Systematic review and meta-analysis of randomised, other-than-placebo (OTP) controlled, trials of individualised homeopathic treatment (IHT):

Study protocol

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INTRODUCTION

Homeopathy is a system of medicine that uses specific preparations of substances whose effects, when administered to healthy subjects, correspond to the manifestations of the disorder (symptoms, clinical signs, pathological states) in the individual patient. The prescription of a homeopathic medicine ensues from an individual's consultation with a practitioner. It is believed that the effect of administering a homeopathic medicine is to stimulate a healing response in the patient.[1]

There are several distinct forms of homeopathy, the main types being 'individualised' (or 'classical') homeopathy, 'clinical' homeopathy, 'complex' homeopathy, and isopathy. In **individualised homeopathy** – as originally defined by its founder, Samuel Hahnemann – typically a single homeopathic medicine is selected on the basis of the 'total symptom picture' of a patient, including his/her mental, general and constitutional type. In **clinical homeopathy**, one or more homeopathic medicines are administered for standard clinical situations or conventional diagnoses. In **complex homeopathy**, several homeopathic medicines are combined in a fixed ('complex') formulation. **Isopathy** is the use of homeopathic dilutions from the causative agent of the disease itself, or from a product of the disease process, to treat the condition.[1]

Our previous review protocols focused on **individualised** or **non-individualised** homeopathy in the context of placebo-controlled randomised controlled trials (RCTs).[2,3] The current protocol focuses solely on the 'whole-person' therapeutic approach of **individualised homeopathic treatment (IHT)**: individualised consultation *together with* the consequently prescribed homeopathic medicine/s [and sometimes including dietary and/or lifestyle recommendations]). The context here is an RCT in which the control (comparator) group is something **other than placebo (OTP)**, and which can be regarded as a 'comparative effectiveness' study. Whereas for placebo-controlled trials of IHT, we were able to ascertain whether individually prescribed homeopathic *medicines* can have effects above those of placebos,[4] our proposed study of OTP-controlled trials will aim to ascertain the comparative effectiveness of IHT *as a whole*.

Two essentially different comparator options exist for OTP study design of RCTs: (I) other therapeutic intervention (e.g. a conventional medicine or a physical therapy), which can be sub-divided into (a) trials in which IHT is given as an *alternative* to the comparator intervention, and (b) trials in which IHT *combined with the other intervention* is compared with the other intervention alone (the '[A+B] versus B' approach); (II) no therapeutic intervention.

The nature of the research evidence in homeopathy has long been a matter of scientific debate. Recently, however, the argument has reached the point of impasse, despite pleas to the contrary.[5] Further clarification of the existing evidence, based on explicit, transparent and replicable systematic review methods, might moderate such divergent opinions: the nature and the quality of the research evidence must be considered, with an additional requirement to reflect the distinction between individualised and non-individualised homeopathy.

The pinnacle of the hierarchy of clinical research publications ('type 1' evidence) comprises systematic reviews (SRs), of which several have been published on RCTs in homeopathy. Some SRs have focused on specific medical conditions, typically examining placebo-controlled trials, with conclusions about efficacy of homeopathic medicines that are variously positive,[e.g. 6,7,8] negative[e.g. 9,10,11] or non-conclusive.[e.g. 12,13,14]

Five 'global', or 'comprehensive', SRs have examined the RCT research literature on homeopathy in general, including the broad spectrum of medical conditions that have been researched, and by all forms of homeopathy. Four of these SRs reached the cautious conclusion that, overall, the effect of a homeopathic medicine differs from that of placebo.[15,16,17,18] When Linde and colleagues carried out a sensitivity analysis on the data that informed their 1997 global SR based on quality of the 89 eligible trials, the observed effects were substantially reduced, though homeopathic medicines remained significantly more effective than placebo for all but the final 5 highest-quality trials collectively.[19] Neither

of Linde's reviews found sufficient evidence to draw conclusions about the efficacy of homeopathic medicines for any specific medical condition. The SR by Shang et al, published in 2005, concluded that there was "weak evidence for a specific effect of homoeopathic remedies…compatible with the notion that the clinical effects of homoeopathy are placebo effects".[20] Shang's methods and conclusions have subsequently been criticised.[e.g. 21]

Only one global SR has considered solely RCTs of individualised homeopathy that were controlled by an OTP intervention.[22] Published 17 years ago, this SR identified just six eligible trials, each of which was judged to contain serious methodological flaws. Findings for these trials were mixed: two favoured individualised homeopathy, two favoured conventional drugs, and two were non-conclusive either way. Each trial investigated a different medical condition, and none had been replicated by an independent research group. It was concluded that the 'value of individualized homoeopathy relative to allopathic treatments' was not known.

Previous reviews contain two key limitations:

1. Global SRs have typically assessed the RCT findings of all forms of homeopathy (individualised, clinical, complex, isopathy) together, as if they are the same intervention. As discussed above, there are marked differences in the nature of the therapeutic interventions, and the distinction between them is important, for it affects the interpretation of the research findings in each case. Placebo-controlled RCTs of a particular homeopathic medicine (non-individualised homeopathy) allows conclusions about that medicine's efficacy for the clinical condition investigated in the cohort of subjects concerned. In placebo-controlled trials of IHT, however, such 'efficacy' applies to the entire range of homeopathic medicines prescribed to the individuals included in the trials; in our meta-analysis of such RCTs, and from sensitivity analysis based on just three trials identified as having minimal risk of bias, we identified a small treatment effect that was attributable to those homeopathic medicines.[4]

2. Though not *systematic* reviews, some previous accounts of homeopathy research, including our own,[23] have summarised the findings of RCTs using 'vote counting', whereby each trial is designated 'positive' or 'negative' or 'non-conclusive' based on its most important statistical findings. While such an approach has the advantage that it overcomes problems associated with heterogeneous groups of trials and reflects the condition-specific nature of the research evidence, it does not grapple adequately with the key matters of treatment effect size or direction, or the strength of the evidence. For an OTP-controlled trial, vote counting might lead to 'no significant difference between test intervention and active comparator' being interpreted as positive evidence for the test intervention, when in fact statistical equivalence can be concluded only for a trial whose hypothesis is based on equivalence rather than superiority.[24] Nor does this 'vote counting' method reflect a single 'main outcome measure' of each trial in a systematic way. There is a need to quantify treatment effects of homeopathic interventions, and the use of a systematically and consistently determined 'main outcome measure' per RCT is helpful in focusing on matters of greatest clinical importance for a given medical condition (or category of conditions).

Four additional matters also need to be addressed:

a. Nearly all SRs to date have examined RCTs of treatment and of prophylaxis indistinguishably. However, the homeopathic approach to these two clinical scenarios is different: a person's *actual* symptoms are the target of homeopathic *treatment*, but his/her *anticipated* or *expected* symptoms are the basis for homeopathic *prophylaxis*.

b. The *internal validity* of a trial (the extent to which the design, conduct and analysis has minimised or avoided biases in its comparison of treatments[25,26]) reflects the quality of its methods of randomisation, blinding, and a number of other key attributes. Some comprehensive reviews have used a numerical system, such as the Jadad score,[27] to assess RCT quality in homeopathy. More modern systems of assessment do not allocate single overall scores; instead, they adopt qualitative standards against which a trial's internal validity is judged as having low, uncertain or high risk of bias.[28] Neither system is intended

to enable the identification of finer distinctions in degree of quality.

c. Concerns about RCT quality in homeopathy go beyond – though include – internal validity (risk of bias).[29] Indeed, in the context of OTP-controlled RCTs, it is the *external validity* (generalisability of results) that should normally be maximised,[30] though it is seldom formally addressed in SRs of such trials.[31] A tool (*PRECIS*), developed by Thorpe et al. in 2009 for use in SRs and their design, is available to assess a trial's positioning on the 'explanatory-pragmatic continuum'.[32] For our SR of OTP-controlled trials, we plan to assess each trial's internal validity and the extent to which it is 'pragmatic' in approach; the model validity of the same trials will be addressed separated in a linked project.

d. Few of the previous SRs in homeopathy have made the distinction between substantive and minor research articles or between the peer-reviewed and non-peer-reviewed research literature: an abstract presented at a conference, for example, has often been given a status equal to that of a paper published in a high-ranking academic journal.[e.g. 17,20] Peer review is an important, though by no means flawless, surrogate for research quality. For some commentators, it is "an essential arbiter of scientific quality" and "information about the status of research results is as important as the findings themselves".[33] SRs in homeopathy need to reflect, *a priori*, the distinction between the substantive peer-reviewed journal literature and other, lesser, categories of research evidence.

Aim of the study

The overarching aim of this SR/meta-analysis is to examine the comparative effectiveness of IHT in adults or children with any medical condition that has been the subject of at least one OTP-controlled trial. We shall include RCTs that have examined IHT (Ia) as alternative treatment, (Ib) adjunctively with any eligible type of other intervention, or (II) compared with no intervention. A single 'main outcome measure' will be identified per RCT.

For each trial and, if feasible, collectively for any medical condition or category of conditions in which there is >1 eligible study, we shall reflect matters of study quality (risk-of-bias assessment), methodologically pragmatic approach, and the precise nature of the OTP-controlled study design, to ascertain the degree of clinical effectiveness of IHT.

METHODS

Eligibility criteria, information sources, study selection and data collection

The eligible research literature was identified, to *PRISMA* standards, in a previous paper by our group.[34] From 489 potentially eligible records found up to and including 2011 (fully up-to-date at the time), 263 fulfilled the criteria of a substantive, non-repeat, journal paper that reported a randomised and controlled study of homeopathy.

Twelve of those records reported an OTP-controlled trial of individualised homeopathy and were published in the peer-reviewed journal literature. A subsequent search update, carried out for the first phase of the systematic review programme proper, identified an additional **five** relevant papers published in 2012 or 2013.[4] A further search update, carried out in March 2016, identified **two** further relevant papers, published in 2014 or 2015. **Figure 1** is based on our original *PRISMA* flowchart,[34] and incorporates the additional seven papers from 2012-2015, giving **19** in total.

Specific **exclusion criteria** have been applied, as appropriate, to these 19 records:

- Trials of homeopathic prophylaxis[a]
- Trials with crossover design[b]
- Research using radionically prepared 'homeopathic' medicines [e.g. 35]

[a] *Prophylaxis:* A trial on healthy individuals in which the homeopathic intervention aims to prevent the occurrence of disease *de novo* (i.e. 'primary prevention'). Studies using a strategy of primary prevention, with subsequent treatment as necessary, are categorised 'treatment' trials.
Treatment: A trial in which the first homeopathic intervention takes places after the onset of symptoms associated with disease. Studies on sub-clinical disease or the control of recurrent disease ('secondary prevention') are categorised 'treatment' trials. RCTs of homeopathic prophylaxis will be appraised in a separate SR.

[b] In due course, crossover trials will be appraised separately from those of parallel-group design.

- The tested intervention is IHT in tandem with other (complementary or conventional) medicine or therapy, but where the nature of the comparator intervention makes it impossible to distinguish any effects due to IHT.[c]
- Other specified reason.

Whereas a placebo-controlled trial of individualised homeopathy can be fully blinded, it is more difficult – and sometimes impossible – to achieve such blinding in a corresponding OTP-controlled trial. Unlike the case for our systematic review of placebo-controlled trials, therefore, patient- and practitioner-unblinded trials *will* be eligible for the current SR of OTP trials. Such trials are likely to be rated 'high risk of bias' in the relevant assessment domain: we recognise that this is a normal feature of an OTP-controlled trial, and which thus inevitably limits its internal validity – see also the section *Sensitivity analyses*, below.

Five records met the above exclusion criteria, leaving 14 therefore that are eligible for SR – see Figure 1. Any record whose main outcome measurement is not extractable (see below) will be ineligible for meta-analysis.

Only published data will be eligible for analysis. Because it is recognised that contacting the original authors of RCTs may lead to limited or overly positive answers,[28] the authors of eligible papers will not be approached for clarification on unclear or missing facets of any of their methods or results; however, original authors' cross-reference to their previously published study methods will be followed up and taken into account as necessary. For trials with more than two study groups, and where such trials have not previously been catalogued under 'placebo-controlled', only the data concerning comparisons between IHT and OTP will be extracted from the 14 papers; in relevant cases of more than one OTP control, a study group comprising actual treatment will be favoured for analysis over one comprising 'no treatment'.

Study characteristics and data items

Two reviewers independently will extract relevant data using a standard data recording approach, in spreadsheet format (Microsoft *Excel*). The data extracted per trial will include, as appropriate: demographics of participants (gender, age range, medical condition); study setting; potency or potencies of homeopathic medicines; dosage frequency; whether a pilot trial; 'main outcome measure' (see below) and measured end-point; other outcome measures reported; funding source/s. The statistical items noted will be: sample size and missing data (including patient non-compliance) for each intervention group; whether power calculation carried out; whether intention-to-treat (ITT), per-protocol, complier-average-causal-effect (CACE),[36] or other type of primary analysis.

Identification of 'main outcome measure' per RCT:

For each trial, and for the purposes of risk-of-bias assessment, we shall identify a single 'main outcome measure' using a refinement of the approaches adopted by Linde et al. and by Shang et al.[17,20] Each trial's 'main outcome measure' will be identified based on the following hierarchical ranking order (consistent with the WHO ICF Classification System for Levels of Functioning Linked to Health Condition):[d]

- Mortality
- Morbidity
    - Treatment failure
    - Pathology; symptoms of disease
- Health impairment (loss/abnormality of function, incl. presence of pain)
- Limitation of activity (disability, incl. days off work/school because of ill health)
- Restriction of participation (quality of life)
- Surrogate outcome (e.g. blood test data, bone mineral density).

[c] This study design is distinct from the eligible 'A versus [A + B]' design, and from eligible studies that allow concomitant conventional medication to remain ongoing in the subjects of *each* study group. Also, this criterion does not – *per se* – disallow studies where IHT intervention includes lifestyle and/or dietary advice in addition to a homeopathic prescription. See *Appendix 1* for tabulation of eligible and ineligible study designs.

[d] Towards a Common Language for Functioning, Disability and Health. ICF: The International Classification of Functioning, Disability and Health. Geneva; World Health Organization, 2002.

We shall follow the WHO ICF system regardless of what measure may have been identified by the investigators as their 'primary outcome'. In cases where, in the judgment of the reviewers, there are two or more outcome measures of equal greatest importance within the WHO ICF rank order, the designated 'main outcome measure' will be selected randomly from those two or more options using the toss of coins or dice.

Unless otherwise indicated, the single end-point (measured from the start of the intervention) associated with the designated 'main outcome measure' will be taken as the last follow-up at which data are reported for that outcome.

Risk of bias in individual studies (internal validity)

Using the standard criteria defined by Cochrane,[28] the extraction of information will enable appraisal of 'low risk', 'uncertain risk' or 'high risk' of bias with respect to: (domain I) the methods used to generate the random sequence; (domain II) the method of allocation concealment used to implement the random sequence; (domain IIIa) the blinding of participants and/or study personnel; (domain IIIb) the blinding of outcome assessors;[e] (domain IV) completeness of the outcome data included in the analysis; (domain V) evidence of selective outcome reporting; (domain VI) evidence of other bias, including data imbalance between the groups at baseline.

Two assessors will mutually scrutinise and compare their judgments, with discrepancies between them resolved by consensus discussion and, if necessary, the input of a third assessor. A risk-of-bias summary table will be produced, characterising each of the 14 eligible records. For domain IV, a trial will normally be regarded as no better than 'unclear' if there is greater than 20% participant attrition rate, irrespective of whether ITT analysis has been carried out. Domain V will automatically be designated 'high risk of balance' if its main outcome measure cannot be extracted to enable calculation of 'relative effect size' (see below).

Assessment of domain VI will explicitly include appraisal of inter-group data imbalance at baseline; the source of any research sponsorship will be taken into account for sub-group analysis (see below), not in risk-of-bias assessment *per se*.

Rating of trials for risk of bias

As per the standard Cochrane approach, each trial will be designated: **low risk of bias** for all key domains; **uncertain risk of bias** for one or more key domains; **high risk of bias** for one or more key domains.[28] We shall use our novel method of nomenclature, based on the Cochrane approach, for rating risk-of-bias characteristics across all domains per trial:[2,3]

A = **Low risk of bias** in all seven domains.
B*x* = **Uncertain risk of bias** in *x* domains; low risk of bias in all other domains.
C*y.x* = **High risk of bias** in *y* domains; uncertain risk of bias in *x* domains; low risk of bias in all other domains.

Assessment of trials for generalisability (external validity):

Equating generalisability to pragmatic trial attitude, we shall adopt the *PRECIS* approach[32] to assess pragmatic/explanatory attitude, taking account of ten domains:

1. Participant eligibility criteria;
2. Experimental intervention flexibility;
3. Experimental intervention practitioner expertise;
4. Comparison intervention;
5. Comparison intervention practitioner expertise;
6. Follow-up intensity;
7. Primary trial outcome;
8. Participant compliance with 'prescribed' intervention;
9. Practitioner adherence to study protocol;
10. Analysis of primary ('main') outcome.

Against a set of standard judgmental criteria,[32] we shall aim to assess each of the ten attributes as 'more explanatory than pragmatic' or 'more pragmatic than explanatory'.

[e] Domains are designated $IIIa_1$, $IIIa_2$ and IIIb to reflect their common – but separately identifiable – focus on matters connected with blinding.

Summary measures for 'main outcome'

A 'summary of findings' table (containing relevant [per-protocol] data from the trials) and a summary risk-of-bias table will be prepared.

For the 14 relevant records of IHT, we shall examine: (1) **overall relative effect size**; (2) **disease-specific relative effect sizes**; (3) **disease category-specific relative effect sizes**. In each of these three cases, 'relative effect size' will be taken as the difference (if relevant – see below) between the homeopathy and the OTP groups at our pre-determined end-point of the trial, and using per-protocol data:

- For **dichotomous measures**: odds ratio (OR), with 95% confidence interval (CI);[f]
- For **continuous measures**: standardised mean difference (SMD), with 95% CI.

In trials where the main outcome measure is a continuous variable, and where there are insufficient data presented to identify the mean and/or the SD per group at the defined end-point, the necessary data will be calculated or estimated, if possible, by imputing relevant other data (e.g. SD at baseline) from the same study.[37]

If the original paper does not provide or inform adequate data on the selected 'main outcome measure' to enable extraction or calculation of mean and/or SD, we shall describe the selected main outcome as 'not estimable': an alternative, estimable, outcome will *not* be sought.

Consistent with the above, the following studies will be excluded from meta-analysis:

- Those that present non-parametric data only, and where there is no information that enables the data distribution to be assessed;
- Those from which the necessary data cannot be extracted (not provided or uninterpretable).

Statistical interpretation

Interpretation of statistical finding: P≤0.05 (direction of effect toward homeopathy):

> *(Ia) Other-intervention control:* IHT is more effective than the other intervention;
> *(Ib) '[A+B] versus B':* IHT+ other intervention is more effective than the other intervention alone;
> *(II) No-treatment control:* IHT is more effective than no intervention.

Interpretation of statistical finding: P≤0.05 (direction of effect toward control):

> *(Ia) Other-intervention control:* IHT is less effective than the other intervention;
> *(Ib) '[A+B] versus B':* IHT+ other intervention is not more effective than the other intervention alone;
> *(II) No-treatment control:* IHT is ineffective.

Interpretation of statistical finding: P>0.05 (direction of effect toward either homeopathy or control):

> *(Ia) Other-intervention control:* Inconclusive whether IHT and the other intervention differ in effectiveness;
> *(Ib) '[A+B] versus B':* Inconclusive whether the effectiveness of IHT+ other intervention differs from that of the other intervention alone;
> *(II) No-treatment control:* IHT is probably ineffective.

Detailed interpretation of the above will reflect, where feasible, whether the other intervention is recognised as best standard care for the relevant medical condition: in cases where the comparator is best effective standard care, IHT is not expected to be found *more* effective statistically, and so our conclusions will instead reflect on matters of equivalence or non-inferiority.[24,38] This judgmental approach will predominate, by default, in the examination of trials that are prospectively attributed 'equivalence' or 'non-inferiority' by their original authors. In the latter cases, we shall reflect the original authors' pre-stated margin of equivalence or non-inferiority, as appropriate. In cases where the comparator is *not* a standard treatment for the medical

[f] If the main outcome is reported as data in more than two categories, these will be dichotomised as appropriate.

condition, a clear judgment about IHT's comparative effectiveness may not be possible.

For any RCT or group of RCTs on a given medical condition/category, the interpretation of IHT as 'effective', 'ineffective' or 'inconclusive' will apply solely to the particular medical condition/ category being examined. We recognise also that we shall be using per-protocol data (as opposed potentially to the original authors' use of ITT or CACE, for example), and that the selected main outcome measure/s might be insensitive to change.

Synthesis of quantitative results (if the extracted data allow)

1) Overall 'relative effective size' of IHT

For groups of eligible RCTs that have compared IHT, as (Ia) alternative or (Ib) adjunct, with another intervention, or (II) with no treatment, the 'main outcome' data will be synthesised for meta-analysis in two separate sets of studies as appropriate: (1) using the OR of each relevant trial; (2) using the SMD of each relevant trial.[39] A summary measure of 'relative effect size' will be identified across all included studies for each of those two sets. Based on the assumption of clinical heterogeneity, the 'random effects' statistical model will be used rather than the 'fixed effects' model.[39] Illustration of findings will be by means of forest plots.

For each of study designs (Ia), (Ib) and (II), data from the two sets of studies above (OR and SMD) will then also be combined into a single forest plot, re-expressing SMDs by transformation to OR, using an approximation method proposed by Chinn[40] and recommended by the Cochrane Statistical Methods Group.[39]

2) Disease-specific 'relative effect size' of IHT

For each specific medical condition or category of conditions, for each of study designs (Ia), (Ib) and (II), and for which there is >1 RCT of given type and with extractable main outcome, the data will be synthesised using meta-analysis methods. For each of these particular analyses, *a single* 'main outcome measure' will be designated, if possible, for each medical condition, and reflecting the WHO classification ranking approach (see above). A summary estimate of 'relative effect size' per condition, with 95% CI and *P* value, will be illustrated by means of forest plot. The 'random effects' statistical model will again be used.[39]

3) Measures of consistency:

Asymmetry of each of the above forest plots will be determined from visual inspection of the associated funnel plot graph and by interpretation of the asymmetry (heterogeneity) statistic, I^2. The I^2 statistic evaluates the variability between studies, taking values between 0% and 100%: high values are indicative of strong heterogeneity.

Additional quantitative analyses on overall 'relative effect size' of IHT *(specified prior to data analysis)*

Sensitivity analyses:

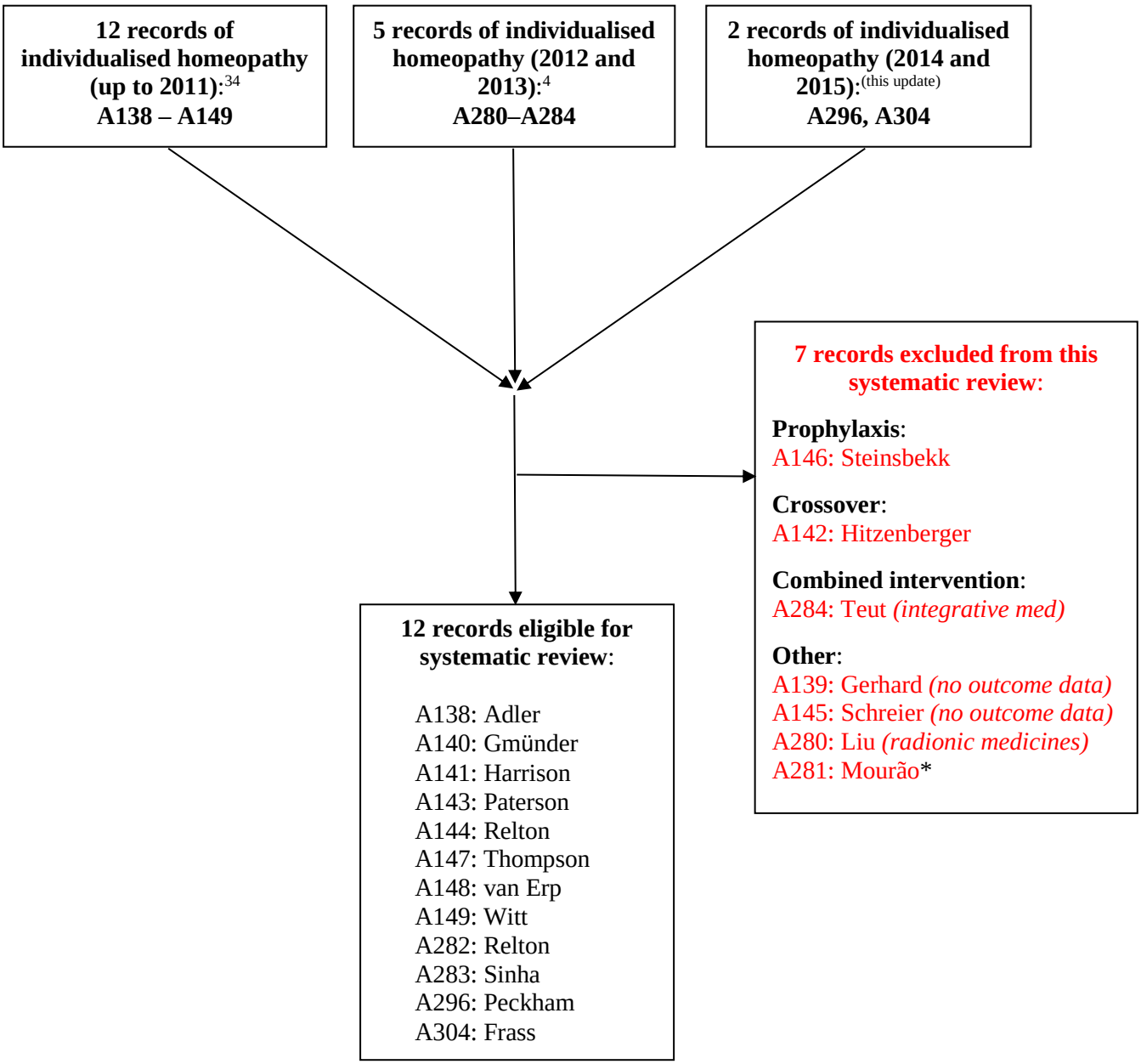
We shall carry out sensitivity analyses based separately on (1) our risk-of-bias ratings and on (2) our assessments of external validity. For a trial categorised as 'more pragmatic than explanatory', we shall accommodate an expectation of high risk of bias in domain IIIa (concerning blinding of participants and/or study personnel), which we recognise as a standard feature of pragmatic trial design in individualised homeopathic treatment.

Sub-group analyses:

Comparative forest plots are planned – for each of study designs (Ia), (Ib) and (II) – on the following sub-groups of trial attributes:

- Whether or not a pilot (or 'preliminary' or 'feasibility') study, as defined by the original authors;
- Whether or not sample size > median for all trials with extractable data;
- Whether or not potency/potencies of homeopathic medicines $\geq$ 12C;
- Whether or not the research sponsor is an organisation (e.g. homeopathic pharmacy) that potentially has vested interest in the trial findings.

Figure 1: *Details of numbered references for OTP trials, as per original* PRISMA *flowchart* [34]*, and updated references published in 2014-2015*



* A281: Mourão was originally designated 'individualised homeopathy' (see *Additional file 2* of Mathie et al. 2014).[4] Its more detailed scrutiny for the current flowchart re-designates it 'non-individualised homeopathy'; it will be included in the relevant systematic review.

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Appendix 1: **Eligible (*Yes*) and ineligible (*No*) combinations of intervention and comparator**

		Intervention	
		IHT	**IHT + Other Intervention #1**
Comparator	**Other Intervention #1**	*Yes*	*Yes*
	Other Intervention #2	*Yes*	*No*
	Nothing	*Yes*	*No*

IHT: Individualised homeopathic treatment