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TABLE OF CONTENTS

[Intervention Protocol]

Psychological therapies delivered remotely for the management of chronic pain (excluding headache) in adults

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ABSTRACT

Objectives

This is a protocol for a Cochrane Review (intervention). The objectives are as follows:

To determine the efficacy and harms of remotely delivered psychological therapies compared to active control, waiting list, or treatment-as-usual for the management of chronic pain in adults.

BACKGROUND

Description of the condition

Chronic pain (defined as pain lasting three months or more) is a global public health challenge. The International Association for the Study of Pain defines pain as “an unpleasant sensory and emotional experience associated with, or resembling that associated with, actual or potential tissue damage" (Raja 2020). The prevalence of chronic pain is estimated to be between 20% and 43% globally (Eccleston 2017; Fayaz 2016; Mansfield 2016; Tsang 2008), with annual societal costs per patient estimated at EUR 10,191 (Mayer 2019). Further, the challenge is increasing as the incidence of chronic pain rises in older age (Fayaz 2016; Tsang 2008). The most common types of chronic pain in adults include chronic back pain, fibromyalgia, neuropathic pain, and headache. Chronic pain can be categorised as the disease itself (e.g. fibromyalgia) and is defined as chronic primary pain, or can be associated with or a consequence of an underlying disease (e.g. chronic cancer-related pain; Treede 2019). The personal consequences of chronic pain may be widespread, disrupting an individual’s ability to engage in everyday life and occupation, affecting social relationships, and deleteriously influencing quality of life (Dueñas 2016; Reid 2011). High levels of depression and anxiety associated with chronic pain may further complicate the emotional impact (Scott 2007). Consequently, chronic pain presents a psychological, as well as physical, challenge.

Description of the intervention

Psychological therapies can address the cognitive, behavioural, and emotional factors associated with the experience of chronic pain to support self-management and the pursuit of personally meaningful goals. These interventions contribute to improvements in mood and pain-related disability (Williams 2020), and are recognised as an important component of effective pain management treatment (Eccleston 2013; Kerns 2011). However, patients and providers report that access to ‘non-pharmacological’, often psychological, pain treatments is constrained by multiple barriers, including geographic and economic restrictions (Becker 2017). Consequently, provision of effective and scalable support for chronic pain remains a substantial challenge.

Technological advances provide new opportunities for treatment delivery that may overcome traditional barriers and provide support remote from clinician involvement. Technology-based delivery offers the potential to liberate healthcare expertise from its temporal, geographic, and economic restrictions through partial or complete automation of treatment. Consequently, such delivery methods may increase access to psychological therapeutic support for health conditions such as chronic pain (McGuire 2017).

Relevant technologies are multiform and multiplying. Correspondingly, recent reviews emphasise the need to evaluate technology-based delivery across multiple modalities (Heapy 2015; Slattery 2019). The encouraging support for technology-based intervention delivery for chronic pain is often tempered by the disproportionate representation of specific modalities (e.g. Internet-based interventions) within the evidence-base. However, increasing investigation of technologies such as smartphones is anticipated (McGuire 2017). Consequently, rigorous verification of intervention effectiveness must match rapidly evolving technology.

This review is concerned with any technology-based delivery of psychological therapy for chronic pain that is remote from both the physical presence of the healthcare professional (HCP) and their active involvement. We employ the term ‘remote delivery’ for its superior descriptive capacity to other terms (such as e-health, telehealth, telemedicine, and digital therapeutics). Fisher and colleagues also employed ‘remote delivery’ in their related review within child and adolescent populations (Fisher 2019), so this enables cross-review comparison. Eligible interventions will utilise technology as the primary agent of delivering psychological therapy. Technology solely facilitating distance contact between client and clinician (such as videoconferencing) does not fulfil our definition of remote delivery because the intervention, whilst remote from the HCP’s physical presence, remains dependent on their active involvement and direction. We place no restrictions on technology type.

How the intervention might work

Psychological therapies comprise multiple modalities with variable intervention targets and therapeutic processes. Existing reviews suggest that psychological therapies have beneficial effects within both adult populations (Williams 2020), and child populations (Fisher 2018). Mainstream psychological approaches supporting individuals experiencing chronic pain typically derive from cognitive and behavioural models of human experience and difficulty (Eccleston 2013; Williams 2020). Traditional cognitive behavioural interventions comprise varying content including psychoeducation, identification and modification of unhelpful patterns of thought and behaviour, and the development and application of coping strategies (Kerns 2011). Whilst content varies, these interventions share an underlying aim to target the interactive relationship between internal experience and external behaviour in order to support personally meaningful engagement with life. Whilst cognitive and behavioural therapies dominate the literature, this review is not limited to any specific therapy modality.

Remote delivery of psychological therapy divorces intervention content from face-to-face clinician delivery. Technology offers increasingly varied media to achieve this end and facilitate new ways to access psychological interventions for chronic pain (including Internet-based, smartphone applications, and virtual reality). The potential impact of delivery method should not be underestimated, particularly given the emphasis within psychological interventions on the therapeutic role of client-clinician relationships (Horvath 2011; Zilcha-Mano 2017). However, as related reviews suggest, remote delivery also offers additional features beyond those of traditional therapy, which may contribute to the impact of interventions, such as immediate 24-hour access to support (Fisher 2019), and exact treatment fidelity (Heapy 2015). Consequently, whilst the intervention content and underlying psychological frameworks may appear comparable between traditional and technology-based delivery, the delivery method has the potential to influence both message and outcome.

Why it is important to do this review

Traditional face-to-face psychological therapies for chronic pain appear useful (Williams 2020). However, access to treatment is restricted by healthcare resources, geography, and cost. Remote technology-based delivery holds the potential to overcome these traditional treatment barriers. However, therapeutic equivalence between traditional and technology-

based delivery requires substantiation. Further, communication technology provides opportunities for content and delivery that outstrip what is possible face to face, and may facilitate novel interventions (Eccleston 2018). Whilst previous reviews of Internet-based psychological interventions for chronic pain are encouraging (Bender 2011; Buhrman 2016; Eccleston 2014), evolving technologies necessitate the need to consider a broader spectrum of technologies capable of remote delivery. Concern remains that technological innovation, such as smartphone applications, is outpacing regulation and evidential support, despite repeat criticism (Lalloo 2015; Portelli 2016; Rosser 2011). Consequently, a review enabling aggregation and evaluation of remote delivery - via multiple technologies - of psychological therapy for chronic pain is warranted. Fisher 2019 provides such a review of remotely delivered psychological interventions for chronic pain in children and adolescents. Williams 2020 provides a review of face-to-face psychological interventions in adults with chronic pain. We aim to complement both reviews by conducting a review of psychological interventions delivered remote from the therapist for adults with chronic pain. This review will update and supersede our previous review focused on Internet-delivered psychological therapies (Eccleston 2014).

OBJECTIVES

To determine the efficacy and harms of remotely delivered psychological therapies compared to active control, waiting list, or treatment-as-usual for the management of chronic pain in adults.

METHODS

Criteria for considering studies for this review

Types of studies

We will include peer-reviewed randomised controlled trials (RCTs). Randomised trials are the best design to minimize bias when evaluating the effectiveness of an intervention. We will exclude equivalence studies, conference abstracts, dissertations, and non-randomised studies as a quality threshold and to support confidence in the reliability of included data. We will search databases without language or date restrictions and report ongoing trials and trials with data.

Types of participants

We will include adults (≥ 18 years of age) with chronic pain. Typically, these conditions include musculoskeletal and neuropathic pain. We will include participants experiencing chronic pain as a primary symptom of a condition or disease. For example, this includes participants experiencing diabetic neuropathy, sickle cell disease, or primary chronic pain conditions. We will exclude life-limiting conditions such as cancer and participants with headache or migraine from the review to be consistent with Williams 2020. We will include studies where only a subset of participants are eligible, if characteristics and outcomes of those participants can be extracted separately. Studies must include a minimum of 20 participants in each trial arm post-treatment to be considered for inclusion.

Types of interventions

We will include psychological therapies that have recognisable psychotherapeutic content or are based on a psychological theory. Psychological therapy delivery must be predominantly remote from the therapist. We define 'remote delivery' as the transfer of intervention content remote from both therapist location and their active guidance. We will exclude trials that involve more than 30% contact time with a clinician, either in-person or via technology-mediated communication (e.g. email, phone, teleconference, online chat).

Eligible trials using remotely delivered psychological therapies may utilise various technologies, such as the Internet or smartphone application. The intervention must include content that requires the participant to engage in one or more psychologically-informed therapeutic activity. We will exclude interventions that only provide education or passively consumed content (e.g. description of psychological theory rather than its application). Eligible interventions must have been developed by (or under the supervision of) a qualified psychologist and based on existing psychological theory, echoing the Cochrane Review of face-to-face delivery of psychological therapies (Williams 2020). Importantly, the intervention must be potentially scalable to reach a large number of people, rather than relying on intensive one-to-one interactions such as delivering interventions via Skype or videoconferencing software. All authors will agree on the included studies.

This is a rapidly evolving field, with new technologies and delivery modes emerging. Therefore, in future updates we may need to expand our inclusion criteria to include new forms of intervention modes as technology evolves.

We will compare intervention arms to control arms. We will include active controls (e.g. education), treatment-as-usual, or waiting list controls. We will combine treatment-as-usual and waiting list controls in the analyses. As described, we will exclude equivalence trials, and therefore control groups that include a psychotherapeutic content will be excluded.

Types of outcome measures

We will compare psychological interventions to control groups at two time points: 1) immediately post-treatment; and 2) follow-up. We determined an eligible timeframe for follow-up as between three and 12 months post-treatment. We will extract outcomes that are assessed by validated measures. In the event of multiple follow-up assessments, we will extract the latter time point closest to 12 months. It may be that some studies provide a longer time point beyond 12 months, and we will extract this as a secondary follow-up time point. However, the control group must remain consistent and, therefore, comparable across these time points. Where studies include multiple measures for the same outcome, we will use the most reliable and frequently employed measure within the field.

Primary outcomes

We will extract the following primary outcomes.

- Pain intensity (continuous data: e.g. numerical rating scale, visual analogue scale). Where possible, we will extract 30% reduction in pain, 50% reduction in pain, or both, separately.
- Functional disability (e.g. Functional Disability Inventory).
- Quality of life (e.g. Short Form-36).
- Adverse events.

Secondary outcomes

We will extract the following secondary outcomes.

- Anxiety (e.g. Hamilton Anxiety Rating Scale).
- Depression (e.g. Beck Depression Inventory).
- Intervention satisfaction (e.g. numerical rating scale).
- Intervention engagement (i.e. measurement of intervention use, technology usage, and/or activity completion).
- Attrition (from baseline to follow-up).

Search methods for identification of studies

Electronic searches

We will search the following electronic databases without date or language restriction.

- CENTRAL (Cochrane Library) current issue.
- MEDLINE (OVID) 1946 to present.
- Embase (OVID) 1974 to present.
- PsycINFO (Ebsco) 1806 to present.

We will employ a comparable search strategy across databases (see Appendix 1 for the MEDLINE strategy).

Searching other resources

We will search clinicaltrials.gov and the WHO ICTRP (apps.who.int/trialsearch/) for any ongoing trials or completed trials not yet published in a peer-reviewed journal. We will enter these into the relevant 'Characteristics of studies' section. We will search the reference lists and conduct a citation search of included trials to identify any further trials that meet our eligibility criteria.

Data collection and analysis

Selection of studies

We will divide the search results between two review author pairs (BR and EK; EF and GD). Each pair will review half of the search results. In each pair, the two review authors (e.g. BR and EK) will independently determine eligibility by screening the title and abstract of studies identified by the search. We will exclude studies that clearly do not satisfy inclusion criteria, and obtain full copies of the remaining studies. Any disagreements that cannot be resolved by discussion between the two authors doing the initial screening will be subject to arbitration by a third author (CE). Two review authors (e.g. BR and EF) will independently read the full texts of the retrieved studies to identify eligible studies. We will resolve any disagreements through discussion, or by involving a third author (CE) if necessary. All review authors will agree on all included studies. We will include a PRISMA flow chart in the full review which will show the status of identified studies (Moher 2009), as recommended in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2020c, hereafter referred to as the *Cochrane Handbook*).

Eligible studies will meet the following criteria.

- An RCT.
- Published in a peer-reviewed scientific journal.
- Twenty or more participants in each trial arm post-treatment, for consistency with Williams 2020. Small studies are known to produce larger effect sizes (Dechartres 2013).
- Therapy is primarily psychological in at least one trial arm.
- Psychological therapy aims to facilitate adults in managing or coping with chronic pain.
- The intervention must include content that requires the participant to engage in one or more psychologically-informed therapeutic activity.
- A qualified psychologist developed or supervised the development of the psychological component of the intervention.
- Technology is the primary delivery mechanism for the psychological therapy.
- Technology-based delivery of psychological therapy is remote from clinician contact and their active guidance.
- Participants receive the intervention in their everyday setting, rather than in a clinic or laboratory.

Data extraction and management

One author will extract data from the included studies and a second author will check these data. A third author will arbitrate any disagreements. We will extract the following data.

- Article details (e.g. authorship, title, year, study funding sources, study author declarations of interest).
- Participant characteristics (e.g. sample size, age, sex, pain condition or characteristics and duration, dropout).
- Intervention characteristics (e.g. psychological theory and content, duration, delivery mode, therapeutic activity requiring participant interaction or involvement).
- Comparison characteristics (e.g. type of comparison and content).
- Methodological characteristics (e.g. study design, randomisation method, assessment points).
- Outcomes (e.g. primary and secondary outcomes).

Where there are multiple reports of the same study, we will amalgamate into a single study summary.

Assessment of risk of bias in included studies

We will assess risk of bias using the Cochrane risk of bias (RoB) version 1 tool for randomised trials (Higgins 2011). The tool assesses bias arising from multiple domains, including selection bias, performance and detection bias, attrition bias, and reporting bias. We will categorise risk for each domain in terms of low, high, or unclear. Two authors will independently assess included articles for risk of bias. We will resolve any assessment discrepancies through discussion, or by involving a third author if necessary.

Selection bias

- Random sequence generation (checking for possible selection bias). We will assess the method used to generate the allocation sequence as: low risk of bias (any truly random process, e.g. random number table, computer random number generator); or unclear risk of bias (method used to generate sequence not clearly stated). Studies using a non-random process (e.g. odd or even date of birth, hospital or clinic record number) will not meet eligibility criteria and will not be included.

- Allocation concealment (checking for possible selection bias). The method used to conceal allocation to interventions prior to assignment determines whether intervention allocation could have been foreseen in advance of, or during recruitment, or changed after assignment. We will assess the methods as: low risk of bias (e.g. telephone or central randomisation; consecutively numbered, sealed, opaque envelopes); or unclear risk of bias (method not clearly stated). Studies that do not conceal allocation (e.g. open list) will be rated as high risk of bias.

Performance and detection bias

- Blinding of participants and personnel (checking for possible performance bias). In line with other Cochrane Reviews of psychological interventions (Fisher 2018; Fisher 2019; Williams 2020), we will not assess blinding of participants and personnel as it is not possible to fully blind delivery of psychological therapies. We will consider the possible influence of performance bias in the interpretation of the findings.
- Blinding of outcome assessment (checking for possible detection bias). We will assess the methods used to blind study participants and outcome assessors from knowledge of which intervention a participant received. We will assess the methods as: low risk of bias (assessments are completed online or directly into a database and are not able to be influenced by an outcome assessor); unclear risk of bias (it is not clear how assessments are taken, or whether the outcome assessor knows of treatment allocation); or high risk of bias (outcome assessors are aware of treatment allocation). As outcome assessment will likely be self-report, we acknowledge the possible influence of bias arising from the difficulties of fully blinding participants when delivering psychological therapies. We will consider this potential influence in interpretation of the findings.

Attrition bias

- Incomplete outcome data (checking for possible attrition bias due to the amount, nature and handling of incomplete outcome data). We will assess the methods used to deal with incomplete data as: low risk (< 10% of participants did not complete the study, or study used ‘baseline observation carried forward’ analysis, or both); unclear risk of bias (used 'last observation carried forward' analysis); or high risk of bias (used 'completer' analysis).

Reporting bias

- Selective reporting (checking for reporting bias). We will assess whether primary and secondary outcome measures were pre-specified and whether these were consistent with those reported: low risk of bias (all pre-specified outcomes are reported in manuscript and no additional outcomes are included); unclear risk of bias (trial registration or protocol is not available, or trial post-registered); or high risk of bias (pre-specified outcomes are missing from trial manuscript, additional outcomes are included in manuscript but not listed in pre-registered database, or primary and secondary outcomes are changed between pre-registration and manuscript).

Measures of treatment effect

We anticipate that most authors will report continuous data on our outcomes of interest. We will extract and analyse these continuous outcome data where reported. We will employ standardised mean differences (SMDs) with 95% confidence intervals (CIs) to evaluate treatment effects for continuous data. We will interpret SMD as small (0.2), moderate (0.5), and large (0.8), in accordance with Cohen 1988.

We do not anticipate dichotomous data as this form of pain treatment outcome data is most commonly associated with headache or migraine reduction, which are excluded from this review, and adverse events are not widely reported in psychological trials. We will employ risk ratios (RRs) with 95% confidence intervals (CIs) for dichotomous data if we encounter it.

Unit of analysis issues

We expect that studies will randomise at the individual level. We will follow *Cochrane Handbook* guidance if cluster-randomisation occurs (Higgins 2020b). The clusters will be considered as the unit of analysis, rather than participants. We will collapse arms into intervention and control groups, respectively, where a study employs multiple interventions or control arms, or both. We will split the control group equally across intervention arms to enable comparisons where notable discrepancy in underpinning psychological approach or theory prohibits meaningful amalgamation. For cross-over trials, we will include the first step comparison of treatment and control. We will not include data from the second step where the arms are crossed over to avoid carryover effects of the intervention in the first step.

Dealing with missing data

We will contact authors if outcome data are missing from published studies. We will use available statistical information from the published study to calculate the necessary data (e.g. standard deviations) where possible, in accordance with *Cochrane Handbook* guidance (Higgins 2020a), in the event that these data are not available from the authors. We will not impute missing variables in analyses where outcome data are not available or calculable. In the unlikely event that both per protocol and intention-to-treat analyses are presented in published manuscripts, we will preferentially extract intention-to-treat data.

Assessment of heterogeneity

We will interpret heterogeneity by visually inspecting forest plots alongside calculating Chi^2 and I^2. We will interpret I^2 with reference to *Cochrane Handbook* guidelines (Deeks 2020):

- 0% to 40%; might not be important;
- 30% to 60%; may represent moderate heterogeneity;
- 50% to 90%; may represent substantial heterogeneity;
- 75% to 100%; considerable heterogeneity.

Where heterogeneity is substantial or considerable, we will investigate, and conduct sensitivity analyses if appropriate.

Assessment of reporting biases

We will assess reporting biases as part of the risk of bias assessment in this review. We will also assess funnel plots, in accordance with *Cochrane Handbook* guidance (Page 2020), where there are at least 10 studies included in each analysis.

Data synthesis

We will analyse data using Review Manager (Review Manager 2020). We will analyse outcome data using random-effects models. Where it is not possible to combine data, we will describe the findings across studies.

We will conduct the comparisons listed below for two individual therapy types initially: cognitive behavioural therapy (CBT), and acceptance and commitment therapy (ACT). We will separate comparisons by control group type (e.g. waiting-list control; active or treatment-as-usual control). We will conduct comparisons on data immediately post-treatment and at follow-up.

- CBT versus waiting-list control (no treatment), post-treatment.
- CBT versus waiting-list control (no treatment), follow-up.
- CBT versus active control, post-treatment.
- CBT versus active control, follow-up.
- ACT versus waiting-list control (no treatment), post-treatment.
- ACT versus waiting-list control (no treatment), follow-up.
- ACT versus active control, post-treatment.
- ACT versus active control, follow-up.

We will add further comparisons as separate categories of psychological therapies are identified within included studies.

Subgroup analysis and investigation of heterogeneity

We plan to conduct subgroup analyses where at least 10 eligible studies are available, evaluating the following.

- Delivery method (e.g. computer versus smartphone application).
- Delivery automation (i.e. fully automated delivery versus some therapist interaction).

We will not conduct subgroup analyses based on pain type; this approach is consistent with Williams 2020.

Sensitivity analysis

We plan to conduct sensitivity analyses in:

- trials with more than 50 participants versus less than 50 participants per arm; and
- trials assessed as low risk of bias across all bias domains.

We will also conduct sensitivity analyses excluding trials where we identify substantial or considerable heterogeneity. We will consider other sensitivity analyses as the literature evolves, and new technologies emerge. We will only conduct sensitivity analyses when there are at least 10 eligible studies to enter into the meta-analysis in either subgroup.

Summary of findings and assessment of the certainty of the evidence

Two review authors (BR and EF) will independently rate the certainty of the body of evidence for the outcomes. We will use the GRADE system to rank the certainty of the evidence using the GRADEprofiler Guideline Development Tool software (GRADEpro GDT), and the guidelines provided in the *Cochrane Handbook* (Chapter 14, Higgins 2020c) and GRADEpro Handbook (Schünemann 2013).

The GRADE approach uses five considerations (study limitations (risk of bias); unexplained heterogeneity and inconsistency of effect; imprecision; indirectness; and publication bias) to assess the certainty of the body of evidence for each outcome. The GRADE system uses the following criteria for assigning grade of evidence.

- High: we are very confident that the true effect lies close to that of the estimate of the effect.
- Moderate: we are moderately confident in the effect estimate; the true effect is likely to be close to the estimate of effect, but there is a possibility that it is substantially different.
- Low: our confidence in the effect estimate is limited; the true effect may be substantially different from the estimate of the effect.
- Very low: we have very little confidence in the effect estimate; the true effect is likely to be substantially different from the estimate of effect.

The GRADE system considers study design as a marker of quality. Randomised controlled trials are considered to yield high certainty of evidence, but they can be downgraded for important limitations. Factors that may decrease the certainty level of a body of evidence are as follows.

- Serious or very serious study limitations (risk of bias).
- Important or serious inconsistency of results.
- Some or major indirectness of evidence.
- Serious or very serious imprecision.
- Probability of publication bias.

We plan to include at least four 'Summary of findings' tables to present the main findings for each therapy type (e.g. CBT, ACT) versus each control comparison (e.g. waiting list, active) in a transparent and simple tabular format. In particular, we will include key information concerning the certainty of evidence, the magnitude of effect of the interventions examined, and the sum of available data on these outcomes: pain intensity, functional disability, quality of life, and adverse events.

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APPENDICES

Appendix 1. Appendix 1: MEDLINE search strategy (OVID)

1. exp Pain/
2. Fibromyalgia/
3. (pain* or fibromyalgia* or neuralgia*).tw.
4. 1 or 2 or 3
5. exp Internet/
6. (Internet or web or blog* or "social media" or online or www or email* or e-mail*).tw.
7. exp Telecommunications/
8. (telemedicine or tele-medicine).tw.
9. (telehealth or tele-health).tw.
10. (ehealth or e-health).tw.
11. (mobile health or mhealth or m-health).tw.
12. ICT.tw.
13. ((inform* or communicat* or interact*) adj6 (computer* or technolog* or software)).tw.
14. ((health* or treat* or therap* or intervention* or assist* or selfmanag* or self-manag*) adj6 (computer* or technolog* or software)).tw.
15. "world wide web".tw.
16. (telephone* or phone* or mobile* or cellphone* or apps or text* or SMS or smartphone*).tw.
17. (virtual reality or augmented reality or VR or AR).tw.
18. ("Interactive voice response" or IVR).tw.
19. 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 13 or 14 or 15 or 17 or 18
20. 4 and 19
21. randomized controlled trial.pt.
22. controlled clinical trial.pt.
23. randomized.ab.
24. placebo.ab.
25. drug therapy.fs.
26. randomly.ab.
27. trial.ab.
28. or/21-27
29. exp animals/ not humans.sh.
30. 28 not 29
31. 20 and 30
32. exp Child/ or exp Adolescent/ or exp infant/
33. 31 not 32
34. exp Psychotherapy/
35. exp PSYCHOLOGY/
36. ((behavio#r* adj therapy) or (behavio#r* adj therapies)).tw.
37. ((cognitive adj therapy) or (cognitive adj therapies)).tw.
38. mindfulness.tw.
39. meditat*.tw.
40. meditat*.tw.
41. psychotherap*.tw.
42. (psychological adj treatment*).tw.
43. ((psychological adj therapy) or (psychological adj therapies)).tw.
44. 34 or 35 or 36 or 37 or 38 or 39 or 40 or 41 or 42 or 43
45. 33 and 44

HISTORY

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CONTRIBUTIONS OF AUTHORS

Ben Rosser will oversee the project and is responsible for updating the review.

Ben Rosser, Emma Fisher, Christopher Eccleston, Geoffrey Duggan, and Edmund Keogh contributed to the design and authoring of the protocol.

All authors will contribute to the enactment of the protocol, analysis, and authoring of the final review.

DECLARATIONS OF INTEREST

BR: None known.

EF: None known.

CE: None known. Since CE is an author as well as being a PaPaS Co-ordinating Editor during the development of this protocol, we acknowledge the input of Neil O'Connell who acted as Sign-off Editor for this review. CE had no input into the editorial decisions or processes for this review.

GD: None known; GD is a clinical psychologist and works with clients with chronic pain.

EK: None known.

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