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Network analysis studies in patients with eating disorders: A systematic review and methodological quality assessment

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1 **Network analysis studies in patients with eating disorders: a systematic**
2 **review and methodological quality assessment**

3
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25
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28 and eating disorder
29
30

ABSTRACT

Introduction: Network psychometrics has been enthusiastically embraced by researchers studying eating disorders (ED), but a rigorous evaluation of the methodological quality of works is still missing. This systematic review aims to assess the methodological quality of cross-sectional network analysis (NA) studies conducted on ED clinical populations. **Methods:** PRISMA and PICOS criteria were used to retrieve NA studies on ED. Methodological quality was evaluated based on 5 criteria: variable-selection procedure, network estimation method, stability checks, topological overlap checks, handling of missing data. **Results:** 33 cross-sectional NA studies were included. Most studies focused on populations that were female, white and, with an anorexia nervosa (AN) diagnosis. Depending on how many criteria were satisfied, 27.3% of studies (n=9) were strictly adherent, 30.3% (n=10) moderately adherent, 33.3% (n=11) sufficiently adherent, 9.1% (n=3) poorly adherent. Missing topological overlap checks and not reporting missing data represented most unreported criteria, lacking respectively in 63,6% and 48.5% of studies. **Conclusions:** Almost all reviewed cross-sectional NA studies on ED report those methodological procedures (variable-selection procedure, network estimation method, stability checks) necessary for a network study to provide reliable results. Nonetheless these minimum reporting data require further improvement. Moreover, elements closely related to the validity of an NA study (controls for topological overlap and management of missing data) are lacking in most studies. Recommendations to overcome such methodological weaknesses in future NA studies on ED are discussed together with the need to conduct NA studies with longitudinal design, to address diversity issues in study samples and heterogeneity of assessment tools.

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66 **INTRODUCTION**

67 Eating disorders (ED) represent a complex category of psychiatric conditions, characterized
68 by heterogeneous presentations (Smith et al., 2018; Wildes et al., 2011), diagnostic migration and
69 comorbidity (Castellini et al., 2011), and high rates of residual diagnoses and symptoms (Tomba et
70 al., 2019). Additionally, there is scarce knowledge of prognostic indicators and predictors of clinical
71 outcomes (Linardon et al., 2017). Recently a large number of studies turned to network analysis (NA)
72 to explore EDs at symptom level, in an attempt to tackle their intricacies (Levinson et al., 2018; Smith
73 et al., 2018).

74 The conceptual basis of NA, that is the representation of psychiatric conditions as a
75 constellation of interacting symptoms (Borsboom 2017), seems particularly relevant to study EDs,
76 allowing researchers to model eating disorders as they present in everyday clinical practice (Treasure
77 et al., 2020). In general, NA is an ensemble of statistical tools which can be applied in a variety of
78 disciplines (Barabási, 2011; Newman et al., 2010; Zweig, 2016) and studies dynamic interactions
79 between phenomena through modelling of networks. These networks are composed of units called
80 *nodes*, representing a variable of choice, which are connected by *edges* (or *links*), representing the
81 pairwise interactions between the nodes. In psychopathology, NA has been proposed as a framework
82 to conceptualize psychological constructs: personality, intelligence, and mental disorders are framed
83 as complex systems of interacting variables (Costantini et al., 2015; Schmittmann et al., 2013).
84 Through its granular level analysis, NA allows to differentiate symptoms in terms of clinical
85 relevance, rather than considering them equivalent indicators of an underlying disorder, and also
86 allows to describe how they influence and reinforce each other (as postulated by many
87 psychopathological theories of specific disorders) beyond diagnostic categories (i.e., to explain
88 psychiatric comorbidities).

89
90 Since its development in the second decade of the 2000's, NA in psychopathology has been rapidly
91 evolving to overcome its limitations, assuage doubts about the methodology (McNally, 2021) and
92 answer the specific needs of clinical research (i.e. studying treatment effects), as well as to integrate
93 latent variable models (Blanken et al., 2019; Haslbeck, Borsboom, & Waldorp 2019; van Bork et al.,
94 2021; van Borkulo et al., 2017; Waldorp & Marsman 2020). The sudden burst in popularity of NA in
95 psychopathology resulted in a high number of studies published in a relatively short amount of time
96 developing different models of networks (i.e. cross-sectional and temporal networks) from different
97 kinds of data and methodological approaches.

98 Within this context groups of NA experts were encouraged to publish a list of reporting guidelines
99 and standards for studies using cross-sectional (Burger et al., 2022) and temporal (Blanchard et al.,

100 2022) networks, to reduce inhomogeneity and clarify which study procedures and results should be
101 reported when adopting the psychological network approach.

102

103 These guidelines mainly cover technical issues and list study elements which are pivotal in
104 critically reviewing a network study from a methodological perspective. Due to the technical nature
105 of these guidelines, though, some of the proposed aspects might not be of immediate relevance for
106 clinicians, or might not be readily accessible to an untrained audience. On the other hand, Burger and
107 colleagues (2022) rightly highlight how the identification of objective reporting standards for network
108 analysis studies represents a fundamental contribute towards improving the quality of empirical
109 network studies. Study quality itself may maximize the validity of study findings and it can be defined
110 as the degree to which researchers conducting a study have taken all appropriate steps (Khan et al.,
111 2011). The validity of a study in turn is closely related to the generalizability of its results: the
112 precision with which a study identifies a psychological construct is linked to its applicability on the
113 broader population sharing those specific psychological characteristics (Yarkoni, 2021). Therefore,
114 making these shared reporting standards explicit and accessible as much as possible should facilitate
115 the quality assessment of network studies, with a positive effect on translatability and reproducibility
116 of their results.

117

118 In their recent systematic review on NA studies on ED populations, Monteleone and Cascino
119 (2021) reported the most important symptoms, the nodes connecting groups of symptoms, and the
120 prognostic value of network nodes. However, the authors did not include a methodological quality
121 assessment of the works selected. Because of missing evaluation of study quality, it is difficult to
122 assess the reliability of the NA study results on ED considered in their review limiting the possibility
123 of confidently translating NA studies results into clinical practice.

124 Thus, the aim of the following systematic review is to evaluate the quality of NA studies on
125 clinical populations with ED. This will be accomplished using a selection of the recently published
126 best practices for reporting standards of cross-sectional NA studies by Burger et al. (2022). The
127 rationale guiding the selection process was to identify and extrapolate in a structured way those
128 aspects of NA methodological procedures which may represent the optimal balance between clinical
129 relevance, methodological rigor and accessibility by non-experts. Through this work we aim to further
130 support the appropriate interpretation of this novel approach in ED research as well as promote its
131 potential, in addition to promoting NA results among clinicians and researchers who might not have
132 technical knowledge of NA methodology.

133

134 **METHODS**

135 **Information sources and searches**

136 The following review has been developed in accordance with the Preferred Reporting Items
137 for Systematic Reviews and Meta-analysis guidelines (Moher et al., 2009). A publication date limit
138 was set between March 2020 and July 2022; Medline and EBSCO PsycINFO databases were searched
139 for published peer-reviewed scientific works, using the following combination of keywords and
140 Boolean operators: “*network analysis*” AND (“*eating disorders*” OR “*disordered eating*” OR
141 “*feeding disorder*” OR “*anorexia nervosa*” OR “*bulimia nervosa*” OR “*binge-eating disorder*” OR
142 “*binge eating*”). Titles and abstracts were screened by two authors independently (M.F.P. and G.T.).
143 Articles that appeared potentially relevant were retrieved and reviewed by M.F.P., and G.T. who
144 independently assessed each of the full reports, arriving at a consensus regarding eligibility. When
145 disagreements between the two authors arose, multiple rounds of full-text revision and discussions
146 were done until consensus was reached, with the involvement of the third author (E.T.) when needed.
147

148 **Eligibility criteria and data extraction**

149 Eligibility criteria and data extraction were based on ED clinical population and study
150 methods (i.e. application of psychological cross-sectional NA) (Centre for Reviews and
151 Dissemination 2006). Studies presenting NA of ED symptoms and symptoms from comorbid
152 disorders in ED clinical populations were deemed eligible. For a more detailed breakdown of
153 Population, Intervention, Comparison and Outcomes (PICOS) (Centre for Reviews and
154 Dissemination, 2006) criteria see Table 1. Eligible works were redacted in English and were selected
155 for inclusion if the sample was composed of eating disorder patients evaluated with DSM IV-TR or
156 DSM 5 (APA, 2000, 2013) in any study design and setting where NA was applied to estimate cross-
157 sectional network models. During the electronic search, studies were excluded for the following
158 reasons: the ED diagnosis was established uniquely through self-report instruments, participants
159 received a diagnosis of clinically relevant obesity only, the sample was composed of medically ill
160 patients with pathologies not related to ED, the study sample was composed of mixed clinical and
161 non-clinical populations, Bayesian, temporal or non-psychometric network analysis was applied (i.e.
162 social network analysis, neural networks, gene networks), the source was a review, opinion statement,
163 meta-analysis, book, case study or progress report. Remaining studies received full-text review to
164 determine the final selection. Data extraction was performed independently by two of the authors
165 (G.T. and M.F.P). The authors followed the PICOS pre-set extraction criteria, see Table 1.
166 Disagreements on inclusion were solved as in eligibility assessment. Data extracted regarding general
167 studies’ characteristics included: demographics (age, gender distribution, race, socioeconomic

168 status), sample size, diagnostic distribution of the samples, study design, ED clinical features included
169 in the network and measurements. The studies included in the final selection were then grouped and
170 discussed based on the outcome of the quality assessment.

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Table 1. PICOS criteria.

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175 **Quality assessment**

176 To assess the methodological quality of the studies included, the reporting guidelines and standards
177 for cross-sectional psychological NA studies proposed by Burger and colleagues (2022) were
178 considered. In their paper, Burger and colleagues (2022) organize the reporting guidelines and
179 standards in two main categories: one pertaining the methodology and the other the results section of
180 NA studies. Each section is further divided in three sub-sections covering: 1) **General Analysis**
181 **Routine** reporting standards shared across all applications of cross-sectional NA models, regardless
182 of the research question or study design; 2) **Analysis-specific routine** reporting standards pertaining
183 to specific research questions and methods; 3) **What to watch out for** general considerations which
184 Burger and colleagues (2022) include in multiple parts of their work. Such considerations are
185 important when reporting network studies on psychological data and are more closely related to
186 interpretation and validity of NA studies.

187 Among these reporting standards, five were selected to assess the methodological quality of
188 the studies included in this review. Specifically, three were selected from the *General Analysis* section
189 (both from Methods and Results subsections): (a) *Variable selection procedure*, (b) *Network*
190 *estimation method*, (c) *Results of accuracy and stability checks*. The remaining fourth and fifth
191 reporting standards were taken from the “*What to watch out for*” box A and box B respectively and
192 include: (d) the need to control for *topological overlap* of variables included in the analyses, (e)
193 *presence and handling of missing data*.

194 The rationale that guided this selection was to include those NA reporting standards common to all
195 studies adopting the network approach and which represent the optimal balance between
196 methodological rigor, clinical relevance and accessibility to non-experts. Moreover, additional
197 criteria other than the five above were excluded because they evaluate aspects of study design or NA
198 methodology which are not necessarily shared across all NA studies. Therefore, they are more
199 focused on the quality assessment of the nature of the NA research question rather than on NA study
200 methodological robustness. More specifically, the reporting standards (a), (b), and (c) were selected

201 because of their importance in any NA study, irrespectively of the design or research question. Those
202 three criteria represent the basic foundation of applying network psychometrics and are the key
203 information around which the most prominent NA tutorial papers revolve (Epskamp et al., 2018a;
204 Hevey et al., 2018), as well as being present in recent guidelines on other network methodologies
205 (Blanchard et al., 2022). These three elements have to be provided for a network study to be
206 interpretable and methodologically acceptable. In particular:

207 a) *Variable selection procedure* informs on what the theoretical background is and the construct being
208 analyzed, as validity of network studies depends on the theoretical assumptions guiding the selection
209 of variables from which the network will be built;

210 b) *Network estimation method* indicates what the methodology (the estimation function) used to
211 recover the network structure was;

212 c) *Results of accuracy and stability checks* inform whether the results (*in particular in term of value*
213 *of the network stability coefficient*) can be generalized, as interpretability and generalizability of a
214 network is dependent upon its stability.

215 The reporting standards (d) and (e) were selected among the general considerations that
216 Burger and colleagues (2022) indicated more as interpretation guidelines than methodological
217 reporting standards. Similarly, to criteria (a), (b) and (c), these two aspects were selected because of
218 their relevance no matter the research question or the network analysis methodology applied, and by
219 virtue of the considerable influence they exert on the quality and validity of the results (Borsboom et
220 al., 2021). Failing to report criteria (d) and (e) may indeed potentially become a source of
221 misinterpretation of the obtained network structures and impact their clinical relevance of the results.
222 In particular, (d) topological overlap is the presence of overlapping variables, which measures at least
223 partially the same construct within a single network. Topological overlap constitutes an important
224 risk of bias in network studies, as it alters the network structure independently from the real data
225 observed. Information on whether steps were taken to remove or minimize the impact of topological
226 overlap, or if present to what extent it influences the network structure should be immediately
227 accessible to the reader. The declaration of (e) *presence and handling of missing data* is also important
228 because the presence and trends (i.e random vs. systematic) of missing data is a critical piece of
229 information, as systematic missingness can hint at study design issues and determine problematic
230 inferences with significant impact on the conclusions drawn from the NA study results.

231 For each of the 5 reporting standards specific criteria were extracted. For details see Table 2.
232 Depending on how many of the 5 reporting standards were included in each study, an overall quality
233 methodology score was assigned. Specifically, each study was defined as either strictly adherent
234 (satisfying 5 out of 5 criteria), moderately adherent (satisfying 4 out of 5 criteria), sufficiently adherent
235 (satisfying 3 out of 5 criteria) or poorly adherent (satisfying 2 or less out of the 5 criteria). Data on
236 methodological quality extraction was performed independently by two of the authors (G.T. and
237 M.F.P.). When disagreements between the two authors occurred, multiple rounds of full-text revision
238 and discussions would be done until consensus was reached, with the involvement of the third author
239 (E.T.) if necessary. During the actual process of assigning methodological quality scores, no
240 disagreement arose between the authors (G.T., M.F.P., E.T.), as each criterion was selected
241 considering whether its presence can be objectively verified. The presence of each of the five criteria
242 in a scientific work can be easily assessed by reading the work itself, leaving little to no room for
243 ambiguity: either a procedure is reported, thus meeting the quality criteria, or it is not. Data on
244 methodological quality extraction and total score assigned to individual studies are also reported in
245 full in Table 3.

246 **RESULTS**

247 **Selection of articles and study characteristics**

248 The search yielded 107 records after removal of duplicates. The abstract screening led to the
249 exclusion of 39 articles. The full text of the remaining 65 articles and 3 dissertations were assessed,
250 leading to the exclusion of an additional 35 articles. The remaining 33 articles were thus included in
251 the review (Brown et al., 2020; Calugi et al., 2020, 2021, 2022; Cascino et al., 2019; Chen et al.,
252 2022; de Vos et al., 2021; DuBois et al., 2017; Elliott et al., 2020; Forrest et al., 2018, 2019;
253 Goldschmidt et al., 2018; Hagan et al., 2021; Hilbert et al., 2020; Kerr-Gaffney et al 2020; Levinson
254 et al., 2017; Mares et al., 2022; Meier et al., 2020; Monteleone et al., 2019, 2020, 2021, 2022; Olatunji
255 et al., 2018; Ralph-Nearman et al., 2021; Schlegl et al., 2021; Smith et al., 2019; Smith et al., 2020;
256 Solmi et al., 2018, 2019; Vanzhula et al., 2019; Vervaeet et al., 2021; Wang et al., 2019; Wong et al.,
257 2021). See figure 1 for the flowchart of the selection process, including reasons for exclusion at each
258 step.

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Figure 1. Selection of studies: PRISMA flowchart.

263 **Socio-demographic information**

264 Twenty-one (63.7%) studies included a mixed age sample (12-67 years). Seven (21.2%)
265 studies examined exclusively adults and four (12.1%) studies examined exclusively
266 children/adolescents. One (3.0%) study did not specify age range of the sample (Chen et al., 2022),
267 thus it is not possible to isolate specific age groups (children, adolescents, adults) involved in the
268 study. For more details, see Table 3.

269 No study explicitly reported considering differences between sex assigned at birth and gender.
270 The majority of studies (n=19; 57.6%) did not use any specific terminology hinting at whether sex or
271 gender was considered, whereas 4 (12.1%) studies used the term “sex” and 10 (30.3%) used the term
272 “gender”. Regarding the range of options available to respondents for gender/sexual identity, only 3
273 studies (Hagan et al., 2021; Meier et al., 2020; Wong et al., 2021) reported including a nonbinary
274 option. Nine (27.3%) provided only two options, “female” and “male”, while in 18 studies (54.5%)
275 the range of alternatives provided was not presented, as “female” was an inclusion criterion for the
276 study or only the percentage of female participants was reported. The remaining 3 studies did not
277 include information regarding sex/gender. The majority of studies (N=22; 66.7%) reported including
278 mixed samples, with a high majority of females (67% - 97.8%), while six (18.2%) studies included
279 female participants only. Five (15.1%) studies did not report the male:female ratio of participants.
280 For details and references, please see Table 3 and Table 4.

281 Fourteen (42.4%) of the 33 studies included reported race composition of the study sample,
282 all examining predominantly white/Caucasian individuals (range: 69.4% - 96%). The remaining 19
283 (57.6%) studies did not include information on race composition of their study sample. More details
284 on race reporting are available in Table 3 and Table 4.

285 The socio-economic status of participants was reported by 11 studies (33.3%). The most
286 frequent indicator used was educational level, in terms of degree obtained (3%) or years of education
287 (7%), followed by employment status, reported by one study (3%). Of the eleven studies reporting
288 socio-economic status, three (9%) included both educational level and employment status. For details,
289 see Table 4.

290

291 **Sample size**

292 Ten (30.3%) of the studies included a sample of more than 600 participants. Twelve (36.4%)
293 studies included between 200 and 600 participants, while eleven studies (33.3%) built networks from
294 less than 200 participants. For details and references, please see Table 3.

296 Diagnostic distribution of the samples

297 The selected studies have been divided by diagnostic group from which networks were built.
298 Studies building different networks for different diagnostic categories are thus reported multiple
299 times, depending on how many diagnosis-specific networks were included in the article. Eighteen
300 (54.5%) of the 33 studies included built networks from participants with AN. Eight studies (24.2%)
301 built networks from participants with BN. Five studies (16.1%) built networks from participants with
302 BED. Two studies conducted networks from participants with Other Specified Feeding or Eating-
303 Disorder (OSFED). Two (6.4%) studies ran networks from a mixed population with AN or BN.
304 Finally, fourteen studies (42.4%) built networks from a mixed eating disorder clinical population
305 sample, from here referred as transdiagnostic sample (TD). See Table 3 for details.
306

307 Study design

308 Among the 33 reviewed studies, six studies (18.2%) modelled cross-sectional networks from
309 data obtained at different time points (i.e. pre- and post- treatment) and compared them, therefore
310 adopting a longitudinal study design (Brown et al., 2020; Calugi et al., 2022; Elliott et al., 2020;
311 Mares et al., 2022; Monteleone et al., 2022; Smith et al., 2019). The remaining 27 studies (81.8%)
312 applied a cross-sectional study design (Calugi et al., 2020, 2021; Cascino et al., 2019; Chen et al.,
313 2022; de Vos et al., 2021; DuBois et al., 2017; Forrest, et al., 2018, 2019; Goldschmidt et al., 2018;
314 Hagan et al., 2021; Hilbert et al., 2020; Kerr-Gaffney et al., 2020; Levinson et al., 2017; Meier et al.,
315 2020; Monteleone et al., 2019, 2020, 2021; Olatunji et al., 2018; Ralph-Nearman, et al., 2021; Schlegl
316 et al., 2021; Smith et al., 2020; Solmi et al., 2018, 2019; Vanzhula et al., 2019; Vervaet et al., 2021;
317 Wang et al., 2019; Wong et al., 2021). Twenty-five studies (75.7%) used network analysis as the
318 primary data analysis procedure, involved treatment seeking patients and were uncontrolled (Brown
319 et al., 2020; Calugi et al., 2020, 2021, 2022; Cascino et al., 2019; Chen et al., 2022; de Vos et al.,
320 2021; DuBois et al., 2017; Forrest et al., 2018, 2019; Goldschmidt et al., 2018; Kerr-Gaffney et al.,
321 2020; Mares et al., 2022; Meier et al., 2020; Monteleone et al., 2019; Monteleone et al., 2020, 2021;
322 Olatunji et al., 2018; Schlegl et al., 2021; Solmi et al., 2018, 2019; Vanzhula et al., 2019; Vervaet et
323 al., 2021; Wang et al., 2019; Wong et al., 2021). Results from the remaining eight (24.3%) of the 33
324 studies included in this review are based on secondary analyses of data (Elliott et al., 2020; Hagan et
325 al., 2021; Hilbert et al., 2020; Levinson et al., 2017; Monteleone et al., 2022; Ralph-Nearman et al.,
326 2021; Smith et al., 2019; Smith et al., 2020). For further details about the design of the studies,
327 including the instruments used and whether network nodes were obtained by psychometric
328 questionnaire subscales scores or single items, please see Table 3.

329

330 **Assessment of methodological quality**

331

332 **Methodological quality total score**

333 Nine studies (27.3%) were found to strictly adhere to all 5 criteria, 9 studies (27.3%) were moderately
334 adherent (satisfying 4 out of 5 of the criteria), 12 studies (36.3%) were sufficiently adherent (satisfying
335 3 out of the 5 criteria), and 3 studies (9.1%) poorly adherent (satisfying 2 out of the 5 criteria). See
336 Table 3 for references.

337 In the 9 studies which were moderately adherent, the main unsatisfied criteria were not reporting for
338 topological overlap control procedures (N=7; 21.2%), and not mentioning presence or exclusion from
339 analyses of missing data (N=2; 6.1%). In those 12 studies found to satisfy the sufficient
340 methodological quality criteria (score: 3/5), in all but one case the studies did not report presence and
341 handling of missing data, and controlling for topological overlap. In only one case, a score of
342 sufficient methodological quality was assigned on the basis of missing specific CS value and not
343 evaluating topological overlap using standardized methods.

344 The three studies (9.1%) considered poorly adherent reported information on variable-selection
345 procedure and a general evaluation of the stability, but either did not mention the exact CS values or
346 did not include the specific network estimation method used. See Table 3 for references.

347

348 **(a) Variable selection procedure**

349 All 33 articles included in this work provided an explanation based on data from scientific
350 literature or theoretical models in support for the rationale guiding selection of the nodes included in
351 the networks prior to performing data analysis.

352

353 **(b) Network estimation function**

354 Thirty-one out of 33 (94%) studies reported applying network estimation functions which
355 included glasso regularization (Friedman et al., 2008). Of these, 15 studies (45.4%) manually tuned
356 the regularization/penalty applied to correlations (λ value) (Elliott et al., 2020; Forrest et al., 2018,
357 2019; Hagan et al., 2021; Hilbert et al., 2020; Kerr-Gaffney et al., 2020; Levinson et al., 2017;
358 Olatunji et al., 2018; Ralph-Nearman et al., 2021; Schlegl et al., 2021; Smith et al., 2019, 2020;
359 Vanzhula et al., 2019; Vervae et al., 2021; Wang et al., 2019; Wong et al., 2021), while the remaining
360 16 (48.5%) derived the λ value using the Extended Bayesian Information Criterion glasso
361 (EBICglasso; Chen & Chen 2005) (Brown et al., 2020; Calugi et al., 2020, 2021, 2022; Chen et al.,
362 2022; de Vos et al., 2021; DuBois et al., 2017; Cascino et al., 2019; Goldschmidt et al., 2018, Meier

363 et al., 2020; Monteleone et al., 2019, 2020, 2021; Solmi et al., 2018, 2019). None of the studies
364 included provided the rationale for deciding whether to apply glasso or EBICglasso or why it was
365 considered necessary to apply a regularization technique. One (3%) article (Mares et al., 2022)
366 derived the network structure from the data without regularization, by estimating a set of regularized
367 networks and subsequently fitting un-regularized networks for each of these models, and finally
368 performing a model selection procedure. Finally, one article (3%) did not report the specific network
369 estimation function applied to retrieve the network model (Monteleone et al., 2022).

370

371 **(c) Network stability coefficient (CS)**

372 In this section we considered the individual CS of networks built for each ED clinical
373 population in each study. As such, a study may appear multiple times if they developed multiple
374 networks within the same study (i.e. baseline and one or more follow-ups, or groups comparison). To
375 avoid confusion, please note that the following percentages of studies are referred to their respective
376 diagnostic categories (AN, BN, BED, OSFED, TD). Concerning CS, we used as reference values
377 those proposed by Epskamp and colleagues (2018) (acceptable CS $\geq .25$; good CS $\geq .5$; excellent CS
378 $\geq .7$).

379 In the AN networks included (N= 23), all the networks considered the stability coefficient following
380 the guidelines proposed by Epskamp and colleagues (2018), however one (Goldschmidt et al., 2018)
381 did not provide the specific value of the CS. Nine networks (39.1%) showed excellent CS coefficient
382 (≥ 0.7 ; range: 0.7 - 0.81), nine networks (39.1 %) showed good CS coefficients (≥ 0.5 ; range: 0.52 –
383 0.67), and four networks (17.4%) showed acceptable CS coefficient values (>0.25 ; range: 0.28 –
384 0.44). In one study (4.4%), authors reported excellent CS coefficient but did not include the specific
385 values.

386 Concerning CS of the eight BN networks considered, only one study (12.5%) reported a CS
387 coefficient value reaching the excellence (≥ 0.7), threshold 0.81). Three studies (37.5%) reported
388 good CS coefficient (≥ 0.5 ; range = 0.59 – 0.67), two studies (25.0%) reported adequate (≥ 0.25)
389 stability coefficient values (0.28; 0.29). Two studies (25.0%) did report good CS coefficient values
390 but did not include the specific values.

391 Concerning CS of 6 BED networks considered, two studies (33.3%) reported excellent (≥ 0.7) CS
392 values (range: 0.74-0.75). Three networks (50.0%) showed good (≥ 0.5 ; range: 0.59- 0.67) CS
393 coefficient values. One network (16.7%) showed acceptable CS coefficient (0.28).

394 Concerning CS of OSFED networks, one study (50%) reported good stability (CS=.52), while another
395 study (50%) did not present the specific CS coefficient of the network, though it is reported by the
396 authors as good.

397 Concerning CS of the 15 TD networks considered, six studies (40%) reported excellent (≥ 0.7) CS
398 coefficient values (range: 0.75 – 0.75), five studies (33.4%) reported good (≥ 0.5) CS coefficient
399 values (range: 0.52 – 0.67) and two (13.3%) reported acceptable (≥ 0.25) CS coefficient value (0.43-
400 0.44). The two remaining studies (14.3%) did not report specific CS values. Individual CS coefficient
401 for each study is available in Table 3.

402

403 **(d) Topological overlap**

404

405 Of the 33 studies included in this work, 12 (36.4%) explicitly mentioned checking for
406 potential topological overlap of nodes and addressing it prior to modelling the network. The
407 remaining 21 studies (63.6%) out the 33 included did not report checking for potential topological
408 overlap of nodes or addressing it prior to modelling the network. For references, please see Table 3.
409 Among the 12 studies which conducted checks for topological overlap, eleven (91.7%) did so using
410 the goldbricker algorithm. The remaining study (8.3%) (Goldschmidt et al., 2018) did not explicitly
411 mention applying the goldbricker algorithm, but did report collapsing items assessing importance of
412 weight and importance of shape in individual nodes.

413

414 **(e) Missing data management**

415 Among the 33 reviewed articles, 17 (51.5%) reported missing data. Fifteen of these (88.2%)
416 also discussed how missing data was managed (imputation or exclusion) (Brown et al., 2020; Calugi
417 et al., 2020; Elliott et al., 2020; Forrest et al., 2018, 2019; Hagan et al., 2021; Hilbert et al., 2020;
418 Meier et al., 2020; Schlegl et al., 2021; Ralph-Nearman et al., 2021; Smith et al., 2020; Smith et al.,
419 2019; Vanzhula et al., 2019; Vervaet et al., 2021; Wong et al., 2021). The remaining 16 articles
420 (48.5%) did not mention either presence nor absence of missing data in their studies. See Table 3.

421

422 **Discussions**

423

424 The aim of the present systematic review was to assess, for the first time, the methodological
425 quality of existing literature on NA studies in clinical populations with ED. To achieve this goal, we
426 proposed a customized quality assessment checklist based on the best practices for reporting standards
427 in cross-sectional NA studies developed by Burger and colleagues (2022). In a relatively novel and
428 emerging field such as NA, clinical professionals outside academia might not be familiar with the
429 methods and interpretation of psychopathological networks. It is therefore important to provide an
430 accessible yet relevant framework with which an untrained audience (clinicians and researchers alike)
431 might interpret the data on psychological network studies in ED, and evaluate their methodological
432 quality and potential validity for their clinical and research practice.

433 Considering the five selected methodological quality criteria, more than half of the studies included
434 were strictly or moderately adherent to them while the remaining studies were found to report them
435 sufficiently or, in a minor part, poorly. Therefore, in the wake of the rapid multiplication of studies
436 applying NA, it is encouraging that almost all of the studies using NA in ED and reviewed here
437 reported those basic criteria which are considered the minimum amount of data that must necessarily
438 be provided for a network study to be both interpretable and be able to obtain reliable results. The
439 minimum data include: reporting the theoretical rationale that guided the nodes selection, the network
440 estimation method used and consider the magnitude of network stability coefficients. Nonetheless,
441 when looking in more detail at how these criteria were reported, there are some aspects of
442 implementation that require further consideration. Moreover, an even greater careful examination
443 should be conducted of the other selected aspects of methodological quality (control for topological
444 overlap and handling for missing data) that were found to not be considered by as much as half of the
445 included NA studies on ED, at the detriment of interpretability and clinical applicability of their
446 results. This represents a significant obstacle that must be overcome to translate NA studies on ED in
447 meaningful data for clinical practice as other authors previously reported in NA studies in general
448 (McNally, 2021).

449 Starting with the criterion of (a) variable selection procedure, all studies reported the rationale
450 for selecting which variables to include in the network and provided a theoretical background of
451 reference to interpret the results. However, the wide breadth of psychometric instruments applied in
452 the NA studies reviewed makes it difficult to pool and interpret their results and gather uniform
453 evidence to inform clinical practice. Specifically, eight different tools were employed across 33
454 studies to assess ED symptomatology alone. Moreover, 10 studies assessed depressive
455 symptomatology using five different instruments, and five studies assessed anxiety using five
456 different instruments. As discussed by Newson and colleagues (2020), heterogeneity of assessment
457 tools significantly and negatively affects the translatability of study results to clinical practice, as the
458 observed results may be affected by bias implicit in the assessment tool of choice. Additionally,
459 roughly half of the studies included selected single instrument scale items as a variable of choice,
460 while the other half opted for instrument sub-scale scores. Unless supported by robust theoretical and
461 methodological choices, the use of single items as nodes might not be optimal when considering that
462 instrument sub-scales and scales are usually controlled for their discriminant and construct validity,
463 while individual items not always undergo this kind of examination. Ultimately, the choice of single
464 items rather than sub-scales represents a further source of risk to the validity and generalizability of
465 NA studies considered. Therefore, future NA studies in the field should provide clear methodological
466 statements able to offer an explanation of their choice of single items or of sub-scales use as nodes.

467

468 Looking at (b) the reporting of network estimation methods (or functions), all but one study
469 reported the applied specific network estimation function. Most studies, reported employing
470 regularized network estimation functions by adopting glasso applications to stabilize the networks.
471 Regularization or thresholding techniques such as glasso are used to improve network stability in
472 specific situations, such as when the sample size is too small for the number of nodes included
473 (Epskamp et al., 2018a). In the studies included in this review, however, the application of
474 regularization functions (i.e.glasso) to retrieve the network model appeared independent of the sample
475 size, and was applied even when the study samples might have allowed to not use it. It is important
476 to note that the indiscriminate use of regularization techniques even when unnecessary may be
477 problematic for a number of reasons (Williams et al.,2019; Wysocki & Rhemtulla, 2021). Among
478 these, the glasso regularization techniques employ a regularizing penalty by which only a relatively
479 small number of strong edges are included in the network, with effects in terms of high sensitivity
480 but lower specificity (Burger et al., 2022). This means that the weaker edges included in the estimated
481 network may be more prone to be false positives (i.e., Type I errors). Consequently, regularization
482 techniques might remove weaker but true positive edges from the network model with a potential loss
483 of information regarding the network structure. A careful interpretation of the remaining edges and
484 the study results in general is therefore necessary.

485 As discussed above, the decision to employ a specific network estimation function is closely
486 related to the topic of the sample size. However, no specific reporting standards regarding sample
487 size were included in the present quality assessment because there are no definitive guidelines in the
488 first place on how to estimate the appropriate number of participants to return a stable network in
489 relation to the variables included (Epskamp et al., 2018b; Blanken et al., 2022). A potential optimal
490 solution as observed by Epskamp and colleagues (2018) and Blanken and colleagues (2022), is
491 increasing sample size to improve stability of a given network to a point where regularization
492 techniques might not be necessary. Preferably, nodes should be limited to a maximum of 30, while
493 sample size should be as large as possible (Blanken et al., 2022). Otherwise, another more technical
494 potential solution is to estimate the stability of a network at different sample sizes prior to data
495 collection by conducting pre-hoc simulations (Epskamp et al., 2018a). However, no study reviewed
496 here reported simulating their data for measuring the stability before starting data collection or data
497 analysis. Considering the scope and limitations of regularization techniques (Williams et al.,2019;
498 Wysocki & Rhemtulla, 2021), it would be advisable that future network studies discuss the rationale
499 supporting the selection of specific network estimation methods. This is especially relevant in light

500 of the widespread application of regularized network estimation methods even when the ratio between
501 network nodes and study participants would allow the application of non-regularized methods.

502

503 Looking at (c) the stability of the networks considered in this work, most NA networks of ED
504 clinical populations reported good to excellent network stability, while a few reached acceptable
505 stability only. Nonetheless, this high rate of stable networks observed among the studies reviewed
506 might be influenced by the almost ubiquitous application of regularization techniques as discussed
507 above. Additionally, it is also likely that publication bias effect is present in NA studies on ED, as
508 well as the tendency to only publish empirical studies that reject the null hypothesis (Ferguson &
509 Heene, 2012).

510

511 The methodological points that however emerged as the most neglected in this review on NA
512 studies on ED are pertaining the (d) lack of control of topological overlap and (e) handling of missing
513 data. Topological overlap was the most overlooked criterion, at the expense of study quality and
514 validity. Only 36.4% of the studies included reported checking for topological overlap and using the
515 goldbricker algorithm, currently considered the recommended procedure to test it (Monteleone and
516 Cascino, 2021). Not accounting for topological overlap may limit generalizability of the results,
517 particularly in relation to the data collection instrument used and the node selection criteria applied
518 (Christensen et al., 2020). This issue is further compounded by the wide range of assessment
519 instruments applied, already discussed above, which pose a challenge when one tries to compare
520 results across studies investigating the same construct utilizing different questionnaires.

521 The goldbricker algorithm is certainly not the only way to check for topological overlap. As
522 alternatives, the identification of one or more overlapping items or sub-scales can be informed by the
523 psychometric properties of the scale (i.e. whether multiple items or scales deliberately measure the
524 same phenomenon) or by the decision process conducted by experienced clinicians or researchers.
525 While the former of these two alternative methods is somewhat more reliable, there is still a margin
526 of error with both approaches. Therefore, we considered the application of goldbricker algorithm as
527 the only reliable procedure to check for topological overlap, as it represents a standardized and
528 consistent method. While it might be argued that goldbricker algorithm was introduced only in 2017
529 (Jones, 2017) and might have not been immediately known to the scientific community, 91% of the
530 NA studies on ED here reviewed that did not applied *goldbricker* were published when the R function
531 and its package (Jones, 2017) were already freely available online. Thus, it would be advisable that
532 all future NA studies on ED apply goldbricker algorithm as a way to assess for topological overlap,
533 and that readers consider whether it is present when interpreting NA study results.

534

535 The other critical issue in many of the reviewed NA studies on samples of patients with ED
536 is the absence of missing data reporting. Reporting missing data management in network studies is
537 extremely important. As explained by Burger and colleagues (2022), accurate inferences are
538 dependent upon a thorough understanding of the dataset composition and causes of missingness;
539 missingness mechanisms need to be elucidated to draw appropriate conclusions from the results. For
540 example, some data might be systematically missing because of specific characteristics of the
541 assessment instrument (i.e. skip question structure of an instrument, such as SCID-5). Therefore, as
542 specific information might be included for some participants but missing for others (i.e. skip question
543 not relevant for a specific case), cases might be erroneously classified as missing. As such,
544 information might not be included in the network structure or would be erroneously classified. This
545 consideration makes reporting missing data a key piece of information not only in network studies,
546 but in psychology research in general. On other hand, to the best of our knowledge there are no
547 specific indications on how much missing data should be considered unacceptable in network studies
548 and, moreover, network estimation function methodology automatically produces a network despite
549 the presence of even large portions of missing data (Burger et al., 2022). This determines a grey area
550 for missingness handling in network models, leaving researchers with a number of options and no
551 gold-standard of how to proceed. Considering the lack of general consensus on how to handle missing
552 data in NA studies, it is even more important to report as much information as possible about missing
553 data to allow correct interpretation of the results.

554

555 From this systematic review one can observe how the methodological quality of NA studies
556 on ED patients tended to improve over time, with more recent studies usually meeting more of the
557 criteria considered here compared to early NA studies. This is likely due to the novelty of this
558 approach, the rapid methodological advancements and the dissemination of guidelines and primers
559 (Epskamp et al., 2018; Hevey, 2018); older works, which represent roughly one fifth (21.2%) of the
560 articles included in this review, might not have had some of the tools or guidelines available to report
561 information we now consider pivotal in evaluating a network study. However, there is still a number
562 of studies which have been published as recently as 2022 that do not show the same degree of
563 adherence to our selection of best practices of reporting data in NA studies on ED when compared
564 with contemporaneous publications on the topic. All of the 24 studies (72.7%%) that received a
565 methodological quality score of 4 or lower either failed to include controls for topological overlap or
566 did not report missing data, or both. Of these, only the works of Levinson and colleagues (2017) and
567 DuBois and colleagues (2017) were published before guidelines and tutorials on these two aspects

568 became available in the literature. The remaining 22 studies (66.6%) did not include either discussion
569 regarding missing data and/or performed checks for topological overlap of variables despite
570 documentation about both topics being available in the literature (Fried & Cramer, 2017; Jones,
571 2017). This would seem to suggest that lack of availability of materials and guidelines might not
572 entirely explain some critical aspects of the studies' methodology, considering the most commonly
573 neglected aspects were already being discussed, and solutions were being proposed (Epskamp et al.,
574 2018; Fried & Cramer, 2017; Jones et al., 2018)

575

576 Further and conclusive comments are related to the general characteristics of the NA studies
577 considered. Study populations were composed mostly, when not exclusively, of females, either adults
578 or adolescents or both. ED have been historically perceived as “female” disorders, and the majority
579 of research on ED diagnosis and treatment has relied predominantly on female samples (Murray et
580 al., 2016). Therefore, it is possible that male-specific ED manifestations might have been overlooked,
581 not recognized (Murray et al., 2017) and not included in NA networks yet. Additionally, more than
582 half the studies did not report information regarding differences between sex assigned at birth and
583 gender. Lack of this differentiation further compounds issues regarding identification of ED in
584 individuals who identify with minoritized groups and limits the applicability of studies' results, as
585 gender minorities present with unique clinical profiles and concerns (Nagata et al., 2020). Less than
586 half the studies included data regarding years of education, a strong indicator of income and wealth
587 (Psacharopoulos & Patrinos, 2018), family income or employment status. Lack of data on these
588 characteristics is a missed chance to further understand how ED might present differently in different
589 socioeconomic groups. Issues regarding diversity are also present in terms of racial diversity. Less
590 than half of the NA studies reviewed here reported the actual racial/ethnic composition of the study
591 sample, and even when present, all studies recruited at least 60% of white/Caucasian participants.
592 Recognition of ED in different socioeconomic and racial/ethnic composition populations is a key
593 aspect to improve treatment of these disorders, given the disparities in ED treatment caused by
594 stereotypes surrounding the typical ED patient – *skinny, white, affluent girls* (Sonneville & Lipson,
595 2018; Franko et al., 2007).

596

597 In terms of diagnoses, AN emerged as the most studied ED diagnostic group, immediately
598 followed by the broader TD population. Studies in BN, BED and OSFED patients were less frequently
599 investigated. This is similar to what was observed by Monteleone & Cascino in their recent review
600 (2021), where they report most studies including a TD sample, with AN being the second most studied
601 clinical population. The disparity in the number of studies investigating the various ED diagnostic

602 groups does not seem justified by epidemiological data, which indicate BED and OSFED as the most
603 common ED conditions (Santomauro et al., 2021).

604

605 In terms of study design, among the utilized network estimation functions to build cross-
606 sectional networks, a minority of the studies (18.2%) proceeded to model multiple cross-sectional
607 networks from different data collection time points, thus adopting cross-sectional networks within a
608 longitudinal study design. However, this is different from building longitudinal networks using
609 specific estimation methods such as the multilevel vector autoregression (mlVAR), with which cross-
610 sectional networks share only some characteristics (Blanchard et al., 2022). Cross-sectional networks
611 are by default undirected, meaning they can show the relations between two variables, but not the
612 direction of that relation (which variable activates or causes the other). As such, limited inferences
613 can be drawn from cross-sectional networks. Even considering these limitations, it is still advisable
614 that future studies choosing to employ cross-sectional networks try to implement them within a
615 longitudinal study design. As also discussed by Monteleone and Cascino (2021), studies adopting a
616 longitudinal design are needed to improve identification of clinically similar subgroups and study
617 changes in response to treatment to advance our understanding and our ability to treat ED.

618 Some important limitations concerning the methodology applied for this review however
619 might affect the results of this work. The selection process adopted for the quality assessment criteria
620 constitutes a major limit to the present work. Despite being based on the most cited and adopted NA
621 tutorials (Epskamp et al., 2018; Hevey et al., 2018), the process of selecting the criteria has been
622 conducted internally by a group of researchers, clinical psychologists and psychotherapists involved
623 in this work and thus may be biased. However, the inclusion of only five among the various criteria
624 proposed by Burger and colleagues (2022) reflects a deliberate decision to include criteria evaluating
625 those specific NA methodological aspects shared by all network studies. The inclusion of additional
626 criteria would have probably created different scores assigned to specific studies by virtue of different
627 research questions rather than methodological robustness. Therefore, examining any of the remaining
628 criteria would not have unduly influenced the conclusions of our assessment on methodological
629 quality. An example is the inclusion of *predictability* of a node as a criterion of methodological quality
630 assessment: NA studies might miss data on predictability because that is not part of their research
631 goals, rather than it being due to poor methodological adherence. In this scenario, the inclusion of a
632 *predictability* report would have contributed to the assignment of a score pertaining to aspects that
633 are not strictly related to methodology quality as much as research interests.

634 Furthermore, the search of the present review was limited to networks built using cross-
635 sectional modelling functions, and in clinical populations with an established diagnosis of ED only.

636 As a result, other relevant studies investigating ED symptomatology in non-clinical populations
637 and/or using other network modelling techniques have been certainly left out, which represents an
638 obvious loss of important information. However, restricting the focus of this work to a specific NA
639 methodology and clinical population provided a coherent framework for comparison of
640 methodological quality and considerations regarding validity and replicability across studies.

641

642 **Conclusions**

643

644 This systematic review on the methodological quality of NA cross-sectional studies in ED
645 confirms that the literature on this topic advanced considerably in a relatively short amount of time
646 with most studies reporting methodological procedures necessary for a network study to provide valid
647 and reliable results. Still, some critical issues in methodological quality emerged which support some
648 recommendations for future NA studies on ED. This is especially relevant considering that the present
649 work already applied a restricted and more accessible version of the checklist of best guidelines for
650 reporting data in NA studies proposed by Burger and colleagues (2022).

651 First of all, it would be advisable that future studies carry out simulations before sample recruitment
652 to estimate accurately the adequate sample size in relation to the number of variables selected, so that
653 ideally one might avoid the use of regularization techniques and thus reduce potential false negative
654 results. Moreover, it is important to minimize the heterogeneity of assessment scales to improve
655 translatability of study results into clinical practice.

656 More importantly, procedures closely related to study validity such as controls for topological
657 overlap and management of missing data, that still were absent in the majority of studies reviewed,
658 need to be included systematically in future NA studies on ED. The exclusion of such information
659 limits the generalizability and validity of the current available results, or needs to be accounted for in
660 their data interpretation to be really accessible for clinicians that must inform their practice with ED
661 patients.

662

663 Finally, our results support and complement the conclusions of previous work from Monteleone and
664 Cascino (2021) who suggest the need to increase the number of longitudinal studies to capture the
665 dynamics of ED symptomatology. Future studies should ideally adopt a longitudinal design more
666 frequently, either confronting multiple cross-sectional networks from different time points or building
667 temporal networks depending on their specific research question (Borsboom et al., 2021; Blanchard
668 et al., 2022). Network psychometrics hold remarkable potential in studying the dynamics that
669 underpin clinical manifestations, comorbidities and diagnostic migrations in ED, and we look forward
670 to more studies designed to adopt such an approach on longitudinal data on ED. Additionally, future

671 NA studies should be more focused on BED and OSFED populations as well as include more racially,
672 ethnically, socioeconomically diverse participants with particular attention to gender-balanced study
673 samples and representation of sexual minorities.

674

675 It should be noted that our attempt to provide a selection of accessible and clinically-relevant criteria
676 to evaluate NA ED studies is not exhaustive nor definitive, and represents but a step of an ongoing
677 process to increase popularity and accessibility of methodology of NA studies on ED. We strongly
678 advocate in favour of future studies reviewing and amending the criteria we proposed, to expand or
679 restrict the range of criteria considered depending on the specific type of study and research
680 methodology, to include other network modelling techniques and test their applicability on ED
681 clinical and non-clinical populations.

682

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685 editing this manuscript in her capacity as native English speaker.

686

687

688 **Public Significance statement**

689 The present work aims to evaluate the quality of ED NA studies to support applications of this
690 approach in ED research. Results show that most studies adopted basic procedures to produce reliable
691 results; however, other important procedures linked to NA study validity were mostly neglected.
692 Network methodology in ED is extremely promising, but future studies should consistently include
693 topological overlap control procedures and provide information on missing data.

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Table 1. PICOS criteria.

PICOS	Inclusion criteria	Exclusion criteria	Data extracted
Patient	<ul style="list-style-type: none"> - individuals with ED diagnosis according to DSM-IV-TR or DSM 5 - adults, adolescents, children or mixed - male, female or mixed 	<ul style="list-style-type: none"> - absence of ED diagnosis according to DSM-IV-TR or DSM 5 or mixed clinical/non-clinical samples - ED diagnosis based on self-report tools - obesity diagnosis - sample composed of medically ill patients with pathologies not related to EDs 	<ul style="list-style-type: none"> - age range, average age and standard deviation - female % - race - sample size - ED diagnosis and comorbidity
Intervention	<ul style="list-style-type: none"> - studies with or without therapeutic intervention 		
Comparison group	<ul style="list-style-type: none"> - studies with or without control groups 		
Outcome	<ul style="list-style-type: none"> - network analysis of specific ED symptoms (with or without other DSM symptoms and clinical variables related to EDs) 	<ul style="list-style-type: none"> - Association networks, Bayesian networks, idiographic networks, temporal networks - social network analysis - neural networks - gene networks - studies on blogs/forum 	<ul style="list-style-type: none"> - centrality indices - network stability assessment - bridge symptoms - network connectivity - prognostic value of network indices
Study design	<ul style="list-style-type: none"> - Prospective or Retrospective cohort, cross-sectional, case-control, or RCT - Any length of follow-up - cross-sectional network estimation method - English language 	<ul style="list-style-type: none"> - Reviews - Case studies - Opinion statements - Book - Meta-analysis - Progress report 	<ul style="list-style-type: none"> - study design - nodes selection criteria - type of network estimation - missing data management - topological overlap management

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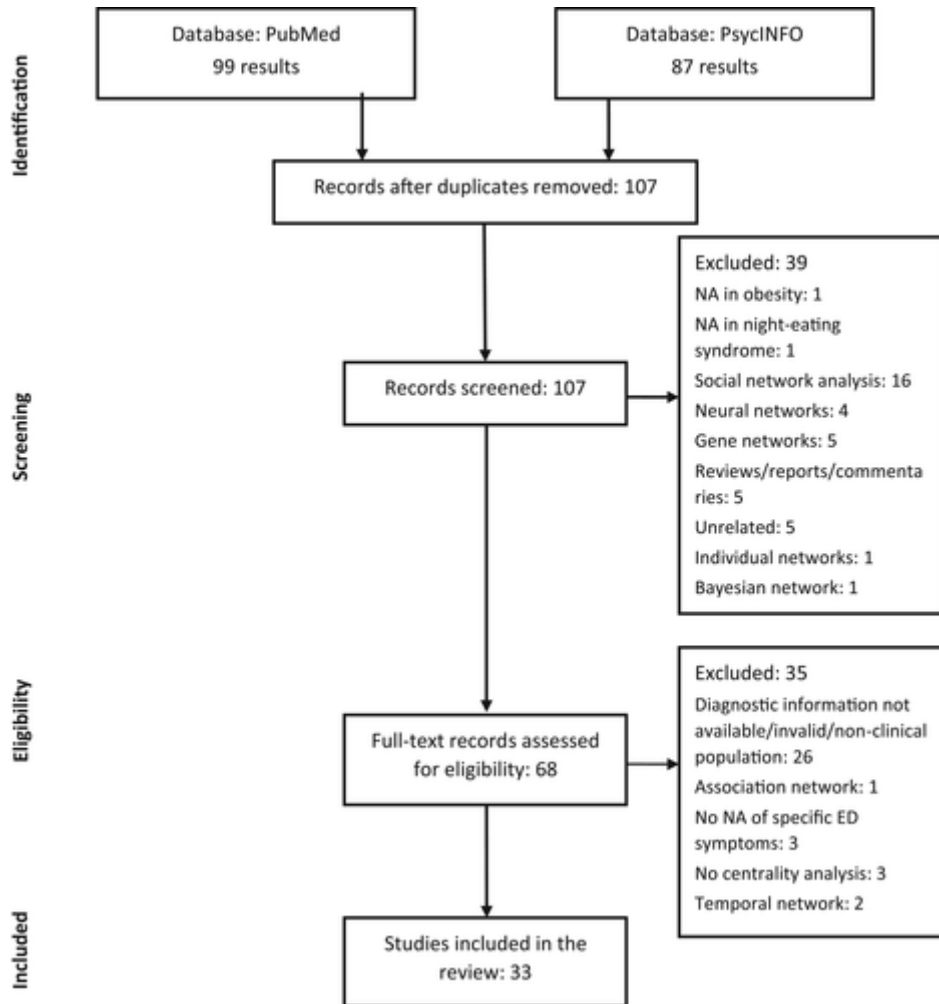
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Figure 1. Selection of studies: PRISMA flowchart.



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728 **APPENDIX**

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Table 2: Quality assessment criteria and evaluation tool

733 For each of the 5 selected reporting standards, check whether the following data were extracted from
734 each study included in this review. Sum the number of YES boxes ticked to obtain the total score,
735 then confront against the quality score interpretation guideline

736 (a) Was the theoretical background used as rationale for node selection process was provided?

737 Yes No

738 (b) Did the authors report the network estimation function was applied (i.e. Graphical least absolute
739 shrinkage and selection operator [graphical lasso, or glasso] or Extended Bayesian Information
740 Criteria glasso [EBICglasso] or mixed graphical models [mgm] or Isingfit)?

741 Yes No

742 (c) Was stability coefficient (CS) and its magnitude reported? (Acceptable $CS \geq 0.25$; good $CS \geq 0.5$;
743 excellent $CS \geq 0.7$);

744 Yes No

745 (d) Was risk of topological overlap assessed through the application of the *goldbricker* algorithm?

746 Yes No

747 (e) Was presence and handling of missing data discussed (i.e. whether missing data was present, in
748 which percentage, were missing data imputed and how)

749 Yes No

750

751 **Total criteria satisfied** /5

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756 **Score interpretation guidelines**

757 5/5: strictly adherent; 4/5: moderately adherent; 3/5: minimally adherent; 2/5: weakly adherent

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Table 4. Information on the demographic diversity of samples reported in the studies

ARTICLE	SEX vs GENDER TERMINOLOGY AND OPTIONS CONSIDERED	SES		RACE AND ETHNICITY	OTHER
		EDUCATION LEVEL	ECONOMIC PARAMETERS		
1. Brown et al., 2020	Sex <i>Options: unclear; only % of female respondents reported.</i>	N.R.	N.R.	Race Caucasian 74,5% Asian 6,1% African American 0,4% Native Hawaiian/Pacific Islander 0,2% American Indian/Alaska Native 0,7% Other 17,8% Ethnicity Hispanic 16,8% Non-Hispanic 83,1%” Reported also for adult and adolescent subgroups	
2. Calugi et al., 2020	Gender <i>Options: unclear; only % of female respondents reported.</i>	N.R.	N.R.	N/R	
3. Calugi et al., 2021	Gender <i>Options: unclear; only % of female respondents reported.</i>	N.R.	N.R.	N.R.	
4. Calugi et al., 2022	Gender <i>Options: unclear; only % of female respondents reported.</i>	N.R.	N.R.	N.R.	
5. Cascino et al., 2019	Gender <i>Options: Unspecified; female gender is an inclusion criterion</i>	N.R.	N.R.	N.R.	
6. Chen et al., 2022	N.R.	Years of education (patients group: 20.8±2.7; control group: 20.9±2.6)	N.R.	N.R.	
7. de Vos et al., 2021	N.R.	N.R.	N.R.	N.R.	
8. DuBois et al., 2017	Sex <i>Options: Male female</i>	High school graduate (or less) 24,7% College student (currently) 24,7%	N.R.	Race White 89,4% Asian 5,6%	Sexual orientation Heterosexual 82,5%

		College graduate 50,6%		African American/Black 4,6% Native Hawaiian/Pacific Islander 0,5% American Indian/Alaska Native 0%	Homosexual 8,8% Bisexual 5,6% Other 3,1%
9. Elliot et al., 2020	Gender <i>Options</i> Female Male	*Years of education 15,8±2,3 (data available for 125/142 participants)	N.R.	In the limitation section, it's acknowledged that the majority of the sample is composed by participants who are White, but no data are provided.	*In relationship 35,2% (data available for 138/142 participants)
* Information not reported in the article; the authors refer to the study by Schmidt et al., (2015) for the original data					
10. Forrest et al., 2018	Unspecified <i>Options:</i> Unspecified; "girls" and "women" are inclusion criteria	AN High school graduate or less 36,5% Technical training 1,0% Some college or Associate's degree 35,1% Bachelor's degree 16,4% Graduate school 10,8% BN High school graduate or less 27,7% Technical training 1,5% Some college or Associate's degree 41,7% Bachelor's degree 19,9% Graduate school 8,8%	N.R.	Race/ethnicity AN European American 86,6% African American 0,7% Asian/Pacific Islander 2,6% Hispanic 3,8% Native American 0,2% Multiracial 2,5% Other 1,7% Race/ethnicity BN European American 78,4% African American 2,7% Asian/Pacific Islander 2,3% Hispanic 6,3% Native American 0,6% Multiracial 4% Other 2,9%"	
11. Forrest et al., 2019	Gender <i>Options:</i> Boy/man Girl/woman	N.R.	N.R.	Race Caucasian 74,7% Asian 5,7% Black 2% Native Hawaiian/Pacific Islander 0,3% American Indian/Alaska Native 0,3% Other 16,6% Ethnicity Hispanic 19,9% Non-Hispanic 76%"	
12. Goldschmidt et al., 2018	Unspecified <i>Options:</i> Unspecified;	N.R.	N.R.	Race White 88,1% Black 6,8% Asian 2,7%	

	<i>only % of female respondents reported.</i>			American Indian/Alaskan Native 0,8% Mixed race/other 1,6%”	
13. Hagan et al., 2021	Child sex <i>Options:</i> Female Male other	N.R.	Family income: <US \$ 50000: 12% US \$ 50000-80000: 11,2% US \$ 81000-100000: 13,9% US \$ 101000-150000: 19,6% > US \$ 150000: 41,1%	Child race Caucasian 69,4% African American 0,7% Asian 12,5% American Indian/Alaska Native 0,2% Native Hawaiian/Pacific Islander 4,6% Multi-racial 7,6% Child ethnicity Hispanic/Latinx 12,0% Non-Hispanic/non-Latinx 69,4%”	Intact family Yes 60,6% No 18,1% Missing data 21,3%
14. Hilbert et al., 2020	*Unspecified <i>Options:</i> Female Male	*Years of education ≥ 12 Self-help group: 53% CBT group: 50%	*Employed Self-help group: 68,7% CBT group: 76,7%	N.R.	*Married Self-help group: 36,1% CBT group: 51,2%
*Information not reported in the article; the authors refer to the study by de Zwaan et al., (2017) for the original data					
15. Kerr-Gaffney et al., 2020	Unspecified <i>Options:</i> Unspecified, only % of female respondents reported.	Years of education 16,33±2,89 (range 10-27)	N.R.	N.R.	
16. Levinson et al., 2017	Unspecified <i>Options:</i> Unspecified, only % of female respondents reported.	N.R.	N.R.	Ancestries European 84,2% African American 5,6% Asian 2,6% Native Hawaiian/Pacific Islander 0,5% Other 5,1%”	
17. Mares et al., 2022	Gender <i>Options:</i> unclear; only % of female respondents reported.	N.R.	N.R.	N.R.	
18. Meier et al., 2020	Unspecified <i>Options:</i> Women 84,82 Men 14,85 Nonbinary 0,33	College degree 68% Other titles not reported	N.R.	“Caucasian (95%)” Other races not reported	
19. Monteleone, et al., 2019	Unspecified <i>Options:</i> Female Male	N.R.	N.R.	N.R.	Family type (united, separated, single parent)

20. Monteleone et al., 2020	Unspecified <i>Options:</i> <i>Unspecified;</i> <i>"Women" is an inclusion criterion</i>	Years of education <i>AN-R 16,05±2,89</i> <i>AN-P 16,06±2,53</i> <i>BN 15,56±3,38</i>	Months unemployed <i>AN-R 5,85±22,34</i> <i>AN-P 9,06±29,21</i> <i>BN 6,56±13,66</i>	N.R.	
21. Monteleone, et al., 2021	Unspecified <i>Options:</i> <i>Female</i> <i>Male</i>	N.R.	N.R.	"Italian population"	
22. Monteleone et al., 2022	Gender <i>Options:</i> <i>unclear; only % of female respondents reported.</i>	Years of education <i>Patients: 11.7±1.9</i> <i>Carers: 15.4±3.6</i>	Employment status <i>Employed (full time/part time)</i> <i>Patients: 8%</i> <i>Carers: 69%</i> <i>Unemployed/retired/sick leave/student</i> <i>Patients: 91%</i> <i>Carers: 21%</i> <i>Other/missing</i> <i>Patients: 0.7%</i> <i>Carers: 9%</i>	N.R.	
23. Olatunji et al., 2018	Unspecified <i>Options:</i> <i>Unspecified,</i> <i>"female" is an inclusion criterion</i>	N.R.	N.R.	Ethnic breakdown Caucasian 93,6% Mixed/Unknown 2,7% Hispanic 2,1% Asian 0,9% African American 0,7% Native American 0,2%"	
24. Ralph-Nearman et al., 2021	Sex <i>Options:</i> <i>Male</i> <i>Female</i>	N.R.	N.R.	Race/ethnicity White 86.5% Hispanic 4.1% Asian 1.2% Black 0.7% Multi-racial 2.2% Not reported 5.2%	
25. Schlegl et al., 2021	Unspecified <i>Options:</i> <i>Unspecified;</i> <i>"female" is an inclusion criterion</i>	N.R.	N.R.	N.R.	
26. Smith et al., 2019	Unspecified <i>Options:</i> <i>Unspecified,</i> <i>only % of female respondents reported.</i>	N.R.	N.R.	"Caucasian (96%)" Other races not reported	
27. Smith et al., 2020	Gender <i>Options:</i> <i>Male</i> <i>Female</i>	N.R.	N.R.	Race/ethnicity (ED group) White 90,7% Asian/Pacific Islander 1,7% Black 1,26% Native American/Alaskan Native 0,8% Hispanic 0,8% Other 0,8% Non reported 0,4%"	

				Data reported also for the two comparison groups.	
28. Solmi et al., 2018	Unspecified <i>Options: Unspecified, only % of female respondents reported.</i>	N.R.	N.R.	N.R.	
29. Solmi et al., 2019	N.R.	Years of education AN-B-P 13,04±2,90 AN-R 12,73±3,12	N.R.	N/R	
30. Vanzhula et al., 2019	Unspecified <i>Options: Unspecified, only % of female respondents reported.</i>	N.R.	N.R.	Ethnicity European American 93,7% African American 0,6% Hispanic 1,9% Asian 0,6% Multiracial 1,9%	
31. Vervaet et al., 2021	Unspecified <i>Options: Female male</i>	N.R.	N.R.	N/R	
32. Wang et al., 2019	Unspecified <i>Options: Unspecified, only % of female respondents reported.</i>	N.R.	N.R.	Race White 73,8% Other races not reported	
33. Wong et al., 2021	Gender <i>Options: Male Female Other</i>	N.R.	N.R.	Ethnicity/ race European American 75.1% Hispanic 4.8% Black 1.3% Chinese or Chinese American 0.9% Indian or Indian-American 0.4% Japanese or Japanese American 0.4% Other Asian origin 0.4% Multiracial or biracial 2.2% Unlisted 0.9% Did not report 13.5%	

765 N.R.: not reported

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