

Digital Appendix Systematic Cochrane Review and Meta-Analysis on Psychological Interventions to Foster Resilience in Healthcare Professionals

Appendix D17 Additional Information

Appendix D17.1 Differences Between Protocol and Review (Including Postprotocol Amendment) –

Taken From the Cochrane Review (Kunzler, Helmreich, Chmitorz, et al., 2020)

All modifications to the methods specified in the protocol (Helmreich et al., 2017) are described in the following section.

1. Title

- a. We changed the title of the review due to the posthoc restriction to healthcare professionals (see Types of participants).

2. Background

- a. Due to the posthoc restriction to healthcare professionals, we adapted the Background sections (Description of the condition, Description of the intervention; How the intervention might work; Why it is important to do this review) for this target group, for example, by adding current references about previous systematic reviews in this population.
- b. Description of the intervention
 - i. We modified the first sentence of this section to illustrate that we refer to resilience intervention research in general.
 - ii. To be consistent, we limited the number of references for each psychotherapeutic method to one reference.
- c. How the intervention might work
 - i. We changed the order of appearance of the different psychotherapeutic approaches, to present theoretical approaches that are associated with cognitive-behavioral therapy (CBT), such as stress inoculation and problem-solving therapy, immediately after CBT, before discussing other theoretical approaches.

- ii. Instead of performing a subgroup analysis on the target group of training, we had planned to conduct a subgroup analysis of the training intensity, and added arguments for whether participants could benefit differently from differing training intensities.
 - iii. To derive the planned subgroup analysis for the theoretical foundation, we modified the section by describing the recent results of Joyce et al. (2018) who analyzed the impact of theoretical foundations of resilience intervention for the first time. At the time of writing the protocol, this systematic review had not been published.
 - d. Why it is important to do this review
 - i. Compared to the protocol, we presented the need for doing this review by integrating the results of recently-published systematic reviews in clinical and non-clinical adult populations (e.g., Joyce et al., 2018).
3. Objectives
- a. We modified the objectives of the review to refer only to healthcare professionals, reflecting the posthoc restriction to this population.
4. Types of participants
- a. **Posthoc change - step 1:** Initially, we planned to include clinical and non-clinical populations (e.g., patients, employees, students, military) in this review (see Helmreich et al., 2017). Based on a broad search strategy in October 2016, we identified 251 studies and 18 ongoing studies evaluating resilience-training programmes in a variety of target groups. To be able to manage the large number of studies with many divergent target groups, we decided to **de-scope the review** based on the populations investigated. This also allowed us to perform top-up searches (i.e., for the period October 2016 to the present) that were specific for the respective target groups. We took the decision to de-scope the review for **two main reasons**. First, using the database of 269 studies found by searches in October 2016, the review that we had originally planned might have been

trumped by the publication of studies since this time point. Studies published since 2016, for example, using innovative delivery formats or different therapeutic methods than earlier studies might possibly have affected the results of subgroup analysis of the review (e.g., concerning delivery format). In addition, we expected a substantial number of new studies fulfilling the eligibility criteria of the review for the period end 2016 to 2019. Especially since 2015, there has been a significant growth of publications in this field. For example, by searching additional sources (e.g., reference lists, trial registers) or study protocols published until October 2016, we identified 26 RCTs published between 2017 and 2018. We also found 16 completed studies that had not yet been published (e.g., manuscripts in preparation or under review). Second, the RCTs identified were spread across a large number of comparisons (e.g., different target groups, theoretical foundations of interventions, or control groups), which might have over-scoped the review and resulted in substantial heterogeneity if we had included all 269 studies. Based on the number of studies identified for the healthcare sector (including healthcare professional like physicians, healthcare students like medical students, and employees associated with the healthcare sector such as human service professionals) in October 2016, we therefore decided to focus on RCTs in these populations. In June 2019, we performed top-up searches focusing broadly on the healthcare sector, in order to guarantee a review of high credibility, which synthesizes the latest evidence on the efficacy of psychological resilience interventions in this group at the time of publication.

- b. **Posthoc change - step 2:** Based on the top-up searches in June 2019, we identified 31 studies and eight ongoing studies that had been performed in healthcare professionals, healthcare students, and employees associated with the healthcare sector. Combining these with the original search, we found a total of 80 RCTs and nine ongoing studies in this population. During the process of writing up this review, and the editorial process, we made further decisions about the eligibility criteria of this review for the “Types of

participants". We **further separated** the pool of 80 RCTs into two groups: **1) healthcare professionals** (i.e., with completed training) and **2) healthcare students**. On the evidence base from the two searches, this review refers to healthcare professionals. In addition, we also considered studies with mixed samples, where healthcare professionals were included as a subgroup (total: 44 RCTs and five ongoing studies). A second review (Kunzler, Helmreich, König, et al., 2020) refers to psychological interventions to foster resilience in healthcare students (total: 30 RCTs and three ongoing studies). We took the decision for the following empirical reasons. First, when summarizing the 80 RCTs in a first review draft, we identified a substantial amount of heterogeneity for these studies that could only be partially explained by the planned subgroup analyses. Similarly, during internal peer review, the question arose about whether the research question of such a review (i.e., including healthcare professionals, students, and different employees associated with healthcare sector) was too broad and the studies too heterogeneous to combine in the same review. We therefore decided to split the review into two publications, one for healthcare professionals and one for healthcare students, in order to create two very focused reviews that are based on sufficiently homogenous studies, are up-to-date, and provide a concise summary of the evidence for the reviews' readers. A second rationale behind the decision to further split the data is the stressor exposure in the two groups of healthcare professionals and healthcare students: Students and qualified staff have different stressor exposures and responsibilities, which might moderate the effect of resilience training. While healthcare professionals are exposed to stressors such as shift work, medical decision-making, or hierarchies, students are confronted with different kind of stressors (e.g., exams, challenging subjects). Thus, a split between these two groups seemed reasonable. Based on both searches, we identified six studies in employees widely associated with the healthcare sector, whose samples were too heterogeneous (e.g., human service professionals, nurse managers) to combine them

with healthcare professionals (e.g., physicians, nurses), that are mostly employed in clinical practice and patient care. In addition, it is questionable whether these employees can be viewed as front-line healthcare staff and should be included in a review of this target group. We therefore decided to omit this group of studies.

- c. Based on these two post hoc changes, which we had discussed with the Cochrane Developmental, Psychosocial, and Learning Problem (CDPLP) editorial team, we adapted the “Types of participants” section accordingly, to specify that this review covered healthcare professionals (i.e., healthcare staff delivering direct medical care such as physicians, nurses, hospital personnel) and allied healthcare staff working in allied health professions distinct from medical care (e.g., psychologists, social workers, counsellors, physical therapists, occupational therapists, speech therapists, medical assistants, medical technicians). Since we also identified several eligible studies in mixed samples, we stated that would include these mixed samples in the review and also included them in meta-analyses, provided we could obtain the data from the study authors for healthcare professionals as a discrete group.

5. Types of interventions

- a. We stated in the protocol that we planned to include also broader, health-promoting interventions (e.g., well-being therapy; Helmreich et al., 2017), but in the full review we included only studies that explicitly defined the aim of fostering resilience, hardiness, or post-traumatic growth by using one or more of these terms in the publications. We made this modification on the basis of a postprotocol amendment in consultation with the CDPLP editorial team and the Cochrane Editorial and Methods Department. During the initial process of literature extraction, we realized that it was not feasible to consider all health-promoting interventions that aim to foster resilience in a broader sense (e.g., mental health, well-being, psychological adaptation in a population with stressor exposure) without including the terms resilience, hardiness, or post-traumatic growth, for

the following reasons. First, it appeared very difficult to decide between which of the very large number of interventions should be included in the review and which should not, since the relationship of the interventions to the concept of resilience was not made explicit in those interventions. This would have left the review authors having to make many assumptions with no objective criteria, resulting in reduced traceability of selection criteria and potentially low inter-rater reliability at the end of screening. Second, since the objective of the review was to synthesize the current evidence on the efficacy of resilience trainings, including broader interventions could have biased the review's conclusions, as fostering resilience was not explicitly formulated as aim in any of those interventions.

6. Types of outcome measures

- a. Based on a suggestion during the internal peer review, we added adverse events as a primary outcome of this review, and marked it with an asterisk for inclusion in the "Summary of findings" table.

7. Electronic searches

- a. We planned to perform searches in October 2016 for a review on psychological resilience interventions in clinical and non-clinical populations. However, due to post hoc modification of the inclusion criteria, the search processes for the review was based on a two-step approach, with searches performed in October 2016 and top-up searches in June 2019.
- b. We expanded the description of the search process by adding details about using the Cochrane Highly Sensitive Search Strategy for MEDLINE and Embase, as specified in the Cochrane Handbook (Lefebvre et al., 2019), in order to present the search strategy in sufficient detail.
- c. We searched the Web of Science Core Collection databases simultaneously rather than individually (Science Citation Index; Social Science Citation Index; Conference Proceedings

Citation Index - Social Science & Humanities; Conference Proceedings Citation Index - Science), since our institutional access to this database only offered this possibility.

8. Selection of studies

- a. We judged the feasibility of selection criteria after 500 instead of 50 studies screened due to the large number of records yield by the searches for this review.

9. Assessment of risk of bias in included studies

- a. We described that we considered the achieved baseline comparability between study conditions as part of selection bias (random-sequence generation) in addition to the standard Risk of bias domains in the Cochrane Handbook (Higgins et al., 2011). We had extracted this additional information from the included studies and judged it to be interesting for the readers of this review.

10. Measures of treatment effect

- a. Continuous data: In the protocol, we said we would calculate standardized mean difference (SMD) effect sizes because resilience-training studies are likely to use different measures for resilience and related constructs. We therefore added a sentence about the variation in measurement scales between included studies and referred to Table 2 and Table 3 (see D13.3 and D13.4 in this thesis) on the outcome scales used. We added the information on how we interpreted the magnitude of effect sizes (SMDs) for continuous outcomes in the review.

11. Unit of analysis issues

- a. Repeated observations on participants: We explained when we judged posttest in interventions studies as postintervention assessment and considered them in the respective meta-analyses. Assessments at more than one week after the end of training were declared as posttest by some study authors, although interim events between the end of the intervention and the assessment might have affected the effects measured.

We wished to differ between such assessments and “real” posttests with greater proximity to the end of training (i.e., within one week after the intervention ended).

12. Dealing with missing data

- a. We supplemented the procedure of dealing with missing data in the review by explaining how we would handle missing (summary outcome) data in studies of mixed samples. We added this information as we also considered studies with mixed samples in the review (see point 4 above).
- b. We added a sentence explaining how we managed missing/incomplete summary outcome data, as well as missing outcome data due to attrition.

13. Assessment of heterogeneity

- a. We added a sentence explaining that we discuss the similarities and differences between the included studies (e.g., in terms of study characteristics) in the Results and Discussion section of the review.
- b. We further described the conventions used to interpret I^2 values on the basis of suggestions in the Cochrane Handbook (Deeks et al., 2019), and added information about conducting subgroup analyses to investigate substantial heterogeneity ($I^2 \geq 50\%$), in order to be transparent.
- c. We added a sentence explaining that we calculated the 95% prediction intervals from random-effects meta-analyses to present the extent of between-study variation according to the Cochrane Handbook (Deeks et al., 2019).

14. Assessment of reporting bias

- a. We had stated in the protocol that we would assess potential publication bias by drawing and inspecting funnel plots. In the review, we expanded the description of this process by adding the information of analyzing reporting bias on the basis of at least 10 studies in the meta-analysis for one outcome.

- b. We inspected contour-enhanced funnel plots for the primary outcomes, as they offer more graphical possibilities to detect a potential publication bias than standard funnel plots.

15. Data synthesis

- a. For several studies in the review that provided no means and *SDs*, but provided alternative data to calculate SMDs and the respective standard error, we described the procedure for combining these with other studies using the generic invariance method in RevMan. This information had been missing from the protocol (Helmreich et al., 2017).
- b. We expanded the description of how we managed scales for depression and burnout, as well as scales for general well-being or quality of life and work-related measures in the same study, because several included studies fitted this case.
- c. We did not conduct a network meta-analysis, as planned, due to the insufficient evidence base in the review.

16. Summary of findings

- a. We added information about the inclusion of the primary outcomes at posttest in the “Summary of findings” table. We took the decision to restrict the outcomes to those assessed at posttest following internal peer review. Adverse events are also included here (see Types of outcome measures and point 6 in this section).
- b. We replaced the term “quality of the evidence” with “certainty of the evidence” throughout the review, in order to be consistent with current guidelines and preferences in the literature (Hultcrantz et al., 2017).
- c. We provided further details concerning the downgrading of studies for each of the five GRADE criteria (study limitations, indirectness, inconsistency, imprecision, publication bias).
- d. We also explained how we interpreted effect sizes and rated the certainty of the evidence, as this information had been missing from the protocol (Helmreich et al., 2017).

17. Subgroup analysis and investigation of heterogeneity

- a. We omitted the preplanned subgroup analysis on “target group in resilience interventions”, due to the review's revised focused on healthcare professionals only.
- b. We added a post hoc analysis regarding training intensity; low-intensity trainings included interventions with a total duration of 5 hours or less or three sessions or fewer (if no duration in hours or minutes was indicated), moderate intensity referred to trainings that included more than 5 to 12 hours or less, or more than three to 12 sessions or fewer; and high intensity to programs of more than 12 hours or more than 12 sessions. We added this subgroup analysis post hoc due to the restriction to healthcare professionals (see point 2c).
- c. We added a subgroup for mobile-based delivery format to the preplanned analysis on delivery format, given the evidence found in this review.
- d. We changed “multimodal intervention” to “combined intervention” to the preplanned analysis on theoretical foundation, to refer to resilience interventions that were based on two or more explicit theoretical foundations such as CBT and ACT or CBT and mindfulness. We also added coaching, positive psychology, and unspecific resilience training. Non-specific training programs included resilience interventions fostering one or several resilience factors but without specifying any explicit theoretical foundation, or where the underlying framework could not be assigned to a certain theoretical foundation. We changed from “multimodal” to “combined intervention” in order to be consistent with other subgroup analyses (compare combined setting, combined delivery). We added the extra subgroups based on the evidence found in this review.
- e. Lastly, we added active and attention control to the preplanned analysis on comparator groups, in order to distinguish between these groups. Attention control groups referred to an alternative treatment that mimicked the amount of time and attention received (e.g., by the trainer) in the intervention group. We used the term “active control” for

alternative treatment (no standard care; for example, treatment developed specifically for the treatment study) but that did not control for the amount of time and attention in the intervention group, and was not attention control in a narrow sense. We made these changes due to the evidence found in this review.

- f. We added information about which subgroup analyses were prespecified and which were added posthoc.

18. Sensitivity analysis

- a. We provided more detail on the planned sensitivity analysis based on risk of bias (i.e., restriction to studies at low and unclear risk of attrition as well as at low and unclear risk of reporting bias, respectively), as this information had been missing from the protocol (Helmreich et al., 2017). We chose attrition and reporting bias as key domains for the sensitivity analyses, since most of the variability between included studies was evident in these domains.

Appendix D17.2 Unused Methods

The following table provides details of methods that had been planned and described in the protocol (Helmreich et al., 2017) but were not employed, as they were not required or not feasible.

Table D17.2.1

Unused Methods Taken From the Cochrane Review (Kunzler, Helmreich, Chmitorz, et al., 2020)

| Section | Proposed methods | Reason for non-use |
|------------------------------|---|--|
| Measures of treatment effect | Dichotomous data We planned to analyze dichotomous outcomes by calculating the risk ratio (RR) of a successful outcome (i.e., improvement in relevant variables) for each trial. We would have expressed uncertainty in each result using 95% confidence intervals (CIs). | We only identified two studies with dichotomous data for depression (West et al., 2014; West et al., 2015). Both studies also provided continuous primary outcome data relevant for this review (burnout) and could be combined in meta-analysis with other studies reporting continuous outcomes. |
| Unit of analysis issues | Cluster-randomized trials In cluster-randomized trials, if the clustering had been ignored and the unit of analysis had been different from the unit of allocation ("unit-of-analysis error"; Whiting-O'Keefe et al., 1984), <i>p</i> values might have been artificially small and resulted in false positive conclusions (Higgins et al., 2019). Had we found such cases, we would have accounted for clustering in the data and followed the recommendations given in the literature (Higgins et al., 2019; White & Thomas, 2005). For those cluster-randomized trials that did not report correct standard errors, we would have tried to recover correct standard errors by applying the usual formula for the variance inflation factor $1 + (M - 1) ICC$, where <i>M</i> is the average cluster size and <i>ICC</i> the intra-cluster correlation coefficient (Higgins et al., 2019). Had it not been possible to extract <i>ICC</i> values from the study, we would have used the average <i>ICC</i> of all cluster-randomized trials in our review that investigated the same primary outcome scale in a similar setting. Had this not been available, we would have used the average <i>ICC</i> of all other cluster-randomized trials in our review. If no such studies had been available, we would have used <i>ICC</i> = 0.05 as a conservative guess for the primary analysis, and added a sensitivity analysis using <i>ICC</i> = 0.10. We planned to conduct sensitivity analyses based on the unit of randomization as well as the <i>ICC</i> estimate in cluster-randomized trials (see Sensitivity analysis). | No cluster-RCT was included in this review. |
| | Multiple treatment groups Had multiple groups in a study been relevant, we | For studies with multiple treatment groups, we considered only one intervention group |

| Section | Proposed methods | Reason for non-use |
|---------------------------|--|--|
| | would have accounted for the correlation between the effect sizes from multi-arm studies in a pair-wise meta-analysis (Higgins et al., 2019). We would have treated each comparison between a control group and a treatment group as an independent study. We would have multiplied the standard errors of the effect estimates by an adjustment factor to account for correlation between effect estimates. In doing so, we would have acknowledged heterogeneity between different treatment groups. | to be relevant for the review and meta-analyses, on the basis of the independent judgement of two reviewers. Thus, in a pair-wise meta-analysis we did not have to account for the correlation between the effect sizes for multi-arm studies. |
| | If there had been an adequate evidence base, we would have considered performing a network meta-analysis (see Data synthesis). | The evidence base was not sufficient to conduct a network meta-analysis. |
| Dealing with missing data | If standard deviations could neither be recovered from reported results nor obtained from the authors, we would have considered single imputation by pooling within-treatment standard deviations from all other studies, provided that fewer than five studies had missing standard deviations. If more than five studies had missing standard deviations, we would have performed multiple imputation on the basis of the hierarchical model fitted to the non-missing standard deviations. | We found no studies using the same scale that had missing standard deviations. In addition, missing standard deviations could always be recovered from alternative statistical values or could be obtained from the study authors. |
| Data synthesis | Had a study reported more than one resilience scale, we would have used the scale with better psychometric qualities (as specified in Appendix 3 in Helmreich et al., 2017), to calculate effect sizes. | All studies measuring resilience only used one resilience scale. |
| | If a study had provided data from two instruments used equally often in the included RCTs, two review authors (AK, IH) would have identified the appropriate measure through discussion (compare Stoffers et al., 2012). | This did not occur in this review. |
| | Network meta-analyses would have been merely exploratory and would only have been conducted if the review results had a sufficient and adequate evidence base. | The evidence base was not sufficient to support a network meta-analysis. |
| | Network meta-analyses offer the possibility of comparing multiple treatments simultaneously (Caldwell et al., 2005). They combine both direct (head-to-head) and indirect evidence (Caldwell et al., 2005; Mills et al., 2012), by using direct comparisons of interventions within RCTs, as well as indirect comparisons across trials on the basis of a common reference group (e.g., an identical control group) (Li et al., 2011). As yet, a network meta-analysis on resilience-training programs does not exist. | |
| | According to Mills et al. (2012), Linde et al. (2016) and the <i>Cochrane Handbook for Systematic Reviews of Interventions</i> (Chaimani et al., 2019), there are three important conditions for the conduct of NMAs: transitivity, | |

| Section | Proposed methods | Reason for non-use |
|----------------------|---|--|
| | <p>homogeneity, consistency. Had an NMA been possible (i.e., the three conditions are fulfilled), we would have conducted an analysis - with expert statistical support as suggested by Cochrane (Chaimani et al., 2019), using a frequentist approach in R (Rücker et al., 2015; Viechtbauer, 2010). For sensitivity analyses, the same models would have been fitted by the restricted maximum likelihood method (Piepho, 2014; Piepho et al., 2012; Rücker et al., 2015). We would have considered categorizing resilience training into seven groups, based on the underlying training concept: (1) cognitive behavioral therapy; (2) acceptance and commitment therapy; (3) mindfulness-based therapy; (4) attention and interpretation therapy; (5) problem-solving therapy; (6) stress inoculation therapy; and (7) multimodal resilience training. We might have included additional groups after the full literature search had been conducted. Reference groups that could have been included in the network meta-analysis were attention control, wait-list, treatment as usual, or no intervention. We planned to investigate inconsistency and flow of evidence in accordance with recommendations in the literature (e.g., Chaimani et al., 2019; Dias et al., 2008; König et al., 2013; Krahn et al., 2013, 2014; Lu & Ades, 2006; Lumley, 2002; Rücker et al., 2015; Salanti et al., 2008; White et al., 2012).</p> | |
| Summary of findings | <p>Depending on the assessment of heterogeneity and possible effect modifiers (see Subgroup analysis and investigation of heterogeneity), we would have created several "Summary of findings" tables; for example, for the clinical status of study populations or the comparator group.</p> | <p>We identified no consistent effect modifiers over the primary outcomes in subgroup analyses and therefore created no separate "Summary of findings" tables.</p> |
| Sensitivity analysis | <p>If cluster-randomized trials had been included, we would have performed sensitivity analyses based on the ICC estimate in cluster-randomized trials that had not adjusted for clustering, by excluding cluster-RCTs where standard errors were not corrected or corrected only on the basis of an externally-estimated ICC. In an additional sensitivity analysis, we would have replaced all externally-estimated ICCs that were less than 0.10, by 0.10. Finally, we would have conducted a sensitivity analysis for the unit of randomization, by limiting the analysis to individually-randomized trials.</p> | <p>No cluster-RCT was included in this review.</p> |

Note. ICC = Intra-cluster correlation coefficient; RCT(s) = randomized controlled trial(s).

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