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Dismantling and personalising task-sharing psychosocial interventions for common mental disorders: a study protocol for an individual participant data component network meta-analysis

Davide Papola,[1,2] Eirini Karyotaki ,[3] Marianna Purgato ,[2] Marit Sijbrandij,[3] Federico Tedeschi,[2] Pim Cuijpers ,[3] Efthimiou Orestis,[4,5] Toshi A Furukawa ,[6] Vikram Patel ,[1] Corrado Barbui[2]

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For numbered affiliations see end of article.

Correspondence to
Dr Davide Papola; davide_papola@hms.harvard.edu

ABSTRACT

Introduction Common mental disorders, including depression, anxiety and related somatic health symptoms, are leading causes of disability worldwide. Especially in low-resource settings, psychosocial interventions delivered by non-specialist providers through task-sharing modalities proved to be valid options to expand access to mental healthcare. However, such interventions are usually eclectic multicomponent interventions consisting of different combinations of evidence-based therapeutic strategies. Which of these various components (or combinations thereof) are more efficacious (and for whom) to reduce common mental disorder symptomatology is yet to be substantiated by evidence.

Methods and analysis Comprehensive search was performed in electronic databases MEDLINE, Embase, PsycINFO and the Cochrane Register of Controlled Trials—CENTRAL from database inception to 15 March 2023 to systematically identify all randomised controlled trials that compared any single component or multicomponent psychosocial intervention delivered through the task-sharing modality against any active or inactive control condition in the treatment of adults suffering from common mental disorders. From these trials, individual participant data (IPD) of all measured outcomes and covariates will be collected. We will dismantle psychosocial interventions creating a taxonomy of components and then apply the IPD component network meta-analysis (IPD-cNMA) methodology to assess the efficacy of individual components (or combinations thereof) according to participant-level prognostic factors and effect modifiers.

Ethics and dissemination Ethics approval is not applicable for this study since no original data will be collected. Results from this study will be published in peer-reviewed journals and presented at relevant conferences.

STRENGTHS AND LIMITATIONS OF THIS STUDY

⇒ We will create a taxonomy of treatment components for task-sharing psychosocial interventions to treat common mental disorders in poor resource settings.
⇒ Thanks to the component network meta-analysis (cNMA) methodology, we will estimate specific incremental effect size for each component.
⇒ Through the individual participant data cNMA (IPD-cNMA), we will identify prognostic factors and effect modifiers for the different components.
⇒ IPD-cNMA is limited by the availability of individual participant data, their quality and their comprehensiveness.

INTRODUCTION

Depression and anxiety are leading sources of disability worldwide,[1 2] with depressive disorders alone being among the leading causes of disease burden globally affecting 246 million people and contributing to 49.4 million disability-adjusted life-years.[3] Anxiety and related somatic complains, with a global prevalence estimated at 7.3%, are also major drivers of disability.[4] The term "common mental disorders" (CMD) is used to describe the heterogeneous presentation of depressive, anxiety and somatic symptoms in community or primary care samples.[5] Although evidence-based psychological and social interventions for CMDs are available, they remain inaccessible for the wide majority of people living in low-resource settings, where less than 5% of people with CMDs receive minimally adequate treatment.[6]

At the roots of this huge treatment gap is the great shortage and inequitable distribution of specialised mental healthcare personnel across the mental healthcare systems globally, and the dominant role played by pharmaceutical interventions.[7] A recent Lancet series has underscored the growing need to identify how scarce resources can be used efficiently, effectively and feasibly to implement

global mental health policies.[8] To this account, 'task-sharing' of psychosocial interventions has proved to be beneficial. The WHO defines task-sharing as 'the rational redistribution of tasks among health workforce teams'.[9] In other words, to make more efficient use of the available human resources for healthcare delivery, specific functions are shared from highly qualified health workers to health workers with fewer qualifications and shorter training . Meta-analyses of randomised controlled trials (RCTs) showed that psychosocial interventions delivered by locally available non-specialist providers (NSPs) in community and primary care settings are effective in treating CMDs in poor resource settings.[10–12] Further insights from a recent individual participant data (IPD) meta-analysis suggested that seven individuals need to be treated to expect one individual with a 50% reduction in baseline depressive symptoms, a proportion comparable with those of the most common antidepressant medications when administered for the treatment of depression, as compared with pill placebo.[13 14]

However, mechanisms and predictors of response to intervention components, key for improving effectiveness and for precision medicine, are poorly understood for at least three reasons.[15 16] First, psychosocial interventions used in the context of task-sharing are usually multicomponent; they comprise multiple, distinct and possibly interacting active psychological and/or social components. These components may include behavioural, interpersonal, cognitive, problem solving, psychoeducational strategies, as well as social work elements (ie, a range of strategies and approaches aimed at addressing individuals' social well-being).[17–21] The standard meta-analysis methodology is not well suited to shed light on the efficacy of each of these multiple components, as they are packed in heterogeneous combinations. Second, the detection of differences among all interventions and all intervention components through individual dismantling studies is not feasible, as it would require a huge number of randomised studies with extremely large samples. Although dismantling studies have been carried out to shed light on the efficacy of selected intervention components,[22] interventions in these studies were conducted by highly skilled psychotherapists, hence it is not clear whether they are still effective when applied by NSPs through the task-sharing modality. Third, it is impossible to ascertain which intervention works best and for whom using aggregate (study-level) information, as analyses that rely on group averages can be misleading about true effects at the level of individual patients.[23–25] Since intervention components are assumed to be important drivers of outcomes, it is key to identify which of them achieve the best outcome, and what their corresponding effect sizes are.[26]

Aim of the study

We aim to investigate which task-shared psychosocial intervention components have the best efficacy in people suffering from CMDs, identifying the impact of participant-level prognostic factors (baseline characteristics which predict the outcome regardless of the intervention) and effect modifiers (covariates which predict differential response to treatments) on intervention outcomes.

To achieve our goal, we will employ the 'individual participant data component network meta-analysis' methodology (IPD-cNMA). We plan to collect IPD from RCTs that tested the efficacy of task-shared psychosocial interventions for people suffering from CMDs and use them in the component network meta-analysis (cNMA).[27] In this way, we will dismantle and compare the efficacy of psychosocial intervention components while personalising research findings at the same time, to detect which types of patients may benefit more from different components, or their combinations.[28] The findings generated by our investigation will allow to identify the best-performing active components of task-shared psychosocial interventions in the global mental health field, and tailor interventions on the needs and preferences of individuals suffering from CMDs.

METHODS

This protocol is reported according to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) for study protocols (see online supplemental file 1).[29]

Eligibility criteria

Types of studies

We will include any studies that allocated participants or clusters of participants by a random method to a task-shared psychosocial intervention or a control condition, or to another task-shared psychosocial intervention. We will exclude RCTs comparing psychosocial interventions against drug treatment (irrespective of drug class and dosage) and/or placebo pill. The study selection process will be reported in accordance with the PRISMA guidelines. There will be no restrictions on publication type, status, language or date.

Types of participants

We will include studies that enrolled adult participants of both sexes, aged 18–65, suffering from CMD as defined by the WHO International Classification of Diseases (ICD)-11 for 'mental and behavioural disorders'. These categories are most likely to be used in low-resource setting service delivery: depressive disorders (ICD-11 code: 6A70–6A7Z); anxiety-related or fear-related disorders (ICD-11 codes: 6B00–6B06).[30] The aforementioned diagnoses will be identified either according to a diagnostic interview (eg, The Mini-International Neuropsychiatric Interview) or judged so by elevated scores at baseline on validated self-report scales measuring psychological distress (eg, the General Health Questionnaire 12), depressive (eg, Patient Health Questionnaire 9 items) or anxiety symptoms (eg, Beck Anxiety Inventory)

and level of functional impairment (eg, WHO Disability Assessment Schedule-2.0).

Comorbidities with another mental or physical disorder (eg, HIV, diabetes, hypertension) do not constitute exclusion criteria. We will exclude studies enrolling participants with severe mental disorders (such as schizophrenia, bipolar or related disorders), somatoform disorders, disorders related to substance abuse, participants with disorders specifically associated with stress (such as post-traumatic stress disorder (PTSD)), mental or behavioural disorders associated with pregnancy, childbirth or the puerperium. We will also exclude participants showing suicidal intent, or with cognitive impairment (eg, intellectual disability, dementia).

Types of interventions and comparators

We broadly conceptualise a psychosocial intervention as a non-pharmacological intervention focused on psychological or social factors or mechanisms, which contributes to an individual's mental health, well-being and social inclusion.[10] We will focus on 'task-sharing' interventions, that is, interventions delivered by NSPs. These are providers who are not mental health specialists but have received some mental health training for the specific purpose of delivering the intervention.[31] The NSP category includes community health workers, community volunteers, lay people and peers.[11]

Consistent with the exclusion of RCTs enrolling participants subject to violence or with disorder specifically associated with stress, trauma-focused interventions will be excluded. The inclusion of people diagnosed with PTSD receiving trauma-focused interventions would create imbalances in the network both in terms of type of components and distribution of characteristic across the network comparisons, threatening the transitivity assumption.[32] Stepped care as well as collaborative care interventions will be excluded as they preclude homogeneity in intervention and components administration within trial arms. We will include interventions delivered in any intervention delivery modality, such as individual in-presence, group in-presence, remote synchronous or asynchronous, guided self-help, or telephone, as long as the intervention is task-shared.[33] We will not exclude studies in which participants were allowed to take antidepressant medications, as long as the prescription is balanced across comparison groups.

The control conditions of interest will include waiting list control, no treatment control or (enhanced) treatment as usual. When no treatment or treatment as usual is used as part of the waiting list, such arms will be classified as the waiting list control at the intervention level but will be appropriately decomposed at the component level. Often in the global mental health field treatment as usual conditions are 'enhanced', as in the trial context additional actions that would not have been implemented under ordinary circumstances are pursued, mostly for ethical reasons.[34] In our study, treatment as usual will be defined as whatever is provided in the facility for the patient's mental health condition, including conventional drug treatment either as part of the general practitioners' care or as part of the study protocol.

We will set no limits in terms of intervention duration, the number of sessions and the minimal number of participants.

Table 1 Hierarchy for the primary efficacy outcome

Hierarchy	Symptom severity rating scales	Abbreviation
1	General Health Questionnaire, 12 items	GHQ-12
2	Hospital Anxiety and Depression Scale	HADS
3	Hopkins Symptom Checklist, 25 items	HSCL-25
4	Depression Anxiety Stress Scales	DASS
5	Self-Reporting Questionnaire, 20 items	SRQ-20
6	Shona Symptom Questionnaire	SSQ-14
7	Patient Health Questionnaire, 4 items	PHQ-4
8	Brief Symptom Inventory, 18 items	BSI-18
9	Patient Health Questionnaire, 9 items	PHQ-9
10	Hamilton depression rating scale	HAMD
11	Montgomery-Asberg Depression Rating Scale	MADRS
12	Beck Depression Inventory, first or second version	BDI / BDI-II
13	Zung self-rating depression scale	Zung
14	Quick Inventory of Depressive Symptomatology	QIDS
15	Hamilton anxiety rating scale	HAMA
16	Anxiety and Related Disorders Interview Schedule	ADIS
17	Clinical Interview for Depression, anxiety subscale	CID anxiety subscale
18	Beck Anxiety Inventory	BAI
19	General Anxiety Disorder, 7 items	GAD-7
20	State-Trait Anxiety Inventory-State Version	STAI-S
21	State-Trait Anxiety Inventory-Trait Version	STAI-T
22	Zung Self-rating Anxiety Scale	ZUNG
23	Symptom Checklist-90, anxiety subscale	SCL-90 anxiety

Types of outcome measures

Our primary outcome will be CMD symptom reduction at study endpoint as measured on a continuous scale. Outcome scales will be prioritised according to the pragmatic outcome hierarchy depicted in table 1. If the studies use different outcome measures, they will be converted into a common metric through the equipercentile linking procedure. The equipercentile linking procedure is a statistical method used to establish a relationship between two different test scores or assessments that allows a nominal translation from one scale to another by identifying those scores on both scales that have the same percentile ranks.[35]

Our secondary outcome will be dropout from treatment, defined as dropout from the end-of-treatment assessment for any reason as a proxy measure of treatment acceptability.

Setting

To expand access to mental healthcare a bridge needs to be built between two fields that are frequently siloed off from each other: research and implementation policies carried out in 'low-income and middle-income countries' (LMICs) and in 'high-income countries' (HICs). There is a misunderstanding regarding the fact that global mental health should be constrained to operate in LMIC, whereas compelling evidence has accumulated to suggest that task-sharing intervention delivery modality can play a substantial role in making mental healthcare better in all contexts, including within HICs.[36–40] For this reason, studies from any country will be included.

Study identification and selection

Four bibliographical databases (MEDLINE, Embase, PsycINFO and the Cochrane Register of Controlled Trials-CENTRAL) and the International Clinical Trials Registry Platform were searched from database inception to 15 March 2023 by two independent researchers to identify RCTs suitable for inclusion according to the above-mentioned inclusion/exclusion criteria. Any disagreement will be resolved by discussion and, where necessary, in consultation with a senior author. In the search strings, we combined index terms and text words indicative of depression, anxiety, psychological distress and interventions delivered through the task-sharing modality in mental health, with filters for RCTs (see online supplemental file 2). We will also add references of trials through other sources, such as other meta-analyses and an existing database of studies on the psychological treatment of depression which served to inform the IPD of Karyotaki *et al*.[13 41 42] We will also ask the primary authors of the eligible studies if they are aware of any other study that has been conducted in the field.

Data collection and integrity checks

Authors of the eligible studies will be contacted and requested to contribute their individual-level data. The corresponding author will be contacted first; if unreachable, a follow-up email will be sent to the senior author of the study. Reminders will be sent after 2 weeks and if necessary, after 4 weeks. If no response is received after an additional 4 weeks, the trial will be classified as 'IPD unavailable' and will be included in the analyses at the aggregate data level.[43 44] Attached to the email, there will be the present study protocol. Individual-level information will include sociodemographic and clinical characteristics, primary and secondary outcome measures, date of randomisation and date of follow-ups. After gathering all primary datasets of the included trials, the data will be checked against the published reports of the trials to ensure the accuracy of the dataset. More specifically, we will check the frequencies of sociodemographic variables (eg, gender, education, marital status) as well as the mean scores of outcome scales. In case we will not be able to replicate the frequencies and means of the data reported on the published papers, we will consult the corresponding author of the trial to clarify the reason for such discrepancies. After checking each dataset, we will merge the data into the IPD meta-analytical dataset. We will harmonise data by converting to the level of the least detailed information. For example, transformation of continuous data to a binary categorisation (ie, number of years employed into 'employed vs unemployed').[45] All study data will be entered in a computerised password-protected database, only accessed by named study staff and securely stored by the Department of Global Health and Social Medicine, Harvard Medical School, Boston, Massachusetts, USA. All study data will be used only for the purposes stated in this study protocol and will not be forwarded to third parties.

Identification of components

Two independent reviewers will classify the identified intervention and comparator trial arms and their constituent components into a taxonomy of active components. We will start by reviewing existing dismantling blueprints (ie, existing taxonomies of common psychological treatment elements and behavioural change techniques used for CMDs).[17–21] Then, we will create the taxonomy using all available information from the publications, reviewing the intervention protocols of the identified RCTs (if available) and inquiring with the original investigators. Working in pairs, we will compile a list of eligible components and review these for duplication and redundancy. After that, each component will be operationalised and coded. Following an iterative process, similarities and discrepancies will be discussed among the coders, and the taxonomy modified accordingly. Any disagreement will be resolved by the two reviewers and, where necessary, in consultation with a senior member of the review team.

Prognostic factors and effect modifiers of intervention outcome

We will start from both study-level and individual participant-level variables. We will select candidate covariates based on previous literature findings, and depending on what will be available in the study datasets. Candidate participant-level variables based on the published literature include for example sex, age, level of education, employment, marital status, duration of current episode, prior treatments, baseline severity, baseline psychomotor symptoms, comorbid alcohol or substance abuse. Candidate study-level variables include duration of intervention and intervention delivery modality.[13 43 46]

Risk of bias assessment

We will assess the risk of bias (ROB) in included studies using Cochrane's second version of the 'ROB' tool for randomised trials (ROB 2).[47] We will assess RoB for the primary outcome at postintervention. Two review authors (DP and MP) will independently use the ROB 2 signalling questions to form judgements of material ROB for the following five domains: (1) bias arising from the randomisation process; (2) bias due to deviations from intended

interventions; (3) bias due to missing outcome data; (4) bias in the measurement of outcome and (5) bias in the selection of the reported outcome. ROB 2 allows for a judgement of overall ROB for each included study: low risk, some concerns or high risk. We will tag each study with a risk level according to the algorithm suggested by the ROB 2 tool guideline.[47] Any disagreements will be resolved by consulting with a senior author.

Missing data

In case of missing data in the IPD studies, available information at the IPD level will be used to impute the missing values; in particular, we will create multiply imputed datasets under the missing at random assumption with the jomo package in R.[48] This allows the imputation of either continuous or discrete, participant level or study level and systematically or sporadically missing data.[43]

Synthesis methods

As a preliminary analysis, we will perform a conventional NMA on aggregated data to verify whether effect modifiers are evenly distributed across network comparisons, that is, verify the validity of the transitivity assumption of the network.[43 44]

Then we will proceed to a two-step random effects network meta-analysis at the treatment level using IPD if we will obtain the datasets from all the included RCTs, otherwise using IPD studies and aggregate data. For IPD studies, we will use multiple imputations based on IPD to impute missing data (see above "missing data"),[48] for aggregate data studies we will use the published data. We will perform the network meta-analysis in a frequentist setting in R using netmeta,[49] assuming common heterogeneity for all treatment comparisons.[50] We will check network inconsistency, a statistical expression of intransitivity, using the back-calculation[51] and the design-by-treatment methods.[52]

If 10 or more studies will be included in a direct pairwise comparison, we will assess publication bias and small study effects by visually inspecting contour enhanced funnel plots, testing for asymmetry with the Egger's regression test.[53] We will assess the certainty in the body of evidence for the primary outcome through the Confidence in Network Meta-Analysis application.[54]

Next, we will use cNMA models that jointly synthesise aggregate data and IPD studies.[43] We will perform a two-step cNMA calculating trial-level estimates of treatment effects from studies for which IPD will be available and, therefore, can be reanalysed, and published trial-level estimates from studies for which IPD will not be available. The model will assume additivity of treatment effect, that is, the total effect of each composite intervention will be assumed equal to the sum of effects of the included components[55] and examines component–covariate interactions using shrinkage methods.[43 44 56]

For the primary outcome (continuous), we will estimate component-specific incremental mean differences to measure the added benefit of adding a component to a psychosocial intervention. The component–covariate interactions will be modelled assuming linearity. We will repeat the procedure for the secondary outcome (binary), using a binomial likelihood, to estimate incremental ORs for each component. We will abide by the intention-to-treat (ITT) principle as far as possible, that is, we will prefer ITT to per-protocol data, but if the trial only reports per-protocol data, the latter will be used. For the dichotomous outcome, we will consider the total number of randomised participants as denominator, and where participants had been excluded from the trial before the endpoint, we will consider this a determination of a negative outcome by the end of the trial. For continuous outcomes, we will use the data as reported in the original studies.

We will use the parameter estimates to develop a web app for which the inputs are patient characteristics and two combinations of components, and the output is the estimated relative treatment effects between the two combinations.

Sensitivity analysis

- We will examine the impact of studies focusing on patients with CMDs comorbid with a physical disorder by excluding such studies from the analyses.
- A sensitivity analysis will be carried out by excluding trials judged to be at 'high ROB' to explore the putative effects of the study quality on efficacy.

ETHICS AND DISSEMINATION

The results of the present project will be published in peer-reviewed journals and disseminated electronically and in print, as well presented as abstracts and/or personal communications during national and international conferences. The present project does not involve primary data collection from humans, as it will be based on secondary analyses of already collected anonymised datasets. This study was considered exempt from review by the Harvard Longwood Campus institutional review board. However, if local ethics committees of the original research consider it necessary to have approval from the local ethics committee, we will abide by their judgements. National and international regulations on patient privacy will be followed.

Author affiliations
[1]Department of Global Health and Social Medicine, Harvard Medical School, Boston, Massachusetts, USA
[2]WHO Collaborating Centre for Research and Training in Mental Health and Service Evaluation, Department of Neuroscience, Biomedicine and Movement Science, Section of Psychiatry, University of Verona, Verona, Italy
[3]Department of Clinical, Neuro and Developmental Psychology, WHO Collaborating Centre for Research and Dissemination of Psychological Interventions, Amsterdam Public Health research institute, Vrije Universiteit Amsterdam, Amsterdam, The Netherlands
[4]Institute of Primary Health Care (BIHAM), University of Bern, Bern, Switzerland
[5]Institute of Social and Preventive Medicine (ISPM), University of Bern, Bern, Switzerland
[6]Department of Health Promotion and Human Behavior, Kyoto University Graduate School of Medicine/School of Public Health, Kyoto University, Kyoto, Japan

Twitter Eirini Karyotaki @KaryotakiEirini and Toshi A Furukawa @Toshi_FRKW

Contributors DP, VP and CB conceived the study. DP drafted the protocol manuscript. EK, MP, MS, FT, PC, EO and TAF assisted in the protocol design and revision. DP, FT, CB, TAF and EO designed the statistical analysis; VP and CB are the guarantors. All authors read and approved the final version of the manuscript.

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ORCID iDs
Eirini Karyotaki http://orcid.org/0000-0002-0071-2599
Marianna Purgato http://orcid.org/0000-0002-3783-8195
Pim Cuijpers http://orcid.org/0000-0001-5497-2743
Toshi A Furukawa http://orcid.org/0000-0003-2159-3776
Vikram Patel http://orcid.org/0000-0003-1066-8584

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