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Review

Intergroup contact and mental health stigma: A comparative effectiveness meta-analysis

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HIGHLIGHTS

- Contact reduces stigma post-intervention in the immediate-, short- and medium-term.
- Contact-based education is no more effective at reducing stigma than contact alone.
- Face-to-face, imagined, video, and presentation contact reduce stigma equivalently.
- Contact with one mental illness reduces stigma against other mental illnesses.
- The effect of contact on long-term behavioral outcomes requires investigation.

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ABSTRACT

Mental health stigma is a substantial problem all over the world. Although many interventions to reduce stigma exist, there is considerable methodological variability, making it difficult for decision-makers to determine what strategies are the most effective and what characteristics make them so. To this end, we conducted a meta-analysis on intergroup contact strategies and examined several potential moderators. We searched 5 databases for published and unpublished studies and retrieved 101 studies from 24 countries that could be included in the analyses. Ninety studies assessed outcomes immediately after the intervention (n = 15,826), 33 in the short-term (n = 3,697), and 7 in the medium-term (n = 842). The effect of contact was significant and small-to-medium in size at all three timepoints, d = −0.384, −0.334, and −0.526, respectively. Intervention effectiveness did not differ between contact with or without an educational component, different contact mediums, or the mental illness of the outgroup member. However, the effect of contact was stronger in non-Western countries and in university students and health professionals compared to community members. These results may inform policy-makers of the most effective and suitable stigma-reduction initiatives to invest in and can guide researchers towards important avenues for future research.

1. Introduction

> "The stigma of mental illness is, in some ways, worse than the illness itself"
>
> beyondblue (2015, p. 11) blueVoices member

The stigma held by members of the public towards mental illness (i.e., public mental health stigma) has a considerable impact on the lives of people with mental illness (PMI). PMI report frequently encountering negative attitudes and experiencing discrimination in many different domains, including from friends and family, in intimate relationships, in employment, housing, and education, and when dealing with law enforcement and health professionals (Lasalvia et al., 2013). As illustrated in the quote above, public mental health stigma can have an even more substantial impact on an individual's wellbeing than the symptoms they experience. Indeed, researchers have linked public stigma to numerous adverse outcomes for PMI, including lower self-esteem, isolation, and suicide (Farrelly et al., 2015). Public stigma is also associated with self-stigma, which occurs when PMI internalize the negative attitudes of the public, resulting in negative self-perceptions (Corrigan, Bink, Schmidt, Jones, & Rüsch, 2016). Self-stigma has further psychological repercussions for PMI such as reducing motivation to pursue goals and worsening depressive symptoms (Corrigan et al., 2016). Compounding the problem, experiences of stigma can delay treatment seeking (Clement et al., 2015) and reduce medication adherence (Thornicroft, 2008).

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The World Health Organization (2013) has recognised the vital importance of reducing mental health stigma in its Comprehensive Mental Health Action Plan 2013-2020, but many organizations call for more research to inform anti-stigma campaigns (e.g., beyondblue, 2015). Clinicians must also stay abreast of advances in this research area. Clinicians can contribute to mental health stigma through the stigmatizing treatment of their patients, but they have also been recognised as powerful agents for change (Schulze, 2007). Historically, many efforts to reduce mental health stigma have been driven by clinicians acting as advocates for PMI. However, clinicians may also hold highly negative attitudes towards PMI, and thus can also benefit from participating in stigma-reduction interventions themselves. Therefore, to guide decision-makers, clinicians, and researchers in the development and implementation of strategies to combat mental health stigma, we compare the effectiveness of interventions aimed at reducing public stigma, hereafter simply *stigma,* specifically those involving contact with PMI.

According to past reviews, establishing contact between participants and PMI is one of the most effective ways of reducing stigma. Contact interventions have their theoretical roots in Allport's (1954) intergroup contact hypothesis, which states that positive interaction between members of different social groups (e.g., White people and Black people) can reduce prejudice and animosity between those groups. In relation to mental health stigma, contact has been found to be more effective than protest strategies that shame individuals for holding stigmatizing views, and more effective than education strategies in which presenters contrast myths about PMI (e.g., violence) with factual information in order to reduce reliance on stereotypes (Corrigan, Morris, Michaels, Rafacz, & Rüsch, 2012; Thornicroft et al., 2016). There is evidence that traditional contact interventions, in which participants experience direct, face-to-face contact with a PMI, significantly reduce mental health stigma (Martinez-Hidalgo, Lorenzo-Sanchez, Garcia, and Regadera, 2018). As well, contemporary indirect contact interventions also appear to be effective. These latter interventions include vicarious contact, where participants observe another individual interacting with a PMI (West and Turner, 2014); imagined contact, where participants visualise themselves interacting with a PMI (Na and Chasteen, 2016); contact via video, in which participants view a PMI on film (Penn, Chamberlin, and Mueser, 2003); and E-contact, where participants converse with a PMI in an online chatroom (in press). Another indirect form of contact, called extended contact, which reminds participants that they have a friend who is friends with a PMI, has also been found to be effective at reducing prejudice against other stigmatized groups (Wright, Aron, McLaughlin-Volpe, & Ropp, 1997) and could be effective at reducing mental health stigma.

In contrast to these *pure* contact interventions, there are also contact-based education strategies which combine contact with education about mental health. Contact-based education interventions provide participants with educational information about mental illness either during contact with a PMI (i.e., the PMI discusses their experience of mental illness) or before or after contact with a PMI (i.e., another individual provides educational information). Unlike education interventions which explicitly correct pervasive myths about PMI in order to reduce stigma, contact-based education interventions educate participants on the symptoms and treatment of mental illness as well as the impact of mental illness and stigma on life goals. Contact-based education is also distinct from pure contact strategies, as the latter do not make mental illness the focus of the interaction between the participants and PMI. However, both interventions can be delivered in direct and indirect formats. Contact-based education interventions have been found to be effective (O'Reilly, Bell, & Chen, 2010), but it is unclear whether the incorporation of educational information makes them more effective than pure contact strategies.

The proliferation of contact strategies to reduce mental health stigma means that researchers and anti-stigma organizations have a smörgåsbord of interventions to choose from. However, there is limited research comparing the effectiveness of these different strategies, making it difficult for decision-makers to accurately assess intervention effectiveness and weigh it against the resources required to implement the intervention. While contact via video and imagined contact interventions may be less costly and easier to distribute to a wide audience, they may also be less effective than face-to-face contact (Giacobbe, Stukas, & Farhall, 2013). When choosing an intervention to invest in, developing an anti-stigma initiative, or advocating for a particular strategy, it is essential the individuals involved are informed by the research literature.

Meta-analyses have consistently found contact with PMI to significantly reduce stigma against PMI, with the effect size ranging from $d = .282$ to $d = .470$ (Corrigan et al., 2012; Griffiths, Carron-Arthur, Parsons, & Reid, 2014; Kolodziej & Johnson, 1996; Morgan, Reavley, Ross, San Too, & Jorm, 2018). However, these reviews have largely failed to distinguish between the contact mediums reviewed above and thus cannot provide answer as to which is the most effective. In the first meta-analysis of the effect of contact on mental health stigma, Corrigan et al. (2012) compared the immediate effect of contact via video and face-to-face contact and found the latter to be *more* effective, but other indirect contact mediums were excluded from the review. Similarly, Mehta et al. (2015) did not distinguish between the different types of indirect contact and only compared their average effect to that of face-to-face contact, finding the latter to be *less* effective 4 weeks after the intervention. Most recently, Morgan et al. (2018) found *no difference* in the effectiveness of face-to-face contact, contact via video, and imagined contact, but only studies involving contact with individuals with schizophrenia, psychosis, or bipolar disorder were included. To date, only Morgan et al. (2018) have differentiated between pure contact and contact-based education, but neither intervention was found to be superior in relation to stigma-reduction.

Other pertinent questions have also gone unanswered, such as whether contact improves stigma across all stigma measures. Measures of stigma are typically categorised as being cognitive, referring to participants' beliefs, attitudes, and stereotypes about PMI; affective, referring to participants' emotional responses towards PMI; or behavioral, referring to participants' actions towards PMI. However, due to the difficulty inherent in measuring participants' actual behavior, researchers often substitute this with measures of behavioral intentions (i.e., how participants believe they would behave). Previous meta-analyses have found some differences in the effectiveness of interventions on these different measures of stigma both when outcomes are measured immediately and in the long-term. For example, although Corrigan et al. (2012) found contact to reduce stigma overall (i.e., averaged across different measures), contact did not appear to significantly reduce affective bias, and the average effect size for behavioral intentions was smaller than that for attitudes. In contrast, Morgan et al. (2018) found the effect size for social distance (a measure of behavioral intentions) to be larger than the effect size for attitudes for both pure contact interventions and contact-based education interventions.

Neither Corrigan et al. (2012) or Morgan et al. (2018) included measures of participants' behavior in their analyses, and Mehta et al. (2015) identified only one study that included such a measure. It has been argued that improving the public's behavior towards PMI is more important than improving their privately held beliefs about PMI. While potentially stigmatizing, beliefs about PMI which are captured by attitude measures are less likely to be observable by PMI, whereas the discriminatory behavior exhibited towards them may be more salient and thus have more pernicious effects (Corrigan et al., 2012). Moreover, the cognitive, affective, and behavioral intentions outcomes identified above and included in previous meta-analyses all comprise explicit, self-report measures of stigma, in which participants indicate their responses on a questionnaire. This is problematic for two reasons. First, explicit measures are vulnerable to presentation biases, with individuals able to alter their responses in order to create a particular

impression (e.g., being less stigmatizing; Teachman, Wilson, & Komarovskaya, 2006). This is not the case for implicit measures, which examine automatic associations between two categories (e.g., PMI and goodness) which are not consciously held by individuals and thus less likely to be inaccurately reported (Neumann, Hülsenbeck, & Seibt, 2004). Secondly, explicit and implicit measures appear to predict behavior differently – explicit bias predict intentional behavior while implicit bias predicts behavior that occurs when individuals are not cognizant of the need to appear tolerant (Neumann et al., 2004). Organizations and researchers implementing anti-stigma strategies should be informed of whether behavior and implicit bias, along with explicit measures of stigma, are improved following contact with PMI.

Another related question to consider is whether the effect of contact endures over time, and whether this differs between types of contact and stigma measures. The majority of researchers measure participants' mental health stigma immediately after the intervention, but organizations implementing these interventions likely intend for them to reduce stigma long-term. As outlined above, Mehta et al. (2015) found the effect of indirect contact on stigma to be significantly larger than that of face-to-face contact when outcomes were measured at least 1 month later. Corrigan, Michaels, and Morris (2015) found face-to-face contact-based education interventions to improve attitudes in the long-term, but there was no significant long-term effect on overall stigma levels, suggesting that the effect of contact may endure on some measures of stigma but not others. Similarly, Morgan et al. (2018) found that only the effect of contact-based education, but not pure contact, endured for up to 6 months post-intervention, but this was only apparent on the behavioral intentions measure. Thus, there is some evidence that the persistence of the intervention's effects may depend on the contact medium and content, as well as differing between stigma measures.

Characteristics of the participants and PMI involved in the interventions may also moderate its effectiveness. Specifically, the attitudes of adolescents and of college students towards PMI may be less immutable than older adults', rendering them more likely to benefit from stigma-reduction interventions (Corrigan et al., 2012; Pettigrew & Tropp, 2006). On the contrary, for other groups like healthcare professionals who routinely experience contact with PMI, contact interventions may not be sufficiently novel to reduce stigma. The effectiveness of these interventions may also depend on participants' cultural beliefs about mental illness. There is some evidence that the level of stigma against PMI differs between cultures (e.g., Abdullah & Brown, 2011), but research is inconsistent regarding the relationship between contact and stigma in non-Western societies. Some research finds contact to be more effective in non-Western samples (e.g., Rao, Feinglass, & Corrigan, 2007) while other research finds no association between the variables (e.g., Yang et al., 2012). As organizations and researchers may prefer to target their interventions towards individuals who are most likely to benefit from them, it is essential to discover what groups will be responsive to these interventions. Similarly, it may be helpful to know whether contact with PMI with certain mental illnesses is more effective than contact with PMI with other mental illnesses, and whether interventions involving contact with a larger number of PMI are more effective than interventions with fewer PMI. There has been substantial variability between studies in relation to these factors, but their impact on the effectiveness of the intervention has not been considered.

Many interventions aiming to reduce mental health stigma do not specifically measure stigma against the mental illness of the PMI involved in the intervention (West, Hewstone, & Holmes, 2010). For example, participants may experience contact with an individual with schizophrenia, but report stigma against depression or the superordinate mentally ill category. West et al. (2010) argue that this inconsistency between intervention and measurement could lead researchers to underestimate the impact of contact on stigma. However, if participants who experience contact with individuals representing one mental illness (i.e., schizophrenia) report reduced stigma against that illness as well as illnesses not represented in the contact experience (i.e., depression), then the intervention is more valuable than previously thought. It decreases the number of interventions required to reduce stigma against *all* mental illnesses. Determining whether contact reduces stigma against mental illnesses represented and not represented in the intervention has considerable implications for individuals administering these initiatives.

The aim of this meta-analysis is to provide answers to these critical questions. First, we will determine the overall effect of contact immediately after the intervention, and at short-term (1–12 weeks) and medium-term (16–52 weeks) follow-ups. Then, at each of these timepoints, we will examine 1) whether some mediums of contact (e.g., face-to-face contact) are more effective than others (e.g., imagined contact), 2) whether contact-based education is more effective than pure contact, 3) whether contact improves implicit bias and actual behavior as well as attitudes, affective bias, and behavioral intentions, 4) what individuals benefit the most from contact, 5) whether contact with some mental illnesses is more effective at reducing stigma than contact with other mental illnesses, 6) whether contact with a larger number of PMI is more beneficial than contact with a smaller number of PMI, and 7) whether contact with one mental illness transfers to reductions in stigma against other mental illnesses. The answers to these empirical questions should guide decision-makers and researchers in developing effective stigma-reduction strategies that are informed by evidence.

2. Method

2.1. Search strategy and eligibility criteria

We included any published or unpublished quantitative primary research studies that experimentally investigated the effect of contact between individuals without mental illness (the participants) and PMI (outgroup members) on mental health stigma. We excluded correlational studies but included RCTs, cluster RCTS, non-RCTs, controlled before and after studies, multi-arm studies, and uncontrolled before and after studies. Potentially relevant studies were evaluated against the eligibility criteria below. The search strategy is described in Appendix A.

2.1.1. Criterion 1: Contact

Contact could be face-to-face, imagined, extended, vicarious, electronic, video, or presentation, provided the interaction had a duration greater than 30 seconds. We excluded contact that occurred with patients in hospitals and mental health facilities as part of clinical training or workplace practice, as such interactions were unlikely to be positive and meet other criteria considered essential to contact interventions, such as equal status (Allport, 1954). We also excluded studies in which contact was self-reported by participants or presumed by researchers due to proximity (e.g., in a classroom), as actual contact between participants and PMI could not be guaranteed.

2.1.2. Criterion 2: Participants

Participants could be of any age, nationality, ethnicity, gender, or occupation, provided that the majority of the participants did not have a mental illness. Studies were excluded if more than 30% of the final sample was comprised of individuals with mental illness, unless the results were reported separately for participants with and without mental illness. Regrettably, some studies did not provide this information about their participants, in which case the studies were included provided they did not mention an intention to recruit participants with mental illness.

2.1.3. Criterion 3: Outgroup member

The outgroup member/s that participants interacted with during the

intervention could be confederates or individuals with a current or past diagnosis of any mental illness described in the Diagnostic and Statistical Manual of Mental Disorders (DSM-5) and/or in the International Classification of Diseases and Related Mental Health Problems (ICD-10). We included studies in which the outgroup member was identified as 'mentally ill', having a 'mental disorder', being a 'consumer of mental health services' or a 'former mental patient', even when the diagnosis was not specified. However, studies were excluded if the illness was not specified beyond 'intellectual disability', 'learning disability', 'mentally retarded', 'mentally handicapped', or 'mentally disabled'.

2.1.4. Criterion 4: Outcome variables

Studies must have used at least one relevant quantitative measure of stigma against people with mental illness, such as explicit attitudes (e.g., attributions, stereotypes), affective bias (e.g., fear, anxiety), behavioral intentions (e.g., social distance, helping), implicit bias, or actual behavior (e.g., chair distance, donations). Mental health literacy or knowledge, self-stigma, attitudes towards treatment or mental health professionals, and perceptions of others' stigma (i.e., perceived stigma) were not eligible.

2.1.5. Criterion 5: Comparison group

Studies must have included a comparison group in which participants completed the relevant measures when they had not experienced contact with PMI. Participants could complete the measures prior to the intervention (baseline) or after experiencing a different intervention (e.g., contact with a person without mental illness).

2.2. Coding of intervention characteristics

We first determined the medium of contact and whether the contact intervention involved an educational component. We then coded participant and design characteristics that might moderate the effect of contact on stigma. The participant characteristics were occupation and the country in which the study was conducted. The outgroup member characteristics were the number of outgroup members the participants had contact with, and the mental illness/es of the outgroup member/s. RM classified all intervention characteristics and at least 15% were checked by a research assistant (SF and CT, respectively). Interrater agreement ranged from good to perfect (κ = .735 – 1.000).

2.2.1. Type of contact

An intervention was classified as contact-based education if general information about mental illness and/or stigma was provided to participants prior to, during, or after experiencing contact and/or if a PMI spoke about their experience of mental illness (including their symptoms, treatment, lifestyle, stigma). In contrast, an intervention was classified as pure contact if participants did not receive any educational information about mental illness during the intervention.

We classified the medium of contact as being face-to-face, imagined, vicarious, extended, electronic, video, or presentation. *Face-to-face contact* was defined as participants interacting reciprocally with an outgroup member while in the same physical location. This was distinguished from *presentation contact*, defined for the first time here as participants observing a lecture, presentation, or performance given by an outgroup member in person, but with limited ability for direct interaction. Similar to presentation contact, we defined *video contact* as participants viewing an outgroup member in a film or video (Corrigan et al., 2012). If the participants witnessed another individual interacting with an outgroup member, either in person, via video, or through a story, this was classified as *vicarious contact* (Dovidio, Love, Schellhaas, & Hewstone, 2017). We defined *electronic contact* as participants interacting with an outgroup member via the Internet (also called virtual contact; White & Abu-Rayya, 2012), and *imagined contact* as participants visualising themselves interacting with an outgroup member (Crisp & Turner, 2009). Finally, participants could be prompted to recall an ingroup member having a positive relationship with an outgroup member, categorised as *extended contact* (Wright et al., 1997).

2.2.2. Outgroup member

Participants might experience contact with one or more outgroup members, and the outgroup member/s could each have one or more mental illnesses (one outgroup member with multiple diagnoses, or multiple outgroup members with different diagnoses). Thus, we coded three factors related to the outgroup member: the number of outgroup members, the number of mental illnesses represented, and the specific mental illnesses represented.

2.3. Coding of dependent measures

We classified each dependent variable as explicit attitudes, affective bias, behavioral intentions, implicit bias, or behavior. Scales assessing self-reported beliefs about PMI, such as agreement with or endorsement of stereotypes (e.g., dangerousness, competence) and attributions were categorised as explicit attitudes. Affective bias was defined as self-reported emotional responses to PMI, including fear and anxiety, whereas behavioral intentions were defined as the participants' self-reported anticipated behavior towards PMI, including their willingness to be in specific social relationships with PMI (i.e., social distance). Reaction-time tasks assessing the strength of the association between PMI and categories such as goodness and badness were classified as implicit bias, and observations of the behavior exhibited by participants towards PMI, such as the rated quality of their interactions, were classified as behavior. We also used a sixth category, prejudice, to refer to scales that included items belonging to more than one of the above categories. Additionally, we determined what mental illness was targeted by the measure and whether this matched at least one of the specific diagnoses represented in contact. This allowed us to explore whether contact only reduced stigma against the mental illness/es of the outgroup member/s involved in contact or other mental illnesses as well. RM coded all dependent variables, and a subset (20%) was checked by JM, with interrater agreement ranging from moderate to perfect (κ = .546 – 1.000).

2.4. Meta-analytic strategy

The overall meta-analysis, meta-regressions, and assessments of heterogeneity and publication bias were performed using Stata Version 14.2, and the commands *metan, metareg, metafunnel,* and *metabias*. We used the DerSimonian and Laird (1986) random-effects model to estimate the effect of contact on stigma, which uses an estimate of the between-study variation to weight each effect size. In contrast to the fixed effect model, the random effects model does not assume that there is only one true effect, and is thus more appropriate when there are noteworthy methodological differences between studies (Palmer & Sterne, 2009). Meta-regression was used to determine if there were any significant moderators of the effect by comparing the effect in each subgroup to the effects in the other subgroup/s when there were at least 4 effect sizes in each (Fu et al., 2011).

We used the standardised mean difference, specifically Cohen's *d*, as the effect size, which is calculated by dividing the mean difference between the intervention and comparison group by the within group standard deviation. Values were adjusted so that a negative effect size would indicate less stigma in the intervention condition compared to the comparison condition. The standardized effect sizes were interpreted using standard conventions, with 0.2, 0.5, and 0.8 representing small, medium, and large effects, respectively (Cohen, 1992). Analyses were first performed with then without effect sizes that were more than 2.5 standard deviations from the mean. Heterogeneity was assessed using Cochrane's Q and I^2 statistics.

Most studies assessed the effect of contact on multiple outcome variables. As including every given effect size would have violated the assumption of independent observations in meta-analysis, we calculated an average effect size for each study on which to conduct the primary meta-analysis. However, in order to investigate differences in the effect of contact on the different dependent variables, a separate meta-analysis was conducted with one average effect size per outcome per study. Where available, we used the preintervention measurement as the comparison group. Otherwise, we prioritised the condition in which no intervention was received by participants, then the condition in which no contact with PMI occurred. For studies that compared more than one contact condition but used the same kind of contact (e.g., symptom vs. recovery-focused testimonial; Norman, Li, Sorrentino, Hampson, & Ye, 2017), the average effect of the contact intervention was computed to allow for a single pairwise comparison with the comparison condition. Studies that compared multiple types of contact were maintained as separate studies when the effect was measured against a baseline group, but combined when they were measured against the same control group, as including multiple comparisons against the same control condition would have violated the aforementioned assumption (Turner & Bernard, 2006).

Following similar meta-analyses (e.g., Miles & Crisp, 2014), we used multiple methods to assess publication bias, including treating publication status as a moderator, examining the correlation between sample size and effect size, and visually and statistically inspecting a funnel plot using Egger's regression (Egger, Smith, Schneider, & Minder, 1997). We also used the trim and fill (Duval & Tweedie, 2000) and fail safe *N* (Rosenthal, 1979) procedures to estimate the effect of the intervention when missing studies were represented and to calculate the number of studies with null findings that would be needed to change the conclusions of the meta-analysis, respectively.

3. Results

Altogether, 121 individual studies from 113 articles were deemed to be eligible for inclusion in the meta-analysis, representing 26,847 participants. Nineteen articles were unpublished and 101 were published. All but one of the published articles were peer-reviewed. The majority of the studies were conducted in the United States ($k = 49$), England ($k = 16$), Canada ($k = 14$) or Australia ($k = 13$). Other studies were conducted in Spain ($k = 3$), Germany ($k = 2$), Hong Kong ($k = 2$), Malaysia ($k = 2$), Indonesia, China, Japan, India, Iran, Saudi Arabia, Turkey, Belgium, Israel, Poland, the Czech Republic, France, Italy, Portugal, Sweden, and Scotland. Most studies targeted university or college students in helping professions (e.g., psychology, pharmacy, nursing, medicine, social work; $k = 74$) and primary or secondary school students ($k = 27$). Other studies targeted community members ($k = 10$), health professionals ($k = 6$), and security or law enforcement personnel ($k = 3$).

All data was extracted by RM. Precise effect sizes could be calculated from 87 studies (71.9% of the total sample), based on means and standard deviations (or standard errors or confidence intervals) provided in the article or through correspondence with the authors. Summary statistics such as *t* and *p* values were used to calculate effect sizes for 9 studies, and effect sizes for 5 studies were calculated from proportional data. For 20 studies, the necessary data was not available (12 authors did not respond to requests, and 8 authors reported the data to be unavailable). Therefore, 101 studies could be included in the meta-analysis. Ninety studies reported on the effect of the intervention immediately after it occurred, 33 reported on the effect of the intervention in the short term (1–12 weeks), and 7 reported on the effect of the intervention in the medium term (16–52 weeks). The characteristics and effect sizes for each of these studies can be found in Table B.1–B.3 in Appendix B. Pooled effect sizes for each subgroup are displayed in Table 1.

3.1. Immediate effect of contact on stigma

The 90 studies that assessed the effects of contact on stigma immediately after the intervention comprised 100 separate interventions and 15,826 participants. Meta-analysis showed that contact had a medium-sized effect on stigma, $d = -0.478$ (95% CI [−0.554, −0.403]). There was a significant variation in the size of the effect across studies ($Q[99] = 2004.92$, $p < .001$; $I^2 = 95.1\%$), which was reduced but remained statistically significant when two outliers (Fernandez, Tan, Knaak, Chew, & Ghazali, 2016; Ke et al., 2015) were excluded ($Q[96] = 988.68$, $p < .001$; $I^2 = 90.3\%$). The average effect of contact on stigma remained significant but decreased to a small-to-medium effect, $d = -0.384$ (95% CI [−0.439, −0.328]).

As depicted in Table 1, there was a significant effect on explicit attitudes, affective bias, behavioral intentions, and prejudice, but no significant effect on implicit bias or behavior. Meta-regression revealed that none of these effect sizes were significantly different from one another ($\beta s < .136$, $ps > .278$). Moreover, there was no significant difference in the effectiveness of contact when the dependent variables targeted the mental illness of the PMI participants experienced contact with and when these variables targeted mental illnesses not represented in the contact intervention ($\beta = .155$, $p = .084$).

3.1.1. Type of contact

We compared the effectiveness of contact with and without an educational component. Both contact-based education and pure contact had significant, small-to-medium sized effects on stigma, and meta-regression found the effect sizes to not significantly differ between the two approaches ($\beta = .169$, $p = .100$).

Seven of the 90 studies compared multiple mediums of contact, one study reviewed the effectiveness of multiple interventions but did not report the results for each medium of contact separately, and one study utilised both imagined and video contact in a single intervention. No study used extended contact. Each medium of contact, with the exception of vicarious contact, had a significant small-to-medium effect on contact. An insufficient number of studies employed vicarious contact and electronic contact, so the effectiveness of these mediums could not be compared to the other contact mediums. Meta-regression revealed no significant difference in the effectiveness of face-to-face, imagined, video, or presentation interventions ($\beta s < .049$, $ps > .102$).

3.1.2. Moderators of the immediate effectiveness of contact: Participant characteristics

The 97 interventions represented 16 countries which could be categorised into two groups, being Western countries (USA [$k = 44$], UK [$k = 13$], Canada [$k = 13$], Australia [$k = 12$], Czech Republic [$k = 2$], Spain [$k = 2$], Belgium, Sweden, Germany), and non-Western countries (Hong Kong, Malaysia, India, Japan, Saudi Arabia, Iran, Israel). One study (Brownlow et al., 2015) recruited participants from countries in both groups and did not differentiate between them, and therefore could not be categorised. Although the immediate effect of contact on stigma was significant in both groups, the effect was significantly larger in the non-Western countries compared to the Western countries ($\beta = .487$, $p = .002$).

Contact had a significant effect on stigma in university students, school students, community members, and health professionals, but not in law enforcement personnel. Meta-regressions revealed some differences in the effect of contact on stigma as a function of participant occupation, however the effect on law enforcement could not be compared. Namely, the effect was significantly smaller in community members compared to university students ($\beta = .252$, $p = .006$) and health professionals ($\beta = .236$, $p = .021$). No other comparisons yielded statistically significant differences ($\beta s < .218$, $ps > .168$).

Table 1
Effect sizes, confidence intervals, and heterogeneity statistics for each subgroup.

Subgroups	Immediate effects						Short-term effects						Medium-term effects					
	k	*n*	*d*	95% CI	*Q*	I^2	*k*	*n*	*d*	95% CI	*Q*	I^2	*k*	*n*	*d*	95% CI	*Q*	I^2
Overall																		
Outliers included	100	15,826	−0.478*	−0.554, −0.403	2004.92*	95.1%	40	3,697	−0.572*	−0.725, −0.419	660.99*	94.1%	8	842	−0.526*	−0.828, −0.225	135.76*	94.8%
Outliers excluded	97	15,445	−0.384*	−0.439, −0.328	988.68*	90.3%	35	3,410	−0.334*	−0.422, −0.247	186.99*	81.8%	–	–	–	–	–	–
Outcome																		
Explicit attitudes	74	12,693	−0.396*	−0.466, −0.326	563.39*	87.0%	23	2,029	−0.318*	−0.411, −0.226	86.36*	74.5%	6	497	−0.664*	−1.232, −0.097	124.67	96.0%
Affective bias	41	3,627	−0.302*	−0.395, −0.208	221.50*	81.9%	11	496	−0.266*	−0.385, −0.068	31.57*	68.3%	3	171	−0.106	−0.426, 0.215	9.62*	79.2%
Intentions	64	11,539	−0.390*	−0.463, −0.316	457.19*	86.2%	22	2,208	−0.319*	−0.429, −0.209	72.04*	70.9%	6	562	−0.303*	−0.463, −0.144	9.24	45.9%
Implicit bias	6	381	−0.367	−1.059, 0.325	61.60*	91.9%	0	–	–	–	–	–	0	–	–	–	–	–
Behavior	5	427	−0.296	−0.604, 0.012	8.08	50.5%	2	235	−0.089	−0.173, 0.352	1.21	17.7%	0	–	–	–	–	–
Prejudice	17	2,747	−0.404*	−0.529, −0.278	50.82*	68.5%	7	623	−0.439*	−0.783, −0.095	62.45*	90.4%	0	–	–	–	–	–
Match between contacted PMI and outcome target																		
Match	43	4,294	−0.293*	−0.360, −0.226	172.82	75.7%	13	848	−0.301*	−0.420, −0.182	35.32*	68.9%	1	102	–	–	–	–
No match	53	10,578	−0.454*	−0.548, −0360	706.82*	93.1%	19	2,188	−0.329*	−0.463, −0.195	131.34*	86.3%	7	740	−0.289*	−0.402, −0.177	14.27*	58.0%
Country																		
Western	89	14,592	−0.340*	−0.391, −0.290	683.51*	87.1%	29	2,605	−0.249*	−0.325, −0.174	103.44*	72.9%	6	665	−0.255*	−0.347, −0.163	7.70	35.1%
Non-Western	7	616	−0.862*	−1.324, −0.401	131.48*	95.4%	6	582	−0.893*	−1.238, −0.547	34.50*	85.5%	2	177	−1.464*	−3.118, 0.189	56.01*	98.2%
Occupation																		
College	61	5648	−0.424*	−0.497, −0.351	388.66*	84.6%	22	1,809	−0.462*	−0.583, −0.341	107.36*	80.4%	6	536	−0.642*	−1.033, −0.251	132.93*	96.2%
School	17	3621	−0.386*	−0.523, −0.250	197.59*	91.9%	6	1,403	−0.158	−0.341, 0.024	48.55*	87.6%	1	270	–	–	–	–
Community	12	5523	−0.151*	−0.208, −0.094	26.39*	58.3%	4	169	−0.197*	−0.291, −0.103	1.64	< 0.0%	1	36	–	–	–	–
Professionals	5	547	−0.641*	−1.208, −0.073	117.17*	96.6%	1	23	–	–	–	–	0	–	–	–	–	–
Law enforcement	2	106	−0.155	−0.494, 0.185	7.51*	86.7%	1	27	–	–	–	–	0	–	–	–	–	–
Educational component																		
Contact-based education	78	14,254	−0.413*	−0.475, −0.350	918.01*	91.7%	32	3,266	−0.353*	−0.447, −0.259	183.09*	83.1%	7	806	−0.581*	−0. 917 −0.244	133.83*	95.5%
Pure contact	20	1,191	−0.258*	−0.379, −0.136	70.59*	73.1%	3	144	−0.167*	−0.321, −0.014	2.39	16.3%	1	36	–	–	–	–
Type of contact																		
Face-to-face	4	189	−0.223*	−0.387, −0.060	10.39*	71.1%	0	–	–	–	–	–	1	36	–	–	–	–
Imagined	14	901	−0.222*	−0.362, −0.081	39.31*	66.9%	3	144	−0.167*	−0.321, −0.014	2.39	16.3%	0	–	–	–	–	–
Vicarious	2	134	−0.332	−1.100, 0.437	9.01*	88.9%	0	–	–	–	–	–	0	–	–	–	–	–
Extended	0	–	–	–	–	–	0	–	–	–	–	–	0	–	–	–	–	–
Electronic	1	90	–	–	–	–	0	–	–	–	–	–	0	–	–	–	–	–
Video	31	4,526	−0.428*	−0.553, −0.302	365.92*	91.8%	13	1,164	−0.374*	−0.533, −0.214	64.63*	81.4%	3	240	−1.067*	−2.098, −0.037	104.12*	98.1%

(*continued on next page*)

Table 1 (*continued*)

Subgroups	Immediate effects						Short-term effects						Medium-term effects					
	k	*n*	*d*	95% CI	*Q*	I^2	*k*	*n*	*d*	95% CI	*Q*	I^2	*k*	*n*	*d*	95% CI	*Q*	I^2
Presentation	44	5,692	−0.422*	−0.503, −0.341	369.92*	88.4%	17	2,131	−0.313*	−0.433, −0.193	103.31*	83.5%	4	566	−0.249*	−0.395, −0.103	6.86	56.3%
Mental illness of outgroup member																		
Severe	46	4,543	−0.344*	−0.419, −0.207	263.89*	83.3%	23	1,615	−0.372*	−0.494, −0.250	143.48*	85.7%	1	75	–	–	–	–
Schizophrenia	31	3,139	−0.348*	−0.438, −0.258	164.69*	81.8%	13	1,089	−0.440*	−0.593, −0.288	66.36*	81.9%	0	–	–	–	–	–
Schizoaffective	2	276	−0.216*	−0.665, 0.233	17.73*	94.4%	3	282	0.055	−0.200, 0.310	10.21*	80.4%	0	–	–	–	–	–
Psychosis	1	103	–	–	–	–	1	39	–	–	–	–	0	–	–	–	–	–
Bipolar disorder	5	757	−0.309*	−0.513, −0.105	22.20*	82.0%	0	–	–	–	–	–	0	–	–	–	–	–
Non-severe	17	2,218	−0.324*	−0.482, −0.167	123.71*	87.1%	2	128	−1.311	−3.489, 0.867	11.33*	91.2%	0	–	–	–	–	–
Anorexia nervosa	2	367	−0.573*	−0.961, −0.185	0.02	< 0.1%	0	–	–	–	–	–	0	–	–	–	–	–
Bulimia nervosa	1	29	–	–	–	–	0	–	–	–	–	–	0	–	–	–	–	–
Eating disorder	2	327	−0.197	−0.617, 0.223	10.15*	90.1%	1	17	–	–	–	–	0	–	–	–	–	–
Depression	4	234	−0.116	−0.257, 0.025	8.74*	65.7%	1	102	–	–	–	–	1	102	–	–	–	–
Autism	2	114	−0.009	−0.167, 0.150	0.18	0	0	–	–	–	–	–	0	–	–	–	–	–
Tourette's syndrome	2	225	−0.440*	−0.654, −0.226	0.01	0	0	–	–	–	–	–	0	–	–	–	–	–
Publication status																		
Published	77	13,334	−0.409*	−0.476, −0.342	733.76*	89.6%	25	2,799	−0.424*	−0.529, −0.320	134.57*	82.2%	8	842	−0.526*	−0.828, −0.225	135.76*	94.8%
Unpublished	20	2,492	−0.285*	−0.386, −0.184	115.76*	83.6%	10	611	−0.109	−0.229, 0.012	24.40*	63.1%	0	–	–	–	–	–

* $p < .05$

3.1.3. Moderators for the immediate effectiveness of contact: Outgroup member characteristics

First, we examined whether the effectiveness of contact varied according to the number of PMI the participants experienced contact with. One study compared contact with an outgroup member with depression to an outgroup member with Tourette's syndrome, so this study was treated as two separate interventions. Of the 98 interventions, 50 involved contact with a single outgroup member and 27 involved contact with between 2 and 12 outgroup members. Twelve studies did not specify the number of outgroup members participants experienced contact with, although ten appeared to use more than 1, one more than 2, and one more than 8. These studies were coded as involving contact with 2, 3, and 9 outgroup members, respectively[1]. A further 9 interventions had participants contact a variable number of outgroup members (e.g., 3–4, 12–15). In these cases, we took the average of the numbers provided. The effectiveness of contact did not significantly change as the number of outgroup members increased ($\beta = .251$, $p = .172$).

We also examined whether the effectiveness of contact varied as a function of the mental illness of the outgroup member/s. The 98 interventions involved contact with a variety of mental illnesses. Most ($k = 38$) involved contact with a single outgroup member with a single mental illness, while fourteen involved contact with multiple outgroup members with the same illness, five involved contact with one individual with multiple illnesses, and five involved contact with multiple individuals with different disorders. The remaining 36 interventions did not identify the mental illness of all the contacted outgroup members, so it is unknown how many mental illnesses each intervention represented. First, a meta-regression was conducted to determine whether the effectiveness of contact differed as a function of the number of mental illnesses represented during contact for the studies for which this information was known. There appeared to be no significant association between these factors ($\beta = .133$, $p = .083$). Second, we compared the effectiveness of contact between interventions involving contact with at least one severe mental illness (schizophrenia, schizoaffective disorder, psychosis, bipolar disorder) to interventions that only involved contact with less severe mental illnesses, for studies in which the mental illness/es of the outgroup members were specified. Contact had a significant effect on stigma when both severe and non-severe illnesses were encountered, and meta-regression revealed no significant difference in the size of the effect ($\beta = .105$, $p = .709$). Finally, we compared the effectiveness of contact with outgroup members with schizophrenia, bipolar disorder, and depression for studies in which only one mental illness was represented. None of the effect sizes were significantly different from each other (βs $< .198$, $ps > .073$).

3.2. Short-term effect of contact on stigma

In total, 33 studies measured the effect of contact on stigma between 1 and 12 weeks. Four studies measured the effect of contact on the outcome variables at multiple time points within a short-term period. These timepoints were averaged together to create one effect size per study. Additionally, four studies employed more than one intervention and a fifth study used the same intervention in three different populations. Thus, the primary meta-analysis was conducted on 40 effect sizes, representing 3,697 participants.

Meta-analysis showed that contact had a medium sized effect on stigma in the short-term, $d = -0.572$ (95% CI [−0.725, −0.419]). There was a significant variation in the size of the effect across studies ($Q[39] = 660.99$, $p < .001$; $I^2 = 94.1\%$), which was somewhat reduced when three outliers (Chiu & Graham, 2017; Fernandez et al., 2016; Ke et al., 2015) were removed ($Q[34] = 186.99$, $p < .001$; $I^2 = 81.8\%$). The average effect remained significant but decreased to a small-to-medium effect, $d = -0.334$ (95% CI [−0.422, −0.247]).

There were small-to-medium effects on explicit attitudes, affective bias, behavioral intentions, and prejudice, and these effect sizes were not significantly different (βs $< .097$, $ps > .402$). There appeared to be no significant effect of contact on behavior, but too few studies measured behavior to allow for comparisons between this variable and the other variables. The effect of contact on stigma did not significantly differ when stigma was measured against the mental illness of PMI participants experienced contact with compared to when it was measured against other mental illnesses ($\beta = .013$, $p = .932$).

3.2.1. Type of contact

We could not compare the effectiveness of contact-based education and pure contact interventions, as there were only 3 pure contact studies which assessed outcomes in the short-term. However, both approaches appeared to have a significant effect on stigma. One study utilised both imagined contact and video contact and one study utilised both video contact and face-to-face contact, but we could only compare presentation and video interventions. There was no significant difference in the relative effectiveness of contact delivered via these mediums ($\beta = .049$, $p = .759$).

3.2.2. Moderators for the short-term effectiveness of contact on stigma: Participant characteristics

The 35 interventions included represented 14 countries, being Western countries (USA [$k = 15$], Canada [$k = 7$], UK [$k = 3$], Australia [$k = 2$], Italy, Spain, Czech Republic) and non-Western countries (China, Hong Kong, Malaysia, Japan, Saudi Arabia, Iran). Again, although the effect of contact on stigma was significant in both regions, it was significantly larger in the non-Western countries ($\beta = .550$, $p = .002$). We next examined the occupation of the participants as a potential moderator, comparing university samples to school samples and community samples. The effect of contact on stigma was not significantly different in these groups (βs $< .330$, $ps > .063$), although the effect was only significant in the college and community samples.

3.2.3. Moderators for the short-term effectiveness of contact on stigma: Outgroup member characteristics

We examined whether the effectiveness of contact varied as a function of the number of outgroup members contacted. Of the 35 samples, 16 experienced contact with just one outgroup member and 15 experienced contact with between 2 and 13 outgroup members. Three studies did not specify the number of outgroup members contacted, although all appeared to use more than 1. For the purpose of meta-regression, these studies were coded as 2[2]. A final study included two groups of participants that had experienced contact with 2 outgroup members and one group that had experienced contact with 1. This was coded as 1.66. The effectiveness of contact was not significantly associated with the number of outgroup members contacted ($\beta = .038$, $p = .227$).

Finally, we investigated whether the effectiveness of contact varied as a function of the mental illness of the outgroup member/s. Seventeen interventions involved contact with just one mental illness, three involved contact with one PMI with multiple illnesses, and three involved contact with multiple PMI with different illnesses. The remaining 13 studies did not identify the mental illness of all the contacted outgroup members. There appeared to be no significant association between the effectiveness of contact and the number of mental illnesses represented ($\beta = .183$, $p = .094$). Again, due to an insufficient number of studies,

[1] When these 12 studies were excluded, the meta-regression remained insignificant ($\beta = .028$, $p = .247$).

[2] When these 3 studies were excluded, the meta-regression remained insignificant, ($\beta = .039$, $p = .296$).

we could not compare the effectiveness of contact with severe vs. non-severe mental illnesses or between any individual mental illnesses on stigma in the short-term.

3.3. Medium-term effect of contact on stigma

Seven studies measured the effect of contact on stigma between 16 and 52 weeks, representing 8 interventions and 842 participants. Meta-analysis showed that contact had a medium-to-large effect on stigma, $d = -0.526$ (95% CI [−0.828, −0.225]), with significant variation in the effect across studies ($Q[7] = 135.76$, $p < .001$; $I^2 = 94.8\%$). There was a significant effect on explicit attitudes and behavioral intentions, but not on affective bias. We compared the relative effectiveness of contact between explicit attitudes and behavioral intentions, and found no significant difference ($\beta = .191$, $p = .301$). There was an insufficient number of studies in each subgroup to compare contact mediums or to evaluate any other factor as a potential moderating variable.

3.4. Publication bias

To test for publication bias, we treated publication status as a moderator of the effect of contact on stigma immediately and in the short term. This was not possible for studies in which the outcomes were assessed in the medium-term, as all seven studies were published. The effect sizes and confidence intervals for each subgroup are displayed in Table 1. Publication status was not a significant moderator when outcomes were measured immediately ($\beta = .112$, $p = .269$) or in the short-term ($\beta = .163$, $p = .262$).

Despite the meta-regression finding no evidence of publication bias, we conducted further analyses to determine if there were likely to be further unpublished studies not captured by the search strategy. There was no significant correlation between sample size and effect size in studies assessing the effect of contact immediately ($r[95] = 0.121$, $p = .241$) or in the short-term ($r[35] = 0.132$, $p = .449$). However, the funnel plots (Figs. C.1 and C.2 in Appendix C) suggest that smaller studies and studies demonstrating a detrimental effect of contact on stigma are missing in both groups. This is consistent with a significant bias on an Egger's regression for immediate outcomes ($\beta = 2.675$ $p < .001$) and short-term outcomes ($\beta = -2.785$, $p = .001$). However, the trim and fill procedure (Duval & Tweedie, 2000) returned "no trimming performed; data unchanged". In addition, fail safe N (Rosenthal, 1979) suggested that 19,090 studies with null effects would be needed to overrule the conclusion that contact interventions reduce stigma immediately after the intervention, and 2,564 in the short-term. Both figures substantially exceed the $5k + 10$ recommendation. As an asymmetrical funnel plot may also indicate significant heterogeneity in effect sizes (Egger et al., 1997) which we know to exist in our sample, it was concluded that there is unlikely to be significant publication bias.

4. Discussion

The aim of this meta-analysis was to answer several critical questions pertaining to the effectiveness of intergroup contact for reducing mental health stigma, to provide guidance to researchers, clinicians, and organizations developing anti-stigma initiatives. The overall, immediate effect of contact on stigma was small-to-medium in size, similar to previous meta-analyses. Although fewer studies ($k = 36$) assessed stigma at least 1-week post-intervention, there was evidence that the beneficial effect of contact on stigma persists in the short- and medium-term. This is an important result, as the value of many stigma-reduction campaign lies in whether a single intervention can have lasting effects on its participants. As well, interventions should ideally improve attitudes, affect, and behavioral intentions as well as behavior and implicit bias towards PMI. However, the present study found no significant effect of contact on measures of actual behavior immediately after the intervention or in the short-term, and no immediate effect of contact on implicit bias. This may suggest that the beneficial effects observed on the self-report variables could be attributable to presentation biases in the participants rather than actual changes in stigma. Alternatively, contact interventions may be inadequate for changing automatic associations with mental illness while nevertheless improving explicit stigma. In order to modify negative associations with mental illness, more intensive interventions may be required. However, as meta-regression revealed the average effect size for contact on implicit bias and behavior to not be significantly different to the effect size related to the other outcome variables, and given that few studies measured these dimensions of stigma, we are cautious in our interpretation of these results.

Few of the factors we expected might impact the effect of contact on stigma were actually found to do so. For instance, pure contact interventions and contact-based education were found to be equally effective at reducing stigma immediately after the intervention and in the short-term, suggesting that testimonials delivered by PMI do not have any benefit beyond establishing contact, at least on measures of stigma. Thus, it seems that there is no need for PMI to discuss their diagnosis and experience of mental illness as part of an intervention if this is not feasible or if they do not wish to do so – rather, for stigma reduction participants need only to be aware of the outgroup member's mental illness during their interaction with them. It is possible that knowledge of mental illness may be improved by contact-based education more than contact alone, but further research is needed to confirm this.

We also detected no significant differences in the effectiveness of contact delivered via different mediums, and the effect did not depend on the number of PMI involved in the intervention or the mental illnesses represented. These findings suggest that more intensive interventions such as a presentation will not necessarily be more effective than having participants watch a film or imagine interacting with a PMI, and that interventions involving multiple PMI will not necessarily reduce stigma more than interventions involving a single PMI. Finally, the effect of contact does not seem to be restricted to reducing stigma against only the mental illness/es of the PMI involved in contact. For example, contact with individuals with bipolar disorder can reduce stigma against cocaine addiction, depression, and psychosis (Corrigan et al., 2001). These transfer effects should be encouraging to researchers, educators and policy-makers. They suggest that interventions targeting stigma against a single mental illness can generalise to reduced stigma against other mental illnesses and mental illness in general. This is consistent with other research in the intergroup contact domain finding a secondary transfer effect in which contact with the primary outgroup reduces prejudice against a secondary, non-contacted outgroup via reduced prejudice against the primary outgroup (Pettigrew, 2009). Together, these results allow organizations to choose the most practical, cost-effective, wide-reaching stigma-reduction intervention without compromising effectiveness. However, we should note that the small number of studies in some subgroups, particularly at the follow-up periods, precluded statistical comparisons. For example, we could not compare the effectiveness of imagined contact to video and presentation contact or contact with PMI with severe vs. non-severe illnesses in the short-term. Additionally, we could not conduct separate analyses for each outcome variable to determine whether, for example, face-to-face contact was superior to presentation contact for improving behavioral intentions.

Two factors did moderate the effectiveness of contact on stigma, both of which were related to the participants. First, the effect of contact immediately after the intervention and in the short-term was significantly larger in non-Western (Asian and Middle Eastern) countries compared to Western countries. Both Asian and Middle Eastern cultures value emotional control and perceive mental illness to be caused by weak characters and supernatural forces (Abdullah & Brown, 2011). There is limited research in the Middle East, but Asians appear to hold more stigmatizing beliefs of PMI (Abdullah & Brown, 2011) and to benefit more from contact interventions (Rao et al., 2007) compared to

Caucasians. Second, the immediate effect of contact was significantly more pronounced in university students and healthcare professionals than in community members. That contact was effective at reducing stigma in healthcare professionals was a surprising result, given that this group already experiences high levels of contact with PMI. One potential explanation for this is that the clinical context in which healthcare professionals ordinarily experience contact is not conducive to reducing stigma. Allport's (1954) intergroup contact hypothesis suggests positive, personal contact in which there is equal status between the parties involved represents the optimum contact condition. The contact experienced by healthcare professionals is unlikely to be of this nature, highlighting the need for formal interventions. As PMI often report experiencing stigma in healthcare settings (Lasalvia et al., 2013), reducing stigma in this power group is vital. But, researchers that only recruit from these populations might be overestimating the effect on community members, who also exhibit stigma against PMI in employment and in social relationships (Lasalvia et al., 2013).

4.1. Strengths, limitations, and future directions

The present study conducted a comprehensive, inclusive search of the literature that yielded a larger number of studies than any previous meta-analysis to date. We also coded several participant and intervention factors that could impact the effectiveness of contact interventions on stigma, with the intention of informing future initiatives. Nevertheless, the results should be considered in light of several limitations. First, although we conducted a thorough search of the literature and included both published and unpublished research, it is possible that some studies, particularly studies in non-English languages, have been missed. Second, we did not conduct a risk of bias assessment of the studies included in the meta-analysis. Initiatives to reduce stigma should be primarily informed by high quality research, and so greater weight should be given to these studies in analyses. We were unable to do so due to consistently poor levels of reporting which would have resulted in uncertain evaluations. For example, many articles failed to specify the number of outgroup members involved in the intervention. Poor reporting also prevented us from exploring other factors that could account for the significant amount of heterogeneity in the effect sizes, such as intervention duration, the content of contact-based education, and whether outgroup members confirm or disconfirm stereotypes about PMI.

We were also unable to explore *how* contact reduces mental health stigma. Such analyses require studies to report the correlation between the intervention, mediating variables, and outcome variables. Regrettably, few of the studies included in this meta-analysis did so. Moreover, although Pettigrew and Tropp (2008) identified knowledge, empathy, and intergroup anxiety to be key mediating variables in the effect of contact on prejudice against a variety of groups, different variables have been implicated in relation to mental health stigma. For instance, studies included in this review found evidence of contact reducing mental health stigma through reduced anger and fear of PMI (Maunder et al.,in press), increased positive feelings (Na & Chasteen, 2016), heightened perceived similarity and reduced sympathy (Norman et al., 2017), and improved attitudes (West & Turner, 2014). Given the lack of consistency in the extant literature, a review and synthesis should be undertaken to evaluate the theoretical and empirical relevance of these variables and others to contact and mental health stigma, specifically. Only then can researchers statistically examine and compare the relative contributions of the identified mediators, as well as whether different contact mediums and pure contact and contact-based education impact stigma through different mechanisms.

The effect of contact interventions on stigma against PMI has now been established in multiple reviews. Now, we suggest that researchers explore other factors that might impact the effectiveness of the intervention. As well, echoing the suggestions made in other reviews (e.g., Gronholm, Henderson, Deb, & Thornicroft, 2017), we recommend that researchers in the mental health stigma-reduction area provide more information about the interventions they employ, evaluate the long-term effect of their interventions, and go beyond self-report measures of stigma to assess both actual behavior towards and implicit bias against PMI. In this review, only 36 studies evaluated the effectiveness of contact more than 1 week after the intervention, and no study reported on the persistence of the effect beyond 1 year. Additionally, few studies included measures of behavior towards PMI, even though behavior is perhaps the most salient and pernicious indication of stigma (Corrigan et al., 2012). Further research is essential to ensuring that the interventions governments and organizations invest in, and which clinicians advocate for, will result in the best possible outcomes for PMI.

5. Conclusion

Mental health stigma has a substantial adverse effect on the quality of life of PMI and can dissuade individuals from seeking and engaging with treatment. The current meta-analysis synthesized the large extant literature ($k = 101$) of the effect of intergroup contact on mental health stigma and compared the content and medium of these interventions to establish the most effective methods. We found contact to have a significant overall effect on mental health stigma, which was consistent regardless of whether the intervention also involved education, and across different contact mediums. These results should provide some guidance to anti-stigma organizations to select the most appropriate strategy for their needs. Intergroup contact researchers should now turn to examining other factors that could moderate the effectiveness of the intervention, while also exploring potential mediating variables and evaluating the long-term and behavioral effects.

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Contributors

Rachel D. Maunder designed the study and wrote the study protocol including the search strategy and eligibility criteria, extracted the relevant data from the included studies, performed all the statistical analyses, and wrote the first draft of the manuscript.

Fiona A. White assisted in the revision of the manuscript.

Conflict of interests

Both authors declare that there are no conflicts of interest associated with this publication.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.cpr.2019.101749.

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