information to make it worthwhile to meta-analytically examine the unique effects of personal and group discrimination (i.e., personal discrimination's relationship with well-being controlling for group discrimination, and vice versa), and thus, we could not test Bourguignon et al.'s predictions directly. However, in the correlational data, we were able to test the general idea that perceived discrimination is more harmful at the personal level than at the group level.

### Within-Study Moderators

There are a number of moderators that we could not test meta-analytically because they did not vary *between* samples in a way that we could code adequately. For example, almost all of the effects we examined could not be coded in terms of the strength of identification with the ingroup. However, we were able to synthesize the results of within-study tests of moderation by vote counting—comparing the number of significant and nonsignificant tests of a moderator. Like Pascoe and Smart Richman (2009), we used vote counting to examine the moderating roles of group identification, social support, and coping strategies.

**Group identification.** Some researchers have argued that identification with an ingroup can serve a protective function, providing social and psychological resources that reduce the costs of perceived discrimination (N. Hansen & Sassenberg, 2006; Mosakowski, 2003; Romero & Roberts, 2003; R. M. Sellers, Caldwell, Schmeelk-Cone, & Zimmerman, 2003; Wong, Eccles, & Sameroff, 2003). In contrast, McCoy and Major (2003) argued that group identification, particularly the centrality of the group identity to the self, makes perceptions of discrimination more self-relevant and thus more *harmful* (cf. R. M. Sellers et al., 2003).

**Social support.** The perceived availability of social support reduces the negative effects of stress on psychological well-being (Cohen & Wills, 1985). Similarly, when people perceive discrimination, how well they adapt to that stressor might depend on emotional or instrumental assistance from friends, family, and their social network more generally. For example, Noh and Kaspar (2003) found that seeking support after experiencing discrimination predicted lower levels of depression.

**Coping strategies.** From a stress and coping perspective (e.g., Lazarus & Folkman, 1984), the consequences of perceived discrimination (a stressor) depend on the strategies that people employ to cope with that stressor. In our vote-counting analysis, we distinguished between engagement coping strategies and disengagement coping strategies (Varni, Miller, McCuin, & Solomon, 2012). Engagement responses are attempts to change the stressful situation or one's reactions to it. Disengagement responses are oriented away from the stressor and include attempts to avoid the stressor or avoid thinking about the stressor. Generally speaking, engagement coping is more likely to promote or protect well-being than disengagement coping (e.g., Connor-Smith, Compas, Wadsworth, Thomsen, & Saltzman, 2000).

### Overview of the Current Research

We conducted a quantitative review of research examining the relationship between perceived discrimination and psychological well-being. In Meta-Analysis 1, we synthesized the results from

correlational studies that measured perceptions of discrimination as well as some indicator of psychological well-being. In line with past reviews, we anticipated that the relationship between these two variables would be negative—consistent with the assumption that perceived discrimination harms psychological well-being. However, because these studies were correlational, we cannot be certain that the observed relationship represents a causal effect of perceived discrimination. We took two steps to address causality. First, in Meta-Analysis 1, we examined longitudinal effect sizes that control for prior levels of well-being. Second, in Meta-Analysis 2, we conducted the first comprehensive meta-analytic synthesis of experimental studies that manipulated perceptions of discrimination and measured well-being. A close examination of experimental studies is also important because it helps to disentangle the consequences of perceiving discrimination from the consequences of experiencing negative events and negative treatment per se.

We meta-analytically tested a number of moderators of the relationship between perceived discrimination and psychological well-being. We examined whether perceived discrimination had different consequences for advantaged compared to disadvantaged groups—a hypothesis rarely considered in primary research but testable meta-analytically. We also tested whether intergroup context (e.g., race, gender, sexual orientation) moderated the relationship between perceived discrimination and well-being. In the analysis of experimental data, we examined the type of manipulation as a moderator, comparing studies that manipulated perceptions of discrimination in general with those that manipulated attributions to discrimination for a single negative outcome.

We also tested the generalizability of the relationship between perceived discrimination and well-being across a variety of indicators of well-being, paying special attention to whether the relationship differed for measures of self-esteem and self-directed affect compared to other measures. We also examined effects across different operationalizations of perceived discrimination, comparing results for measures of group discrimination and personal discrimination. Finally, we used a vote-counting technique to review the evidence for moderators that we were not able to test meta-analytically: group identification, social support, and coping strategies.

### Location of Relevant Studies

A literature search was conducted using PsycINFO and Socio-File to locate relevant studies using the keywords discrimination, prejudice, or stigma, *and* well-being, mental health, self-esteem, adjustment, depression, anxiety, stress, affect, emotion, life satisfaction, or mood. Studies were limited to those written in English. In addition, articles found through this search were examined to locate other relevant studies. We also contacted researchers working in the area and posted announcements on psychological e-mail lists to solicit unpublished data. The literature search was completed October 15, 2012, and is inclusive of studies published up to that date.

### Inclusion Criteria

Two types of studies were retained for analysis: correlational studies measuring perceptions of discrimination and experimental

studies manipulating perceptions of discrimination. For inclusion, studies were also required to measure psychological well-being, broadly construed, including measures of mood, self-esteem, anxiety, depression, life satisfaction, or other measures of mental health. Measures of anger were not included. Studies were only included if they provided the necessary effect sizes or sufficient statistical information for the estimation of those effect sizes or if effect sizes were obtained from the authors of the study. Thus, when an article only reported that an effect was not significant, we could not compute an estimate of the effect size, and such a study was excluded. Two effect sizes (from one study: Spalding, 1999) were dropped from the experimental data set because they reported a failed manipulation check of perceptions of discrimination. Studies contributing effect sizes to the meta-analyses are noted in the References, marked with a <sup>C</sup> for studies contributing cross-sectional effect sizes (Meta-Analysis 1), an <sup>L</sup> for longitudinal effects (Meta-Analysis 1), and an <sup>E</sup> for experimental effects (Meta-Analysis 2).

### Meta-Analysis 1: Correlational Data

In the analysis of the correlational data, we considered several potential moderators of the relationship between perceptions of discrimination and well-being. Based on theoretical arguments put forth by Schmitt and Branscombe (2002b), we tested the hypothesis that the relationship between perceived discrimination and well-being would be stronger (more negative) for disadvantaged samples than for advantaged samples. We also examined the intergroup context involved (e.g., racism, sexism, heterosexism) and the extent to which the self-aspect that was the target of discrimination was concealable and controllable. We conducted exploratory analyses to examine whether the relationship between perceived discrimination and well-being differed between three age groups: children, adolescents, and adults.

Furthermore, we examined effect sizes based on a number of different operationalizations of well-being. For the main analysis, we examined well-being broadly construed and included effect sizes based on many different operationalizations of well-being. We conducted additional analyses on effect sizes based on more narrow definitions of well-being, examining the relationship between perceptions of discrimination and self-esteem, psychological distress, depression, anxiety, life satisfaction, and other variables. We also examined effect sizes based on whether they were based on perceptions of discrimination directed at the individual or discrimination directed at the group as a whole. To address the issue of causality, we examined the mean effect size for longitudinal studies that controlled for initial levels of well-being.

### Method

**Computation of effect sizes.** We calculated effect sizes ( $r$ ) for the relationship between perceived discrimination and well-being. Each effect size was weighted by the inverse of its variance. The main analyses included effect sizes based on a number of different measures of well-being. When multiple measures of well-being or perceived discrimination were used in a sample, we created a single composite effect size by averaging across the multiple effect sizes. The data set included 328 independent effect sizes from 144,246 participants. Analyses were conducted on effect sizes

after employing Fisher's  $Zr$  transformation; effect sizes reported in this article have been converted back to  $r$  for ease of interpretation. Negative effect sizes indicate that perceived discrimination is negatively related to well-being (or positively related to negative outcomes such as psychological distress.)

For 43 samples, studies reported a relationship between perceived discrimination and well-being that controlled for one or more covariates. In our analyses, we retained the effect sizes with covariates ( $r = -.22$ ) because they did not differ significantly from those based on a zero-order relationship ( $r = -.23$ ),  $Q(1) = 0.09$ ,  $p = .77$ .

**Different operationalizations of well-being.** In addition to the main analysis that examined the relationship between discrimination and well-being in general, we examined additional sets of effect sizes based on more specific aspects of well-being. We calculated effect sizes based on self-esteem, depression, anxiety, life satisfaction, control, positive well-being, positive affect, and negative affect. We also created a set of effect sizes based on psychological distress defined broadly, including depression, anxiety, somatic symptoms, obsessive-compulsive disorder, and post-traumatic stress disorder. Because many studies included multiple measures of well-being, these different sets of effect sizes are not independent, and we cannot compare them statistically within the same analysis. Thus, we examined each set of effect sizes in separate analyses.

**Group versus personal discrimination.** We created two additional data sets of effect sizes based on group or personal discrimination. Because some studies included both personal and group measures, these two sets of effect sizes are not independent, and we therefore examined each set of effect sizes separately. For the measures of personal discrimination, about half asked participants to report the frequency with which different discriminatory events happened to them. For example, the Schedule of Racist Events (Landrine & Klonoff, 1996) asks participants to report, on a 1 (*never*) to 7 (*almost all the time*) scale, how frequently they encounter racist events like being called a racist name. Most other measures of discrimination asked participants to report whether specific discriminatory acts had happened to them (e.g., Williams et al., 2008) or asked participants to respond to statements about their experience with discrimination (e.g., "To what extent are you personally as a Turk discriminated against?"; Verkuyten, 1998). A few studies asked participants to rate the likelihood of discrimination for hypothetical negative events (e.g., Branscombe et al., 1999). Group-level measures asked about perceptions of discrimination directed at the group as a whole (e.g., "Africans are often confronted with discrimination", Bourguignon et al., 2006). Some studies used measures that contained a mixture of personal and group discrimination items (e.g., Garstka et al., 2004). We did not include effect sizes based on measures of negative events and mistreatment that did not use the term *discrimination* or describe group/stigma-based differential treatment in either the items or instructions. We also did not include effect sizes based on measures that confounded perceptions of discrimination with appraisals of the stressfulness of the discrimination or the internalization of stigma.

**Coding of study characteristics.** Two people coded effect sizes based on a number of study and sample characteristics. For all moderators examined in Meta-Analysis 1, interrater agreement

was high (>88%). When coders disagreed, they met to resolve the discrepancy.

**Status of the target ingroup.** Studies were coded according to the relative status of the ingroup targeted by discrimination—whether it was disadvantaged, advantaged, or mixed. Women who responded to items about discrimination against women would be coded as a disadvantaged group; men who responded to items about discrimination against men would be coded as an advantaged group. An effect size based on a sample of men and women considering gender discrimination against their respective ingroups would be coded as mixed status.

**Intergroup context.** Effect sizes were coded based on whether the discrimination involved race (which included groups based on race, ethnicity, national origin, or religion), gender, sexual orientation, mental illness, physical disability, HIV+ status, or body weight. Effect sizes that did not fit into the previous categories were grouped in an *other* category—including discrimination against the unemployed, the formerly incarcerated, people with low socioeconomic status, young people, and old people, as well as effects from studies that measured discrimination toward multiple groups or did not specify the target of discrimination in their measures.

**Concealability and controllability.** Effect sizes were coded based on whether the target of discrimination involved a stigma that was highly concealable (e.g., sexual orientation, unemployed), somewhat concealable (i.e., concealable for some but not all persons with the stigma, e.g., schizophrenia), or not typically concealable (weight, gender, race, age group).

Similarly, we coded effect sizes based on whether the stigma was controllable. Attempts to code the stigmas based on actual controllability are problematic because, for many stigmas, how much control people have over their stigma is not something that can be objectively defined and the degree of control people have might be context dependent. In addition, it was not possible for us to code effect sizes based on the stigmatized individual's beliefs about controllability because stigmatized individuals within the same group often differ in their beliefs regarding the controllability of stigma. Therefore, we coded based on general social perceptions of controllability. We coded for two types of controllability: controllability of stigma onset and controllability of the removal of the stigma. However, these ratings were highly correlated, as they differed in only a few cases (e.g., being HIV+ or having another sexually transmitted disease were coded as high in onset controllability but low in control over removal of the stigma). Because these measures were so highly correlated, we only present the results for stigma onset controllability. Analyses using controllability of removing the stigma produced similar results.

We coded stigmas as low in onset controllability if most people would generally not see this stigma as controllable (e.g., gender, race, age, deafness). We coded stigmas as somewhat controllable if some people, but not all people, would see the stigma as controllable (sexual orientation, depression) and as high in controllability if most people would see the stigma as controllable (weight, drug use, being unemployed).

For simplicity, we dichotomized the concealability variable by combining the effect sizes coded as somewhat or highly concealable into a single concealable category. Similarly, we combined the effect sizes coded as somewhat or highly controllable into a single controllable category. The three-level constructions of these

variables did not predict any additional variance in effect sizes compared to their dichotomized versions. For both controllability and concealability, there were a small number of effect sizes ( $k_s = 8$  and  $6$ , respectively) that could not be coded because multiple target groups were included in the sample.

**Age of participants.** Effect sizes were coded based on whether participants were children (under 13 years), adolescents (13–18 years), or adults (18 years or older).

## Results

We used a random-effects model in which the random variance component was determined using maximum likelihood. The mean weighted effect size across  $K = 328$  studies ( $N = 144,246$ ) was significantly negative,  $r = -.23$ , 95% confidence interval (CI)  $[-.24, -.21]$ ,  $z = -30.18$ ,  $p < .001$ ,  $Q_w(327) = 2,268.79$ ,  $p_{Q_w} < .001$ , indicating that perceptions of discrimination are negatively related to psychological well-being. The mean weighted effect size is similar to that found by Pascoe and Smart Richman (2009), 95% CI  $[-.22, -.18]$ .

**Tests of moderators.** To examine the moderating effects of study variables, we conducted analyses using the SPSS macros by Wilson (2002). We first examined the effect of each moderator independently, as presented in Table 1. We then examined the effect of each moderator in a multivariate regression-analog analysis controlling for the effects of the other moderators. Results for the multivariate analysis, including unstandardized regression coefficients testing the effect of each moderator, are presented in the text. As all of our moderators are categorical, we used dummy coding (0, 1) to test their effects in the multivariate analysis. For categorical variables with more than two levels, we created multiple dummy codes that compared each category to a reference category. In the multivariate analysis, the overall regression model was significant,  $Q(12) = 75.67$ ,  $p < .001$ ,  $R^2 = .19$ , indicating that as a set, the moderators account for almost one fifth of the variance in the effect sizes.

**Cross-sectional versus longitudinal.** The relationship between discrimination and well-being was weaker for longitudinal effect sizes compared to cross-sectional effect sizes (see Table 1). Similarly, when controlling for the effect of moderators, the contrast between longitudinal versus cross-sectional data was significant ( $B = .10$ ,  $z = 3.45$ ,  $p < .001$ ). More important, however, the mean weighted effect size for longitudinal studies was significantly negative ( $r = -.15$ ). For the longitudinal studies only, we tested the months between data collection waves as a potential moderator. Time between waves was a significant predictor of effect sizes ( $B = .0017$ ,  $z = 2.01$ ,  $p = .04$ ,  $R^2 = .16$ ). For every 12 months between waves, effect sizes were smaller by  $r\Delta = .02$ .

**Ingroup status.** The status of the ingroup significantly predicted effect sizes. Effect sizes from samples of disadvantaged groups and from samples of mixed status were larger than those from advantaged samples. Controlling for other moderator variables in the multivariate analysis, the effect size was again significantly smaller (more positive) for advantaged samples compared to disadvantaged samples ( $B = .13$ ,  $z = 3.17$ ,  $p = .002$ ). Mixed samples did not differ from disadvantaged samples in the multivariate analysis ( $B = .02$ ,  $z = 0.72$ ,  $p = .47$ ).

**Intergroup context.** The intergroup context had a significant moderating effect. In particular, effect sizes for studies of racism

Table 1  
*Effect Sizes  $r$  and Tests of Moderation for the Relationship Between Perceived Discrimination and Well-Being in Correlational Studies*

Moderator	$r$	95% CI	$z$	$k$	$Q$
Total	-.23**	-.24, -.21	-30.18	328	2,268.79**
Study methodology					
Cross-sectional	-.23**	-.25, -.22	-29.60	307	316.75
Longitudinal	-.15**	-.21, -.09	-4.98	21	12.92
Between-groups $Q$					8.18**
Status of ingroup					
Disadvantaged	-.24**	-.25, -.22	-29.00	293	304.12
Mixed	-.20**	-.25, -.15	-7.87	26	22.65
Advantaged	-.10**	-.18, -.01	2.18	9	1.71
Between-groups $Q$					10.91**
Intergroup context					
Racism	-.21**	-.22, -.19	-22.81	211	225.61
Sexism	-.18**	-.24, -.13	-6.61	23	14.67
Heterosexism	-.28**	-.33, -.23	-9.94	21	26.10
Mental illness	-.31**	-.37, -.25	-9.29	18	11.23
Physical illness/disability	-.41**	-.49, -.32	-8.32	8	9.28
HIV+	-.33**	-.39, -.26	-9.14	12	8.16
Weight	-.28**	-.38, -.17	4.84	8	3.88
Other	-.26**	-.31, -.21	-10.08	27	24.45
Between-groups $Q$					46.46**
Concealability					
Not concealable	-.21**	-.22, -.19	-24.08	246	242.61
Concealable	-.30**	-.33, -.28	-19.77	76	78.74
Between-groups $Q$					33.48**
Controllability (onset)					
Not controllable	-.21**	-.23, -.20	-24.77	254	262.96
Controllable	-.29**	-.32, -.26	-17.31	66	57.64
Between-groups $Q$					20.09**
Age					
Children	-.26**	-.32, -.21	-9.18	22	32.73**
Adolescents	-.22**	-.26, -.18	-11.25	52	33.31
Other adults	-.23**	-.24, -.22	-25.69	254	261.04
Between-groups $Q$					1.92

Note. Values in the  $Q$  column represent the heterogeneity within each level of a variable ( $Q_w$ ) or the test for heterogeneity between levels of a variable ( $Q_b$ ). CI = confidence interval.

\*\*  $p < .05$ .

and sexism were smaller than those for studies of stigma based on sexual orientation, mental illness, physical disability, HIV+ status, or weight (see Table 1). For the multivariate analysis, we created seven dummy codes comparing studies of different contexts with studies of racism. Studies of sexism did not differ from studies of racism in the multivariate analysis ( $B = .01$ ,  $z = 0.53$ ,  $p = .59$ ). However, studies of heterosexism ( $B = -.07$ ,  $z = -2.58$ ,  $p = .01$ ), mental illness ( $B = -.13$ ,  $z = -3.67$ ,  $p < .001$ ), physical disability ( $B = -.22$ ,  $z = -4.17$ ,  $p < .001$ ), HIV+ status ( $B = -.13$ ,  $z = -3.46$ ,  $p = .001$ ), and other contexts ( $B = -.06$ ,  $z = -2.27$ ,  $p = .02$ ) produced larger negative effect sizes than studies of racism. Although nonsignificant, studies of weight prejudice also tended to produce larger effects than racism studies ( $B = -.08$ ,  $z = -1.36$ ,  $p = .17$ ). Thus, both the univariate and multivariate results suggest that perceptions of discrimination are less harmful when based on gender or race compared to other types of discrimination.

We conducted an additional analysis of the studies of racism, comparing studies of anti-Black, anti-Asian, anti-Latina/o, anti-Arab/Middle Eastern, and anti-White discrimination, as well as other studies—those that included multiple racial/ethnic groups (e.g., if the sample consisted of Blacks and Latino/as) and those

that were highly specific and thus infrequent (e.g., southern Italians, Russian immigrants to Israel). The effect size was significantly moderated by racial ingroup,  $Q(5) = 15.48$ ,  $p = .009$ . Effect sizes for studies of anti-White discrimination ( $r = -.10$ ,  $z = -2.25$ ,  $p = .02$ ,  $k = 8$ ) were smaller than for the other groups ( $r_s > -.17$ ,  $p_s < .001$ ), replicating the effect of ingroup status. To examine differences between the minority groups, we conducted an additional analysis excluding studies of anti-White discrimination. Racial ingroup was now only a marginally significant moderator,  $Q(4) = 9.10$ ,  $p = .059$ . Studies of anti-Black discrimination ( $r = -.17$ ,  $z = -9.44$ ,  $p < .001$ ,  $k = 50$ ) produced the weakest effect size. Studies of discrimination against Asians ( $r = -.26$ ,  $z = -11.12$ ,  $p < .001$ ,  $k = 35$ ) and anti-Arab/Middle Eastern discrimination ( $r = -.24$ ,  $z = -7.54$ ,  $p < .001$ ,  $k = 18$ ) produced the largest effect sizes. Studies of discrimination against Latina/os ( $r = -.20$ ,  $z = -9.57$ ,  $p < .001$ ,  $k = 37$ ) and other groups ( $r = -.21$ ,  $z = -12.64$ ,  $p < .001$ ,  $k = 63$ ) had effect sizes that were in between. However, these differences are fairly small, and on the whole, results suggest a fair degree of consistency across racial minority groups.

**Concealability and controllability.** As shown in Table 1, concealability was a significant moderator at the univariate level.

Effect sizes were weaker for stigmas coded as not concealable than for stigmas coded as more concealable. Controllability of stigma onset was also a significant moderator; stigmas that were not controllable produced weaker effects than those that were controllable.

Because controllability and concealability were correlated in our data set, we conducted a regression analysis to assess the unique moderating effects of each dimension. The more concealable the stigma, the more discrimination was negatively related to well-being ( $B = -.10, z = -3.21, p = .001$ ). Controllability, however, did not predict effect sizes ( $B = -.003, z = -0.09, p = .93$ ).

We did not include concealability and controllability in our main multivariate regression analysis because it was so highly confounded with the intergroup context variable. When concealability and controllability were added to the multivariate analysis with all the other moderators including intergroup context, neither concealability nor controllability was a significant predictor of effect sizes ( $ps > .6$ ). Results for the other moderators are unchanged, with two minor exceptions: the moderating effect of heterosexism ( $B = -.10, p = .14$ ) and discrimination against *other* stigmas ( $B = -.07, p = .22$ ) were no longer significant when concealability and controllability were added to the regression model.

**Age.** The relationship between perceived discrimination and well-being was negative for all three age groups. In the univariate analysis, the effect of age was not significant. In the multivariate analysis, however, the contrast comparing children to adult samples was significant ( $B = -.07, z = -2.54, p = .01$ ), indicating larger (more negative) effects for children. The contrast comparing adolescents to adult samples was not significant ( $B = -.02, z = -0.78, p = .44$ ).

#### Examining different operationalizations of well-being.

Effect sizes were significantly negative regardless of how well-being was operationalized. However, effects based on sense of control ( $r = -.15, z = -5.38, k = 24$ ), self-esteem ( $r = -.16, z = -4.71, k = 140$ ), life satisfaction ( $r = -.17, z = -7.98, k = 37$ ), positive well-being ( $r = -.18, z = -2.76, k = 3$ ), and especially positive mood ( $r = -.06, z = -3.13, k = 21$ ) were smaller compared to effects based on psychological distress defined broadly ( $r = -.26, z = -30.06, k = 234$ ), depression ( $r = -.26, z = -21.66, k = 146$ ), anxiety ( $r = -.25, z = -15.31, k = 62$ ), and negative mood ( $r = -.22, z = -8.37, k = 21$ ). Thus, like Paradies (2006b), we found that the relationship between discrimination and negative outcomes was larger than the relationship with positive outcomes.

To determine whether patterns of moderation differed depending on how well-being was operationalized, we examined the moderators separately for effects based on self-esteem and effects based on psychological distress. As shown in Table 2, patterns of moderation for both self-esteem and psychological distress mostly replicated the moderation findings in the main analysis. Patterns of moderation were similar for ingroup status, with advantaged groups showing weaker effects than disadvantaged groups on both self-esteem and psychological distress. Longitudinal effects were significant for both self-esteem and psychological distress. The moderating effect of intergroup context was significant for both self-esteem and psychological distress—replicating the overall pattern of sexism and racism studies producing smaller effects than the other contexts. However, other patterns of moderation differed

Table 2

*Effect Sizes  $r$  and Tests of Moderation for the Relationship Between Perceived Discrimination and Different Operationalizations of Well-Being in Correlational Studies*

Moderator	Self-esteem	Psychological distress
Total	-.16** (140)	-.26** (234)
Study methodology		
Cross-sectional	-.17** (131)	-.27** (218)
Longitudinal	-.12** (9)	-.16** (16)
Between-groups $Q$	1.07	12.42**
Status of ingroup		
Disadvantaged	-.17** (128)	-.27** (209)
Mixed	-.15** (8)	-.23** (19)
Advantaged	-.04 (4)	-.12** (6)
Between-groups $Q$	3.70	10.08**
Intergroup context		
Racism	-.13** (86)	-.25** (156)
Sexism	-.09** (11)	-.22** (18)
Heterosexism	-.17** (6)	-.29** (18)
Mental illness	-.31** (15)	-.29** (7)
Physical illness/disability	-.54** (2)	-.39** (6)
HIV+	-.24** (2)	-.34** (11)
Weight	-.21** (5)	-.38** (4)
Other	-.21** (13)	-.30** (14)
Between-groups $Q$	79.74**	18.93**
Concealability		
Not concealable	-.13** (107)	-.25** (177)
Concealable	-.28** (33)	-.32** (52)
Between-groups $Q$	37.60**	11.60**
Controllability (onset)		
Not controllable	-.14** (110)	-.25** (182)
Controllable	-.26** (30)	-.32** (45)
Between-groups $Q$	19.24**	10.36**
Sample type		
Children	-.19** (12)	-.32** (14)
Adolescents	-.18** (32)	-.28** (38)
Adults	-.16** (96)	-.25** (182)
Between-groups $Q$	0.71	4.69*

Note. Numbers outside parentheses are effect sizes  $r$ ; numbers inside parentheses =  $k$ .

\*  $p < .10$ . \*\*  $p < .05$ .

for self-esteem and psychological distress. The effects of concealability and controllability were significant for both self-esteem and psychological distress, with larger effects for controllable and concealable stigmas; however, the moderating effects were stronger for self-esteem. Children showed worse effects than other sample types when effect sizes were based on psychological distress, but age group did not significantly moderate effect sizes based on self-esteem.

We also examined patterns of moderation for effect sizes based on depression, anxiety, and life satisfaction. Results for depression and anxiety were highly similar to results for psychological distress more broadly defined, and results for life satisfaction were similar to those for self-esteem. For other operationalizations of well-being, there were too few effect sizes to adequately test moderation.

**Group versus personal discrimination.** As shown in Table 3, the mean weighted effect size was significantly negative regardless of whether discrimination was operationalized in terms of group discrimination or personal discrimination. Although we cannot compare them statistically, the effect size for personal

Table 3  
*Effect Sizes  $r$  and Tests of Moderation for the Relationship Between Perceived Personal and Group Discrimination and Well-Being in Correlational Studies*

Moderator	Personal discrimination	Group discrimination
Total	-.23** (296)	-.15** (55)
Study methodology		
Cross-sectional	-.24** (277)	-.15** (52)
Longitudinal	-.14** (19)	-.16 (3)
Between-groups $Q$	10.56**	0.00
Status of ingroup		
Disadvantaged	-.24** (263)	-.16** (51)
Mixed	-.21** (25)	-.17 (1)
Advantaged	-.10** (8)	-.01 (3)
Between-groups $Q$	9.82**	2.01
Intergroup context		
Racism	-.22** (196)	-.07** (26)
Sexism	-.19** (23)	-.05 (5)
Heterosexism	-.28** (17)	-.17** (2)
Mental illness	-.35** (8)	-.27** (10)
Physical illness/disability	-.42** (8)	-.59** (1)
HIV+	-.32** (11)	-.32** (6)
Weight	-.28** (8)	-.21 (1)
Other	-.27** (25)	-.16** (4)
Between-groups $Q$	41.51**	57.49**
Concealability		
Not concealable	-.21** (230)	-.06** (32)
Concealable	-.31** (60)	-.28** (23)
Between-groups $Q$	30.69**	32.42**
Controllability (onset)		
Not controllable	-.22** (234)	-.08** (34)
Controllable	-.30** (54)	-.27** (21)
Between-groups $Q$	17.56**	20.94**
Age		
Children	-.26** (22)	—
Adolescents	-.22** (49)	-.06 (8)
Adults	-.23** (225)	-.17** (47)
Between-groups $Q$	1.46	5.93*

Note. Numbers outside parentheses are effect sizes  $r$ ; numbers inside parentheses =  $k$ . The dash indicates that there were no studies of that type to analyze.

\*  $p < .10$ . \*\*  $p < .05$ .

discrimination was larger than that for group discrimination. The effect size based on measures that included both personal and group discrimination items was significantly negative ( $r = -.25$ ,  $z = -5.45$ ,  $k = 14$ ).

Patterns of moderation by ingroup status were similar for group and personal discrimination. Effects from longitudinal studies were either significant (personal discrimination) or similar in magnitude to cross-sectional effects (group discrimination). Effects were larger for disadvantaged samples compared to advantaged samples for both personal and group discrimination. However, for other moderating variables, patterns of moderation did differ for personal and group discrimination. The pattern of moderation by intergroup context was similar for personal and group discrimination, with racism and sexism producing weaker effects than discrimination against other groups. However, moderation by intergroup context was especially clear for group discrimination. Similarly, concealability and controllability were significant moderators for both group and personal discrimination, with concealable and controllable stigmas producing larger effect sizes, but

moderation was especially strong for group discrimination. For group discrimination, effects from adolescent samples were smaller than those from adult samples. For personal discrimination, however, moderation by sample age was not apparent.

**Tests for publication bias.** Following the recommendations of Ferguson and Brannick (2012), we used a tandem procedure for detecting publication bias: (a) We calculated Orwin's fail-safe  $N$  to determine the number of file-drawer studies that would be necessary to alter the overall conclusions, (b) we visually examined funnel plots of Effect Sizes  $\times$  Standard Error and used Egger's regression method to quantify possible biases in the funnel plot, and (c) to estimate how much observed results could be influenced by bias, we implemented Duval and Tweedie's trim-and-fill procedure (see Borenstein, 2009, for details). There was no evidence for publication bias on any of the indicators.

## Discussion

Analyses of the correlational data confirm that perceptions of discrimination are negatively correlated with psychological well-being. Importantly, the relationship between perceived discrimination and well-being was significantly negative among the longitudinal effect sizes as well, offering support for the hypothesis that perceived discrimination has a causal effect on well-being. A negative relationship was evident across different operationalizations of well-being for both group and personal discrimination and for discrimination toward a range of groups.

However, the size of the negative effect of perceived discrimination varied systematically according to study characteristics. As predicted, perceptions of discrimination were more strongly linked to well-being among disadvantaged groups than among higher status groups. This effect of status is consistent with the prediction that perceptions of discrimination have more negative consequences the more they reflect pervasive discrimination but could also reflect other differences related to group status, such as the severity of discriminatory events (Schmitt & Branscombe, 2002b).

Overall, studies of racism and sexism produced weaker effects compared to discrimination based on sexual orientation, mental illness, physical disability, HIV+ status, or weight. Concealable and controllable stigmas had more negative effect sizes compared to nonconcealable and uncontrollable stigmas. The moderating role of concealability was most evident for effect sizes based on self-esteem and especially for effect sizes based on measures of group discrimination. When we examined intergroup context, concealability, and controllability simultaneously, concealability and controllability were not significant moderators, and the effects of intergroup context remained largely unchanged. Thus, the differences between intergroup contexts in the effect of perceived discrimination on well-being cannot be explained in terms of concealability and controllability alone.

In the multivariate analyses, we found that effect sizes were larger for children compared to adults. Similarly, in a meta-analysis of research on Black Americans, D. L. Lee and Ahn (2013) found that the effect of perceived racism on psychological distress was larger for children than it was for adults. When we compared the effect of age for effects involving self-esteem compared to psychological distress, we found larger effects on children for psychological distress but not for self-esteem.

The relationship between discrimination and well-being was significantly negative across a variety of operationalizations of well-being, although self-esteem, life satisfaction, and positive affect produced somewhat weaker effects than measures of psychological distress or negative affect. Thus, results suggest that perceived discrimination has negative effects on a broad range of well-being outcomes, but the relative strength of that effect varies depending on the type or aspect of well-being considered. These findings are consistent with Paradies's (2006b) review that found twice as many significant harmful effects for negative outcomes than for positive outcomes. Furthermore, the finding that self-esteem was more weakly related to perceived discrimination is consistent with Major et al.'s (2002) argument that perceived discrimination has different consequences for self-directed affect (self-esteem) than for other aspects of well-being.

Consistent with perspectives suggesting that perceived personal discrimination is more harmful than perceived group discrimination (Bourguignon et al., 2006; Crosby, 1984), perceived discrimination directed at the person was more negatively correlated with well-being than perceived discrimination directed at the group as a whole. The personal–group difference was most obvious for adolescent samples compared to adult samples, studies of racism and sexism compared to studies of other intergroup contexts, and controllable or concealable stigmas compared to uncontrollable or unconcealable stigmas.

### Meta-Analysis 2: Experimental Data

The cross-sectional and longitudinal data provide evidence consistent with the idea that perceptions of discrimination negatively affect psychological well-being; however, responses to measures of perceived discrimination are confounded with actual experiences with discrimination. Thus, it is difficult to tell with self-reports how much of the relationship between perceived discrimination and well-being reflects the effect of *perceptions* of discrimination per se and how much is the effect of actual encounters with discrimination and negative treatment (Major et al., 2002). Indeed, at least some of the relationship between measures of perceived discrimination and psychological well-being is likely due to those who report high levels of discrimination actually having faced worse treatment from others. However, by randomly allocating participants to conditions that lead them to perceive more or less discrimination while keeping life circumstances and prior experiences with discrimination constant across conditions, experimental studies can remove the confound between perceived and experienced discrimination and provide evidence for the causal effect of discrimination perceptions.

In order to compare the effects of isolated versus pervasive perceptions of discrimination, we examined two types of experimental manipulations, which together represent the bulk of the existing experimental research on perceived discrimination and well-being. Some studies compared the emotional consequences of an attribution to discrimination for a single negative event with the consequences of making a personal attribution for the same negative event. For example, Crocker et al. (1991, Study 1) presented female participants with a negative evaluation from a man who held sexist attitudes or from a man who was not sexist. As intended, women attributed their evaluation more to sexism when the evaluator was sexist. Other studies manipulated perceptions of

how frequent and widespread discrimination is against the ingroup. These *pervasive discrimination* studies typically compared a condition in which participants are led to perceive discrimination against their ingroup as pervasive in society with a comparison condition (e.g., discrimination against the ingroup was made to appear rare and isolated; Schmitt, Branscombe, & Postmes, 2003, Study 1).

There are good theoretical reasons to suppose that these two types of manipulations would produce different effects. As argued by Schmitt and Branscombe (2002b), studies manipulating pervasive discrimination are likely to harm well-being, but studies manipulating attributions for single negative events might not be harmful, as they do not necessarily reflect the perception that discrimination is pervasive across time or contexts (Branscombe et al., 1999). The attributional ambiguity hypothesis would suggest that attributions to discrimination for isolated events would have a positive effect on self-directed affect because they discount attributions to personal deservingness for the negative event (Major, Kaiser, & McCoy, 2003).

In the analysis of experimental effect sizes, we again tested the prediction that attributions to discrimination would be more harmful for participants considering discrimination against a disadvantaged ingroup than for those considering discrimination against a relatively advantaged ingroup. Likewise, we compared effect sizes from studies examining different intergroup contexts (e.g., racism, sexism). We also examined effect sizes separately depending on how well-being was operationalized, with a focus on the distinction between self-directed affect and other types of well-being (Major et al., 2002).

Age, concealability, and controllability could not be adequately examined with the experimental data because there was too little variance. When we tested those moderators, none were significant ( $ps > .4$ ).

### Method

**Computation of effect sizes.** We calculated effect sizes (Cohen's  $d$ ) with negative numbers representing a harmful effect of perceived discrimination on well-being ( $K = 54$ ,  $N = 2,640$ ). When a sample contributed more than one effect size because it contained multiple well-being measures, we created a composite effect size based on their average. For one study, multiple comparison conditions were included. Kaiser et al. (2004) compared conditions in which women read about pervasive discrimination against women with a condition in which women read about discrimination against an outgroup, as well as with a control condition with no content about discrimination. Including multiple effect sizes based on a common experimental condition would violate assumptions of independence between effect sizes. Therefore, in this case, we calculated a single effect size comparing the high-discrimination condition with the combined control conditions (the  $n$ -weighted average of means and standard deviations across the conditions). Similarly, two studies included multiple discrimination conditions. For example, Barreto, Ellemers, and Palacios (2004) compared a condition in which the ingroup had been treated fairly with two discrimination conditions in which either all or almost all ingroup members were excluded. Major, Quinton, and Schmader (2003) compared a situation in which discrimination was not a plausible attribution to a situation in

which discrimination was plausible but ambiguous and to a situation in which discrimination was blatant. For these two cases, we combined the two discrimination conditions and compared them to the control condition.

As in the correlational meta-analysis, we created different sets of effect sizes based on how well-being was measured and conducted separate analyses on each set of effect sizes. Because of the smaller number of experimental studies and a lack of variability in operationalizations of well-being, we created just two sets of effect sizes—one based on a broad definition of well-being and the second comprising only effect sizes based on self-directed affect (self-esteem, depressed affect). We focused on this distinction following Major et al. (2002), who argued that the self-protective effects of attributions to discrimination would be limited to self-directed emotions. It is worth noting that the well-being measures in the experimental studies are on the whole different from those in the correlational analysis. Most of the measures are either state measures of mood or state measures of self-esteem (Heatherton & Polivy, 1991), but a few studies measured trait self-esteem or life satisfaction.

**Coding of study characteristics.** Two people coded effect sizes based on study and sample characteristics. The coders met to resolve disagreements. For all variables, interrater agreement was high (>90%).

**Manipulation type.** Each effect size was coded based on the way perceptions of discrimination were manipulated. *Single-event studies* compared contexts in which discrimination was a plausible attribution for a negative outcome with contexts in which discrimination was not a plausible attribution. All of these types of studies examined discrimination attributions in comparison to a condition in which personal deservingness was made plausible through information provided by the researcher (Major, Kaiser, & McCoy, 2003) or because personal attributions seemed highly plausible in the experimental context (e.g., receiving negative feedback about the self or one's performance; Crocker et al., 1991). We did not include effect sizes based on comparisons between attributions to discrimination and external attributions because such comparisons are not appropriate for testing the attributional ambiguity perspective. We limited our analysis to studies in which participants actually faced negative treatment and excluded studies that used a scenario methodology—those that asked participants to *imagine* a negative outcome that was attributable to discrimination or to other causes. Thus, all the included effect sizes from single-event studies are from studies that compared well-being in a context designed to make discrimination a plausible attribution for a negative outcome and a context designed to make a personal attribution plausible for the same negative outcome.

Other studies, coded as *pervasive discrimination studies*, manipulated perceptions of the pervasiveness of discrimination against the ingroup in general. These pervasive discrimination studies (e.g., Schmitt, Branscombe, & Postmes, 2003, Study 1) typically compared conditions in which participants were led to perceive discrimination against their ingroup as pervasive in society with a comparison condition (e.g., discrimination against the ingroup was made to appear rare and isolated or participants read about discrimination against an outgroup). Other studies manipulated whether participants learned of a past pattern of discrimination against their ingroup (Barreto et al., 2004; Platow, Byrne, & Ryan, 2005). We did not include any effect sizes based on a

comparison between discrimination against the ingroup and discrimination *in favor* of the ingroup because any difference between such conditions could result as much from the effects of discrimination in favor of the ingroup as from the effects of discrimination against the ingroup. Only the latter is of interest to this investigation.

**Status of the target ingroup.** As in the correlational data, studies were categorized according to the relative status of the ingroup targeted by discrimination—whether the group was disadvantaged, advantaged, or neither/mixed.

**Intergroup context.** Effect sizes were coded based on the intergroup context. More than half of the effect sizes were based on studies of sexism ( $k = 29$ ), and 20% were based on racism ( $k = 11$ ). Nine studies examined discrimination based on an academic identity (e.g., academic major, public vs. private institutions). We categorized the five remaining studies as *other*: studies manipulating perceptions of discrimination against people stigmatized as overweight, people with visible body piercings, and people who are single.

## Results

As in Meta-Analysis 1, analyses were conducted using a random-effects model, with the random variance component determined through maximum likelihood. Overall, the mean weighted effect size across  $K = 54$  samples ( $N = 2,640$ ) did not differ significantly from zero,  $d = -.08$ , 95% CI =  $[-.18, .02]$ ,  $z = -1.51$ ,  $p = .314$ ,  $Q_w(53) = 85.00$ ,  $p_{Q_w} = .004$ .

We examined the effect of each potential moderator both independently (as shown in Table 4) and simultaneously in a multivariate analysis. Manipulation type significantly moderated the effect of perceptions of discrimination on well-being. For studies that examined the effects of perceptions of pervasive discrimination, the mean weighted effect size was significantly negative ( $d = -.25$ ,  $p < .001$ ). The effect size for single-event studies did not differ from zero. Effect sizes did not differ significantly depending on the status of the ingroup or between studies of sexism, racism, academic identities, or other types of prejudice. In the multivariate analysis, manipulation type was again the only significant moderator; a significant contrast indicated that manipulations of pervasive discrimination against the ingroup produced more negative effect sizes than the single-event studies ( $B = -.33$ ,  $z = -3.42$ ,  $p < .001$ ). None of the contrasts testing the moderating effects of status or intergroup context were significant ( $ps > .4$ ). The overall multivariate model was significant,  $Q(6) = 12.66$ ,  $p = .049$ ,  $R^2 = .19$ .

When the data set was restricted to only effect sizes involving self-direct affect ( $K = 46$ ,  $N = 2,221$ ), again the mean weighted effect sizes were not significant,  $d = -.06$ , 95% CI =  $[-.17, .05]$ ,  $z = -1.10$ ,  $p = .27$ ,  $Q_w(45) = 69.64$ ,  $p = .011$ . Examining moderation, the results for self-directed affect replicated what was found for well-being more generally—in the univariate analyses, only manipulation type moderated effect sizes (see the right side of Table 4). Effects from single-event studies did not differ from zero, and effects from pervasive discrimination studies were significantly negative. The effect of the manipulation type was also significant in the multivariate analysis of self-directed affect ( $B = -.27$ ,  $z = -2.48$ ,  $p = .013$ ), and none of the other moderators approached significance ( $ps > .4$ ). An analysis of

Table 4  
Moderators of the Effect Size  $d$  of Perceived Discrimination on Well-Being in Experimental Studies

Moderator	Well-being in general					Self-directed affect				
	$d$	95% CI	$z$	$k$	$Q$	$d$	95% CI	$z$	$k$	$Q$
Total	-.08	-.18, .02	-1.51	54	85.00**	-.06	-.17, .05	-1.10	46	69.64**
Manipulation type										
Single event	.06	-.07, .18	0.87	31	26.67	.05	-.09, .18	0.67	27	26.03
Pervasive discrimination	-.25**	-.38, -.12	-3.80	23	26.19	-.21**	-.36, -.06	-2.76	19	19.60
Between-groups $Q$					11.18**					6.27**
Ingroup status										
Disadvantaged	-.09	-.20, .03	-1.48	43	44.75	-.06	-.19, .06	-1.03	36	36.03
Mixed/irrelevant	-.08	-.39, .22	-0.53	4	3.87	-.09	-.39, .22	-0.55	4	4.02
Advantaged	-.04	-.33, .24	0.31	7	4.72	-.03	-.33, .28	-0.18	6	5.91
Between-groups $Q$					0.07					0.07
Intergroup context										
Sexism	-.04	-.18, .09	-0.62	29	24.97	-.02	-.17, .13	-0.24	23	16.18
Racism	-.11	-.36, .14	-0.82	11	11.55	-.13	-.38, .11	-1.08	11	13.79
Academic identities	-.12	-.36, .11	-1.03	9	7.72	-.11	-.33, .12	-0.91	9	9.43
Other	-.15	-.45, .14	-1.00	5	8.14*	-.06	-.40, .28	-0.33	3	6.25**
Between-groups $Q$					0.88					0.81

Note. Values in the  $Q$  column represent the heterogeneity within each level of a variable ( $Q_w$ ) or the test for heterogeneity between levels of a variable ( $Q_b$ ). CI = confidence interval.

\* $p < .10$ . \*\* $p < .05$ .

effects on self-esteem only (ignoring depressed affect) replicated the results found for self-directed affect.

**Tests for publication bias.** As in Meta-Analysis 1, we tested for publication bias following the recommendations of Ferguson and Brannick (2012). Because we hypothesized that there would be substantial differences between studies manipulating single events and studies manipulating perceptions of the pervasiveness of discrimination, we examined potential publication bias for these two categories separately. For pervasiveness studies, there was no evidence for publication bias on any of the indicators. However, there was some evidence of bias among single-event studies. Egger's regression was significant ( $p = .04$ ), and visual inspection of the funnel plot showed that there were more studies with small sample sizes and large positive effects than would be expected. Orwin's fail-safe  $N$  was 31, exactly the number of studies ( $k$ ) in this set—indicating that bias may be a borderline concern. However, the trim-and-fill method suggested that correcting the bias would not change the overall conclusions: The aggregate effect size after correction was still very small and not significantly different from zero. Thus, we conclude that there is no indication of bias among the pervasiveness studies and that although there was some indication of bias in favor of studies showing positive outcomes among the single-event studies, there is no indication this substantially affects results.

## Discussion

As expected, we found that the manipulation type moderated the effect of discrimination on well-being in experimental studies. Evidence for reduced well-being was apparent in studies that manipulated pervasive perceptions of discrimination against the ingroup. In studies that manipulated attributions to discrimination for a single negative event, however, there was no overall negative effect of discrimination on well-being. The difference in effect sizes between manipulation types supports the idea that per-

ceiving discrimination in general has more costs than making attributions to discrimination for single events. The finding that manipulations of perceptions of pervasive discrimination harm well-being allows us to infer a causal effect of perceptions of discrimination and is not subject to the alternative explanations that apply to studies based on self-reports of discrimination (which are likely confounded with actual discrimination and worse life circumstances).

Studies of single events produced a mean weighted effect that did not differ from zero. In other words, we did not find support for the attributional ambiguity hypothesis; on the whole, the meta-analysis suggests that attributions to discrimination for single events are in general neither harmful nor self-protective relative to attributions to personal deservingness.

Although there was a tendency for effect sizes from disadvantaged groups to be more negative than effect sizes from other samples, ingroup status was not a significant moderator. The intergroup context also did not moderate effect sizes. However, it should be noted that the experimental effect sizes did not permit examination of a broad range of intergroup relationships as did the correlational data.

## Within-Study Moderators

Several proposed moderators of the effect of perceived discrimination could not be examined meta-analytically because there was not adequate variance on these moderators *between* studies. Therefore, we used a vote-counting procedure to synthesize evidence for moderation that was tested *within* studies. We focused on three moderators: group identification, social support, and coping strategies. We coded tests of moderation as buffering if, at higher levels of the moderator, the negative effects of discrimination were significantly reduced; as exacerbating if higher levels of the moderator significantly increased the harm of perceived discrimination; or as nonsignificant. For

the purposes of the vote-counting analysis, we considered interactions significant if  $p < .10$ .

### Group Identification

We identified 137 tests of group identification as a moderator of the relationship between perceived discrimination and well-being, from 35 independent samples and 30 studies. Seventy-eight percent of the interaction tests were nonsignificant, 15% found that group identification buffered the effects of perceived discrimination on well-being, and 7% found that group identification exacerbated the negative consequences of perceived discrimination.

Because of the variability in the number of interaction tests conducted per sample, in a vote count of *tests*, some samples get more votes than others. Therefore, we conducted an additional analysis at the level of each sample. Forty-six percent of samples found no significant moderating effects of group identification, 40% found at least one significant buffering effect of discrimination, and 11% found at least one significant exacerbating effect. One sample found both buffering and exacerbating effects. Thus, the sample-level analysis provides tentative evidence that group identification acts as a buffer.

Because some researchers have argued that group identification is a multifaceted construct (Leach et al., 2008), we examined whether evidence of moderation differed depending on how group identification was measured. *Positive regard* for the ingroup refers to positive affect toward the group and one's membership in it (e.g., "I am glad I am a . . ."), including feelings of pride and private collective self-esteem (Luhtanen & Crocker, 1992). *Ingroup centrality* concerns the role of group membership in self-definition, including how frequently a person thinks about that identity (e.g., "I often think about the fact that I am . . .") and the subjective importance the person places on that identity as a part of self (e.g., "Being . . . is an important part of my self-image").

Of 17 individual tests of positive regard for the ingroup, one supported buffering, and the rest were nonsignificant. At the sample level, four out of five samples found no significant moderating effect of positive regard, and one sample found evidence of buffering. Of 28 tests of identity centrality as a moderator, three supported buffering, two supported exacerbation, and 23 were nonsignificant. At the sample level, two out of seven samples found at least one buffering effect of identity centrality, one found an exacerbating effect, and four found no significant moderating effects. However, for more general measures of group identification (i.e., measures that included multiple aspects of identification), 16 tests supported buffering, seven supported exacerbation, and 50 were nonsignificant. Of the 24 separate samples, 11 (46%) found no significant moderating effects, nine (38%) found at least one buffering effect, three (13%) found at least one exacerbating effect, and one found both a buffering and an exacerbating effect. Thus, there was a trend for stronger evidence of buffering when group identification was measured in a general way than when limited to positive regard or centrality.

### Social Support

We found 75 tests of social support as a moderator of the relationship between perceived discrimination and psychological well-being (28 samples, 14 articles). Social support was measured

in terms of either the availability or receipt of support from friends, family, classmates, and so on. Fifty-six (77%) of 73 tests did not find a significant moderating effect of social support, 11 tests (15%) found a buffering effect, and five tests (7%) found that social support exacerbated the effects of perceived discrimination on psychological well-being. At the sample level, in 16 (59%) of the 28 samples, there was no significant moderation, there was buffering in six (22%), and there were exacerbating effects in four (15%). In one sample, there were both significant buffering and exacerbating effects. Thus, the majority of tests did not demonstrate a significant moderating effect of social support, and of the significant interactions, there were similar levels of support for buffering and exacerbating effects. In sum, our results do not provide much evidence that social support moderates the effects of perceived discrimination.

### Coping Strategies

We divided tests of moderation by coping based on whether the coping strategy engaged with stress (e.g., problem solving, exerting control over emotional responses, cognitive reappraisals) or disengaged (e.g., avoidance, distraction). Eight tests of coping strategies did not fit into the disengagement or engagement categories. Six found no significant moderating effects, one found an exacerbating effect, and one was not codeable due to a higher order interaction. Of the 20 tests of engagement coping as a moderator, 17 were nonsignificant; the three remaining tests all supported buffering. At the sample level, two of the four samples found buffering effects, and the remaining two samples found no significant effects. Of the eight tests of disengagement coping, four were nonsignificant, and four found that disengagement strategies exacerbated the negative effects of perceived discrimination. Of the three samples that tested disengagement coping, two found significant exacerbating effects (the other found no significant effects). In sum, we found weak evidence that engagement coping strategies buffer the effects of perceived discrimination on psychological well-being and somewhat stronger evidence that disengagement strategies exacerbate effects.

### Additional Within-Study Tests of Moderation

In addition to the tests of moderation involving group identification, social support, and coping, we identified 229 additional tests of moderators that were not tested in the meta-analysis, of which 151 (66%) provided evidence for moderation ( $p < .10$ ). Describing all of those tests is beyond the scope of this article; however, together they do suggest that the relationship between perceived discrimination and well-being is not fixed and can vary depending on other factors.

### General Discussion

The results of these meta-analyses are consistent with the hypothesis that, in general, perceiving the self as a target of discrimination harms psychological well-being (Pascoe & Smart Richman, 2009). In Meta-Analysis 1, analysis of correlational studies revealed a robust negative relationship between perceptions of discrimination and well-being that was significantly negative across a variety of intergroup contexts, across a range of well-

being measures, and for both personal and group discrimination. Analysis of longitudinal effect sizes found that perceptions of discrimination negatively predicted future well-being, even when controlling for the effects of prior levels of well-being.

In Meta-Analysis 2, we analyzed the well-being consequences of experimental manipulations of discrimination perceptions. Analysis of experimental studies is critical for determining causality and for eliminating confounding variables that might be present in the correlational studies. Among studies that manipulated perceptions of the pervasiveness of discrimination, higher levels of perceived discrimination led to worse psychological well-being. However, studies comparing attributions to discrimination for a single negative event with an attribution to personal deservingness produced an overall effect size near zero. In sum, results suggest that perceiving relatively pervasive discrimination is harmful to well-being but that perceiving single instances of discrimination is no more harmful than attributions to personal deservingness.

### Pervasiveness of Discrimination

Pervasive discrimination implies rejection not just by particular individuals but by (dominant) society more generally, thwarting basic needs for acceptance and inclusion (Schmitt & Branscombe, 2002b). Because pervasive discrimination reflects negative treatment across contexts, it also implies a generalized lack of control over life outcomes (Verkuyten, 1998) and that future negative treatment is likely. Our most direct evidence of the role of pervasiveness comes from Meta-Analysis 2. Experiments manipulating perceptions of the pervasiveness of discrimination against the ingroup produced a significant negative effect size, but experiments manipulating attributions to discrimination for a single negative event produced an effect that did not differ from zero. Similarly, Stroebe, Dovidio, Barreto, Ellemers, and John (2011) found that for women who were told that discrimination was pervasive, attributions to discrimination were negatively related to self-esteem. In contrast, among women who were told discrimination was rare, attributions to discrimination were positively related to self-esteem.

Based on the idea that discrimination is likely to be seen as more pervasive and more severe for disadvantaged groups than advantaged groups, Schmitt and Branscombe (2002b) argued that perceptions of discrimination are more harmful for members of disadvantaged groups than for advantaged groups. Analysis of the correlational data supports this prediction; effect sizes from low-status samples were more negative than effect sizes from advantaged samples. We cannot determine with this analysis whether it is the pervasiveness of discrimination, severity of discriminatory events, or both that account for the status group difference. However, this difference does indicate that the meaning and implications of perceived discrimination are shaped by one's place in the social structure.

Moderation by status was not evident in the experimental studies. One mundane but plausible reason for the lack of replication in the experimental studies could be the smaller number of effect sizes resulting in less power to detect moderation. On the other hand, there are substantive differences between the experimental and correlational studies that could explain this difference. In studies that manipulate perceptions of pervasive discrimination,

high- and low-status groups are more equalized in terms of the pervasiveness of discrimination, as a function of the manipulation. A similar issue occurs in the single-event studies. In most everyday contexts, attributions to discrimination will be more likely seen as reflecting pervasive discrimination for disadvantaged groups than for high-status groups. However, in laboratory studies manipulating attributions to discrimination for specific events, discrimination will not necessarily be subjectively understood as pervasive, even for disadvantaged groups, and thus, both disadvantaged and advantaged groups might easily conceive of the discrimination encountered in the lab as relatively isolated.

### Intergroup Context

We found evidence for a negative relationship between discrimination and well-being across a range of different intergroup contexts. However, in the correlational data, which afforded examination of a variety of intergroup contexts, we did find heterogeneity in the size of that negative relationship. We found weaker effects for studies of unconcealable and uncontrollable stigmas, such as race and gender. When we examined concealability and controllability simultaneously, we found that only concealability predicted effect sizes, thus providing stronger evidence of the role of concealability than of controllability. Concealable stigmas undermine opportunities for and otherwise discourage connecting with other ingroup members to engage in mutual support. However, our results suggest that controllability and concealability are not the full story. When we examined concealability, controllability, and the specific intergroup contexts simultaneously, neither controllability nor concealability was significant, but the differences between intergroup contexts remained.

One dimension of stigma that might account for the observed intergroup differences is the degree to which discrimination based on a particular kind of stigma is socially legitimized (Hansen & Sassenberg, 2011; Major et al., 2002). Unfortunately, a limitation of our analysis is that we did not code for legitimacy, which means we can only speculate about its moderating role. That said, the intergroup contexts that produced the weakest effects (racism and sexism) are ones for which discrimination tends to be seen as illegitimate, while intergroup contexts that produced larger effects tend to be more socially legitimized (heterosexism, discrimination against people who are mentally or physically disabled or who are HIV+ or overweight). Given that appraisals of the legitimacy of group-based treatment are central to both individual psychological well-being and collective resistance to oppression (Jetten et al., 2011), future meta-analyses would benefit from greater attention to legitimacy appraisals.

### Type of Well-Being

On the whole, we did not find markedly different effects depending on the type of well-being under consideration. In the correlational analysis, the effect size was significant and negative regardless of the way that well-being was operationalized. Like Paradies (2006b), however, we found that perceived discrimination was more strongly related to negative outcomes such as depression, anxiety, and psychological distress than to positive outcomes such as self-esteem, life satisfaction, and positive affect. The relatively weak effect for self-esteem resonates with the

argument made by Major et al. (2002) that the consequences of perceived discrimination differ for self-directed affect compared to other kinds of well-being. On the other hand, Major et al.'s predictions regarding self-directed affect were specific to attributions to discrimination for a specific negative event. When we examined experimental studies, including those that manipulated attributions for a single event, we did not observe differences between self-directed affect and well-being more generally.

### Group Versus Personal Discrimination

Supporting the idea that it is more threatening to perceive discrimination against the individual self than against the ingroup as a whole (Bourguignon et al., 2006; Crosby, 1984), effect sizes based on measures of group discrimination were smaller than those based on personal discrimination. However, there are other findings that complicate our understanding of this difference between group and personal discrimination. In the correlational studies, the difference between group- and personal-level discrimination was more apparent for some groups (racism, sexism) than others (mental illness, HIV+). Thus, although we found evidence for the differential consequences of group and personal discrimination, those differences were not invariant and were not observed for certain kinds of stigma.

### Perceptions of Discrimination and Discounting Self-Blame for Negative Events

The attributional ambiguity perspective claims that perceived discrimination can protect self-directed affect when an attribution to discrimination for a negative event shifts responsibility away from one's personal deservingness to a prejudiced other (Crocker & Major, 1989; Major, Kaiser, & McCoy, 2003). When we examined the subset of studies relevant to the attributional ambiguity hypothesis—studies comparing an attribution to discrimination to an attribution to personal deservingness—the effect size did not differ from zero. Thus, we do not have much evidence to support the attributional ambiguity hypothesis. We also do not have evidence that attributions to discrimination are necessarily *harmful* in comparison to attributions to personal deservingness. Both of these attributions—"I personally deserve this negative treatment" and "I'm being treated negatively because of my group membership"—are threatening, and neither is obviously more harmful *relative to the other*. It seems likely that the consequences of specific attributions to discrimination compared to personal deservingness depend on a potentially large array of individual and situational factors that influence the meaning and costs of both attributions to discrimination and plausible alternative attributions. Indeed, work on the attributional ambiguity hypothesis has backed away from predicting a main effect of attributions to discrimination compared to personal deservingness and has instead begun to examine many possible moderators, including optimism (Kaiser et al., 2004), group identification (McCoy & Major, 2003), pervasiveness of discrimination (Schmitt, Branscombe, & Postmes, 2003), group status (Schmitt & Branscombe, 2002a), threats to fairness (Stroebe et al., 2011), and threats to worldview (Major et al., 2007).

Although we found no evidence that attributions to discrimination are generally self-protective, our findings do suggest that

attributions to discrimination have more *potential* to protect well-being if discrimination is perceived to be an isolated occurrence. However, this meta-analysis makes clear that attributions to discrimination are not likely to be self-protective over the long term. If people repeatedly make attributions to discrimination across different social contexts, they are likely to come to see discrimination as pervasive. As that happens, such attributions are likely to become more costly than self-protective.

### Developmental Stage: Implications for Children

In the correlational meta-analysis, we found that effect sizes were larger for children than for other samples, particularly on psychological distress. D. L. Lee and Ahn (2013) also found that the effect of perceived racism on psychological distress was larger for children than it was for adults. Although more research is necessary before we can confidently interpret this finding, we speculate that as children develop and are socialized, they acquire resources and strategies to more effectively cope with discrimination, thus weakening its effect as they become adults. Although age was not a central variable in our analysis, if children do indeed suffer more from perceived discrimination, it is of both practical and theoretical significance to investigate these effects further with younger populations.

### Group Identification

Like prior reviews of the moderating effect of group identification (Pascoe & Smart Richman, 2009), our results do little to clarify group identification's moderating role in the relationship between perceived discrimination and well-being. Some researchers have argued that group identification can serve a protective function and thereby reduce the costs of perceived discrimination (e.g., R. M. Sellers et al., 2003). In contrast, McCoy and Major (2003) suggested that group identification, particularly the centrality of the group identity to the self, makes perceptions of discrimination more *harmful*. Our results provide some weak support for the buffering hypothesis—although most tests of group identification as a moderator were nonsignificant, of those that were significant there was greater support for a protective effect of identification than an exacerbating effect.

We examined three different operationalizations of identification—either as two separate components, centrality and positive regard, or as a more general measure of identification. We found stronger evidence of buffering for more general measures. General measures of identification might show more evidence of buffering because the buffering results from aspects of identification other than centrality or positive regard (e.g., solidarity) or because the more general construct of identification provides the most stable indication of what it means to be identified with one's group (Postmes, Haslam, & Jans, 2013).

Future research on the moderating effects of group identification should consider the ideological content of identity, not just the strength of attachment. For example, R. M. Sellers and Shelton (2003) found that among Black Americans, having a nationalist racial identity reduced the distress caused by discrimination, but identity centrality had no effect. Future research should also attend to how perceptions of legitimacy moderate the effects of group identification; Hansen and Sassenberg (2011) found that group

identification had a buffering effect in contexts in which discrimination was appraised as illegitimate, but group identification exacerbated the costs of perceived discrimination when discrimination was legitimized.

Future meta-analyses might usefully consider the role of group identification from the perspective of the rejection–identification model, which argues that while the overall effect of perceived discrimination on well-being is negative, perceived discrimination also encourages group identification, which in turn positively affects well-being (Branscombe et al., 1999). In other words, group identification increases in response to perceived discrimination, and that partially counteracts (suppresses) the negative effect of perceived discrimination. The rejection–identification model has received support in studies examining discrimination against a variety of groups, including Black Americans (Branscombe et al., 1999), Latina/o Americans (Cronin, Levin, Branscombe, van Laar, & Tropp, 2012; Spencer-Rodgers & Collins, 2006), women (Leonardelli & Tormala, 2003; Schmitt et al., 2002), older adults (Garstka et al., 2004), and people who are HIV+ (Molero, Fuster, Jetten, & Moriano, 2011). Other studies, however, have offered only qualified support (see Armenta & Hunt, 2009; Bourguignon et al., 2006; Fernández et al., 2012; Postmes & Branscombe, 2002) or provided evidence inconsistent with the model (Eccleston & Major, 2006). Perceived discrimination does not always encourage identification and can sometimes undermine group identification, particularly when discrimination is socially legitimized (e.g., Jetten et al., 2011). Other studies have shown that the relationship between group identification and well-being is sometimes negative (Hansen & Sassenberg, 2011) or a mix of both positive and negative effects (Crabtree, Haslam, Postmes, & Haslam, 2010). A systematic review of the literature on perceived discrimination and group identification, as well as on group identification and well-being, would help establish the conditions that facilitate or deter group identification's ability to suppress the negative consequences of perceived discrimination.

### Caveats and Future Directions

Our findings confirm that perceived discrimination in general represents a threat to psychological well-being. However, it is important to note that the consequences of *perceiving* discrimination are not exclusively negative, particularly if discrimination really is a frequent part of one's life experience. For members of disadvantaged groups, perceiving discrimination is part of forming a functionally accurate understanding of the world. Although denial and minimization might have some psychological benefits, they will not be helpful in dealing with the problem directly or bringing about change. Perceiving discrimination might hurt psychological well-being, as this meta-analysis has revealed, but it is also critical for choosing effective coping strategies. Recognizing discrimination could help people avoid situations in which they are vulnerable to mistreatment or allow them to strategically compensate for other people's biases in order to improve their chances in life (C. T. Miller, Rothblum, Felicio, & Brand, 1995). Perceiving discrimination might also be important for accurate assessment of one's skills and abilities—recognizing that one's outcomes might be determined by discrimination rather than a lack of competency (Major et al., 2002).

Perceptions of discrimination are necessary for individuals to protest specific instances of discrimination and for building the type of collective movements that historically have lifted the position of minority groups (Foster, 2000; Tajfel & Turner, 1979). Thus, while recognizing discrimination harms individual psychological well-being, it might also lead to collective resistance that reduces the pervasiveness and severity of discrimination. The extent to which coping strategies can fully attenuate the costs of perceived discrimination depends not only on individual resilience but also on how effectively the group as a whole can rally together to bring about social change. In addition to improving the collective lot, endorsing and participating in collective action can increase feelings of empowerment (Drury & Reicher, 1999) and improve individual psychological well-being (Cronin et al., 2012; Molero et al., 2011; Outten, Schmitt, Garcia, & Branscombe, 2009).

Meta-analyses provide an opportunity to reflect on what methodologies and paradigms have been applied to a particular research question. Because the bulk of the empirical literature on perceived discrimination and well-being is correlational, experimental studies play an important role by directly testing causal relationships. However, existing experimental work has its own limitations. In particular, researchers need to expand on the types of discriminatory situations investigated in experimental studies. The majority of the experimental work has been on single discriminatory events in which participants receive negative feedback on performance or a negative personal evaluation, usually from a peer. Although negative feedback and evaluations are certainly part of the experience of discrimination, they are a narrow representation of the many kinds of discriminatory events that people encounter. Furthermore, the bulk of experimental work examines the consequences of perceived discrimination in comparison to attributions to one's ability, performance, or other indicators of personal deservingness. However, there are many forms of discrimination for which attributions to personal deservingness are not a plausible comparison, such as being the target of a hate crime, being the recipient of derogatory labels, or being avoided on a public bus. Finally, experimental work has mostly focused on sexism—likely due to the convenient access to women participants on university campuses.

### Concluding Remarks

Confirming findings of prior research synthesis (Pascoe & Smart Richman, 2009), we found that perceiving discrimination was negatively related to psychological well-being. Going beyond prior research, the present meta-analysis found that this negative relationship was present across a wide range of well-being measures and for both personal and group discrimination. In the analysis of correlational data, effect sizes from disadvantaged samples were larger, indicating greater harm, compared to effects from advantaged groups. Perceived discrimination was negatively related to well-being across a range of stigmas; however, compared to discrimination based on gender or race, perceived discrimination was more strongly related to well-being for sexual minorities, people with mental illness, people with a physical disability, and people stigmatized as overweight. Although most studies in this review were correlational and thus subject to alternative causal explanations, we found evidence of a causal effect of

perceptions of discrimination in longitudinal studies and in studies manipulating perceptions of pervasive discrimination. In contrast, effects were not different from zero in studies manipulating discrimination attributions for a single negative event. Overall, these analyses make clear that perceiving pervasive discrimination negatively affects well-being, but perceiving isolated events as discrimination is less likely to harm well-being.

## References

References marked with a <sup>C</sup> contributed to the correlational analysis (Meta-Analysis 1), those marked with an <sup>L</sup> contributed longitudinal effects (Meta-Analysis 1), and those marked with an <sup>E</sup> contributed experimental effect sizes (Meta-Analysis 2).

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