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Cardiac perception and self-reported interoception in common mental health disorders: A systematic review and meta-analysis

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ABSTRACT

Atypical interoception may represent a transdiagnostic marker for mental health difficulties. Reviews to date exploring the relationships between interoception and mental health have presented mixed findings, with a narrow focus on cardiac measures of interoception. We conducted a systematic review and meta-analysis of studies that measured the association of cardiac perception and self-reported interoception with common mental health disorders, including anxiety, depression, schizophrenia, bipolar disorder, and obsessive-compulsive disorder, in clinical and non-clinical samples of adults. A total of 206 effect sizes from 57 articles (total $N = 6652$) were eligible for inclusion in random effects meta-analyses. Overall, pooled associations showed no significant association of mental health difficulties with performance on the Heartbeat Counting Task ($r = -.042$), but a significant, negative association with Heart Rate Discrimination Task performance ($r = -.137$). Problems in adaptive interoceptive attention ($r = -.183$) but not sensing ($r = .136$), or interpretation ($r = -.117$), were associated with worse mental health symptomology, as was greater maladaptive interoceptive attention ($r = .381$). Moderator analyses showed that the size of the effect often varied between different mental health disorders, and there was some evidence of overly influential effect sizes as well as wide 95% prediction intervals. Our findings highlight the importance of methodological choices and potential differences between different performance-based tasks of cardiac interoception and different self-reported interoceptive dimensions. Our results show some support for interoception as a transdiagnostic marker for mental health difficulties that can support the development of evidence-based interventions.

Mental ill health represents the world's leading cause of disability, contributing significantly to reduced quality of life, economic burden, and risk for premature mortality (Arias et al., 2022; Friedrich, 2017). Mental distress has traditionally been categorized using formal taxonomic systems (Kendler, 2009), with long-standing taxonomies including the Diagnostic and Statistical Manual of Mental Disorders Fifth Edition (DSM-5; American Psychiatric Association, 2013)) and the International Classification of Diseases 11th Revision (ICD-11; World Health Organisation, 2021). Despite the success of these systems, high levels of comorbidity, poor discrimination between different disorders, and heterogeneity within disorders, highlight the need for novel and more integrated approaches to mental ill health (Dalgleish et al., 2020; Insel, 2014; Kotov et al., 2017). To overcome these limitations, an alternative "transdiagnostic" approach has been proposed to identify shared mechanisms that cut across traditional diagnostic boundaries (Dalgleish et al., 2020; Fusar-Poli et al., 2019). The concept of interoception, defined as the process by which the nervous system senses, interprets, and integrates signals originating from within the body, across both conscious and unconscious levels of processing (Crucianelli and Ehrsson, 2023; Jenkinson et al., 2024; Khalsa et al., 2018), has been identified as a potential transdiagnostic process that cuts across a variety of common mental health conditions (Khalsa et al., 2018).

Interoceptive signals include, but are not limited to, heart rate, temperature, and hunger, while key visceral systems for interoception include the respiratory, cardiovascular, and gastrointestinal systems (Heim et al., 2023). Problems sensing, interpreting, and integrating these different interoceptive signals may be associated with heightened distress and poor mental health. This idea developed during the early 2000's (Critchley et al., 2004) and has gained increased traction since the publication of a pioneering whitepaper outlining the roadmap between interoception and poor mental health (Khalsa et al., 2018).

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Traditionally, the role of interoception in mental health has been considered in the context of specific mental disorders, but more recently, it has been suggested that interoception might instead represent a shared risk factor or 'transdiagnostic' mechanism conferring a common vulnerability for mental health difficulties, crossing diagnostic boundaries and symptom categories (Brewer et al., 2021; Nord and Garfinkel, 2022).

The transdiagnostic nature of interoception might reflect potential links that have been identified between interoception and a range of cognitive and affective abilities, including emotion processing and affective experience (Brewer et al., 2021; Critchley and Garfinkel, 2017; Tsakiris and Critchley, 2016; Zamariola et al., 2019), learning and decision making (Brewer et al., 2021), self-awareness (Candia-Rivera et al., 2024; Salvato et al., 2020), and self-representation (Seth, 2013; Tsakiris and Critchley, 2016). However, more work is needed to understand the physiological mechanisms that underlie many of these associations. Several articles have reviewed the associations of interoception with individual diagnostic or symptom categories, including anxiety (Paulus and Stein, 2010), depression (Eggart et al., 2019), psychosis (Yao and Thakkar, 2022), bipolar disorder (Perry et al., 2019), and obsessive-compulsive disorder (OCD; (Bragdon et al., 2021), but have yielded mixed results. Indeed, heightened, lowered, and typical levels of interoceptive abilities have sometimes been reported in relation to the same symptom category or diagnostic construct.

These differences may, in part, be explained by methodological inconsistencies, including differences in measurement format, task parameters, and psychometric properties. As an example, cardiac interoception is commonly assessed using heartbeat counting (tracking) and discrimination paradigms, which differ substantially in task demands, parameters, and susceptibility to response biases (Brener and Ring, 2016; Hickman et al., 2020; Murphy et al., 2018). Other issues contributing to inconsistencies in the literature include variation in sample characteristics and clinical heterogeneity, and problems defining and measuring interoception, with definitions continuing to evolve and often the subject of controversy (Crucianelli and Ehrsson, 2023).

Inconsistent results might also be explained by the complex and multifaceted nature of interoceptive processing, which spans peripheral physiological processes and higher-order processing of interoceptive cues (Desmedt et al., 2025). The multidimensional model of interoception, described by Garfinkel and colleagues (2015), represents one of the most widely accepted frameworks, categorizing interoception along three core dimensions: 1) interoceptive accuracy, referring to the ability to accurately detect the physical state of the inner body, typically measured using behavioural tasks (e.g., the heartbeat counting task); 2) interoceptive sensibility, referring to the subjective belief or self-reported tendency to be aware of one's own internal bodily processes, usually assessed via questionnaires; and 3) interoceptive awareness, referring to correspondence between objectively measured interoceptive accuracy using a specific test, and confidence judgements relating to performance on that test (Garfinkel et al., 2015).

Despite its application in interoception research, the multidimensional model has nonetheless been subject to some criticism. One important criticism relates to the classification of different cardiac perception tasks as measures of the same interoceptive dimension. This has been highlighted as problematic given the low convergence between performance-based measures, both within and between bodily systems. For example, a meta-analysis showed that performance on the heartbeat counting task (Schandry, 1981) and the heart rate discrimination task (Whitehead et al., 1977), which are both considered measures of cardiac interoceptive accuracy, shared only 4.4% of the variance (Hickman et al., 2020). These differences may be due, in part, to conceptual error or measurement error, and call into question the assumption that these two tasks measure the same construct (Desmedt et al., 2025). Similar issues have also been identified for the sensibility dimension of the multidimensional model, where different self-report measures of interoception, and indeed different subscales of the same measure, cover a variety of phenomena and again show low convergence (Desmedt et al., 2022). These conceptual issues pose a threat to the interpretation of current empirical results and are at risk of leading to replication failures.

Given these criticisms, an alternative conceptual model has been suggested by Desmedt et al. (2025). In this framework, interoception is divided into four broad categories or factors): interoceptive attention, referring to attentional processes directed toward internal signals; interoceptive sensing, referring to detection of internal signals by the nervous system at both conscious and nonconscious levels; interoceptive interpretation, encompassing the interpretation, beliefs, attitudes, and categorization of internal signals; and interoceptive memory, encompassing memory processes related to internal signals. Because of limited convergence across different measurement formats (e.g., questionnaires, behavioural tasks, and neuroimaging techniques) and bodily domains (e.g., cardiac, respiratory, and gastrointestinal), Desmedt et al. (2025) recommend that both the measurement format and bodily domain should be explicitly specified. This framework has several advantages, most notably reducing the risk of overgeneralization based on broad and heterogeneous dimensions, and greater inclusivity of both conscious and nonconscious processes.

Although several articles have reviewed the associations of interoception with individual diagnostic conditions or symptom categories (Adams et al., 2022; Desmedt et al., 2022; Eggart et al., 2019; Paulus and Stein, 2010; Yao and Thakkar, 2022), none have systematically reviewed all the available evidence across a range of common mental disorders and interoceptive measurement types and dimensions. Two reviews included a quantitative synthesis of the relationship between interoception and anxiety (Adams et al., 2022; Desmedt et al., 2022), with neither review finding consistent evidence to support a relationship, and both reviews focussing on cardiac interoceptive accuracy at the expense of other interoceptive dimensions. In another review, this time of systematic reviews, meta-analyses, and relevant theory papers, a particular focus on cardiac interoceptive accuracy was again highlighted, with no convincing evidence for a relationship of interoceptive accuracy with mental ill health (Jenkinson et al., 2024). The review also highlighted the absence of a quantitative synthesis of the relationship between interoceptive dimensions and psychosis (Jenkinson et al., 2024).

In this article, we aimed to conduct a systematic review and meta-analysis of the association of cardiac performance-based tasks and self-report questionnaires of interoception with common mental disorders, including anxiety (e.g., generalized anxiety disorder), OCD, mood disorders (e.g., depression, bipolar disorder), and schizophrenia, which provide a broad and substantial contribution to population-level mental health burden based on large-scale psychiatric epidemiological studies (Morris et al., 2025; Steel et al., 2014; Vos et al., 2020), and share common transdiagnostic features relevant to interoception (Jenkinson et al., 2024). We aimed to examine the size and consistency of the relationship across tasks of cardiac perception and self-reported interoception and different interoceptive dimensions, in clinical and non-clinical populations. A better understanding of these relationships will enhance understanding of the development and maintenance of common mental health disorders and aid the design of targeted therapeutic interventions through a transdiagnostic lens.

1. Methods

1.1. Search strategy

The review protocol and analysis strategy was preregistered on PROSPERO on 18 December 2023 (CRD42023494042). The review process was guided by the Preferred Reporting Items for Systematic Reviews and Meta-analyses (PRISMA; (Moher et al., 2009)). Studies were identified by systematically searching four electronic databases:

Medline, PsycINFO, Scopus, and Web of Science Core Collection. There were no restrictions on date. Searches were conducted in December 2023 and updated on 15 July 2024. The search strategy used a combination of keyword search terms associated with interoception (Interocept* OR heartbeat detection OR heartbeat counting OR heartbeat discrimination OR heartbeat awareness OR body awareness OR bodily awareness) and common mental disorders (anxiety OR anxious OR panic OR depress* OR psychosis OR schizophrenia OR bipolar OR obsessive compulsive). To ensure a comprehensive search strategy, the British Library e-theses online service (EThOS) and Proquest were screened for relevant doctoral theses in the grey literature. The decision to include unpublished theses was taken to reduce publication bias, with null findings often less likely to be published, and to enhance the comprehensiveness of the evidence base and yield more accurate estimates of the true size of the effect (McAuley et al., 2000; Paez, 2017). Google Scholar was also searched for any unidentified records, and the reference lists of all eligible papers were hand searched for additional relevant articles.

1.2. Study eligibility criteria

To be eligible for inclusion, studies had to be written in English or have full-text availability in English, had to be published in a peer-reviewed journal or a doctoral/master's thesis, and had to include analyses of quantitative data on the relationship of cardiac perception or self-reported interoception with common mental disorders (depression, anxiety, schizophrenia, bipolar disorder and/or OCD). We limited our review to cardiac performance-based tasks, for example, the Heartbeat Counting Task (Schandry, 1981), and validated self-report questionnaires of interoception, given the more limited evidence base for performance-based tasks in other domains e.g., gastric. To be considered as valid, there should be published literature to highlight the general validity of the approach or measure (e.g., psychometric properties).

Common mental health disorders could be assessed using either a formal diagnosis, structured diagnostic interview, or self-report measure. Intervention studies, including randomised controlled trials (RCTs) were included if they reported baseline data on the relationship between interoception and common mental disorders. Eligible studies had to include adult participants aged 18 years and over with a mean age under 65 years, with participants recruited from either clinical or non-clinical populations. To narrow the scope of the review and avoid findings that were heavily influenced by the presence of comorbid conditions, studies that specifically recruited participants with a not-included disorder e.g., a physical health condition, an eating disorder, or a neurodevelopmental disorder, or samples with intellectual/learning disability, were excluded. The outcome measure of interest was the association of interoception with anxiety, depression, schizophrenia, bipolar disorder, and/or OCD, including correlational effect sizes and group comparisons (e.g., comparing high versus low mental health symptom groups, or groups with and without a diagnosis of a relevant mental health disorder).

1.3. Study selection

All records were exported into Endnote for the removal of duplicates and then exported into Rayyan for screening and selection for inclusion. The initial screening of titles and abstracts was performed by one reviewer (the first author). Clearly ineligible studies were excluded. To establish reliability, a proportion (10%) of records at title and abstract phase were also independently screened for inclusion by a second reviewer. The lead author ensured that the second rater was familiar with the inclusion and exclusion criteria and how these should be employed. The first author also examined all articles that progressed to full text review, and a second reviewer again independently screened a proportion (10%) of full texts chosen at random to establish reliability. Any discrepancies or disagreements were resolved by discussion between the two reviewers to achieve a consensus.

1.4. Data extraction

The first author reviewed all full text articles and extracted data for: 1) Publication: author, title, year, document type (journal article, thesis); 2) Design: type of study (correlational, between-group comparison), type of sample (clinical, non-clinical); 3) Participant characteristics: age, gender, ethnicity, education level; 4) Outcome measures: sample size, measurement of mental health condition/diagnosis, type of interoception task, and data used to calculate relevant effect sizes (e.g., Pearson's *r*, mean values and SD/Standardized Mean Difference). Based on the included studies, no other statistical parameters required extraction and conversion to *r*. Although some studies reported beta values, these were based on regression models with multiple predictors and were therefore not suitable for extraction. All relevant effect sizes of interest were extracted for inclusion in meta-analytic models. Where effect sizes were reported for measures with various subscales, the extraction of effect sizes based on subscale scores was prioritised over total scores. Given that subscales from the Multidimensional Assessment of Interoceptive Awareness (MAIA; (Mehling et al., 2012; Mehling et al., 2018)) assess interoception across different domains and do not converge on a single construct (Desmedt et al., 2025), total scores on the MAIA were not included in meta-analyses. Studies that reported MAIA total scores in the absence of subscale scores were excluded. In a deviation from the protocol registered on PROSPERO, data extraction for a random sample of 30% of the included studies, and not all included studies, was checked for accuracy by a second reviewer. Any discrepancies were resolved within the research team.

Effect sizes were coded for analysis based on type of mental health condition, interoception measurement format objective versus self-report), and interoception dimension, consistent with the framework of Desmedt et al. (2025). For performance-based tasks of cardiac perception, meta-analyses were conducted separately for different tasks (e.g., accuracy on the Heartbeat Counting Task and the Heart Rate Discrimination Task), consistent with work showing that different performance-based tasks do not converge (Desmedt et al., 2025; Hickman et al., 2020) and the need to avoid overgeneralization.

Effect sizes based on self-report measures were categorized according to the framework developed by Desmedt et al. (2025), as either interoceptive attention, sensing, interpretation, or memory. Further, earlier work has distinguished between adaptive versus maladaptive interoceptive processes (Mehling, 2016), with Trevisan et al. (2023) and Van Bael et al. (2024) quantifying these distinctions and highlighting relationships with clinical outcomes including somatisation and alexithymia. As such, and consistent with others (Treves et al., 2025), in our analyses we further categorized interoceptive processes as either adaptive or maladaptive (see Table S1, Supplemental Material 1).

1.5. Quality assessment and risk of bias

The Appraisal Tool for Cross-Sectional Studies (AXIS tool; (Downes et al., 2016)) was used to assess the methodological quality and risk of bias of all included studies. The AXIS tool comprises 20 questions to guide the rater in critically appraising all aspects of studies, reporting across five areas (introduction, methods, results, discussion, and other). The questions invited categorical responses ('yes', 'no', 'do not know') and the methodological quality was determined by an overall consideration of strengths and weaknesses of the selected studies. Although not originally noted in the registered protocol, a second reviewer quality assessed 10% of papers. Uncertainty in appraisal decisions were discussed and resolved within the research team.

1.6. Data reduction and analysis

Multilevel random effects meta-analyses were conducted using the

'rma.mv' function from the metafor package in *R* (Viechtbauer, 2010). Multilevel meta-analyses were favoured as there were often multiple effect sizes (e.g., for more than one subscale or measure) from the same study, and a multilevel model helps to overcome issues of non-independence of these effect sizes (Van Den Noortgate and Onghena, 2003). For each meta-analysis, we performed a Loglikelihood Ratio test and selected the multilevel model where the portioning of variance within studies and between studies was a significantly better fit for the data than a single level model ($p < .05$). The most commonly reported effect size was the correlation coefficient *r*. To obtain a common effect size for inclusion in pooled analyses, SMDs were converted to correlation coefficients using the formula outlined in Polanin and Snilstveit (Polanin and Snilstveit, 2016). The reporting of main effects is aided by common language effect size statistics to provide practical interpretations (Mastrich and Hernandez, 2021), as well as prediction intervals (IntHout et al., 2016). We also planned to undertake moderation analyses on mental health condition (anxiety *vs.* depression *vs.* schizophrenia *vs.* bipolar disorder *vs.* OCD). Because small numbers of effect sizes can impact the power of the meta-analysis, to mitigate this we only examined subgroups including a minimum of five effect sizes (Jackson and Turner, 2017). Across all effect sizes, only three effect sizes for bipolar disorder were extracted for inclusion; these were included where relevant in pooled models but were excluded from any tests of moderation.

The I^2 index (Higgins and Thompson, 2002), was used as a measure of heterogeneity to provide an estimate of the proportion of variability in the meta-analysis that is explained by differences between the included studies rather than by sampling error (Thorlund et al., 2012). Values of I^2 are expressed as a percentage, with values greater than 50% indicative of moderate heterogeneity, and values greater than 75% indicative of substantial heterogeneity. We present funnel plots for each pooled model to assess the distribution of studies and potential asymmetry associated with publication bias and other biases (Mavridis and Salanti, 2014). The metafor package does not support Egger's test for multilevel meta-analyses so we report these on single level models, where the effect sizes within each study are pooled using the 'aggregate' function. All data and analysis code are available on the Open Science Framework at https://osf.io/r2957/.

1.7. Deviations from protocol

Changes were made to the originally pre-registered protocol and were intended to improve the internal consistency of the eligibility criteria and their alignment with the scope and objectives of the review. First, and consistent with the pre-registered search strategy, the inclusion and exclusion criteria were revised to include only cardiac performance-based tasks and self-report measures of interoception, with objective measures in other domains e.g., gastric) excluded. The pre-registered protocol also referred to coding of effect sizes based on e.g., interoceptive accuracy, and interoceptive sensibility. Instead, each effect size was coded based on the framework outlined by Desmedt et al. (2025), to avoid overgeneralizations between separable cardiac performance-based tasks and to discriminate between distinct self-reported interoceptive dimensions.

2. Results

2.1. Studies identified for inclusion

A total of 8393 records were identified through database searches. Following the removal of 3539 duplicates, 4854 individual titles and abstracts were screened. A random selection of all records (10%; $n = 485$) were independently assessed by a second reviewer. The rate of agreement was calculated as 99.6% (two discrepancies), highlighting excellent internal agreement (Cohen's Kappa = 0.96). One hundred and fifty-nine articles progressed to full text screening, with 54 papers selected for inclusion in the review. A random selection of all full text records (~10%; $n = 7$) were also independently assessed by a second reviewer. The rate of agreement was calculated as 100% (zero discrepancies), highlighting excellent internal agreement (Cohen's Kappa = 1). A further three papers were identified via Google Scholar searches, cited articles and reference list checks, resulting in a total of 57 articles that were eligible for inclusion, yielding 206 effect sizes from a total sample size of $N = 6652$. The screening and selection process is outlined in Fig. 1, using the PRISMA 2020 flowchart (Moher et al., 2009; Page et al., 2021).

2.2. Study and sample characteristics

Participant demographics and study characteristics for all included studies are displayed in Table 1. One study was a thesis, with all remaining studies published in peer-reviewed journals between 1991 and 2024. Fourteen studies were conducted in the USA, 15 in Germany, seven in China, five in the UK, four in Italy, two each in Japan and the Netherlands, and one each in Australia, Canada, Cyprus, Hungary, Israel, Korea, Serbia, and Switzerland. Twenty-four studies used a group-based design, 22 studies employed a correlational design, and the remaining 11 studies used both correlational and group-based designs. Over half of the studies ($n = 30$) recruited both clinical and non-clinical samples, 21 studies included only a non-clinical sample, and six studies included only a clinical sample. Most of the non-clinical samples were recruited from university student populations, with other samples recruited from the community, online research platforms, and advertisements in newspapers and flyers. Clinical samples were recruited from inpatient facilities and primary and secondary community mental health teams. Healthy participants in studies that included a comparison group were mostly recruited from local communities, universities, and through online advertising. Forty-five studies included effect sizes for anxiety, including generalised anxiety disorder, panic, health anxiety, social anxiety, and OCD, 27 studies included effect sizes for mood disorders, including depression and bipolar disorder, and six included effect sizes for schizophrenia. Total sample sizes ranged from $N = 18$–549.

2.3. Assessment of mental health condition

Most studies recruiting clinical populations used various versions of the DSM (DSM-III; DSM-IV; DSM-5) or the ICD (ICD-9; ICD-10), to determine a diagnosis. Diagnoses were either recorded on participants' clinical records or identified using structured clinical interviews conducted by trained clinicians. Studies that recruited non-clinical populations used self-report measures to assess mental health symptomology, some of which were also used to complement diagnosis and gain baseline measures of symptom severity in studies that included clinical samples. The most frequently used self-report instruments were the State Trait Anxiety Inventory (STAI; (Spielberger et al., 1971)) for the measurement of anxious symptoms, the Beck Depression Inventory (BDI; (Beck et al., 1996)) for the measurement of depressive symptoms, and the Positive and Negative Syndrome Scale (PANSS; (Kay et al., 1987)), to measure symptoms of psychosis.

2.4. Measurement of interoception

A variety of measures were used to assess interoception and its various dimensions. Tasks used to measure cardiac perception largely fall into one of two main task types outlined by Brewer et al. (2021) and Murphy (2024): 1) those that use counting procedures to measure the perception of heartbeat sensations (e.g., (Dale and Anderson, 1978; Schandry, 1981)); and 2) those that assess multisensory integration of heartbeat sensations and external cues, commonly referred to as heartbeat detection or heartbeat discrimination procedures (Brener et al., 1993; Whitehead et al., 1977). For a description of these tasks and a critique of their validity, see Brewer et al. (2021) and Murphy (2024).

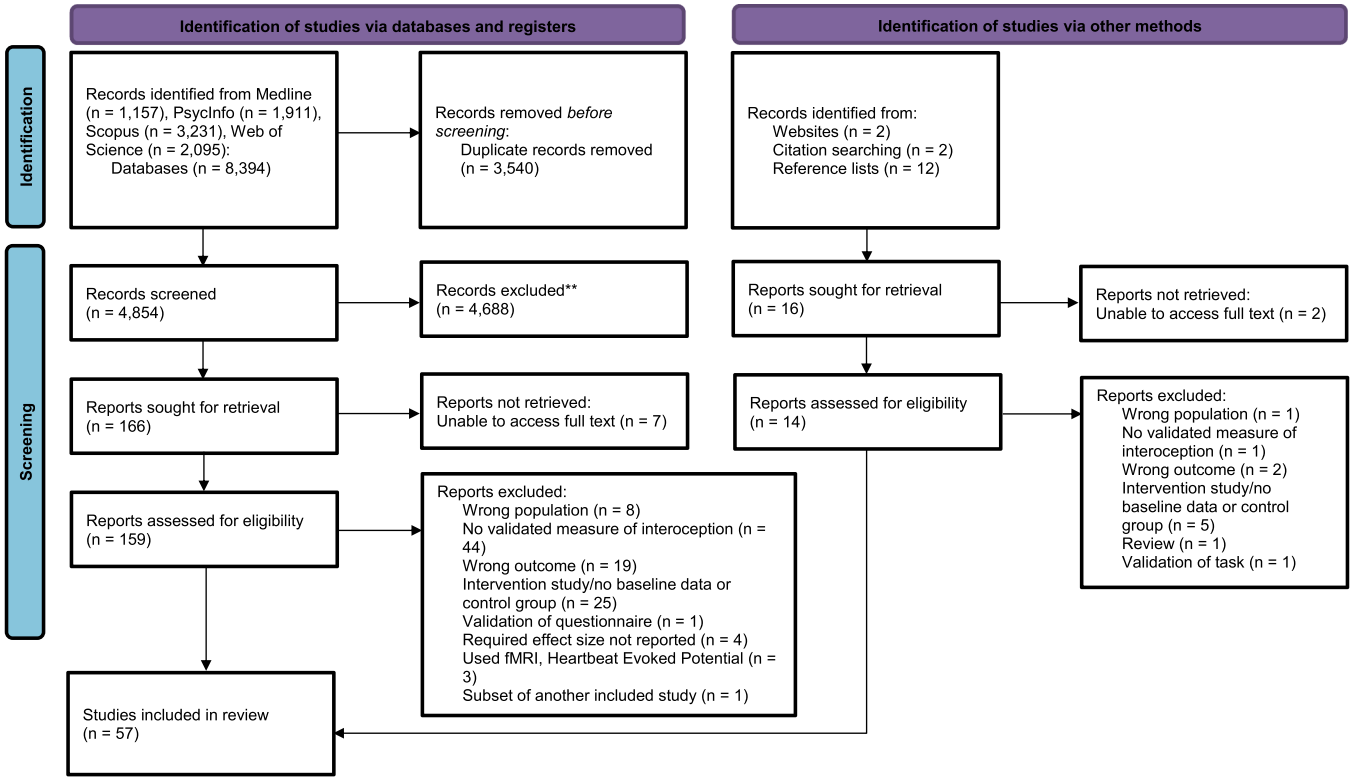


Fig. 1. PRISMA Flowchart detailing the Study Selection Process.

Self-report questionnaires used to measure interoception included the Multidimensional Assessment of Interoceptive Awareness (MAIA; Mehling et al., 2012; Mehling et al., 2018), the Body Perception Questionnaire (BPQ; Cabrera et al., 2018), the Self-Awareness Questionnaire (SAQ; Longarzo et al., 2015), the Interoceptive Accuracy Scale (IAS; Murphy et al., 2020) and the Interoception Sensory Questionnaire (ISQ; Fiene et al., 2018). The reporting of effect sizes based on self-report measures of interoception varied across studies and measures. For example, effect sizes were sometimes reported for some but not all subscales of the MAIA. Only effect sizes based on individual subscale scores of the MAIA were extracted for inclusion to facilitate pooled analyses consistent with the framework of Desmedt et al. (2025).

2.5. Quality assessment of selected studies

The results of the quality assessment of all papers included in the review are presented in Table S2 in Supplemental Material 1. All studies clearly reported their aims and objectives, had an appropriate study design, clearly defined their population of choice, used validated assessment measures, selected an appropriate sampling frame, and included variables appropriate to the study aims. Most studies (48/57) had a replicable methods section, with the remaining nine studies partially meeting this criterion. All studies accurately reported descriptive statistics, reported results from all analyses described in the methods, and reported conclusions that were justified based on the results.

Notably, only 11 studies included a power analysis, and some studies reported sample sizes as small as $N = 18$. Thirty-eight studies declared a conflict of interest and/or funding, 9 studies did not include a conflict-of-interest statement, and 10 studies reported no conflict of interest or funding being awarded. Over half of all studies (41/57) reported having ethical approval. Four studies were of particularly high quality and displayed little risk of bias (Benau, 2023; Eggart and Valdés-Stauber, 2021; Krautwurst et al., 2014; Leonidou et al., 2020).

Most studies highlighted limitations, two studies (De Pascalis et al., 1991; Demartini et al., 2021) failed to detail any methodological concerns that may have impacted the results, and two studies only partially discussed limitations of their research (Ehlers et al., 1995; Mussgay et al., 1999). The findings reported by Antony et al. (1995) should be interpreted with a greater level of caution; the categorisation of non-responders was not discussed, clear statistical significance was not specified, internal consistency of results and methodological limitations were only partially detailed, and ethical approval, informed consent, and sources of funding or conflicts of interest (if any) were unreported.

3. Meta-analyses of objective tests

3.1. Heartbeat counting task

The multilevel meta-analysis was a better fit of the data ($X^2(1) = 16.31$, $p < .001$). There was a non-significant pooled association of accuracy on the Heartbeat Counting Task with symptoms of common mental health disorders ($r = -.042$ [95% CI: −.121–.036], $p = .291$, $I^2 = 76\%$), based on 76 effect sizes from 39 studies. The common language interpretation of this effect is that if two individuals are randomly selected, a person with a worse interoception score will also have higher mental health symptomology 51.3% of the time. The 95% prediction intervals were −0.463–.394, suggesting that the true effect in a future study could plausibly range from $r = -.463$ to $r = .394$. The wide prediction intervals are reflective of substantial heterogeneity.

Removal of the smallest ($r = -.038$ [95% CI: −.449–.386]) and the largest ($r = -.045$ [95% CI: −.123–.033]) effect sizes did not substantially influence the significance of the pooled effect. The funnel plot for study-level data is shown in Fig. 2. Egger's test was not significant ($Z = 1.33$, $p = .182$).

There was a significant moderating effect of mental health disorder on heartbeat counting accuracy ($X^2(2) = 39.00$, $p < .001$). The pooled association for both depression (26 effect sizes from 20 studies) and schizophrenia (8 effect sizes from 4 studies) showed that heightened depressive ($r = -.115$ [95% CI: −.184 to −.045], $p = .001$, $I^2 = 45.5\%$),

Table 1
Study characteristics.

Author (year), location	Format of publication	Study design, sample	Sample size (*N*)	Gender (%), *M*age, *SD* (years)	Ethnicity (%)	Mental health condition (measures)	Interoception measure
Abrams et al. (Abrams et al., 2018), USA	Journal article	Correlational, non-clinical	Total *N* = 61	Female (57), *M*age = 22, *SD* = 3.73	White (67.2), Asian (16.4), Black (8.2), Hispanic (4.9), Other (3.3)	Anxiety (ASI-3, STAI-T)	Heartbeat Counting Task
Antony et al. (Antony et al., 1995), USA	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 60, PD (*N* = 20), social phobia (*N* = 20), healthy controls (*N* = 20)	Female (60), *M*age PD = 29.95, *SD* = 5, *M*age social phobia = 30.75, *SD* = 5.83, *M*age healthy controls = 28.35, *SD* = 5.48	N/S	Social phobia (ADIS-R)	Heartbeat Counting Task
Ardizzi et al. (Ardizzi et al., 2016), Italy	Journal article	Group-based, separate clinical and non-clinical comparisons; correlational, clinical	Total *N* = 46, schizophrenia (*N* = 23), healthy controls (*N* = 23)	Male (80), *M*age schizophrenia = 33.78, *SD* = 6.33, male healthy controls (87), *M*age = 31.91, *SD* = 9.18	N/S	Schizophrenia (SCID-I/ SCID-II, PANSS); anxiety (STAI-T)	Heartbeat Counting Task
Benau (Benau, 2023), USA	Journal article	Correlational, non-clinical	Total *N* = 98	Male (71), *M*age = 36.21, *SD* = 10.2	White (78), Other (22)	Depression, anxiety (DASS-21)	IAS
Blickle et al. (Blickle et al., 2024), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons; correlational, clinical	Total *N* = 60, depression (*N* = 30), healthy controls (*N* = 30)	Female depression (70), *M*age = 62.50, *SD* = 7.69, female healthy controls (50), *M*age = 62.6, *SD* = 6.05	N/S	Depression (SCID-I, BDI, HAMD); anxiety (STAI-T)	Heartbeat Counting Task
Burrows et al. (Burrows et al., 2022), USA	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 136, major depressive disorder (*N* = 48), healthy controls (*N* = 41)	Female MDD (69), *M*age = 32.31, *SD* = 11.28, female healthy controls (56), *M*age = 31.49, *SD* = 10.54	N/S	Depression (SCID-I, MINI, PHQ-9)	MAIA (NO, ND, NW, EA, BL, T, AR, SR)
Critchley et al. (Critchley et al., 2023), UK	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 374, depression (*N* = 59), anxiety (*N* = 29), bipolar (*N* = 56), schizophrenia (*N* = 19), healthy controls (*N* = 67)	Female depression (58), *M*age = 38.4, *SD* = 15.3, female anxiety (66), *M*age = 35.1, *SD* = 16.1, female bipolar (57), *M*age = 42.8, *SD* = 12.7, male schizophrenia (67), *M*age = 40.9, *SD* = 13.7	N/S	Depression, anxiety, bipolar disorder, schizophrenia (recorded psychiatric diagnosis, BDI, STAI)	Heartbeat Counting Task, Heartbeat Discrimination Task, BPQ
Cui et al. (Cui et al., 2016), China	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 61, GAD (*N* = 21), PD (*N* = 18), healthy controls (*N* = 22)	Male anxiety (65), *M*age = 39.95, *SD* = 12.24, male panic (67), *M*age = 37.17, *SD* = 11.31, male healthy controls (14 out of 22), *M*age = 38.05, *SD* = 10.32	N/S	Anxiety (SCID-I, MINI, HAMA, STAI)	BPQ
Cui et al. (Cui et al., 2020), China	Journal article	Group-based, separate clinical and non-clinical comparisons; correlational, clinical	Total N = 62, GAD (*N* = 32), healthy controls (*N* = 30)	Male GAD (66), *M*age = 33.1, *SD* = 8.3, male healthy controls (57), *M*age = 31, *SD* = 6.4	N/S	Anxiety (SCID-I, MINI, HAMA, GAD7)	BPQ
Demartini et al. (Demartini et al., 2021), Italy	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 36, OCD (*N* = 18), healthy controls (*N* = 18)	Female OCD (56), Mage = 40.72, SD = 15.78, female healthy controls (50), Mage = 44.28, SD = 16.81	N/S	OCD (DSM-V criteria)	Heartbeat Counting Task
De Pascalis et al. (De Pascalis et al., 1991), Italy	Journal article	Correlational, non-clinical	Total *N* = 78	N/S	N/S	Anxiety (STAI)	Heart Rate Discrimination Task
Dunn et al. (Dunn et al., 2007), UK	Journal article	Correlational, clinical and non-clinical	Total *N* = 54, community depressed (*N* = 18), clinic depressed (*N* = 18)	Female (72), *M*age community depressed = 40.1, *SD* = 15.6, *M*age clinically depressed = 47.1, *SD* = 9.9	N/S	Depression (SCID-I, BDI)	Heartbeat Counting Task
Dunn et al. (Dunn et al., 2010), UK	Journal article	Correlational, non-clinical	Total *N* = 113	Female (73), *M*age = 43.09, *SD* = 16.1	N/S	Depression, anxiety (semi-structured interviews, MASQ, BDI-I, STAI-T)	Heartbeat Counting Task
Eggart and Valdés-Stauber., (Eggart and	Journal article	Correlational, clinical	Total *N* = 87	Female (56), *M*age = 47.57, *SD* = 10.64	N/S	Depression (ICD-10 criteria)	MAIA (NO, ND, NW, EA, BL, T, AR, SR)

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Table 1 (*continued*)

Author (year), location	Format of publication	Study design, sample	Sample size (*N*)	Gender (%), *M*age, *SD* (years)	Ethnicity (%)	Mental health condition (measures)	Interoception measure
Valdés-Stauber, 2021)), Germany							
Ehlers and Breuer (Ehlers and Breuer, 1992), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 188, PD (*N* = 65), healthy controls (*N* = 46)	Female PD (74), *M*age = 33.3, *SD* = 9.3, female healthy controls (34 out of 46), *M*age = 34.3, *SD* = 10	N/S	Anxiety (SCID, STAI)	Heartbeat Counting Task
Ehlers et al. (Ehlers et al., 1995), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 152, PD (*N* = 112), healthy controls (*N* = 40)	Female PD (68), Mage = 36.3, SD = 7.7, female healthy controls (68), Mage = 36.4, SD = 9.1	N/R	Anxiety (SCID, ADIS-R, STAI)	Heartbeat Counting Task
Eng et al. (Eng et al., 2020), USA	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 157, OCD (*N* = 81), healthy controls (*N* = 76)	Female OCD (65), *M*age = 34.1, *SD* = 12.6, female healthy controls (51), *M*age = 31, *SD* = 10.1	N/S	OCD (MINI, DOCS)	MAIA (NO, ND, NW, EA, BL, T, AR, SR)
Eng et al. (Eng et al., 2022), USA	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 130, OCD (*N* = 77), healthy controls (*N* = 53)	Female OCD (50 out of 77), Mage = 31.6, SD = 10.9, female healthy controls (26 out of 53), Mage = 31.8, SD = 10.8	N/S	OCD (MINI, Y-BOCS)	MAIA (NO, ND, NW, EA, BL, T, AR, SR)
Ewing et al. (Ewing et al., 2017), UK	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 180, depression (*N* = 44), anxiety (*N* = 24), healthy controls (*N* = 42)	Female depression (69), *M*age = 34.27, *SD* = 14.25, female anxiety (83), *M*age = 24.88, *SD* = 8.54, female healthy controls (81), *M*age = 28.19, *SD* = 9.8	N/S	Depression, anxiety (DSM-V or ICD-10 criteria)	Heartbeat Counting Task, Heart Rate Discrimination Task, BPQ
Furman et al. (Furman et al., 2013), USA	Journal article	Group-based, separate clinical and non-clinical comparisons; correlational, clinical and non-clinical	Total *N* = 61, depression (*N* = 25), healthy controls (*N* = 36)	Female depression (100), *M*age = 38.2, *SD* = 11.9, female healthy controls (100), *M*age = 36, *SD* = 12.5	N/S	Depression (SCID, BDI-II)	Heartbeat Counting Task
Grimaldi (Grimaldi, 2022), USA	Thesis	Correlational, non-clinical	Total *N* = 519	Male (56), *M*age = 36.49, *SD* = 12.13	White (69), Asian/Asian American (12), Black (11), Hispanic/Latinx (5), Multiracial (2), Other (1)	OCD (OCI-R)	MAIA (NO, ND, NW, EA, BL, T, AR, SR)
Grossi et al. (Grossi et al., 2017), Italy	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 36, anxiety (*N* = 22), healthy controls (*N* = 14)	Male anxiety (83), *M*age = 38.9, *SD* = 10.2, female healthy controls (64), *M*age = 38.85, *SD* = 14.01	N/S	Anxiety (DSM-V criteria, IAS (b), STAI-T)	SAQ
Harrison et al. (Harrison et al., 2021), Switzerland	Journal article	Group-based, separate non-clinical comparisons; correlational	Total *N* = 60, moderate anxiety (*N* = 30), very low anxiety (*N* = 30)	Male moderate anxiety (50), *M*age = 24.2, *SD* = 5, male very low anxiety (50), *M*age = 25.4, *SD* = 3.9	N/S	Depression (CESD); anxiety (STAI-T, GAD7)	BPQ
Jeganathan et al. (Jeganathan et al., 2024), Australia	Journal article	Group-based, separate clinical and non-clinical comparisons; correlational, clinical	Total *N* = 81, psychosis (*N* = 40), healthy controls (*N* = 41)	Male psychosis (85), *M*age = 37, *SD* = 8.1, male healthy controls (85), *M*age = 39.6, *SD* = 8.4	Psychosis White (88) healthy controls White (95)	Psychosis (DSM-V criteria, PANSS); depression (HAMD)	Heart Rate Discrimination Task
Jin et al. (Jin et al., 2020), China	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 39, PD (*N* = 18), healthy controls (*N* = 21)	Female PD (67), *M*age = 38.11, *SD* = 11.82, female healthy controls (13 out of 21), *M*age = 38.05, *SD* = 10.32	N/S	PD (DSM-V or ICD-10 criteria, MINI, HAMA)	Heartbeat Counting Task
Jokić and Purić (2021), Serbia	Journal article	Correlational, non-clinical	Total *N* = 549	Female (62), Mage = 19, SD = 0.2	N/S	OCD (OCI-R)	MAIA (NO, ND, NW, EA, BL, T, AR, SR)
Koreki et al. (Koreki et al., 2021), Japan	Journal article	Group-based, separate clinical and non-clinical comparisons; correlational, clinical	Total *N* = 72, psychosis (*N* = 42), healthy controls (*N* = 30)	Female psychosis (50), *M*age = 40.5, *SD* = 9.9, female healthy controls (57), *M*age = 40.1, *SD* = 9.9	N/S	Schizophrenia (DSM-V criteria, PANSS)	Heartbeat Counting Task, MAIA (NO, ND, NW, EA, BL, T, AR, SR)

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Table 1 (*continued*)

Author (year), location	Format of publication	Study design, sample	Sample size (*N*)	Gender (%), *M*age, *SD* (years)	Ethnicity (%)	Mental health condition (measures)	Interoception measure
Körmendi, et al. (2023), Hungary	Journal article	Correlational, non-clinical	Total *N* = 102	Female (87), Mage = 20.8, SD = 5.08	N/S	Depression (BDI-II); anxiety (STAI-T)	Heartbeat Counting Task
Krautwurst et al. (Krautwurst et al., 2014), Germany	Journal article	Correlational, non-clinical	Total *N* = 100	Female (79), Mage = 23.7, SD = 3.4	N/S	Health anxiety (WI, MIHT)	Heartbeat Counting Task
Krautwurst et al. (Krautwurst et al., 2016), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 81, health anxiety (*N* = 49), healthy controls (*N* = 56)	Gender N/S, *M*age N/S	N/S	Health anxiety (SCID, WI)	Heartbeat Counting Task
Lackner and Fresco (Lackner and Fresco, 2016), USA	Journal article	Correlational, non-clinical	Total *N* = 101	Female (74), Mage = 19.63, SD = 2.04	Caucasian (69), African American (13), Other (8), Biracial (5), Latino (3), Asian (2)	Depression, anxiety (self-reported clinical symptoms, MASQ-S)	Heartbeat Counting Task
Lee et al. (Lee et al., 2024), Korea	Journal article	Correlational, clinical	Total *N* = 110	Female (73), Mage N/S, age range 19–65	N/S	Depression, anxiety (SCID, PHQ-9, STAI);	MAIA (NO, ND, EA, BL, T, AR)
Leonidou et al. (Leonidou et al., 2020), Cyprus	Journal article	Group-based, separate non-clinical and control comparisons	Total *N* = 90, high anxiety (*N* = 30), low anxiety (*N* = 30)	Female high anxiety (90), *M*age = 21.68, *SD* = 3.69, female low anxiety (73), *M*age = 21.6, *SD* = 3.86	N/S	Anxiety (IAS, SHAI)	Heartbeat Counting Task
Li et al. (Li et al., 2020), China	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 36, anxiety (*N* = 18), healthy controls (*N* = 18)	Male anxiety (72), *M*age = 41.89, *SD* = 10.79, male healthy controls (61), *M*age = 38.17, *SD* = 10.56	N/S	Anxiety ((DSM-IV criteria, SCID, HAMA)	Heartbeat Counting Task
Limmer et al. (Limmer et al., 2015), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 93, PD (*N* = 40), healthy controls (*N* = 53)	Female PD (63), *M*age = 44, *SD* = 12, female healthy controls (72), *M*age = 41, *SD* = 14	N/S	PD (DSM-IV criteria); depression (PHQ-9), anxiety (STAI)	BPQ
Mallorquí-Bagué et al. (2014), UK	Journal article	Correlational, non-clinical	Total *N* = 36	Female (56), *M*age = 24.1, *SD* = 6.4	N/S	Anxiety (STAI)	Heartbeat Counting Task
Mussgay et al. (Mussgay et al., 1999), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 546, PD (*N* = 53), healthy controls (*N* = 48)	Female PD (58), *M*age = 41.3, female healthy controls (60), *M*age = 35.8	N/S	PD; diagnosis (ICD-9 criteria), STAI	Heartbeat Counting Task
Noda et al. (Noda et al., 2022), Japan	Journal article	Correlational, clinical	Total *N* = 32	Female (100), *M*age = 32.8, *SD* = 4.9	N/S	Anxiety (STAI); postnatal depression (EPDS)	Heartbeat Counting Task, MAIA (NO, ND, EA, BL, T, AR)
Pollatos, Herbert, et al. (Pollatos et al., 2007), Germany	Journal article	Correlational, non-clinical	Total *N* = 18	Male (100), *M*age = 25.2, *SD* = 2.9	N/S	Anxiety (STAI)	Heartbeat Counting Task
Pollatos, Traut-Mattausch, et al. (Pollatos et al., 2007), Germany	Journal article	Correlational, non-clinical	Total *N* = 102	Female (66), *M*age = 26.9, *SD* = 6.3	N/S	Anxiety (STAI)	Heartbeat Counting Task
Pollatos et al. (Pollatos et al., 2009), Germany	Journal article	Correlational, non-clinical	Total *N* = 119	Female (82), *M*age = 27.2, *SD* = 6	N/S	Anxiety (STAI); depression (BDI-II)	Heartbeat Counting Task
Scheffers et al. (Scheffers et al., 2019), Netherlands	Journal article	Correlational, clinical	Total *N* = 74	Female (59), *M*age female = 41.9, *SD* = 12, *M*age male = 45.2, *SD* = 12.2	N/S	Depression (DSM-IV criteria, IDS-SR)	SAQ
Schultchen et al. (Schultchen et al., 2019), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons; correlational, clinical	Total *N* = 52, OCD (*N* = 26), healthy controls (*N* = 26)	Male OCD (54), *M*age = 28.6, *SD* = 7.2, male healthy controls (54), *M*age = 26.5, *SD* = 5.6	N/S	OCD (ICD-10 criteria, Y-BOCS); depression (BDI); anxiety (STAI-T)	Heartbeat Counting Task
Smith et al. (Smith et al., 2020), USA	Journal article	Group-based, separate clinical and non-clinical comparisons	Total *N* = 434, depression (*N* = 69), anxiety (N = 15), healthy controls (*N* = 52)	Female anxiety (67), *M*age = 36.42, *SD* = 10.01, female depression (70), *M*age = 37.12, *SD* = 11.83, female healthy controls (50), *M*age = 32.04, *SD* = 11.08	N/S	Anxiety, depression (OASIS, PHQ-9, MINI)	Heartbeat Counting Task

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Table 1 (*continued*)

Author (year), location	Format of publication	Study design, sample	Sample size (*N*)	Gender (%), *M*age, *SD* (years)	Ethnicity (%)	Mental health condition (measures)	Interoception measure
Solano Lopez and Moore (2019), USA	Journal article	Correlational, clinical	Total $N = 76$	Female (71), *M*age = 52, *SD* = 11.7	African American (91), Other (5), White (4)	Anxiety, depression (PROMIS, DASS)	Heartbeat Counting Task, MAIA (NO, ND, EA, BL, T, AR, SR)
Stern et al. (Stern et al., 2020), Israel	Journal article	Correlational, non-clinical	Total $N = 70$, sample in correlational analysis ($N = 23$)	Female (53), *M*age = 25.4	N/S	Anxiety; STAI	Heartbeat Counting Task
Stevens et al. (Stevens et al., 2011), Germany	Journal article	Group-based, separate non-clinical and control comparisons; correlational	Total $N = 48$, high SA ($N = 25$), low SA ($N = 23$)	Female high SA (52), *M*age = 21.6, *SD* = 1.9, female low SA (52), *M*age = 22.4, *SD* = 3.8	N/S	Anxiety, depression (FNE, SPS, SIAS, ASI, STAI-T)	Heartbeat Counting Task
Stewart et al. (Stewart et al., 2001), Canada	Journal article	Group-based, separate clinical and non-clinical comparisons	Total $N = 30$, high anxiety ($N = 15$), low anxiety ($N = 15$)	Female (70), *M*age = 20.1, *SD* = 1.4	N/S	Anxiety (ASI)	Heartbeat Counting Task
Tan et al. (Tan et al., 2018), China	Journal article	Correlational, non-clinical	Total $N = 50$	Gender N/S, *M*age N/S	N/S	Anxiety (STAI)	Heartbeat Counting Task
Terhaar et al. (Terhaar et al., 2012), Germany	Journal article	Group-based, separate clinical and non-clinical comparisons	Total $N = 32$, depression ($N = 16$), healthy controls ($N = 16$)	Female depression (81), *M*age = 41.75, *SD* = 12.4, female healthy controls (75), *M*age = 39.81, *SD* = 17.6	N/S	Depression (DSM-IV criteria, HAMD, BDI)	Heartbeat Counting Task
Torregrossa et al. (Torregrossa et al., 2022), USA	Journal article	Group-based, separate clinical and non-clinical comparisons	Total $N = 58$, schizophrenia ($N = 30$), healthy controls ($N = 28$)	Male schizophrenia (53), Mage = 47.93, SD = 10.21, male healthy controls (50), Mage = 48.68, SD = 8.2	N/S	Schizophrenia (DSM-V criteria/ SCID, BPRS, SAPS, SANS)	Heartbeat Counting Task
Trevisan et al. (Trevisan et al., 2023), USA	Journal article	Correlational, non-clinical	Total $N = 327$, sample in correlational analyses ($N = 322$)	Male (53), *M*age = 42.2	White (88), Black or African American (7), Asian (4), Mixed (2)	Anxiety, health anxiety (PSWQ, HAI)	BPQ, ISQ
Van der Does et al., (Van der Does et al., 1997)), Netherlands	Journal article	Group-based, separate clinical and non-clinical comparisons	Total $N = 60$, PD ($N = 23$), depression ($N = 16$), healthy controls ($N = 21$)	Female PD (65), *M*age = 41.1, *SD* = 10.6, female depression (81), *M*age = 40.4, *SD* = 13, female healthy controls (62), *M*age = 44.5, *SD* = 14.1	N/S	PD, depression (DSM-IV criteria, ADIS-R, ASI, SDS)	Heartbeat Counting Task
Wiebking et al. (Wiebking et al., 2010), Germany	Journal article	Correlational, clinical	Total $N = 34$, depression ($N = 17$)	Female (65), *M*age = 41.88, *SD* = 12.1	N/S	Depression (DSM-IV criteria, BDI, BHS)	BPQ
Zhou et al. (Zhou et al., 2022), China	Journal article	Group-based, separate clinical and non-clinical comparisons	Total $N = 54$, depression ($N = 27$), healthy controls ($N = 27$)	Male depression (59), *M*age = 27, *SD* = 6.7, male healthy controls (56), *M*age = 25.3, *SD* = 3.8	N/S	Depression (DSM-IV criteria, MINI); depression (BDI-II)	Heartbeat Counting Task
Zhou et al. (Zhou et al., 2024), China	Journal article	Group-based separate non-clinical and control comparisons; correlational	Total $N = 79$, depression ($N = 39$), controls ($N = 40$)	Gender N/S, *M*age depression = 20.87, *SD* = 2.11, *M*age healthy controls = 22.57, *SD* = 2.64	N/S	Depression (CES-D, somatic symptom subscale, psychological symptom subscale)	Heartbeat Counting Task, BPQ
Zoellner and Craske (Zoellner and Craske, 1999), USA	Journal article	Group-based separate non-clinical and control comparisons; correlational	Total $N = 58$, panic ($N = 31$), controls ($N = 27$)	Male panic (55), *M*age = 19, *SD* = 3.11, female controls (52), *M*age = 18.57, *SD* = 1.04	N/S	Anxiety (DSM-III criteria, ASI)	Heartbeat Counting Task

Note. ASI: Anxiety Sensitivity Index, STAI-T: State Trait Anxiety Inventory – trait version, N/S: Not stated, ADIS-R: Anxiety Disorders Interview Schedule - Revised, SCID-I / SCID-II: Structured Clinical Interview for Diagnostic and Statistical Manual of Mental Disorders Version 1.0 / Version 2.0, DSM-III / IV / V: Diagnostic and Statistical Manual of Mental Disorders, 3rd Edition / 4th Edition / 5th Edition, PANSS: Positive and Negative Syndrome Scale, HDRS / HAMD: Hamilton Depression Rating Scale, DASS-21: The Depression, Anxiety and Stress Scale – 21 Items, IAS (a): Interoceptive Accuracy Scale, MAIA: Multidimensional Assessment of Interoceptive Awareness, NO: MAIA Noticing, ND: MAIA Not Distracting, NW: MAIA Not Worrying, EA: MAIA Emotional Awareness, BL: MAIA Body Listening, T: MAIA Trusting, AR: MAIA Attention Regulation, SR: MAIA Self-regulation, BDI / BDI-II: Beck Depression Inventory / 2, MINI: Mini International Neuropsychiatric Inventory, PHQ-9: Patient Health Questionnaire, BPQ: Body Perception Questionnaire, HAM-A: Hamilton Anxiety Rating Scale, GAD7: Generalised Anxiety Disorder Questionnaire, PROMIS: Patient-Reported Outcomes Measurement Information System, MASQ: Mood and Anxiety Symptom Questionnaire, N/R: Not reported, ICD-9 / 10: International Classification of Diseases, Ninth / Tenth Revision, DOCS: Dimensional Obsessive-Compulsive Scale, Y-BOCS: Yale-Brown Obsessive-Compulsive Scale, OCI-R: Obsessive Compulsive Inventory - Revised, IAS (b): Illness Attitude Scale, SAQ: Self-Awareness Questionnaire, CES-D: Centre for Epidemiologic Studies Depression Scale, GAF: Global Assessment of Functioning, WI: Whiteley Index, MIHT: The Multidimensional Inventory of Hypochondriacal Traits, OASIS: Overall Anxiety Severity and Impairment Scale, SHAI: Short Health Anxiety Inventory, EPDS: Edinburgh Postnatal Depression Scale, IDS-SR: The Inventory of Depressive Symptomatology Self Report, FNE: Fear of Negative Evaluation Scale, SPS: Social Phobia Scale, SIAS: Social interaction Anxiety Scale, BPRS: Brief Psychiatric Rating Scale, SAPS: Scale for the Assessment of Positive Symptoms, SANS: Scale for the Assessment of Negative Symptoms, PSWQ: Penn State Worry Questionnaire, HAI: Health Anxiety Inventory, ISQ: Interoception Sensory Questionnaire, BHS: Beck Hopelessness Scale, OBQ-TRIP: The Obsessive Beliefs Questionnaire, PD = Panic

Disorder; MDD = Major depressive disorder; SA = Social anxiety; OCD = Obsessive compulsive disorder; GAD = Generalised anxiety disorder; USA = United States of America; UK = United Kingdom

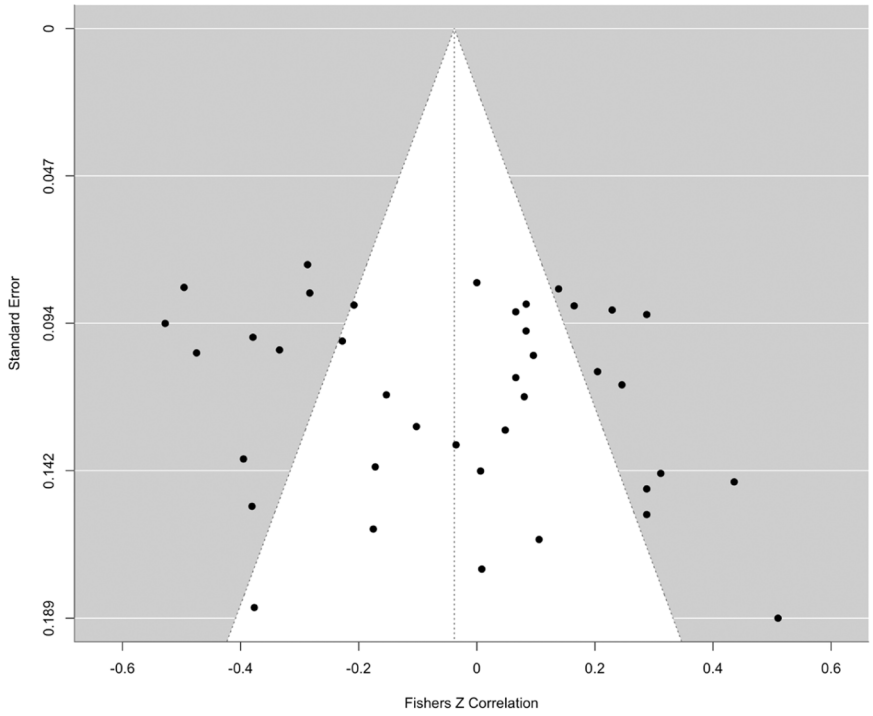


Fig. 2. A funnel plot of effect sizes for heartbeat counting accuracy.

and schizophrenia (r = -.435 [95% CI: −.561 to −.290], $p < .001$, I^2 = 45.0%), symptomology were both associated with worse accuracy on the Heartbeat Counting Task. The pooled association of heartbeat counting accuracy with anxiety (38 effect sizes from 30 studies) was non-significant (r = .073 [95% CI: −.005–.149], p = .065, I^2 = 64.8%). There were only three available effect size for OCD; these were not included in the moderation analysis.

3.2. Heart rate discrimination task

The multilevel-meta-analysis was not a better fit of the data ($X^2(1)$ = 0.00, p = 1.00). There was a significant, negative pooled association of heart rate discrimination accuracy with symptoms of common mental health disorders (r = -.137 [95% CI: −.202 to −.071], $p < .001$, I^2 = 0%), based on 12 effect sizes from 4 studies. The common language interpretation of this effect is that if two individuals are randomly selected, a person with a worse interoception score will also have higher mental health symptomology 54.4% of the time. Removal of the smallest (r = -.121 [95% CI: −.193 to −.048]) and the largest (r = -.146 [95% CI: −.212 to −.080]) effect sizes did not significantly impact the findings. Neither study averaged funnel plots nor Egger's test were conducted due to the small number of studies. The 95% prediction intervals were −0.202 to −.071, suggesting that the true effect in a future study could plausibly range from r = -.202 to r = -.071. The narrow range reflects limited heterogeneity. Tests of moderation by mental health disorder could not be performed due to the small number of effect sizes in each subgroup (n = 3 for anxiety, 3 for depression, 5 for schizophrenia).

4. Meta-analyses of self-report questionnaires

4.1. Adaptive interoceptive sensing

The multilevel meta-analysis was a better fit of the data ($X^2(1)$ = 5.45, p = .020). There was a non-significant pooled association of adaptive interoceptive sensing with common mental health disorders (r = .136 [95% CI: −.008–.276], p = .065, I^2 = 87.8%), based on 15 effect sizes from 11 studies. The common language interpretation of this effect is that if two individuals are randomly selected, a person with better adaptive interoceptive sensing score will also have higher mental health symptomology 54.4% of the time. The 95% prediction intervals were −0.327 to −.547, suggesting that the true effect in a future study could plausibly range from r = -.327 to r = -.547.

Removal of the smallest (r = .14,95% CI: −.003–.278]) and the largest (r = .099, [95% CI: −.040–.234]) effect sizes did not significantly impact the findings. The funnel plot for the study-level effect is shown in Fig. 3. Egger's test was significant (Z = 2.22, p = .026), indicating potential publication bias or small study effects.

Tests of moderation by mental health disorder could not be performed due to the small number of effect sizes in each subgroup (n = 4 for anxiety, 6 for depression, 1 for schizophrenia, 4 for OCD).

4.2. Adaptive interoceptive attention

The multilevel meta-analysis was not a better fit of the data ($X^2(1)$ = 0.00, p = .999). There was a significant, negative pooled association of adaptive interoceptive attention with common mental health disorders (r = -.183 [95% CI: −.255 to −.110], $p < .001$, I^2 = 87%), based on 35 effect sizes from 10 studies. The common language interpretation of this effect is that if two individuals are randomly selected, a person with a worse adaptive interoceptive attention score will also have greater mental health symptomology 55.5% of the time. The 95% prediction intervals were −0.527–0.212, suggesting that the true effect in a future study could plausibly range from r = -.527 to r = .212.

Removal of the smallest (r = .170 [95% CI: −.240 to −.099]) and the largest (r = -.196 [95% CI: −.264 to −.126]) effect sizes did not significantly impact the results.

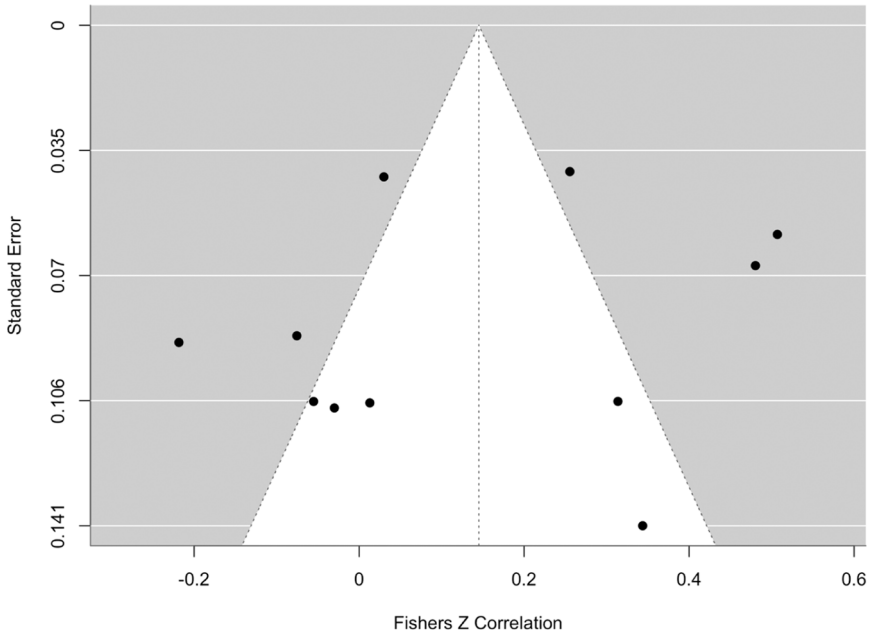


Fig. 3. A funnel plot of effect sizes for adaptive interoceptive sensing.

The funnel plot for the study-level effects is shown in Fig. 4. Egger's test was not statistically significant ($Z = 1.42$, $p = .155$).

The moderating effect of mental health disorder was non-significant ($X^2(2) = 2.50$, $p = .286$). The pooled associations of adaptive interoceptive attention with anxiety (7 effect sizes from 3 studies: $r = -.242$ [95% CI: −.425–.038], $p = .020$, $I^2 = 80.7\%$), depression (13 effects from 5 studies: $r = -.241$ [95% CI: −.369 to −.102], $p < .001$, $I^2 = 80.7\%$), and OCD (12 effects from 4 studies: $r = -.116$ [95% CI: −.201 to −.030], $p = .009$, $I^2 = 86.3\%$), were all significant and negative, with worse adaptive interoceptive attention associated with greater mental health symptomology. Only three effect sizes were included for schizophrenia.

4.3. Adaptive interoceptive interpretation

The multilevel meta-analysis was a better fit of the data ($X^2(1) = 6.45$, $p = .011$). There was a non-significant pooled association of adaptive interoceptive interpretation with common mental health disorders ($r = -.117$ [95% CI: −.233–.001], $p = .053$, $I^2 = 92\%$), based on 46 effect sizes from 10 studies. The common language interpretation of this effect is that if two individuals are randomly selected, a person with a worse adaptive interoceptive interpretation score will also have greater mental health symptomology 53.5% of the time. The 95% prediction intervals were −0.568–0.388, suggesting that the true effect in a future study could plausibly range from $r = -.568$ to $r = .388$.

Removal of the smallest ($r = -.107$ [95% CI: −.217 to −.006]) effect

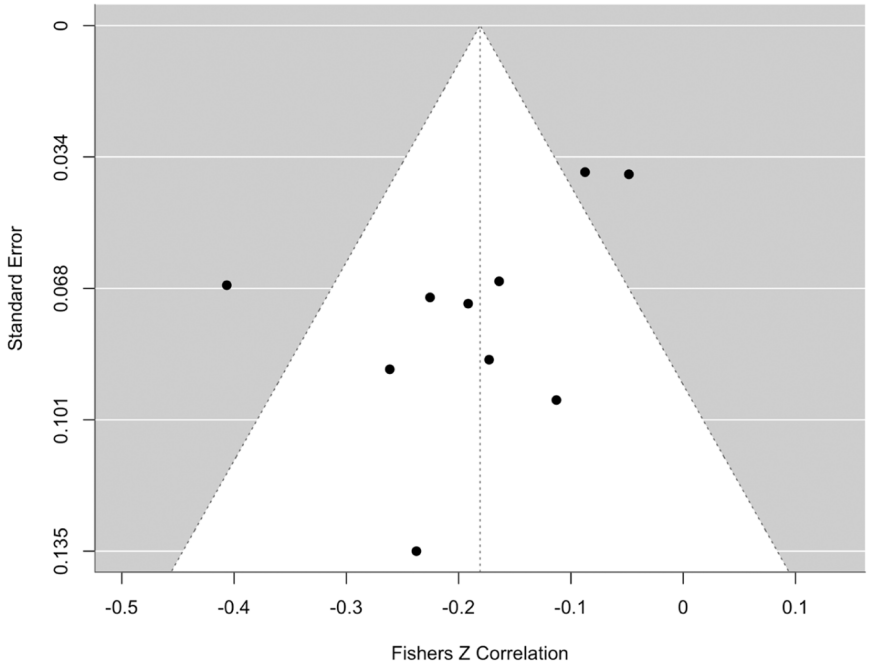


Fig. 4. A funnel plot of effect sizes for adaptive interoceptive attention.

size did not impact the statistical significance. Removal of the largest (r = -.132 [95% CI: −.246 to −.014]) effect size resulted in a pooled model that was statistically significant (p = .028). The funnel plot for the study-level effects is shown in Fig. 5. Egger's test was not statistically significant (Z = 0.72, p = .468).

The moderating effect of mental health disorder was non-significant (X^2(2) = 2.45, p = .293). The pooled association of adaptive interoceptive interpretation with depression (17 effect sizes from 5 studies: r = -.232 [95% CI: −.367 to −.087], p = .002, I^2 = 74.3%) showed a significant, negative association. The pooled associations with anxiety (9 effect sizes from 3 studies: r = -.141 [95% CI: −.511–.274], p = .515, I^2 = 90.3%) and OCD (16 effect sizes from 4 studies; r = -.031 [95% CI: −.166–.105], p = .652, I^2 = 74.3%),) were not significant. Only four effect sizes were included for schizophrenia.

4.4. Maladaptive interoceptive attention

The multilevel meta-analysis was a better fit of the data (X^2(1) = 17.30, p = .001). There was a significant, positive pooled association of maladaptive interoceptive attention with common mental health disorders (r = .381 [95% CI:.233–.512], p < .001, I^2 = 82.5%), based on 19 effect sizes from 9 studies. The common language interpretation of this effect is that if two individuals are randomly selected, a person with a greater maladaptive interoceptive attention score will also have greater mental health symptomology 62.4% of the time. The 95% prediction intervals were −0.076–0.706, suggesting that the true effect in a future study could plausibly range from r = -.076 to r = .706.

Removal of the smallest (r = .383 [95% CI:.241–.510]) and the largest (r = .365 [95% CI:.210–.502]) effect sizes did not impact the statistical significance of the pooled effect. The funnel plot for the study-level effects is shown in Fig. 6. Egger's test was not statistically significant (Z = 0.63, p = .527).

The moderating effect of mental health disorder was non-significant (X^2 (1) = 1.43, p = .231), with the pooled associations of maladaptive interoceptive interpretation with depression (5 effect sizes from 5 studies: r = .331 [95% CI:.047–.566], p = .023, I^2 = 86.1%), and anxiety (12 effect sizes from 7 studies: r = .362 [95% CI:.218–.490], p < .001, I^2 = 76.8%) both showing that higher levels of maladaptive interoceptive interpretation were associated with greater depressive and anxious symptomology. Only 1 effect size was included for schizophrenia.

5. Discussion

We aimed to clarify the relationship between interoception and common mental health disorders, including anxiety, depression, schizophrenia, bipolar disorder, and OCD, by synthesizing data across a range of studies assessing cardiac perception and self-reported interoceptive dimensions, in clinical and non-clinical populations. A total of 57 papers published between 1991 and 2024, yielding a total of 206 effect sizes from a total sample size of N = 6652, were identified for inclusion in meta-analyses.

Our findings indicated that, overall, effect sizes were small and associations of cardiac perception and self-reported interoceptive dimensions with common mental health disorders were mixed. Consistent with work showing that objective and self-reported measures do not converge on a common construct, and even different tests of cardiac perception appear to measure different aspects of interoception (Desmedt et al., 2025), we performed meta-analyses separately for different objective tests, and for different dimensions of interoception. Findings for different tests of cardiac perception showed conflicting results, with a non-significant pooled association of Heartbeat Counting Task performance with common mental health disorders. Prediction intervals for the Heartbeat Counting Task were wide, showing substantial heterogeneity, but a significant moderator analysis indicated that higher depressive and schizophrenia symptomology was associated with worse Heartbeat Counting Task performance, with a non-significant association for anxiety. Heart Rate Discrimination Task performance on the other hand showed a significant, negative pooled association across mental health disorders, indicative of greater interoceptive difficulties among those with worse mental health, but there were too few effect sizes to conduct tests of moderation.

These differences might reflect differing demands placed on participants by these different tasks. Performance on the Heartbeat Counting Task requires participants to count heartbeats over specific intervals, placing demands on working memory and sustained attention, while

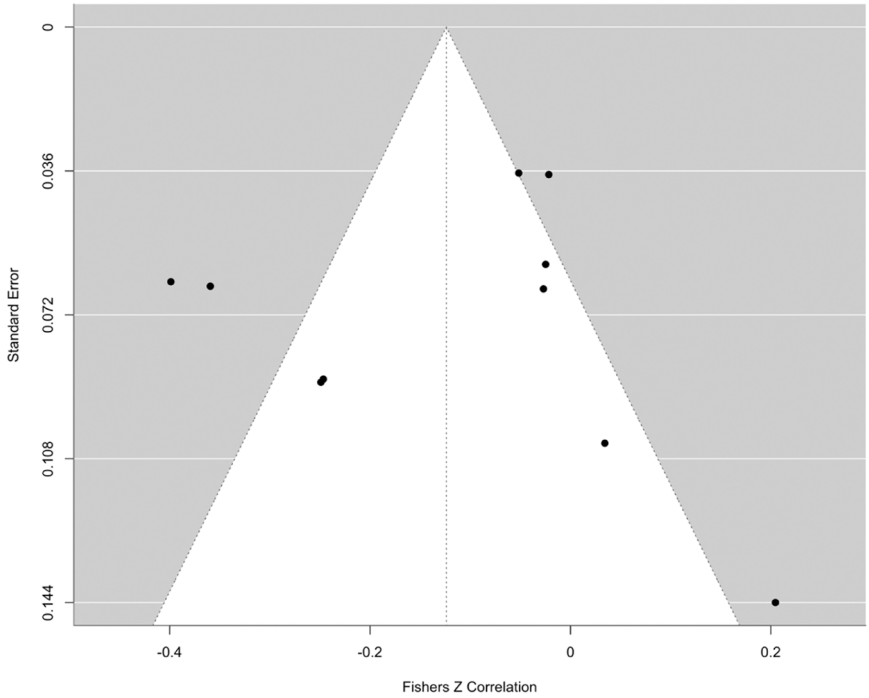


Fig. 5. A funnel plot of effect sizes for adaptive interoceptive interpretation.

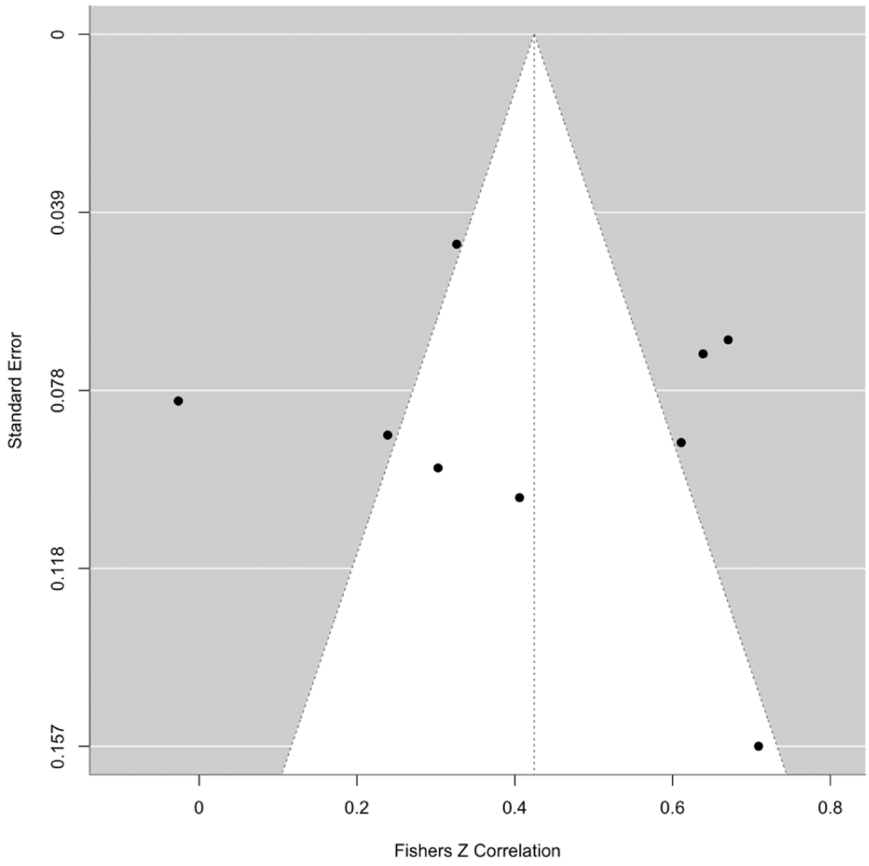


Fig. 6. A funnel plot of effect sizes for maladaptive interoceptive attention.

performance on the Heart Rate Discrimination Task requires the multisensory integration of exteroceptive and interoceptive stimuli (Hickman et al., 2020). Earlier reviews have also reported mixed outcomes when examining the relationship of cardiac perception with mental health symptomology (Adams et al., 2022; Desmedt et al., 2022; Eggart et al., 2019). For example, a small-scale narrative review (Eggart et al., 2019) concluded that worse cardiac perception was associated with greater depressive symptomology, but a later meta-analysis found no association between Heartbeat Counting Task performance and anxious and depressive symptomology (Desmedt et al., 2022). Further, Adams et al. (2022) found no evidence for an overall meta-analytic association between cardiac perception and anxiety, and a narrative synthesis of systematic reviews and meta-analyses similarly concluded that there was no reliable association between cardiac perception and anxiety, depression or psychosis (Jenkinson et al., 2024). The results of our meta-analyses highlight the importance of design and measurement choices. Overall, our findings show mixed support for the hypothesis that cardiac perception may represent a transdiagnostic marker of mental ill health (Brewer et al., 2021; Khalsa et al., 2018; Nord and Garfinkel, 2022), while highlighting the importance of a more nuanced approach and the need to avoid overgeneralizing between tasks (Desmedt et al., 2025; Hickman et al., 2020).

When considering these results, it should be noted that methodological concerns have been raised about the validity of commonly used measures of cardiac perception. For example, the Heartbeat Counting Task is vulnerable to a reliance on non-interoceptive signals to count heartbeats, participants being able to draw on prior knowledge and awareness of heart rate to estimate heartbeat frequency, and there are criticisms that commonly used tasks of cardiac perception cannot disentangle interoceptive inaccuracy from wider perceptual abnormality (Brewer et al., 2021; Desmedt et al., 2018; Ring and Brener, 2018). The Heart Rate Discrimination Task, however, asks participants to attend to their cardiac sensations and then to decide on each trial whether a "feedback" tone series is faster or slower than their heart rate, and has shown greater precision and reliability (Legrand et al., 2022). However, shortfalls have also been highlighted in restricting the method of interoceptive measurement to cardiac tasks (Desmedt et al., 2022; Eggart et al., 2019; Jenkinson et al., 2024), and the use of tasks that span different interoceptive signals and that are matched in terms of task demands has been recommended (Brewer et al., 2021).

Our results also show mixed associations of self-reported interoception with common mental health disorders. Considering recent criticisms that self-reports do not measure a common unitary construct, with low overall convergence and weak correlations between items (Desmedt et al., 2022), we conducted meta-analyses on separable interoceptive dimensions outlined by #Desmedt et al. (2025), further distinguishing between adaptive and maladaptive processes (Treves et al., 2025; Trevisan et al., 2023). Our results showed non-significant pooled associations for adaptive interoceptive sensing including MAIA 'noticing' subscale, and the IAS), and interpretation (including MAIA 'not worrying', 'emotional awareness', 'body listening', and 'trusting' subscales), with mental health symptomology, but removal of the largest effect size for interpretation yielded a significant, negative pooled association. Moderator analyses showed that while depressive symptomology was associated with problems in adaptive interoceptive interpretation, there was no association for either anxiety or OCD symptomology. The pooled association for adaptive interoceptive attention (including MAIA 'attention regulation, 'self-regulation', and 'not distracting' subscales) also showed a significant negative association, with associations for anxiety, depression, and OCD all significant and negative. Van Bael et al. (2024) emphasise how these facets involve not ignoring and acceptance of noxious and uncomfortable sensations, control of attention toward bodily sensations, and regulation of distress through attention control. Together, our results suggest that depression

in particualr is associated with problems surrounding interpretation, beliefs, attitudes, and categorization of internal signals, while depression, anxiety, and OCD all share difficulties in attentional processes related to internal signals.

For maladaptive processes, sufficient numbers of effect sizes were only available for maladaptive interoceptive attention, and all relied on the BPQ or its subscales, with the BPQ capturing attention to specific bodily sensations such dryness of the mouth or noises associated with digestion. The pooled association showed a significant, positive association with mental health symptomology, with a moderator analysis showing significant positive associations for both anxious and depressive symptomology.

In terms of clinical applications, therapeutic interventions that focus on the specific interoceptive domains (e.g., cardiac) and dimensions associated with different mental health disorders may have greater clinical efficacy than standard interoceptive training. For example, our findings indicated that cardiac perception was more impaired in people who experienced worse mental health, but the specific pattern of findings varied dependent on the task employed. Our results suggest that the development or adaptation of interventions focussing on the ability to perceive cardiac sensations should be pursued in future work. The mixed findings and absence of a moderator analysis for the Heart Rate Discrimination Task mean future work is needed to determine who may benefit the most from these interventions. Interventions targeting adaptive interoceptive attention in people with anxious, depressive, and obsessive-compulsive symptomology should also be pursued. Mindfulness-based practices, such as body mapping and controlled breathing, have been widely shown to cultivate awareness and accurate identification of bodily signals, improving the ability to accurately perceive internal sensations (Fischer et al., 2017; Weng et al., 2021). More specifically, research has highlighted their efficacy for enhancing cognitive processes focussed on interoceptive stimuli, including sustained attention, cognitive monitoring, and meta-awareness (Dahl et al., 2015; Tang et al., 2015), and emotion regulation (Lam et al., 2020).

The findings of our review question the idea that interoception is a single, unified construct, instead emphasising that the relationship between interoception and mental health can vary across different interoceptive dimensions and measurement modalities. This distinction has implications for how interoception is understood, assessed, and clinically applied. Differences within a specific interoceptive dimension, or single interoception measurement modality (e.g., objective versus self-report), may not necessarily indicate problems across other dimensions or modalities, highlighting the need for a comprehensive, multi-modal approach to assessment and intervention. Future research should seek to probe deeper into the reliability and validity of different measures of interoception, and specifically the interoceptive dimensions that they claim to measure. Mirroring a recent review (Jenkinson et al., 2024), it is reasonable to ask the question of whether a standardised, comprehensive, and multifaceted assessment of interoception can be developed and employed to encapsulate individual interoceptive profiles of different levels and bodily domains, across mental health conditions and diverse populations. We hope that our approach to distinguishing between different self-reported interoceptive dimensions, in contrast to approaches that pool these associations under broad constructs of interoceptive accuracy or sensibility, aids interpretation and replication efforts.

6. Strengths and limitations

Our review had several strengths. We used a comprehensive search strategy, we established the reliability of the study selection and data extraction processes, and we employed an assessment of methodological quality. However, some limitations must also be acknowledged. First, our review did not include any studies that employed measures of gastric or respiratory interoception, which may be relevant to symptoms of mental ill health, and particularly those relating to ‘core mood’ such as anxiety and depression (Banellis et al., 2025). Second, all studies that were eligible for inclusion were based on cross-sectional data, meaning that neither the temporal ordering of relationships nor causality can be inferred. It is important that future work employs interoception measurements spanning objective and self-report measures, and different interoceptive senses, as part of longitudinal population cohorts to examine the direction of effects over time. Third, some subgroups included in moderator analyses included a relatively small number of effect sizes, and for some analyses the number of effect sizes was too small to perform tests of moderation (e.g., Heart Rate Discrimination Task). We were also unable to identify enough effect sizes for more maladaptive interoceptive processes except for maladaptive interoceptive attention. Although small numbers of effect sizes can have implications for statistical power, to mitigate this we only examined subgroups that included a minimum of five effect sizes (Jackson and Turner, 2017). Fourth, the considerable heterogeneity across most models led to wide prediction intervals and increased uncertainty of possible associations in future studies.

Many of the studies in our review failed to report how results varied based on gender and ethnicity and did not include data on other potential moderating factors, including medication and comorbidities. This is important because previous studies have revealed cross-cultural differences in interoceptive processes and somatic awareness (Chentsova-Dutton and Dzokoto, 2014; Ma-Kellams, 2014), while potential moderating factors such as the use of medication and associated side-effects could also influence interoceptive processing and mental ill health (Burrows et al., 2022). Future studies should seek to collect information on ethnicity to allow for the examination of cross-cultural differences in interoceptive abilities, including exploration of the mechanistic origins of such differences, and their outcomes for culturally bound psychopathologies and therapeutic treatment responses. Finally, although we employed some procedures to ensure that the selection of papers, the extraction of data, and quality assessment was reliable, this involved a second reviewer checking a proportion of all papers, and this may not have been sufficient to ensure complete reliability (e.g., compared to these procedures being completed in duplicate for 100% of all records).

7. Conclusion

The results of our systematic review and meta-analyses indicate that the relationships of cardiac perception and self-report interoception with mental health difficulties is not straightforward. Our findings show mixed evidence that differences in interoceptive processing may represent a transdiagnostic marker of mental ill health. However, design and measurement choices are important, with different interoceptive dimensions associated with different magnitudes of effect. Our findings support claims that objective measures of cardiac perception and self-report measures of interoception may not measure a single unitary construct, and indeed that different tests of cardiac perception can show differing patterns of results with mental health outcomes. Future work is needed to address ambiguities regarding the definition, measurement and functional consequences of interoceptive differences as a potential transdiagnostic marker for mental health difficulties.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.neubiorev.2026.107061.

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