Discrimination, Life Stress, and Mental Health among Muslims: A Preregistered Systematic Review and Meta-Analysis

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Graduate school is not an easy path, but there is nothing else I would have rather spent the last six years doing, and that is especially thanks to all the people that I met along the way.

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To all the Muslim participants: thank you for participating in these studies and sharing your experience with us. I hope we can learn from these findings to create change.

Dedications Page

I dedicate my life to my first love, my sister, Zahara.

You wrote, “I want to be active in my job, I want to meet new people every day in a real way- not on a transactional basis. I want to explore, discover and be surprised OFTEN. I want to experience all…the joy, sadness, love, loneliness that life brings. I don’t have a picture of my life except for what I want to feel.”- *Zahara Kathawalla*

As a counseling psychologist, I can promise I will connect with people in a real way and truly feel all the feelings life has to offer.

Abstract

Research suggests that experiences of discrimination and life stressors are associated with negative mental-health outcomes for marginalized populations. Studies focusing on Muslim populations in western countries have found *varied* associations between negative mental-health outcomes and Islamophobia, anti-Muslim prejudice, and discrimination. We use meta-analytic methods to examine two predictors of mental health, thereby contributing to the debate on the impact of discrimination vs. life stressors on the development of mental-health outcomes. These meta-analyses include 295 correlations from 130 unique samples and 27,725 individuals, thus synthesizing the associations of both (a) perceived discrimination and mental health, and (b) life stressors and mental health for Muslims living in western countries.

Discrimination was significantly associated with negative mental-health outcomes with an omnibus effect-size estimate between r = .22–.23. Discrimination was most strongly associated with depression (r = .30). We show that both perceived individual discrimination and group discrimination were significantly associated with worse mental-health outcomes, and the association was stronger for experiences of individual discrimination. We found that the association between discrimination and mental-health outcomes was consistent across ethnicity and continent. Some between-study variability for the discrimination dataset was explained by discrimination level, mental-health outcome direction, number of discrimination measure items, and refugee status.

Life stressors were significantly associated with negative mental-health outcomes with an omnibus effect-size estimate between r = .32–.37. Life stressors were most strongly related to somatization (r = .41) and Post-Traumatic Stress Disorder, or PTSD (r

= .36). Some between-study variability for the life stressors dataset was explained by publication bias, sample population, number of life stressor measure items, continent, and ethnicity. Both omnibus effect-size estimates were robust to publication bias, outliers, and within-study dependence.

We also conducted a qualitative review of identity moderators and suggest future directions. It is important to note that we synthesized the unique associations of discrimination and life stressors on mental health, showing that both are important and distinct predictors of Muslim mental health. In the current sociopolitical climate, this study is an important step to better serve the mental health needs of the growing global Muslim community.

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Introduction

Muslims have a long history of being "othered" and subjected to discrimination, dating from the colonization of land ruled by the Ottoman Empire to U.S. Executive Order 13769 (also known as the 'Muslim ban;' in 2017), a law banning individuals from Muslim-majority countries from attaining a U.S. visa (Poynting & Mason, 2007; Runnymede Trust, 1997). Indeed, Islamophobia is widespread in contemporary society, evidenced by anti-Muslim stereotypes and propaganda across North America, Europe, New Zealand, and Australia (Trevino, Kanso, & Nelson, 2010; Ahmed, 2019). As a result, a pressing question has emerged: how are the patterns of Islamophobia impacting the psychological well-being of Muslims living in western countries?

Evidence suggests that experiences of discrimination and racism are associated with negative mental-health outcomes for marginalized, minority populations (e.g., Paradies et al., 2015). A large majority of studies, specifically with Muslim populations living in Western countries, has found an association between perceived discrimination and negative mental-health outcomes (Samari, Alcalá, & Sharif, 2018). However, the results have been inconsistent across studies, suggesting the need to synthesize existing findings to identify a summary effect and examine potential moderators. Moreover, current literature on the etiology of mental health for minorities has overlooked the association between general life stressors and mental health. Accordingly, the purpose of the present study is to conduct a systematic review and meta-analysis of the correlations between perceived discrimination and life stressors as it relates to mental health among Muslims living in western countries.

Racialization, Islamophobia, and Discrimination

Understanding the experiences of Muslims living in western countries requires knowledge of the interrelated contexts of racialization, Islamophobia, and discrimination. Herein, we discuss each.

Racialization

Racialization is the process through which racial groups are formed (Garner & Selod, 2015). Race is socially constructed and situated within a system of power, often used to disadvantage certain groups within society. Racism includes stereotypes, prejudice, and discrimination[1], and occurs overtly, covertly, intentionally, unintentionally, and institutionally (Fish & Syed, 2018; Harrell, 2000). There is little evidence for a biological basis for race; instead, the differences between groups are due to consequences of the majority imposing power onto the minority (see Syed & Kathawalla, 2018 for discussion). Thus, even though Muslims are quite diverse with respect to ethnicity, nationality, and religious orientation, the universal anti-Muslim sentiment has "racialized" Muslims, creating a racial and pan-ethnic group (Cesari, 2014; Richardson, 2012; Selod, 2015).

Conceptualizing Muslims as a racialized group can be somewhat confusing due to their heterogeneity and because they are often thought of as a religious group of members that can share *some* aspects of culture (e.g., tradition, language, food). It is only in the global context of Islamophobia that the group became racialized (Meer, 2019). Bolstering this conceptualization is the fact that the racialization process has extended to non-Muslims whom people think are Muslim (e.g., Sikhs, South Asians of other religious, Christian Arabs); this creates a "Muslim-like" identity group that has been racialized as

[1] The term discrimination includes racial, ethnic, and religious discrimination.

Muslims (Meeussen et al., 2013; Abdulrahim, James, Yamout, & Baker, 2012). And indeed, the context of Islamophobia has been connected to individuals conceptualizing their own identity as Muslim (Wang, Raja, & Azhar, 2019) and working toward combating racism and prejudice (Cesari, 2014; Garner & Selod, 2015; Selod, 2015; Meer, 2019).

Islamophobia

As noted above, the racialization of Muslims has occurred in the context of Islamophobia, which was originally defined as an irrational fear toward Muslims and Islam that results in explicit and subtle discrimination toward Muslim people (Runnymede, 1997). Islamophobia involves negative beliefs about Muslim values—that they hate freedom, oppress Muslim women, and want to instill sharia law (Islam, 2018). Moreover, there is a common rhetoric that Muslim faith leads to less commitment within mainstream value systems in the U.S. and Europe (e.g., Hervik, 2019; Ahmed, 2019; Martinovic & Verkuyten, 2012). As a *phobia*, Islamophobia involves a sense of "justified fear" that allows for justification of discriminatory or racist actions toward Muslims for protection or survival of majority group members. Media portrayals of radicalization of Muslims and the European refugee "crisis" provide justification for the fear (Awan & Zempi, 2016; Jackson, 2010; Pew Research Center, 2017; Shaver, Sibley, Osborne, & Bulbulia, 2017; Trevino et al., 2010; Vedantam, 2017), even in the face of contradictory evidence. For example, most of the terrorist attacks (and the deadliest) carried out in the name of Islam have occurred in the Middle East, North Africa, South Asia, and Sub-Saharan Africa (Miller, 2017). In addition, white supremacists carried out more attacks than Islamists in the U.S. from 2008–2016 (Neiwert, 2017). Most importantly, the

majority of Muslims in the world do not engage in violent attacks, and thus the fear of Muslims is *unjustified*. Nevertheless, fear of Muslims and Islam has been connected to anti-terror initiatives, such as the anti-Muslim laws in Europe and more recently in the United States (Neiwert, 2017; Sivanandan, 2010).

Whereas Islamophobia pertains to group-level discrimination, individual-level discrimination directed towards Muslims must also be considered. In the context of Islamophobia, there has certainly been an increase in anti-Islamic or Muslim hate-related incidents in the U.S. over the past 20 years (see Figure 1; Federal Bureau of Investigation, 2000-2019). Studies including Muslims in the U.S., Canada, and Europe have consistently found high rates of perceived discrimination directed to oneself or toward people they know (Ashraf & Nassar, 2018; Sirin & Fine, 2007; Nadal et al., 2012; Brüß, 2008; Wilkins-Laflamme, 2018). Qualitative research has also found that Muslims report unique experiences of discrimination compared to other marginalized, minority groups in the U.S. (Nadal et al., 2012; Sirin & Fine, 2007) and Europe (e.g., Moffit et al., 2018). In a related national study amongst Canadians, it was found that non-Muslims reported the lowest rating toward Muslims living in Canada compared to other minority groups (Wilkins-Laflamme, 2018).

Discrimination

Discrimination is "any behavior which denies individuals or groups of people equality of treatment" based upon social identities or characteristics outside of one's control (e.g., religion, color of skin, gender, sexuality; Stroebe & Insko, 1989, p. 50). The overall experience is the same—unfair treatment—but the focus or perception of the unfair treatment may differ based upon domain. We use ethnic, racial, and religious

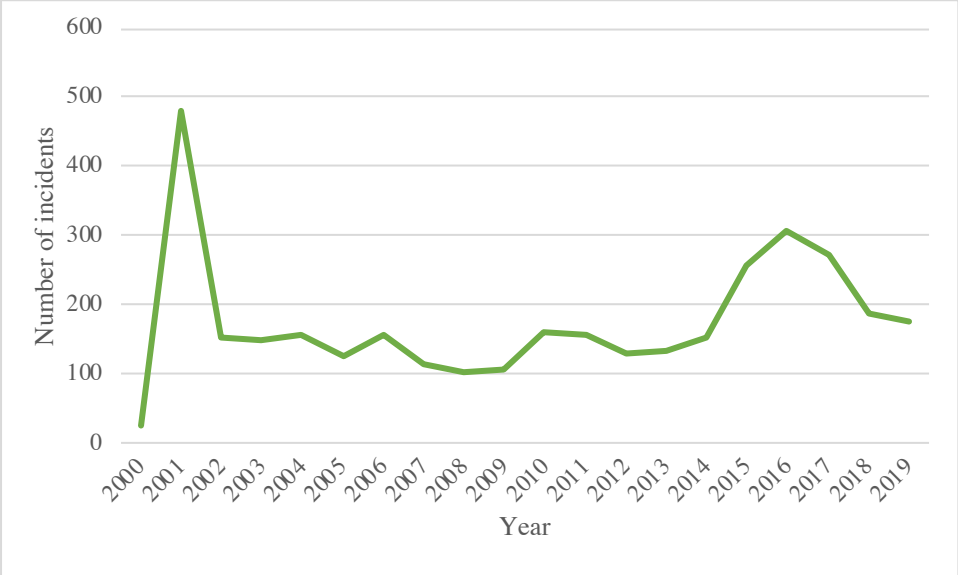
discrimination interchangeably due to evidence that discriminatory events are motivated by all three types of discrimination (i.e., being called a terrorist, comments about the hijab, and being told to go back to their own country; Nadal et al., 2012) and also due to the "racialization" of this group described earlier (Garner & Selod, 2015). In a systematic review of Islamophobia and health for Muslims, findings suggest that experiencing racial, ethnic or religious discrimination is associated with increased psychological distress (Samari et al., 2018); evidence indicates similar levels of perceived discrimination and impact of Islamophobia across ethnic groups (Abu-Ras, Suárez, & Abu-Bader, 2018).

As mentioned previously, discrimination can be conceptualized on multiple levels. Individuals may experience individual-level discrimination that includes, implicit racism (e.g., unconscious biases), subtle discrimination (e.g., being told they speak English well), explicit discrimination (e.g., being hit or called names) and group/systemic discrimination (e.g., hearing others talk about how Muslims are dangerous and the Muslim immigration ban; Fish & Syed, 2018). Research has largely found negative associations between discrimination and mental health for all levels, but there is also evidence for differential associations with mediator and moderator variables between individual and group discrimination (Schmitt, Branscombe, Postmes, & Garcia, 2014; Lui & Quezada, 2019). Research based in Europe has found that anti-Muslim prejudice is a distinct construct; even though it is related to other forms of prejudice (e.g., cultural, economic), it is not related to blatant discrimination (Elchardus & Spruyt, 2014). In contrast, meta-analytic results between subtle and overt discrimination have similar associations with health, demonstrating there may not actually be a difference in the effect on outcomes (Jones, Peddie, Gilrane, King, & Gray, 2016). In total, meta-analytic

results suggest the need for further examination of the association of individual vs. group-based discrimination with mental health.

Figure 1

Number of Anti-Islamic (Muslim) hate crime incidents in the United States.



Data retrieved from the Federal Bureau of Investigation Hate Crime Reports 2000-2019.

Life Stressors and Discrimination

Despite the conceptual overlap, life stressors and discrimination are discussed as entirely separate constructs, which may or may not be the case. Life stressors are "conditions and stimuli where predictability and controllability are at stake" (Koolhaas et al., 2011, p. 1292). Additionally, life stressors are life events that "are negative, fateful, unpredictable, and central in the sense of being life-threatening or otherwise disruptive of the usual activities and major goal of the individual" (Dohrenwend, 2000, p. 13). What is considered a life stressor can be vague and broad, including diverse types of stressors, such as: financial hardship, unemployment, and caregiving stress. For the purposes of this review, we focus on the definition of life stressors as defined by The Traumatic Life Events Questionnaire (TLEQ; Kubany et al., 2000) and the abuse items of the Behavioral

Risk Factor Surveillance System (BFRSS) Adverse Childhood Experiences (ACEs) questionnaire (Centers for Disease Control and Prevention, retrieved 2019). The TLEQ was developed to assess exposure to a wide range of traumatic events, including 18 specific events (e.g., natural disaster, war trauma). The BFRSS ACEs questionnaire includes five abuse-related items, including physical abuse (one item), psychological abuse (one item), and sexual abuse (three items). Traumatic life events and adverse childhood experiences are commonly examined in relation to the development of psychopathology in majority groups (e.g., Steel et al., 2009; Hughes et al., 2017).

The literatures on life stressors (as defined above) and perceived discrimination are almost entirely separate. Life stressors tend to be the focus in research with majority group members, whereas discrimination is a stronger focus in research with minority group members. Research undeniably supports the notion that stress exposure is shaped by individuals' context, which is certainly the case for discrimination; and discrimination is consistent with the definition of stress as being negative, unpredictable, and uncontrollable (Dohrenwend, 2000; Koolhass et al., 2001). Substantial theoretical evidence suggests that discrimination is a unique stressor, given that discriminatory actions are based upon factors (e.g., ethnicity, race, stereotypes), all of which are uncontrollable; but this is rarely tested in conjunction with life stressors (Harell, 2000).

Harrell's (2000) model of racism-related stress is one of the few—if not the only—model that equally emphasizes both discrimination and general life stressors in the development of psychopathology. Additionally, the model of racism-related stress postulates the moderator role of internal characteristics. The model of racism-related stress (Harrell, 2000) separates racism-related stress into six different types of stressors:

1) racism-related life events, 2) vicarious racism experiences, 3) daily racism micro-stressors, 4) chronic-contextual stress, 5) collective experience of racism, and 6) transgenerational transmission of group traumas (Harrell, 2000). The model posits that racism-related stress is differentially associated with physical, psychological, social, functional and spiritual outcomes due to antecedent variables (e.g., race, gender, age), familial and socialization influences (e.g., family characteristics, racial socialization), and internal and external mediators (e.g., self-esteem, coping, cultural values, spirituality, identity; Harrell, 2000). Although the model's focus is on racism-related stressors, Harrell (2000) emphasizes the importance of also considering the stressors "common to the mainstream of society" and outside of the minority status, as the combined effect may be especially detrimental (Harrell, 2000; p. 47).

The studies that have examined both discrimination and life stressors have indicated mixed results regarding the strength and significance of the associations (e.g., Pieterse, Carter, & Ray, 2013; Pieterse & Carter, 2007; Hassouneh & Kulwicki, 2007; Alemi & Stempei, 2018). Some findings even suggest that perceptions of discrimination contribute almost nothing to the racial differences in health outcomes compared to the contribution of life events and traumas (Taylor & Turner, 2002). Hence, a major goal of the current study is to examine both the association between perceived discrimination and mental health, and the association between life stressors and mental health, for the purpose of better understanding both constructs' contribution to health outcomes.

Stressors and Health

The focus of the present study is on the associations between stressors (discrimination and life stressors) and mental health. We focus on mental health due to

the downstream effects of mental health on other physical health and social outcomes (e.g., Adam et al., 2017) and the well-supported evidence of the impact of discrimination and stressors on mental health (Pascoe & Smart Richman, 2009).

Discrimination

Meta-analytic results demonstrate racial/ethnic discrimination is associated with adverse physical and mental-health outcomes (Pascoe & Smart Richman, 2009; Benner et al., 2018; Paradies et al., 2015; Pieterse, Todd, Neville, & Carter, 2012; Schmitt et al., 2014). The meta-analytic correlations vary slightly based upon type of discrimination, outcome, and sample. Generally, experiences of discrimination have shown small to moderate associations with negative mental health and a small negative association with positive mental health (Benner et al., 2018; Pascoe & Smart Richman, 2009; Schmitt et al., 2014; Lee & Ahn, 2011). There is evidence of similar associations between different types of racism (e.g., direct, group, vicarious) and negative mental-health outcomes (Paradies et al., 2015). It is important to note that there seem to be differences based upon study design, with the association being nonsignificant for experimental studies and significant, but attenuated, in longitudinal studies (Schmitt et al., 2014).

A recent systematic review of Islamophobia and health summarizes the results of 34 articles examining mental health in Western countries (out of 53 in the review; Samari et al., 2018). The review found support for a consistent relationship between experiences of discrimination and poorer mental health among Muslims and Muslim-like populations (Samari et al., 2018). Moreover, the association between discrimination and psychological distress (e.g., depressive symptoms, anger, and anxiety) was consistent for discrimination due to race, ethnicity, and religion (Samari et al., 2018). However, there

was heterogeneity in effect sizes and significance levels between discrimination and various types of outcomes, which were not explored (e.g., Ahmed, Kia-Keating, & Tsai, 2011; Assari & Lankarani, 2017; Ellis, MacDonald, Lincoln, & Cabral., 2008; Hakim, Molina, & Branscombe, 2017; Rippy & Newman, 2006). To summarize, although there seems to be consistent evidence for the negative associations of various types of discrimination with mental-health outcomes for Muslims in Western countries, there is variation in the literature potentially due to study-related decisions, demographic, or other factors.

Life Stressors

A body of evidence suggests that traumatic events (e.g., Steel et al., 2009) and adverse childhood experiences (ACEs; Hughes et al., 2017; Kessler et al., 2010) are associated with negative mental-health outcomes. A meta-analysis on the effects of torture and potentially traumatic events among populations exposed to mass conflict and displacement from 40 different countries, found that torture was associated with PTSD and depression (Steel et al., 2009), and that potentially traumatic events were also associated with PTSD and depression (Steel et al., 2009). Additionally, previous trauma, general childhood adversity, and childhood abuse demonstrate meta-analytic effects with PTSD (Brewin, Andrews, & Valentine, 2000). With respect to ACEs, individuals with at least four ACEs were at greater risk for anxiety, depression, and low life satisfaction (Hughes et al., 2017). These meta-analyses provide evidence for a small to moderate association between negative life events and negative mental-health outcomes.

Studies that include both discrimination and general life stressors for Muslim samples suggest mixed results regarding the association of life stressors and mental

health. For example, perceived discrimination and life stressors are both unique predictors of mental-health outcomes (e.g., Alemi & Stempei, 2018; Mölsä, Kuittinen, Tiilikainen, Honkasalo, & Punamäki, 2017[2]), but effect sizes varied across studies. Conversely, studies also found that both predictors were not significantly associated with specific negative mental-health outcomes (e.g., Mölsä et al., 2017; Hassouneh & Kulwicki, 2007). These differential results not only emphasize the need for rigorous statistical methods to clarify the effect, but also suggest the importance of both discrimination and general life stress as predictors for mental-health outcomes.

In sum, there is research on the impact of discrimination and life stressors, but results are somewhat varied for Muslims, and very few studies examine both predictors (with minority groups generally or Muslims specifically). Thus, to address this, in the proposed study, we examine the associations of both perceived discrimination (individual and group) and life stressors with mental health. Examining both discrimination and life stressors may be important based upon preliminary qualitative interviews of second-generation Muslim Americans (n = 53; Kathawalla & Syed, 2019). Qualitative work indicated most second-generation Muslim Americans experienced discrimination or systematic racism in the United States but seemed to be differentially affected by the discrimination based upon life adversity, demographic factors, and multiple intersecting identities (Kathawalla & Syed, 2019). Additionally, some participants experienced individual-level discrimination, whereas others experienced group discrimination (Kathawalla & Syed, 2019; Wheeler, Kathawalla & Syed, 20192019). Thus, based upon

[2] To note, Mölsä and colleagues (2017) is not included in the meta-analysis as we were unable to extract a bivariate correlation from the data and authors did not respond to our email request for the data and/or the bivariate correlations.

the current state of the literature and previous qualitative work, there are added benefits for further investigation of discrimination and life stress as predictors of mental health.

Potential Moderators

Demographic

Meta-analyses and systematic reviews consistently support investigating age, gender, immigration status, education, and region as moderators of stressors and mental health due to the theoretical support of their potential impact (e.g., Harrell, 2000). In the recent systematic review of health effects of Islamophobia, gender moderated the relation, such that discrimination and psychological distress were significant only for men in some studies (Samari et al., 2018; Assari & Lankarani, 2017; Rippy & Newman, 2006). However, a meta-analysis examining the relation between racism and mental health did not find any significant moderation effects with age, sex, birthplace, or education level (Paradies et al., 2015). Conversely, meta-analytic results for PTSD demonstrate consistent support for lack of education being as a risk factor, but results vary based upon sample and method (Brewin et al., 2000). There is some evidence that generational status or years living in the U.S. affects the association between stressors and mental health (Abu-Ras et al., 2018). Thus, evidence suggests a moderation effect for gender and generational status, but there is less clarity regarding education. Results from a systematic review demonstrated that the associations between discrimination and mental health were not affected by racial identification, skin color, or area of residence (Samari et al., 2018). In the current study, we aim to test potential demographic moderators including, but not limited to gender, age, immigrant status, and country/region.

Identity

Harrell's (2000) model of racism-related stress includes identity domains or personal factors as potential moderators of the association between stressors and mental-health outcomes. Identity is especially relevant to Muslims, given that the context of Islamophobia may lead individuals to want to distance themselves from their identity and negative Muslim stereotypes (Sadek, 2017). Thus, there is not only a negotiation between Muslim values and Western values, but there is also a negotiation between Muslim religious values, Muslim secular values, and Muslim fundamentalism (Sadek, 2017; Martinovic & Verkuyten, 2012). For example, in a study of Muslims living in New Zealand, stronger Muslim identification was associated with increased susceptibility to the negative effects of perceived religious discrimination (Jasperse, Ward, & Jose, 2012).

Erikson's (1968) theory of identity development asserts that at various stages of development, healthy development involves negotiation of tensions in order to build a coherent and healthy sense of self. Life stressors and adversity create obstacles that make negotiating these tensions potentially more challenging. Erikson (1968) describes psychopathology as severe symptoms of identity confusion. Thus, Erikson's theory highlights the importance of culture and social context on identity development.

The literature suggests multiple domains of cultural identity, including, but not limited to ethnic identity, national identity, identity integration, religious identity, and visible identity. However, there are many domains of identity, and most studies include only one identity construct while excluding others. It is unclear which aspects of identity may be more protective or whether there are differential mitigating factors of identity for ethnic minorities (e.g., ethnic identity commitment is a protective factor, while ethnic

identity exploration is an exacerbating factor; Yip, Wang, Mootoo, & Mirpuri, 2019; Phinney, 1990).

For example, ethnic identity may be an important protective factor for Muslims that may mitigate some of the negative effects of discrimination and stress on the development of mental health (e.g., Sheldon, Oliver, & Balaghi, 2015; Ahmed et al., 2011; El Bouhaddani, van Domburgh, Schaefer, Doreleijers, & Veling, 2019). However, there evidence also suggests that stronger ethnic identities may be associated with more experiences of perceived personal and group discrimination for Muslims (Verkuyten & Nekuee, 2001). In relation, there is support for the impact of American identity (e.g., Sirin et al., 2008), identity integration (e.g., Britto & Amer, 2007; Saleem, Dubow, Lee, & Husemann, 2018; Sirin & Fine, 2007; Isik-Ercan, 2015; Saroglou & Galand, 2004; Lowe et al., 2018), religious identity (e.g., Abu-Ras et al., 2018; Ahmed et al., 2011; Adam & Ward, 2016; Peek, 2005), and visible identity (e.g., Awan & Zempi, 2016; Abdulrahim et al., 2012; Gulamhussein & Eaton, 2015; Selod, 2015; Rahmath, Chambers, & Wakewich, 2016) on mental health or experiences of perceived discrimination for Muslims.

Study-Related Moderators

The major methodological limitations in the current state of literature for Muslim mental health include publication bias, homogeneous samples, inconsistent measurement of predictors and outcomes, and neglect of relevant constructs. The current study addresses these limitations.

Publication bias. The most recent systematic review conducted by Samari and colleagues (2018) did not include dissertations or unpublished studies. This is a major

limitation, given that publication bias (i.e., selective publication based on arbitrary factors such as statistical significance) leads to biased estimates of psychological phenomena; therefore, publication status (unpublished vs published studies) is an important consideration (Siddaway, Wood, & Hedges, 2018). Moreover, most studies examining mental health among Muslims include small sample sizes and are underpowered. Underpowered studies lead to Type 1 and Type 2 errors and therefore can bias findings. Furthermore, there has been an emphasis on statistical significance and *p*-values in research without careful consideration of the effect sizes (Siddaway et al, 2018). Meta-analytic methods can account for some of these concerns.

Homogeneous samples. Current research of Muslim experiences of discrimination and mental health tends to be location- and group-specific, and therefore very homogeneous. This limits generalizability to the broader Muslim population in Western countries. There is a need to capture the experiences of diverse Muslims in terms of geographic location and ethnicity (Samari et al., 2018). For example, many studies use data from the Detroit Arab American Study (Baker et al., 2006). Although the study provides a rich dataset, there is a need for studies to use other samples of Muslims living in Western countries. Others may argue that homogeneous samples lead to less statistical 'noise' in the data; while that point has merit, due to the racialization of Muslims, there is a benefit for more research to examine processes on a broader scale and test for relevant moderators.

Inconsistent measurement. Variation in measurement and definitions of discrimination lead to challenges in the field. Researchers use many different measures of discrimination that range from validated measures with 40 or more items to a single item

question (Kathawalla, Gulamhussein, Chan, Riegelman & Syed, in prep). This is further elaborated on in a forthcoming systematic review of measures of Islamophobia and discrimination for Muslims (Kathawalla, Gulamhussein, Chan, Riegelman & Syed, in prep). For example, a study measured Islamophobia using two items that have not been validated in other studies (Abus-Ras et al., 2018). Additionally, studies measure individual or group discrimination, but tend to discuss the results without indicating the discrimination type. Even in terms of potential moderators and mediators, different measures (e.g., ethnic identity, Black identity, importance of identity, collective self-esteem) are used, and it is challenging to identify clear and robust effects. This is evident even in the literature reviewed above where effect sizes change depending on the measure (e.g., direct exposure vs. indirect exposure; Paradies et al., 2015).

Neglect of other relevant constructs. Another limitation is the general neglect of life stressors along with discrimination—a major limitation that the proposed project seeks to address. Harrell's (2000) model of racism-related stress offers alternative possibilities that emphasize both discrimination and life stressors. As described above, meta-analytic results do show a significant association between (1) discrimination and mental-health outcomes, and (2) potential moderators and mediators (e.g., Paradies et al., 2015; Yip et al., 2019); but individual studies that include both discrimination and life stressors demonstrate varied results (e.g., Mölsä et al., 2017). Research on the etiology of mental-health problems for the general population tend to include life stressors and should be investigated in minority populations as well. Investigating the influence of life stressors may help in understanding differential negative effects of discrimination among mental-health outcomes.

Present Study

Broadly, the aim of the current meta-analysis is to examine the associations between (1) discrimination and mental health, and (2) life stressors and mental health for Muslims living in Western countries (i.e., United States, Canada, Europe—including France, UK, Germany, Austria, Belgium, Netherlands, Denmark, Norway, Sweden, Bulgaria, Greece—New Zealand, or Australia). We further explored the difference between these two stressors—discrimination and life stressors. Additionally, we examined various moderators of the two associations, including study-related (e.g., publication status, level of discrimination, outcome type), demographic-related (e.g., age, gender, education level, country of origin, region/continent), and identity-related (e.g., ethnic identity, religious identity, identity integration) variables.

Research Questions

All hypotheses described below do not include control variables.

RQ1: What is the bivariate effect-size estimate between discrimination (combined individual, group, and individual-group discrimination measures) and negative mental health for Muslims living in Western countries?

Hypothesis: We expected a positive relation between increased discrimination and negative mental-health outcomes.

RQ2: What study-related, demographic-related, and identity-related variables moderate the relation between discrimination and negative mental-health outcomes?

Study-Related Variables

2a) Is publication status a significant predictor of the association between discrimination and negative mental-health outcomes? What are the effect-size estimates for the separate publication status predictors?

Hypothesis: We expected publication status to be a significant predictor where effect-size estimates for published studies would be stronger than unpublished studies.

2b) Are characteristics of discrimination measures (i.e., measure name, number of items) significant predictors of the association between discrimination and negative mental-health outcomes?

Hypothesis: We expected discrimination-measure characteristics to be a significant predictor. There were not enough consistent findings in the literature for us to propose a specific directional hypothesis.

2c) Is outcome type (i.e., positive vs. negative mental health) a significant predictor of the association between discrimination and mental-health outcomes? What are the effect-size estimates for the two outcome types?

Hypothesis: We expected outcome type to be a significant predictor, with the effect-size estimate for discrimination and negative mental health being stronger than discrimination and positive mental-health outcomes.

2d) Is level of discrimination (i.e., individual, group, or individual-group discrimination) a significant predictor of the association between discrimination and mental-health outcomes? What are the effect-size estimates for the separate level of discrimination predictors?

Hypothesis: We expected level of discrimination to be a significant predictor, with the effect-size estimate for individual discrimination and negative mental-health outcomes being stronger than group and individual-group discrimination.

Demographic-Related Variables

2e) Are age, gender, refugee/immigration status, country of origin, region/continent, and Muslim sample type (Muslim sample vs. Muslim-majority country sample) significant predictors of the association between discrimination and negative mental-health outcomes?

Hypothesis: We expected gender to be a significant predictor, with the association between discrimination and negative mental-health outcomes being stronger for men than it would be for women. Based on past literature, there were not enough consistent results for us to hypothesize the effects of other demographic predictors.

Identity-Related Variables

2f) Are identity-related variables (e.g., ethnic identity, religious identity, national identity) significant moderators of the association between increased discrimination and negative mental-health outcomes?

Hypothesis: We expected identity-related variables to be significant moderators, with higher levels of identity attenuating the relation between discrimination and negative mental-health outcomes.

RQ3: What is the bivariate effect-size estimate between life stressors and mental-health outcomes for Muslims living in Western countries?

Hypothesis: We expected a positive relation between increased life stressors and negative mental-health outcomes.

RQ4: What study-related, demographic-related, and identity-related variables moderate the relation between life stressors and negative mental-health outcomes?

Study-Related Variables

4a) Is publication status a significant predictor of the association between life stressors and negative mental-health outcomes? What are the effect-size estimates for the separate publication status predictors?

Hypothesis: We expected publication status to be a significant predictor, with the effect-size estimate for published studies being stronger than unpublished studies.

4b) Are the characteristics of life-stressor measures (i.e., measure name, number of items) a significant predictor of the association between life stressors and negative mental-health outcomes?

Hypothesis: We expected life-stressor-measure characteristics to be a significant predictor. There were not enough consistent findings in the literature for us to propose a specific directional hypothesis.

4c) Is outcome type (i.e., positive vs. negative mental health) a significant predictor of the association between life stressors and mental-health outcomes? What are the effect-size estimates for the two outcome types?

Hypothesis: We expected outcome type to be a significant predictor, with the effect-size estimate for life stressors and negative mental-health outcomes being stronger than life stressors and positive mental-health outcomes.

Demographic-Related Variables

4d) Are age, gender, refugee/immigration status, country of origin, region/continent, and Muslim sample type (Muslim sample vs. Muslim-majority country

sample) significant predictors of the association between life stressors and negative mental-health outcomes?

Hypothesis: We expected gender to be a significant predictor, with the association between discrimination and negative mental-health outcomes being stronger for men than for women. Based on past literature, there were not enough consistent results for us to hypothesize effect of other demographic predictors.

Identity-Related Variables

4e) Are identity-related variables (e.g., ethnic identity, religious identity, national identity) significant moderators of the association between increased life stressors and negative mental-health outcomes?

Hypothesis: We expected identity-related variables to be significant moderators, with higher levels of identity weakening the relation between life stressors and negative mental-health outcomes.

RQ5. Is stressor type (discrimination vs. life stressors) a significant predictor of the association between stressors and mental-health outcomes?

Hypothesis: We expected stressor type not to be a significant predictor, with similar effect-size estimates across the different kinds of stressors. (*Note*: We combined individual, group, and individual-group combined discrimination measures.)

RQ6. Do discrimination and life stressors uniquely predict negative mental-health outcomes when in the same model?

Hypothesis: We expected discrimination to uniquely predict negative mental health while controlling for life stressors. We expected life stressors to uniquely predict negative mental-health outcomes while controlling for life discrimination.

Method

This study was accepted as a Stage 1 Registered Report on November 14, 2019, and registered on the Open Science Registered Report page (https://osf.io/4wsg3); therefore, all decisions were made a-priori unless otherwise noted (Stage 1 RR: https://doi.org/10.31234/osf.io/dj5en).

Literature Search

We searched electronic databases to locate both published and unpublished studies. These included PsycINFO, PubMed, Academic Search Premier, ATLA Religion Database with ATLASerials, Index Islamicus, JSTOR, Sociological Abstracts, Ovid Medline, SHARE, and ProQuest Dissertations and Theses Global. The search was conducted between December 10, 2019 – January 29, 2020, by the first author after consultation with a university Librarian. We searched using the title, abstract, keywords, and subject headings. Search terms included a combination of *discrimination terms* (e.g., discrimination, Islamophobia, anti-Muslim), combined with OR, *life stressor terms* (e.g., life stress, childhood adversity, trauma), combined with AND*, mental health terms* (e.g., mental health, psychological distress, self-esteem), combined with AND *identifier of Muslim or Muslim-like identity terms* (e.g., Muslim, Islam, Arab, Pakistani). See Appendix A for exact search terms and supplemental materials (https://osf.io/nvhxt/) for reproducible search information for each database.

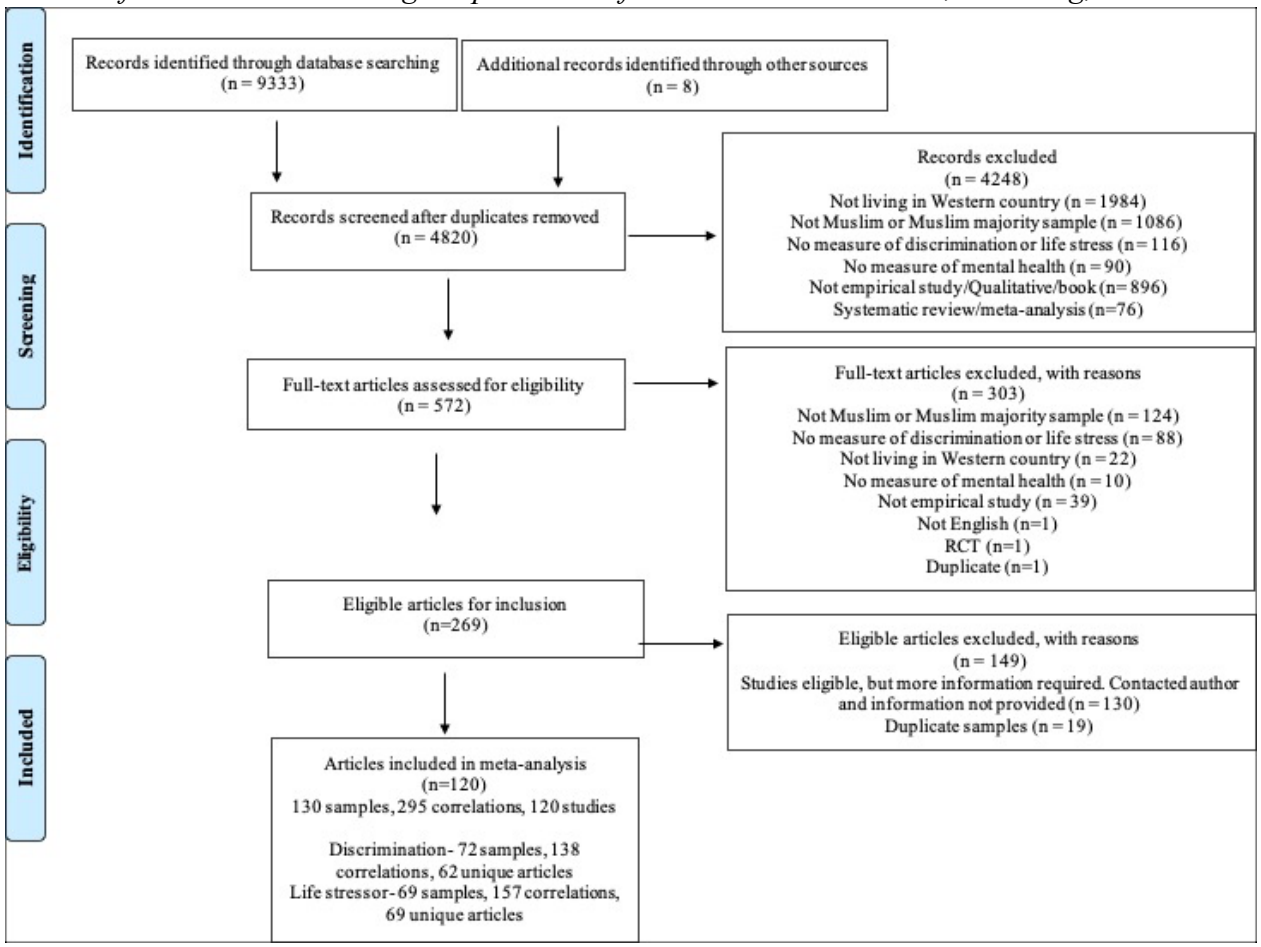
To search for additional unpublished studies, researchers in the in the field of Muslim mental health were contacted using the procedures outlined by Siddaway et al. (2018). First, we identified 16 authors across the United States, Canada, Europe, and Australia/New Zealand with one or more publications on the topic; we emailed each of

the authors to see if they had unpublished studies or forthcoming data. Authors were given one month to respond and were sent one follow-up email after two weeks. Lastly, the reference lists of related systematic reviews were examined to conduct a forward search.

The database searches were combined in EndNote Desktop (citation management software) and resulted in 9,333 records, with an additional eight (8) records identified through other sources (see Figure 2 for PRISMA flowchart; EndNote X9 Desktop, Version 19.3.2.15235). After duplicates were removed, the search resulted in 4,820 records for screening. An online systematic review software, Rayyan QCRI, was used to assist in study selection and track the reliability of inclusion for articles identified through the search procedure outlined (Rayyan QCRI; Ouzzani, Hammady, Fedoroqicz, & Elmagarmid, 2016). Two researchers independently evaluated articles through a two-part process. First, using an iterative screening process, two researchers screened 10 articles and discussed discrepancies; then, they screened another 10 articles and discussed discrepancies before continuing to screen the whole set of articles. The first author screened the titles and abstracts of all 4,820 records and retained those that appeared to meet the following inclusion/exclusion outlined below for the next stage of screening. A research assistant double-screened 22% of records (1,063 records) with a percent agreement of 92%. Second, the 572 records were marked for full text review; then, they were reviewed and evaluated for inclusion and exclusion criteria. The first author reviewed all potential articles, and an undergraduate research assistant double-evaluated 27% of the articles (155 articles) resulting in 90% agreement. All discrepancies were discussed and resolved by consensus. Reasons for study exclusion (e.g., qualitative study,

not Western country) were tracked (see Figure 2). The screening process resulted in 130 unique samples (i.e., datasets), with 295 correlations, from 120 articles. The PRISMA Statement and Checklist were used to transparently document the research procedure (Liberati et al., 2009; see Figure 2).

Figure 2.
PRISMA flowchart summarizing the processes of article literature search, screening, and inclusion.



Inclusion and Exclusion Criteria

Article type. Unpublished, published, and dissertations were included. If the exact same sample was used in a dissertation and a published article, only the published article was included.

Variables. Quantitative and mixed-method studies that had a measure of discrimination, Islamophobia, prejudice, racism, life stressors, life adversity, or ACEs and a measure of mental health (e.g., depression, anxiety, PTSD, paranoia, internalizing symptoms, psychological distress, well-being, self-esteem) were included. For this study, life stressors were defined by TLEQ (Kubany et al., 2000) and the BFRSS ACEs questionnaire (Centers for Disease Control and Prevention, retrieved 2019) and not as other types of stress (e.g., racism-related stress, financial strain, relationship conflict). When it was unclear whether a measure fit the inclusion criteria based on the information provided in the article, we then looked at the original measure article for clarification.

Any study from which we were able to extract the effect size (i.e., zero-order correlation coefficient) was included. If the statistic was not included in the study, we calculated the correlation coefficient using other statistics provided in the article (detailed process outlined in the coding section) or contacted the study author for more information. One hundred thirty studies were eligible, but did not contain the necessary statistics; therefore, those authors were contacted for additional information and provided instructions on how to conduct analyses in R. Approximately two email attempts were made to author(s) in a four-week period. Ten studies were included after the author provided additional information. Studies were excluded when zero-order correlation coefficients could not be calculated or were not provided by authors.

Participants and geographic location. Relevant studies included a sample of Muslim or Muslim-majority-country (i.e., a country that is 90% or above Muslim (see Appendix B); e.g., Arab, Middle Eastern, Iranian, Afghani, Pakistani) participants who lived in the United States, Canada, Europe (including: France, UK, Germany, Austria, Belgium, Netherlands, Denmark, Norway, Sweden, Bulgaria, Greece), New Zealand, or Australia. These European countries were included, as they each have at least a five percent (5%) Muslim population (Pew Research Center, 2017-2). Relevant studies included samples collected within the Western countries listed. With respect to the Muslim-majority-country sample, we also included Indonesian individuals who live in the Western countries delineated, given that—although not a Muslim-majority (by 90% or above)—Indonesia has the largest population of Muslims as of 2015 (Diamant, 2019).

Studies with samples that were not exclusively Muslim or Muslim-majority, or in which the results were not stratified by religion, were still included when the sample was 90% Muslim (90% Muslim or Muslim-majority country of origin; e.g., 90% Pakistani). Authors were contacted for a Muslim-specific correlation when the study sample was at least 70% Muslim/Muslim-majority (n = 13).

We included Muslim-majority groups, given that, even though their religious identity may not have been measured, participants likely experience negative group discrimination due to the racialization process described earlier. Studies were included regardless of participants' age or gender.

Language. We included articles only published in English.

Research design. Cross-sectional, longitudinal, mixed-method, and experimental studies were included. We included intervention studies when they included baseline

measures of required variables. Otherwise, we excluded interventions and randomized control trials, as the intervention can affect the magnitude of the correlation. We also excluded systematic reviews and meta-analyses, qualitative studies, commentaries, editorials, book chapters, and corrections. However, we cross-referenced all systematic reviews and meta-analyses for relevant studies.

Time period. There were no exclusion criteria based on the date of publication.

Coding

The first author and research assistant coded at least 10 articles together to refine the coding spreadsheet and manual. Data-extraction notes for each article were documented following Hartgerink's (2015) recommendations. Extracted data were saved in an Excel spreadsheet (see supplemental folder titled "Coding & Data" on OSF for all data extraction materials; https://osf.io/enb97/).

Reliability. Each article was coded independently by the first author and an undergraduate research assistant. The first author and research assistants discussed discrepancies and resolved them by consensus. The percent agreement across all items, a measure of interrater reliability, was 99%, which met our predetermined level of 90% (Syed & Nelson, 2015; Siddaway et al., 2018)[3].

Zero-order correlation coefficients. We extracted the study effect sizes (Pearson's *r* coefficient) between discrimination and mental health or life stressors and mental health. The correlation coefficients were extracted in one of four different ways: (1) the Pearson's correlation coefficient was included in the article; (2) the standardized

[3] Cohen's kappa was not used as kappa is difficult with a moving denominator, so percent agreement makes more sense (Syed & Nelson, 2015).

beta from a bivariate regression model was included in the article; (3) a phi correlation was calculated using a contingency table of two dichotomous outcomes; (4) when the bivariate correlation was not reported—Chi-squared statistics with one degree of freedom, unadjusted odds ratio, and sample means and standard deviation were used to calculate the Pearson's *r* coefficient using *compute.es* package in R (R Core Team, 2020*;* Del Re, 2013; Johnson & Eagly, 2014). We did not calculate correlation coefficients from regression coefficients with covariates. Authors were contacted for additional statistic information, if necessary, and were given one month to respond. All effect sizes retrieved from studies were coded so that increased discrimination is related to worse mental health/negative mental health. The effect sizes of increased discrimination and positive mental health were reverse-coded in estimating the combined summary effect size.

When a study included correlations for multiple outcomes, all effect sizes were included as a unique entry. We included only one study per dataset; when two studies used the exact same predictor and outcome variables, we included the most recent study with the largest sample size. When two studies use the same dataset, but one study focuses on adults and another on children, both samples were included; similarly, when studies included different variables but used the same dataset, all studies were included.

We used meta-analytic procedures to maintain independence of effect sizes that are outlined below. Notably, although most meta-analyses average effect sizes within a study or across studies with the same sample size, these methods have been found to be unreliable (Hedges, Tipton, & Johnson, 2010; Moeyaert et al., 2017); therefore, we used alternative methods as described below.

Moderators. Moderators that fall under study-related, demographic-related, and identity-related variables were coded.

Study-related. Consistent with other meta-analyses, we extracted conventional publication characteristics (e.g., measures, study design; Siddaway et al., 2018; see OSF project page for data extraction spreadsheet, https://osf.io/enb97/). Additionally, we coded for discrimination-level (individual, group/systematic, and individual-group combined items), life-stressor type (e.g., trauma, general life, abuse, torture) and extracted the stressor measure used. We extracted measure information for both predictor and outcome measures: number of items, Cronbach's alpha, response type, time period. Outcome was coded for type (e.g., depression, PTSD, self-esteem) and for positive or negative mental health measures. We coded for publication status of the article (i.e., published or unpublished).

Demographi-related. Various sociocultural characteristics—including mean age, gender, mean years of education, country of origin, region/continent, refugee/immigration status, and Muslim sample type (Muslim vs. Muslim-majority sample)—were coded to examine variations of the associations of sociocultural characteristics in the literature. Average age was computed when the mean age was reported for different genders or ethnic/racial groups. When the range was provided instead of the mean, we used the midpoint of the reported range.

Identity-related. Twenty-eight unique samples in the full combined discrimination and life-stressor dataset included identity-type variables that fit under ethnic identity (k^4 = 18), national identity (k = 6), religious identity (k = 16), identity integration (k = 3), or

[4] k refers to unique samples.

visible identity (k = 2). The mean, standard deviation, and range were extracted for each identity measure.

Quality ratings. The quality of the articles was assessed using a modified version of National Institutes of Health's (NIH) Quality Assessment Tool for Observational Cohort and Cross-Sectional Studies (https://www.nhlbi.nih.gov/health-topics/study-quality-assessment-tools; the author modified version can be found on the OSF project page). The quality ratings included three areas of evaluation: (1) sample size (e.g., clearly specified study population, power analysis), (2) measurement (e.g., reliability of measures, exposure measured prior to outcome), and (3) robust disclosures (e.g., transparent analytical choices, disclosure of non-significant findings).

Studies rated as *good* included many of the following factors across the three areas of evaluation: moderate to large sample sizes or justification of the sample size with a power analysis, samples that were clearly defined, randomly recruited participants to increase generalizability, validated measures, pre-registration, transparent reporting of results, and alternative analysis models. Studies rated as *fair* included some of the factors listed under good or exceled in only one of the three areas of evaluation. Studies rated as *poor* included studies with small samples sizes, non-validated measures, selective reporting of significant results, evidence of *p*-hacking, or analyses without justification. The quality of the studies (good, fair or poor) are presented but are not used to exclude studies per recommendations from Siddaway et al. (2018). The quality of the studies was assessed by two raters with a percent agreement of 82%. All discrepancies were discussed by the research team and resolved by consensus.

Data Analysis Plan

To answer RQ1-RQ4, analyses were conducted separately for each stressor type (discrimination and life stressors), and to answer RQ5–RQ6, analyses were conducted with the combined data. To account for within-sample dependence, we conducted the main effect-size estimation analyses in two ways: (1) random-effects model (aggregating effect sizes across sample), and (2) robust variance estimation (RVE).

Random-Effects Model

We used a random-effects model with the restricted maximum likelihood (REML) estimator to calculate the weighted mean correlation using the *metafor* package in R (*R Core Team, 2020;* Viechtbauer, 2010). Random-effects models assume the average effect size in the population varies randomly from study to study and, thus, the study effect sizes will be heterogenous (Field & Gillett, 2010). Based upon the small number of studies from preliminary literature searches, REML estimator is generally recommended (Viechtbauer, 2005; Veroniki et al., 2014). Additionally, random-effects models allow for generalizability of the findings to the population (Field & Gillett, 2010).

Because a random-effect model assumes independent samples, we created an aggregate effect size per sample using the *agg* function of *MAc* package in R for samples with multiple effect sizes (R Core Team, 2020*;* Hunter & Schmidt, 2004, see pp. 435-438; Quintana, 2015; Del Re & Hoyt, 2010). The extracted aggregated sample effect sizes were then converted to Fisher's *Z* scores for calculations and later converted back to Pearson's *r* coefficient for interpretation. Forest plots of study effect sizes and the overall random-effect model effect-size estimate are presented (see Figures 3 & 6).

Robust Variance Estimation

We used robust variance estimation (RVE) using the *robumeta* package in *R* to calculate an effect-size estimate that accounts for within-study and within-sample dependence (Fisher & Tipton, 2015). We used the default ρ = .8 setting and conducted a sensitivity analysis for within-study effects (ρ) from the interval (0,1) (Fisher & Tipton, 2015). This method has been found to be robust with small samples (Tipton, 2015). The full extracted data, including sample duplicates, were converted to Fisher's *Z* scores for calculations and later converted back to Pearson's *r* coefficient for interpretation. Correlation coefficients were benchmarked according to the empirically-derived cut-offs described by Hemphill (2003) as $r < .20$ (small effect), $.20 <= r < .30$ (medium effect), and $r >= .30$ (large effect). The p-values and confidence intervals for the effect sizes are presented (alpha = .05 level). Forest plots of study effect sizes, RVE weights, and the overall RVE effect-size estimate are presented (Figures S1 (https://osf.io/yhnur/) & S6 (https://osf.io/xmk42/) in supplemental materials on OSF).

Heterogeneity

To examine heterogeneity between-studies, Cochran's *Q*-statistic, I^2, tau-squared, and prediction interval statistics were calculated (Higgins, Thompson, Deeks, & Altman, 2003; Quintana, 2015; Borenstein, 2019). Cochran's *Q*-statistic is the standard significance test for between-study heterogeneity that demonstrates whether the variability is greater than what is expected from sampling error (Quintana, 2015; Siddway et a., 2018). We estimated I^2 to examine the proportion of variance in the observed effect sizes that would be attributable to the actual difference between studies, specifically where I^2 values represent low (25), medium (50), or high (75) heterogeneity (Quintana, 2015; Borenstein et al., 2017). Tau-squared was estimated to quantify

between-study variance in random-effects models, where zero indicates no heterogeneity (Quintana, 2015). Prediction intervals were calculated as an index of dispersion of how widely the true effect size varied (Borenstein, 2019).

In order to see if any specific studies are overly-influencing heterogeneity, we used two strategies to examine for outliers. The distribution of effect sizes was examined for extreme values (i.e., outliers). Also, we used the *metafor* package in R to identify outliers and potential influential cases (Quintana, 2015; Viechtbauer, 2010). As a sensitivity test, we excluded effect sizes that were larger than 2.5 absolute deviations below or above the median or were identified as an influential case (Leys, Ley, Klein, Bernard, & Licata, 2013). We then calculated meta-analytic effect-size estimates with and without outliers to compare the results (see Figures 3 & 6).

Publication Bias

In order to assess for publication bias, differences between effect-size estimates of unpublished and published studies, funnel plots, forest plots, and sensitivity analyses with weighted correlations were conducted (Jin, Zhou, & He, 2015). We used meta-regression with random-effect model using REML (mixed-effects model) and RVE to test when publication status was a significant predictor (Tipton, 2015). We calculated and compared separate effect-size estimates for published and unpublished studies using random-effects models for descriptive purposes.

Publication bias was explored through a funnel plot to visualize the effect size plotted against sample size. Publication bias may be present when the graph looks asymmetrical, specifically where more data points are toward larger effects and samples

(Greco, Zangrillo, Biondi-Zoccai, & Landoni, 2013). We also explored publication bias through a funnel plot to visualize the effect size plotted against standard error.

Although the Egger test is widely used to test for publication bias, there is some evidence that suggests it leads to an inflated Type I error rate (Lin & Chu, 2018). Thus, the symmetry of the funnel plot was quantified by the Egger's test, but it was interpreted cautiously. For an additional visual representation of publication bias, forest plots were generated and sorted by publication status and sample size (Viechbauer, 2010).

In order to quantify the possible impact of publication bias, we conducted a sensitivity analysis and used the "moderate one-tailed selection" weight function outlined by Vevea and Woods (2005). The method proposed by Vevea and Woods (2005) has proven to be appropriate for meta-analyses with fewer than 100 studies. We used the *weightr* package (Coburn & Vevea, 2019) in *R*. Selection model approaches outperform other traditional methods (e.g., trim-and-fill method; Carter et al., 2019)

Moderation

To answer RQ2 and RQ4, moderation analyses were conducted using meta-regression approaches in a random-effect model using REML (mixed-effects model) and/or RVE;); these analyses were conducted using the *metafor* and *robumeta* packages in R, which allow for continuous and categorical moderators (Borenstein Hedges, Higgins, & Rothstein, 2009; Viechtbaer, 2010; Fisher & Tipton, 2015. Only mixed-effects model results are presented, unless RVE approaches make more theoretical sense (i.e., where correlations within a sample were aggregated and important information was lost; for example, when mental-health outcome was aggregated, information was lost) or when results differed between the two models. All meta-regression results not presented

can be found in the supplemental materials on OSF (https://osf.io/gkt8u/). Moderation analyses were conducted when there were at least 10 effect estimates per one covariate or per group as recommended for sufficient power to detect the true effect (Borenstein & Higgins, 2013; Higgins & Thompson, 2004; Harrer et al., 2019). Each covariate was examined in an individual model.

For RQ2d, we tested whether level of discrimination (individual vs. group, vs. individual-group combined discrimination) was a significant predictor. We calculated separate effect-size estimates by level of discrimination using RVE for descriptive purposes.

For RQ2a-c and RQ4a-c, we tested whether study-related, demographic-related, or identity-related variables were significant moderators for the associations between discrimination and mental health and life stressors and mental health with each predictor in a separate model. We calculated separate effect estimates by specific group variables (i.e., continent/region, Muslim sample type) using random-effects models by REML or RVE for descriptive purposes.

For RQ5, we tested whether stress type (discrimination vs. life stressors) was a significant moderator by combining the discrimination and life stress data and conducting moderation analyses using mixed-method and RVE approaches. Additionally, we used equivalence testing to more accurately test the presence or absence of an effect using two TOST methods[5]—transformed correlation coefficients (TOST-p) and the untransformed TOST (KTOST-p) (Counsell & Cribbie, 2015; Lakens et al., 2018; Lakens, 2017). We

[5] In our Stage 1 Registered Report we said we would use the TOSTER function by Lakens (2017), but the function is used to compare correlations to equivalence bounds instead of comparing correlations to each other.

used the two one-sided tests (TOST) procedure to test for the presence of the smallest effect size of interest (SESOI). We chose the SEOSI of $r = .15$, where the difference between effect sizes estimates needs to be greater than .15 (-.075, .075) to be relevant (Lakens et al., 2018; Counsell & Cribbie, 2015). We used Hemphill (2003) as a guide for a small effect to determine our SESOI.

For RQ6, we used systematic review methods to provide a qualitative review of the studies that include both discrimination and life stressors as predictors. Notably, 14 samples included both predictors. Since at least 10 samples were identified, we later coded for partial correlations between 1) discrimination and mental health (controlling for life stressors), and 2) life stressors and mental health (controlling for discrimination). However, only five samples included partial correlations[6].

The list of articles, data spreadsheet, coding process description sheet, analysis scripts, and supplemental materials are available on OSF (https://osf.io/enb97/).

Results

The full meta-analytic dataset includes 295 correlations, from 130 unique samples and 120 articles, including 27,725 individuals (full dataset can be found at https://osf.io/enb97/). The discrimination dataset includes 138 correlations, from 72 unique samples and 62 articles, including 16,042 individuals. In terms of quality ratings, 60% of discrimination samples were rated as fair, 22% good, and 18% poor. The life-stressor dataset includes 157 correlations, from 69 unique samples and 69 articles, including 15,099 individuals. In terms of quality ratings, 54% of life stressor samples

[6] Decided a priori, 10 studies were needed for analyses. Therefore, the analyses presented of partial correlations are exploratory.

were rated as fair, 10% good, 32% poor, and 4% combined fair and poor. Fourteen samples from 18 articles[7], including 3,416[8] individuals, overlap between the discrimination and life-stressor datasets.

According to an a priori power analysis, we had adequate sample sizes to continue with our analyses. We used Quintana and Tiebel's (2018) Excel calculator, which indicated that we needed at least 10 studies for a power of .80, assuming $r = .2$, an average number per group = 85, and moderate heterogeneity (Quintana, 2017).

We conducted the main effect-size estimation analyses in two ways: (1) random-effects model (aggregating effect across sample), and (2) robust variance estimation (RVE). Thus, analyses across both methods resulted in four main effect-size estimates for the association between 1) discrimination and negative mental-health outcomes, and 2) life stressors and negative mental-health outcomes (no additional covariates were included). We first present the results for the discrimination dataset, then the life-stressor dataset; and then last, we compare the two predictors. We conducted the same data-analyses plan outlined in the data-analysis section earlier for each stressor dataset. Therefore, in the section below, we aim to reduce redundant information as much as possible between the discrimination and life-stressor datasets results by removing repetitive information when presenting the life-stressor dataset results.

Discrimination

[7] From the 14 samples, only five included statistics required for a meta-analysis; therefore, we conducted a qualitative review.

[8] There were 11 samples in the analytical sample and 14 in qualitative review as three samples only presented one of the two correlations. One article only presented the correlation for life stressors and mental health and did not include the correlation for discrimination and mental health. One other article (with two unique samples) only presented the correlation for discrimination and mental health and did not include the correlation between life stressors and mental health. The authors were contacted for the bivariate correlations, but they did not provide the information needed.

Sample characteristics

Table 1 summarizes the individual sample (i.e., dataset) details included in the discrimination meta-analysis. Table 2 summarizes descriptive statistics of the discrimination dataset (see supplemental materials, Table S7 (https://osf.io/baesz/) for additional individual sample and correlation details, including measure names). The 72 unique samples had a mean sample size of 244 (*SD* = 206.79; range = 30–1215) and represented individuals living in 10 different Western countries (see Table 1). The mean age of participants was 26 years old (*SD* = 9.73; range = 10–49 years old), and 55% were women; the collapsed majority ethnic breakdown was 61% Middle Eastern, 15% Asian/South Asian, 12% North African, 9% Somali, and 4% other combination majority (see Table 1 for individual sample information and Table 2 for a more detailed breakdown of the majority ethnicities represented by each sample). In terms of discrimination-level, the majority of samples used a measure of personal discrimination (62.5%), followed by a measure of combined personal and group discrimination items (30.6%), and last, a measure of group discrimination (6.9%).[9] Samples measured diverse mental-health outcomes, including depression (k = 5), PTSD (k = 1), anxiety (k = 1), and self-esteem (k =11), along with broader categorizations, such as, psychological distress (k = 15), well-being (k = 6), and internalizing disorders (k = 8).[10] Many samples also measured multiple types of mental-health outcomes, such as, anxiety and depression (k =

[9] Percentages do not match with Table 5 as samples included different types of measures that can be analyzed separately at times and other times combined.

[10] Number of samples do not match Table 6 due to different ways of categorizing mental-health outcomes. In order to increase the number of samples per mental-health outcome to conduct RVE analyses (see Table 6), outcomes were approximately categorized using the Hierarchical Taxonomy of Psychopathology dimensions (Kotov et al., 2017).

3), depression and well-being (k = 2), PTSD and depression (k = 2), well-being and psychological distress (k = 3).

Table 1.

Discrimination dataset individual sample details

Sample	*N*	Age	%F	Ctry	Muslim type	%Ethnicity	Sample Type	Pub	Design	Quality	*r*	# corr /sample	Stress Types	MH Types	Identity
Adam & Ward, 2016	167	32	65	NZ	Muslim	32.9 IN/SAS, 17.4 EA, 16.8 MDE, 16.2 W, 9 AFR 4.8 PASF	comm.	P	C	fair	0.32	2	P.DIS	W.B, DIS	Y
Ahmed, 2006	240	16	48	USA	Muslim	48.3 LB, 19.2 IQ, 15.8 YE, 16.7 OTHA	school	T	C	good	0.27	4	C.DIS	EXT, INT	Y
Aichberger et al., 2015	205	37	100	DE	Turkish	100 TR	comm.	P	C	fair	0.34	1	P.DIS	DIS	
Alemi et al., 2017	133	26	64	USA	Afghan	100 AF	comm.	P	C	fair	0.36	1	P.DIS	DEP	Y
Alemi & Stempel, 2018	259	49	51	USA	Afghan	100 AF	comm.	P	C	fair	0.27	1	P.DIS	DIS	Y
Ameline et al., 2019	88	32	53	FR	Muslim	40 TR/MA/DZ, 60 born FR	comm.	P	C	poor	-0.17	1	G.DIS	SE	Y
Brenick et al., 2018	297	12	49	DE	Turkish	100 TR	school	P	C	fair	0.29	2	P.DIS	DEP, W.B	
Cakir & Guneri, 2011	248	34	100	UK	Turkish	100 TR	comm.	P	C	poor	0.2	1	P.DIS	DIS	
East et al., 2018(s1)	198	10	44	USA	Muslim	100 SO	comm.	P	C	good	0.67	1	C.DIS	DEP	
Ellis et al., 2008	129	15	38	USA	Muslim	100 SO	comm.	P	C	fair	0.51	2	P.DIS	PTSD, DEP	
Every & Perry, 2014	49	30	43	AU	Muslim	NR	comm.	P	C	poor	-0.16	2	G.DIS, P.DIS	SE	
Feddes et al., 2015	46	17	22	NL	Muslim	85 MA, 11 TR, 1 SR, 1 PK	comb.	P	I	poor	-0.18	2	G.DIS, P.DIS	SE	
Goforth et al., 2014	128	16	45	USA	Muslim	100 AB	clinic	P	C	fair	0.31	1	C.DIS	DIS	
Hashemi et al., 2019	275	30	48	AU	Muslim	100 MDE	comm.	P	C	fair	0.23	1	P.DIS	W.B	Y
Hassouneh & Kulwicki, 2007	30	38	100	USA	Muslim	33 PAL, 30 IQ, 23 LB, 7 SY, 3 YE, 3 AA	comm.	P	C	poor	0.31	6	P.DIS	ANX, DEP	
Hakim et al., 2018(s1)	228	42	47	USA	Muslim	100 MDE/NRAFR	comm.	P	C	fair	0.05	1	C.DIS	W.B	Y
Hakim et al., 2018(s2)	422	42	49	USA	Muslim	100 AB	comm.	P	C	fair	0.05	1	C.DIS	W.B	Y
Hernandez, 2012	127	12	63	USA	Muslim	62 AB, 16 EA, 13 OTH, 4 AFR, 3 AA, 1 H, 2 W	school	T	C	fair	-0.03	1	P.DIS	SE	Y
Hosseini et al., 2017	182	37	46	AU	Iranian	100 IR	comm.	P	C	fair	0.15	1	P.DIS	DEP	
Husain, 2008	131	15	54	USA	Muslim	100 SO	org.	T	C	fair	0.04	1	P.DIS	SE	Y
Ikizler & Szymanski, 2018	58	35	60	USA	Muslim	100 MDE/AB	comm.	P	C	fair	0.37	1	P.DIS	DIS	
Jasperse et al., 2012	153	28	100	NZ	Muslim	60 A, 24 MDE, 7 AFR, 7 WEST, 3 PASF	comm.	P	C	fair	-0.06	2	P.DIS	W.B, DIS	Y
Jibeen, 2011	308	36	47	CA	Pakistani	100 PK	comm.	P	C	fair	0.48	2	P.DIS	W.B, DIS	
Thijs et al., 2018	707	10	57	NL	Muslim	43 MA, 28.4 TR, 1.4 NL, 23.5 OTH	school	P	C	good	0.25	2	G.DIS, P.DIS	SE	Y
Matheson et al., 2008/Jorden et al., 2009	169	29	64	CA	Muslim	100 SO	comm.	P	C	fair	0.38	2	P.DIS	PTSD, DEP	
Kheirieh, 2019	91	30	78	USA	Muslim	100 MDE	comm.	T	C	poor	-0.06	1	G.DIS	DIS	
Kira et al., 2010/2012(s2)	501	36	45	USA	Muslim	100 IQ	comm.	P	C	fair	0.34	2	C.DIS	DIS, PTSD	
Knauss et al., 2015	46	NR	NR	DE	Turkish	100 TR	school	P	C	poor	0.73	2	C.DIS	DIS, SOM	
Kunst et al., 2013(s1)	167	24	58	DE	Muslim	100 DE-AB	comm.	P	C	good	0.25	2	G.DIS, P.DIS	DIS	
Kunst et al., 2013(s2)	184	24	57	DE	Muslim	100 DE-TR	comm.	P	C	good	0.24	2	G.DIS, P.DIS	DIS	

Table 1 (continued)

Sample	*N*	Age	%F	Ctry	Muslim type	%Ethnicity	Sample Type	Pub	Design	Quality	*r*	# corr /sample	Stress Types	MH Types	Identity
Kunst et al., 2013(s3)	205	24	43	UK	Muslim	100 UK-PK	comm.	P	C	good	0.29	2	G.DIS, P.DIS	DIS	
Kunst et al., 2013(s4)	262	25	47	DE	Muslim	100 DE-TR	comm.	P	C	good	0.16	1	G.DIS	DIS	
Kunst et al., 2013(s5)	277	25	50	FR	Muslim	100 FR-NRAFR	comm.	P	C	good	0.22	1	G.DIS	DIS	
Kunst et al., 2013(s6)	249	25	42	UK	Muslim	100 UK-PK	comm.	P	C	good	0.23	1	G.DIS	DIS	
Lindencrona et al., 2008	115	34	38	SE	Middle Eastern	100 MDE	org.	P	C	poor	0.29	2	P.DIS	DIS, PTSD	
Lindert et al., 2008	232	38	54	NL	Iranian	100 IR	comm.	P	C	fair	0.37	1	P.DIS	W.B	
Lowe et al., 2019	141	21	74	USA	Muslim	NR	school	P	C	fair	0.34	2	P.DIS	ANX, DEP	Y
Mewes et al., 2015	214	33	63	DE	Turkish	100 TR	comb.	P	C	fair	0.28	2	P.DIS	DIS	Y
Musa, 2015	202	25	76	USA	Muslim	38 MDE, 37 SAS, 11 W, 5 AA, 4 A, 4 M	comm.	T	C	fair	0.5	2	P.DIS	ANX, DEP	Y
Musa, 2018	138	26	69	USA	Muslim	43 SAS, 35 MDE, 7 W, 2 AA, 5 M, 3 EA	comm.	T	C	fair	0.39	2	G.DIS, P.DIS	INT	Y
Oppedal, 2011(s1)	90	16	50	NO	Somali	100 SO	comb.	P	C	fair	0.09	1	P.DIS	INT	
Oppedal, 2011(s3)	103	16	50	NO	Turkish	100 TR	comb.	P	C	fair	0.22	1	P.DIS	INT	
Oppedal, 2011(s2)	74	12	48	NO	Somali	100 SO	comb.	P	C	fair	0.4	1	P.DIS	INT	
Oppedal, 2011(s4)	96	12	48	NO	Turkish	100 TR	comb.	P	C	fair	0.34	1	P.DIS	INT	
Rippy & Newman, 2006	152	34	39	USA	Muslim	44 AB, 22 EA, 18 AA, 16 W	comm.	P	C	fair	0.08	1	C.DIS	ANX	
Schaafsma, 2013	243	35	45	NL	Moroccan	57 MA, 43 TR	comm.	P	C	good	0.21	1	P.DIS	W.B	
Schaafsma, 2011	320	30	52	NL	Turkish	52 TR, 48 MA	comm.	P	C	good	0.13	2	P.DIS	W.B	Y
Sedighdeilami, 2003	205	36	50	CA	Iranian	100 IR	comm.	T	C	fair	0.25	3	C.DIS	EXT, W.B, INT	
Shah, 2018	123	NR	71	USA	Muslim	NR	comb.	T	C	fair	0.17	1	P.DIS	SE	Y
Shawahin, 2016	272	33	61	USA	Muslim	10.3 W, 23.2 AA, .4 H, .4 A, 26.8 SAS, 26.8 MDE, .7 NAA, 11.4 OTH	comm.	T	C	good	0.24	8	G.DIS, P.DIS	INT, DEP, INTP, SOM	
Stevens et al., 2005a/2005b+Stevens & Thijs, 2018	361	14	52	NL	Muslim	100 MA	comm.	P	C	fair	0.13	15	G.DIS, P.DIS	EXT, INT, SE	Y
Stuart & Ward, 2018	155	20	70	NZ	Muslim	52 A 27 MDE 21 AFR	comm.	P	C	fair	0.24	2	P.DIS	DEP, W.B	Y
Tolaymat, 2014	217	32	100	USA	Muslim	36 A, 31 AB/MDE ,12 W, 9 AA, 4.6 M, 6 OTH, 1 H	comm.	T	C	good	0.25	1	C.DIS	INT	
van de Beek et al., 2017	267	24	87	NL	Moroccan	100 MA	comm.	P	C	fair	0.25	2	P.DIS	PSYC, DEP	
Vedder et al., 2016	303	15	55	NL	Muslim	34 MA 46 MA	school	P	C	fair	0.27	1	P.DIS	INTP	
Verkuyten & Nekuee, 1999	67	32	40	NL	Iranian	100 IR	org.	P	C	poor	0.28	4	P.DIS	INT, W.B, SE	Y
Vedder et al., 2017	369	15	52	NL	Muslim	54.5 TR 29 MA	school	P	C	fair	0.23	1	P.DIS	INTP	
Verkuyten, 1998(s1)	92	14	55	NL	Turkish	100 TR	school	P	C	fair	0.24	2	G.DIS, P.DIS	SE	
Verkuyten, 1998(s2)	78	14	55	NL	Moroccan	100 MA	school	P	C	fair	0.21	2	G.DIS, P.DIS	SE	
Verkuyten & Thijs, 2004(s1)	161	15	49	NL	Turkish	100 TR	school	P	C	fair	0.17	1	P.DIS	SE	
Verkuyten & Thijs, 2004(s2)	112	12	53	NL	Turkish	100 TR	school	P	C	fair	0.08	1	C.DIS	SE	

Table 1 (continued)

Sample	*N*	Age	%F	Ctry	Muslim type	%Ethnicity	Sample Type	Pub	Design	Quality	*r*	# corr /sample	Stress Types	MH Types
Wadsworth et al., 2007	206	29	30	UK	Bangladeshi	100 BD	comm.	P	C	fair	0.14	4	P.DIS	DEP, INTP, SOM, ANX
Wilson, 2017	43	23	45	USA	Middle Eastern	100 MDE	comm.	T	C	poor	0.17	2	P.DIS	SE, DEP
Abuelezam & El-Sayed, 2018	354	40	47	USA	Arab American	100 AB	comm.	P	C	poor	0	2	P.DIS	DEP, DIS
Gorst-Unsworth & Goldenberg, 1998	84	39	0	UK	Muslim	100 IQ (45 KUR)	clinic	P	C	poor	0.32	1	P.DIS	DEP
Ikram et al., 2015	834	40	53	NL	Turkish	100 TR	comm.	P	C	good	0.16	2	P.DIS	DEP
Sheridan, 2006	222	22	53	UK	Muslim	61.7 PK, 12.9 IN, 8.1 BD, 7.7 W, 4.8 OTHAS, 3.8 AB, .5 M .5 AA	comm.	P	C	poor	0.13	1	P.DIS	DIS
Lecerof et al., 2016	587	35	53	SE	Iraqi	100 IQ	comm.	P	C	fair	0.27	1	P.DIS	DIS
Tinghog et al., 2010(s1)	259	42	47	SE	Iraqi	100 IQ	comm.	P	C	good	0.11	1	P.DIS	INT
Tinghog et al., 2010(s2)	250	42	44	SE	Iranian	100 IR	comm.	P	C	good	0.05	1	P.DIS	INT
Tinghog et al., 2017	1215	34	37	SE	Syrian	100 SY	comm.	P	C	fair	0.2	3	P.DIS	INT, PTSD, W.B
von Haumeder et al., 2019	127	32	34	DE	Syrian	100 SY	org.	P	C	fair	0.43	1	P.DIS	PTSD

NR = Not reported; Country: NZ = New Zealand; SE = Sweden; FR = France; DE = Germany; UK = United Kingdom; CA = Canada; NL = Netherlands; AU = Australia; DK = Denmark; NO = Norway; CH = Switzerland; BE = Belgium; Ethnicity: A = Asian or Asian American; AA = African American or Black; AAA= African Arab; AB = Arab or Arab American; AF = Afghanistan; AFR = Africa; AL = Albania; BD = Bangladesh; DE = Germany; DZ = Algeria; EA = East Asian; EG = Egypt; FR = France; H = Hispanic or Latinx; IN = India; IQ = Iraq; IR = Iran; KUR = Kurdish; LB = Lebanon; M = Multiethnic; MA = Morocco; MDE = Middle East; NAA = Native American; NL = Netherlands; NRAFR = North Africa; OTH = Other; OTHA= Other Arabic; OTHAS = Other Asian; OTHNA= Other non-Arabic; PAL = Palestine; PASF = Pasifika; PK = Pakistan; SAS = South Asian; SO = Somalia; SR = Suriname; SY = Syria; TR = Turkey; UK = United Kingdom; W = White; WEST = Western; YE = Yemen; Study-Deisgn: C = crosssectional; L = longitudinal; I = intervention; Publication Status: P = published; T = thesis; Stressor: P.DIS = personal discrimination; G.DIS = group discrimination; C.DIS = combination discrimination; Mental health: INT = internalizing; PTSD = PTSD; W.B = wellbeing; DIS = psychological distress; EXT = externalizing; INTP = interpersonal; DEP = depression; ANX = anxiety; SE = self-esteem; SOM = somatization; PSYC = psychotic; Sample Type: comm. = community; comb. = combination; org. = organization

Table 2.

Discrimination dataset sample characteristics

Sample characteristics	Details
k	72
kc	138
N (range)	244 (30-1215)
Age (M(sd))	26.13 (9.73)
% Female (sd)	54.62 (17.77)
Data from published sources (%)	85
Country	
- Australia	k=3
- Canada	k=3
- France	k=2
- Germany	k=8
- Netherlands	k=15
- New Zealand	k=3
- Norway	k=4
- Sweden	k=5
- United Kingdom	k=6
- USA	k=23
Majority Ethnicity	
- Asian/South Asian	k=10 (14.5%)
- Middle Eastern	k=42 (60.9%)
- Middle Eastern/North African	k=1 (1.4%)
- Middle Eastern/North African/South Asian	k=2 (2.9%)
- North African	k=8 (11.6%)
- Somali	k=6 (8.7%)
Majority Ethnicity (detailed)	
- Afghani	k=2
- Arab	k=5
- Asian/South Asian	k=5
- Bangladeshi	k=1
- British-Pakistani	k=2
- French-Maghrebi	k=1
- German-Arab	k=1
- German-Turkish	k=2
- Iranian	k=5
- Iraqi	k=4
- Lebanese	k=1
- Middle Eastern	k=5
- Middle Eastern/Arab American	k=1
- Middle Eastern/North African	k=1
- Middle Eastern/South Asian	k=1
- Moroccan	k=7
- Pakistani	k=2
- Palestinian	k=1
- Somali	k=6
- Syrian	k=2
- Turkish	k=13
- Turkish/Moroccan/Algerian	k=1

k = number of samples; kc = number of outcomes or correlations.

RQ1: Bivariate effect-size estimate between discrimination and negative mental health

Weighted mean correlations. Figure 3 shows the weighted mean correlations using both a random-effects model with the restricted maximum likelihood (REML) estimator and RVE estimator, along with the univariate effect-size estimates and their 95% confidence intervals for the 72 samples in the discrimination dataset. Using the random-effects model approach, we found a statistically significant summary effect, r = .234 (95% CI [.20, .27]), z = 11.95 p < .001. Using the RVE approach, we also found a statistically significant summary effect, r = .215 (95% CI [.18, .25]), t (68.8) = 11.96 p <.001. The RVE estimate is slightly attenuated compared to the random-effects estimate. Supplemental Figure 1 (https://osf.io/yhnur/) on OSF includes the RVE weight estimates per correlation in each study. Both estimates have confidence intervals that showed the true association is unlikely to be zero, and the narrow confidence intervals show precision in this summary effect. Table 3 summarizes these statistics with three decimal places. Consistent with our hypothesis, increased experiences of discrimination are positively associated with worse mental-health outcomes for Muslims living in Western countries. The summary correlation is positive and falls between the small to moderate range using Hemphill's (2003) guidelines (r < .20 (small effect), .20 <= r < .30 (medium effect), and r >= .30 (large effect)).

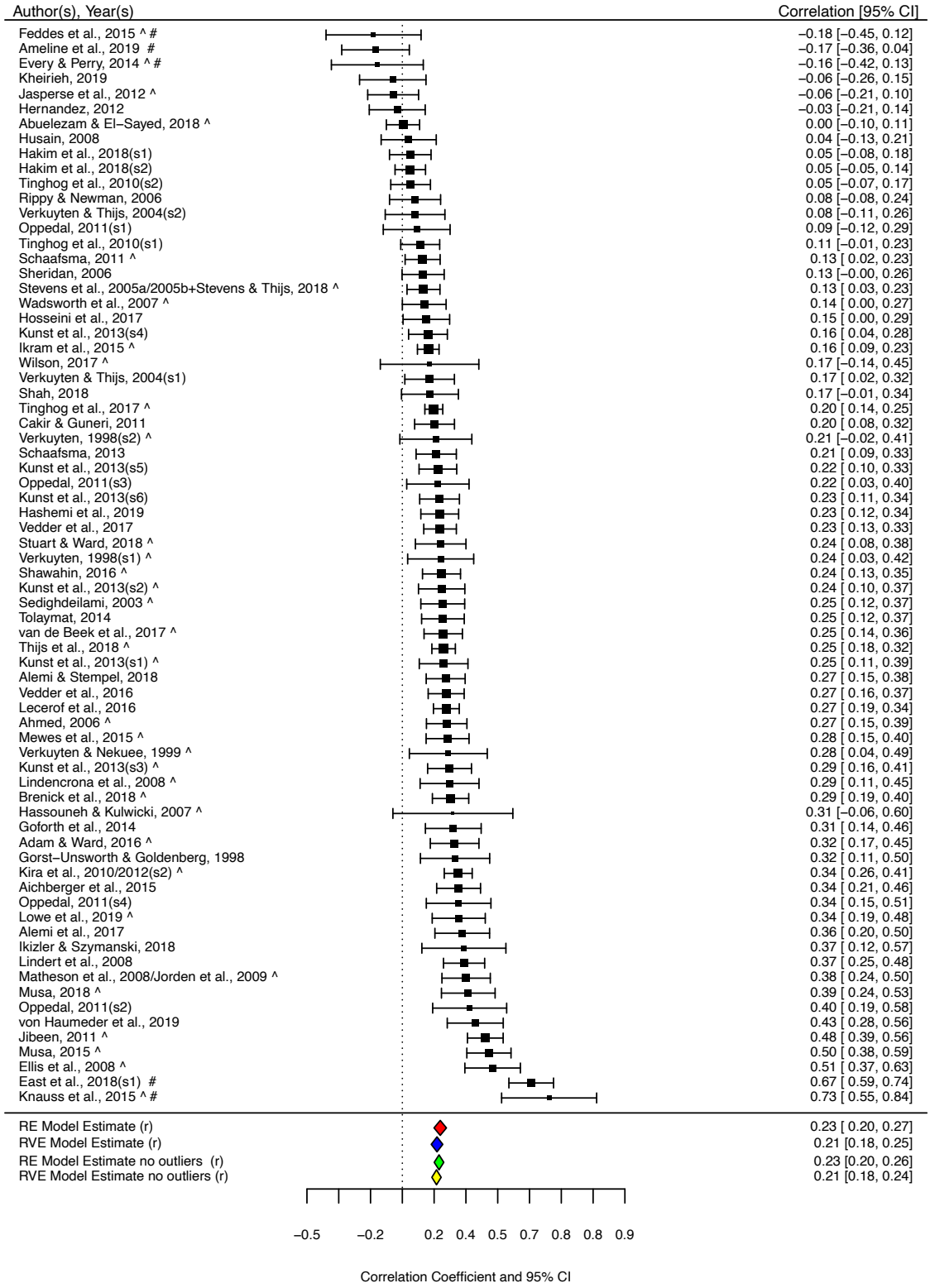
Heterogeneity. The data showed a statistically significant and large degree of between-study variability, $Q(71) = 338.30$, $p < .001$, $\tau^2 = 0.022$, $I^2 = 82.77\%$ (see Table 3). Heterogeneity examines how much the variation between studies is true variation in the population. The statistically significant Q statistic indicated that the included studies do not share a common true effect size. The I^2 statistic indicated substantial heterogeneity

in the true effect, meaning that a large proportion of the observed variance between discrimination and mental-health outcomes was attributed to variances in the true effect rather than sampling error. The index of dispersion for the correlation of interest was -0.055 to 0.487, which shows that, within the population, bivariate correlations would fall between this range 95% of the time. Therefore, in some cases there is no effect; in other cases, there are worse outcomes, but the data suggests that experiences of discrimination do not lead to any positive mental-health outcomes. Given the large index of dispersion, it is important to examine potential moderators of the association between discrimination and mental-health outcomes.

Influential cases and outliers. Figure 3 identifies the five influential samples identified through examining DFFITS values, Cook's distances, hat values, DFBETAS values, and effect sizes 2.5 absolute deviations below/above the median. As a sensitivity analysis, we removed the five influential samples and recalculated the effect-size estimate ($k = 67$, $k_c = 130$). The weighted random-effects and RVE estimates were unchanged, still statistically significant with high heterogeneity, $r = .231$ (95% CI [.20, .26]) and $r = .211$ (95% CI [.18, .24]), respectively (see Figure 3 for effect-size estimates without outliers and see supplemental materials on OSF, Figures S2 (https://osf.io/kfthj/) & S3 (https://osf.io/dwc8a/), for all outlier statistics). Additionally, we conducted a *leaveout* analysis in R where each sample was removed, and the random-effect model estimate was recalculated. Each influential case slightly attenuated the omnibus effect size but did not alter the statistical significance of the effect, and heterogeneity remained high (see supplemental materials on OSF, Table S1 (https://osf.io/gkt8u/)). Thus, findings showed

that influential cases and outliers did not alter the omnibus weighted mean correlation or account for the heterogeneity between studies.

Figure 3.
Forest plot summarizing mean sample-level correlations (Pearson's r) between discrimination and mental-health outcomes, random-effect-size estimate, robust variance estimation effect-size estimate, and estimates without outliers.



 # = influential case/outlier; ^ = includes aggregated correlations

Sensitivity analysis for between- and within-study variance. Table 3 also summarizes the results from the RVE method using different levels of ρ from 0.0 to 1.0 where ρ = 1 is the most conservative approach. The sensitivity analysis did not affect the summary correlation, confidence intervals, or τ^2. Overall, findings showed that the omnibus weighted mean correlation was not sensitive to the dependence of within-study effects.

Table 3.

Discrimination meta-analysis results using random-effects and robust variance estimation models.

Measure	Random-Effects Model	RVE p = .8	RVE p = 0	RVE p = .2	RVE p = .4	RVE p = .6	RVE p = 1
r	0.234	0.215	0.215	0.215	0.215	0.215	0.215
95% CI	[0.197, 0.271]	[0.18, 0.249]	[0.18, 0.249]	[0.18, 0.249]	[0.18, 0.249]	[0.18, 0.249]	[0.18, 0.249]
z-value	11.945						
t-value		11.96	11.961	11.961	11.961	11.96	11.96
tau-squared	0.022	0.018	0.018	0.018	0.018	0.018	0.018
Q-stat	338.295						
I-squared	82.773	79.51	79.439	79.457	79.474	79.492	79.528
Index of Dispersion	[-0.055, 0.487]						

Robust variance estimation (RVE) model at p = .8 is the weight used in all RVE models.
95% CI: 95% confidence intervals around the weighted mean correlation.

Publication bias. Table 4 summarizes the meta-regression results for publication status; 85% of samples were from published (k = 61) versus unpublished studies (k = 11). We conducted meta-regressions using both a random-effects model using REML (mixed-effects model) and RVE; using these two methods we found—inconsistent with our hypothesis—that publication status was not a significant moderator, z = -.435, p = .664 (Table 4, mixed-effects model); t (13.34) = -.696, p = .498 (Table S2 (https://osf.io/gkt8u/), RVE results can be found in the supplemental materials on OSF). Using random-effects models, the effect-size estimate for published data was r = .238 (95% CI [.197, .277]) and the effect-size estimate for unpublished data was r = .214 (95% CI [.109, .313]). Second, we created two funnel plots: a) to visualize the effect size

plotted against sample size, and b) effect size plotted against standard error. Publication bias may be present if the graph looks asymmetrical, with more data points favoring larger effects and samples. Visually, both funnel plots seemed symmetrical (see Figures 4 - 5). The symmetry of the two funnel plots was quantified by the Egger's test, and both results were not significant (sample size: z = .071, p = .943; standard error: z = -.393, p = .694).

For an additional visual representation of publication bias, forest plots were created and sorted by publication status and sample size, and neither displayed any obvious pattern (see supplemental materials on OSF for Figure S4 sorted by publication status (https://osf.io/njtvk/) and Figure S5 sorted by sample size (https://osf.io/abx7t/)).

We conducted a sensitivity analysis developed by Vevea and Wood (2005). Using a "moderate one-tailed selection" as proposed Vevea and Woods (2005), we created intervals of *p*-values. We altered the weights so that significant *p*-values were grouped together; there were at least four effect sizes included in each interval. The adjusted effect-size estimate was r = .207, which although slightly attenuated, is still within the confidence interval of the omnibus effect size. Thus, the effect-size estimate between discrimination and mental health seemed to be robust to publication bias with the current methods used to detect and correct for bias.

Figure 4.
Funnel plot of sample size and correlation coefficient for discrimination dataset.

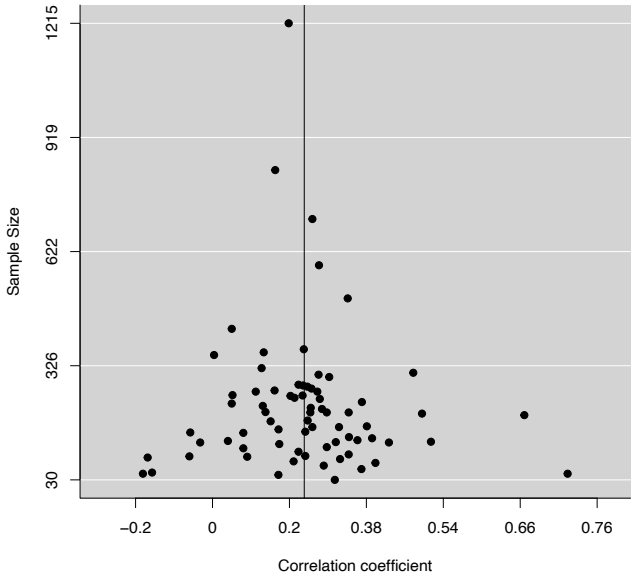
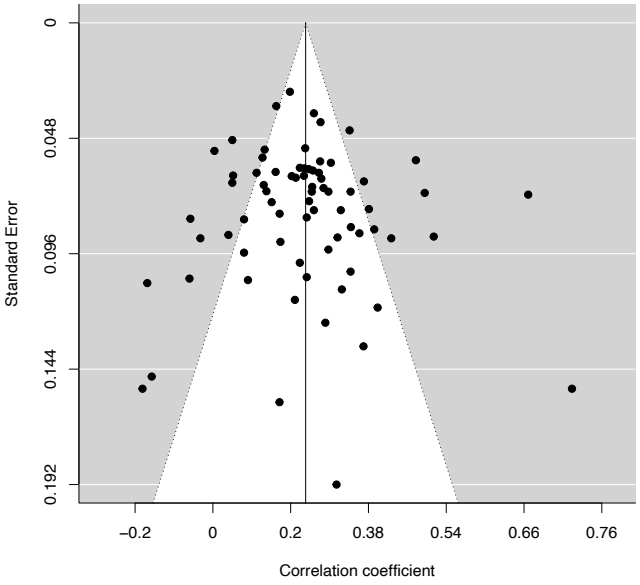


Figure 5.
Funnel plot of standard error and correlation coefficient for discrimination dataset.



RQ2: Moderating effect of study-related, demographic-related, and identity-related variables

We examined study-related (i.e., study-design characteristics, stress measure, and mental health measure[11]), demographic-related, and identity-related moderators when there were at least 10 effect estimates per one covariate or per group, as recommended, in order to aim for sufficient power to detect the true effect (Borenstein & Higgins, 2013; Higgins & Thompson, 2004; Harrer et al., 2019). Most analyses were conducted using mixed-effects models unless specified that RVE meta-regression was a better approach. When results differed between the two models, both results were presented. Supplemental Tables S2 and S3 on OSF (https://osf.io/gkt8u/) include the categorical and continuous moderator results for the model not presented in this section.

[11] Mental-health measure moderators are unregistered exploratory analysis.

Table 4.

Study-related moderators for the discrimination dataset

Moderator	k/kc	b/r	SE	z	p	t	df_t	Q(df)	M(sd)/%	Range
Publication Status	72				0.664			0.189(1)		
Published (reference)	61	0.242/0.238	0.022	11.103						
Unpublished	11	-0.025/0.214	0.057	-0.435	0.664					
Type of Sample Population	72				0.870			0.279(2)		
Community (reference)	47	0.231/0.227	0.025	9.385						
Combination	13	0.015/0.242	0.056	0.274	0.784					
School	12	0.028/0.253	0.056	0.496	0.620					
Correlation Type	72				0.273			1.204(2)		
Pearson r (reference)	63	0.247/0.242	0.021	11.551						
non-Pearson r	9	-0.063/0.182	0.058	-1.097	0.273					
Discrimination Measure:										
Discrimination-level	138									
Personal discrimination (reference)	92	0.233/0.229	0.018			12.918	50.281			
Combination discrimination	18	0.033/0.259	0.075		0.670	0.435	14.423			
Group discrimination	28	-0.137/0.096	0.046		0.008	-2.969	17.765			
Respondent- Discrimination Measure	138									
Self-report (reference)	112	0.201/0.198	0.016			12.578	59.343			
Interview/Combination	26	0.128/0.318	0.087		0.166	1.473	11.996			
Measure Quality Moderators										
Mean Discrimination Measure Items	67	0.006	0.003	2.458	0.014				9.56 (7.96)	(1-36)
Mean Discrimination Measure Alpha	55	0.158	0.299	0.528	0.597				0.84 (0.08)	(0.59-0.96)
Mental Health Measure:										
Mental Health Outcome Direction	138									
Negative outcome (reference)	103	0.256/0.251	0.021			12.398	50.191			
Positive outcome	35	-0.135/-0.121	0.035		<.001	-3.87	38.218			
Respondent- Mental Health Measure	138									
Self-report (reference)	108	0.203/0.201	0.016			12.515	59.073			
Interview/Combination	30	0.102/0.296	0.084		0.244	1.219	13.586			
Measure Quality Moderators										
Mean Mental Health Measure Items	67	0.002	0.001	1.678	0.093				16.62 (19.82)	(1-115)
Mean Mental Health Measure Alpha	58	-0.168	0.27	-0.624	0.533				0.84 (0.08)	(0.6-0.96)
Mean Mental Health Response Period	38	0	0.001	0.045	0.964				49.16 (64.62)	(3-180)

k = number of samples; k_c = number of outcomes or correlations. Note: Random-effects models and mixed-effects models display z statistics, while robust variation estimation (RVE) models display t statistics. Z values are based upon the population mean while t statistics are based upon the sample mean. As random-effects models and mixed-effects model estimate the population means, those models use z values.

Study-design characteristics. In terms of study design, all but one sample in the discrimination dataset used a cross-sectional design. Therefore, there was insufficient power to conduct moderation analyses between cross-sectional and longitudinal designs. We examined the type of sample population as a categorical moderator of the association between discrimination and mental health (see Table 4). The majority of samples were community-based samples (k = 47), followed by school (k = 12), official organization (k = 4), clinic (k = 2), and a combination of samples (k = 7). We grouped organization, clinic, and combination samples together so that there were at least 10 samples in each category. Using mixed-effects modeling, the type of sample population did not explain the between-study variance of the association between discrimination and mental health, $Q_{between}(2) = 0.279$, $p = .870$. We also examined when correlation type—a categorical variable—was a moderator (see Table 4). The majority of samples included a Pearson correlation (k = 63), however, we did calculate a Pearson correlation for some of the samples from various statistics: chi-squared (k = 2), odds ratio (k = 2), and phi correlation (k = 5). We grouped all non-Pearson correlations together to include almost 10 (k = 9), and correlation type was not a significant moderator, $z = 1.097$, $p = .273$. Although there was insufficient information to test if study-design was a significant moderator, the results for the type of sample population and correlation type seem to be robust (see supplemental Table S2 on OSF (https://osf.io/gkt8u/) for RVE results).

Stress measure. We examined discrimination measure characteristic moderators, which included number of items, the measure's internal reliability or Cronbach's alpha, and respondent (Tables 4). The average number of discrimination measure items was 9.56 (*SD* = 7.96; range = 1-36; k = 67). Number of measure items was a significant

moderator, $z = 2.458$, $p = .014$, such that a one-item increase above the mean number of items ($M = 9.56$) resulted in a $r = .006$ increase from $r = .233$. The average Cronbach's alpha or measure of internal reliability was 0.84 ($SD = 0.08$; range = 0.59 – 0.96; k = 55); it was not a significant moderator of the association between discrimination and mental health, $b = .158$, $z = .528$, $p = .597$. Measure respondent was examined using RVE meta-regression approach, as one sample included discrimination reports from multiple respondents. Out of the 138 correlations, the majority included a self-report measure ($k_c = 112$), followed by an interview ($k_c = 21$), and a combination of self-report and a parent interview ($k_c = 5$). Due to the numbers per group, we grouped together all non-self-report measures and found respondent did not explain between-sample variation in effect size, $t(12) = 1.47$, $p = .166$ (see Table 4). Based upon this data, the results are mixed as it relates to our hypothesis, specifically where number of measure items was a significant moderator between discrimination and mental health, but other discrimination measure quality-related variables (Cronbach's alpha and respondent) were not.

Discrimination-level was examined using RVE meta-regression approach, as 11 samples included both correlations for group and personal discrimination. Out of the 138 correlations, the majority included a measure of personal discrimination (k_c[12] = 92), followed by group discrimination ($k_c = 28$), and then a combination measure of both personal and group discrimination ($k_c = 18$). Discrimination-level was a significant moderator of the association between discrimination and negative mental health (Table 4). Personal discrimination, $r = .23$, $p < .001$ resulted in a significantly higher estimate than group discrimination, $r = .10$, ($t(17.8) = -2.969$, $p = .008$). Using a RVE approach,

[12] k_c = number of outcomes or correlations

all three levels of discrimination were significantly associated with negative mental health (Table 5). Thus, consistent with our hypothesis, these results suggest that—although group discrimination is significantly associated with negative mental-health outcomes—personal discrimination may be more detrimental than systematic/group discrimination.

Table 5.
Discrimination-level univariate analyses using RVE

	k	kc	r	95% CI	t.value	p	Tau.squared	I.squared
Group Discrimination	16	28	0.096	[0.008, 0.183]	2.332	0.035	0.015	74.762
Personal Discrimination	56	92	0.229	[0.196, 0.262]	13.387	<.001	0.011	71.197
Combination Discrimination	11	18	0.268	[0.106, 0.416]	3.64	0.005	0.053	91.809

k = number of samples; k_c = number of outcomes or correlations.

Mental health measure. Positive vs. negative mental health was examined using RVE meta-regression approach, with nine samples including both correlations for positive and negative mental health. Out of the 138 correlations, the majority included a measure of negative mental health (k_c = 103), and only 25% of all correlations measured positive mental health (k_c = 35). Consistent with our hypothesis, mental-health outcome direction was a significant moderator between the association of discrimination and mental health, with the relation between discrimination and positive mental-health outcomes, r = -.12, p <.001, being significantly weaker than discrimination and negative mental-health outcomes, r = .25, p <.001 (t (38.2) = -3.87, p < .001; Table 4).

Twenty-three samples included multiple mental-health outcome correlations. Out of the 138 correlations, the breakdown of mental-health outcomes was as follows: anxiety (k_c = 6), depression (k_c = 22), externalizing (k_c = 9), internalizing (k_c = 23), interpersonal (k_c = 5), psychological distress (k_c = 26), psychotic (k_c = 1), PTSD (k_c = 6), self-esteem (k_c = 20), somatization (k_c = 4), and well-being (k_c = 16). Because many of the outcomes

included fewer than 10 correlations, we were unable to perform a moderation analysis of mental-health outcome. However, we conducted subgroup analyses using RVE for the mental-health outcomes with more than 10 correlations (see Table 6)[13]. The results suggest that all outcomes, except for self-esteem and somatization, are significantly associated with discrimination. To highlight the results from specific mental-health outcomes, discrimination was most strongly associated with depression (r = .30; k_c = 22), followed by psychological distress (r = .23; k_c = 26), interpersonal outcomes (r = .22; k_c = 5), externalizing outcomes (r = .22; k_c = 9), anxiety (r = .22; k_c = 6), and well-being (r = -.17; k_c = 16). These results suggest that discrimination has a stronger relationship with negative, psychopathological mental-health outcomes.

Table 6.

Mental health univariate analyses using RVE for the discrimination dataset

	r	95% CI	kc	k	t.value	p	Tau.squared	I.squared
Negative Mental Health	0.254	[0.215, 0.292]	103	55	12.712	<.001	0.017	79.596
Positive Mental Health	-0.124	[-0.18, -0.067]	35	26	-4.468	<.001	0.016	75.161
Depression	0.298	[0.211, 0.381]	22	17	6.977	<.001	0.028	85.072
Anxiety	0.217	[0.006, 0.409]	6	5	2.942	0.046	0.021	73.747
Externalizing	0.218	[0.143, 0.29]	9	3	12.287	0.007	0.02	82.67
Internalizing	0.204	[0.136, 0.271]	23	14	6.364	<.001	0.013	75.355
Interpersonal	0.224	[0.129, 0.315]	5	4	7.457	0.005	0.005	56.235
Psychological distress	0.231	[0.165, 0.295]	26	22	7.098	<.001	0.019	80.477
Self-esteem	-0.074	[-0.152, 0.005]	20	14	-2.034	0.064	0.018	71.006
Somatization	-0.299	[-0.857, 0.581]	4	3	-1.381	0.303	0.073	91.216
Wellbeing	-0.168	[0.094, 0.24]	16	14	4.908	<.001	0.011	75.262

k = number of samples; k_c = number of outcomes or correlations.

Note: Estimates are exploratory when kc/k < 10.

We also examined mental-health measure-quality-related moderators[14], which included number of items, the measure's internal reliability or Cronbach's alpha, respondent and response time period (see Table 4). The average number of mental-health

[13] These analyses are unregistered exploratory analysis.

[14] Mental health measure moderators are unregistered exploratory analysis.

measure items was 16.62 (*SD* = 19.82; range = 1–115; k = 67). Number of measure items was a not a significant moderator, *b* = .002, *z* = 1.678, *p* = .093. The average Cronbach's alpha (index of internal reliability) was 0.84 (*SD* = 0.08; range = 0.60–0.96; k = 58) and was not a significant moderator of the association between discrimination and mental health, *b* = -.168, *z* = -0.624, *p* = .533. Measure respondent was examined using RVE meta-regression approach, as two samples included mental-health outcomes from multiple respondents. Out of the 138 correlations, the majority included a self-report measure (k_c = 108), followed by an interview (k_c = 21), interview with parent (k_c = 6), and teacher-report (k_c = 3). Due to the numbers per group, we grouped together all non-self-report measures and found measure respondent was not a moderator of the association between discrimination and negative mental health, *t* (13.6) = 1.22, *p* = .244. Because self-report measures were the overwhelming majority, it is important to note that the shared method variance is likely leading to an inflated correlation. The average response time period was 49 days (SD=64.62, range = 3-180) and was not a significant moderator. In sum, based upon this data, no mental-health measure-quality significantly moderated the association between discrimination and negative mental-health outcomes.

Demographic characteristics. Table 7 summarizes the meta-regressions for sociocultural characteristics. Sample mean age (*M* = 26.13, *SD* = 9.73) was tested as a continuous moderator in the mixed-effects meta-regression model and did not explain between-sample variation in effect size (*b* = -.002, *z* = -1.235, *p* = .217). Inconsistent with our hypothesis, the percent of women (*M* = 55% women, *SD* = 17.77), tested as a continuous moderator, did not explain between-sample variation in effect size (*b* = .0001, *z* = .092, *p* = .927). In terms of education, 40% of the meta-analytic sample had a

bachelor's degree or higher; however, education data was extracted from only 33 samples. Education as a continuous moderator did not significantly moderate the association between discrimination and mental health (b = -.0001, z = -.096, p = .924).

The majority of samples were from Europe (k = 40), followed by North America (k = 26), and then by Australia/New Zealand (k = 6). Continent was tested as categorical moderator and did not explain between-study variance, $Q_{between}$ (2) = 3.315, p = .191. Although there were not 10 samples per continent, we performed the analysis given that there were more than 10 correlations in the nonaggregate dataset (analyses demonstrate consistent findings; see supplemental materials Table S2 (https://osf.io/gkt8u/) for RVE meta-regression results).

Table 7.

Demographic moderators for the discrimination dataset

Moderator	k/kc	b/r	SE	z	p	t	df_t	Q(df)	M(sd)/%	Range
Mean Age	70	-0.002	0.002	-1.235	0.217				26.13 (9.73)	(10.02-48.75)
% Female	71	0.0001	0.001	0.092	0.927				54.62 (17.77)	(0-100)
% Bachelors+	33	-0.0001	0.001	-0.096	0.924				40.04 (24.40)	(0-87.4)
% Born in Western country	33	0.0001	0.001	0.053	0.958				59.67 (25.85)	(0-100)
% First generation	34	-0.001	0.001	-0.73	0.465				60.57 (29.71)	(11-100)
% Refugee	9	0.002	0.001	2.463	0.014				76.33 (36.10)	(20-100)
Continent	72				0.191			3.315(2)		
Europe (reference)	40	0.228/0.224	0.026	8.634						
North America	26	0.048/0.27	0.042	1.139	0.255					
Australia/New Zealand	6	-0.085/0.143	0.075	-1.135	0.257					
Ethnicity (80+)	93									
Middle Eastern (reference)	53	0.226/0.222	0.021			10.614	32.721			
Asian/South Asian	9	0.038/0.258	0.073		0.630	0.523	3.792			
North African	22	-0.085/0.14	0.055		0.189	-1.531	4.773			
Somali	8	0.152/0.361	0.125		0.265	1.218	6.564			
Middle Eastern/North African	1	-0.176/0.05	0.021		<.001	-8.261	32.721			
Ethnicity	133									
Middle Eastern (reference)	70	0.22/0.216	0.02			10.939	39.264			
Asian/South Asian	19	0.005/0.221	0.048		0.913	0.112	13.839			
North African	26	-0.038/0.18	0.038		0.352	-0.979	9.472			
Somali	8	0.158/0.361	0.125		0.250	1.268	6.278			
Other	10	-0.177/0.043	0.096		0.192	-1.846	2.24			
Muslim sample type	72				0.571			0.322(2)		
Majority Muslim country (reference)	33	0.251/0.246	0.03	8.445						
Muslim	39	-0.023/0.224	0.04	-0.567	0.571					

k = number of samples; k_c = number of outcomes or correlations. Note: Random-effects models and mixed-effects models display z statistics, while robust variation estimation (RVE) models display t statistics. Z values are based upon the population mean while t statistics are based upon the sample mean. As random-effects models and mixed-effects model estimate the population means, those models use z values.

Because there were not 10 samples per country, we were unable to perform moderation analysis. Instead, we present the random-effect model estimates between discrimination and negative mental health for each country (see Table 8). To note, only the estimates for the Netherlands and the United States are a priori analyses, whereas the estimates for countries with fewer than 10 samples are exploratory analyses. Using the random-effects model approach, we found a statistically significant summary effect between discrimination and negative mental health for samples in the Netherlands (k = 15), *r* = .206 (95% CI [.16, .25]). We also found a statistically significant summary effect for samples in the United States (k = 23), *r* = .254 (95% CI [.17, .34]).

Table 8.

Country univariate analyses using RVE for the discrimination dataset

Country	k	r	95% CI	z	p	Tau squared	I squared
Australia	3	0.113	[-0.082-0.299]	1.137	0.255	0.021	74.809
Canada	3	0.377	[0.236-0.502]	4.976	<.001	0.014	75.666
Germany	8	0.332	[0.218-0.436]	5.493	<.001	0.025	81.773
France	2	0.038	[-0.333-0.398]	0.193	0.847	0.069	89.981
Netherlands	15	0.206	[0.164-0.247]	9.446	<.001	0.003	42.024
Norway	4	0.262	[0.128-0.387]	3.768	<.001	0.009	43.547
New Zealand	3	0.17	[-0.058-0.381]	1.467	0.142	0.035	84.296
Sweden	5	0.184	[0.102-0.264]	4.335	<.001	0.006	70.784
United Kingdom	6	0.207	[0.152-0.26]	7.251	<.001	<.001	0.004
USA	23	0.254	[0.169-0.335]	5.69	<.001	0.040	87.933

In order to test if ethnicity or country of origin was a significant moderator, we conducted our analysis with the samples that had 80% or more of the same ethnic group (k = 51). Ethnicity did not account for between-study variance in the estimate using either approach. We also tested to see whether Muslim-sample type (Muslim vs majority Muslim country) was a significant moderator. There were 39 Muslim samples and 33 majority-Muslim-country samples. Muslim sample type was not a significant moderator,

z = -.567, p = .571 (see Table 7 and supplemental material Table S2 (https://osf.io/gkt8u/) for random-effect estimates).

Immigration status was examined through percent born in the western country (60%; k = 33), percent first generation immigrants (61%; k= 34), and percent refugee (76%; k = 9; see Table 7). Percent born in a western country was tested as continuous moderator and did not explain between-study variance in effect size (b = .0001, z = .053, p = .958); percent first generation immigrant did not explain between-study variance in effect size either (b = -.001, z = -.730, p = .465). Percent refugee was slightly unpowered, as only nine samples included that information; tested as a continuous moderator, refugee status did explain between-study variance in effect size, with refugee status related to an r = .002 increase in the association between discrimination and negative mental health. These results suggest that among the indicators of immigration status, only *refugee status* moderated the association between discrimination and mental health (see Table 7 and supplemental material Table S3 (https://osf.io/gkt8u/) for random-effect estimates).

Identity. Twenty-two unique samples included identity variables in the discrimination dataset. Identity variables included: ethnic identity (k = 15), religious identity (k = 12), national identity (k = 4), identity integration (k =1), and visible identity (k = 2).[15] The mean, standard deviation, and range were extracted for each identity measure and used to calculate the Percentage of Maximum Possible (POMP) score, which is a useful way to standardize across different metrics of measurements (Cohen et al., 1999; Fisher & Milfont, 2010). There was only enough information to conduct the

[15] The number of samples listed in Table 9 do not match the numbers listed here as Table 9 only includes the samples with extractable data.

moderation analyses for ethnic identity and religious identity,[16] therefore, qualitative reviews of other identity moderators were conducted.

Ethnic identity. Fourteen samples included the information needed to conduct the moderation analysis of ethnic identity on the association between discrimination and negative mental health. Inconsistent with our hypothesis, the results showed that ethnic identity (M = 68.64, SD = 17.18; k = 14) did not explain the between-study variance in the effect-size estimate, b = -.001, z = -.19, p = .85 (see Table 9 and Table S3).

Table 9.

Identity moderators for the discrimination dataset

	k/kc	b	SE	z	p	M(sd)/%	Range
Ethnic Identity	14	-0.001	0	-0.19	0.85	68.64 (17.18)	(39.8-85)
Religious Identity	8	0.0004	0.01	0.03	0.97	78.13 (6.95)	(65-85)

k = number of samples; k_c = number of outcomes or correlations. Note: Random-effects models and mixed-effects models display z statistics, while robust variation estimation (RVE) models display t statistics. Z values are based upon the population mean while t statistics are based upon the sample mean. As random-effects models and mixed-effects model estimate the population means, those models use z values.

The qualitative review that includes 15 samples, with 33 correlations, showed varied results for the moderating effect of ethnic identity on the association between discrimination and mental health. The results varied based upon the subscale of ethnic identity (e.g., centrality vs. private regard), type of discrimination (group vs. personal) and outcome type (e.g., depression vs. anxiety), but there does not appear to be a pattern among the results. Three samples found that stronger ethnic identity protects against the deleterious association between discrimination and mental health (Musa, 2015; Schaafsma, 2011; Stevens & Thijs, 2018). Four samples found that stronger ethnic identity exacerbates the deleterious effects of the association between discrimination and

[16] The results for religious identity are exploratory as the analysis is underpowered (k = 8). Table S3 includes RVE analyses for both ethnic identity and religious identity that shows consistent nonsignificant results.

mental health (Alemi & Stempel, 2018; Jasperse et al., 2012; Lowe et al., 2019; Stevens & Thijs, 2018). Finally, five samples found that ethnic identity did not moderate the association between discrimination and mental health (Ahmed, 2006; Lowe et al., 2019; Alemi et al., 2017; Musa, 2018; Musa, 2015).

Three samples suggest that stronger ethnic identity protects against the deleterious association between discrimination and mental health (Musa, 2015; Schaafsma, 2011; Stevens & Thijs, 2018). In a sample of Turkish and Moroccan individuals living in the Netherlands, ethnic identity moderated the relationship between subtle and blatant discrimination and well-being. For those who reported high ethnic identity and high subtle discrimination (interaction ß = .10, p <.05) or blatant discrimination (interaction ß = .08, p <.05), they had higher well-being compared to those who reported lower ethnic identity and high subtle or blatant discrimination (Schaafsma, 2011). Additionally, in a sample of Muslim-American adults, ethnic-identity centrality was found to be a protective factor against anxiety symptoms in the context of discrimination (interaction ß = -.09, p = .04), and ethnic-identity-based private regard moderated the association between discrimination and anxiety (where lower private regard was a risk factor for increased anxiety; interaction ß = -.08, p = .04; Musa, 2015). Similarly, in a sample of Dutch Moroccan adolescents, ethnic identity moderated the association between group discrimination and internalizing outcomes, with ethnic identity being a protective factor in the association between the experience of group-based discrimination and personal internalizing outcomes (interaction ß = -.19– -.25, p <.01; Stevens & Thijs, 2018). Thus, these results suggest there is some evidence of a small moderating effect of stronger

ethnic identity as a protective factor of the deleterious association between discrimination and mental health.

Four samples suggest that stronger ethnic identity exacerbates the association between discrimination and mental health (Alemi & Stempel, 2018; Jasperse et al., 2012; Lowe et al., 2019; Stevens & Thijs, 2018). In a sample of Afghan refugees who are resettled in the United States, ethnic identity moderated the relation between personal discrimination and psychological distress, with increased ethnic identity exacerbating the negative association between discrimination and negative mental health (interaction ß = .28, p <.01; Alemi & Stempel, 2018). Similarly, in a sample of Dutch Moroccan adolescents, ethnic identity moderated the association between personal discrimination and internalizing outcomes, with ethnic identity exacerbating the deleterious effects of personal discrimination on internalizing outcomes (interaction ß = .14–.20, p <.01–.05; Stevens & Thijs, 2018). Furthermore, in a sample of Muslim women in New Zealand, ethnic identity moderated the relation between religious discrimination and psychological symptoms (interaction ß = .27, p <.01) and life satisfaction (interaction ß = -.19, p <.05), and individuals with stronger ethnic identity contain greater risk for negative consequences of discrimination (Jasperse et al., 2012). Similarly, in a sample of Muslim-American students, ethnic identity moderated the relation between generalized anxiety disorder symptoms and discrimination; individuals with stronger ethnic identity appear to experience higher levels of discrimination and anxiety symptoms (interaction ß = .01, p <.05; Lowe et al., 2019). Thus, these results suggest there is some evidence that increased ethnic identity may exacerbate the deleterious association of discrimination and mental health.

Five samples found that ethnic identity did not moderate the association between discrimination and mental health (Ahmed, 2006; Lowe et al., 2019; Alemi et al., 2017; Musa, 2018; Musa, 2015). Then, studies from six samples did not conduct moderation analyses (Ameline et al., 2019, Hakim et al., 2018., Husain, 2008; Mewes et al., 2015; Shah, 2018; Verkuyten & Nekuee, 1999).

These mixed results from the qualitative review are parallel with results from a recent meta-analysis on the moderating role of ethnic identity on the association between discrimination and adjustment (Yip et al., 2019), with varied dimensions of ethnic identity being associated differentially with psychological outcomes. Yip and colleagues (2019) found that results also varied based on sample demographics. Therefore, this qualitative review of Muslim samples highlights a varied impact of ethnic identity on the association between discrimination and mental health. Therefore, there is a need for preregistered, high-powered studies that use validated measures of ethnic identity to examine the moderating effect of components of ethnic identity on different types of discrimination/mental-health outcomes for Muslim samples.

Religious identity. Eight samples included the information needed to conduct the moderation of religious identity on the association between discrimination and negative mental health. Inconsistent with our hypothesis, the exploratory results showed that religious identity (M = 78.13, SD = 6.95; k = 8) did not explain the between-study variance in effect size, b = .0004, z = .03, p = .97 (see Table 9 and Table S3).

The qualitative review of the 12 samples, which included 15 correlations, showed that in three samples, religious identity served as a protective factor that weakened the association between experiences of discrimination and mental-health outcomes (Jasperse

et al., 2012; Adam & Ward, 2016; Stuart & Ward, 2018); also, in three additional samples, no significant association was found (Adam & Ward, 2016; Ahmed, 2006; Stuart & Ward, 2018).

Three unique samples from New Zealand found that religious identity moderated the association between discrimination and mental health, with stronger religious identity buffering against the negative association of discrimination on mental-health outcomes (Jasperse et al., 2012; Adam & Ward, 2016; Stuart & Ward, 2018). In a sample of Muslim women in New Zealand, religious identity was found to moderate the relationship between a) religious discrimination and life satisfaction (interaction ß = .18, p <.05) and b) religious discrimination and psychological distress **(**interaction ß = -.30, p <.01), with those who were less engaged in religious practice being at greater risk for negative consequences of discrimination (Jasperse et al., 2012). Similarly, in an ethnically diverse sample of Muslims living in New Zealand, Adam and Ward (2016) found that behavioral religious coping moderated the relation between personal discrimination and life satisfaction (interaction ß = .79, p <.05); specifically, the negative effects of discrimination on life satisfaction were attenuated for individuals who engage in high levels of behavioral religious coping (Adam & Ward, 2016). Moreover, in a sample of New Zealand Muslims, religious identity moderated the association between discrimination and depression with religious identity buffering the detrimental influence of discrimination on depression (interaction ß = -.49, p <.001; Stuart & Ward, 2018). Thus, these findings suggest a moderate to large effect of religious identity being protective against the negative impact of discrimination on mental-health outcomes for Muslims in New Zealand.

Three samples found that religious identity did not moderate the association between discrimination and mental health (Adam & Ward, 2016; Ahmed, 2006; Stuart & Ward, 2018). Seven samples did not include moderation analyses (Stevens et al., 2005-1; Stevens et al., 2005-2; Hakim et al., 2018; Hashemi et al., 2019; Hernandez, 2012; Husain, 2008; Musa, 2018; Wilson, 2017). In summary, the qualitative review suggests religious identity may either protect or have no effect on the deleterious association between discrimination and mental health.

National identity. Four samples included national identity (Hakim et al., 2018(s1,s2); Schaafsma, 2011; Thijs et al., 2018). Only Schaafmsa (2011) conducted moderation analyses, and in a sample of Turkish and Moroccan individuals living in the Netherlands, Schaafmsa found that stronger national identity exacerbated the negative association between a) subtle discrimination and well-being (interaction ß = -.07, p <.05) and b) blatant discrimination and well-being (interaction ß = -.05, p <.05). Although the results were significant, the strength of association is near zero.

Identity integration. Only one sample included a measure of identity integration (Alemi & Stempel, 2018). Researchers found that, in a sample of Afghan refugees resettled in the United States, identity integration did not moderate the relation between discrimination and mental health.

Visible identity. Two samples included a measure of visible identity, and no moderating effect was found. Jasperse and colleagues (2012) did not find a significant moderating effect of visible identity between religious discrimination and life satisfaction or psychological symptoms. Musa (2015) did not conduct moderation analyses.

Summary of meta-analytic results of the association between discrimination and mental health

The summary correlation between experiences of discrimination and mental health for Muslims living in Western countries was consistent with our hypothesis—it was positive and falls within the small to moderate range. The estimate was robust as it relates to outliers, dependence within-study effects, and publication bias with the methods used to detect and correct for biases. There was high heterogeneity within the sample, but it appeared that the heterogeneity was due to true variance within the population. In terms of demographic characteristics, these meta-analytic results were from an ethnically and geographically diverse, educated, and young Muslim population.

Interestingly, both discrimination level and mental-health-outcome direction significantly explained between-study variance in the effect-size estimate. Consistent with our hypothesis, discrimination level was a significant moderator with personal discrimination having a stronger association with mental health compared to group discrimination; but, importantly, all discrimination types were significantly association with mental-health outcomes. Also consistent with our hypothesis, mental-health-outcome direction was a significant moderator between the association of discrimination and mental health, specifically where the relation between discrimination and positive mental-health outcomes was significantly weaker than discrimination and negative mental-health outcomes; but, again, both positive and negative outcomes were significantly associated with discrimination.

All moderation analyses outlined in our research questions were challenging due to limiting information available in articles and small samples per moderator. The meta-

analytic estimate appeared to be unaffected by most other study-, demographic- and identity-related moderators that were tested. However, evidence suggested that the number of discrimination-measure items was a significant moderator; thus, as the number of discrimination-measure items increased, the association between discrimination and mental health grew stronger. Additionally, although slightly underpowered by one study, refugee status appeared to be a significant moderator of the association between discrimination and negative mental-health outcomes, specifically where refugee status increased the association between discrimination and negative mental health.

Life stressors

We conducted a parallel meta-analysis for the life-stressor dataset. We follow the same analysis plan outlined in the data analysis section. In the section below, we aim to reduce redundant information that was already explained in greater detail while presenting the meta-analytic results on discrimination.

Sample Characteristics

Table 10 summarizes individual sample (i.e., dataset) details included in the life-stressor meta-analysis, and Table 11 summarizes descriptive statistics of the life-stressor dataset (see supplemental materials, Table S8 (https://osf.io/gkt8u/) for additional individual sample and correlation details, including measure names). The 69 unique samples had a mean sample size of 236 (*SD* = 236.07; range = 10–1,215) and represented individuals living in 11 different Western countries (see Table 11). The mean age of participants was 30 years old (*SD* = 11.82; range = 3–49 years old), with 49% women and a collapsed majority ethnic breakdown of 59% Middle Eastern, 25% Somali, 9% Asian/South Asian, 6% Bosnian Peninsula, and 1% North African (see Table 11 for a

detailed ethnicity breakdown and Table 10 for breakdown individual majority ethnicities represented by each sample). Many life stressors were measured, including abuse (1%; k =1), bullying (1%; k =1), general life (13%; k =9), migration trauma (3%; k =3), physical abuse (1%; k =1), verbal abuse, sexual trauma (1%; k =1), trauma (1%; k =1), war trauma (46%; k =32), and a combination of these stressors (19%; k =13; e.g., war trauma and general life (k =2)).[17] Samples measured diverse mental-health outcomes, including depression (k = 9), PTSD (k = 10) and anxiety, and broader categorizations, such as psychological distress (k = 13), wellbeing (k = 1), somatization (k = 3), and externalizing (k = 1). Many samples also measured multiple types of mental-health outcomes (k = 32), such as depression and PTSD (k = 10), PTSD and psychological distress (k = 3), and depression and anxiety (k = 2).[18]

[17] Number of samples do not match Table 14 as stressors were grouped differently for analyses.
[18] Number of samples do not match Table 15 due to different ways of categorizing mental-health outcomes and the number of samples that measured multiple mental-health outcomes. When necessary, mental-health outcomes were approximately categorized using the Hierarchical Taxonomy of Psychopathology dimensions (Kotov et al., 2017).

Table 10.

Life stressor individual sample details

Sample	N	Age	%F	Ctry	Muslim type	%Ethnicity	Sample Type	Pub	Design	Quality	*r*	# corr /sample	Stress Types	MH Types
Albdour, 2017	150	14	30	USA	Muslim	45.3 YE, 21.3 LB, 8 IQ, 4 SY, 6 JO, 12 PAL, 3.3. OTH	org.	T	C	fair	0.5	2	BUL	DIS, SOM
Aldhalimi, 2015	48	37	0	USA	Iraqi	NR	comb.	T	C	poor	0.31	1	WR.TR	PTSD
Alemi & Stempel, 2018	259	49	51	USA	Afghan	100 AF	comm.	P	C	fair	0.49	1	WR.TR	DIS
Alhasanat et al., 2017	50	30	100	USA	Muslim	48 YE, 2 SA, 2 QA, 4 PAL, 30 LB, 2 JO, 10 IQ, 2 EG	org.	P	C	poor	0.3	1	G.LIFE	DEP
Almqvist & Broberg, 1999	39	8	26	SE	Iranian	100 IR	comm.	P	L	poor	0.33	4	WR.TR, BUL	W.B, DIS
Altawian, 2016	246	36	100	USA	Muslim	100 SA	school	T	C	poor	0.84	4	PH.AB, V.AB	PTSD, DEP
Arnetz et al., 2012	350	46	44	USA	Iraqi	100 IQ	comm.	P	C	fair	0.42	4	WR.TR	SOM, PTSD, DEP
Arnetz et al., 2014	298	33	46	USA	Iraqi	100 IQ	comb.	P	C	fair	0.23	4	WR.TR, PH.AB, G.LIFE	PTSD, DEP
Arnetz et al., 2013	128	41	59	USA	Iraqi	58.6 IQ, 41.4 MDE (LB, JO, EG, and YE)	comm.	P	C	poor	0.37	2	AB	PTSD, DIS
Bentley et al., 2012	74	39	26	USA	Somali	100 SO	comm.	P	C	fair	0.57	8	WR.TR, MIG.TR	SOM, PTSD, DEP, ANX
Bronstein et al., 2012+Bronstein & Montgomery, 2013	222	16	0	UK	Afghani	100 AF	org.	P	C	poor, fair	0.48	2	TR	PTSD, DIS
Dadouch, 2019	19	35	45	USA	Muslim	100 SY	org.	T	C	poor	-0.12	2	TR	SOM, PTSD
East et al., 2018(s1)	198	10	44	USA	Muslim	100 SO	comm.	P	C	good	0.5	2	BUL, TR	DEP
East et al., 2018(s2)	198	39	100	USA	Muslim	100 SO	comm.	P	C	good	0.25	2	WR.TR	DEP
Elsayed et al., 2019	103	8	53	CA	Syrian	100 SY	comm.	P	C	fair	-0.07	2	G.LIFE	EXT, INT
Ellis et al., 2015	79	21	0	USA	Somali	100 SO	comm.	P	C	fair	0.41	2	G.LIFE, PH.AB	PTSD
Ellis et al., 2008	129	15	38	USA	Muslim	100 SO	comm.	P	C	fair	0.49	2	WR.TR	PTSD, DEP
Ellis et al., 2013	30	13	37	USA	Somali	100 SO	school	P	L	good	0.38	2	WR.TR	PTSD, DEP
Groen et al., 2019	57	36	35	NL	Iraqi	51 IQ, 49 AF	clinic	P	C	fair	0.27	2	WR.TR	INT, PTSD
Hassouneh & Kulwicki, 2007	30	38	100	USA	Muslim	33 PAL, 30 IQ, 23 LB, 7 SY, 3 YE, 3 AA	comm.	P	C	poor	-0.02	3	WR.TR	ANX, DEP
Hellemans et al., 2015	392	34	50	BE	Muslim	100 TK	comm.	P	C	good	0.14	2	PH.AB, V.AB	W.B
Jakobsen et al., 2017	138	16	0	NO	Afghan	74 AF, 23 SO, 2 IR, 1 DZ	org.	P	L	fair	0.07	1	TR	DIS
Jobson et al., 2016	43	42	40	UK	Iranian	100 IR	comm.	P	C	poor	-0.14	2	TR	PTSD, DEP
Matheson et al., 2008/Jorden et al., 2009	156	28	64	CA	Muslim	100 SO	comm.	P	C	fair	0.29	12	WR.TR, TR, PH.AB, G.LIFE	PTSD, DEP
Jorgenson, 2017	80	30	62	USA	Somali	100 SO	org.	T	C	fair	0.38	2	WR.TR	PTSD, INT

Table 10 (continued)

Sample	N	Age	%F	Ctry	Muslim type	%Ethnicity	Sample Type	Pub	Design	Quality	*r*	# corr /sample	Stress Types	MH Types	Idenity
Kia-Keating & Ellis, 2007	76	16	46	USA	Somali	100 SO	comm.	P	C	fair	0.53	2	WR.TR	PTSD, DEP	
Kira et al., 2010/2012(s2)	501	36	45	USA	Muslim	100 IQi	comm.	P	C	fair	0.27	1	TR	DIS	
Kira et al., 2012(s1)	286	40	62	USA	Iraqi	60.1 IQ, 19.8 LB, 10.1 YE, 6.9 OTHA, and 6 OTHNA	clinic	P	C	fair	0.16	1	WR.TR	DIS	
Koch et al., 2019	74	20	0	DE	Afghan	100 AF	comb.	P	C	fair	0.25	3	TR	ANX, PTSD, DEP	
Kulwicki et al., 2015	312	36	100	USA	Muslim	43.6 LB, 38.5 IQ, 8.3 YE, 5.8 PAL, 3.8 OTH	comm.	P	C	good	0.44	1	PH.AB	DEP	
Areba, 2014	156	21	75	USA	Muslim	100 SO	school	T	C	fair	0.38	3	G.LIFE	ANX, DEP, W.B	Y
Lincoln et al., 2016	135	15	38	USA	Somali	100 SO	school	P	C	fair	0.49	2	WR.TR	PTSD, DEP	
Lindencrona et al., 2008	115	34	38	SE	Middle Eastern	100 MDE	org.	P	C	poor	0.33	2	G.LIFE	PTSD, DIS	
Lembcke et al., 2020	31	3	55	DE	Syrian/Iraqi	100 SY or IQ	comm.	P	C	poor	-0.12	1	WR.TR	PTSD	
Mghir et al., 1995	38	18	45	USA	Afghani	100 AF	comm.	P	C	poor	0.48	1	WR.TR	DIS	
Muller et al., 2012/2018	334	43	60	DE	Turkish	100 TK	clinic	P	C	poor, fair	0.23	2	MIG.TR	W.B, PTSD	
Mohlen et al., 2005	10	13	40	DE	Kozovo	90 KOS-AL, 10 RO	org.	P	I	poor	0.89	4	WR.TR	PTSD, W.B, ANX, DEP	
Montgomery & Foldspang, 2006	311	8	49	DK	Iraqi	54 IQ, 7 LB, 10 IR, 4 SY, 24 PAL, .003 TR	org.	P	C	fair	0.4	5	WR.TR	DIS	
Nilsson et al., 2008	62	38	100	USA	Muslim	100 SO	org.	P	C	poor	0.76	2	PH.AB, V.AB	DIS	
Oberoi, 2014	177	17	67	USA	Muslim	58 A, 23 W, 17 OTH, 3 AA, 1 H	school	T	C	fair	0.58	1	G.LIFE	DIS	Y
Lien et al., 2010(s1)	602	42	40	NO	Iranian	100 IR	comm.	P	C	fair	0.19	2	WR.TR	DIS	
Lien et al., 2010(s2)	448	45	44	NO	Pakistani	100 PK	comm.	P	C	fair	0.21	2	WR.TR	DIS	
Lien et al., 2010(s3)	426	42	46	NO	Turkish	100 TR	comm.	P	C	fair	0.13	2	WR.TR	DIS	
Perera et al., 2013	437	38	49	USA	Somali	58 SO, 42 ORO	org.	P	L	good	0.44	3	WR.TR, G.LIFE	PTSD	
Mghir & Raskin, 1999	15	17	29	USA	Pashtun	100 PAS	comm.	P	C	poor	0.69	1	WR.TR	PTSD	
Stevens et al., 2005a/2005b+Stevens & Thijs, 2018	376	14	52	NL	Muslim	100 MA	comm.	P	C	fair	0.19	4	G.LIFE	EXT, INT	Y
Turner et al., 2003	842	38	53	UK	Kosovan	100 KOS	org.	P	C	fair	0.38	4	WR.TR	DIS, PTSD, DEP, ANX	
Woods, 2004	220	36	41	UK	Somali	100 SO	comm.	T	C	fair	0.4	3	WR.TR	INT, EXT, PTSD	
Zoellner et al., 2018	39	NR	36	USA	Muslim	100 SO	org.	P	C	poor	0.4	1	WR.TR	PTSD	
Abuelezam & El-Sayed, 2018	354	40	47	USA	Arab American	100 AB	comm.	P	C	poor	0.01	6	PH.AB, V.AB, SE.TR	DEP, DIS	
Ahmed et al., 2017	12	27	100	CA	Syrian	100 SY	org.	P	C	poor	0.37	1	MIG.TR	DEP	
Almqvist & Brandell-Forsberg, 1997	50	6	28	SE	Muslim	100 IR	comm.	P	L	poor	0.2	1	TR	PTSD	
Chaudhry, 2007	202	37	100	UK	Muslim	100 PK	comm.	T	L	fair	0.4	1	G.LIFE	DEP	

Table 10 (continued)

Sample	N	Age	%F	Ctry	Muslim type	%Ethnicity	Sample Type	Pub	Design	Quality	*r*	# corr /sample	Stress Types	MH Types	Idenity
Chung et al., 2018	564	35	32	SE	Syrian	100 SY	comm.	P	C	fair	0.24	4	WR.TR, G.LIFE	PTSD	
Daud et al., 2008	67	43	0	SE	Iraqi	54 IQ, 46 RB	clinic	P	C	fair	0.69	1	WR.TR	SOM	
Fox & Johnson-Agbakwu, 2020	879	31	100	USA	Somali	68 SO, 26 SOB	comm.	P	C	fair	0.22	1	TR	DIS	
Ghazinour et al., 2004	100	37	34	SE	Iranian	100 IR	comb.	P	C	poor	0.25	1	WR.TR	DEP	
Gilgen et al., 2002	98	36	60	CH	Turkey	63 TR, 37 BA	clinic	P	C	poor	0.35	1	WR.TR	DIS	
Gorst-Unsworth & Goldenberg, 1998	84	39	0	UK	Muslim	100 IQ (45 KUR)	clinic	P	C	poor, fair	0.41	2	SE.TR, WR.TR	PTSD, DEP	
Hondius et al., 2000	156	26	34	NL	Turkish/Iranian	100 TR/IR	org.	P	C	poor	0.14	2	WR.TR	DEP, ANX	
Husain, 2010	237	28	100	UK	Muslim	100 PK	clinic	T	L	good	0.28	1	G.LIFE	DEP	
Husain et al., 1997	77	36	73	UK	Pakistani	100 PK	clinic	P	C	fair	0.39	1	G.LIFE	DEP	
Mirza, 2014	57	15	60	USA	Muslim	50.9 SAS, 38.7 M, 10.5 AAA, 35.1 MDE	comm.	T	C	fair	0.43	1	TR	EXT	Y
Momartin et al., 2003	86	47	61	AU	Muslim	100 BA	comb.	P	C	fair	0.42	1	WR.TR	PTSD	
Tinghog et al., 2017	1215	34	37	SE	Syrian	100 SY	comm.	P	C	fair	0.18	3	WR.TR	INT, PTSD, W.B	
Tomlins, 2016	74	44	100	USA	Iranian	43 IR, 32 IQ, 24 AF	org.	T	C	fair	0.93	2	SE.TR	PTSD, DEP	
Westermeyer et al., 2010	622	37	48	USA	Somalian	100 SO	comm.	P	C	fair	0.32	1	WR.TR	SOM	
Willard et al., 2014	497	42	44	USA	Iraqi	100 IQ	org.	P	C	poor	0.4	1	WR.TR	SOM	
Ai et al., 2002	129	35	45	USA	Muslim	98 AL/KOS	org.	P	C	fair	0.39	1	WR.TR	PTSD	

NR = Not reported; Country: NZ = New Zealand; SE = Sweden; FR = France; DE = Germany; UK = United Kingdom; CA = Canada; NL = Netherlands; AU = Australia; DK = Denmark; NO = Norway; CH = Switzerland; BE = Belgium; Ethnicity: A = Asian or Asian American; AA = African American or Black; AAA= African Arab; AB = Arab or Arab American; AF = Afghanistan; AL = Albania; BA = Bosnia; DZ = Algeria; EG = Egypt; H = Hispanic or Latinx; IQ = Iraq; IR = Iran; JO = Jordan; KOS = Kosovo; KUR = Kurdish; LB = Lebanon; M = Multiethnic; MA = Morocco; MDE = Middle East; ORO = Oromo; OTH = Other; OTHA= Other Arabic; OTHNA= Other non-Arabic; PAL = Palestine; PAS = Pashtun; PK = Pakistan; QA = Qatar; RO = Romania; SA = Saudi Arabia; SAS = South Asian; SO = Somalia; SOB = Somali Bantu; SY = Syria; TR = Turkey; W = White; YE = Yemen; Study-Deisgn: C = crosssectional; L = longitudinal; I = intervention; Publication Status: P = published; T = thesis; Stressor: TOR = torture; WR.TR = war truama; MIG.TR = migraiton trauma; TR = trauma; G.LIFE = general life; PH.AB = phsyical abuse; V.AB = verbal abuse; BUL = bullying; AB = abuse; SE.TR = sexual trauma; Mental health: INT = internalizing; PTSD = PTSD ; W.B = wellbeing; DIS = psychological distress; EXT = externalizing; INTP = interpersonal; DEP = depression; ANX = anxiety; SE = self-esteem; SOM = somatization; PSYC = psychotic; Sample Type: comm. = community; comb. = combination; org. = organization

Table 11.

Life stressor dataset sample characteristics

Sample characteristics	Details
k	69
kc	157
N (range)	236 (10-1215)
Age (M(sd))	29.59 (11.82)
% Female (sd)	49.05 (25.11)
Data from published sources (%)	83
Country	
- Australia	k=1
- Belgium	k=1
- Canada	k=3
- Denmark	k=1
- Germany	k=4
- Netherlands	k=3
- Norway	k=4
- Sweden	k=7
- Switzerland	k=1
- United Kingdom	k=8
- USA	k=36
Majority Ethnicity	
- Asian/South Asian	k=6 (8.7%)
- Bosnian Peninsula	k=4 (5.8%)
- Middle Eastern	k=41 (59.4%)
- North African	k=1 (1.4%)
- Somali	k=17 (24.6%)
Majority Ethnicity (detailed)	
- Afghani	k=5
- Albanian/Kosovar	k=1
- Arab	k=1
- Asian/South Asian	k=2
- Bosnian	k=1
- Iranian	k=6
- Iraqi	k=11
- Kosovan	k=2
- Lebanese	k=1
- Middle Eastern	k=2
- Moroccan	k=1
- Pakistani	k=4
- Palestinian	k=1
- Pashtun	k=1
- Saudi Arabia	k=1
- Somali	k=17
- Syrian	k=5
- Syrian/Iraqi	k=1
- Turkey	k=1
- Turkish	k=3
- Turkish/Iranian	k=1
- Yemen	k=1

k = number of samples; kc = number of outcomes or correlations.

RQ3: Bivariate effect-size estimate between life stressors and mental health

Weighted mean correlations. Figure 6 shows the weighted mean correlations, using both a random-effects model with the restricted maximum likelihood (REML) estimator and RVE estimator, along with the univariate effect-size estimates and their 95% confidence intervals of the 69 samples in the life stressors dataset. Using the random-effects model approach, we found a statistically significant summary effect, r = .366 (95% CI [.31, .42]), z = 11.30 p < .001. Using the RVE approach, we also found a statistically significant summary effect, r = .320 (95% CI [.27, .36]), t (64.1) = 12.99, p <.001. The RVE estimate is slightly attenuated compared to the random-effects estimate. Supplemental Figure S6 (https://osf.io/xmk42/) on OSF includes the RVE weight estimates per correlation in each study. Both estimates have confidence intervals that showed the true association is unlikely to be zero, and the narrow confidence intervals show precision in this summary effect. Table 12 summarizes these statistics with three decimal places. Consistent with our hypothesis, the summary correlation between life stressors and mental health for Muslims living in Western countries falls between the moderate to large range.

Heterogeneity. The data showed a statistically significant and large degree of between-study variability, $Q(68) = 701.86$, $p < .001$, $\tau^2 = 0.068$, $I^2 = 93.52\%$ (see Table 12). The significant Q statistic indicated that the included studies do not share a common true effect size. The I^2 statistic indicated substantial heterogeneity in the true effect, meaning that a large proportion of the observed variance between life stressors and mental-health outcomes can be attributed to variances in the true effect rather than sampling error. The index of dispersion for the correlation of interest was -0.13 to 0.72; this shows that, within the population, bivariate correlations would fall between this

range 95% of the time. Therefore, in some cases, there is a small, negative effect, and in others, there are worse outcomes. Given the large index of dispersion, it is important to examine potential moderators of the association between life stressors and mental-health outcomes.

Table 12.

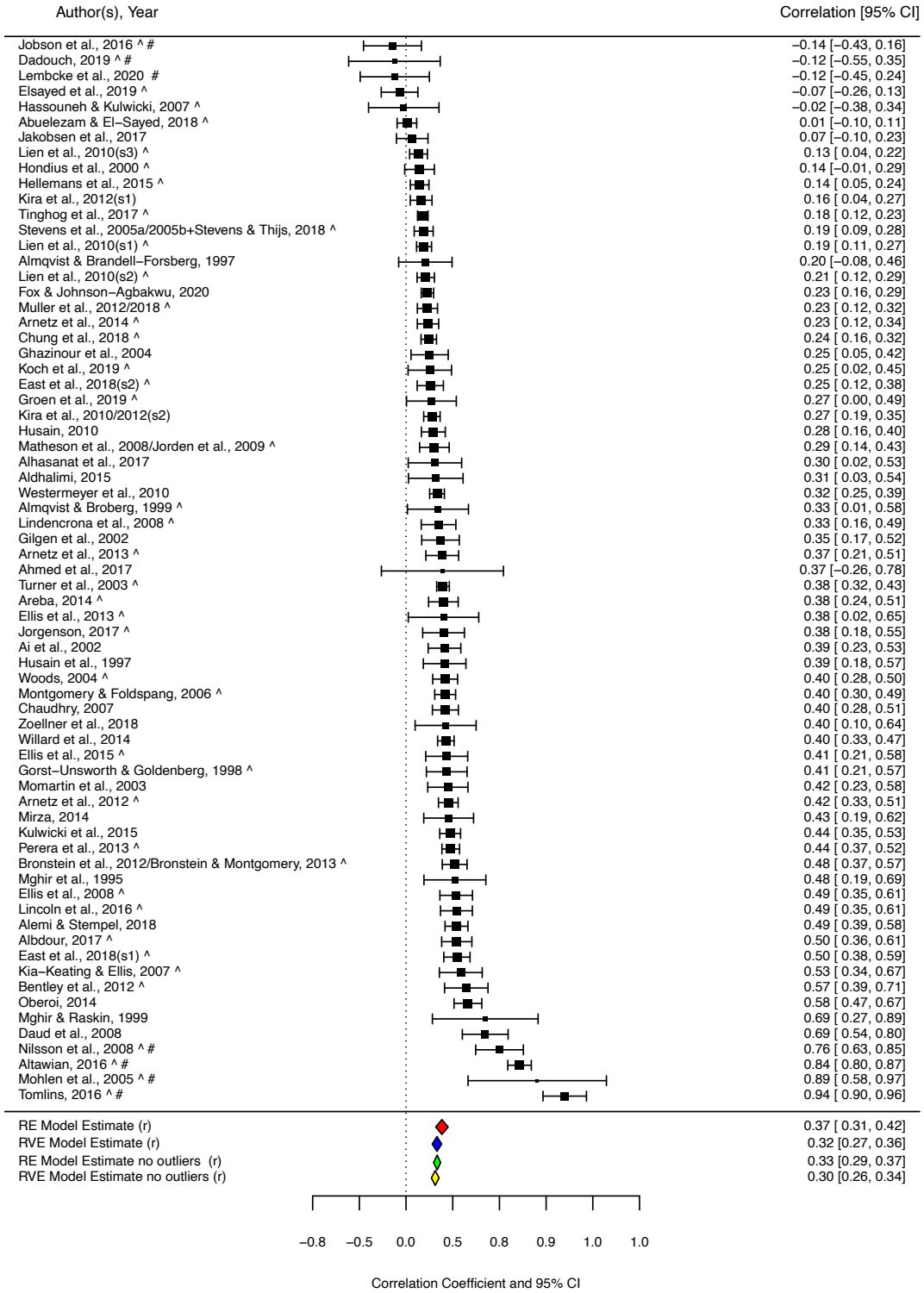
Life stressor meta-analysis results using random-effects and RVE models.

Measure	Random-Effects Model	RVE ρ = .8	RVE ρ = 0	RVE ρ = .2	RVE ρ = .4	RVE ρ = .6	RVE ρ = 1
r	0.366	0.32	0.32	0.32	0.32	0.32	0.32
95% CI	[0.307, 0.423]	[0.273, 0.365]	[0.273, 0.365]	[0.273, 0.365]	[0.273, 0.365]	[0.273, 0.365]	[0.273, 0.365]
z-value	11.303						
t-value		12.99	12.991	12.991	12.991	12.991	12.99
tau-squared	0.068	0.032	0.032	0.032	0.032	0.032	0.032
Q-stat	701.855						
I-squared	93.52	87.031	86.966	86.983	86.999	87.015	87.047
Index of Dispersion	[-0.13, 0.716]						

Note: Robust variance estimation (RVE) model at ρ = .8 is the weight used in all RVE models.

Figure 6.

Forest plot summarizing mean sample-level correlations (Pearson's r) between life stressors and mental-health outcomes, random-effect-size estimate, robust variance estimation effect-size estimate, and estimates without outliers.



 # = influential case/outlier; ^ = includes aggregated correlations

Influential cases and outliers. Figure 6 shows the seven influential samples identified through examining DFFITS values, Cook's distances, hat values, DFBETAS values, and effect sizes that are 2.5 absolute deviations below/above the median (Viechtbauer & Cheung, 2010). As a sensitivity analysis, we removed the seven influential samples and recalculated the effect-size estimates ($k = 62$, $k_c = 140$). The weighted random-effects and RVE estimates were unchanged and still statistically significant with high heterogeneity (albeit, lower heterogeneity), $r = .333$ (95% CI [.30, .37]) and $r = .303$ (95% CI [.26, .33]), respectively (see Figure 6 for estimates without influential cases, and see supplemental materials, Figures S7 (https://osf.io/baesz/) & S8 (https://osf.io/3uwfk/), for all outlier statistics). The *leaveout* analysis in R demonstrated that, when removed, each influential case either slightly attenuated or increased the omnibus effect size, but it did not alter the statistical significance of the effect, and heterogeneity remained high (see supplemental materials, Table S4 (https://osf.io/gkt8u/)). Thus, findings showed that influential cases and outliers did not alter the omnibus weighted mean correlation nor account for a significant amount of heterogeneity between studies.

Sensitivity analysis for between- and within-study variance. Table 12 also summarizes the results from the RVE method using different levels of ρ. The sensitivity analysis did not affect the summary correlation, confidence intervals, or τ^2. Overall, findings showed that the omnibus weighted mean correlation was not sensitive to the dependence of within-study effects.

Publication bias. Table 13 summarizes publication status moderation results; 83% of samples were from published ($k = 57$) vs. unpublished studies ($k = 12$). We

conducted meta-regressions with random-effect model using both REML (mixed-effects model) and RVE; consistent with our hypothesis, we found publication status to be a significant moderator, $z = 2.902$, $p = .004$ (mixed-effects model); t (15.1) = 2.3, $p = .036$ (RVE). However, the moderation result was not in the direction we hypothesized; rather, unpublished studies (k = 12) had an increased estimate by $b = .243$. Using a random-effects model, the effect-size estimates for published data was $r = .325$ (95% CI [.28, .37]); for unpublished data, it was $r = .516$ (95% CI [.30, .68]). Second, we created two funnel plots to visualize a) the effect size plotted against sample size, and b) the effect size plotted against standard error. Visually, both funnel plots seemed symmetrical (see Figures 7 & 8). Moreover, the symmetry of the two funnel plots were quantified by the Egger's test, and both tests were not significant (sample size: $z = -1.725$, $p = .085$; standard error: $z = 1.266$, $p = .205$).

Figure 7.
Funnel plot of sample size and correlation coefficient for life-stressor dataset.

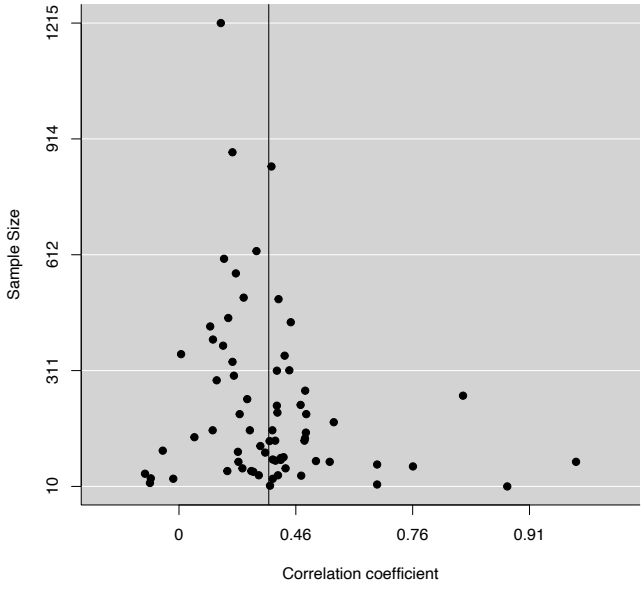
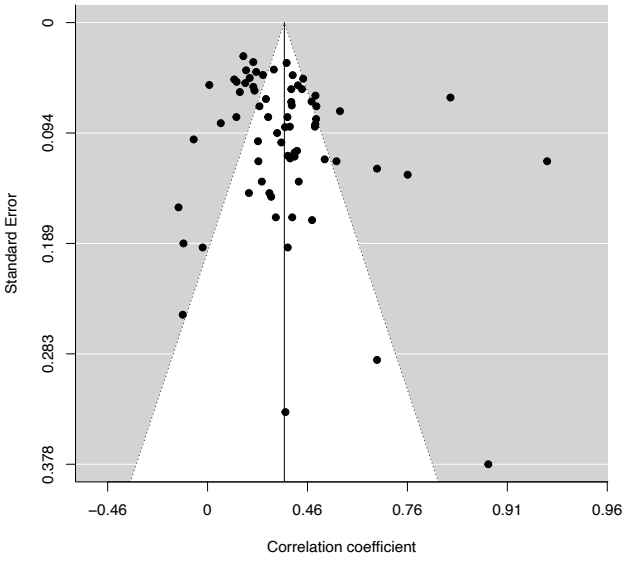


Figure 8.
Funnel plot of standard error and correlation coefficient for life-stressor dataset.



For an additional visual representation of publication bias, forest plots were created and sorted by publication status and sample size; both displayed visual patterns consistent with evidence of bias, with larger confidence intervals having been found with unpublished studies and smaller sample sizes (see supplemental materials on OSF, Figure S9 (https://osf.io/q9uer/) & S10 (https://osf.io/rg6em/)). The sensitivity analysis proposed by Vevea and Wood (2005) showed the adjusted effect-size estimate was r = .329, which is slightly attenuated, but within the confidence interval of the omnibus effect size. Thus, even though the results suggest some evidence of publication bias, the effect-size estimate between life stressors and mental health seems to be robust to publication bias with the current methods used to detect and correct for publication bias.

RQ4: Moderating effect of study-related, demographic-related, and identity-related variables

We examined study-related (i.e., study-design characteristics, stress measure, and mental health measure), demographic-related, and identity-related moderators when there were at least 10 effect estimates per one covariate or per group. We use mixed-effects

model, unless specified otherwise. When results differed between the mixed-effects and RVE approaches, both models are presented. Supplemental Tables S5 and S6 include results from the model that is not presented (https://osf.io/gkt8u/).

Table 13

Study-related moderators for the life stressor dataset

Moderator	k/kc	b/r	SE	z	p	t	df_t	Q(df)	M(sd)/%	Range
Publication Status	69				0.004			8.42(2)		
Published (reference)	57	0.341/0.329	0.035	9.793						
Unpublished	12	0.243/0.526	0.084	2.902	0.004					
Study Design	69				0.908			0.013(2)		
Crosssectional (reference)	61	0.385/0.367	0.036	10.753						
Longitudinal/Intervention	8	-0.013/0.356	0.11	-0.115	0.908					
Type of Sample Population	69				0.047			6.105(2)		
Community (reference)	33	0.304/0.295	0.047	6.522						
Combination	18	0.128/0.407	0.079	1.63	0.103					
Organization	18	0.186/0.454	0.081	2.303	0.021					
Correlation Type	157									
Pearson r (reference)	119	0.344/0.331	0.028			12.239	44.182			
Phi	24	-0.088/0.25	0.048		0.081	-1.844	18.516			
Chi-squared/Means	14	0.036/0.362	0.116		0.764	0.308	11.075			
Life Stressor Measure:										
Mean Life stressor Measure Items	51	0.003	0.001	2.236	0.025				18.79 (19.43)	(1-104)
Mean Life stressor Measure Alpha	17	0.706	0.545	1.295	0.195				0.82 (0.09)	(0.63-0.97)
Life Stressor Type	157									
Abuse (reference)	27	0.438/0.412	0.086			5.085	9.42			
General Life	23	-0.139/0.29	0.102		0.189	-1.362	19.079			
Trauma	29	-0.146/0.284	0.114		0.213	-1.287	19.594			
War trauma	78	-0.111/0.316	0.09		0.239	-1.226	15.018			
Respondent- Life stressor Measure	157									
Clinical interivew (reference)	18	0.288/0.28	0.051			5.674	5.913			
Interview	45	0.073/0.345	0.067		0.306	1.085	9.142			
Other	8	0.064/0.338	0.075		0.417	0.853	8.435			
Parent-report	16	-0.157/0.13	0.087		0.102	-1.812	9.503			
Self-report	70	0.066/0.339	0.068		0.358	0.965	9.465			
Mental Health Measure:										
Mean Mental Health Measure Items	53	0.007	0.004	1.889	0.059				17.44 (10.68)	(1-72)
Mean Mental Health Measure Alpha	35	1.007	0.577	1.745	0.081				0.87 (0.07)	(0.62-0.97)
Mean Mental Health Response Period	37	-0.002	0.001	-1.751	0.08				22.93 (35.83)	(7-180)

k = number of samples; k_c = number of outcomes or correlations. Note: Random-effects models and mixed-effects models display z statistics, while robust variation estimation (RVE) models display t statistics. Z values are based upon the population mean while t statistics are based upon the sample mean. As random-effects models and mixed-effects model estimate the population means, those models use z values.

Study-design characteristics. Table 13 presents study-design characteristic moderators. In terms of study design, most studies used a cross-sectional design (k = 61), seven used a longitudinal design, and one used an intervention design. We combined longitudinal and intervention design studies so that there were nearly ten samples per category, however, the analysis was still slightly underpowered. Study design did not explain the between-variance in the effect-size estimate (see Table 13). We also examined to see if the type of sample population was a moderator of the association between life stressors and mental health (see Table 13). The majority of samples were community-based samples (k = 33), followed by official organization (k = 18), clinic (k = 8), school (k = 5), and a combination of samples (k = 5). We grouped school, clinic, and combination samples together so that there were at least 10 samples in each category. Using mixed-effects modeling, the type of sample population was a significant moderator of the association between life stressors and mental health, $Q_{between}$ (2) = 6.105, p = .047, whereas community samples, r = .295, p <.001, had a significantly lower estimate compared to samples recruited from official organizations, r = .454 (z = 2.303, p = .021). Using RVE meta-regression with the nonaggregate data—which allowed for more categories—the type of sample population also was a significant moderator (see supplemental materials, Table S5 (https://osf.io/gkt8u/)). Samples from schools, r = .497, p = .045 (t (4.95) = 2.66, p = .045), resulted in a stronger estimate. Samples from organizations were approaching significance, r = .378, p = .06 (t (29.46) = 1.954, p = .06), when compared to community samples, r = .263, p < .001 (t (30.01) = 8.90, p < .001; see supplemental materials, Table S5 (https://osf.io/gkt8u/)). Although the p-values varied slightly between models, the beta estimates were nearly the same.

We also examined when correlation type was a moderator (see Table 13). The majority of samples included a Pearson correlation (k = 46); however, we did calculate a Pearson correlation for some of the samples from various statistics: chi-squared (k = 1), means (k = 2), and phi correlation (k = 13). Since three samples included a combination of correlation types, we used RVE to test when correlation type was a significant moderator. We grouped chi-squared and mean-calculated correlations (k_c = 14), and correlation type was not a significant moderator (see Table 13 and supplemental materials on OSF, Table S5 (https://osf.io/gkt8u/)). The evidence suggests study design and correlation type were not significant moderators, while the type of sample population explains some of the between-study variance in the omnibus estimate.

Stress measure. Type of life stressor was examined using RVE meta-regression approach, as 13 samples included correlations for multiple stressors. Out of the 157 correlations, the majority included a measure of war trauma (k_c[19] = 78), followed by general life stress (k_c = 23), abuse (k_c = 22), trauma (k_c = 17), migration trauma (k_c =7), sexual trauma (k_c =5), and bullying (k_c = 5). We re-grouped the stressors to form categories of at least 10 stressors: abuse (abuse and bullying; k_c = 27), general life stress (k_c = 23), trauma (trauma, sexual trauma, and migration trauma; k_c = 29), and war trauma (k_c =78). Inconsistent with our hypothesis, life-stressor type was not a significant moderator of the estimate between life stressors and negative mental health (see Table 13). All life-stressor types were significantly associated with negative mental-health outcomes (see Table 14).

[19] k_c = number of outcomes or correlations

Table 14

Life stressor type univariate analyses using RVE

	k	r	95% CI	kc	t.value	p	Tau.squared	I.squared
Abuse	12	0.401	[0.245, 0.536]	27	5.357	<.001	0.084	94.169
General life stress	14	0.271	[0.181, 0.356]	23	6.361	<.001	0.018	79.091
Trauma	17	0.267	[0.13, 0.394]	29	4.081	0.001	0.041	86.545
War trauma	39	0.307	[0.258, 0.354]	78	12.173	<.001	0.022	83.302

k = number of samples; k_c = number of outcomes or correlations

We also examined life-stressor measure quality-related moderators, which included: number of items, the measure's internal reliability or Cronbach's alpha, and measure respondent (see Table 13). The average number of life-stressor measure items was 18.79 (*SD* = 19.43; range = 1–104; k = 51). Aligned with our hypothesis, number of items was a significant moderator, $z = 2.236$, $p = .025$, with a one-item increase above the mean number of items, resulting in a $r = .003$ increase from $r = .344$ (Table 13). However, in terms of *p*-value significance, this result did not replicate using RVE meta-regression, although the beta estimate was the same (see supplemental Table S6 (https://osf.io/gkt8u/)). The average Cronbach's alpha or measure internal reliability was 0.82 (*SD* = 0.09; range = 0.63–0.97; k = 17) and did not explain the between-study variance in the estimate between life stressors and mental health, $z = 1.295$, $p = .195$. Measure respondent was examined using RVE meta-regression approach, as one sample included life-stress reports from multiple respondents. Out of the 157 correlations, the majority included a self-report measure ($k_c = 70$), followed by an interview ($k_c = 45$), then, clinical interview with professional ($k_c = 18$), parent-report ($k_c = 16$), medical record ($k_c = 4$), and a combination of reporters ($k_c = 4$). Due to the numbers per group, we grouped together medical record and combination of reporters and found measure respondent did not explain between-sample variation in effect size (see Table 13). Based upon this data, the results were mixed as it relates to our hypotheses for stress measure

moderators; aligned with our hypothesis, increased number of measure items significantly explained some between-study variance between life stressors and mental health, but other life-stressor measure quality-related variables did not.

Mental health measure. We examined mental-health measure quality-related moderators[20], which included: number of items, the measure's internal reliability or Cronbach's alpha, measure response time period, and respondent (Table 13). The average number of mental-health measure items was 17.44 (*SD* = 10.68; range = 1–72; k = 53). Number of mental-health measure items was a not a significant moderator, $z = 1.889$, $p = .059$. The average Cronbach's alpha or measure internal reliability was 0.87 (*SD* = 0.07; range = 0.62–0.97; k = 35) and was not a significant moderator of the association between discrimination and mental health, $z = 1.745$, $p = .081$. The average response time period was 23 days (SD=35.83, range = 7-180) and was not a significant moderator. Measure respondent was examined using RVE meta-regression approach, as four samples included mental-health outcomes from multiple respondents. Out of the 157 correlations, the majority included a self-report measure ($k_c = 74$), followed by an interview ($k_c = 44$), then, clinical interview ($k_c = 16$), parent-report ($k_c = 16$), medical record ($k_c = 3$), and combination of reports ($k_c = 4$). Due to the numbers per group, we grouped together medical records and combination reports, and we found that measure respondent did not account for between-study variance in the association between life stressors and negative mental health (see Table 13). Based upon this data, no mental-health measure quality significantly moderated the association between life stressors and negative mental health.

[20] Mental-health measure moderators are unregistered exploratory analysis.

Three samples included both correlations for positive and negative mental health. Out of the 157 correlations, the majority included a measure of negative mental health (k_c = 152), and only five measured positive mental health (k_c = 5). Due the low number of positive mental health correlations, we were unable to conduct a meta-regression analysis to test our hypothesis. Table 15 summarizes the RVE model estimates for life stressors with negative mental-health outcomes and positive health outcomes[21].

Thirty-two samples included correlations for multiple mental-health outcomes. Out of the 157 correlations, the breakdown of mental-health outcomes was as follows: anxiety (k_c = 8), depression (k_c = 40), externalizing (k_c = 5), internalizing (k_c = 7), psychological distress (k_c = 31), PTSD (k_c = 49), somatization (k_c = 9), and well-being (k_c = 8). Because many outcomes included fewer than 10 correlations, we were unable to perform a moderation analysis. However, we conducted subgroup analyses using RVE for the mental-health outcomes with nearly 10 or more correlations (see Table 15). These results show that life stress was most strongly related to somatization, r = .412 (95% CI [.25, .55]), and PTSD, r = .360 (95% CI [.28, .43]), followed by, depression, r = .331 (95% CI [.24, .42]), psychological distress, r = .299 (95% CI [.21, .38]), anxiety, r = .264 (95% CI [.09, .42]), internalizing, r = .234 (95% CI [.07, .38]), and well-being, r = -.153 (95% CI [-.28, -.02]).

[21] Analyses with fewer than 10 effect size per group are exploratory.

Table 15.

Mental health univariate analyses using RVE for the life stressor dataset

	r	95% CI	kc	k	t.value	p	Tau.squared	I.squared
Negative Mental Health	0.322	[0.275, 0.368]	152	68	12.831	<.001	0.033	87.54
Positive Mental Health	-0.188	[-0.411, 0.055]	5	4	-3.431	0.079	0.01	50.005
Depression	0.331	[0.241, 0.416]	40	28	7.215	<.001	0.042	87.026
Anxiety	0.264	[0.093, 0.42]	8	7	3.995	0.012	0.015	65.62
Internalizing	0.234	[0.071, 0.384]	7	6	3.78	0.015	0.013	75.232
Externalizing	0.146	[-0.197, 0.457]	5	4	1.375	0.266	0.026	80.281
Psychological distress	0.299	[0.214, 0.379]	31	20	7.093	<.001	0.024	87.481
PTSD	0.360	[0.283, 0.431]	49	34	9.017	<.001	0.043	88.951
Somatization	0.412	[0.252, 0.551]	9	7	6.059	0.001	0.028	85.828
Wellbeing	-0.153	[-0.279, -0.022]	8	6	-3.48	0.033	0.01	66.126

k = number of samples; k_c = number of outcomes or correlations.

Note: Estimates are exploratory when kc/k < 10.

Demographic characteristics. Table 16 summarize the meta-regressions for sociocultural characteristics. Sample mean age was tested as a continuous moderator in the mixed-effects meta-regression model and did not explain between-sample variation in effect size (z = .393, p = .694). Percent women (M = 50% women; SD = 27.83) was also tested as a continuous moderator and—inconsistent with our hypothesis—it did not explain between-sample variation in effect size (z = 1.611, p = .107). In terms of education, 15 percent of the sample had a bachelor's degree or higher; however, education data was only extracted from 30 samples. Where education was tested as a continuous moderator, it proved not to be a significant moderator of the association between life stressors and mental health (z = .564, p = .573).

Table 16

Demographic moderators for the life stressor dataset

Moderator	k/kc	b/r	SE	z	p	t	df_t	Q(df)	M(sd)/%	Range
Mean Age	68	0.001	0.003	0.393	0.694				29.59 (11.82)	(2.8-48.75)
% Female	69	0.002	0.001	1.611	0.107				50.17 (27.83)	(0-100)
% Bachelors+	30	0.002	0.003	0.564	0.573				14.53 (21.26)	(0-90.2)
% Born in Western country	19	-0.003	0.002	-1.465	0.143				27.62 (37.57)	(0-100)
% First generation	18	-0.002	0.001	-1.251	0.211				80.27 (31.68)	(0-100)
% Refugee	38	-0.002	0.002	-1.136	0.256				93.21 (18.22)	(25.7-100)
Continent	69				0.051			5.952(2)		
Europe (reference)	29	0.291/0.283	0.05	5.815						
North America	39	0.162/0.425	0.067	2.428	0.015					
Australia/New Zealand	1	0.156/0.42	0.278	0.563	0.574					
Ethnicity (80+)	131									
Middle Eastern (reference)	66	0.26/0.254	0.039			6.646	26.613			
Somali	46	0.141/0.381	0.053		0.012	2.689	27.191			
Bosnian Peninsula	10	0.158/0.395	0.073		0.122	2.149	2.95			
Other	9	0.023/0.275	0.069		0.749	0.336	5.607			
Ethnicity	157									
Middle Eastern (reference)	86	0.294/0.286	0.039			7.599	37.568			
Bosnian Peninsula	10	0.128/0.398	0.075		0.191	1.711	2.836			
Somali	50	0.095/0.37	0.05		0.070	1.882	29.641			
Other	11	0.065/0.344	0.08		0.444	0.803	8.308			
Muslim sample type	69				0.410			0.679(2)		
Majority Muslim country (reference)	45	0.364/0.348	0.042	8.628						
Muslim	24	0.059/0.399	0.071	0.824	0.410					

k = number of samples; k_c = number of outcomes or correlations. Note: Random-effects models and mixed-effects models display z statistics, while robust variation estimation (RVE) models display t statistics. Z values are based upon the population mean while t statistics are based upon the sample mean. As random-effects models and mixed-effects model estimate the population means, those models use z values.

The majority of samples were from North America (k = 39; k_c = 91), followed by Europe (k = 29; k_c = 65) and Australia (k = 1; k_c = 1). Continent was tested as categorical moderator and nearly explained between-study variance, $Q_{between}$ (2) = 5.952, p = .051. The results showed that samples living in North America (r = .425) had a significantly larger association by b = .162, compared to samples living in Europe (r = .283; see Table 16)[22]. Because there were not 10 samples per country, we were unable to perform moderation analysis. Instead, we present the random-effect model estimates between life stressors and negative mental health for each country (see Table 17). It is worth noting that only the estimate for the United States is a priori analysis, as the estimates for countries with less than 10 samples are exploratory analyses. Using the random-effects model approach, we found a statistically significant summary effect between life stressors and negative mental health for samples in the United States (k = 36), r = .440 (95% CI [.35, .52]).

Table 17.

Country univariate analyses using RVE for the life stressor dataset

Country	k	r	95% CI	z	p	Tau squared	I squared
Australia	1	0.42	[0.228-0.58]	4.079	<.001	0	0
Belgium	1	0.144	[0.046-0.24]	2.867	0.004	0	0
Canada	3	0.161	[-0.134-0.43]	1.069	0.285	0.046	75.403
Germany	4	0.35	[-0.165-0.715]	1.345	0.179	0.255	93.619
Netherlands	3	0.183	[0.103-0.26]	4.449	<.001	0	0
Norway	4	0.17	[0.122-0.217]	6.866	<.001	0	0.014
Sweden	7	0.321	[0.169-0.458]	4.029	<.001	0.037	88.769
United Kingdom	8	0.364	[0.293-0.432]	9.373	<.001	0.007	58.515
USA	36	0.44	[0.354-0.519]	9.026	<.001	0.086	94.314

In order to test when ethnicity or country of origin was a significant moderator, we conducted our analyses with the entire life-stressor dataset and also only with samples

[22] We conducted unregistered exploratory moderation analysis between samples from North American and Europe and found continent was a significant moderator, $Q_{between}$ (1) = 5.8971, p = .02.

that were ethnically homogenous (had 80% or more of the same ethnic group; k = 54, k_c = 131). Both approaches resulted in underpowered meta-regression analyses with fewer than 10 samples per ethnic group (see Table 16 and supplemental materials on OSF, Table S5 (https://osf.io/gkt8u/)). We combined ethnic groups so that there were at least 10 samples per group, and we found ethnicity to explain between-study variance in the effect-size estimate, whereas the Somali sample (r = .370–.381) had a stronger effect, b = .095–.141, than the Middle Eastern sample (r = .254–.286; see Table 16 and TableS5). The beta estimation for the Somali sample was similar across analytic approaches, but the p-value level of significance was not consistent (see Table 16 and supplemental materials, Table S5 (https://osf.io/gkt8u/)). Overall, results suggest that ethnicity may explain some between-study variance in the meta-analytic estimate, specifically where the effect between life stressors and mental health is stronger for the Somali sample. We also tested to see when Muslim sample type (Muslim vs majority-Muslim country) was a significant moderator (Table 16). There were 24 Muslim samples and 45 majority-Muslim-country samples. Muslim sample type was not a significant moderator, z = .824, p = .41.

Table 16 summarizes the results of immigration status. Immigration status was examined through percent born in a western country (28%; k = 19), percent first-generation immigrants (80%; k = 18), and percent refugee (93%; k = 38). Percent born in a Western country tested as a continuous moderator did not explain between-study variance in effect size (z = -1.465, p = .143), nor did percent first-generation immigrant explain between-study variance in effect size (z = -1.251, p = .211). Percent refugee was tested as a continuous moderator and did not explain the between-study variance in effect

size. Notably, there was very little variation among the studies that presented percent refugee status; from the 38 samples with refugee status information, most included a 100% refugee sample population. These results suggest that none of the indicators of immigration status explained between-study variance in the effect-size estimate; however, there was little variation among the studies that presented immigration status information.

Identity. Six unique samples included identity variables in the life stressors dataset (see Table 10 to identify the six samples). Identity variables included: ethnic identity (k = 3), national identity (k = 2), religious identity (k = 4), and identity integration (k = 2). There were not enough samples with identity variables to conduct meta-regression analyses. The qualitative review of the six unique samples revealed that none of the studies included analyses to determine if identity moderated the association between life stressors and mental health; therefore, no conclusions can be made regarding the moderating effect of identity.

Summary of meta-analytic results of the association between life stressors and mental health

The summary correlation between life stressors and mental health for Muslims living in western countries was consistent with our hypothesis—it was positive and falls within the moderate to large range. There was evidence of publication bias where unpublished studies were found to have a stronger and positive effect-size estimate compared to published studies. However, the meta-analytic estimate appeared to be robust for outliers, dependence within-study effects, and publication bias with the methods used to detect and correct for biases. There was high heterogeneity within the

sample, but it appeared that the heterogeneity was due to true variance within the population. In terms of demographic characteristics, these meta-analytic results were from a majority Middle Eastern, majority North American, lower-educated, and young Muslim population.

All moderation analyses outlined in our research questions were challenging due to limiting information available in articles and small samples per moderator. The meta-analytic estimate appeared to be unaffected by *most* study-, demographic-, and identity-related moderators tested. However, a few study- and demographic-related variables did explain some between-study variance in the meta-analytic estimate. Interestingly, beta estimates of moderators were consistent across analyses models, but *p*-value significance varied.

Briefly, evidence suggested that the type of sample population was a significant moderator with a) samples from organizations and b) samples from schools, as these have stronger associations between life stressors and mental health compared to samples from the community. Additionally, some evidence suggested that number of life-stressor measure items was a significant moderator; specifically, as the number of life-stressor measure items increased, the association between life stressors and mental health was stronger. In terms of demographic-related variables, ethnicity appeared to be significant moderator of the association between life stressors and negative mental-health outcomes, specifically where Somalis were found to have a stronger association between life stressors and negative mental health compared to other ethnic groups. Additionally, some evidence suggests that continent explained between-study variance, specifically where

samples in North America were found to have a stronger association between life stressors and mental-health outcomes compared to samples from Europe.

Discrimination and Life Stressors

Fourteen samples measured both experiences of discrimination and life stressors. The analytic subset that included both measures of discrimination and life stressors includes 11 unique samples[23], with 75 correlations and 3,416 individuals. The 11 unique samples had a mean sample size of 310 (range = 30–1,215) and represented individuals living in five different Western countries (see Table 18). The mean age of participants was 31 years old (range = 10–49 years old), with 47% women and a (collapsed) majority ethnic breakdown of 64% Middle Eastern, 27% Somali, and 9% North African (see Table 18 for a detailed ethnicity breakdown).

[23] 14 unique samples are included in the qualitative review as there were three samples that we could not extract correlation data for both stressors.

Table 18.
Characteristics for the subset with both measures of discrimination and life stressors

Sample characteristics	Details
k	11
kc	75
N (range)	310 (30-1215)
Age (M(sd))	30.73 (12.45)
% Female (sd)	46.82 (23.69)
Data from published sources (%)	100
Country	
- Canada	k=1
- Netherlands	k=1
- Sweden	k=2
- United Kingdom	k=1
- USA	k=6
Majority Ethnicity (detailed)	
- Afghani	k=1
- Arab	k=1
- Iraqi	k=2
- Middle Eastern	k=1
- Moroccan	k=1
- Palestinian	k=1
- Somali	k=3
- Syrian	k=1
Majority Ethnicity	
- Middle Eastern	k=7 (63.6%)
- North African	k=1 (9.1%)
- Somali	k=3 (27.3%)

k = number of samples; kc = number of outcomes or correlations.

RQ5: Effect of stress type (discrimination vs. life stressors) on mental-health outcomes

Using mixed-effects modeling with the full combined dataset (k = 141, k_c = 295), stress type did explain the between-study variance of the association between stressor (discrimination and life stressors) and mental health, $Q_{between}$ (2) = 14.08, p <.001, where both stress types were significantly associated with mental health. However, the association was stronger for life stressors and mental-health outcomes (see Table 19) than for discrimination and mental-health outcomes. We tested the equivalency between the

discrimination-mental-health effect-size estimate and the life stressors-mental health effect-size estimate using the TOST-*p* and KTOST-*p* test using an SESOI of r = .15, which is a small effect size difference (Counsell & Cribbie, 2015).[24] Notably, Counsell and Cribbie (2015) found that TOST-*p* and KTOST-*p* are conservative tests at lower samples sizes, wherein their simulations, "the null hypothesis was never rejected for sample sizes below 250 per group" (pp. 299). Therefore, we used another equivalence test for independent correlation coefficients for further verification (AH-*p*), which has proven to be more reliable for smaller sample sizes; results confirmed that we could not reject the hypothesis that the coefficients were not equivalent, $p = .42$ (Counsell & Cribbie, 2015). Inconsistent with our original hypothesis, results of the three tests consistently found the discrimination-mental health effect-size estimate and life stressor-mental health effect-size estimate (including meta-analytic estimates without outliers or publication bias corrected) were not equivalent.

Table 19.

Moderation of stress type (discrimination vs. life stressors) using full sample

Categorical Moderator	k/kc	b/r	SE	z	p	t	df_t	Q(df)
Stress Type (REM)	141				<.001			14.075(2)
Discrimination (reference)	72	0.239/0.234	0.026	9.077				
Life Stressor	69	0.144/0.365	0.038	3.752	<.001			
Stress Type (RVE)	295							
Discrimination (reference)	138	0.21/0.207	0.017			12.354	66.191	
Life Stressor	157	0.125/0.323	0.031		<.001	4.093	120.235	

k = number of samples; k_c = number of outcomes or correlations. Note: Random-effects models and mixed-effects models display z statistics, while robust variation estimation (RVE) models display t statistics. Z values are based upon the population mean while t statistics are based upon the sample mean. As random-effects models and mixed-effects model estimate the population means, those models use z values.

[24] We did not use the TOST package in R (Lakens, 2017) and instead wrote our own function that can be found in our R file.

Conversely, using mixed-effects modeling with the subset of data that included both discrimination and life stressors (k = 11, k_c = 75), stress type did not moderate nor explain the between-study variance of the association between stressor (discrimination and life stressors) and mental health, $Q_{between}$ (1) = .05, p = .822 (see Table 20). We tested the equivalency between a) the discrimination-mental health effect-size estimate (r = .271 - .301) and b) the life stressors-mental health effect-size estimate (r = .273 - .319), which were derived from the subset of data, and we found mixed results. Using the TOST-p and KTOST-p test with an SESOI of r = .15, the estimates were not found to be statistically equivalent (Counsell & Cribbie, 2015).[25] Using the equivalence test that has proven to be more reliable for smaller sample sizes (AH-p), the coefficients were statistically equivalent, p = .006 - .032 (Counsell & Cribbie, 2015). Results of the three tests provided varied results, which appears to be related to the very small sample size. Therefore, it appears the discrimination-mental-health effect-size estimate and life stressors-mental health effect-size estimate may be equivalent when examining the subset of data that measures both stressors.

[25] We did not use equivalence tests comparing dependent correlations outlined in Counsell and Cribbie (2015) as we were only able to extract the correlations between discrimination and life stressors from six samples, which is under our predetermined minimum of 10 studies needed to conduct a meta-analytic estimate.

Table 20.

Moderation of stress type (discrimination vs. life stressors) using subset with both measures

Categorical Moderator	k/kc	b/r	SE	z	p	t	df_t	Q(df)
Stress Type (REM)	11				0.822			0.05(2)
Discrimination (reference)	11	0.33/0.301	0.063	5.204				
Life Stressor	11	-0.02/0.319	0.09	-0.225	0.822			
Stress Type (RVE)	75							
Discrimination (reference)	37	0.278/0.271	0.058			4.8	8.3	
Life Stressor	38	0.003/0.273	0.05		0.961	0.05	9.72	

k = number of samples; k_c = number of outcomes or correlations. Note: Random-effects models and mixed-effects models display z statistics, while robust variation estimation (RVE) models display t statistics. Z values are based upon the population mean while t statistics are based upon the sample mean. As random-effects models and mixed-effects model estimate the population means, those models use z values.

In summary, the meta-analytic correlations of both a) discrimination and mental health and b) life stressors and mental health were significantly and positively associated. Results are mixed in terms of the moderation effect of stress type and equivalence of effect-size estimates. Inconsistent with our hypothesis, analyses that included the entire combined dataset show that stress type is a significant moderator, and that the effect-size estimates for the two datasets are not equivalent, specifically where the association between life stressors and mental health is stronger. However, consistent with our hypothesis, results from the subset of data—that includes only the parallel samples that measure both discrimination and life stressors—suggest that stress type was not a significant moderator and that the effect-size estimates appear equivalent, although noting the small sample. These findings suggest that other characteristic differences between the discrimination and life-stressor datasets may be contributing to the difference in meta-analytic correlations.

RQ6: Unique contribution of discrimination and life stressors to the association between stressors and mental health

From the 11 samples included in the analytic subset, only six samples presented partial correlations of the associations between (1) discrimination and mental health, accounting for the effect of life stressors and/or (2) life stressors and mental health, accounting for the effect of discrimination. We conducted exploratory analyses using RVE with the five samples and found a statistically significant summary effect of the partial correlation between discrimination and mental health, adjusting for the effect of life stressors, r = .19 (95% CI [.02, .35]), t (4.98) = 2.8, p =.038. We also found a statistically significant summary effect of the partial correlation between life stressors and mental health, adjusting for the effect of discrimination, r = .22 (95% CI [.0004, .41]), t (3.99) = 2.78, p =.0497 (see supplemental Table S9 (https://osf.io/gkt8u/)). Thus, consistent with our hypothesis, these underpowered analyses provide initial support that discrimination and life stressors uniquely contribute to mental-health outcomes.

Qualitative review. Due to limited quantitative data, we conducted a qualitative review of the 14 samples that included both discrimination and life-stressor measures to examine the unique contribution of discrimination and life stressors as they relate to mental-health outcomes. See Appendix C for qualitative review of eight samples that did not include partial correlations; no consensus was found among the eight samples regarding the association between discrimination and life stressors with mental-health outcomes.

Partial correlations of discrimination and life stressors. Briefly, from the six samples that included partial correlations, data from four samples revealed that both discrimination and life stressors were significantly associated with mental health (Alemi & Stempel, 2018; Ellis et al., 2008; Kira et al., 2010; Tinghög et al., 2017). One sample

showed that only life stressors remained significantly associated with mental health (Jorden et al., 2009), and one sample revealed varied results for both discrimination and life stressors, depending on the type of mental-health outcome measured (Abuelezam et al., 2018).

Data from four samples showed that the adjusted discrimination and life-stressors estimates were positively and moderately associated with mental health; the adjusted life-stressor estimate appeared to be the stronger predictor. In data provided by a sample of Afghani refugees resettled in the United States, a model including other resettlement factors revealed that both life stressors and discrimination predicted negative mental-health outcomes. Here, the partial correlation of life stressors was the stronger predictor (ß = 0.30, $p > .05$) but did not maintain significance, and experiences of discrimination (ß = 0.26, $p < .001$) was the second strongest predictor (Alemi & Stempel, 2018). Similarly, in a sample of Iraqi refugees living in Michigan, discrimination (ß = 0.22, p = NR) and previous trauma (ß = 0.46, p = NR) were both associated with PTSD, but previous trauma was the stronger predictor (Kira et al., 2010). Moreover, in a sample of Somali teens, the adjusted estimates of discrimination and life stressors remained significant predictors of PTSD symptoms (discrimination, ß = 0.246, $p < .01$; life stressors, ß = 0.32, $p < .01$) and depressive symptoms (discrimination, ß = 0. 238, $p < .01$; life stressors, ß = 0.199, $p < .05$), even when accounting for other factors (Ellis et al., 2008). Although the study included only the adjusted correlation for discrimination, in a sample of Syrian refugees living in Sweden, discrimination was strongly associated with low well-being (ß = 0.43, p = NR), internalizing symptoms (ß = 0.41, p = NR[26]), and PTSD (ß = 0.40, p = NR),

[26] Statistics calculated from odds ratios.

specifically when adjusted for various covariates, including traumatic events (Tinghög et al., 2017). The non-adjusted association between life stressors and mental-health outcomes varied: low well-being (r = 0.06, *p* = NR), internalizing symptoms (r = 0.18, *p* = NR), and PTSD (r = 0.20, *p* = NR; Tinghög et al., 2017).

Conversely, in a sample of Somali adults in Canada, the adjusted model found only various traumatic experiences significantly predicted depression (individual, ß = 0.14, *p* <.05; familiar assault, ß = .20, *p* <.01) and trauma symptoms (threat to other, ß = 0.20, *p* <.001; Jorden et al., 2009). And adjusted discrimination did not significantly predict depression (ß = .08, *p*> .05) or trauma symptoms (ß = 0.14, *p* >.05; Jorden et al., 2009).

In one sample, the strength and direction of adjusted correlations differed based upon type of mental-health outcome for both discrimination and life stressors. In a sample of Arabs living in Michigan, life stressors appeared more consistently related to depression than discrimination (discrimination, ß = 0.07, *p* = NR[27]; life stressors, ß = 0.11–0.26, *p* = NR). Discrimination—compared to life stressors—appeared more consistently related to psychological distress (discrimination, ß = 0.26, *p* = NR; life stressors, ß = -0.12–0.29, *p* = NR; Abuelezam et al., 2018).

Interestingly, one study included results of the interaction of discrimination and life stressors in predicting mental-health outcomes. Life stressors was a significant moderator of the association between discrimination and psychological distress, specifically where increased life stressors exacerbated the effect of discrimination on psychological distress (interaction, ß = 0.403, *p* <.05; Alemi & Stempel, 2018).

[27] Statistics calculated from odds ratios.

Summary of meta-analytic results of stressor type

In summary, exploratory quantitative analyses and the qualitative review suggest that discrimination and life stressors are uniquely associated with mental-health outcomes. However, results are varied across samples. Interestingly, *p*-value significance and strength of the correlation appear to vary greatly within the 14 samples. It appears that the variation may be due to study-related characteristics such as sample size, type of life stressor, type of discrimination, or mental-health outcome. Overall, however, consistent with our hypothesis, there is initial support that discrimination and life stressors uniquely contribute to mental-health outcomes.

Discussion

This comprehensive preregistered meta-analysis and qualitative review, including 130 unique samples and 27,725 individuals, examined the associations between both (a) discrimination and negative mental-health outcomes and (b) life stressors and negative mental-health outcomes. Given the widespread context of Islamophobia in contemporary society, as evidenced by anti-Muslim stereotypes and propaganda across North America, Europe, New Zealand, and Australia, we addressed a pressing topic of how this context of Islamophobia impacts the psychological well-being of Muslims living in Western countries. Notably, this is also the first meta-analysis that focuses on Muslims living in Western countries, and furthermore, that examines the impact of both stressors—discrimination and life stressors—on mental-health outcomes. Using both meta-analytic and qualitative review approaches, we examined the associations between two types of stressors and mental-health outcomes and calculated omnibus effect-size estimates that have shown to be robust to outliers, within-study dependence, and publication bias.

Overall, our meta-analyses for Muslims living in Western countries found (1) a statistically significant summary correlation between discrimination and mental health that is in the small to moderate range, and (2) a statistically significant summary correlation between life stressors and mental health is in the moderate to large range. In terms of strength of predictors, results are varied, and although it appears that life stressors are statistically stronger predictors of mental-health outcomes, there is mixed evidence that warrants future exploration. It is important to highlight, all measures were overwhelmingly based on self-report, and therefore, the shared method variance likely led to an inflated correlation.

In addition to the omnibus effect, we identified study-related and individual factors that did and did not explain some between-study heterogeneity. Our detailed sample-level tables summarize the variety of stressor- and mental-health measures and their psychometric properties that are used within the literature, which highlights gaps and shortcomings within individual research studies. In addition, we examine the partial associations of discrimination and life stressors on mental health, demonstrating that both are important and are unique predictors of Muslim mental health. Finally, our qualitative review of identity moderators contributes to the literature by summarizing the variation in the literature, and therefore, suggesting future directions.

Advancing the Conceptualization of a Racialized Group and Widespread Islamophobia

A major contribution of this work is that our results support the assumption that Islamophobia is widespread, and that Muslim and "Muslim-like" groups appear to have similar experiences. Therefore, supporting previous research (e.g., Kaya, 2014; Selod,

2015), it seems that Muslim and Muslim-like groups have been racialized and may be viewed as a homogenous group—or at least have a shared racialized experience. Muslim and Muslim-like ethnic groups both were found to have similar associations between stressors and mental-health outcomes. Moreover, ethnicity was not a significant moderator for the association between discrimination and mental-health outcomes, which suggests that deleterious effects of discrimination occur across ethnic groups, in turn, providing more evidence that Muslims are a racialized group. This provides support that individuals who may be considered to be Muslim—even if they do not religiously identify themselves as such—may also experience the negative impact of Islamophobia. Additionally, we found similar results between discrimination and mental-health outcomes across continents, highlighting that the effects of discrimination on mental health are widespread for Muslims living in Western countries.

However, there is some evidence to suggest that some demographic characteristics exacerbated the association between stress and mental-health outcomes. Specifically, some evidence shows that the association between life stressors and negative mental-health outcomes was stronger for Somali individuals compared to Middle Eastern individuals. This finding may suggest evidence of the compounding effect of being Black and Muslim, with intersecting marginalized identities exacerbating the severity of mental-health outcomes. Moreover, some evidence shows that refugee status is a significant moderator of the association between discrimination and negative mental-health outcomes, again suggesting the compounding effect of refugee status and the experience of discrimination. This aligns with the results of the interaction of discrimination and life stressors in predicting mental-health outcomes, specifically where

life stressors exacerbated the effect of discrimination on psychological distress (Alemi & Stempel, 2018). Additionally, results suggest that the association between life stressors and mental-health outcomes is stronger in North America compared to Europe. Thus, this finding warrants further evaluation, but it may be due to the large number of Muslim refugee samples that have experienced cumulative trauma prior to resettlement in North America; or, it may be related to Muslims' specific experiences living in North America (e.g., compounding effect of discrimination that is not measured, availability of resources).

Theoretical Considerations: Predictors of Negative Mental Health for Muslims

This study contributes to our understanding of important predictors of negative mental health for Muslims living in Western countries. Briefly, the meta-analytic results suggest that discrimination and life stressors are significant and unique predictors of mental-health outcomes, as are experiences of personal and group-based discrimination. Additionally, evidence suggests that Muslims connected to organizations and Muslim youth in schools may be especially at-risk for experiencing the deleterious effects of life stressors. As the studies included in these meta-analyses were overwhelmingly correlational, it limits our ability to confirm explicit causal claims on the relationship between discrimination, life stress and mental-health outcomes. The theoretical considerations outlined follow the assumption that experiences of discrimination and life stressor lead to worse mental health. This assumption is based upon previous longitudinal and experimental studies with other groups of people that show the causal relationship between these factors (CITE). The alternative causal relationship is that individuals with worse mental health are more susceptible to life stress or more aware of

experiences of discrimination (CITE?). However, the alternative causal relationship has not be proven in longitudinal and experimental studies with other populations (CITE). Thus, taken together, although we acknowledge we do not have evidence to make causal claims with the current data, the theoretical considerations make the assumption our results suggest that discrimination and life stress leads to worse mental health.

Mental-Health Outcomes. These extensive meta-analyses include meta-analytic correlations for different mental-health outcomes. Experiences of discrimination and life stress are important contextual factors that can inform the development of psychopathology or our understanding of adaptive and maladaptive functioning (Causadias, 2013). Although we mainly use the umbrella term "mental-health outcomes," we present mental-health outcome specific results in Table 6 and Table 15 that shows a) experiences of discrimination are most strongly associated with increased depression, and b) experiences of life stress are most strongly associated with somatization and PTSD. It appears that these different contexts of stress may impact mental-health outcomes differently.

Mental-health outcome direction was able to be tested only in the discrimination dataset, and it explained some between-study variance. Negative mental-health outcomes (e.g., depression, anxiety) had a stronger association between discrimination and mental health more so than positive mental-health outcomes (e.g., self-esteem, wellbeing); although, both outcomes resulted in a statistically significant outcome.

Discrimination and Life Stressors. An important contribution of this study is that both discrimination and life stressors are significant predictors of negative mental health. Additionally, the evidence from these meta-analyses suggest that discrimination cannot

be subsumed by life stressors; instead, evidence suggests that experiences of discrimination are distinct, unique predictors and should be treated as such. Thus, our results align with Harrell's (2000) model of racism-related stress that equally emphasizes both discrimination and general life stressors in the development of psychopathology. Harrell's (2000) model separates different types of racism-related stressors (e.g., racism-related life events; vicarious racism experiences), and although we did not conduct analyses for all the different types of racism-related stressors, we did find that both individual and group-based discrimination were significantly associated with mental-health outcomes. Another conceptual model, specifically developed to capture the experiences of Americans of Middle Eastern and North African (MENA) descent, uses different terminology, but postulates the interaction of life-stress and discrimination experiences with identity-related factors on mental and physical health (Awad et al., 2019). Additionally, another framework—developed to understand the etiology of mental health outcomes for Black youth—describes culturally-informed Adverse Childhood Experiences (ACEs) that highlights the impact of racism in experiencing traumatic events (Bernard et al., 2020). Our meta-analytic results provide support for these different conceptual models in the development of psychopathology.

Notably, there is considerable variation in how discrimination and life stressors are measured in the literature. We were purposeful in including only measures that were captured by our definitions outlined a priori, and when necessary, we looked at the original measure article cited for further clarification of what was measured. Thus, another contribution of this study is that we did not include measures of discrimination or

life stress that also included a distress rating (e.g., how much the stressor caused you distress), because that would have been conflated with negative mental health.

Personal and Group Discrimination. Furthering the systematic review by Samari and colleagues (2018), results suggest a differential impact between personal and group discrimination on mental health, where personal discrimination is more strongly associated with mental-health outcomes. Importantly, group discrimination still has a significant association with mental-health outcomes. These results align with other meta-analytic results (Lui & Quezada, 2019; Schmitt et al., 2014) which also found that experiences of personal discrimination were more strongly related to mental-health outcomes more so than group-level discrimination. This may be due to the increase in ambiguity from group-based discrimination that allows an individual to rationalize that discriminatory action as not directed personally to them—resulting in utilizing different coping strategies than with personal discrimination.

At-Risk Populations. Results from the life-stressor dataset suggest that the most at-risk Muslim populations for negative mental-health outcomes are those connected to official organizations, compared to community samples. Official organizations tend to serve refugee and at-risk populations, which can help to understand the stronger association between experiences of discrimination and mental-health outcomes for samples from official organizations (e.g., Albdour, 2017; Jorgenson, 2017). Individuals connected to official organizations may be more vulnerable to external stressors, as they do have the same resources as individuals from Western countries (e.g., Awad et al., 2019). Some evidence suggests that Muslim populations in schools may be at risk for experiencing the deleterious psychological effects of stressors, which may indicate that

Muslim youth populations are at risk. The source of this risk among Muslim youth remains unclear, given that results do not reveal age to be a significant moderator of the relation between stressors and mental-health outcomes. However, previous research has found that Muslim youth may have some challenges integrating their Muslim and national identities (e.g., Sirin et al., 2007; Sirin et al., 2008), which may limit coping strategies in the face of stress. Thus, these results highlight that it may be beneficial to target these Muslim populations to provide interventions and resources to support the impact of life stressors.

Explaining Between-Study Variance: Potential Moderators

Gaining insight to explain high heterogeneity or between-study variance was another goal of these meta-analyses, hence the examination of potential moderators. Overall, we did not identify many significant moderators, which may be due to a) not enough variation among the studies, or b) a shortcoming in the choice of potential moderators for which to search. In terms of study-related factors, both meta-analyses provide evidence to suggest that number of items in the stressor measure was a significant moderator, such that, as the number of items increased above the mean number of items, the strength of association between stressor and mental health increased. Therefore, evidence suggests number of items in the stress measure may impact the strength of the association between stressor and mental-health outcome. In terms of demographics, as discussed above, some evidence suggests refugee status (discrimination dataset), ethnicity (life-stressors dataset), and continent (life-stressors dataset) were significant moderators of the relation between stressor and mental-health outcomes.

Publication bias is an important consideration, as it is common for statistically significant results to be overrepresented in the published literature. Interestingly in this data, publication status is a significant moderator only in the life stressors dataset, and the direction of its influence was unexpected. The association between life stressors and mental-health outcomes was actually weaker in published studies—in which powerful effects seem more likely to appear—than in unpublished studies. Still, these unpublished studies included smaller samples (from more distressed populations) from organizations, schools and clinics, and these sample characteristics may have contributed to the stronger empirical associations. However, publication status did not appear to affect the meta-analytic estimate, likely because 83% of the samples were from published studies. Although the analytical methods used to detect and correct for publication bias did not find evidence that the bias impacted the meta-analytic estimates, it is important to note that current analytic techniques for accounting for publication bias may not account for all sources of publication bias. Therefore, it is important to still be aware of potential biases that can occur due to publication bias.

We were only able to investigate identity in the discrimination dataset. The mixed results from the qualitative review parallel the results from a recent meta-analysis on the moderating role of ethnic identity on the association between discrimination and adjustment (Yip et al., 2019). We found that ethnic identity a) sometimes had no association, b) sometimes protected respondents, and c) other times exacerbated the negative effects of discrimination on mental health. Due to variation in measurement and analytic decisions, it was impossible to determine the patterns of which ethnic identity domains were protective vs. exacerbating for Muslims living in Western countries. The

qualitative review of religious identity provided evidence that engaging in religious behaviors and having a stronger religious identity was protective for the deleterious effects of discrimination on mental-health outcomes. The other identity domains included only one to two studies, which makes an overall conclusion difficult as it relates to the moderating role.

Limitations

As is the case with all research, our meta-analyses are not without limitations. With that said, by conducting a Registered Report meta-analyses, we made many a priori analytic decisions, received feedback from reviews, and, thus, our analytic strategy and decisions are transparent and not altered due to the findings/results. Even with that in mind, the results from the current research should be interpreted in the context of the following limitations.

A major limitation of these meta-analyses is that the studies included were overwhelmingly correlational. The lack of longitudinal and experimental studies limits our ability to make causal claims. We may assume that experiences of discrimination and life stress predict negative mental-health outcomes, but due to the correlational data, we cannot confirm the direction of the association.

There were substantial differences in transparent reporting across studies. For example, some studies reported only significant correlations. We did contact authors for bivariate correlations if they were not presented, but very few authors responded with the information. Therefore, we then needed to exclude the study or include only the correlations that were presented in the article.

Additionally, there are differences between studies in the way authors defined constructs and presented information, thus, making synthesis challenging. For example, researchers define generational status inconsistently (1st vs. 1.5 vs. 2nd generation), which made testing that moderator challenging. Moreover, there are differences between how European countries and the United States define the terms migrant vs. immigrant, which makes testing immigrant status challenging.

Similarly, researchers define stress constructs inconsistently. The discrimination measures were extremely varied, such that some authors defined discrimination in terms of religious discrimination, and others defined it in terms of ethnic discrimination; however, it appeared that the two measures—religious discrimination and ethnic discrimination—measured the same construct. It would be helpful for future studies to investigate if individuals differentially experience religious discrimination vs. ethnic discrimination cognitively. The discrimination dataset included measures of Islamophobia that we categorized as group-based discrimination, but teasing apart group-based discrimination and Islamophobia warrants future exploration.

The life-stressor measures also varied. For example, a common measure, TLEQ, only counts trauma that individuals say caused them fear, helplessness, or horror; this may explain the high correlation with PTSD symptoms. It is important to note that there were eight life-stress correlations that included a component of discrimination, which we included in the life-stressor dataset. We did not conduct a full review of every measure of life stress, and there is a possibility that measures of life stress also include items that measure the experience of discrimination.

Notably, all measures in the discrimination dataset were overwhelmingly based on self-report. All the measures in the life stress dataset were also overwhelming based on self-report (75% self-report measure or interview), but there were also parent-report measures, clinical interviews with professionals, and use of medical records. Therefore, the shared method variance likely led to inflated correlations. Both discrimination and life-stress measures also varied in terms of psychometric validity, and some authors used one item to measure experiences of stress, whereas others used validated multi-item measures. As mentioned, this difference appeared to suggest differences in association strength, as number of stress measure items was a significant moderator.

Overall, testing various moderators was also challenging due to inconsistent reporting. This resulted in small samples per group, which led to combining groups and creating unnecessary 'noise' in the data. Notably, to test the moderating role of identity, we used POMP scores to compare across studies. However, it would have been better if we used partial correlations, but that was not an option due to the lack of information in the studies.

These differences and inconsistencies also made it difficult to conduct the quality ratings as originally planned in the Registered Report. The articles provided varied information, and in many cases, the primary research question of an article was not related to the data we extracted. Thus, we modified the quality rating forms so that they were more relevant to our meta-analysis and also to highlight articles that were more or less transparent in terms of reporting.

Lastly, another important limitation of these meta-analyses is that many of the participant samples are from the same geographic areas. Although we excluded duplicate

samples, we did include samples that were from the same geographic area; therefore, it is possible to have participant overlap. Common ethnic and geographic areas include: Somalis in Boston (three studies), Somalis in Minneapolis (three studies), Somalis in New England (two studies); Middle Easterners in Dearborn, Michigan (two studies), Iraqis in Detroit, Michigan (seven studies), and Arabs in Michigan (two studies). Even with these limitations, we believe our meta-analytic results further our understanding of the impact of discrimination and life stressors on mental-health outcomes for Muslims living in Western countries.

Clinical Considerations

Although not the primary focus of the current study, we found that experiences of discrimination and life stress lead to the development of different mental-health outcomes. A body of literature has examined the pathways in which different experiences may impact individuals differently (e.g., Harrell et al., 2011). Our results suggested that experiences of discrimination were more strongly related to depression, while experiences of life stress were more strongly related to somatization and PTSD. The specificity may be useful in deciding what clinical interventions may be useful for an individual experiencing discrimination or increased life stress. For instance, an individual experiencing discrimination and depression may benefit from acceptance and self-esteem interventions; and an individual experiencing life stress and somatization or PTSD may benefit from more mindfulness, mind-body, and cognitive processing of threat intervention. It may even be that certain interventions could be counter-indicated for certain experiences (e.g., the cognitive processing of threat might be counter-indicated for addressing discrimination).

It may be useful to better understand why individuals in North America have a stronger association between life stressors and mental health than individuals in Europe. It may be prudent to evaluate the resources available to Muslims, especially Muslim refugees, in North American compared to Europe. Our results suggested that there may be a gap that can be addressed to help support Muslims experiencing life stress in North America.

Conclusions and Future Directions

These meta-analyses begin to provide clarity that both discrimination and life stressors are distinct predictors of negative mental health. Our results align with Harrell's (2000) model of racism-related stress and other theoretical models (e.g., Awad et al., 2019; Bernard et al., 2020) that emphasize both discrimination or cultural context and general life stressors as unique contributors in the development of psychopathology. Future work examining the impact of discrimination should also consider life stressors, and vice versa.

Our work emphasizes future directions with respect to open science practices, psychometric development, and new content areas. Researchers studying Muslim populations are encouraged to conduct preregistered analyses and to provide transparent reporting to further science and to improve our understanding of the experiences of Muslims in Western countries (see Kathawalla, Silverstein & Syed, 2021 for a tutorial on engaging with open science practices).

It would be beneficial for future studies to focus on methodological approaches that can elucidate our understanding of the relation between stress and mental health. First, longitudinal and experimental studies would provide evidence for causation in the

relation between stress and mental health. It would be beneficial for studies to use non-self-report measures of discrimination, stress, and mental health for more objective measurement of the constructs. Using different methodology may also assist in our understanding of an individual's cognitive experience and interpretation of various events. There is an important need for clear construct definitions for discrimination and life stressors and also related measures for those constructs.

Future work should examine Muslims' experiences in other non-Western countries. The context of Islamophobia is not limited to Western countries, and Muslims are discriminated against across the world. For example, Muslim minority groups in China and India have been—and continue to be—persecuted. To further capture the experiences of Muslims, it would be helpful to also include research published in other languages, such as French and Arabic.

Our results suggest that future work should a) explore the compounding effect of life stressors and discrimination and b) to take more of an intersectional approach to see the compounding effect of: (1) race and Muslim identification or (2) refugee status and Muslim identification. Furthermore, our work suggests that Islamophobia is widespread and supports the notion that Muslim and Muslim-like groups have been racialized and may be seen as a homogenous group. Thus, future intervention work can target Muslims and Muslim-like groups, given that they appear to be similarly (and negatively) impacted by experiences of discrimination and life stressors. We hope this research can support the mental health of Muslims living in Western countries.

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Appendices

Appendix A

Search Terms

Search terms included a combination of *discrimination terms*: (discrimination) or (Islam#[28]phobi*) or (racism) or (prejudice) or (anti#Arab) or (anti#Muslim) or (anti#Islam); combined with OR, *life stressor terms*: (life stress*) or (stressor*) or (childhood adversit*) or (war trauma*) or (refugee*) or (ACEs) or (childhood trauma*) or (traumatic) or (life adversit*) or (abuse) or (natural disaster) or (violence) or (sexual assault); combined with AND*, mental health terms*: (mental health) or (mental illness) or (psychological well#being) or (psychological distress) or (depression) or (anxiety) or (PTSD) or (post#traumatic stress disorder) or (psychological adjustment) or (depressive symptom*) or (self-esteem) or (mood disorders) or (psychopathology) or (DSM diagnos*) or (somatization) or (psychotic experience*) or (well#being) or (psychological outcome*); combined with AND *identifier of Muslim or Muslim-like identity terms*: (Muslim*) or (Moslem*) or (Islam) or (Shia) or (Sunni) or (Arab*) or (Middle East*) or (North Africa*) or (MENA) or (South Asian*) or (Moroccan*) or (Afghani) or (Afghan) or (Tunisian*) or (Iranian*) or (Mauritania*) or (Tajikistani*) or (Yemeni*) or (Iraqi*) or (Jordanian*) or (Mayotte) or (Mahorais) or (Somali*) or (Turkish) or (Azerbaijani*) or (Maldivian*) or (Comorian) or (Nigerian) or (Algerian*) or (Palestinian*) or (Saudi Arabian*) or (Djibouti*) or (Libyan) or (Uzbekistani*) or (Pakistani*) or (Senegalese) or (Gambian*) or (Egyptian*) or (Turkmen) or (Syrian*) or (Malian*) or (Kosovan) or (Bangladeshi*) or (Indonesian*)

[28] The inclusion of '#' represents a wildcard that accounts for alternative spellings or phrasing of the same word.

Appendix B

Muslim-majority countries 2010 sorted by percent population Muslim.

	Country	% Muslim as of 2010
1	Morocco	99.9
2	Afghanistan	99.8
3	Tunisia	99.8
4	Iran	99.7
5	Western Sahara	99.6
6	Mauritania	99.2
7	Tajikistan	99
8	Yemen	99
9	Iraq	98.9
10	Jordan	98.8
11	Mayotte	98.8
12	Somali	98.6
13	Tukey	98.6
14	Azerbaijan	98.4
15	Maldives	98.4
16	Comoros	98.3
17	Niger	98.3
18	Algeria	98.2
19	Palestine	97.5
20	Saudi Arabia	97.1
21	Djibouti	97
22	Libya	96.6
23	Uzbekistan	96.5
24	Pakistan	96.4
25	Senegal	95.9
26	Gambia	95.3
27	Egypt	94.7
28	Turkmenistan	93.4
29	Syria	92.8
30	Mali	92.4
31	Kosovo	91.7
32	Bangladesh	90.4
33	Kyrgyzstan	88.8
34	Indonesia	88.1
35	Oman	87.7

36	Kuwait	86.4
37	Guinea	84.2
38	Albania	82.1
39	Bahrain	81.2
40	Qatar	77.5
41	United Arab Emirates	76
42	Sierra Leone	71.5
43	Sudan	71.4
44	Malaysia	61.4
45	Lebanon	59.7
46	Burkina Faso	58.9
47	Kazakhstan	56.4
48	Chad	55.7
49	Brunei	51.9

Data retrieved from PewResearchCenter (2011). The future of the global Muslim population, projections for 2010-2030. Pew-Templeton Global Religious Futures Project. <https://www.pewresearch.org/wp-content/uploads/sites/7/2011/01/FutureGlobalMuslimPopulation-WebPDF-Feb10.pdf>

Appendix C

Qualitative review of bivariate associations between stress type and mental-health outcomes for subset with measures of both discrimination and life stressors.

There was no consensus found on the association between stress type and mental health after reviewing the eight samples that included measures of discrimination and life stressors (but did not report adjusted correlations). Three samples found that both discrimination and life stressors had positive associations with mental-health outcomes (East et al., 2018; Lindencrona et al., 2008; Gorst-Unsworth & Goldenberg, 1998); three samples found varied associations between stressor and mental-health outcomes (Stevens et al., 2005-1; Stevens et al., 2005-2; Stevens & Thijs, 2018; Tinghög et al., 2010(s1); Hellemans et al., 2015); and, two samples found that neither discrimination nor life stressors were significantly related to mental-health outcomes (Hassouneh & Kulwiki, 2007; Tinghög et al., 2010(s2)).

Three samples that included measures of discrimination and life stressors found that both discrimination and life stressors had small to large positive associations with mental-health outcomes. Both discrimination ($r = 0.67$) and life stressors (peer bullying, $r = 0.67$; traumatic experiences, $r = 0.19$) were associated with worse mental-health outcomes in a sample of Somali youth (East et al., 2018). Additionally, in a sample of Middle Eastern refugees living in Sweden, discrimination and trauma were significantly associated with psychological distress (discrimination, $r = 0.29$; life stressors, $r = 0.31$) and PTSD (discrimination, $r = 0.21$; life stressor, $r = 0.27$; Lindencrona et al., 2008). In a sample of Iraqi refugees living in the United Kingdom, discrimination ($r = 0.32$) and life

stressors (r = 0.30) were associated with depression in a similar way (Gorst-Unsworth & Goldenberg, 1998).

Three samples that included measures of discrimination and life stressors found that both discrimination and life stressors were differentially associated with mental-health outcomes depending on life-stressor type, discrimination type, or mental-health outcome. In a sample of Moroccan youth in the Netherlands, both life stressors and group discrimination were differentially associated with externalizing symptoms (discrimination, r = 0.12–0.22; life stressors, r = 0.12– 0.18) and internalizing symptoms (discrimination, r = -0.07–0.16; life stressors, r = 0.14–0.15; Stevens et al., 2005-1; Stevens et al., 2005-2; Stevens & Thijs, 2018). The large variation for the association of group discrimination and mental-health outcomes is due to two different measures of group discrimination (Stevens et al., 2005-1; Stevens et al., 2005-2; Stevens & Thijs, 2018). Group discrimination was consistently a predictor of externalizing problems but not internalizing problems (Stevens et al., 2005-1; Stevens et al., 2005-2; Stevens & Thijs, 2018). Personal discrimination was significantly associated with externalizing symptoms (r = 0.19–0.24) and differentially associated with internalizing symptoms (r = 0.07–0.15; Stevens et al., 2005-1; Stevens et al., 2005-2; Stevens & Thijs, 2018). Group discrimination was significantly associated with self-esteem (r = -0.11), but personal discrimination was not significantly associated with self-esteem (r = -0.03; Stevens et al., 2005-1; Stevens et al., 2005-2; Stevens & Thijs, 2018). Comparably, in a sample of Turkish adults living in Flanders, physical abuse (r = 0.06) did not have a significant bivariate association, but psychological abuse (r = 0.19, $p < .01$) had a significant bivariate association with worse mental-health outcomes (Hellemans et al., 2015). No

comparable analyses were conducted between racial discrimination and mental-health outcomes (Hellemans et al., 2015). In a sample of Iraqi refugees in Sweden, discrimination was associated with internalizing symptoms with a small effect (r = 0.11, *p* = NR; Tinghög et al., 2010(s1)). The bivariate correlation between life stressors and internalizing symptoms could not be extracted, but people with internalizing disorders reported a significantly higher number of traumatic events compared to those without the mental health diagnosis (3.55 vs. 2.64; Tinghög et al., 2010(s1)).

Two samples found that neither discrimination nor life stressors had bivariate associations with mental health. In a sample of Palestine adults living in the United States, discrimination nor life stressors were significantly associated with depression (discrimination, r = 0.06–0.31; life stressors, r = -0.01) or anxiety (discrimination, r = 0.21; life stressors, r = -0.04; Hassouneh & Kulwiki, 2007). Similarly, in a sample of Iranian refugees in Sweden, the bivariate association between discrimination and internalizing symptoms was near to zero (r = 0.05, *p* = NR; Tinghög et al., 2010(s2)). The bivariate correlation between life stressors and internalizing symptoms could not be extracted, but people with internalizing disorders reported a significantly higher number of traumatic events compared to those without the mental health diagnosis (2.53 vs. 2.17; Tinghög et al., 2010(s2)).