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To cite this article: Oliver Rumle Hovmand, Jasmin Rejaye Gryesten, Ole Jakob Storebø, Nina Reinholt & Sidse M. Arnfred (2024) Collaborative assessment as an intervention in the treatment of mental illness: a systematic review, *Nordic Journal of Psychiatry*, 78:6, 457-464, DOI: [10.1080/08039488.2024.2357565](https://doi.org/10.1080/08039488.2024.2357565)

To link to this article: <https://doi.org/10.1080/08039488.2024.2357565>

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REVIEW ARTICLE



Collaborative assessment as an intervention in the treatment of mental illness: a systematic review

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ABSTRACT

Background: Three meta-analyses suggested that the psychological assessment as a therapeutic intervention approach might have therapeutic effects but had unspecific inclusion criteria.

Methods: We searched four databases for RCTs that reported on the use of psychological assessment as an intervention. Two reviewers independently selected papers, extracted data, and assessed study quality.

We conducted and reported the systematic review following the PRISMA statement. We assessed the Risk of bias in included studies using the Risk of Bias tool and graded the strength of the evidence with GRADE.

Results: We included two RCTs: The first study investigated Therapeutic Assessment (TA) combined with Manual-Assisted Cognitive Behavior Therapy (MACT) compared with MACT only in 16 outpatients with personality disorders. The trial found among completers ($n=7$) no difference in borderline symptomatology but a possible difference regarding suicidality favoring MACT+TA. The trial did not provide any outcomes relating to readiness for treatment. The other trial investigated TA compared with a Goal-focused Pretreatment Intervention in a sample of 74 outpatients with personality disorders. The results found no intervention effects on symptomatology but suggested that TA might improve patient expectancy for future treatment among completers of the intervention. Both trials were judged at a high risk of bias and with very low certainty of evidence.

Discussion: We found no support for the clinical effect of psychological assessment as a therapeutic intervention due to the high risk of bias and low certainty of the evidence.

ARTICLE HISTORY

Received 3 January 2024

Revised 3 May 2024

Accepted 15 May 2024

KEYWORDS

Assessment; therapeutic assessment; collaborative assessment



Introduction


A psychological assessment is usually exclusively carried out in order to diagnose mental health disorders, collect information about patients' symptoms, and plan and monitor treatment. This has, by Finn (1996), been described as the 'information gathering model of assessment' [1]. However, recent evidence suggests that a psychological assessment can be useful as a psychotherapeutic intervention, if it is performed in a way that enlists the patient as an active collaborator. Several researchers have developed frameworks for utilizing standardized psychological assessment procedures as therapeutic interventions. The most studied of these are the Therapeutic Assessment (TA) and Collaborative Assessment (CA) intervention frameworks [2,3]. In this text, we will describe these interventions as C/TA [3].

Fischer developed CA in the 1970s. She regarded collaborating with patients as an effective means of individualizing the assessment process and contextualizing patients'

problems in their life contexts. CA has yet to be applied broadly in clinical research but has served as an inspiration for TA [3]. Further, CA has formed the basis for the Collaborative Assessment and Management of Suicidality (CAMS). CAMS is a therapeutic framework for suicide-specific assessment and treatment of a patient's suicidal risk, where the clinician and patient engage in a collaborative assessment of developing a personalized treatment plan utilizing a standardized form [4].

During the 1990s, Finn and colleagues developed TA as a semi-structured, brief intervention grounded in psychological assessment. The goal of TA is to provide the patient with therapeutic benefits from the assessment process itself and change their narrative about themselves and their environment to a more positive one in opposition to the traditional information-gathering assessment paradigms [3]. The TA process starts with the gathering of 'assessment questions', where the patient makes a list of questions they would 'like to ask' the psychological tests, after which the assessor

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 Supplemental data for this article can be accessed online at <https://doi.org/10.1080/08039488.2024.2357565>.

begins standardized testing with those instruments that seem most related to the patient's main therapeutic questions.

The C/TA literature on clinical samples mainly comprises textbooks [1–3] and single case studies in a wide range of psychiatric illness [5–22]. In addition, four randomized clinical trials (RCTs) have been published on TA with non-diagnosed 'clients' [23–26], and two RCTs have been conducted on C/TA in the treatment of personality disorders, which are included in the present systematic review [25,26].

Poston and Hanson (2010) [27] conducted a systematic review and meta-analysis of 17 randomized clinical trials or non-randomized studies ($n=1,496$) and examined the effect of C/TA style interventions delivered in counseling psychology ($n=10$) and clinical psychology settings ($n=7$) [27]. They found a moderate effect of C/TA on across the 52 examined outcomes (Cohen's $d = .42$ [95% CI, 0.321 to 0.525]).

Based on a critique [28] for overly broad sampling and exaggerated results, Hanson and Poston (2011) [29] reanalyzed 14 remaining studies and reached a significant weighted Cohen's $d = .40$ [95% CI, 0.30 to 0.50] across outcomes.

Furher, Durosini and Aschieri (2021) published a systematic review and meta-analysis on nine primary studies ($n=491$) which compared TA to a control condition and calculated [30]. They found significant effects for each outcome (treatment process: Hedges $g = .46$, [95% CI, .33 to .59]; $p < .001$, $Q(17) = 8.62$, $p = .951$, $I^2 = \text{NA}$.; clients' symptoms: Hedges $g = .34$, [95% CI, .06 to .63]; $p = .021$, $Q(16) = 19.26$, $p = .255$, $I^2 = 32.94$; clients' self-enhancement: Hedges $g = .37$, [95% CI, .05 to .69]; $p = .029$, $Q(6) = 7.110$, $p = .311$, $I^2 = 19.37$).

Lastly, Aschieri et al. (2023) conducted a systematic review and meta-analysis of C/TA interventions (including CAMS) applied in a 'clinical setting' [31].

They reported on the results of 10 primary studies ($n=444$) comparing C/TA to a control group and calculated effect sizes for 70 different variables.

They found that C/TA, including CAMS interventions, had a moderate, significant effect on the treatment process ($d = .59$, $p=.0219$; 7 studies, 27 variables, $n=320$), a small but significant effect on symptom reductions ($d = .19$, $p=.036$; 7 studies, 32 variables, $n=332$), and lastly, a significant small-to-medium effect on personal growth ($d = .42$, $p=.017$; 5 studies, 11 variables, $n=264$) compared to control conditions. Based on these results, Aschieri et al. (2023) advocated implementing C/TA methods, including CAMS, when conducting a psychological or psychosocial assessment.

Nevertheless, the previous systematic reviews [27,29–31] had considerable methodologically flaws: considerable heterogeneity among the included trials regarding study design, populations, and outcomes and outcome measures. They placed no criteria whatsoever on diagnostic assessment, clinical diagnosis, and employed no clear definitions of clinical vs. non-clinical populations, or which studies used clinical vs. non-clinical samples. Further, they did not conduct tests for multiplicity [32]. Finally, the systematic reviews did not include appropriate assessment of the risk of bias of the included studies or graded the certainty of the evidence as suggested by contemporary guidelines [33,34].

Thus, the current status of the evidence for the possible effects and harms of C/TA remains unclear. Therefore, we aimed to conduct an updated systematic review and meta-analysis in clinical populations to provide an evidence synthesis of the potential benefits and harms of C/TA as compared with assessment as usual or other pretreatment interventions for adult primary or secondary outpatient mental health service settings. An intervention focusing on the psychological assessment of psychopathology could be particularly advantageous in the clinical mental health service setting where the complexity is high in contrast to an assessment carried out in non-clinical settings with less affected subjects.

Potentially, the current review could assist clinicians and policymakers in deciding which kind of assessment should be provided in clinics and whether the psychological assessment should be used as an intervention in and of itself or only as a diagnostic assessment. In addition, the review could guide future research on C/TA forward.

Methods

Searches

We conducted this review according to the requirements established in the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) protocols [35]. We undertook searches in electronic databases PUBMED, EMBASE, PsycInfo, Cochrane Library, Scopus, Web of Science and the Grey Matters database utilizing a search strategy that we had tested for sensitivity in advance. See [Appendix 1](#) for a detailed search strategy. In addition to this systematic search we also performed a supplementary hand search of the reference sections of retrieved papers, where we looked for references which sounded to be of interest, in case our search did not produce these. In addition to this, did we search the Therapeutic Assessment Institute's list of published studies on TA and CA for titles or abstracts that might not be included in the above search, as were the literature lists of two textbooks on the subject [2,3]. We searched the databases since their inception. We completed the main search for studies on February 22nd, 2021, and the search was updated on May 27th, 2022, and again on February 18th, 2024. See [Figure 1](#) for details.

Study selection

Two authors (ORH and JRG) screened all 2723 abstracts to determine their relevance to the current study. Titles and abstracts irrelevant to the current study were discarded, and the remaining 127 references were retrieved as full-text papers for evaluation of inclusion and exclusion criteria. See [Figure 1](#) for details. Disagreements between the two researchers screening the papers were resolved by discussion. In total, we were able to include two randomized clinical trials in the systematic review. The process was carried out using the Covidence software [36].

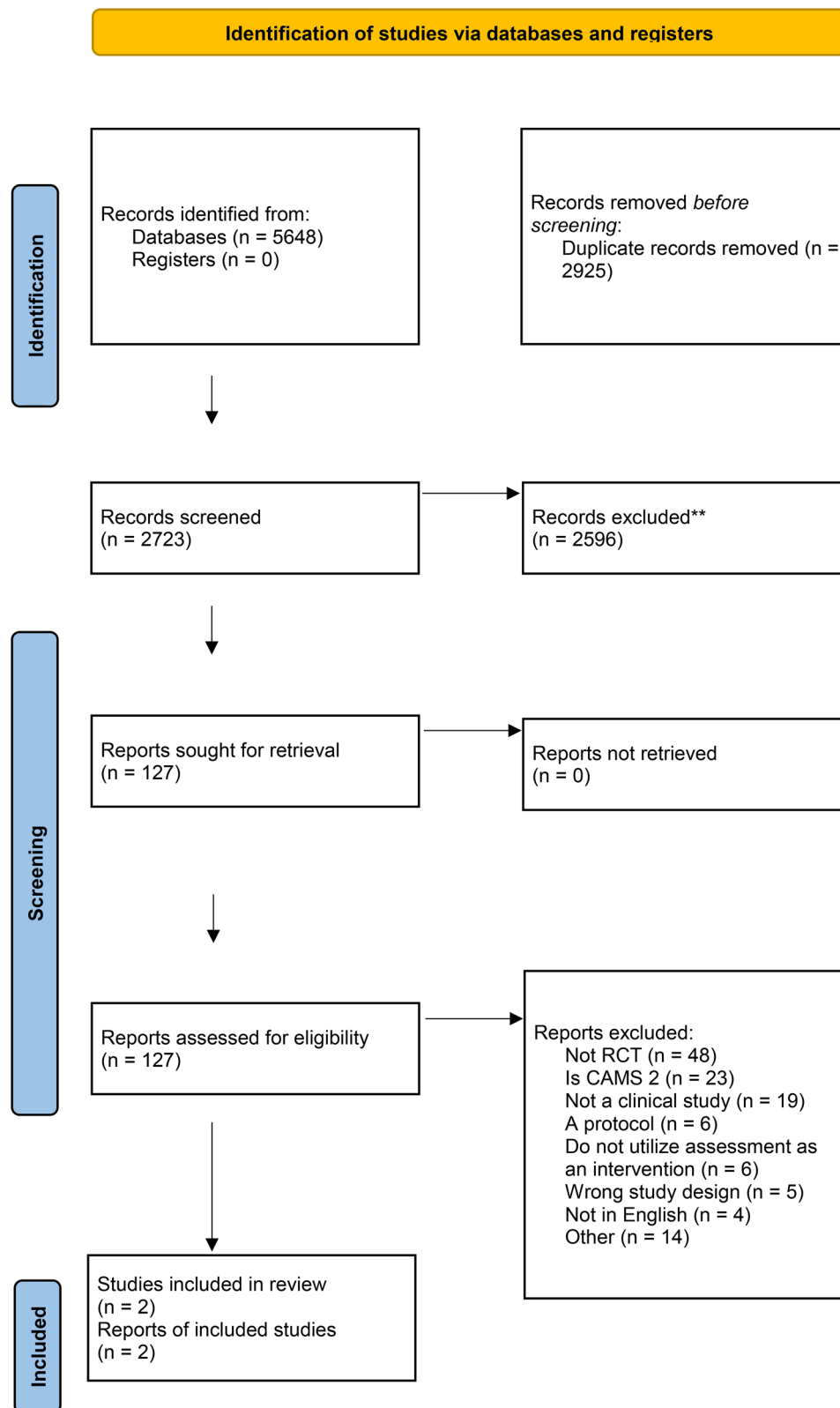


Figure 1. PRISMA 2020 flow diagram for new systematic reviews.

Inclusion and exclusion criteria

Following the published protocol for our systematic review, we checked the retrieved papers against the following inclusion/exclusion criteria: (1) Participants were adults (18–65 years) who (2) received care in a primary care setting or secondary (mental

health service) outpatient setting with (3) a principal diagnosis of any psychiatric illness according to the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders (DSM) III, IV, and 5 [36,37] or the WHO's International Statistical Classification of Diseases (ICD) 9 or 10 [38,39] with or without

comorbid conditions, except for mental retardation which was not allowed as comorbidity. Further, the paper was included if it (4) investigated diagnostic assessment with standardized tests and instruments, which was carried out as a therapeutic intervention (e.g. the therapeutic assessment was not solely done to establish a diagnosis or monitor the effect of the treatment). This could involve personalized feedback in the testing process, collaborative administration of the tests, etc; (5) the trial was published in English in a peer-reviewed journal, (6) had a randomized clinical design, (7) included symptomatology-related outcomes with sound psychometric properties and regular use in clinical and/or research settings.

Data extraction

The authors (ORH and JRG) extracted data from the included trials by reading the paper and extracting the included information into a data-extraction sheet. The template for this is in [supplementary materials](#). They extracted data regarding treatment delivery, intervention details, study design, participant details, and symptomatology-related outcome measures. In case of doubt, ORH and JRG would seek to reach a consensus or could consult NR.

Study quality assessment

We assessed the quality of the included RCTs as described in the protocol [40]. Previous studies have shown that studies judged at a high risk of bias tend to overestimate intervention effects [41]. For this purpose, we utilized the updated Risk of Bias (RoB 2.0) tool of the Cochrane Collaboration [33]. For each included trial, two authors (ORH and JRG) independently evaluated each risk of bias domain as being at low risk, some concern, or high risk. Any disagreements were resolved by discussion. Following this initial rating, each trial had an overall rating according to its highest risk of bias in any of the assessed domains. We did, however, also rate trials with 'some concerns' in multiple domains as having a 'high risk' of bias, as per the RoB 2.0 instructions [33]. In case of doubt, ORH and JRG would seek to reach a consensus or could consult NR.

As recommended by the Cochrane Collaboration [42], we further assessed the certainty of the body of evidence on our two primary outcomes, symptomatology and adverse effects, with the Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach [34]. Within GRADE, randomized clinical trials are initially rated as evidence of high certainty. However, the level of evidence of RCTs is downgraded if the study has methodological flaws. The domains assessed are risk of bias, consistency, imprecision, indirectness, and publication bias. A high GRADE certainty score indicates that further research is unlikely to change our confidence in the estimate of effect; moderate scores indicate that further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate; low certainty scores indicate that further research is highly likely to have an important impact on our confidence in the estimate of effect and is

likely to change the estimate; and very low certainty scores indicate that any estimate of effect is highly uncertain [34].

Deviations from protocol

We published a protocol for the current systematic review in 2022 [40]. In the protocol, we aimed to conduct a systematic review and meta-analysis, including a series of subgroup analyses. However, following our inclusion process, we found the data too limited to conduct a meta-analysis. We also searched the Grey Matters database.

Results

Study inclusion

[Figure 1](#) summarizes the process of inclusion of studies for the current systematic review. We included two trials that met our inclusion criteria [25,26]. Both studies had randomized designs. Neither trial had a long-term follow-up, which we defined as follow-up extending beyond the end-of-treatment assessment. The two studies investigated two different treatment protocols.

Study characteristics

The trial by Morey et al. (2010) was conducted in an outpatient setting in the United States on 16 patients with borderline personality disorder [25]. The sample was predominantly Caucasian females, on average 31.1 years of age ($SD = 8.9$). The patients had various co-morbidity, but mainly depression.

The trial by De Saeger et al. (2014) was conducted in an outpatient setting in the Netherlands on 74 patients with personality disorders (not further specified) [26]. The included patients were a subsample of the 117 patients on the waiting list of the facility. The remaining 43 patients did not start their pretreatments because they were not able to start their assigned psychotherapy. The participants in the subsample were predominantly Caucasian females, averaging 38.8 years ($SD = 10.8$) old. The study did not include data regarding possible co-morbidity.

Treatments

The experimental treatment in the study by Morey et al. (2010) [25] was two sessions of TA. TA was administered individually and was applied as a pretreatment to the standard of care psychotherapy. The TA intervention utilized the SCID-2 as a diagnostic instrument. The standard of care was Manual-Assisted Cognitive Behavior Therapy (MACT) [43], a six-session, manualized therapy that targets deliberate self-harm. The authors did not describe the length of the intervention. A waitlist-type comparator was used, where the included patients were administered the same psychological tests as administered in TA but without the use of the therapeutic questions and feedback used in TA. The authors did not describe the length of the comparator.

The experimental treatment in the study by De Saeger et al. (2014) was four sessions of TA [26], which were administered individually and was applied as a pretreatment to the standard of care psychotherapy (not further specified by the authors, but simply described as 'awaiting an already assigned course of treatment'). The TA intervention utilized the Diagnostic Interview for DSM-IV Personality Disorders (DIPD-IV), the Personality Assessment Inventory (PAI), and the Borderline Features Scale (BOR). The authors did not describe the length of the individual sessions. The comparator was goal-focused pretreatment intervention (GFPTI), a protocol-driven method based on a widely-used model in the Netherlands, the so-called 'five sessions model.' Similar to the TA condition, it included four sessions of unknown length.

Level of symptoms

Morey et al. (2010) found no difference in borderline-symptomatology between groups on the PAI-BOR for completers of the intervention ($n=3$ and $n=4$ in the TA+MACT and MACT conditions, respectively). Although no difference was observed in the total PAI-BOR, there might be a between-group difference on the affective instability subscale of the PAI-BOR ((BOR-A; $F(1, 5)=16.84, p=.01, d=1.78$) and a moderately significant change in self-harm (BOR-S; $F(1, 5)=5.71, p=.07, d=0.48$) favoring the MACT+TA condition. Regarding suicidal ideation, there might be a difference between groups favoring MACT+TA ((PAI-SUI ($F(1, 5)=16.59, p=.01, d=2.01$)).

De Saeger et al. (2014) found that among those who did complete the intervention ($n=37$ and $n=37$ in the TA and GFPTI condition, respectively), no difference in outcome at the end of the TA/GFPTI intervention was observed between treatment groups measured by clinician-rated symptom severity (Global Symptom Index (GSI)) [44] ($F(2, 66)=1.23, p=.30$).

Readiness for treatment

Morey et al. (2010) reported no data on readiness for treatment.

De Saeger et al. (2014) reported a difference regarding treatment outcome expectancy as TA results in higher expectations than GFPTI (Expectancy for Future Treatment Scale ($F(1, 72) 7.69, p=.01, d=0.65$)) as assessed for completers between groups at the end of the TA/GFPTI intervention.

Adverse effects

Neither of the included studies reported any data on adverse effects.

Quality assessment

As illustrated in Table 1, the included randomized clinical trials were both judged with an overall high risk of bias when

assessed with the RoB 2.0 tool [33]. The randomization process was judged with some concerns or a low risk of bias. The deviations from intended interventions were judged with some concerns or a high risk of bias. Both trials were judged with a high risk of bias in missing outcome data. Measurement of the outcome was judged with high a risk of bias or some concerns. The selection of the reported results was judged as a high risk of bias or some concerns.

We assessed the certainty of the evidence using the GRADE approach [34]. We rated the certainty of evidence as 'very low' on all outcomes.

As illustrated in Table 2, the strength of the evidence provided by the included randomized clinical trials was judged to be of low or very low certainty.

Discussion

We aimed to conduct a systematic review and meta-analysis of RCTs examining the effect of applying a pre-described therapeutic approach in the process of assessment in clinical populations comprising patients with psychiatric disorders, applying standardized, systematic review and meta-analysis methodology. We carried out a substantial screening of the literature based on a search strategy that we had tested for sensitivity in advance. We identified two trials, including a total of 90 patients. One trial investigated the effect of adding C/TA to a Manual-assisted Cognitive Therapy intervention compared to Manual-assisted Cognitive Therapy alone, and one trial investigated the effect of C/TA as a stand-alone intervention compared to a Goal-focused Pretreatment Intervention. Both trials were conducted in university-based psychotherapy clinics. Neither of the studies found a significant difference in the effect on any symptomatology-related outcome compared with the control group. Regarding readiness for treatment, one trial found a superior effect of C/TA when analyzing between-group data [26]. Information on possible adverse effects of C/TA was not reported. The included studies were judged to be at a high risk of bias and limited by substantial methodological shortcomings, as judged by the RoB 2 tool, and with low certainty of the evidence.

Our study protocol [40] initially outlined a plan to conduct a systematic review and meta-analysis. However, unlike the meta-analyses conducted by Hanson and Poston (2011), Durosini and Aschieri (2021), and Aschieri et al. (2023), the paucity of studies meeting our inclusion criteria precluded a meta-analysis in our study. While all studies included in these previous meta-analyses [32–34,45] were identified in our search, many were excluded due to the absence of participants with a formal diagnosis or sound randomized controlled designs. Our current review specifically targets TA trials in well-defined clinical populations, in contrast to the broader inclusion criteria of the earlier studies [29,30], which

Table 1. RoB 2.0 assessment.

Study	Randomization process	Deviations from intended interventions	Missing outcome data	Measurement of the outcome	Selection of the reported result	Overall judgment
Morey (2010)	Some concerns	Some concerns	High risk	High risk	High risk	High risk
De Saeger (2014)	Low risk	High risk	High risk	Some concerns	Some concerns	High risk

Table 2. Strength of evidence assessment comparing C/TA to non-C/TA interventions in patients with psychiatric illness.

Outcomes	No. of participants	Quality of the evidence (GRADE)
Symptomatology (primary)		
TA	90 (2 trials)	ψψψ very low ^{a,b}
Readiness for treatment		
TA	74 (1 trial)	ψψψ very low ^{a,b}

^aDowngraded due to risk of bias in more than one domain.

^bDowngraded due to imprecision (few or no trials).

encompassed non-clinical and diagnostically undefined samples and included both randomized and non-randomized designs.

Some trials with clinically well-defined samples included in Hanson and Poston (2011) or Aschieri et al. (2023) could not be included currently as the intervention was not TA but CAMS. We excluded all CAMS trials because we considered it a research field on its own: CAMS rests upon the application of the Suicide Status Form, which is an interview guideline and questionnaire set specifically designed for that intervention. CAMS does not open up for the application of existing psychological or psychopathological assessment instruments, decided in collaboration with the patient, as do other interventions in the C/TA group of interventions. Swift et al. (2021) recently conducted a meta-analysis of CAMS compared to other interventions for suicidality and found support for CAMS [46], while McCutchan et al. (2022) found it to be cost-effective [47]. Hence, we do not generalize our results to CAMS interventions.

Our strict inclusion criteria may have reduced the number of eligible studies. Nonetheless, we find these narrow criteria necessary for an estimate of the clinical evidence base for C/TA in routine clinical settings.

The actual finding of the present systematic review is, therefore, that there is limited research of sufficient quality concerning the application of Therapeutic Assessment for psychiatric patients. This conclusion is in opposition to the prior systematic reviews and meta-analyses, which, as discussed in the introduction, gives the reader the impression that the field of C/TA is highly consolidated. We find this to be misleading. Therefore, in opposition to the prior publications, we find largely no research of high quality. Acknowledging this will help the research field move forward and mature, and produce high-quality trials which can inform of the effectiveness of C/TA interventions in psychiatric populations.

The studies included in Hanson and Poston (2011), Durosini and Aschieri (2021), and Aschieri et al. (2023) suggest that there may be substantial heterogeneity among effect studies on C/TA regarding populations, interventions, and outcomes used in the studies. The latter issue relates to both clinical outcomes and process outcomes. We, therefore, advise that trialists agree on which outcomes are most useful in the study of C/TA interventions to increase homogeneity in future meta-analyses.

Our systematic review only found trials on personality disorders. We advise that future studies also examine C/TA-style interventions in the treatment of common psychiatric illnesses such as anxiety disorders and depression.

We notice a lack of trials that investigate C/TA-style interventions as enhancers of basic diagnostic assessment in itself. From our perspective, this could be a promising application for TA-style interventions since it could be administered instead of a regular diagnostic assessment at intake in outpatient mental health clinics. Using C/TA in this context would allow for active psychological treatment upon the patient's entry to the psychiatric clinic, and the collaborative approach in the assessment process is in line with the increasing emphasis on user involvement and shared decision-making in the mental health service [45]. We hypothesize that such an intervention *via* the therapeutic question utilized in TA could allow the diagnostic assessment to evolve around what, in the patient's perspective, is the key issue that has made them seek treatment. That is, what is the most troubling issue from the patient's perspective? In addition to this, it could serve as a psychoeducative intervention where clinician and patient have a thorough discussion of the assessment outcome and potentially a discussion of when precisely the patient will feel adequately treated. Such an intervention providing detailed information beyond diagnosis might also be highly relevant for the clinician in subsequent psychotherapy or pharmacological decision-making.

In the two included trials, C/TA-style interventions were not utilized as a part of the diagnostic process in the clinics but solely as a treatment. Thus, it was not a specific outcome whether some of the patients as a part of the TA were found to be better described by another psychiatric diagnosis than the one that led to their inclusion in the trials. We find this a missed opportunity and suggest that future research into C/TA includes the percentage of patients whose initial diagnosis was changed following C/TA as a specific outcome. This could help investigate whether C/TA-style interventions are superior to regular, non-patient guided diagnostic assessments due to their unique personalized focus or whether the possible added benefit of C/TA only relates to treatment outcomes.

Based on the current empirical evidence, it remains unclear whether C/TA interventions other than CAMS are effective in clinical outpatient populations. Thus, their effectiveness remains to be tested in large-scale trials with sound research methodology. We advise that more trials on C/TA should be conducted. Specifically, the results of this systematic review yield no evidence for the efficacy of C/TA over other pre-therapy interventions for any psychiatric disorder. Few RCTs on C/TA are available, and those that exist have a high risk of bias and have small samples. Large-scale RCTs are required to investigate the effect of C/TA on patients with psychiatric disorders.

Authors' contributions

SA, ORH, NR, OJS, and JRG conceptualized the systematic review. ORH & JRG carried out literature screening and full-text reading and tabulation. ORH was responsible for writing the first draft of the manuscript. All authors have discussed, reviewed, and approved the manuscript.

Disclosure statement

The authors declare there are no competing interests in this review. The authors have no allegiance to TA or CA groups.

Funding

This systematic review was funded by Region Zealand Mental Health Service and Trygfonden (ID: 152461)

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