

Social identification-building interventions to improve health:

A systematic review and meta-analysis

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Abstract

There is growing evidence that social identity processes play an important role in a range of health outcomes. However, we know little about the nature and effectiveness of interventions that build social identification with the aim of promoting health. In the present research, we systematically review and meta-analyze interventions that build social identification to enhance health and wellbeing. A total of 27 intervention studies were identified ($N=2,230$). Using random-effects meta-regression, results indicate that social identification-building interventions had a moderate-to-strong impact on health (Hedges $g=0.66$; 95% CIs [0.34, 0.97]). Analyses revealed significant variation in intervention effectiveness as a function of its type: group-relevant decision making ($g=1.26$), therapy programs ($g=1.03$), shared activities ($g=0.40$), and reminiscence ($g=-0.05$). By contrast, there was much less variation across health outcomes: quality of life ($g=0.81$), physical health ($g=0.77$), self-esteem ($g=0.69$), well-being ($g=0.67$), (reduced) anxiety ($g=0.61$), (reduced) depression ($g=0.58$), cognitive health ($g=0.55$), and (reduced) stress ($g=0.49$). Finally, speaking to the mechanism of the interventions, results suggest that interventions tended to be more effective to the extent that they succeeded in building participants' social identification with the intervention group. We discuss the theoretical and practical implications of social identification-building interventions to foster health and outline an agenda for future research and practical application.

Keywords: social identity; social cure; health; intervention; meta-analysis; systematic-review

A growing body of research speaks to the fact that social identification with a group is an important predictor of a range of positive health outcomes (Haslam et al., 2018; Jetten, Haslam, & Haslam, 2012; Walsh, Fortune, Gallagher, & Muldoon, 2014). For instance, research informed by the social identity approach to health shows that when people belong to, and identify with, a social group this (a) reduces the likelihood of them suffering from depression (Sani, Herrera, Wakefield, Boroch, & Gulyas, 2012), (b) facilitates their well-being when facing new life challenges (Iyer, Jetten, Tsivrikos, Postmes, & Haslam, 2009), (c) diminishes post-traumatic stress symptoms (Muldoon & Downes, 2007), and (d) reduces burnout and enhances health in the workplace (Avanzi, Schuh, Fraccaroli, & van Dick, 2015). Indeed, a (if not *the*) core hypothesis of this theory, that has received most research attention to date, is that strength of social identification will have positive implications for individuals' health (Haslam et al., 2018).

Building on this maturing research field, there is increasing recognition of the policy and practice implications of group and social identity processes for health, and of their capacity to be the basis for “social cures” (Jetten, Haslam, Haslam, Dingle, & Jones, 2014; Wakefield, Bowe, Kellezi, McNamara, & Stevenson, 2019). However, while empirical examinations of the relationship between social identification and health have increased significantly in recent years, we know little about the extent to which *interventions* that build social identification are effective in bringing about (i.e., *causing*) an improvement in individuals' health. This lack of understanding is surprising given that intervention studies are an important means of establishing causality and of testing the core hypothesis that building social identification positively affects health. This lack of understanding is important too in light of increasing calls for better specification of the ‘active ingredient’ in the social group processes involved in the range of interventions that leverage social groups (Borek &

Abraham, 2018; Drum, Swanbrow Becker, & Hess, 2011; Foyd & Moyer, 2010; Hoddinott, Allan, Avenell, & Britten, 2010).

In the present research, we address this gap in the literature and advance the social identity approach to health by means of a systematic review and meta-analysis of social identification-building interventions. Here we define *social identification-building interventions to improve health* as intervention studies that (a) enhance participants' identification with a group and (b) have the aim of improving, and assessing the impact of the intervention on indicators of, participants' health. In line with the World Health Organization's (2019) encompassing definition of health as "a state of complete physical, mental, and social well-being and not merely the absence of disease or infirmity" (as set out in the first principle of the Preamble to the WHO Constitution:

<https://www.who.int/about/who-we-are/constitution>), we consider health here as capturing not only traditional forms of (physical) health, but also mental health and well-being. In the interest of minimizing repetition, we use the term 'health' to refer to this conceptualization throughout the manuscript.

Social Identification and Health

The social identity approach is comprised of two inter-related theories, *social identity theory* (Tajfel & Turner, 1979) and *self-categorization theory* (Turner, Hogg, Oakes, Reicher, & Wetherell, 1987), and has at its core the insight that people's sense of self can be, and often is, informed by their internalization of group membership — so that, to the extent they identify with a given group, they see themselves not just as 'I' and 'me' but as 'we' and 'us'. It has been argued that identification with a social group is beneficial for health because it provides the basis for a range of social and psychological resources including social support, a sense of meaning and purpose, and a sense of control over one's life (Cruwys et al., 2014; Haslam et al., 2018; Jetten et al., 2012; Sani, 2012; Walsh et al., 2014).

By way of an example, consider Nina and Anna who are both members of an online support group for cancer survivors. Imagine that they differ in their identification with the group, such that Nina has a strong sense of identification with others in the group, while Anna does not identify at all with the group. Social identity theorizing suggests that this diverging sense of social identification is likely to have important implications for their experience of, and the benefits they derive from, the group. Amongst other things, Nina is more likely to perceive the support that others offer as genuine and helpful, to offer support to others in the group, and to experience the group as something that provides her with a sense of meaning and purpose. Anna, in contrast, is more likely to be skeptical of any support that others in the group offer, to have no great desire to try to contribute to the group, and to experience the group as expendable and not in ways that furnish her with a sense of purpose and direction.

Indeed, there is evidence that shows that social identification provides access to important psychological resources that have significant implications for health, including a sense of belonging and social support (Avanzi et al., 2018; Haslam, Reicher & Levine, 2012; Kearns, Muldoon, Msetfi, & Surgenor, 2017; Walter, Jetten, Dingle, Parsell, & Johnstone, 2016), a sense of meaning and purpose (van Dick & Wagner, 2002; Wegge, van Dick, Fisher, Wecking, & Moltzen, 2006), and sense of control and agency (Greenaway et al., 2015; Hopkins et al., 2016). Indeed, two recent meta-analyses bear testimony to the broad evidence base that confirms the relationship between social identification and health. The first of these examined the relationship between social identification and health in organizational contexts (Steffens, Haslam, Schuh, Jetten, & van Dick, 2017) and the second examined the link between social identification and depression (Postmes, Wichmann, van Valkengoed, & van der Hoef, 2018). Both analyses provide support for the association between social identification and health. More specifically, Steffens and colleagues (2017) observe a

correlation of $r = .21$ between organizational identification and health (based on 58 independent samples) and Postmes and colleagues (2018) observe a correlation of $r = .15$ between social identification and reduced depression (based on 76 independent samples).

Nevertheless, the vast majority of studies that each of these meta-analyses included were observational and cross-sectional. Indeed, each meta-analysis identified only two intervention studies. As a result, there is clearly scope for research to more clearly signpost how researchers and practitioners can capitalize on research and theory in this field — in particular, by uncovering the nature of effective interventions — to improve individuals' health. As a preface to our attempts to provide guidance of this form, we review the range of previous studies that speak to this question.

Social Identification-Building Interventions to Improve Health

Over the course of the past decade, researchers have designed and tested a variety of social identification-building interventions that develop a sense of commonality with others – a sense of 'we' – with the view to improving participants' health. These efforts have involved at least four different types of interventions: (a) those organized around group-relevant decision making, (b) those with reminiscence groups, (c) those engaged in shared activities, and (d) those that center on group-based therapy programs. Table 1 presents an overview of these four types of interventions, including definitions and illustrative references for each.

An example of a social identification-building intervention involving group-relevant decision making is reported by Knight et al. (2010) conducted in care home facilities. In the intervention group, residents of the care home facility were provided with the opportunity to collectively decide how to decorate their residential home environment, while in the control group the decisions about decorating the residential home environment were made by staff members without input from residents. The researchers anticipated that the intervention would enhance participants' identification with other residents and thereby contribute to their

health and well-being. To assess the interventions' effectiveness, participants indicated their social identification with other residents and responded to various health measures (e.g., including physical health, psychological well-being, and quality of life).

An illustration of an identification-building intervention involving shared activities is provided by Morris, Chambers, Campbell, Dwyer, and Dunn (2012). This study comprised two intervention groups (in the US and Australia) in which women diagnosed with breast cancer took part in a 1000-mile motorcycle ride to raise funds for women with breast cancer over 10 days. It was predicted that participation in the intervention would build participants' social identification with their group and reduce their experience of cancer-related distress. To evaluate the effectiveness of the intervention, participants responded to (pre- and post-ride) measures assessing their social identification with the group and their distress.

An example of a social identification-building intervention focusing on reminiscence is provided by the work of Haslam and colleagues (2014b). In their study, older adults participated in a reminiscence group focusing on story-based, secular song-based, or religious song-based reminiscence in which they talked about past memories and experiences with others in their group (about their life stories or songs from the various periods in the past). The researchers hypothesized that regardless of modality (i.e., whether it revolved around stories or songs) group reminiscence would enhance health and well-being over time and that this would be particularly true to the extent that participants identified with their reminiscence group. To assess the interventions' effectiveness, participants' social identification with their reminiscence group and measures of their health (i.e., anxiety, cognitive health, life satisfaction) were assessed at both the beginning and end of the intervention and their identification with the group was assessed at the end of each session.

Finally, an example of a therapy-based social identification-building intervention is provided by Meuret and colleagues (2016). As part of the intervention, participants took part

in a manualized program of group-based behavioral therapy for social anxiety disorder. The researchers reasoned that engagement with group psychotherapy to treat social anxiety might increase participants' social identification with the treatment group. To evaluate the intervention's impact on social identification and health, participants' social identification with the treatment group as well as clinical health measures were taken at baseline and post-treatment.

From this review, it is evident that while social identification-building interventions vary in form, they also vary in the type of health outcomes that they focus on. Indeed, the impact of these interventions has been examined with regard to a variety of health measures including (a) depression (Haslam et al., 2016), (b) anxiety (Meuret et al., 2016), (c) quality of life (Dingle et al., 2015), (d) physical health (Gleibs et al., 2011a), (e) self-esteem (Scarf et al., 2018), (f) stress (Morris et al., 2012), (g) cognitive health (Gleibs et al., 2011b), and (h) well-being (Knight et al., 2010). At the same time, it is noteworthy that these interventions vary in the magnitude of their effect on health. For example, Morris and colleagues (2012) observed a small effect size (a standardized mean difference of Hedges $g = 0.22$) of the intervention on reduced distress (assessed by Weiss & Marmar's (1997) Impact of Events Scale-Revised), while Haslam and colleagues (2016) observed moderate-to-strong effect sizes (standardized mean differences ranging between 0.32 and 0.76) on various health outcomes (including depression, anxiety, and stress assayed by the DASS-21; Lovibond & Lovibond, 1995).

Given the diversity in intervention type and range of health outcomes, there is a clear need for a systematic review of the range of social identification-building interventions that have been conducted to date that directly assess their impact on health. Such a review will help to establish the value of such interventions given they are likely to differ in their effectiveness as a function of substantive differences in the nature and type of interventions

and the health outcomes they target, as well as random sampling error (particularly in interventions conducted with small samples). Moreover, it will allow us to answer three theoretically and practically important questions. First, overall, how effective are social identification-building interventions in improving health? Second, to what extent, and how, do different types of interventions vary in effectiveness? Third, to what extent, and how, do interventions vary in their capacity to deliver different kinds of health outcomes?

The Present Research

Despite the growing interest in social identification and health, no attempt has been made to provide a systematic quantitative synthesis of intervention studies that build social identification with a view to improving health. In light of emerging applications of the social identity approach to health, this is a significant shortcoming. The need for such analysis is also warranted by the growing use of interventions delivered in a group format (Borek, Abraham, Greaves, & Tarrant, 2018; Swancutt, Tarrant, & Pinkney, 2019) — a method which itself raises questions about the role that social (i.e., group) identification plays in supporting and enhancing participants' health. In these various ways, a meta-analysis of intervention studies not only constitutes a robust test of underlying theoretical principles of the social identity approach to health but also has practical utility for both health practitioners and policymakers.

In view of these considerations, the present research seeks to provide a systematic review of social identification-building interventions to improve health. This takes the form of a meta-analysis that estimates both the summary effect size and variability in effectiveness. Following best-practice recommendations for systematic reviews and meta-analysis, the reporting of the meta-analysis includes the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA; Liberati et al., 2009) and includes the information recommended by the Meta-Analysis Reporting Methods (MARS; Appelbaum et

al., 2018), while the reporting of the study protocol follows the PRISMA-P guidelines (Moher et al., 2015). The PRISMA and PRISMA-P statements are available as supplementary materials online.

Method

Preregistration

Following best practice guidelines (Stewart, Moher, & Shekelle, 2012; see also Moher et al., 2015) and to contribute to reproducible science and enhance confidence in findings by reducing bias, the protocol for the present meta-analysis was prospectively registered on PROSPERO before data extraction and analysis [http://www.crd.york.ac.uk/PROSPERO/display_record.php?ID=CRD42018092834]. The data and analysis code are included as supplementary materials for review and are available on the Open Science Framework:

https://osf.io/mpw5j/?view_only=b092ab3d41024235b378ee51f35e0f15). We followed the pre-registered study protocol in conducting the research by adhering to the inclusion criteria, the coding of the studies, and the analysis strategy as specified in advance. In what follows, we report the planned summary effect analyses to estimate the overall mean effect and heterogeneity indices. The included studies are further described by providing summary mean effect for each intervention type and each outcome category.

Eligibility Criteria

To be included in the meta-analysis, studies had to fulfil the following criteria: (a) have an intervention design (i.e., a single-group pre-post-intervention design, an independent-groups post-intervention design, or an independent-groups pre-post-intervention design), (b) be conducted in an applied context (e.g., in a community, health/ clinical, educational, or organizational setting), (c) include a measure of social identification with a specific group/collective relevant to the intervention, (d) report quantitative statistics on the impact of

the intervention using at least one indicator of health or well-being, and (e) be reported in English. Studies were excluded if they used other designs (e.g., cross-sectional, prospective, or observational), were conducted in the laboratory, included only a measure of social connectedness or identification in a general sense (e.g., not with the specific group that the intervention targeted or with groups in general) but not social identification with a group that the intervention targeted, or had assessed the impact of the intervention only on variables other than health.

Search Strategy

The various steps of the systematic literature search process are presented in Figure 1. Two members (the first two authors) of the review team independently conducted all steps of the literature search (any discrepancies were resolved through discussion). We used several search strategies to maximize the chance of capturing all relevant work. We conducted a search using the databases Web of Science Core Collection and PubMed to retrieve relevant published work, and ERIC, ProQuest Dissertations, and Theses Global to retrieve relevant unpublished work (grey literature). Prior to pre-registering the study, the research team considered and discussed at length the various databases in the development of the study design and conducted a scoping review of potentially relevant databases and keywords. The scoping review revealed that Web of Science covered a large proportion of potentially relevant outlets across disciplines (including social and life sciences) while PubMed covered a large proportion of potentially relevant outlets particularly from the medical and health sciences. It is our sense that, together with the use of ProQuest Dissertations and Thesis Global to identify potential unpublished literature and a call for unpublished data, this ensured that the study search was comprehensive in capturing potentially relevant studies.

In addition, with the aim of ensuring comprehensiveness of the search strategy, we used various keywords that revealed a large number of hits. We devised the search terms with

the aim of (a) using terms that were sensitive to the target field by maximising the chance of capturing potentially relevant studies, while minimizing the number of false negatives and (b) capturing the diverse nature of studies and their diverse foci to identify as many as possible potentially relevant studies. For the sake of consistency, we used the same parameters across the three databases (we did not use other database-specific search methods such as MeSH type subject headings offered by PubMed). We used the search terms to identify articles in the databases that contained any of the following search terms in the article's title and/ or abstract: [(intervention OR experiment* OR treatment OR therapy OR program OR programme) AND ("social identi*" OR "group identi*" OR "organi?ational identi*" OR "work* identi*" OR "team identi*" OR "club identi*" OR "school identi*" OR "crowd identi*") AND (health* OR well-being OR wellbeing OR depression OR anxiety OR stress OR quality of life OR life satisfaction)]. A detailed description of the search process including the exact search terms used for each data base are provided in the PROSPERO pre-registration document (see Supplementary Materials). The search was conducted (last updated) on 28 March 2019.

This search yielded a total of 2,744 records, of which 349 were duplicates leaving a total of 2,395 unique records. Each coder (X & Y, anonymized for the sake of the review process) separately screened titles and abstracts of all 2,395 unique records with the view to determining the article's potential to include relevant data (deciding whether to include or exclude in the subsequent full-text screen). The coders agreed in their judgment in 2,220 out of 2,395 articles (92.7%), indicating high levels of agreement. To maximize the likelihood of including a potentially relevant record, all records that were coded by only one judge as potentially relevant were included in the next phase of the full-text screening. In the second phase, each coder then independently conducted a full-text screen of the selected set of articles that the previous phase identified to determine whether (or not) these fulfilled the

inclusion criteria. The coders agreed in 96.8% (274 out of 283) of all articles, indicating a high degree of inter-reliability. The coders discussed cases of disagreement and arrived at a consensus about how best to deal with these. The full-text screening of these 283 records yielded a total of 20 records that fulfilled the inclusion criteria.

In addition, we sent out calls for data to the European Association of Social Psychology, who publish the *European Journal of Social Psychology* where an edited Special Issue on the ‘Social Cure’ appeared in 2017, and to participants who attended the 4th International Conference on Social Identity and Health (ICSIH4; Desrichard et al., 2018; $N \approx 80$). This yielded five additional relevant records, resulting in a total of 25 records reporting 27 independent samples (56 effect sizes, $N = 2,230$) that met the inclusion criteria (full references of all included data can be found in the supplementary materials online).

Data Extraction

The final sample of included studies is presented in Table 2. The first and second author of the present work extracted all data and resolved any discrepancies through discussion. The following information was extracted: authors, title, date of publication, study population description, participant demographics, sample size, study design, intervention description, delivery format, outcome measure, social identification measure, risk of bias characteristics, and effect size statistics.

To calculate each effect size, we extracted means, standard deviations, and sample size. Following recommendations from Borenstein, Morris, and colleagues (Borenstein, Hedges, Higgins, & Rothstein, 2009; Morris, 2008; Morris & DeShon, 2002), we calculated a common effect size metric in the form of standardized mean difference to allow for comparable effect size estimations across studies. For independent-groups post-intervention designs (that employed only one post-intervention measurement) we used the following

formula (Morris & DeShon, 2002): $d_{IG} = \frac{M_{post,E} - M_{post,C}}{SD_{post,P}}$, where M_{post} is the sample mean

post-intervention for the treatment and control group, respectively, and $SD_{post,P}$ is the pooled within group standard deviation of post-intervention scores. For designs employing pre-and-post measures we used the pooled *pre*-intervention standard deviation as the denominator for the effect size calculation, because this is not influenced by the intervention and provides more accurate estimations that are more comparable across studies (Morris, 2008). For independent-groups pre-post-intervention designs we used the following formula (Morris, 2008): $d_{IGPP} = \frac{(M_{post,E} - M_{pre,E}) - (M_{post,C} - M_{pre,C})}{SD_{pre,P}}$, where M_{post} is the sample mean post-intervention and M_{pre} is the sample mean pre-intervention (for the treatment and control group), and $SD_{pre,P}$ is the pooled pre-intervention standard deviation. For single-group pre-post intervention designs we used the following formula (Morris & DeShon, 2002): $d_{SGPP} = \frac{M_{post,E} - M_{pre,E}}{SD_{pre,E}}$, where $SD_{pre,E}$ is the pre-intervention standard deviation. Finally, to account for the positive bias of Cohen's d , we applied Hedges correction factor to each effect size using the following formula (Hedges, 1981): $J = 1 - \frac{3}{4df-1}$ where df is the degrees of freedom.

Focal Analysis

We conducted all analyses using R open-source software (version 3.5.0) and the package *metafor* (Viechtbauer, 2010). Some studies reported multiple outcomes, so to account for dependency between effect sizes, we conducted a three-level meta-analytic model (Moeyaert et al., 2017; Van den Noortgate, López-López, Marín-Martínez, & Sánchez-Meca, 2015). The three-level model extends the two-level (univariate) random-effects model by specifying random effects at both level 2 (to each effect size within a study) and level 3 (the mean effect size at the study level). The three-level model estimates the sampling variance for each effect size (level 1), the within-study variance (level 2), and the between-study variance (level 3) and combines regression equations at each level in a three-level meta-

analytic regression equation (see Moeyaert et al., 2017). Simulation studies show that standard errors are unbiased and confidence intervals for summary effect sizes are robust within nominal ranges (Van den Noortgate, López-López, Marín-Martínez, & Sánchez-Meca, 2013; 2014).

A three-level meta-analysis has a number of advantages over other traditional meta-analysis models in accounting for dependency in outcomes. First, it allows for more precise estimation of the variance by splitting the overall variance into between-study (level 3) and within-study (level 2) variance (Assink & Wibbelink, 2016; Pastor & Lazowski, 2018). Second, three-level meta-analysis does not require imputing or assuming the correlations between multiple outcomes, which is beneficial because correlations between dependent outcomes are often missing from reports of original studies. Finally, because multiple effect sizes from the same study are not averaged together (as is practiced traditionally), the standard errors are not overestimated, decreasing the chance of Type-2 error (for review see Moeyaert et al., 2017).

We conducted a three-level random-effects meta-analysis to calculate the summary standardized mean effect size including 95% confidence intervals around the mean across the studies. In addition, we estimated various indicators of the heterogeneity in the effect size distribution including absolute variance (σ) and the proportion of variance (I^2) at both level 2 (within-study variance — i.e., between different outcomes) and level 3 (between-study variance), as well as the 80% prediction interval around the mean effect size (indicating the 80% likelihood of the range of the effect size of a new study; Raudenbush, 2009).

In addition to the summary analysis across all studies, we estimated the summary effect size per type of intervention and per type of outcome (see Tables 3 and 4). The type of interventions included (a) therapy programs (e.g., the G4H program focusing on enhancing people's group-based social connections to enhance health by Haslam et al., 2016), (b)

group-relevant decision making (e.g., the SUSTAIN intervention focusing on facilitating community residents' decision-making by Heath et al., 2017), (c) shared activities (e.g., the Adventure Education Program focusing on facilitating shared engagement in a sailing trip by Scarf et al., 2018), and (d) reminiscence reflecting together about past experiences (e.g., the Song- and Story-based Reminiscence Program focusing on facilitating the discussion of past memories by Haslam et al., 2014b).

The studies reported outcomes comprised measures assessing (a) depression (e.g., using the Depression scale from the DASS-21 from Lovibond & Lovibond, 1995), (b) anxiety (e.g., using the Beck Anxiety Inventory from Beck, Epstein, Brown, & Steer, 1988), (c) quality of life (e.g., using the Satisfaction With Life Scale from Diener, Emmons, Larsen, & Griffin, 1985), (d) cognitive health (e.g., using Addenbrooke's Cognitive Examination Revised, ACE-R, instrument from Mioshi, Dawson, Mitchell, Arnold, & Hodges, 2006), (e) physical health (e.g., using the Physical Health scale from the Centers for Disease Control and Prevention, 2005), (f) stress including burnout (e.g., using the Stress scale from the DASS-21 from Lovibond & Lovibond, 1995), (g) self-esteem (e.g., using the Self-Esteem Scale from Heatherton & Polivy, 1991), (h) and psychological well-being (e.g., using the Resilience Scale from Wagnild & Young, 1993).

Bias Analysis

We also conducted several bias analyses. First, we estimated missing data due to potential publication bias using several techniques that included (a) analysis of asymmetry in the funnel plot, which tests whether the effect size was predicted by the standard error (Egger, Smith, Schneider, & Minder, 1997), (b) meta-regression by sample size to examine whether the effect size was predicted by a study's sample size (Kühberger, Fritz, & Scherndl, 2014), and (c) analysis by publication status to examine whether the effect size in published studies differed from that in unpublished studies (Egger, Juni, Bartlett, Holstien, & Sterne,

2003; McAuley, Tugwell, & Moher, 2000). In addition, we conducted a risk of bias analysis for each study using the Cochrane tool to assess risk of bias in the key domains highlighted by Higgins et al. (2011): (1) absence of selection bias (i.e., inadequate sequence generation /randomization to intervention groups and inadequate allocation concealment /method to assign participants to intervention groups), (2) performance bias (i.e., non-blinding of participants and of personnel), (3) detection bias (i.e., non-blinding of outcome assessment), (4) attrition bias (i.e., systematic dropping out of participants), (5) reporting bias (i.e., selective reporting), and (6) other bias. For each study, it was coded whether the risk of bias in each domain was high, low, or unclear. We then conducted an additional meta-regression to examine whether the effect size was affected by a study's risk of bias.

As indicated in the preregistration protocol, in designing the present research we had planned to examine several moderators. However, the data collection obtained a total of only 23 independent samples. As a result, the moderation analyses would be based on few independent samples per level of a planned moderator and have little accuracy and power to reliably detect moderation effects. For this reason, we decided not to proceed with moderation analyses.

We conducted additional analyses to establish the summary effect size for the measure of social identification. As for the primary outcome analysis, we used a three-level random-effects meta-analysis to estimate the summary effect size for social identification with the group. Furthermore, to examine whether the effectiveness of interventions in influencing health was associated with the degree to which an intervention increased social identification with the group, we estimated the correlation between the effect size for the primary outcome and the effect size for social identification. To account for dependency between multiple measures of both health outcomes and social identification, studies that provided multiple health outcomes and social identification measures were averaged (i.e.,

aggregated to the study-level) before estimating the correlation between the social identification effect size and the health effect size.

Sensitivity Analysis

Finally, we conducted a series of sensitivity analyses to identify particularly influential observations within the data, and analyzed the effect of these observations on the summary model. To identify influential observations we inspected (a) Cook's distances (Cook & Weisberg, 1982), which in a multilevel data structure can be interpreted as Mahalanobis distance between all predictors when the i th study is included and when the i th study is excluded (Viechtbauer & Cheung, 2010), (b) standardized residuals, where scores outside ± 2.24 standard deviation are considered extreme (Aguinis, Gottfredson, & Joo, 2013; Martin & Roberts, 2010), and (c) hat values (i.e., the diagonal element of the hat matrix), where those greater than $2(k+1)/n$ are considered as extreme observations and k is the number of predictors and n the sample size (Aguinis et al., 2013; Cohen, Cohen, West, & Aiken, 2003). We excluded each influential observation in turn from the summary model to examine the degree of influence of each observation (indicated by a reduction in variance at both level 2 and 3).

Results

Focal Analysis

Overall effectiveness. An overview of the sample that includes the coding of all study characteristics is present in Table 2. A three-level random-effects model across the studies (56 effect sizes, $k = 27$, $N = 2230$) indicated that social identification-building interventions had an overall moderate-to-large positive effect on health, Hedge's $g = 0.66$, 95% CIs [0.34, 0.97], $t(55) = 4.15$. A substantial amount of the total variance ($Q(55) = 781.86$, $p < .001$) was due to differences between-studies (Level 3: $I^2 = 84.73$), rather than within-studies (Level 2: $I^2 = 11.57$) and sampling error (Level 1: $I^2 = 3.69$). There was

significant within-study variance $\sigma_{level\ 2}^2 = .080$, $\chi^2(1) = 24.56$, $p < .001$, and between-study variance $\sigma_{level\ 3}^2 = .583$, $\chi^2(1) = 34.90$, $p < .001$. A forest plot displaying the effect size distribution (including mean effect size and confidence intervals) is presented in Figure 2. The 80% prediction interval (Raudenbush, 2009) indicated an 80% chance that the effect size of a new study will fall within the range of a standardized mean effect size (Hedges' g) of -0.42 to 1.73.

Effectiveness as a function of intervention type. Results of the effectiveness of social identification-building interventions as a function of intervention type are presented in Table 3. Results indicate an overall large effect of interventions that involve group-relevant decision-making (Hedges $g = 1.26$, $k = 5$, 95%CIs[0.61, 1.91]) and therapy programs (Hedges $g = 1.03$, $k = 6$, 95%CIs[0.44, 1.61]), while the overall effect of interventions involving shared activities (Hedges $g = 0.40$, $k = 13$, 95%CIs[-0.01, 0.81]) and reminiscence (Hedges $g = -0.05$, $k = 3$, 95%CIs[-0.87, 0.76]) were weaker and non-significant. The 80% prediction interval revealed a large amount of heterogeneity within each type of intervention (group-relevant decision making: 0.23 to 2.28; therapy programs: 0.02 to 2.04; shared activities: -0.57 to 1.38; reminiscence: -1.13 to 1.02), indicating that the effect size of a new study is likely to fall within a wide range of possible magnitudes. The total residual heterogeneity when accounting for type of intervention was significant, $Q(52) = 609.14$, $p < .001$, with significant remaining within-study $\sigma_{level\ 2}^2 = .080$, $\chi^2(1) = 24.77$, $p < .001$, and between-study variance $\sigma_{level\ 3}^2 = .441$, $\chi^2(1) = 27.33$, $p < .001$.

Effectiveness as a function of health outcome. Table 4 presents the results for the effectiveness of social identification-building interventions as a function of health outcomes. The mean effect sizes of the interventions varied little across the various health outcomes (ranging from 0.48 to 0.79). The smallest effects were observed for (reduced) stress (Hedges $g = 0.49$, $k = 5$, 95%CIs[-0.04, 1.01]), cognitive health ($g = 0.55$, $k = 4$, 95%CIs[-0.02,

1.13]), (reduced) depression ($g = 0.58, k = 8, 95\%CIs[0.11, 1.06]$), and (reduced) anxiety ($g = 0.61, k = 8, 95\%CIs[0.15, 1.07]$), while the largest effects were observed for well-being ($g = 0.67, k = 9, 95\%CIs[0.20, 1.14]$), self-esteem ($g = 0.69, k = 5, 95\%CIs[0.11, 1.27]$), physical health ($g = 0.77, k = 5, 95\%CIs[0.25, 1.29]$), and quality of life ($g = 0.81, k = 8, 95\%CIs[0.39, 1.24]$). There was substantial total heterogeneity remaining when accounting for health outcome, $Q(48) = 710.99, p < .001$, comprising both significant residual within-study variance, $\sigma_{level 2}^2 = .102, \chi^2(1) = 20.45, p < .001$, and between-study variance, $\sigma_{level 3}^2 = .572, \chi^2(1) = 24.65, p < .001$.

Additional Analyses

We conducted additional analyses on the secondary outcome of social identification. First, we estimated the summary effect of the interventions on social identification across all studies that provided sufficient data (24 effect sizes, $k = 20, N = 1,252$). Five studies reported social identification with two target groups and therefore, as in the main analyses of the primary health outcome, we conducted a three-level random-effects meta-analysis to account for the dependency in the measures. Analysis indicated that overall the interventions were successful in increasing social identification, Hedges $g = 0.50, 95\%CIs [0.23, 0.76], t(23) = 3.91$. There was a substantial amount of heterogeneity in effect sizes, $Q(23) = 391.33, p < .001$, which was largely due to between-studies differences (Level 3: $I^2 = 72.02$) and within-studies differences (Level 2: $I^2 = 23.51$) rather than sampling error (Level 1: $I^2 = 4.46$). Both within-study variance, $\sigma_{level 2}^2 = .070, \chi^2(1) = 29.85, p < .001$, and between-study variance were significant, $\sigma_{level 3}^2 = .214, \chi^2(1) = 4.34, p = .037$. The 80% prediction interval (Raudenbush, 2009) indicates that the true effect size of a new study is likely to fall within the range of -0.23 to 1.22.

Furthermore, it has been suggested that social identification is likely to be a mechanism accounting for the interventions' effectiveness (see Tarrant et al., 2016). To

examine this possibility, we examined the association between the effect size for the impact of the interventions on social identification and the effect size for the impact of the interventions on health. Results indicated a positive association of moderate magnitude, $r = .33$. Consistent with the theorized mechanism, this suggests that interventions are more effective to the extent that they build social identification with the target group. Nevertheless, this analysis was not pre-registered, the sample size for this analysis was small ($k = 20$), and the association is only moderate in magnitude and so, in addition to interpreting this finding with caution, there is a clear need to investigate this relationship further as additional intervention studies are conducted.

Bias Analysis

Funnel plot asymmetry analyses. The funnel plot displays the relationship between standard error and effect size. Funnel plot visualizations have not been developed for three-level meta-analyses, and so multiple dependent effect sizes within a study were averaged before plotting the effect size against standard error (in the funnel plot). Visual inspection of the funnel plot (see Figure 3) revealed no clear indication of asymmetry. To provide a formal test of asymmetry, we conducted Egger and colleagues' (1997) regression test, examining whether the effect size was predicted by an estimate's standard error. The analysis indicated that the standard error was not a significant predictor of the effect size, $t(25) = 1.90, p = .069$, providing no evidence of asymmetry.

Analysis by sample size. To further investigate publication bias, we examined whether the effect size could be predicted by a study's sample size. Publication bias is typically indicated by a significant relationship between sample size and effect size, such that small studies tend to report relatively larger effects (Kühberger et al., 2014). Meta-regression by sample size yielded no evidence that the effect size was predicted by sample size, $F(1, 54) = 0.001, p = .619$, thereby providing no evidence of publication bias.

Analysis by publication status. Furthermore, we examined whether there was evidence of publication bias indicated by inflated effect sizes found in the published compared to unpublished research findings (Egger et al., 2003; McAuley et al., 2000). The summary effect based on published samples was positive, Hedges' $g = 0.66$, $k = 23$, 95%CIs [0.30, 1.01], and the summary effect based on unpublished samples positive and of moderate size, Hedges' $g = 0.66$, $k = 4$, 95%CIs [-0.15, 1.47]. The confidence interval for the effect size in unpublished studies crossed zero, which is likely due to large variation within the small sample of four unpublished studies. The analysis yielded no significant differences in the effect sizes of published and unpublished studies, $F(1, 54) = 0.015$, $p = .988$, indicating no evidence of bias by publication status.

Risk of bias analyses. We used the Cochrane risk of bias tool (Higgins et al., 2011) to examine risk of bias in the studies. This was conducted jointly by two authors (X & Y, anonymized for the sake of the review process) in the first instance and then, to safeguard against potential bias in evaluating the studies, a third author (Z) who is not a co-author of any of the reviewed studies independently coded all included studies. This yielded 51.4% initial agreement. Most disagreements occurred for the criteria, “adequate allocation concealment” and “blinding of participants / personnel to condition” (coded systematically differently as ‘unclear’ and ‘high risk’ respectively), highlighting the difficulty of applying these criteria to studies that do not employ a medical RCT design (e.g., quasi experimental, pre-post designs) or those where it is not possible to blind participants/facilitators. Removing these criteria yielded an agreement level of 69.6% (94 out of 135 ratings). We also note that across all judgments (without removing all criteria), there were very few instances where coders disagreed entirely (just 7.9% of criteria being rated ‘high risk’ by one and ‘low risk’ by the other, corresponding to 17/216 codings). Disagreements between raters were resolved through discussion. Figure 4 presents an overview of the final assessment of risk of bias

across key domains across the studies (while the separate codings for each study are presented in Supplementary Table 2). This revealed that for most domains, studies provided an insufficiently detailed description of important study characteristics (insufficiently clear or not reported at all), preventing an accurate estimation of the risk of bias in the data quality and analysis. It is noteworthy that the Cochrane risk of bias tool is customized for medical randomized-control trials (RCTs), which reduces the appropriateness of the bias dimensions in the present set of psychological studies that comprised only two RCTs (Gee, Hawes, & Cox, 2019; Haslam et al., 2018b).

For instance, while it may be possible to have blind assessment of outcomes in psychological interventions of the present form, it is not feasible to have blind personnel/facilitators (as facilitators need to have an understanding of the intervention they are delivering) and in most studies, it is impossible to have blind participants (as participants provide their consent to participate in, and are aware of the content of, the intervention). As can be seen in Figure 4, in the majority of studies the reporting was unclear with regard to blinding of personnel (100% unclear), selective reporting (96% unclear), blinding of participants (89% unclear), and incomplete data addressed and allocation concealment (52% unclear). As we discuss in more detail in the Discussion, this pattern suggests that future work should provide more comprehensive reporting of the interventions (particularly around blinding of personnel and participants, selective reporting, and incomplete data addressed and allocation concealment) and that there is a need for additional (pre-registered) RCTs in a next wave of research in this field.

To further examine whether risk of bias influenced the effect size, we calculated a summary risk of bias score for each study by adding up high risk of bias across the study characteristics (where higher numbers indicate greater risk of bias). We then used this score in a meta-regression to examine whether the effect size was predicted by the studies' risk of

bias. Meta-regression indicated that the effect size was not predicted by the overall risk of bias, $F(1, 54) = 0.116, p = .735$, providing no indication that the studies' findings were affected by risk of bias.

Sensitivity Analysis

We conducted a series of sensitivity analyses which together revealed little evidence of outliers and particularly influential studies impacting the overall results. Inspection of Cook's distances indicated two outlier effect sizes (ordered by degree of influence: Meuret et al., 2016; Grodnitzky, 1993; Dingle et al., 2015). In addition, three studies were found to have residual standard scores greater than 2.24 standard deviations (ordered by size of residual score: Knight et al., 2010, effect size c; Knight et al., 2010, effect size a; Meuret et al., 2016). Inspection of the hat values did not reveal any extreme values. Finally, to inspect the degree of influence of each outlier effect size on the overall summary effect, we removed each effect size in turn from the summary model. This resulted in similar effect sizes to the summary model (where they had been included), all Hedges g 's = 0.62 – 0.67 [0.30-0.34, 0.94-1.00], and variances at level 2 and 3: $\sigma_{level 2}^2 = .080 - .084, \chi^2(1) = 24.09 - 25.25$, all p 's < .001; $\sigma_{level 3}^2 = .577 - .609, \chi^2(1) = 33.43 - 35.10$, all p 's < .001.

Discussion

The present study advances the social identity approach to health by providing an integrative analysis of the impact of social identification-building interventions on participants' health. Results indicate that social identification-building interventions have a moderate-to-strong overall positive effect on health (Hedges $g = 0.66$). At the same time, results reveal substantial variation in the effect size of different types of interventions. Interventions involving group-relevant decision making or therapy programs had the largest effect on health (Hedges' $g = 1.26$ and 1.03), while those based on shared activities or reminiscence had comparatively smaller effects on health outcomes (Hedges' $g = 0.40$ and -

0.05). At the same time, the magnitude of the effect varied little across different domains of health (from $g = 0.49$ and 0.55 for reducing stress and increasing cognitive health, to $g = 0.77$ and 0.81 for increasing physical health and quality of life). Speaking to potential mechanisms of these interventions, additional analyses indicated that the extent to which interventions were able to build social identification was positively associated (of moderate strength: $r = .33$) with those interventions' capacity to improve health. Furthermore, results provided little evidence of publication bias across the set of studies. Nevertheless, risk of bias analysis revealed that many study characteristics were not sufficiently detailed in their reporting (a common finding in reviews of psychological interventions; Kirby, Tellegen, & Steindl, 2017) and were therefore unclear, rendering it imperative for future work to use high-quality designs (including RCTs) and to improve reporting of methods and results with the aim of enhancing replicability and comparability of results.

Implications for Theory and Practice

The present study has at least three important implications. First, research informed by the social identity approach that has been conducted in applied, health, and clinical contexts has grown significantly over the last decade (for a recent review, see Haslam et al., 2018). Despite this growing evidence base, the potential usefulness of this research to *change* individuals' health, and to inform practice and policy, has been hampered by the fact that to date there have been limited attempts to synthesize the intervention research in the field (cf. Jetten et al., 2014). The present work addresses this shortcoming by providing a systematic integration of social identification-building interventions that aim to improve health that estimated the magnitude of their causal impact. By showing that social identification-building interventions make a significant contribution to health, the current study underlines the usefulness of the social identity approach to health as a framework that delineates how best to harness social group dynamics to promote positive orientations towards health (see also

Tarrant, Hagger, & Farrow, 2012). In this regard, the study highlights the value of being explicit about theory as an important avenue for future intervention development (Gourlan et al., 2016; Hayes, Long, Levin, & Follette, 2013; Taylor, Conner, & Lawton, 2012). It further supports a biopsychosocial model of health as an important complementary model to a biomedical model which can foster stigma (Kvaale, Haslam, & Gottdiener, 2013) and undermine innovation (Deacon, 2013).

Second, it is noteworthy that there was large variation in the extent to which interventions were effective in impacting health, with the effect size of a new future study likely to fall (with 80% certainty) within the range of a standardized mean difference of -0.42 and 1.73. The practical implication of this is that not all interventions are created equal when it comes to their impact on health, a finding that suggests it is important to interrogate this review's main finding further. Indeed, homing in on the type of social identity-building intervention revealed additional nuances on the effect of interventions on health. Specifically, interventions involving group-relevant decision making and therapy programs were found to be particularly effective. This may reflect the fact that therapy programs typically target individuals who have specific issues and needs (e.g., with diagnosed anxiety, depression), where the fit between participants and the group — and hence their resulting identification — is likely to be higher (Cruwys et al., 2019). Moreover, interventions that involve group-relevant decision making may be particularly effective because they have an explicit focus on developing and improving participants' collective sense of self (e.g., by deciding together what 'we' as a group want to do), which may enhance their sense of social support and collective self-efficacy, thereby promoting their health (Junker, van Dick, Avanzi, Häusser, & Mojzisch, 2018). In this regard, if we improve our understanding of the multiple potential mechanisms involved in each of these interventions, effect sizes are more likely to be

consistent and positive, and this in turn will allow us to better tailor interventions to individuals' needs in order to maximize their benefit.

In addition, it is worth stressing the more general point that, as with any intervention, for social identification-building interventions to be translated to practice, it is important to understand the mechanisms of change (i.e. the active ingredients) that underlie symptom reduction and improvement in functioning (Moore et al., 2015). This would be useful for the refinement and development of more effective interventions (Kadzin, 2007). In this regard, our additional analyses (which were not pre-registered) provided some indication in line with theoretical considerations that those interventions that succeeded in building participants' collective sense of self (enhancing their social identification) tended to be more effective. While the limited amount of relevant data meant that this was based on a small sample ($k = 20$), these findings are consistent with qualitative data which point to social identification as a mechanism of change in group-based intervention (see Tarrant et al., 2016) as well as empirical studies that have tested this mechanism (Haslam et al., 2016). Further, previous work suggests that change in self-related constructs (e.g., self-beliefs, self-awareness) may be a basis for individuals' reduction in social anxiety disorder as a result of cognitive behavioral therapy (Gregory & Peters, 2017). There is also research showing that individuals' role identity of self as a person who engages in physical activity can promote health behavior (Rhodes, Kaushal, & Quinlan, 2016). The current findings are consistent with these ideas by highlighting that individuals' sense of self deriving from group membership may be an additional important mechanism of change. Nevertheless, these findings highlight the importance of including measures of treatment group identification in future group intervention studies and examining group identification explicitly as a mechanism (e.g., through mediation analysis).

Finally, the defining characteristics of the reviewed interventions center on strengthening people's group-based social connections to others. Evidence of their efficacy thus provides an important avenue to respond to calls for greater attention to problems arising from lack of social connection (e.g., Gerst-Emerson, & Jayawardhana, 2015; Leih-Hunt et al., 2017). This is because in this context it appears that social identification-building interventions have an impact on health outcomes that are at least as large, or larger, than the impact of alternative interventions. For example, interventions using cognitive behavioural techniques to address social disconnection have been found to have an overall effect size on reducing loneliness of a standardized mean difference of 0.33 (Masi, Chen, Hawkey, & Cacioppo, 2011), while positive psychological interventions have been found to have a standardized mean effect size of between 0.20-0.34 on (decreased) depression and (increased) well-being (Bolier et al., 2013), and exercise interventions have been found to have a standardized mean effect size of 0.61 in reducing depression (Silveira et al., 2013). In this context, the present results for interventions that attune to social identity processes hold promise. These findings also highlight the potential for the social identity approach to serve as a framework for delivering group interventions for health outcomes and the need for future intervention studies to include more measures of mechanism and to examine when and for whom particular mechanism(s) are conducive to better health.

Limitations and Recommendations for Future Research and Practice

The present research has highlighted several shortcomings in the existing literature that limit the conclusions we can draw and that future work should improve upon. Most obviously, the present analysis is based on the limited number of interventions that have been conducted to date, with few involving manualized programs or RCTs. This meant that the present research was not able to shed light on several theoretically important questions (e.g., when and for whom particular programs may or may not work). Clearly, there would be

benefit in more large-scale high-powered, controlled studies that can provide conclusive answers to such questions. This may subsequently provide an even more comprehensive evidence base for interested practitioners to make use of this work in their practice. In this regard, it is promising that a few RCTs are underway, including an intervention to develop social identity targeted at patients with chronic obstructive pulmonary disease attending exercise rehabilitation (Levy et al., 2018) and a social identity derived intervention, GROUPS 4 HEALTH (G4H), to treat loneliness associated with psychological distress (Haslam et al., 2018b; ACTRN12617001602314). G4H is particularly promising (and unique) in the sense that it is a comprehensive theory-informed intervention in which building group-based social connections is not incidental but instead an explicit focus. Specifically, across five modules, it aims to help participants understand the importance of social groups before then working with them in concrete ways to build and sustain their network of social groups. Indeed, in light of the benefits that this program has been shown to deliver (Haslam et al., 2016, 2018b), in future work it will be worthwhile seeking to disentangle the range of mechanisms that appear to underpin its effectiveness — not least exploring the extent to which building social identification with multiple groups (as opposed to just one) and the extent to which this enables access to psychological resources (e.g., of social support, enhanced control, and belonging) is implicated in positive health outcomes.

In addition, the reporting of the methods and results of the present set of studies was unclear in relation to many study characteristics. There would be value in improving the reporting of the study characteristics in ways that allow for greater accuracy in describing the studies and evaluating the quality of their methods and data (e.g., by providing more information about blinding, selective reporting, incomplete data addressed, and allocation concealment and by using checklists for interventions; Borek, Abraham, Smith, Greaves, & Tarrant, 2015; Hoffmann et al., 2014). Furthermore, although the sample included studies

that assessed physical health, the number of these was small. It is also noteworthy that these studies did not include objective measures of physiological functioning (e.g., heart rate variability, mobility), and this points to another lacuna for future research to address.

Finally, the present study was not able to evaluate the long-term effectiveness of social identification-building interventions. Future examinations of the longevity of the effects of social identification-building interventions would be valuable. This is particularly important as relapse is a common problem for many health problems (Cruwys et al., 2013; Solomon et al., 2000; Vittengle, Clark, Dunn, & Jarrett, 2007). In this context too, it might also be useful to establish whether (a) social identification-building interventions that are embedded in people's lives (i.e., that involve working with the social groups in individuals' existing communities; e.g., Chatterjee, Camic, Lockyer, & Thomson, 2018) and (b) those that focus not only on building individuals' social identification but also on building collective shared identities (van Dick, Ciampa, & Liang, 2018) have the capacity to deliver effects that are as (or more) long-lasting as current treatment approaches.

Conclusion

The present research advances the social identity approach to health through a systematic review and meta-analysis of social identification-building interventions that impact individuals' health. Key results show that social identification-building interventions have an overall moderate-to-strong positive effect on health outcomes. Furthermore, the benefits of these interventions are similarly strong across a variety of outcomes ranging from reducing aversive experiences such as depression and anxiety, to building positive experiences such as physical health and quality of life. At the same time, interventions involving group-relevant decision-making and therapy interventions had relatively large effects, while those involving shared activities and reminiscence had relatively small effects.

The present research expands upon the field of social identity and health by synthesizing the strength of the causal effects obtained in previous studies, and in doing so attests to the clear impact of individuals' social group-based connections on their health and well-being. Overall, then, the social identity approach to health may not only help address important theoretical puzzles but also form the basis for practical efforts to improve the quality of people's lives.

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Tables and Figures

Table 1

Overview of four types of social identification-building interventions to improve health including definition and illustrative references.

<i>Intervention type</i>	<i>Definition of content</i>	<i>Illustrative references</i>
Group-relevant decision making	Participants engage in activities that focus on reflecting on and making decisions about the group and its members. Participants explore aspects of group life (including interests, preferences, procedures, space) that define how the group works together as a group and/or how members contribute to group functioning.	Knight et al. (2010); Haslam et al. (2014a)
Reminiscence	Participants engage in activities that revolve around reflecting on past memories, experiences, and events that are important to individual members or the group as a whole.	Grodnitzky (1993); Haslam et al. (2014b)
Shared activities	Participants engage in joint activities. Joint activities include those in which group members simultaneously perform the same individual tasks (e.g., individual painting) and those in which group members perform interdependent tasks (e.g., in team sports).	Gleibs et al. (2011a); Morris et al. (2012)
Therapy programs	Participants engage in programs that have a psycho-educational or psycho-therapeutic focus. Interventions have the explicit focus (and participants are of this focus) on developing the self to improve functioning and health.	Dingle et al. (2015); Meuret et al. (2016)

Table 2

Overview of Studies including Sample Size and Standardized Mean Difference as a Function of Intervention Type and Health Outcome ($k = 27$, 56 Effect Sizes, $N = 2,230$).

Study	Intervention Type	<i>N</i>	Design	Effect size (Hedges' <i>g</i>)	Population	Country	Length (Number of Sessions)	Social Identification Scale	Health Outcome (Measure)
Groditzky (1993)	Reminiscence	48	IGPPD	-0.30	High school students with maladaptive behavior patterns	US	9 weeks (1 session of 1 hour per week)	Luhtanen & Crocker (1992): 17 item	Self-Esteem (SSES)
Dingle et al. (2010)	Shared activities	11	SGPPD	0.06	Choir members experiencing chronic mental health problems	AU	6 weeks (1 session per week)	Swann et al. (2009): 1 item ^{S1} ; Doosje et al. (1995): 4 items ^{S2}	Depression (DASS-21: DS)
	Effect size b	11		0.34					Anxiety (DASS-21: AS)
	Effect size c	11		0.05					Stress (DASS-21: SS)
Haslam et al. (2010)	Reminiscence	73	IGPPD	-0.19	Care home residents	UK	6 weeks (1 session of 30-minutes duration per week)	Leach et al. (2008): 1 item	Depression (HADS: DS)
	Effect size b	73		0.12					Anxiety (HADS: AS)
	Effect size c	73		-0.12					Quality of Life (QoLADS)
	Effect size d	73		-0.18					Quality of Life (LIS)
	Effect size e	73		0.80					Quality of Life (QoLCS)
	Effect size f	73		0.05					Cognitive Health (ACE-R)
Knight et al. (2010a)	Group-relevant decision making	24	IGPPD	4.34	Care home residents	UK	4 weeks (2 sessions in intervention group)	Doosje et al. (1995): 2 items ^{S1} ; Doosje et al. (1995): 1	Physical Health (PHSRS)

								item ^{S2}	
Effect size b		24		2.63					Quality of Life (QoLOB)
Effect size c		24		4.27					Well-Being (PCSRS)
Knight et al. (2010b), Study 2	Group-relevant decision making	47	IGPD	1.45	Office workers in commercial companies	UK	1 hour (1 session of 1-hour duration)	Doosje et al. (1995) & Haslam (2004): 3 items	Well-Being (PCSRS)
Effect size b		47		0.87					Physical Health (PHSR)
Gleibs et al. (2011a)	Shared activities	45	IGPPD	0.05	Care home residents	UK	8 weeks (1 session of 20-30-minutes duration per week)	Doosje et al. (1995): 4 items	Quality of Life (QoL-4S)
Effect size b		45		-0.40		UK			Physical Health (GP-C)
Gleibs et al. (2011b)	Shared activities	26	SGPPD*	0.10	Care home residents		12 weeks (1 session every second week)	Doosje et al. (1995): 2 items	Cognitive Health (ACE-R)
Effect size b		26		0.23					Quality of Life (LS-SIS)
Effect size c		26		0.51					Depression (HADS: DS)
Effect size d		26		0.16					Anxiety (HADS: AS)
Morris et al. (2012)	Shared activities	51	SGPPD	0.22	Women diagnosed with breast cancer	AU/US	10 days (continuous)	Cameron (2004): 12 items	Stress (IES-R)
Cruwys et al. (2014a), Study 1	Therapy program	92	SGPPD	0.47	Adult outpatients diagnosed with depression or anxiety	AU	4 weeks (2 sessions of 3.5-hours duration per week)	Hinkle et al. (2008), Leach et al. (2008), & Luhtanen & Crocker (1992): 11 items	Depression (ZSRDS)
Effect size b		92		0.20					Anxiety (BAI)
Cruwys et al. (2014b), Study 1	Shared activities	52	SGPPD	0.17	Community members participating in recreation group	AU	3 months (1 session per week)	Doosje et al. (1995): 4 items	Depression (DASS-21: DS)

Haslam et al. (2014a)	Group-relevant decision making	36	IGPPD	0.49	Care home residents	UK	1 week (3 sessions of 30-60-minutes duration each)	Doosje et al. (1995) & Haslam (2004): 5 items	Cognitive Health (ACE-R)
Haslam et al. (2014b)	Reminiscence	40	SGPPD*	-0.03	Older adults residing in congregated living communities (independent living, retirement living, or assisted care)	CA	6 weeks (1 session of 30-minutes duration per week)	Aron et al. (1992): 1 item	Cognitive Health (SAGE)
	Effect size b	40		-0.09					Anxiety (GAI-SF)
	Effect size c	40		0.15					Quality of Life (SWLS)
Dingle et al. (2015)	Therapy program	52	SGPPD	1.60	Residents of a drug and alcohol therapeutic community	AU	Nominal length of residential program was 6 months (average length of stay was 99 days)	Doosje et al. (1995): 4 items	Quality of Life (SWLS)
Haslam et al. (2016)	Therapy program	51	IGPPD	0.39	University students experiencing social isolation or distress	AU	5 sessions of 60-75-minute duration each (first 4 sessions were weekly and the last session one month later)	Postmes et al. (2013): 4 items	Depression (DASS-21: DS)
	Effect size b	51		0.32					Anxiety (DASS-21: AS)
	Effect size c	51		0.20					Stress (DASS-21: SS)
	Effect size d	51		0.32					Quality of Life (SWLS)
	Effect size e	51		0.76					Self-Esteem (SISE)
Meuret et al. (2016)	Therapy program	169	SGPPD	2.60	Adult outpatients diagnosed with social anxiety disorder	US	12 weeks (1 session of 2.5-hours duration each per week)	Schubert & Otten (1992): 1 item	Anxiety (LSAS)
Scarf et al. (2016)	Shared activities	180	IGPPD	0.73	High school students participating in sea	NZ	10 days (continuous)	Sheldon & Bettencourt	Well-Being (RS)

					voyage			(2002): 3 items ^{S1} ; Ellemers et al. (1999): 3 items ^{S2}	
Heath et al. (2017)	Group-relevant decision making	88	IGPPD	0.87	Community members in residential program	UK	6 weeks (1 session of 90-minutes duration each per week)	Cameron (2004): 6 items ^{S1} ; Cameron (2004): 7 items ^{S2}	Self-Esteem (RSE)
	Effect size b	88		-0.03					Well-Being (BRS)
	Effect size c	88		1.75					Quality of Life (SWLS)
Scarf et al. (2017)	Shared activities	180	IGPPD	0.47	High school students participating in sea voyage	NZ	10 days (continuous)	Sheldon & Bettencourt (2002): 3 items	Well-Being (RS)
Haslam et al. (2018b)	Therapy program	82	IGPPD	0.34	Adults with a mental health diagnosis (predominantly major depression and anxiety disorder)	AU	5 sessions of 60-90-minute duration each (first 4 sessions were weekly and the last session one month later)	Postmes et al. (2013): 4 items	Depression (DASS-21: DS)
	Effect size b	82		0.55					Anxiety (M-SPIN)
	Effect size c	82		1.10					Physical Health (GP-V)
Haslam et al. (2018c)	Therapy program	205	SGPPD	0.94	Residents of a drug and alcohol therapeutic community	AU	Data collected at commencement of treatment (mean of 22 days since admission)	Doosje et al. (1995): 4 items ^{S1} ; Sellers et al. (1998): 1 item ^{S2}	Quality of Life (ATOP-QoL)
	Effect size b	205		0.71					Stress (K-10)
Mertens et al. (2018)	Group-relevant decision making	80	IGPPD	0.25	Members of recreational (amateur) basketball teams	BE	30 weeks (3-4 sessions per week)	Cameron (2004): 12 items	Stress (ABM)
	Effect size b	80		0.49					Physical Health (CDC HRQOL-

Effect size c		80		0.35					PH) Well-Being (CDC HRQOL- SM)
Effect size d		80		0.31					Physical Health (CDC HRQOL- EL)
Scarf et al. (2018), Study 1	Shared activities	173	IGPPD	0.39	High school students participating in sea voyage	NZ	10 days (continuous)	Sheldon & Bettencourt (2002): 3 items	Self-esteem (LESC)
Scarf et al. (2018), Study 2	Shared activities	171	IGPPD	0.41	High school students participating in sea voyage	NZ	10 days (continuous)	Sheldon & Bettencourt (2002): 3 items	Self-esteem (SDQ-III-SF)
Gee et al. (2019)	Shared activities	12	IGPPD	1.67	University students self-identifying as stressed, anxious, or depressed	UK	5 weeks (one session per week)	Postmes et al. (2013): 4 items	Depression (HADS: DS)
Koni et al. (2019), Study 1	Shared activities	136	SGPPD	0.74	Adolescents participating in sea voyage	NZ	10 days (continuous)	Sheldon & Bettencourt (2002) & Ellemers et al. (1999): 6 items	Well-Being (RS)
Koni et al. (2019), Study 2	Shared activities	91	SGPPD	0.76	Adolescents (self-identifying Māori and New Zealand-European) participating in a sea voyage	NZ	7 days (continuous)	Sheldon & Bettencourt (2002) & Postmes et al. (2013): 4 items	Well-Being (RS)
Williams et al. (2019)	Shared activities	19	SGPPD	0.07	Adults with a chronic mental health condition	AU	10 months (with bi-weekly sessions)	Doosje et al. (1995): 4 items	Well-Being (WEMWBS)

Note. *N* = total sample size comprising participants from treatment and control groups contributing to summary mean effect size; SGPPD = Single group pre-post design; SGPPD* = Single group pre-post design (study included several intervention groups and so design treated as single group pre-post design); IGPD = Independent groups post design; IGPPD = Independent groups pre-post design. US = United States; AU = Australia; UK = United Kingdom; CA = Canada; NZ = New Zealand; BE = Belgium; *Hedges' g* = bias-corrected standardized mean effect size; *Effect size_i* = effect size associated with one of multiple outcomes in a sample. S1 = Social Identification Scale 1; S2 = Social Identification Scale 2; SSES – State Self Esteem Scale (Heatherton & Polivy, 1991); DASS-21: DS = Depression Anxiety and Stress Scales: Depression Subscale (Lovibond & Lovibond, 1995); DASS-21: AS = Depression Anxiety and Stress Scales: Anxiety Subscale (Lovibond & Lovibond, 1995); DASS-21: SS = Depression Anxiety and Stress Scales: Stress Subscale (Lovibond & Lovibond, 1995); HADS: DS = Hospital Anxiety and Depression Scale: Depression Subscale (Zigmond & Snaith, 1983); HADS: AS = Hospital Anxiety and Depression Scale: Anxiety Subscale (Zigmond & Snaith, 1983); QoL: ADS = Quality of Life in Alzheimer’s Disease Scale (Logsdon, Gibbons, McCurry, & Terry, 1999); LIS = Life Improvement Scale (Haslam, Holme, Haslam, Iyer, Jetten, & Williams, 2008); QoLCS = Quality of Life Change Scale (Haslam, O’Brien, Jetten, Vormedal, & Penna, 2005); ACE-R = Addenbrooke’s Cognitive Examination – Revised (Mioshi, Dawson, Mitchell, Arnold, & Hodges, 2006); PHSRS = Physical Health Self-

Report Scale (Spector, Allen, Poelmans, Cooper, Bernin et al., 2005); QoLOB = Quality of Life Observational Measure (Fitzpatrick, Gitelson, Andereck, & Mesbur, 2005); Psychological PCSRS = Psychological Comfort Self-Report Scale (Vischer, 2005); QoL-4 = Quality of Life 4-Item Scale (Jetten, Haslam, Pugliese, Tonks, & Haslam, 2010); GP-C = Number of General Practitioner Calls; LS-SI = Life Satisfaction – Single Item Scale (Andrews & Whitney, 1976); IES-R = Impact of Events Scale – Revised (Weiss & Marmar, 1997); ZSRDS = Zung Self-Rating Depression Scale (Zung, Richards, & Short, 1965); BAI = Beck Anxiety Inventory (Beck, Epstein, Brown, & Steer, 1988); SAGE = Self-Administered Gerocognitive Examination (Scharre, Chang, Murden, Lamb, Beversdorf et al., 2010); GAI-SF = Geriatric Anxiety Inventory-Short Form (Byrne & Pachana, 2011); SWLS = Satisfaction With Life Scale (Diener, Emmons, Larsen, & Griffin, 1985); SISE = Single-Item Self-Esteem Scale (Robins, Hendin, & Trzesniewski, 2001); LSAS = Liebowitz Social Anxiety Scale (Liebowitz, 1987); RS = Resilience Scale (Wagnild & Young, 1993; Neill & Dias, 2001); RSE = Rosenberg’s Self-Esteem Scale (Rosenberg, 1965); BRS = Brief Resilience Scale (Smith, Dalen, Wiggins, Tooley, Christopher, & Bernard, 2008); M-SPIN = Mini-Social Phobia Inventory (Connor, Kobak, Churchill, Katzelnick, & Davidson, 2001); GP-V = Number of General Practitioner Visits; ATOP-QoL = Australian Treatment Outcomes Profile – Quality of Life Scale (Ryan, Holmes, Hunt, Dunlop, Mammon, Holland et al., 2014); K-10 = Kessler’s Psychological Distress Scale (Kessler, Andrews, Colpe, Hiripi, Mroczek, Normand et al., 2002); ABM = Athlete Burnout Measure (Raedeke & Smith, 2001); CDC HRQOL-PH = Centers for Disease Control and Prevention Health Related Quality of Life Measure – Physical Health Subscale (Centers for Disease Control and Prevention, 2000); CDC HRQOL-SM = Centers for Disease Control and Prevention Health Related Quality of Life Measure – State of Mind Subscale (Centers for Disease Control and Prevention, 2000); CDC HRQOL-EL = Centers for Disease Control and Prevention Health Related Quality of Life Measure – Energy Levels Subscale (Centers for Disease Control and Prevention, 2000); LESC = Life Effectiveness Self-Concept Subscale (Richards, Ellis, & Neil, 2002); SDQ-III-SF = Self-Description Questionnaire III – Short Form (Marsh & O’Neill, 1984); WEMWBS = Warwick-Edinburgh Mental Wellbeing Scale (Tennant et al., 2007).

Table 3

Meta-analytic results for the effectiveness of social identification-building interventions as a function of type of intervention.

<i>Intervention type</i>	<i>k</i>	<i>ES</i>	<i>N</i>	Hedges' <i>g</i>	95% CIs	<i>t</i>	Heterogeneity 80% Pred Int
Group-relevant decision making	5	13	275	1.26	[0.61, 1.91]	3.89 ^{***}	0.23, 2.28
Therapy programs	6	14	651	1.03	[0.44, 1.61]	3.52 ^{***}	0.02, 2.04
Shared activities	13	19	1143	0.40	[-0.01, 0.81]	1.96	-0.57, 1.38
Reminiscence	3	10	161	-0.05	[-0.87, 0.76]	0.13	-1.13, 1.02

Note. *k* = number of independent samples; *ES* = number of effect sizes; *N* = number of participants. Hedges' *g* = bias-corrected standardized mean effect size; 95% CIs = 95% confidence intervals for standardized mean difference; *t* = *t*-value corresponding to standardized mean difference; Heterogeneity 80% Pred Int = heterogeneity statistic indicating 80% prediction interval for likely effect size of new study.

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

Table 4

Meta-analytic results for the effectiveness of social identification-building interventions as a function of health outcome.

<i>Health outcome</i>	<i>k</i>	<i>ES</i>	<i>N</i>	Hedges' <i>g</i>	95% CIs	<i>t</i>	Heterogeneity 80% Pred Int
Quality of life	8	11	604	0.81	[0.39, 1.24]	3.83 ^{***}	-0.29, 1.91
Physical health	5	6	278	0.77	[0.25, 1.29]	2.98 ^{**}	-0.35, 1.89
Self-esteem	5	5	527	0.69	[0.11, 1.27]	2.38 [*]	-0.44, 1.82
Well-being	9	9	845	0.67	[0.20, 1.14]	2.84 ^{**}	-0.44, 1.82
Anxiety	8	8	544	0.61	[0.15, 1.07]	2.68 ^{**}	-0.50, 1.72
Depression	8	8	399	0.58	[0.11, 1.05]	2.49 [*]	-0.53, 1.69
Cognitive health	4	4	175	0.55	[-0.02, 1.13]	1.94	-0.58, 1.68
Stress	5	5	398	0.49	[-0.04, 1.01]	1.86	-0.63, 1.61

Note. *k* = number of independent samples; *ES* = number of effect sizes; *N* = number of participants. Hedges' *g* = bias-corrected standardized mean effect size; 95% CIs = 95% confidence intervals for standardized mean difference; *t* = *t*-value corresponding to standardized mean difference; Heterogeneity 80% Pred Int = heterogeneity statistic indicating 80% prediction interval for likely effect size of new study.

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

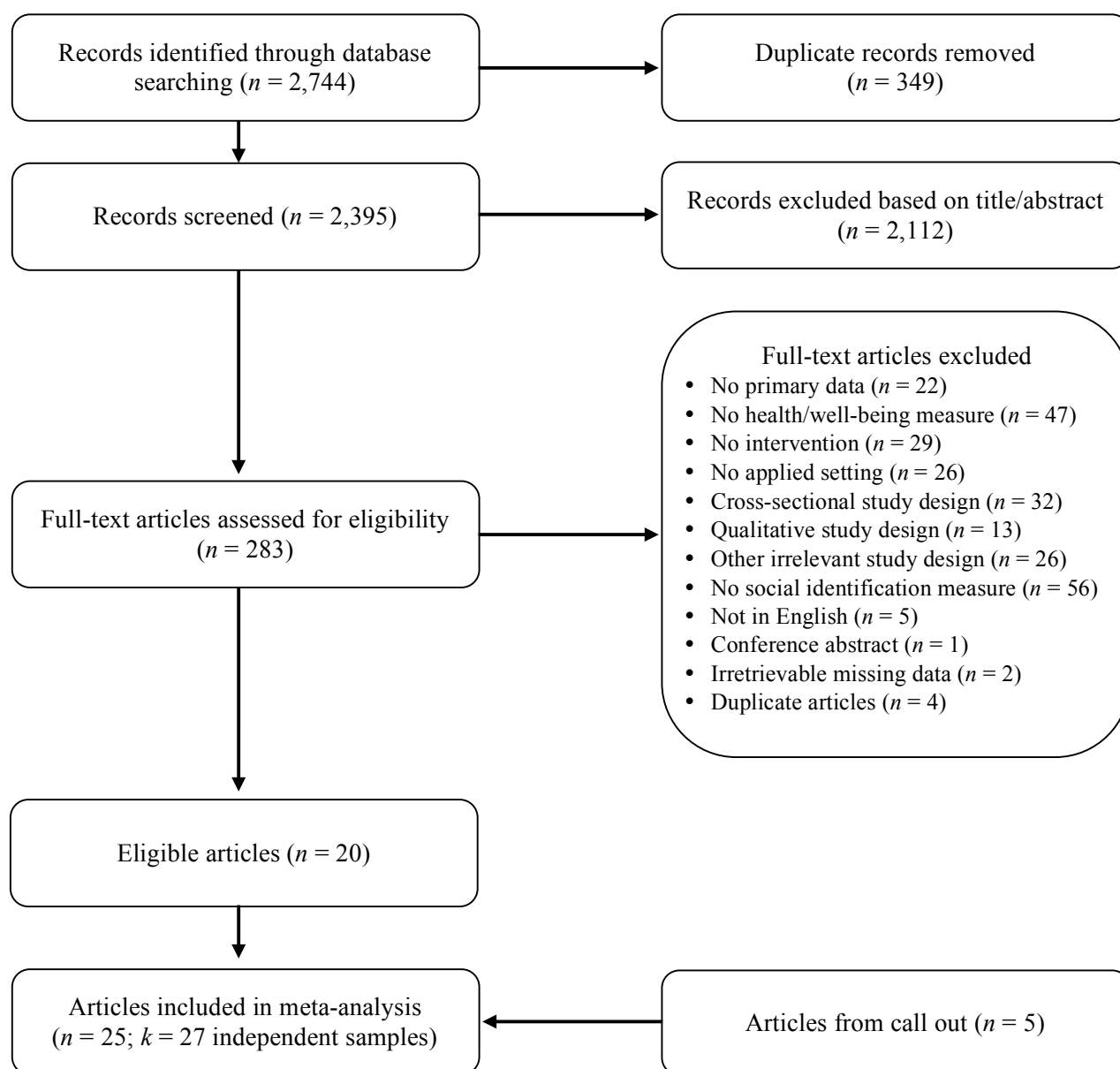


Figure 1. Flow chart displaying search strategy including identification and selection of final sample (based on layout from PRISMA statement; Moher et al., 2009).

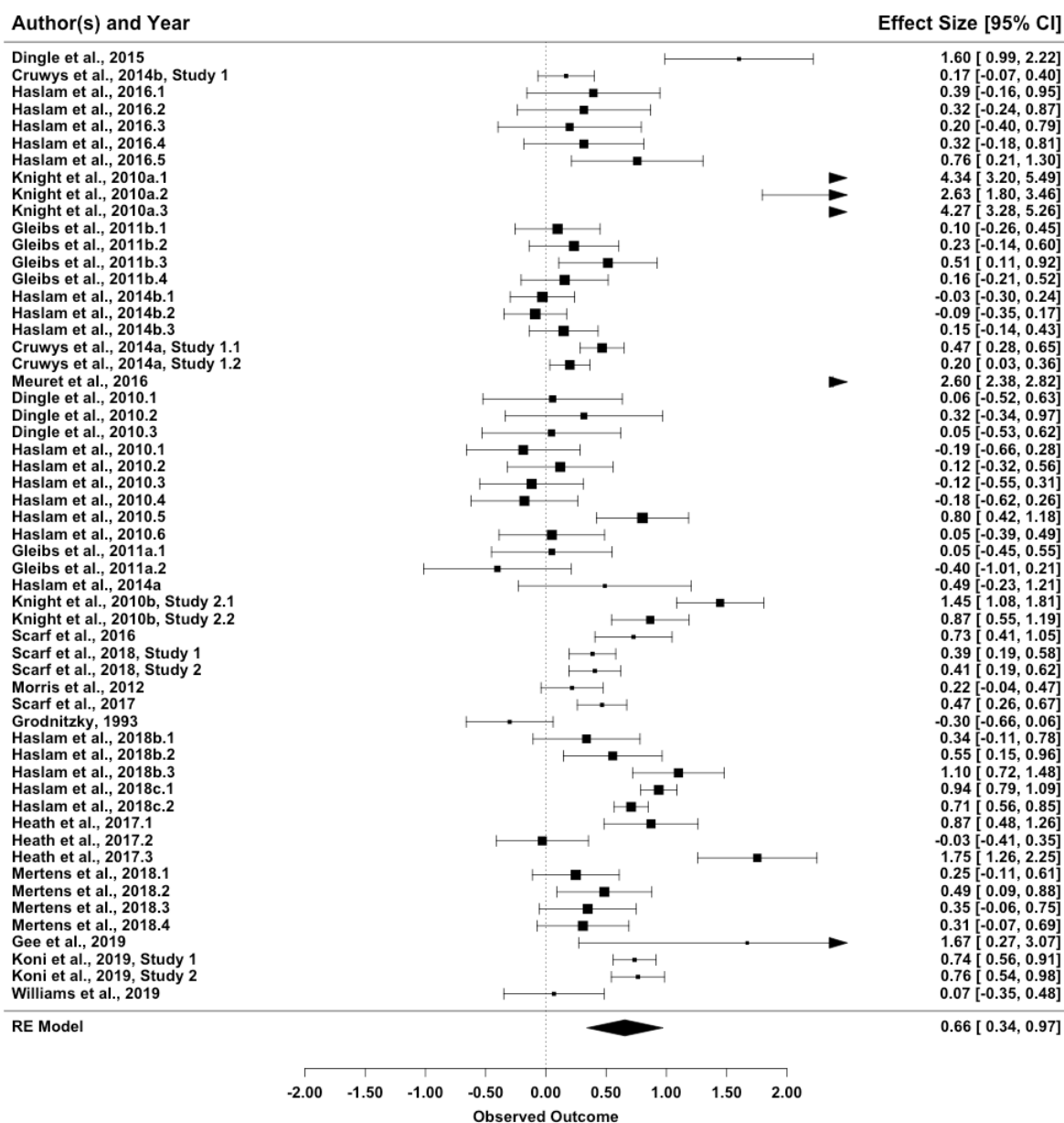


Figure 2. Forest plot displaying effect sizes and confidence intervals and weights in assessing summary effect size (bias-corrected standardized mean difference, Hedges’ *g*). On the x-axis, positive values to the right side of the line indicate an effect of the intervention on improved health outcomes, while negative values to the left of the line indicate an effect on impoverished health outcomes.

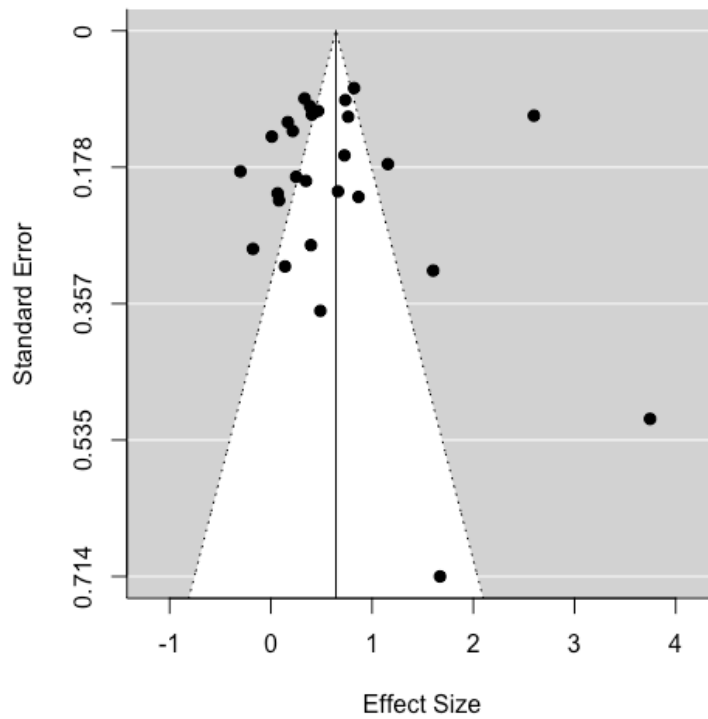


Figure 3. Funnel plot displaying relationship between effect size (Hedges' g) and standard error.

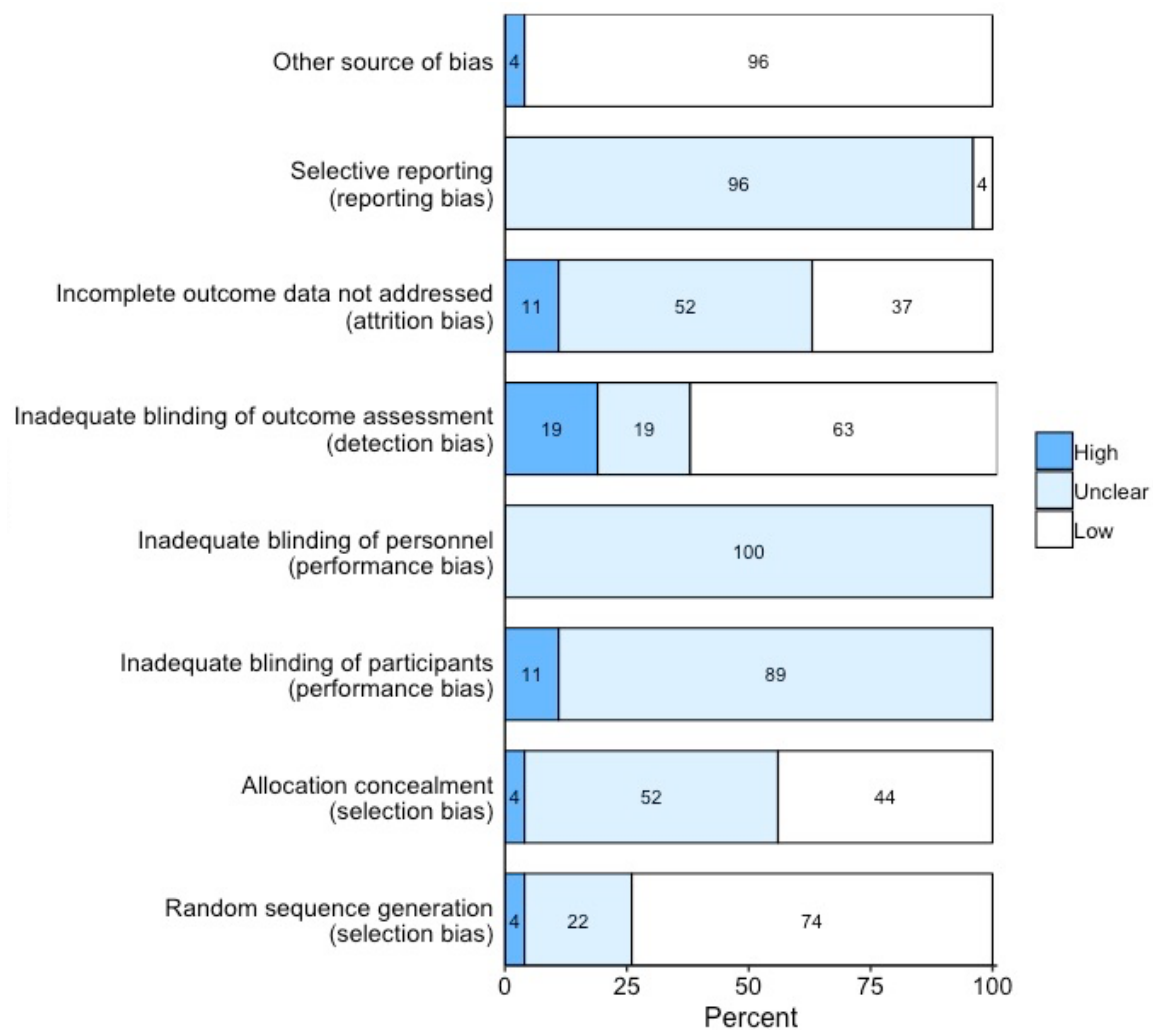


Figure 4. Bar diagram displaying risk of bias analysis in key domains across studies (based on Cochrane risk of bias tool; Higgins et al., 2011).