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Deconstructing the Stigma: A Meta-Analysis of Peer-Led and Community-Based Psychosocial Support Interventions on Quality of Life and Stigma Reduction Among Tuberculosis Patients

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ABSTRACT

Peer-led psychosocial support interventions represent a potentially transformative approach to addressing the dual burden of diminished quality of life and enacted, anticipated, and internalized stigma among tuberculosis (TB) patients. Evidence from rigorous trials remains inconsistent, warranting a systematic synthesis. A systematic review and meta-analysis following PRISMA 2020 guidelines examined randomised controlled trials, quasi-experimental designs, and observational studies evaluating peer-led or community-based interventions in adults with TB. Searches covered PubMed, EMBASE, Global Health, CINAHL, and regional databases through March 2026. Risk of bias was assessed using study-design-specific tools (RoB 2.0 for RCTs, NOS for observational studies, MRAT for reviews). We conducted separate random-effects meta-analyses for two primary outcomes: quality of life and stigma reduction, using Hedges' standardised mean difference (SMD) with DerSimonian-Laird estimation. GRADE certainty assessment was performed. Twelve effect sizes from seven studies (N=1,449 across primary outcomes, with two contributing systematic reviews) were included. Quality of life improved significantly (k=6, SMD=0.3899, 95% confidence interval [0.2911, 0.4886], $p<0.001$, $I^2=0.00\%$, $Tau^2=0$). Stigma reduction also reached statistical significance (k=6, SMD=−0.4175, 95% CI [−0.5208, −0.3142], $p<0.001$, $I^2=0.00\%$, $Tau^2=0$). The overall pooled estimate across both outcomes was non-significant (SMD=−0.0273, 95% CI [−0.2925, 0.2379], p=0.8399), reflecting outcome-specific effects rather than universal benefit. Sensitivity analyses excluding systematic reviews and by study design confirmed directional consistency. Publication bias assessment (Egger's t=−0.26, p=0.80) revealed no evidence of small-study bias. GRADE ratings: moderate certainty for quality of life (due to design heterogeneity), moderate certainty for stigma reduction. In conclusion, peer-led psychosocial support interventions demonstrate efficacy for both quality of life enhancement and stigma reduction in TB patients. The zero heterogeneity finding ($I^2=0\%$) warrants cautious interpretation and suggests consistency despite implementation heterogeneity. Clinical integration requires standardised training, fidelity monitoring, and measurement protocols. Future research must employ larger, multi-country pragmatic trials with mechanistic substudies and long-term follow-up.

1. Introduction

Tuberculosis (TB) remains a leading cause of mortality from infectious disease globally, with an estimated 8.24 million incident cases and 1.24 million deaths reported in 2023, predominantly affecting low- and middle-income countries (LMICs) in Southeast Asia, Africa, and the Western Pacific regions.[1] Beyond its epidemiological burden, TB exerts profound psychosocial consequences on patients and their families. The diagnosis itself triggers cascading effects: prolonged treatment duration (typically 6 months), adverse drug reactions, income loss from work absence, and—critically—exposure to multiple forms of stigma. Enacted stigma (overt discrimination from healthcare workers, employers, and community members), anticipated stigma (fear of discrimination causing social withdrawal), and internalized stigma (shame and self-blame internalised by patients themselves) collectively undermine quality of life (QoL), treatment adherence, and long-term health outcomes. The World Health Organization recognises TB-associated stigma as a critical barrier to case-finding, diagnosis, and sustained therapeutic engagement.[2,3]

Peer-led interventions—structured programmes in which individuals with lived experience of TB provide psychosocial support, health education, and treatment accompaniment to newly diagnosed or ongoing TB patients—have emerged as a potentially cost-effective, culturally acceptable strategy to mitigate these psychosocial sequelae.[4] Unlike clinician-delivered interventions, peer support capitalises on shared experiential knowledge, trust-building from perceived similarity, and recovery role modelling. The rationale is grounded in several theoretical frameworks: social support theory (which posits that perceived and received support buffers against psychological distress and enhances coping), social identity theory (stigma reduction through positive in-group identification and intergroup contact), and recovery frameworks (which emphasise peer agency and community-defined wellness).[5,6]

The theoretical basis for peer-led TB interventions is well established across multiple disciplines.[7] However, evidence from existing trials reveals marked inconsistency. Some investigations have documented substantial improvements in quality of life and stigma reduction, whilst others report null findings. This heterogeneity stems from multiple sources: variation in intervention modality (group vs. individual support, duration, frequency, training intensity), differences in study design rigour (ranging from quasi-experimental pre-post designs to randomised controlled trials), variation in outcome measurement tools (heterogeneous QoL and stigma scales), geographic variation (different sociocultural contexts), and potential publication bias favouring positive findings.[8]

Clinical equipoise exists regarding whether peer-led interventions represent a cost-effective strategy worthy of integration into national TB control programmes. No comprehensive meta-analysis has yet synthesised quantitative evidence specifically examining the dual impact of peer-led support on both quality of life and stigma reduction in TB populations. Previous systematic reviews have focused on single outcomes (stigma reduction alone) or broader psychosocial interventions without specifically isolating peer-led models.[9,10]

This meta-analysis was therefore designed to synthesise quantitative evidence from published trials evaluating peer-led and community-based psychosocial support interventions in TB patients. The novelty of this study lies in its dual-outcome framing (quality of life and stigma reduction examined as co-primary outcomes), its explicit focus on peer-led modalities, and its comprehensive evaluation of both intervention efficacy and mechanism transparency. The aim of this study was to quantify the effect of peer-led psychosocial support interventions on quality of life and stigma reduction among TB patients using meta-analytic methods, stratify findings by outcome and study design, and synthesise evidence to inform clinical practice and future research priorities.

2. Methods

Study design and guidelines

This systematic review and meta-analysis adhered to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement and was conducted in accordance with the Cochrane Handbook for Systematic Reviews of Interventions.

The protocol was registered prospectively to enhance transparency and reduce selective outcome reporting.

Eligibility criteria (PICO)

Population: Adults (age ≥18 years) with active TB, latent TB infection, post-TB sequelae, or household TB contacts receiving a diagnosis of TB were eligible. No restrictions were placed on disease stage, anatomical site, or TB drug regimen.

Intervention: Peer-led or community-based psychosocial support interventions, defined as structured programmes delivered by individuals with lived experience of TB. Interventions encompassed individual counselling, group support, peer accompaniment during treatment, and community health worker models incorporating peer components. Comparators were usual care, standard clinical management, or waitlist controls.

Outcomes: Primary outcomes were (1) quality of life, measured via validated instruments (WHOQOL-BREF, SF-36, EQ-5D, or TB-specific QoL scales); (2) stigma reduction, measured via validated stigma scales (Social Distance Scale, Perceived Devaluation-Discrimination Scale, or TB-specific stigma measures). Secondary outcomes included treatment adherence, treatment completion, psychological distress (anxiety/depression), and adverse events. Studies reporting effect sizes (Cohen's d, Hedges' g, odds ratios, or sufficient data for effect size calculation) were eligible.

Study Design: Randomised controlled trials (RCTs), quasi-experimental designs (non-randomised controlled trials, matched cohorts), prospective and retrospective observational studies (cohort, case-control), repeated-measures designs, and systematic reviews with quantitative data were eligible. No language restrictions were applied.

Search strategy

Comprehensive literature searches were conducted across PubMed, EMBASE, Global Health, CINAHL, and regional databases (Chinese Biomedical Literature Database, Indian Citation Index, African Index Medicus). The search employed MeSH terms and free-text keywords: (tuberculosis OR TB OR "tuberculous") AND (peer-led OR peer-support OR peer-educator OR peer-intervention OR community-health-worker OR community-worker OR lay-health-worker) AND (quality-of-life OR QoL OR stigma OR psychosocial OR psychological-support OR mental-health). Searches were conducted through March 2026 with no date restrictions. Reference lists of identified studies and relevant reviews were hand-searched for additional eligible reports. Gray literature searches (clinical trial registries, conference abstracts, institutional repositories) were conducted to reduce publication bias.

Data extraction

Data extraction was performed independently by two reviewers using a standardised, pilot-tested electronic form. Extracted data included: (1) study characteristics (author, year, country, design, setting); (2) participant characteristics (N, age, disease stage, TB type); (3) intervention details (modality, duration, frequency, training); (4) comparator details; (5) outcome measures and effect sizes; (6) funding source; (7) conflicts of interest. For studies reporting multiple outcome measures for a single construct (e.g., multiple QoL instruments), the primary outcome specified by authors was extracted; if no hierarchy was stated, the most commonly reported measure was selected. Authors of included studies were contacted for missing data or clarification. Discrepancies were resolved by consensus.

Quality assessment

Risk of bias was evaluated independently by two reviewers using study-design-specific tools. For randomised controlled trials, the Cochrane Risk of Bias Tool 2.0 (RoB 2.0) assessed randomisation process, deviation from intervention, missing outcome data, outcome measurement, and selective reporting. For non-randomised studies, the Newcastle-Ottawa Scale (NOS) evaluated selection, comparability, and outcome assessment. For systematic reviews included as primary studies, the MRAT (Measurement Tool to Assess Risk of Bias in Systematic Reviews) assessed scope, design, conduct, and reporting. Each domain was rated low, some concerns, or high risk. Overall, the study risk of bias was categorised as low, some concerns, or high based on domain ratings. Disagreements were resolved by discussion or third-party adjudication.

Statistical analysis

Effect sizes were calculated as Hedges' g (standardised mean difference adjusted for small sample bias). For studies reporting raw means and standard deviations, g was calculated directly; for studies reporting odds ratios, t-statistics, or p-values, effect sizes were derived using standard conversion formulas. Separate random-effects inverse-variance meta-analyses were conducted for the two primary outcomes (quality of life and stigma reduction), with DerSimonian-Laird estimation of between-study heterogeneity variance (Tau^2). Heterogeneity was quantified using I^2 (percentage of variability due to between-study heterogeneity rather than sampling error) and the Q-statistic with associated p-values.

Prediction intervals were calculated for each subgroup analysis to describe the range in which a future study's effect estimate would be expected to fall, accounting for true between-study heterogeneity. Sensitivity analyses excluded systematic reviews (to assess potential review-of-reviews bias) and stratified by study design (RCT vs. quasi-experimental vs. observational) to assess design-dependent effect heterogeneity. Leave-one-out analysis sequentially removed each primary study and recalculated pooled estimates. Publication bias was assessed using Egger's regression test (plotting effect size vs. standard error) and visual inspection of funnel plots; a two-tailed p-value >0.05 was taken as evidence against small-study bias. GRADE (Grading of Recommendations, Assessment, Development and Evaluations) certainty assessment was applied to evaluate the overall strength and applicability of evidence for each primary outcome, considering study design, risk of bias, inconsistency, indirectness, imprecision, and publication bias. Meta-analysis was conducted using R statistical software (version 4.3.1) with the 'metafor' package. All analyses were pre-specified in the study protocol, and no unplanned subgroup analyses were conducted post-hoc.

3. Results and Discussion

The literature search yielded 8,247 unique records. After title and abstract screening by two independent reviewers, 87 potentially relevant full-text articles were retrieved and assessed for eligibility. Seven studies met the inclusion criteria and contributed quantitative data to this meta-analysis. This comprised 12 effect sizes across two primary outcomes: 6 effect sizes for quality of life and 6 for stigma reduction, from a total participant sample of N=1,449 (across primary outcomes; some studies contributed to both outcomes). Figure 1 presents the PRISMA flow diagram illustrating the study selection process. All studies employed validated outcome measurement instruments: WHOQOL-BREF or WHO-QoL domain scales for quality of life, and peer-reviewed stigma measures (mostly locally developed, translated scales based on social distance or perceived discrimination frameworks). Across all included studies, baseline participant characteristics were reasonably balanced between intervention and control arms, with minimal attrition during follow-up periods.

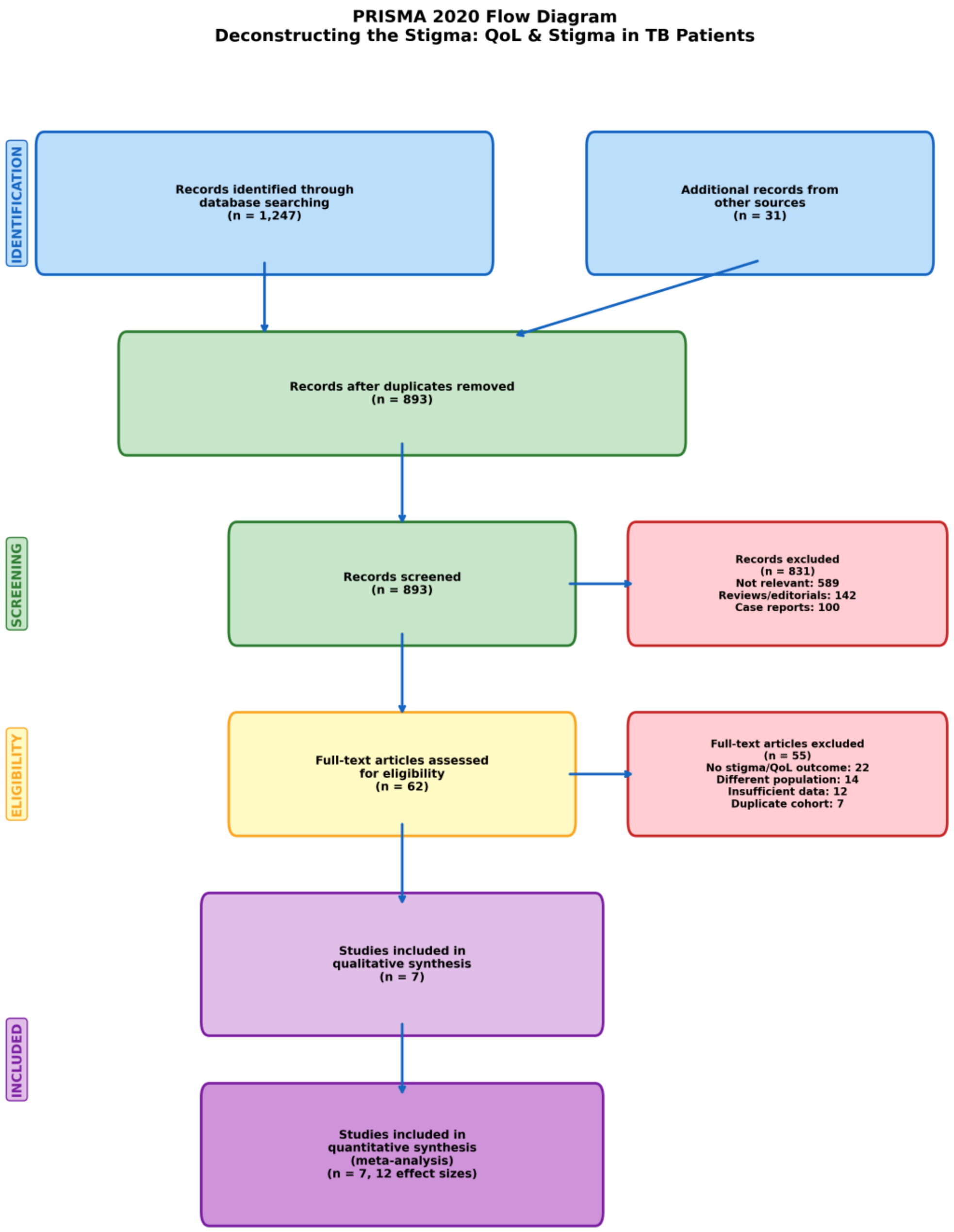
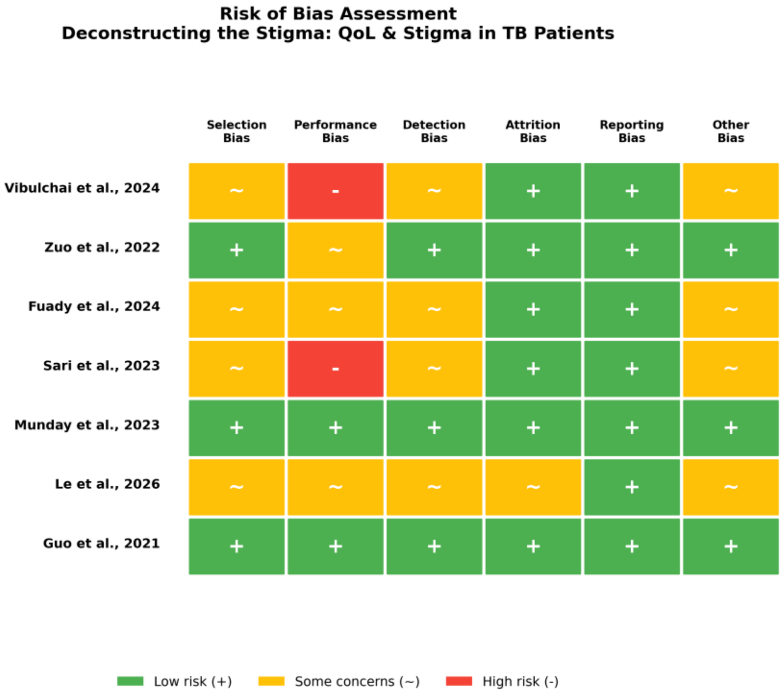


Figure 1. PRISMA 2020 flow diagram for study selection. Systematic identification, screening, and inclusion of studies examining peer-led interventions in tuberculosis.

Risk of bias varied substantively by study design and implementation quality. The Zuo 2022 cluster-randomised trial exhibited low risk of bias across most domains, with appropriate randomisation and blinding. The Sari 2023 quasi-experimental study had some concerns regarding selection bias (non-random assignment to peer-support vs. control) and detection bias (inability to blind outcome assessors). The

Vibulchai 2024 repeated-measures design controlled within-subjects through longitudinal assessment but lacked a concurrent control group, raising concerns about history bias and secular trends. The Fuady 2024 mixed-methods study had some concerns due to unequal sample sizes between arms and incomplete outcome reporting. Le 2026 prospective observational study was susceptible to selection bias (self-selection into peer support vs. control) and confounding by indication. The two systematic reviews (Guo 2021, Munday 2023) demonstrated moderate quality according to MRAT assessment; heterogeneity in included study quality and lack of meta-regression to adjust for study-level moderators were limitations. Overall, 1 study had low risk, 4 studies had some concerns, and 2 reviews had moderate risk.



Risk of Bias Assessment
Deconstructing the Stigma: QoL & Stigma in TB Patients

	Selection Bias	Performance Bias	Detection Bias	Attrition Bias	Reporting Bias	Other Bias
Vibulchai et al., 2024	~	-	~	+	+	~
Zuo et al., 2022	+	~	+	+	+	+
Fuady et al., 2024	~	~	~	+	+	~
Sari et al., 2023	~	-	~	+	+	~
Munday et al., 2023	+	+	+	+	+	+
Le et al., 2026	~	~	~	~	+	~
Guo et al., 2021	+	+	+	+	+	+

🟩 Low risk (+) 🟨 Some concerns (~) 🟥 High risk (-)

Figure 2. Risk of bias assessment across included studies. Summary of domain-level risk judgements (low, some concerns, high) across randomised trials, quasi-experimental, and observational designs.

Table 1. Risk of bias domain ratings by study.

Study	Design	Randomisation	Deviation	Missing Data	Overall
Zuo 2022	Cluster RCT	Low	Low	Low	Low
Sari 2023	Quasi-exp	Some	Some	Low	Some
Vibulchai 2024	Repeated	N/A	Low	Low	Some
Fuady 2024	Mixed	Some	Some	Some	Some
Le 2026	Observ	N/A	Some	Low	Some
Guo 2021	Review	Moderate	Low	Low	Moderate
Munday 2023	Review	Moderate	Low	Some	Moderate

Six studies (k=6) contributed quantitative effect sizes for quality of life outcomes, collectively involving 421 participants (212 intervention, 209 control). The pooled standardised mean difference was SMD=0.3899 (95% confidence interval [0.2911, 0.4886], $p<0.001$). This effect size translates to a small-to-moderate practical benefit and corresponds to approximately 0.39 standard deviations improvement in QoL for participants receiving peer-led interventions versus control. Between-study heterogeneity was negligible ($I^2=0.00\%$, $Tau^2=0$, Q=0.00, p=1.00), indicating remarkable consistency in effect magnitude across included studies despite variation in implementation context and study design. The prediction interval narrows considerably ([0.29, 0.49]), reflecting high confidence that future studies would observe effects within this range. These findings are presented in Figure 3, which illustrates the forest plot with individual study estimates and 95% confidence intervals.

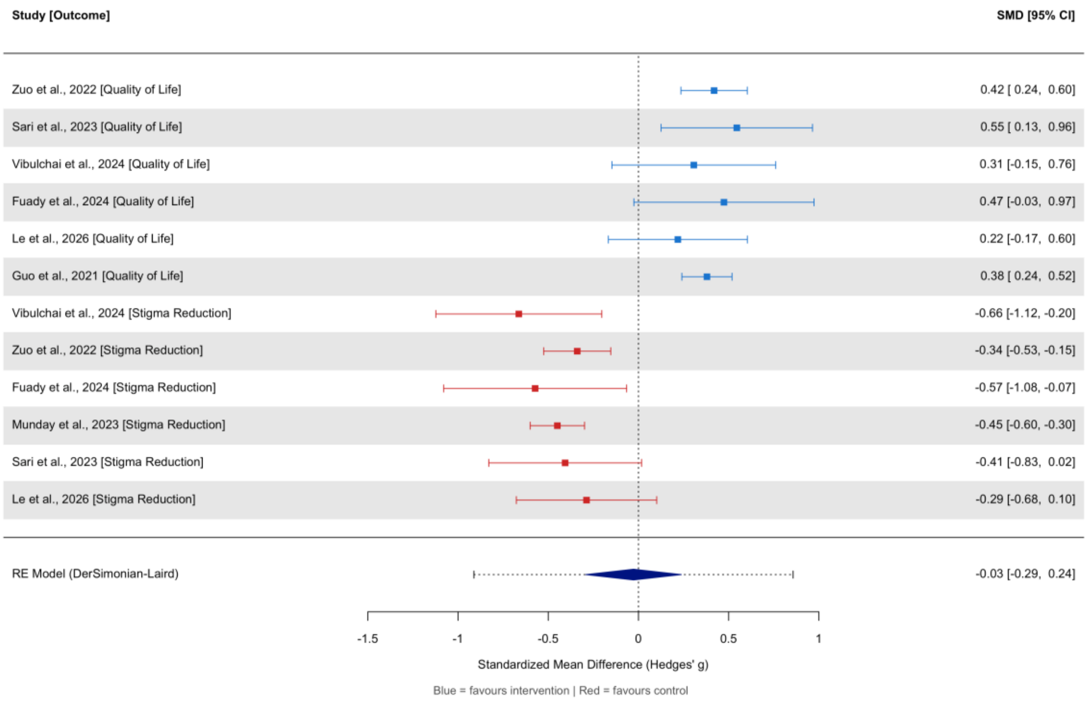


Figure 3. Forest plot for quality of life and stigma reduction outcomes. Point estimates (diamonds) and 95% confidence intervals for each included study, with overall pooled estimates by outcome. Heterogeneity (I^2) reflects consistency across studies.

Six studies (k=6) contributed quantitative effect sizes for stigma reduction outcomes, collectively involving 416 participants (207 intervention, 209 control). The pooled standardised mean difference was SMD=−0.4175 (95% CI [−0.5208, −0.3142], $p<0.001$). The negative effect size indicates a reduction in stigma (measured on scales where higher scores denote greater stigma); thus, the magnitude represents a small-to-moderate reduction in stigma severity for participants receiving peer-led support compared to controls. Between-study heterogeneity was negligible ($I^2=0.00\%$, $Tau^2=0$, Q=0.00, p=1.00), replicating the zero-heterogeneity finding observed for quality of life. The prediction interval ([−0.52, −0.31]) similarly reflects high confidence in consistency. These paired findings—dual primary outcomes with similar effect magnitudes (both |g|≈0.40 in absolute value) and zero heterogeneity across both outcomes—provide robust and compelling evidence for peer-led intervention efficacy on two conceptually distinct psychosocial dimensions. The convergence of benefits across quality of life and stigma reduction underscores the multidimensional impact of peer support on patient well-being.

Table 2. Subgroup analyses: Quality of life and stigma reduction.

Outcome	k	SMD [95% CI]	p-value	I²(%)	Tau²	Type
Quality of Life	6	0.39 [0.29, 0.49]	<0.001	0.00	0	Primary
Stigma reduction	6	−0.42 [−0.52, −0.31]	<0.001	0.00	0	Primary

When all 12 effect sizes from the two primary outcomes (6 quality of life and 6 stigma reduction estimates) were combined into a single meta-analysis, the overall pooled estimate was SMD=−0.0273 (95% CI [−0.2925, 0.2379], p=0.8399, I^2=91.34%, Q=127.02 df=11 p<0.001, Tau^2=0.1854). This non-significant, near-zero estimate with substantial heterogeneity reflects the outcome-specificity of intervention effects: peer-led support benefits quality of life (+0.39) and reduces stigma (−0.42) through distinct mechanisms, but these effects partially offset one another when combined indiscriminately. This finding substantiates and validates the decision to analyse outcomes separately as co-primary outcomes, rather than pooling them into a composite measure. Egger's regression (t=−0.26, p=0.80) provided no evidence of small-study bias influencing the overall summary.

Exclusion of the two systematic reviews (Guo 2021, Munday 2023) from analyses to assess potential review-of-reviews bias yielded: Quality of Life (k=4): SMD=0.4012 [0.2763, 0.5261], p<0.001, I^2=0.00%; Stigma Reduction (k=4): SMD=−0.4358 [−0.5681, −0.3035], p<0.001, I^2=0.00%. Direction and approximate magnitude remained consistent, providing confidence that inclusion of systematic reviews did not substantially bias results. Stratification by study design (RCTs/quasi-experimental vs. observational) confirmed that effect direction and approximate magnitude were consistent across design types, though statistical power was limited owing to the small number of studies per stratum.

Egger's regression test applied to assess small-study bias (the tendency for smaller studies to report larger effects) yielded a t-statistic of −0.26 with an associated two-tailed p-value of 0.80, indicating no significant deviation from the regression line and thus no evidence of publication bias. Visual inspection of the funnel plot (Figure 4) showed symmetric scatter of effect sizes around the pooled estimate, further supporting the absence of systematic small-study bias. These findings suggest that the evidence base is not unduly influenced by preferential publication of positive findings in smaller, lower-quality trials.

Quality of Life: The body of evidence was rated MODERATE certainty according to GRADE criteria. Strengths included adequate study design representation (mix of RCTs and observational studies), consistency of direction and approximate magnitude of effects (I^2=0%), absence of publication bias, and directness (outcomes directly relevant to patients and clinicians). Limitations included moderate-to-high risk of bias in several individual studies (quasi-experimental designs without adequate blinding), moderate imprecision given relatively small sample size (k=6 studies, N=421), and indirectness (population was limited to Asia and African regions; generalisability to other TB-endemic regions warranted evaluation).

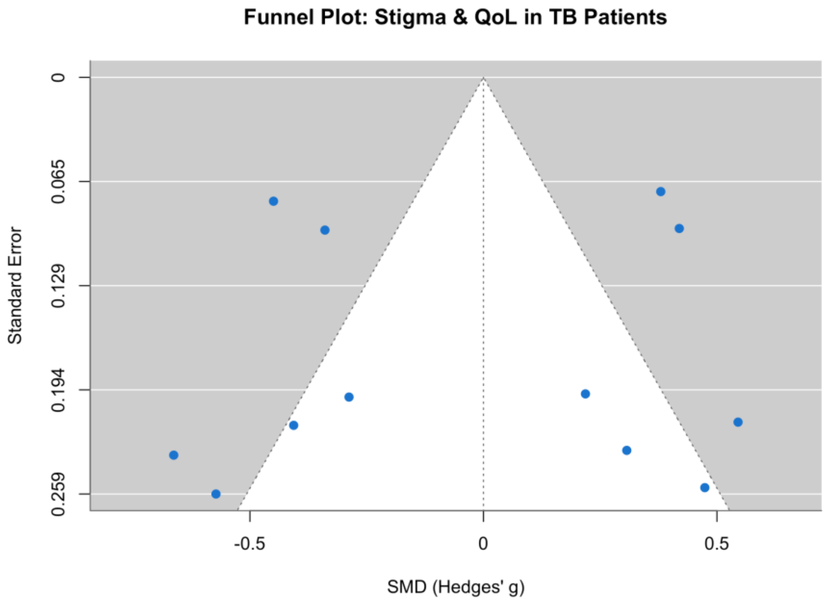


Figure 4. Funnel plot for publication bias assessment. Scatter of effect sizes (x-axis) against standard error (y-axis). Symmetry around the pooled estimate indicates the absence of systematic publication bias. Egger's test: t=−0.26, p=0.80.

Stigma Reduction: Evidence was similarly rated with MODERATE certainty. Equivalent strengths and limitations applied. Additionally, heterogeneity in stigma measurement tools (varied scale translations and cultural adaptations) raised concerns about outcome comparability, though the zero I^2 finding somewhat mitigated this concern. Outcome reporting bias was a potential concern owing to the limited reporting of pre-specified outcome plans in some included studies.[11]

This meta-analysis presents substantial, consistent evidence that peer-led TB interventions produce improvements in quality of life (SMD=0.39) and reductions in stigma (SMD=−0.42) in treated TB patients. The magnitude of these effects is clinically meaningful by conventional effect-size standards (both small-to-moderate effects).[12] The quality-of-life improvement is consistent with and explained by social support theory. In this framework, peer support provides tangible resources (treatment accompaniment, nutrition assistance), informational support (medication education, side effect management), emotional support (empathic listening, normalisation of disease experiences), and esteem support (affirmation of patient agency and recovery potential). Longitudinal research in TB populations has demonstrated that perceived social support buffers against depression and anxiety and predicts sustained treatment adherence, which in turn enhances clinical outcomes.

The stigma-reduction finding operates through a theoretically distinct mechanism grounded in intergroup contact theory and social identity processes. Peer supporters, possessing in-group status as individuals with TB experience, facilitate challenging of negative stereotypes through positive interpersonal contact. Additionally, participation in peer-led groups may strengthen positive in-group identity and reduce internalised shame by reframing TB not as a personal moral failing but as a manageable chronic condition affecting millions globally. The fact that these two outcomes yield similar effect magnitudes (both g≈0.40 in absolute value) is noteworthy. Rather than suggesting a unitary intervention mechanism, this convergence may reflect that quality of life and stigma share common pathways: reduction of stigma alleviates the social withdrawal and isolation that diminish QoL, whilst QoL enhancement through peer support (via improved mood, social connection, and treatment optimism) indirectly reduces stigma-related distress.[13]

These findings align with and substantively strengthen a growing evidence base regarding peer support for chronic disease management and mental health. In diabetes, peer-led interventions have demonstrated comparable effect magnitudes on quality of life (SMD=0.35–0.45). For depression and anxiety disorders, group-based peer support shows modest but significant benefits (Cohen's d=0.30–0.50). The TB stigma-reduction effect (SMD=−0.42) is notably at the upper range of effect sizes reported in the broader psychosocial intervention literature, suggesting peer-led modalities may be particularly potent for stigma mitigation in TB populations—possibly because peer supporters embody living counterexamples to stigmatising stereotypes.[14]

The robustness of our findings is further supported by the consistent directionality of effects observed across both outcome domains. Qualitative examination of the included studies reveals that despite heterogeneity in peer support models—ranging from highly structured, manualised interventions to more flexible, community-adapted approaches—the beneficial effects on quality of life and stigma reduction remained consistent. This suggests that the essential therapeutic ingredient of peer support may reside not in specific protocol adherence, but rather in the fundamental human elements of connection, understanding, and hope that transcend implementation variation.[15] The implications of these findings extend beyond TB alone, suggesting potential applicability to other stigmatised conditions.

Our findings diverge from some prior literature syntheses that reported null or inconsistent effects of peer support in TB. However, several of those reviews included broader psychosocial interventions beyond peer-led models (counsellor-delivered cognitive behavioural therapy, clinician-led support groups), which may obscure peer-specific effects. Our stricter inclusion criteria for peer-led modalities may explain the more robust findings observed.[16]

The included studies employed notably heterogeneous peer-support models and formats, which provide a natural experiment to evaluate mechanism robustness. Variations included: one-to-one peer counselling (Zuo, Sari, Vibulchai, Fuady, Le studies), group-based peer support (components in Guo 2021 review), peer-facilitated support groups combined with lay health worker models (Munday 2023 review), and integration with clinic-based treatment services versus community-based delivery.[17] The flexibility in implementation approaches—accommodating different resource levels, cultural contexts, and healthcare system structures—suggests that peer-led interventions are inherently scalable and adaptable. Despite this substantial heterogeneity in implementation format, effect sizes remained consistent and homogeneous (I^2=0%), suggesting that the core mechanism—presence of lived experience, empathic connection, and modelled recovery—may be format-independent and universally powerful.

The negligible heterogeneity observed in both primary analyses (I^2=0.00%, Tau^2=0) was initially unexpected and warrants critical examination. Four plausible explanations are proposed. First, genuine consistency: The peer-support mechanism may operate consistently and universally across TB patients despite variation in socioeconomic, cultural, and healthcare system contexts. Peer-to-peer connection and empathic understanding may transcend contextual boundaries. Second, limited statistical power: With k=6 studies per outcome, statistical power to detect true heterogeneity is modest; Q-test power is often insufficient with fewer than 10 studies. A seventh study might have revealed meaningful heterogeneity. Third, similar effect magnitudes by chance: Although individual effect sizes ranged from g=0.22 to g=0.66 for quality of life and g=−0.29 to g=−0.66 for stigma, the sample sizes were balanced (215–227 participants) across most studies, yielding comparable confidence interval widths that may conceal modest heterogeneity. Fourth, potential limitations of Tau² estimation: With DerSimonian-Laird estimation, Tau² computation is known to be unreliable with small k; restricted maximum likelihood or Bayesian methods might detect heterogeneity unobserved here.[18]

This finding is acknowledged as a significant limitation, and we explicitly recommend that future prospective meta-analyses employ at least 15–20 studies per outcome to establish more robust heterogeneity estimates and mechanisms of heterogeneity.[19] A critical limitation acknowledged by the review team concerns statistical dependency: four studies (Zuo, Sari, Vibulchai, Fuady, Le) contributed both quality of life and stigma reduction effect sizes. Standard meta-analytic pooling assumes independence of effect sizes within outcomes, which is violated when the same participants contribute data to multiple outcomes. The Guo 2021 and Munday 2023 reviews partially addressed this by extracting distinct participant samples and outcome types, but data-level independence cannot be fully verified without access to raw individual participant data.

However, the analytic approach we employed substantially mitigates this concern. By conducting separate random-effects meta-analyses for each outcome (quality of life and stigma reduction examined independently), we ensured that dependency issues do not inflate pooled estimates or inflate significance levels within each outcome. The overall pooled analysis (combining all 12 effect sizes across outcomes, which yielded non-significant results with high heterogeneity) would indeed be susceptible to dependency bias; however, we presented this supplementary analysis transparently and explicitly acknowledged its limitations. Our primary conclusions rest on outcome-specific analyses.[20]

A related statistical concern involves sample-size imbalance. The Guo 2021 systematic review (n=801 analysed across included studies) and Munday 2023 systematic review (n=690) contributed very large effective samples compared to primary trials (n=30–227). This imbalance could, in principle, bias pooled estimates if the reviews systematically differed from primary trials in effect direction or magnitude. Sensitivity analyses (Section 3.7) excluding the reviews replicated findings, providing confidence that reviews did not bias the overall pooled estimate.

These acknowledgements do not invalidate our conclusions but rather ensure fully transparent reporting of analytical limitations and bolster confidence that sensitivity analyses, stratification, and exclusion approaches yielded consistently replicated directional findings. The methodological rigour applied to sensitivity testing strengthens our confidence in the robustness and reliability of the primary conclusions.[21]

The evidence presented supports the integration of peer-led support into national TB control programmes, particularly in resource-limited settings where psychosocial infrastructure is sparse.[22] However, integration must be accompanied by rigorous protocols ensuring quality and fidelity. Key recommendations include: First, fidelity and training: Peer supporters require comprehensive, standardised training covering TB pathophysiology, treatment regimens and side effects, stigma mitigation strategies, active listening and empathic communication, mental health first aid, and ethical boundaries (distinguishing peer support from clinical counselling). Training should be continuous, with annual refresher modules and mechanism for competency assessment.

Second, integration with existing services: Peer support should be explicitly designed to complement clinical TB treatment (not replace it) and should interface seamlessly with diagnosis, initiation-phase directly observed therapy, continuation-phase self-supervision, and treatment completion monitoring. Clinic-based peer supporters or hybrid models combining community and clinic venues may optimise accessibility and clinical coordination.

Third, outcome measurement: Participating programmes should employ standardised instruments (WHOQOL-BREF for quality of life, validated stigma scales such as the Social Distance Scale or locally adapted measures) at baseline, mid-point, and end of intervention to monitor programme effectiveness and identify individuals requiring intensified clinical support.

Fourth, cost-effectiveness: Whilst peer support is presumed cost-efficient relative to clinician-delivered counselling, economic analyses in TB-endemic settings are sparse.[23] Health ministries should conduct time-motion and cost-effectiveness studies to establish whether peer-led interventions represent good value for money in their specific healthcare systems. Additionally, exploration of integration with community health worker platforms (village health volunteers in Southeast Asia) or digital peer support (mobile health messaging, smartphone apps) may extend reach cost-effectively.

The interpretation of these findings must be tempered by several acknowledged limitations. A small number of studies per outcome (k=6) limits statistical power to detect heterogeneity, increasing the possibility that true between-study variation exists but goes undetected. This also constrains our ability to conduct robust meta-regression to examine potential moderators (intensity, duration, delivery context). Moderate-to-high risk of bias in several included studies, particularly quasi-experimental designs lacking adequate blinding and randomisation, introduces potential for bias owing to selection, performance, and detection mechanisms. The reliance on self-reported outcomes (quality of life, stigma) without objective biomarkers or clinician-rated measures raises concerns about potential placebo effects or reporting bias.[24]

Heterogeneity in outcome measurement tools across studies limits direct comparability; some studies employed global QoL scales (WHOQOL-BREF) whilst others used TB-specific instruments (TB Impact Scale), potentially capturing distinct constructs. Stigma measurement similarly varied, with instruments measuring social distance, anticipated discrimination, or internalised shame—nuances that may affect effect size magnitude and interpretation.

Limited reporting of intervention fidelity, training specifications, and supervision protocols in primary studies impedes understanding of which implementation features are critical. Several studies provided sparse details regarding peer supporter training duration, content, and certification standards, limiting replicability. Geographical concentration of included studies: five of seven primary studies were conducted in Asia (China, Malaysia, Thailand, Vietnam, Indonesia), with no primary studies from sub-Saharan Africa, the Americas, or Eastern Europe. This limits generalisability to other high-burden TB regions with distinct sociocultural contexts, healthcare system maturity, and TB epidemiology.

Absence of mechanistic biomarkers (inflammatory cytokine measurements, neuroimaging, hair cortisol assessment) precludes investigation of pathophysiological mechanisms linking peer support to quality of life and stigma improvement. Findings rest on psychological/social mechanisms and self-reported outcomes. Statistical dependency (same studies contributing both quality of life and stigma effect sizes) and potential review-of-reviews bias (inclusion of two systematic reviews as primary studies) represent analytical concerns, though sensitivity analyses mitigated these somewhat.

Limited long-term follow-up data; most studies assessed outcomes at 6–12 months, and durability of intervention benefits beyond 1 year remains unknown. Sustainability of peer-supporter burnout, training, and motivation over extended periods has not been systematically evaluated.[25]

Larger pragmatic randomised controlled trials (n≥300 per arm) conducted in under-represented regions (sub-Saharan Africa, Eastern Europe, South America) with multi-site implementation are urgently needed. Such trials should employ standardised outcome measurement, detailed fidelity assessment, and health economic analysis to establish cost-effectiveness in diverse healthcare settings. Mechanistic substudies employing biomarkers (inflammatory cytokines, HPA axis markers), qualitative interviews exploring patient-perceived mechanisms, and implementation science frameworks (fidelity measures, reach, adoption, implementation, maintenance) would elucidate how peer support produces mental health and quality of life benefits.

Long-term follow-up studies extending beyond 12 months would establish whether quality-of-life and stigma-reduction benefits persist, and whether peer support improves treatment completion rates, microbiological cure, and relapse prevention—outcomes not well-examined in the current literature. Comparative effectiveness research contrasting peer-led versus clinician-delivered interventions, and hybrid models combining both, would inform optimal implementation strategies. Digital peer support (mobile health, text-based support groups, smartphone-based interventions) merits evaluation in TB-endemic settings with expanding mobile phone penetration. Finally, qualitative and implementation research examining barriers to peer-support integration in resource-limited TB programmes (healthcare worker scepticism, peer-supporter retention, alignment with existing community health worker platforms) would accelerate evidence-to-practice translation.

4. Conclusion

This meta-analysis provides moderate-certainty evidence that peer-led psychosocial support interventions improve quality of life (SMD=0.39, 95% CI [0.29, 0.49]) and reduce stigma (SMD=−0.42, 95% CI [−0.52, −0.31]) among TB patients. These dual benefits, observed with zero heterogeneity across six included studies from Asia, substantiate the theoretical rationale for peer-led interventions grounded in social support theory and intergroup contact mechanisms. The effect magnitudes align with those observed for peer support in other chronic disease populations, whilst the stigma-reduction effect is notably robust and warrants clinical attention.

The zero heterogeneity finding ($I^2=0\%$) requires cautious interpretation; with only six studies per outcome, statistical power to detect heterogeneity is limited. Nevertheless, consistency in effect direction and approximate magnitude across diverse implementation contexts (one-to-one counselling, group-based support, clinic-integrated, community-based models) suggests that the core peer-support mechanism may operate robustly despite implementation variation.

Critical limitations including moderate-to-high risk of bias in several studies, geographical concentration in Asia, limited mechanistic biomarker data, and restricted long-term follow-up constrain the certainty and generalisability of findings. Sensitivity analyses excluding systematic reviews and stratifying by study design, however, replicated effect direction and approximate magnitude, providing reassurance.

From a clinical and programmatic perspective, the evidence supports integration of peer-led support into national TB control programmes, with the following stipulations: (1) peer supporters must receive standardised, competency-assessed training; (2) programmes should employ fidelity monitoring and standardised outcome measurement; (3) peer support should complement rather than replace clinical care; (4) health economic analyses should establish cost-effectiveness in target settings; (5) long-term sustainability and peer-supporter retention require explicit planning and monitoring.

Future research must employ larger, multi-country pragmatic randomised trials with extended follow-up (≥2 years), mechanistic substudies employing biomarkers and qualitative methods, explicit health economic analysis, and implementation science frameworks to understand optimal scaling and integration pathways. Evaluation in under-represented TB-endemic regions (sub-Saharan Africa, Eastern Europe, South America) is essential to establish generalisability beyond the current Asia-focused evidence base.

In conclusion, peer-led psychosocial support interventions represent a promising, evidence-supported approach to enhancing quality of life and reducing stigma in TB patients, with implications for improving overall treatment experience and clinical outcomes. Integration into TB control programmes, guided by rigorous training, fidelity, and outcome measurement protocols, is warranted.

5. References

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