

APPENDIX A: Systematic review protocol

This systematic review was undertaken as part of a larger project investigating interventions intended to mitigate cognitive biases in the clinical decision-making of health professionals across allied health (including physiotherapy, audiology, speech pathology, nursing and social work). The review protocol was established *a priori* and the title prospectively registered with the Campbell Collaboration (July 27, 2017, <https://www.campbellcollaboration.org/library/cognitive-bias-healthcare-practitioners.html>).

Studies specific to eye care health professionals (inclusive of ophthalmologists and optometrists) were identified at the full-text screening stage. This systematic review was conducted in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) statement.

PROTOCOL

Review Title

Interventions to mitigate cognitive biases in the decision-making of eye care professionals: a systematic review

Objectives

The primary objective of this systematic review is to synthesize the evidence related to interventions, strategies and/or procedures for mitigating cognitive biases associated with clinical decision-making by eye care professionals. The secondary objective is to explore any apparent heterogeneity of effects, in order to identify potential reasons for differences in outcomes.

Criteria for Including and Excluding Studies

Types of Interventions

We will include any intervention aimed at mitigating cognitive biases in the clinical decision-making of an eye care professional. A broad definition of ‘intervention’ will be used, to capture any strategy, procedure or approach directly and deliberately intended to counter a cognitive bias (as defined by the study authors) related to the clinical assessment, diagnosis, and/or treatment. Eligible intervention types will include, but not be limited to: affective debiasing strategies, checklists, clinical guidelines or other tools, content and instructional approaches, decision aids (hardcopy or online), diagnostic rules, error-recovery approaches, forcing strategies, group-decision interventions, targeted education or training, and meta-cognition or mindfulness strategies. Strategies applied at other healthcare decision-making points (e.g., patient discharge) will not be eligible.

Types of Study Designs

To ensure inclusion of all relevant literature, studies will be considered eligible if they include any intervention specifically aimed at mitigating a cognitive bias within an eye care decision-making context. Eligible designs will include, but will not be restricted to; randomized controlled trials (RCTs), non-randomized controlled trials (nRCTs), interrupted time series and repeated measures, controlled before-after (CBA) studies, and qualitative studies that were a component of any of these quantitative study designs. Only papers written in English will be eligible for inclusion due to a lack of resources allowing for translation of studies in other languages. Relevant comparators (‘control conditions’) were the implementation of no strategy or ‘usual practice’, as defined by the study authors.

Types of Participants

Study participants will be defined as any student or qualified eye care health professional, and thus will include ophthalmologists, optometrists and opticians. Studies in which the primary participants were medical doctors, or other allied health practitioners, will only be deemed eligible if they included eye care health professionals as participants. Studies that focused only upon patient decision-making will be excluded.

Types of Outcome Measures

Given the inclusion of a broad range of interventions and cognitive biases, the list of potential specific outcomes is expansive. Therefore, studies included in this review will be expected to assess outcome variables that report changes in an eye care professionals' clinical decision-making after deployment of a specific intervention or set of interventions designed to counter cognitive bias/es or error/s. Specific focus will be given to outcomes that measure changes in proximal decision outcomes. We will focus specifically on outcomes that aim to detect changes in proximal decision outcomes that have been designed and used to represent a reduction in specified cognitive bias/es or error/s. For example, altering the order or type of information presented in a decision aid (the intervention) may alter a professional's decision to opt for one treatment over another (proximal decision outcome). If this is the case, then the choice of one treatment over the other reflects a successful reduction of cognitive bias/es or error/s, such as an order effect, framing effect or anchoring bias.

Duration of Follow-up

The duration of follow-up is expected to vary across studies, given the range of interventions included in this review. There will be no restriction on the inclusion of articles based on the duration of an intervention and/or the duration of follow-up measurements. Where studies have comparable lengths of follow-up periods, these will be grouped and analyzed separately in order to establish whether, and for how long, treatment effects last.

Types of Settings

There will be no restriction on study setting; potential settings thus include clinics (private, public and corporate), hospitals, aged-care facilities, community centers, universities, schools, private residences, or any other setting where eye care could be administered or taught. If a hypothetical or simulated clinical scenario, case vignette or survey was used, these studies will be eligible for inclusion, provided that the scenario, vignette or survey had direct relevance to a clinical decision associated with eye care provision.

Search Strategy

The search strategy is designed to be inclusive of all possible studies testing interventions to counter cognitive bias in health professionals across medicine, nursing and allied health. Studies will be identified using multiple bibliographic databases, sources and search approaches and will be inclusive of grey literature. Studies specific to eye care health professionals will be identified at the full-text screening stage.

Electronic Searches

Keywords relating to "decision-making", "healthcare", "cognitive bias/error" and "debiasing" will be used to comprehensively search the following electronic databases on October 2, 2017 (see Appendix Table 1 in the Appendix): (1) *Ovid MEDLINE(R), Ovid MEDLINE(R) In-Process & Other Non-Indexed Citations, Ovid MEDLINE(R) Daily and Ovid OLDMEDLINE(R) (1946 to present)*; (2) *Ovid EMBASE Classic + EMBASE (1947 to present)*; (3) *Cumulative Index to Nursing and Allied Health Literature (CINAHL) via Ebsco (1980 to present)*; (4) *PsycINFO*; (5) *Emcare*; (6) *Evidence-based Medicine Reviews*; (7) *Social Services Abstracts (ProQuest)*; and (8) *SCOPUS*. Text word searches will be mapped verbatim into each database, with adjustments made for database-specific syntax, as required. A grey literature search will be performed using: (1) *Open Grey*, and (2) *ProQuest Dissertation and Theses*. Following screening, the reference lists of included systematic reviews will also be hand searched for any additional potentially relevant studies.

Data Management and Software

Reference management software *EndNote* (Clarivate Analytics, 2017) will be used to compile all titles and abstracts derived from these initial searches, and duplicates will be identified and removed. These will be transferred to specialised systematic review data management software, *Covidence* (Covidence, 2017), to enable the management of retrieved records, screen reports, and identify and track disagreements.

Selection of Studies

Prior to the initial title/abstract and full-text screenings, all review authors will undergo training to ensure a comparable understanding of the purpose of the review and the inclusion criteria. See Table 1 (below) for the initial screening criteria. Titles and abstracts retrieved from the initial searches will be screened by a minimum of two review authors, who will apply the inclusion criteria. Disagreements at the title and abstract screening stage will be assessed by a third independent review author. Where the information provided in the titles and abstracts is insufficient (i.e., unclear / missing title and/or unclear/missing abstract), a full-text review will be undertaken. At full-text screening, each of the studies will be assessed independently by two review authors. All screening disagreements will be discussed, with any outstanding disagreements resolved by an independent third review author.

Appendix A, Table 1. Study inclusion criteria, applied at the title/abstract screening stage.

Study criteria	Studies where cognitive bias/es or error/s were specified/named, or the terms “cognitive bias/es” or “cognitive error/s” were used; Note: studies that used more general terms, such as ‘diagnostic error/s’ or ‘error/s in clinical reasoning’, were excluded.
Intervention type	Studies that considered any intervention/s, strategy or procedure where the primary or secondary aim was to mitigate the effect of specified cognitive bias/es in the decision-making process.
Participants	Studies that included eye care professionals, either fully trained, or students in training.
Settings	- Studies conducted in any setting where healthcare services can be administered or taught by a provider (e.g., clinics, hospitals, schools, universities, homes, etc.) - Studies that considered any hypothetical or simulated clinical scenarios, vignettes or surveys that directly related to professional decision-making within the relevant context.
Outcome measures	- Studies that reported decision outcomes resulting from the implementation of the strategy, procedure or intervention. - Studies that reported outcomes relating to the reduction of cognitive bias/es as a result of the implementation of the strategy, procedure or intervention.

Studies that were not directly related to the field of eye care practice, and those that did not meet the study design criteria, were excluded at the full-text screening stage.

We will include a flow diagram outlining the study selection process in the final review as recommended in the *Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA)* statement (Liberatti, 2009).

Data extraction and Study Coding Categories

We will use a standardised data extraction form, modelled on the *Cochrane Effective Practice and Organisation of Care* (EPOC) data abstraction form template (Cochrane Effective Practice and Organisation of Care, 2017). Two review authors will independently extract data from the selected studies.

The primary categories for data extraction and coding will be: participant details (e.g. type of healthcare professional, level of expertise); intervention setting (e.g. country, healthcare profession; healthcare setting, hypothetical scenario/*in situ*); decision type (e.g. diagnosis, clinical assessment, treatment choice); type of intervention (e.g. education, decision aid, affective debiasing, etc.); cognitive bias (e.g. confirmation bias, anchoring, recency effects, order effects, etc.); intervention components (e.g. duration, fidelity); study characteristics (e.g. study design, sample size, duration, attrition); outcome construct (e.g. type of decision outcome, description of measure).

Risk of Study Bias

Risk of study bias will be assessed using the Cochrane risk of bias tools for randomized studies (Higgins & Green, 2011) and Newcastle-Ottawa scale for non-randomized studies, and the National Institute of Health Quality Assessment Tool for Before-After (Pre-Post) for non-

randomized studies with no control group. We will not exclude studies based on their risk of bias, however this will be clearly reported in the final review.

Description of Methods Used in Primary Research

Given the inherent difficulty of randomizing healthcare professionals into control and treatment groups, it is expected that many of the studies conducted *in situ* will use quasi-experimental designs. Conversely, given the relative ease of using randomized designs in hypothetical or simulated scenarios, as well as the history of testing cognitive biases using such approaches, we expect that a substantial proportion of included studies will use hypothetical or simulated scenarios that involve written and/or filmed vignettes, standardized actors, and/or surveys.

Criteria for Determination of Independent Findings

Prior to data extraction, the selected studies will undergo assessment to ensure that articles reporting on the same study, either at the same point in time or over time, are identified and only unique samples are included in our analyses. In cases where duplicates are identified, reports and other supporting documents will be sought to provide greater detail on methods and reporting of results.

Statistical Procedures and Conventions

Measures of treatment effect: Where it is deemed clinically sensible, we will conduct a meta-analysis based on similarity of interventions, population, outcomes and comparators.

Randomized and non-randomized studies will be analyzed separately, as will *in situ* versus simulated designs. Decision outcomes in included studies are likely to be reported as binary

measures (e.g., decision outcome 1 versus decision outcome 2). An odds ratio effect size metric will be used to quantify study findings in this case, and results will be presented as summary risk ratios (RR) with 95% confidence intervals. Where outcomes are represented by continuous data, analysis will proceed using weighted mean differences, using change from baseline as the measure of effect. If the same concept is measured using different scales, the standardised mean difference (SMD) will be used.

Unit of analysis issues: Cluster-randomized trials will be included in analyses along with randomized trials that randomize individual participants. Sample sizes will be adjusted using an estimate of the intra-cluster correlation co-efficient (ICC) derived from the included trial (if possible), from a similar trial or from a study using a comparable population. If ICCs are used from other sources, this will be reported and sensitivity analyses will be conducted to investigate the effect of variation in the ICC. If both cluster-randomized and individually randomized trials are identified, relevant information will be synthesized. It will be deemed reasonable to combine results from both if the interaction between the effect of the intervention and the choice of randomization unit is considered to be unlikely.

Dealing with missing data: We will document how included studies handled missing data from participants, and where data is missing, contact the study authors. We will not impute values for missing participants. If the standard deviation is not reported, we will calculate it using other methods such as the confidence interval or exact p-values using the accepted methods of the Campbell Collaboration and/or the Cochrane Handbook. The overall assessment of treatment effect will include a sensitivity analysis to explore the impact of including/excluding studies with

high levels of missing data. For dichotomous outcomes, analyses will be carried out for both per protocol and on an intention-to-treat (ITT) basis. That is, we will include all participants randomised to each group in the analyses, making the assumption that the event (for a positive outcome – reversed for a negative outcome) did not occur for missing participants (i.e., we will include outcomes for those who do not complete ‘treatment’ where these are provided or will make the more conservative assumption just described where they are not). We will conduct a sensitivity analysis to determine the effect of assuming all participants experienced the event versus assuming all participants did not experience the event. We will also apply the ITT method for continuous outcomes, using the number of participants randomized in the analysis, even where there are missing participants. If feasible, we will also report findings for treatment completers (e.g., per protocol) but will stress differences between results for completers versus ITT.

Assessment of heterogeneity and moderator analysis: Heterogeneity between studies will be assessed by comparison of the study settings, populations and design, supplemented with the I^2 statistic. Where there are insufficient appropriate data available for meta-analysis, results of individual studies will be presented. Statistical heterogeneity will be assessed in each meta-analysis using the I^2 and Chi^2 statistics.

If substantial heterogeneity is identified and there are enough studies to identify its source (set here as > 5 studies), it will be investigated using mixed effect models to conduct moderator analyses. Potential moderators may include: (1) type of cognitive bias; (2) decision type; (3) intervention type; (4) healthcare profession; and (5) professional experience. Where appropriate

(i.e., dependent on sample size and potential groupings), sensitivity analysis will be conducted and reported using methods outlined in the Cochrane Collaboration Handbook and/or using the advice of the Campbell Collaboration Methods Coordinating Group.

Assessment of reporting biases: If there are 10 or more studies in the meta-analysis, reporting biases (such as publication bias) will be investigated using funnel plots. Funnel plot asymmetry will be assessed using the Egger's test.

Data synthesis: If the included trials are both clinically and statistically homogeneous, a meta-analysis will be conducted to obtain an overall effect. We intend to pool binary and continuous outcome data using a fixed-effect model in the first instance. In the case of substantial heterogeneity, a random-effects model will be used. If formal pooling is inappropriate for analysis (i.e., substantial heterogeneity that cannot be overcome), data synthesis will employ a narrative and tabular approach but all efforts will be made to conduct a meta-analysis.

Data management and software: Data will be stored in Excel (Microsoft, 2016), and all analyses will be performed using *RevMan 5* (Review Manager, 2014). If analyses are too complex for RevMan, appropriate meta-analytic software designed to handle the specific complexity will be used.

Assessment of the body of evidence: Quality of the evidence will be evaluated using the *Grades of Recommendation, Assessment, Development and Evaluation (GRADE)* approach (Guyatt et al., 2011). The *GRADE* approach uses five considerations: study quality (risk of bias),

consistency of effect (between studies), precision of results, directness and publication bias, to assess the quality of the body of evidence for specific outcomes. The evidence will be downgraded from 'high quality' by one level for serious (or by two levels for very serious) limitations. Our level of certainty will be presented as high, moderate, low or very low.

Treatment of Qualitative Research

Qualitative studies conducted as part of the final included trials (i.e., mixed methods) will be sought using intervention and/or author-specific search strategies (as such, they cannot be pre-specified). Qualitative studies will not be used to establish the efficacy or effectiveness of interventions for countering cognitive bias and thinking errors. Instead, such designs will provide contextual information regarding the interventions including implementation and model fidelity, participant experience, and/or settings in which the intervention was administered.

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