Revised Cochrane risk-of-bias tool for randomized trials (RoB 2)

TEMPLATE FOR COMPLETION

Edited by Julian PT Higgins, Jelena Savović, Matthew J Page, Jonathan AC Sterne  
on behalf of the RoB2 Development Group

**Version of 22 August 2019**

The development of the RoB 2 tool was supported by the MRC Network of Hubs for Trials Methodology Research (MR/L004933/2- N61), with the support of the host MRC ConDuCT-II Hub (Collaboration and innovation for Difficult and Complex randomised controlled Trials In Invasive procedures - MR/K025643/1), by MRC research grant MR/M025209/1, and by a grant from The Cochrane Collaboration.



This work is licensed under a [Creative Commons Attribution-NonCommercial-NoDerivatives 4.0 International License](http://creativecommons.org/licenses/by-nc-nd/4.0/).

|  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Study details**   |  |  | | --- | --- | | **Reference** | Liu, K., Duan, Y., & Wang, Y. (2021). The effectiveness of a web-based positive psychology intervention in enhancing college students’ mental well-being. Social Behavior and Personality: An international journal, 49(8), e10459. https://doi.org/10.2224/sbp.10459 |   **Study design**   |  |  | | --- | --- | | £ | Individually-randomized parallel-group trial | | £ | Cluster-randomized parallel-group trial | | £ | Individually randomized cross-over (or other matched) trial |   **For the purposes of this assessment, the interventions being compared are defined as**   |  |  |  |  | | --- | --- | --- | --- | | Experimental: | Positive Psychology Intervention | Comparator: | Covid-19 health reminders (type of control?) |  |  |  | | --- | --- | | **Specify which outcome is being assessed for risk of bias** | Subjective well-being - Positive And Negative Affect Schedule (PANAS) Mackinnon et al., 1999 |  |  |  | | --- | --- | | **Specify the numerical result being assessed.** In case of multiple alternative analyses being presented, specify the numeric result (e.g. RR = 1.52 (95% CI 0.83 to 2.77) and/or a reference (e.g. to a table, figure or paragraph) that uniquely defines the result being assessed. | Pre and post-intervention for both experimental and control group (journal article: table 1 for pre and table 3 for post). |   **Is the review team’s aim for this result…?**   |  |  | | --- | --- | | £ | to assess the effect of *assignment to intervention* (the ‘intention-to-treat’ effect) | | X | to assess the effect of *adhering to intervention* (the ‘per-protocol’ effect) |   **If the aim is to assess the effect of *adhering to intervention***, select the deviations from intended intervention that should be addressed (at least one must be checked):  X occurrence of non-protocol interventions  £ failures in implementing the intervention that could have affected the outcome  X non-adherence to their assigned intervention by trial participants  **Which of the following sources were obtained to help inform the risk-of-bias assessment? (tick as many as apply)**  X Journal article(s) with results of the trial  £ Trial protocol  £ Statistical analysis plan (SAP)  £ Non-commercial trial registry record (e.g. ClinicalTrials.gov record)  £ Company-owned trial registry record (e.g. GSK Clinical Study Register record)  £ “Grey literature” (e.g. unpublished thesis)  £ Conference abstract(s) about the trial  £ Regulatory document (e.g. Clinical Study Report, Drug Approval Package)  £ Research ethics application  £ Grant database summary (e.g. NIH RePORTER or Research Councils UK Gateway to Research)  £ Personal communication with trialist  £ Personal communication with the sponsor |

## Risk of bias assessment

Responses underlined in green are potential markers for low risk of bias, and responses in red are potential markers for a risk of bias. Where questions relate only to sign posts to other questions, no formatting is used.

**Domain 1: Risk of bias arising from the randomization process**

|  |  |  |
| --- | --- | --- |
| **Signalling questions** | **Comments** | **Response options** |
| **1.1 Was the allocation sequence random?** | “All students who completed the consent form along with submitting baseline measurements were then randomly assigned to either the web-based PPI condition or the control condition”. | NI  Answer ‘Yes’ if a random component was used in the sequence generation process. Examples include computer-generated random numbers.  Answer ‘No’ if no random element was used in generating the allocation sequence or the sequence is predictable.  Answer ‘No information’ if the only information about randomization methods is a statement that the study is randomized. |
| **1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?** | NI  Answer ‘Yes’ if the trial used any form of remote or centrally administered method to allocate interventions to participants, where the process of allocation is controlled by internet-based randomization service providers.  Answer ‘No’ if there is reason to suspect that the enrolling investigator or the participant had knowledge of the forthcoming allocation. |
| **1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?** | Group sizes are relatively similar – 420 intervention and 448 control.  No differences between groups based on baseline characteristics or prognostic factors: “Results show that the gender distribution was not significantly different across the two conditions, χ2 (1,868) = .286, p = .593, V = .036. Table 1 shows the baseline characteristics for both groups, including gender and age, and baseline affective state as measured by the PANAS score. In addition, we ran two independent-samples t tests to assess whether there was a difference in PA and NA scores between the two groups in the pretest phase. Analyses revealed that neither PA nor NA scores differed significantly for the two groups (see Table 1).” | N  Answer ‘Yes’ if there are imbalances that indicate problems with the randomization process, including:  (1) substantial differences between intervention group sizes, compared with the intended allocation ratio; or  (2) a substantial excess in statistically significant differences in baseline characteristics between intervention groups, beyond that expected by chance; or  (3) imbalance in one or more key prognostic factors, or baseline measures of outcome variables, that is very unlikely to be due to chance and for which the between-group difference is big enough to result in bias in the intervention effect estimate. |
| **Risk-of-bias judgement** |  | Some concerns |
| Optional: What is the predicted direction of bias arising from the randomization process? |  | Unpredictable |

Domain 2: Risk of bias due to deviations from the intended interventions (*effect of adhering to intervention*)

|  |  |  |
| --- | --- | --- |
| **Signalling questions** | **Comments** | **Response options** |
| **2.1. Were participants aware of their assigned intervention during the trial?** | Interventions differed substantially and participants in control group were probably aware they didn’t actively conduct an intervention (they might’ve only received 1 health related email in between pre and post measures):  “For those assigned to the PPI group, the web-based intervention program that followed positive psychological guidelines was sent to the respondent’s university email account. Once participants had accessed the intervention website, which was also affiliated with Harbin University of Science and Technology, the instructions for positive future imagination based on the original version proposed by Sheldon and Lyubomirsky (2006) appeared on the screen. Immediately after completing the second session of PPI, participants received an automatically generated email directing them to return to the posttreatment assessment website.  After the participants assigned to the control group had submitted the baseline measures, they received only mental health reminders sent to their university email account. These reminders were sent to the control group members four times during the two weeks following their submission of the baseline measure, preceding their first postintervention assessment using the PANAS.”  2.2 intervention delivery is fully automated digital. | PY |
| **2.2. Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?** | N |
| **2.3. [If applicable:] If Y/PY/NI to 2.1 or 2.2: Were important non-protocol interventions balanced across intervention groups?** | The intervention is a single session, and people signing up to the study didn’t specifically sign up as they were hoping to receive an intervention (it was a mandatory part of their university course). Therefore, it is considered unlikely – despite no information being provided on this. | PN |
| **2.4. [If applicable:] Were there failures in implementing the intervention that could have affected the outcome?** |  | NA |
| **2.5. [If applicable:] Was there non-adherence to the assigned intervention regimen that could have affected participants’ outcomes?** | There is no information on the level of non-adherence or whether this was the reason a certain number of participants were excluded from the intervention. | NI |
| **2.6. If N/PN/NI to 2.3, or Y/PY/NI to 2.4 or 2.5: Was an appropriate analysis used to estimate the effect of adhering to the intervention?** | Two analysis were conducted, one on positive affect and one on negative affect:  “Two 2 × 4 mixed-design analyses of variance (ANOVAs) with Bonferroni adjustments were applied to test the effectiveness of the PPI over the 6-month duration of the experiment.”  A completers only analysis was conducted:  “The data of 132 participants were removed because they did not submit complete responses. Thus, 868 students remained” | N |
| **Risk-of-bias judgement** |  | High |
| Optional: What is the predicted direction of bias due to deviations from intended interventions? |  | Unpredictable |

Domain 3: Missing outcome data

|  |  |  |
| --- | --- | --- |
| **Signalling questions** | **Comments** | **Response options** |
| **3.1 Were data for this outcome available for all, or nearly all, participants randomized?** | Unknown at what stage participants dropped out and whether this was prior to or following randomisation: “We initially recruited 1,000 second-year undergraduate students … The data of 132 participants were removed because they did not submit complete responses. Thus, 868 students remained: 420 students in the PPI group and 448 in the control group.” | PN  The appropriate study population for an analysis of the intention to treat effect is all randomized participants.“Nearly all” should be interpreted as that the number of participants with missing outcome data is sufficiently small that their outcomes, whatever they were, could have made no important difference to the estimated effect of intervention.For continuous outcomes, availability of data from 95% of the participants will often be sufficient. |
| **3.2 If N/PN/NI to 3.1: Is there evidence that the result was not biased by missing outcome data?** | There is no evidence in the journal article. | N  Evidence that the result was not biased by missing outcome data may come from: (1) analysis methods that correct for bias; or (2) sensitivity analyses showing that results are little changed under a range of plausible assumptions about the relationship between missingness in the outcome and its true value. However, imputing the outcome variable, either through methods such as ‘last-observation-carried-forward’ or via multiple imputation based only on intervention group, should not be assumed to correct for bias due to missing outcome data. |
| **3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?** |  | NI  If loss to follow up, or withdrawal from the study, could be related to participants’ health status, then it is possible thatmissingness in the outcome was influenced by its true value. However, if all missing outcome data occurred for documented reasons that are unrelated to the outcome then the risk of bias due to missing outcome data will be low (for example, failure of a measuring device or interruptions to routine data collection).In time-to-event analyses, participants censored during trial follow-up, for example because they withdrew from the study, should be regarded as having missing outcome data, even though some of their follow up is included in the analysis. Note that such participants may be shown as included in analyses in CONSORT flow diagrams. |
| **3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?** | NI  Reasons for answering ‘Yes’ are:  1.Differences between intervention groups in the proportions of missing outcome data.  2.Reported reasons for missing outcome data provide evidence that missingness in the outcome depends on its true value.  3.Reported reasons for missing outcome data differ between the intervention groups.  4.The circumstances of the trial make it likely that missingness in the outcome depends on its true value. For example, in trials of interventions to treat schizophrenia it is widely understood that continuing symptoms make drop out more likely.  Answer ‘No’ if the analysis accounted for participant characteristics that are likely to explain the relationship between missingness in the outcome and its true value. |
| **Risk-of-bias judgement** |  | High |
| Optional: What is the predicted direction of bias due to missing outcome data? |  | Unpredictable |

Domain 4: Risk of bias in measurement of the outcome

|  |  |  |
| --- | --- | --- |
| **Signalling questions** | **Comments** | **Response options** |
| **4.1 Was the method of measuring the outcome inappropriate?** | According to the journal article the short version of PANAS containing 10 items, 5-items for PA and 5-items for NA was used, measured using a 5-point Likert scale. If so, scores are expected to range from 5-25, with means in a Chinese population for PA around 18 and NA around 12 (Thompson, 2012). However, tables indicate all scores in this study exceed 25. Perhaps the 20-item version was used instead, however, in this case the NA score would still be highly unlikely: baseline NA in the study is 27 whilst the norm is considered around 17.  “We employed the abbreviated form of the PANAS (Mackinnon et al., 1999) to assess the general affective state of all participants. The PANAS is designed around 10 items of affect (five for PA, the other for NA) and comprises words that describe different emotions and feelings. Participants are instructed to read each item and use a 5-point Likert scale to indicate to what extent they feel these emotions at the moment: 1 (very slightly or not at all), 2 (a little), 3 (moderately), 4 (quite a bit), and 5 (extremely). The frame of reference was the current psychological state of the participant. This abbreviated PANAS was completed four times by all participants, at four time points (baseline, posttreatment, 3-month follow-up, and 6-month follow-up) amid the whole study period. Cronbach’s alphas indicating degree of internal consistency in this sample were .71, .75, .73 and .81 for the NA items and .73, .78, .72, and .79 for the PA items at the baseline assessment, postintervention evaluation, 3-month follow-up, and 6-month follow-up time points, respectively. These results are indicative of good internal consistency.” | N  Answer ‘Yes’ or ‘Probably yes’ if the method of measuring the outcome is inappropriate, for example because:  (1) it is unlikely to be sensitive to plausible intervention effects (e.g. important ranges of outcome values fall outside levels that are detectable using the measurement method); or  (2) the measurement instrument has been demonstrated to have poor validity. |
| **4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?** | Standardised questionnaire which was automatically delivered via digital technology. | N  Comparable methods of outcome measurement (data collection) involve the same measurement methods and thresholds, used at comparable time points. Differences between intervention groups may arise because of ‘diagnostic detection bias’ in the context of passive collection of outcome data, or if an intervention involves additional visits to a healthcare provider, leading to additional opportunities for outcome events to be identified. |
| **4.3 If N/PN/NI to 4.1 and 4.2: Were outcome assessors aware of the intervention received by study participants?** |  | NA  Answer ‘No’ if outcome assessors were blinded to intervention status. For participant-reported outcomes, the outcome assessor is the study participant. |
| **4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of intervention received?** |  | NA  Knowledge of the assigned intervention could influence participant-reported outcomes (such as level of pain), observer-reported outcomes involving some judgement, and intervention provider decision outcomes. They are unlikely to influence observer-reported outcomes that do not involve judgement, for example all-cause mortality. |
| **4.5 If Y/PY/NI to 4.4:** **Is it likely that assessment of the outcome was influenced by knowledge of intervention received?** | NA  This question distinguishes between situations in which (i) knowledge of intervention status could have influenced outcome assessment but there is no reason to believe that it did (assessed as ‘Some concerns’) from those in which (ii) knowledge of intervention status was likely to influence outcome assessment (assessed as ‘High’). When there are strong levels of belief in either beneficial or harmful effects of the intervention, it is more likely that the outcome was influenced by knowledge of the intervention received. Examples may include patient-reported symptoms in trials of homeopathy, or assessments of recovery of function by a physiotherapist who delivered the intervention. |
| **Risk-of-bias judgement** |  | High |
| Optional: What is the predicted direction of bias in measurement of the outcome? |  | Unpredictable |

Domain 5: Risk of bias in selection of the reported result

|  |  |  |
| --- | --- | --- |
| **Signalling questions** | **Comments** | **Response options** |
| **5.1 Were the data that produced this result analysed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?** | No analysis plan, pre-registration, study protocol, etc. was found. | NI  If the researchers’ pre-specified intentions are available in sufficient detail, then planned outcome measurements and analyses can be compared with those presented in the published report(s). To avoid the possibility of selection of the reported result, finalization of the analysis intentions must precede availability of unblinded outcome data to the trial investigators. |
| **Is the numerical result being assessed likely to have been selected, on the basis of the results, from...** |  |  |
| **5.2. ... multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain?** | Potentially a different (multiple) scale(s) was used.  PANAS subscale 10-item was mentioned but likely 20-item was used. | PY  Answer ‘Yes’ or ‘Probably yes’ if:  There is clear evidence (usually through examination of a trial protocol or statistical analysis plan) that a domain was measured in multiple eligible ways, but data for only one or a subset of measures is fully reported (without justification), and the fully reported result is likely to have been selected on the basis of the results. Selection on the basis of the results can arise from a desire for findings to be newsworthy, sufficiently noteworthy to merit publication, or to confirm a prior hypothesis. For example, trialists who have a preconception, or vested interest in showing, that an experimental intervention is beneficial may be inclined to report outcome measurements selectively that are favourable to the experimental intervention.  Answer ‘No’ or ‘Probably no’ if:  There is clear evidence (usually through examination of a trial protocol or statistical analysis plan) that all eligible reported results for the outcome domain correspond to all intended outcome measurements.  Or  There is only one possible way in which the outcome domain can be measured (hence there is no opportunity to select from multiple measures).  or  Outcome measurements are inconsistent across different reports on the same trial, but the trialists have provided the reason for the inconsistency and it is not related to the nature of the results.  Answer ‘No information’ if:  Analysis intentions are not available, or the analysis intentions are not reported in sufficient detail to enable an assessment. |
| **5.3 ... multiple eligible analyses of the data?** | We don’t consider it likely, but it’s impossible to rule this out entirely due to the difficulty assessing whether an appropriate measure was used and unexpected outcome data (e.g. NA 27 instead of 17). | NI  There is clear evidence (usually through examination of a trial protocol or statistical analysis plan) that a measurement was analysed in multiple eligible ways, but data for only one or a subset of analyses is fully reported (without justification), and the fully reported result is likely to have been selected on the basis of the results. |
| **Risk-of-bias judgement** |  | High |
| Optional: What is the predicted direction of bias due to selection of the reported result? |  | Unpredictable |

Overall risk of bias

|  |  |  |
| --- | --- | --- |
| **Risk-of-bias judgement** |  | High |
| Optional: What is the overall predicted direction of bias for this outcome? |  | Unpredictable |



This work is licensed under a [Creative Commons Attribution-NonCommercial-NoDerivatives 4.0 International License](http://creativecommons.org/licenses/by-nc-nd/4.0/).