Abstract

OBJECTIVE: Functional analytic psychotherapy (FAP) is an approach based on contextual behavioral principles with a focus on interpersonal issues. This review quantitatively synthesized the evidence on the efficacy of FAP in addressing various clinical outcomes.

METHOD: A systematic literature search was conducted. After study selection, coding, and data extraction, outcomes were meta-analyzed using a random-effects model. Subgroup analyses, meta-regressions, and bias assessments were performed.

RESULTS: Seventy studies met the inclusion criteria. Results from single-case designs showed significant improvements in idiographic clinically relevant behaviors (i.e., avoidance of intimacy and conflict vs. engaging in intimacy-promoting behaviors, assertive expression of feelings and needs). Controlled group studies showed robust results for depression and anxiety symptoms. No significant changes were found for anger, experiential avoidance, and mindfulness.

CONCLUSIONS: Thus, our results provide empirical support for FAP in the treatment of interpersonal behaviors and internalizing symptoms, although bias in the included studies may limit these findings.

Keywords: Functional analytic psychotherapy; Contextual behavioral science; Interpersonal problems; Internalizing symptoms

Public health significance statements

- Functional Analytic Psychotherapy (FAP) aims to change idiographically defined clinically relevant behaviors through therapist-client interaction during the sessions. Because these behaviors are often functionally similar to out-of-session behaviors, the change that occurs in sessions can be generalized to other similar situations in daily life.
- Our meta-analyses of single-case studies suggest that FAP shows promise in the treatment of idiographic defined interpersonal behaviors. In addition, meta-analyses of group studies

suggest that FAP leads to significant improvements in anxiety and depression compared to controls, but not in mindfulness or anger.

- These findings may empirically support the use of FAP in clinical practice to address individual interpersonal problems and related emotional symptoms. However, these findings must be considered preliminary due to potential methodological limitations in the reviewed studies.

Meta-analysis of randomized controlled trials and single-case designs on the efficacy of functional analytic psychotherapy

Functional analytic psychotherapy (FAP) is a psychotherapeutic approach based on contextual behaviorism in which clinical outcomes are achieved through verbal shaping during client-therapist verbal contingent interactions (Kohlenberg & Tsai, 1991; Pardo-Cebrian et al., 2022). The rationale behind FAP is to understand human suffering according to the principles of behavior analysis, such as positive and negative reinforcement, escape, and avoidance. Therefore, in the context of a FAP intervention, the therapist's behavior serves as a catalyst for improvements in the client's behavior.

FAP operates under the premise that many forms of human suffering and psychological disturbances stem from interpersonal problems. Positive social interactions can improve health and well-being (Raymo & Wang, 2022) and longer life expectancy (Bhatia et al., 2023). In contrast, unsatisfactory social relationships can lead to suffering (Cacioppo et al., 2006; Holt-Lunstad & Smith, 2012). Hence, client difficulties may arise in interpersonal relationships due to the inseparable dyadic nature of human beings and their interactions with the world. To truly understand individuals, it is necessary to understand their *Dasein*, a concept that involves recognizing them in the broader context of their values by exploring their lifeworld (LeJeune & Luoma, 2019). Psychotherapy is considered to be an intermediary institution between the culture and the individual, which facilitates the reconciliation of the interpersonal and intrapersonal spaces (Hersch, 2015). The improvements that occur through human interaction, guided by the principles of behavior, allow for the dissection of the functional anatomy of that interaction.

Thus, the therapeutic relationship provides an opportunity and a responsibility to achieve positive clinical outcomes, particularly when clients' psychological problems involve difficulties in social interactions. FAP operates on the principle that the social environment of a therapeutic relationship should be functionally equivalent to other social interactions in the client's life. Therefore, in FAP, the therapist can elicit interpersonal behaviors from the client within the

session that are similar to those that occur outside of the session. The therapist can then naturally reinforce the improved behaviors and extinguish the problematic behaviors during the therapy session. Thus, the therapeutic relationship functions as a vehicle for the change proposed by FAP (Kohlenberg & Tsai, 1991). FAP distinguishes between different clinically relevant behaviors (CRB). CRB1s refer to actual in-session instances of problematic behavior, CRB2s are in-session improvements in that maladaptive behavior, and CRB3s are considered observations and descriptions of the elicitors and reinforcers stimuli of the client's behavior. CRBs are shaped by natural contingencies within the session to generalize the change to everyday life, bringing the behavior closer to its consequences to maximize the effect of therapy (Kohlenberg & Tsai, 1994). CRB3s are especially important for FAP because it helps with the out-session generalization of CRB2s (Tsai et al., 2009). In addition, some FAP researchers have referred to out-session behaviors that are functionally and sometimes even topographically similar to CRBs as O1s (for outside problems) and O2s (for outside improvements) (Callaghan et al., 2003).

FAP guidelines are distilled into five rules that should be flexible to accommodate individual differences. FAP therapists are encouraged to observe CRBs as they occur in session (Rule 1); to evoke CRBs (Rule 2); to naturally reinforce CRB2s and CRB3s; to block or ignore CRB1s (Rule 3); to observe the possible effects of their behavior on CRBs (Rule 4); and to make connections between elicitor and reinforcer stimuli of in-session CRBs and their functional equivalents occurring in out-sessions to improve generalization (Rule 5) (Callaghan et al., 2003; Kohlenberg & Tsai, 1991).

In terms of its clinical application, FAP has been used as a stand-alone intervention approach or in combination with other treatments, such as acceptance and commitment therapy (FACT; Callaghan et al., 2004), behavioral activation (Kanter et al., 2008), and cognitive-behavioral therapy (Kohlenberg et al., 2002). Several narrative reviews have summarized the existing evidence of its clinical utility (Ferro García, 2008; Kanter et al., 2017; Mangabeira et al., 2012; Muñoz-Martínez et al., 2012). The most recent review by Kanter et al. (2017) found 19

single-case publications and six controlled and uncontrolled trials. They concluded that although the research support for FAP is promising, it is not sufficient to consider it evidence-based for specific psychiatric disorders. On the other hand, they found that FAP does lead to positive changes in individually defined behavioral problems and that its proposed mechanism is active and valid. Singh and O'Brien (2018) quantitatively synthesized FAP single-case designs (SCDs). They included 20 studies, most of which used an A/B design with a total of 37 participants and found that FAP achieved large effect sizes (ES) on CRBs. In addition, they concluded that the FAP literature could be improved with more rigorous design and outcome assessment methods and that studies may be limited by designs that provide weak causal inference, uncertain construct validity, and questionable generalizability (Singh & O'Brien, 2018). However, previously published reviews used evidence accumulated until 2017 (Kanter et al., 2017; Singh & O'Brien, 2018), most of them in a narrative rather than quantitative manner (Ferro García, 2008; Mangabeira et al., 2012; Muñoz-Martínez et al., 2012), or focused on one type of experimental design (Singh & O'Brien, 2018), ignoring evidence from randomized controlled trials (RCTs) or other group studies.

Therefore, the purpose of this study was to systematically review and meta-analyze published literature on the efficacy of FAP on various clinical outcomes, such as CRBs, O1/O2s, anxiety, anger and depression symptoms, mindfulness, experiential avoidance/psychological flexibility, fear of intimacy, and marital quality/satisfaction, with an updated body of evidence. In addition, this study aimed to examine the potential effect of several clinical variables (e.g., diagnosis, age of sample, and type of therapy) and methodological variables (e.g., type of publication and comparison group). The quantitative synthesis of this study could help train therapists on the fundamental processes that promote clinical outcomes and introduce FAP as an evidence-based therapy because of its flexibility and philosophical roots that understand the human being in a global social framework (Hayes & Hofmann, 2021).

Method

This study did not receive specific funding from public, commercial, or nonprofit sources. This review adhered to the PRISMA statement for reporting in systematic reviews and meta-analyses (Page et al., 2021). The PRISMA checklist can be consulted in this link. This review has not been pre-registered.

Eligibility criteria

We were aware that the potential sample of eligible studies was small; therefore, we set broad selection criteria. To avoid potential bias, we then conducted sensitivity analyses. Following the PICOS approach, studies with the following characteristics were included:

- Population: A sample of children, adolescents, and adult participants (no age restrictions) diagnosed with one or more disorders according to the DSM classification (American Psychiatric Association, 2013) and without a formal diagnosis.
- Intervention: One or more experimental groups received an intervention based on either stand-alone FAP or FAP combined with other behavioral therapies (e.g., FACT, functional-enhanced behavioral activation, or functional-enhanced cognitive therapy).
- Comparison/Study Designs: RCTs, non-randomized controlled trials, uncontrolled pre-post group trials, and SCDs. For controlled trials, both active and inactive comparisons were included.
- Outcomes: Any quantitative measure of a clinical outcome (in- and out-session CRBs, anxiety, anger and depression symptoms, emotional dysregulation, experimental avoidance, mindfulness, marital quality/satisfaction, quality of life, or fear of intimacy) from standardized and validated questionnaires or direct measure of behavioral parameters. SCDs should have included baseline or comparison phases.
- Additional criteria: Peer-reviewed articles published in a scientific journal were preferred, but master's or doctoral theses were also included. Articles had to be written in English, Spanish

or Portuguese, as these were the languages understood by the authors. Articles written in Farsi were also included, but we sought the help of native speakers to oversee the translation process.

Information sources and search strategy

Since a more restrictive and specific search approach resulted in a small number of entries, we opted for a broader method. This involved searching only for the term "functional analytic psychotherapy" in titles, abstracts, and keywords (where available) in MedLine (via PubMed), PsychInfo, and Scopus databases. The Google Scholar database was also consulted in order to make the search approach as exhaustive as possible, as this source also covers potentially eligible gray literature. The same term was used in all the databases. No librarian was consulted on the search strategy. Reference lists of identified articles were reviewed to identify additional sources. In addition, a forward reference search of articles citing identified studies was conducted to potentially identify additional eligible studies. The last search was performed on August 10, 2023.

Selection process

The initial search was conducted by [MASKED], who selected potentially relevant studies by reading their titles and abstracts. [MASKED] and [MASKED] independently made the final selection by applying the selection criteria after reading the full text. The Cohen's Kappa coefficient for the agreement was considered *almost perfect* ($\kappa = 0.88$). Disagreements were resolved by consensus. The result of the selection process can be viewed online here, under the "Study Selection" tab.

Data collection and data items

After the final selection of studies, [MASKED] and [MASKED] independently coded the following variables: (A) *study design* (e.g., RCTs, non-randomized controlled trials, pre-post uncontrolled group trials, and SCDs); (B) *publication type* (e.g., peer-reviewed journal article, non-peer-reviewed journal article, master's or doctoral theses); (C) *population diagnosis* (e.g.,

Axis I, Axis II, Axis I+II, no diagnosis, therapists); (D) *sample mean age and percentage of women;* (E) *intervention type* (e.g. FAP, FACT, FAP + cognitive-behavioral therapy, functional-enhanced behavioral activation, or other) and *the number of sessions*; (F) *comparison* (e.g., active control, inactive control); and (G) *outcomes included* (e.g. a list of the quantitative measures included). Disagreements were resolved by consensus. Agreement across variables ranged from $\kappa = 0.52$ (moderate) and $\kappa = 1$ (perfect). The result of the coding process and the codebook can be viewed online here, under the "Study Coding" tab.

[MASKED] then extracted the means and standard deviations of each study arm from pre- and post-uncontrolled trials, randomized and non-randomized controlled trials, and the frequencies or percentages of CRBs from SCDs. The standard deviation value was derived from confidence intervals (CIs) or standard errors according to the formulas proposed by Higgins and Green (2011). [MASKED] double-checked a randomly selected subset of the data for accuracy. Quantitative data presented visually as graphs and figures were extracted using the WebPlotDigitizer version 4.5 web tool (Rohatgi, 2021). For studies with insufficient data to calculate the ES, or in cases of doubt, we contacted the corresponding author of the paper. The fully extracted dataset can be consulted online here, through the tabs named after each outcome.

Assessment of risk of bias

The risk of bias in each study was assessed using the Cochrane Risk of Bias Tool 2 (Sterne et al., 2019) for RCTs, ROBINS-I for nonrandomized and uncontrolled trials (Sterne et al., 2016), and the SCD RoB tool for SCDs (Reichow et al., 2018).

Data analysis

Changes in CRBs and O1/O2s across the treatment, and post-treatment and long-term changes in experiential avoidance/psychological inflexibility, mindfulness, fear of intimacy, marital quality/satisfaction, anxiety, and depression and anger symptoms were considered the primary outcomes in this review.

For the group trials, the ESs and associated variances were estimated following Morris's suggestion for within-subject (Morris, 2000) and between-group outcomes (Morris, 2008). In both cases, these estimates were multiplied by a small sample factor correction (Morris, 2000, 2008). Since the *r*-value was not reported in the studies, a value of 0.07 was used to calculate the variance according to Rosenthal (1991). The standard error was calculated as the square root of the estimated variance. Following Cohen's rule of thumb (Cohen, 1988), reference values of 0.2, 0.5, and 0.8 were interpreted as small, moderate, and large ESs, respectively.

For ES estimation of SCDs, we chose non-overlapping measures because parametric indices have often been criticized for being inappropriate for SCD data (Solomon et al., 2015). For this reason, the baseline corrected Tau (τ_{BC}) index was chosen for the ES estimation of SCDs. In addition, τ_{BC} takes into account time trends and measurements are likely to be interdependent. Moreover, following the recommendations of Pustejovsky (2019), the log response ratio (LRR) was calculated for SCDs because it showed less procedural sensitivity. For the calculation of τ_{BC}, we followed the proposal of Tarlow (2017) and performed a pretest for baseline trend, adjusting it if significant, while LRR was calculated according to Pustejovsky (2018). These analyses were performed using the SingleCaseES package (Pustejovsky et al., 2022) for R (R Core Team, 2022) in RStudio (RStudio Team, 2020). Following the proposal by Pustejovsky (2019), benchmark τ_{BC} values of 0.3 and 0.84 were set for moderate and large ESs, respectively. Since estimating ES requires at least three observations for each phase, SCDs with fewer than three observations (Arco-Tirado et al., 2005; Martín-Murcia et al., 2011; Montaño et al., 2018; Singh & O'Brien, 2017) had values imputed using the last observation carried forward method.

The ES of each individual study were then weighted by their inverse variance for each outcome. We chose a random-effects model expecting high heterogeneity between studies and assuming that true ES may vary from study to study (Borenstein et al., 2010). Funnel and forest plots were constructed for each outcome. The I^2 index was calculated to assess heterogeneity between studies (Higgins et al., 2003), with values of 25, 50, and 75% reflecting low, moderate,

and high heterogeneity, respectively. Meta-analytic procedures were performed using IBM SPSS v28.0 (IBM Corp., 2021).

Additional analyses

Potential publication bias was assessed using several methods, in addition to the visual examination of symmetry in funnel plots. Following the recommendation of Higgins et al. (2011), only funnel plots from the outcomes of more than 10 studies were examined. In addition, the trim-and-fill method (Duval & Tweedie, 2000) was used to trim the asymmetric results from one side of the funnel plot and fill them with their imputed counterparts on the other side, estimating a newly corrected ES. Finally, the Egger's test was performed (Egger et al., 1997) to assess bias by predicting the standardized effect (ES divided by the standard error) using the precision of each ES (the inverse of the standard error).

To explore potential sources of heterogeneity, subgroup analyses were performed by comparing groups using *Q*-tests. Studies were grouped according to clinically relevant variables (e.g., diagnosis, age of sample, and type of therapy) and methodological variables (e.g., type of publication and comparison). Additional sensitivity analyses were performed to assess the robustness of the results by excluding nonrandomized controlled trials from the BETWEEN-GROUP results and studies in which data were imputed using the last observation carried forward method. Meta-regression analyses were performed to assess the influence of continuous variance on the observed heterogeneity.

Results

Of the 523 records initially screened, 70 studies were finally included in the meta-analysis, with 58/690 participants per outcome at post-treatment and 61/191 participants retained at follow-up. Among the included studies, there were 45 SCDs, 16 RCTs, 4 non-randomized controlled trials, and 5 group pre-post uncontrolled trials. From the group studies, eight conducted a follow-up assessment (Bowen et al., 2012; Foroutan et al., 2018; Kanter et al., 2018; Keng et al., 2017; Kohlenberg et al., 2002; Siroos et al., 2021; Tsai et al., 2020; Vandenberghe &

Leite, 2018), with an average duration of 19.37 weeks. Six previously published reviews were identified. Figure 1 shows the results of the search and selection process. In addition, Table 1 outlines the main characteristics of the included studies. Detailed eligibility results are available online here. Authors of several studies that met the inclusion criteria (Holman et al., 2012; Maitland et al., 2016; Olaz et al., 2022) were asked to provide more information on incomplete data or to clarify doubts. We received responses from one of them (Maitland et al., 2016) that provided the requested data. The main reasons for exclusion were lack of quantitative measures of clinical outcomes for group studies (n = 15) and lack of baseline measures for SCD (n = 6). We found one duplicate record reporting the same study data (Knott, 2014; Knott et al., 2019), and decided to include the first record because it reported more data.

Risk of bias assessment results

Figure 2 shows the results of the risk of bias assessment. All studies, except one, had an overall high risk of bias. Specifically, the measurement of the outcome domain, the randomization process, and deviations from the intended interventions in RCTs were identified as the primary factors influencing this high risk of bias. For SCDs, the most relevant source of risk of bias was the lack of blinding of the participants/staff and outcome assessors.

Regarding the assessment of publication bias, funnel plots are shown in Supplementary Figures A.1 to A.5, in Supplementary Material A. Only funnel plots with 10 or more studies are shown (for between-group and within-subject depression and anxiety symptoms, CRB1, O1, CRB2, O2, and CRB3). The full results of the trim-and-fill and Egger's tests are presented in Supplementary Table B.1, in Supplementary Material B. The clearest evidence of publication bias was found for post-treatment between-group and within-subject depression symptoms and O2s, as indicated by asymmetric funnel plots and significant results from Egger's test. Trim-and-fill also adjusted O2s ES to be non-significant. Egger's test was also significant for post-treatment between-group and within-subject anxiety symptoms, post-treatment within-subject experiential avoidance/psychological inflexibility, CRB2s, and O1s. In addition, trim-and-fill imputed studies

on the minority of outcomes, and the adjusted ES was no longer significant only for post-treatment between-group and within-subject fear of intimacy, while the others remained significant.

Meta-analyses results

Table 2 shows the τ_{BC} and LRR estimates for CRBs and O1/O2s, and Table 3 shows the standardized mean differences for the between-group and within-subject outcomes, along with the results of the heterogeneity I^2 analyses and 95% confidence intervals. Forest plots for the between-group and within-subject outcomes are shown in Supplementary Figures A.6 through A.13 in Supplementary Material A.

Single case designs

Significant reductions, with moderate ES estimates, were observed in CRB1s and O1s. These problem behaviors showed a decrease of -28.64% in-session after FAP was implemented, which was greater out-session (-42.65%). The results also showed a large effect size estimate for the increase in CRB2s, along with moderate estimates for the changes in CRB3s and O2s. The percentage of change in these improved and more flexible behaviors was greater in the in-session (131.72% for CRB2s and 82.90% for CRB3s) than in the out-session.

Group designs

For the between-group and within-subject post-treatment outcomes, the largest estimates were obtained for depression and anxiety symptoms, with ESs ranging from large to very large. At follow-up, the effect on between-group depression decreased to small to moderate, but the within-subject estimates increased to very large. In addition, large ESs were estimated for marital quality/satisfaction, and moderate to large for between-group fear of intimacy. Regarding the effect on mindfulness, between-group estimates were not significant at post-treatment but increased to moderate at follow-up, and within-subject estimates were moderate at both assessment points. However, the ES estimates for the between-group experiential avoidance,

anger symptoms, and quality of life were not significant. For these variables, only significant within-subject effects were observed at the post-treatment.

Subgroup and sensitivity analyses results

The results of the statistically significant subgroup analyses and full meta-regression results are shown in Supplementary Tables B.2 and B.3, in Supplementary Material B. Owing to the small number of studies included in some subgroups in the type of comparison group, age, and diagnosis of the sample, and particularly the type of intervention, the results must be interpreted with caution. We now proceed to comment only on the significant differences or regressions found.

Study characteristics: Type of publication and type of comparison group

The ES estimates for studies published in peer-reviewed journals were significantly higher than those published as master's or doctoral theses for O1s ($Q = 6.42$, $P > 0.01$), CRB2s ($Q = 9.66$, $P > 0.01$), O2s ($Q = 18.33$, $P > 0.01$) and CRB3s ($Q = 18.45$, $P > 0.01$) outcomes. Regarding the comparison groups, studies comparing FAP with active control groups achieved significantly lower ES only for between-group anxiety at post-treatment ($Q = 6.94$, $P = 0.01$) and at follow-up ($Q = 21.03$, $P > 0.01$).

Participant characteristics: Age, sex and diagnosis of the participants

The results of the meta-regressions showed that the mean age of the participants significantly predicted the ES only for O1s ($\beta = 0.02$; SE = 0.01; $P = 0.01$). According to these results, each additional year reduced effectiveness by 0.02 points, suggesting that FAP was more effective among younger people. Nevertheless, the model only explained the variance of $R^2 = 0.04$. Regarding the diagnosis of the participants, ES was significantly higher in studies in which participants were diagnosed with DSM Axis I + II disorders than in those in which participants were diagnosed with Axis I disorders only or were not formally diagnosed for CRB2s ($Q = 62.37$, $P > 0.01$), CRB3s ($Q = 18.45$, $P > 0.01$), and between-group anxiety at follow-up ($Q = 21.03$, $P > 0.01$) outcomes. In addition, ES estimates for studies with non-diagnosed participants were

significantly lower than those with participants diagnosed with an Axis I disorder on O2s (Q = 18.33, P > 0.01), between-group experimental avoidance/psychological flexibility post-treatment (Q = 7.48, P = 0.01), follow-up (Q = 11.63, P > 0.01), and within-subject quality of life post-treatment (Q = 27.42, P > 0.01); while the opposite was true for between-group anxiety post-treatment (Q = 9.87, P > 0.01).

Intervention characteristics: Type of intervention and number of sessions

FACT achieved a significantly higher ES for CRB1s (Q = 17.92, P > 0.01), O2s (Q = 48.35, P > 0.01), and on between-group (Q = 21.31, P > 0.01), and within-subject (Q = 6.11, p = 0.03) anxiety post-treatment. In contrast, a significantly higher ES was obtained for FAP alone for between-group depression post-treatment (Q = 19.83, P > 0.01) and for FAP + cognitive-behavioral therapy for within-subject depression at post-treatment (Q = 46.97, P > 0.01) and follow-up (Q = 5.11, P = 0.02). In addition, the number of sessions significantly predicted the ES only for O1s (β = 0.02; SE = 0.01; P = 0.01).

Sensitivity analyses: Non-randomized trials and studies with imputed data sensitivity analyses

After eliminating the non-randomized controlled trials, only the outcome of between-group depression at follow-up was no longer significant, with an ES estimate of SMD = 0.34 (95% CI = -.19 to 0.88). All other outcomes remained significant. Regarding the studies in which we imputed data, after removing these studies, the ES estimates were τ_{BC} = 0.45 (95% CI = 0.28 to 0.61) and LRR = -0.32 (95% CI = -0.48 to -0.15) for CRB1s; τ_{BC} = 0.59 (95% CI = 0.48 to 0.70) and LRR = -0.61 (95% CI = -0.77 to -0.46) for O1s; τ_{BC} = 0.99 (95% CI = 0.98 to 1.00) and LRR = 1.26 (95% CI = 1.01 to 1.51) for CRB2s; τ_{BC} = 0.53 (95% CI = 0.34 to 0.72) and LRR = 0.62 (95% CI = 0.28 to 0.98) for O2s; and no change for CRB3s. Therefore, no substantial changes were observed when the studies with imputed data were excluded from the analysis.

Discussion

This review aimed to quantitatively summarize the evidence to investigate the efficacy of FAP on various clinical outcomes. These clinical outcomes include idiographic clinically relevant behaviors and other more globally defined constructs, such as anxiety, anger and depression symptoms, mindfulness, experiential avoidance/psychological inflexibility, fear of intimacy, marital quality/satisfaction, and quality of life.

Results of the literature search

The initial noteworthy discovery of this review was the quantity of studies and their methodological quality have significantly increased since the most recently published reviews (Kanter et al., 2017; Singh & O'Brien, 2018). For instance, the number of RCTs has increased from five (Kanter et al., 2017) to sixteen, and the number of SCDs has doubled since the last published review on this topic (Singh & O'Brien, 2018). This indicates the rapid development of this field, although most studies still have several methodological shortcomings, such as the lack of a control group, small sample sizes, and unclear reporting. However, some authors have indicated that previous research has mainly focused on SCDs and uncontrolled group trials, which may limit the external validity of the findings and introduce bias (Kanter et al., 2017; Kohlenberg & Tsai, 1994). Therefore, they suggest that further research on the efficacy of FAP is needed (Maitland & Gaynor, 2012).

In contrast, FAP SCDs have utilized methodologically sound designs. They clearly operationalize the CRBs and directly measuring them, rather than relying on standardized questionnaires. In fact, some authors in contextual behavioral science advocate for SCDs (Singh & O'Brien, 2018) to create tailored interventions that consider the significant variability between individuals. They note that a functional analysis of CRBs must be done in an idiographic manner, as individuals with seemingly similar problems may differ in the processes that led to them (Fisher et al., 2018). Thus, drawing conclusions based on study processes at a normative level may lead to flawed analyses (Hayes & Hofmann, 2021). According to Busch (2009), efficacy

research data may be limited because FAP focuses on functionally defined therapeutic processes rather than on manualized treatment techniques. One possible alternative is to develop protocols based on functional similarity, such as group iterative multiple model estimation and other complex network models. It is important to note that while individuals share macro-contingencies and principles of learning, each person's learning history is unique and cannot be transferred. Regarding the risk of bias, our assessment revealed issues with the methodological quality of the included studies. Concerning group designs, many studies did not randomize allocation, and among those that did, the process was often insufficiently described. Another potential source of bias that we have identified is the self-reporting of the outcome measures. However, there are some aspects to be considered when applying the Cochrane risk of bias assessment tools in the context of psychotherapy research. Some authors argue that reliance on self-reported outcomes is common in psychotherapy research and that there is limited evidence that it leads to overestimation of treatment effects (Munder & Barth, 2018). These authors add that blinding patients and therapists is not possible in psychotherapy, making the inclusion of an active comparator more informative. In this regard, the majority of the controlled trials included in the review used one or more active control groups (Bowen et al., 2012; Etemadi et al., 2018; Foroutan et al., 2018; Gifford et al., 2011; Kanter et al., 2018; Kohlenberg et al., 2015; Maitland et al., 2016; Maitland & Gaynor, 2016; Reyes-Ortega et al., 2020; Tsai et al., 2020). For SCDs, the primary source of bias was the lack of clarity in describing procedural fidelity and insufficient information on blinding of outcome assessment. Therefore, future studies should address these methodological shortcomings to improve the validity of the results and their interpretation.

Results of the meta-analyses

Nevertheless, despite the potential threats to the validity of these studies, the SCD meta-analysis results indicate that FAP effectively reduces problem behaviors and increases positive behaviors both in and out of therapy sessions. The findings of Singh et al. (2018), the most recent review of FAP SCDs, are partially supported by our results. However, our review has a larger

sample size and showed a lower percentage change in CRB1s and a higher percentage change in CRB2s. These results indicate that FAP is effective in addressing its primary goal of increasing improved behaviors and decreasing problem behaviors. Our results are consistent with other recent reviews that suggest the efficacy of Acceptance and Commitment Therapy for a wide range of conditions (Gloster et al., 2020), which further supports the use of contextual therapies in clinical practice. These more recent interventions are now achieving an empirical status similar to that of earlier behavioral therapies.

Similarly, we found that CRB1 decreased more outside of sessions, while CRB2 showed greater improvement during sessions. These results are consistent with previous research (Singh & O'Brien, 2018). This suggests that reductions in undesirable behaviors may be more generalizable to daily life, while improved behaviors may not be easily transferred to interactions outside of the therapy sessions. However, it is possible that these results may be influenced by the client's bias towards focusing on problem behaviors rather than improved alternatives, as out-of-session changes are typically self-reported. Other factors may be involved in the client's bias, such as a desire to appear more distressed in order to get more help or attention, or a lack of awareness of positive changes (Lambert & Shimokawa, 2011; Westen et al., 2004).

Regarding the results from the group designs, the most robust benefits compared to controls were observed for depression and anxiety symptoms, with large ES estimates. Our study supports the findings of previous reviews (Kanter et al., 2017), but with a larger sample size of participants and studies. Another important finding was that improvements in depression symptoms were sustained over the follow-up period, and there were no differences between active and non-active controls. This provides further evidence of the specific efficacy of FAP on these outcomes. Therefore, while the evidence may be considered preliminary, these findings broaden the usefulness of FAP beyond purely interpersonal issues and may extend its application to emotional disorders. Although our findings were based on only a few studies, they suggested improvements in fear of intimacy compared to controls. This is consistent with the premise of

FAP (Kohlenberg & Tsai, 1991), as fear of intimacy behaviors often arise within therapy sessions and can be directly shaped by the therapist. As suggested by Wetterneck and Chad (2012), FAP may be therapeutic in addressing intimacy, which behavior analytic theory has defined as a behavior that develops from a learning history in which interpersonally vulnerable behaviors are reinforced by others. On the other hand, fear of intimacy has been defined as the inhibited ability, due to anxiety, to share personal thoughts and feelings with a valued partner, including fears of exposure, rejection, abandonment, or loss of control, and is associated with fewer intimate interactions and less comfort with self-disclosure (Manbeck et al., 2020). This is a transdiagnostic issue that can be difficult to target with other behavioral interventions, such as cognitive-behavioral therapy (Wetterneck & Hart, 2012). In the context of FAP, intimacy-promoting behaviors (i.e., openly sharing one's feelings about private events) could be conceptualized as CRB2s, while fear of intimacy behaviors (i.e., avoiding explicitly talking about these feelings) could be considered CRB1s. In fact, many of the SCDs we included in our meta-analysis described behaviors as CRBs that clearly fit into this category, making our results more consistent. In addition, improvements in intimacy may lead to increased overall satisfaction in an individual's relationships, which is also consistent with our findings suggesting significant improvements in marital quality and satisfaction.

However, the between-group effect was unexpectedly found to be null for the remaining outcomes (e.g., anger symptoms, experiential avoidance, and mindfulness). Only a within-subject effect was observed. Nevertheless, although being based on a small number of studies, our results suggest that the long-term effect on mindfulness is significant, suggesting that the development of such behaviors may take more time. These null results are particularly surprising for experiential avoidance/psychological flexibility, which is often considered a target behavior in FAP. One possible explanation for this is that the results are self-reported and, as noted above, potentially underestimate the positive change (Lambert & Shimokawa, 2011; Westen et al., 2004). Cognitive flexibility, the tendency to respond to situations in ways that facilitate the pursuit of valued goals

(Doorley et al., 2020), is a key mechanism underlying changes in acceptance and commitment therapy. Thus, our results may suggest that the integration of FAP with ACT (FACT) (Callaghan et al., 2004), may be relevant for targeting such behaviors.

Subgroup analyses

Despite potential limitations due to the small sample size, the subgroup and meta-regression analyses provided valuable clinical insights. First, age and sex did not significantly predict ES for most outcomes, suggesting that FAP may be a feasible and effective treatment approach for a broad population regardless of demographic characteristics. Second, participants diagnosed with DSM Axis I and II disorders benefited more from treatment than those without a diagnosis, showing greater improvement across multiple outcomes. Although contextual therapies often take a transdiagnostic approach (Hayes & Hofmann, 2021), clients with a psychiatric diagnosis may have had a more severe clinical profile at baseline and thus greater potential for improvement over the course of treatment. Third, our results suggest that FACT achieved higher ESs for multiple outcomes (e.g., CRB1s, O2, and anxiety symptoms), whereas stand-alone FAP showed greater efficacy for depression symptoms, which is consistent with the findings of a previous review (Kanter et al., 2017). These preliminary results suggest that the combination of ACT and FAP may facilitate out-session generalization of in-session changes and may be particularly useful for clients with anxiety problems. Fourth, it appears that the number of sessions did not affect the observed effect, at least for most outcomes. This may indicate that in FAP, unlike other approaches, therapeutic change occurs from the earliest stages of the intervention. Finally, sensitivity analyses show that the results are generally robust, with most results remaining significant when studies with imputed data or non-randomized trials are excluded.

Generalizability of the results

When interpreting the generalizability of the results, it is important to consider certain aspects. First, with regard to the characteristics of the study participants, almost two-thirds of the

participants (65.36%) were women, in contrast to the 42.0% of women who participated in psychiatric studies (Sosinsky et al., 2022). In addition, the mean age ranged from 12.75 to 49.60 years old, with most studies including young adult participants. Only two RCTs (Gholami et al., 2021; Macías et al., 2022) and seven SCDs (Cattivelli et al., 2012; Costa & da Rocha, 2020; Gaynor & Lawrence, 2002; Martín-Murcia et al., 2011; Moreira, 2018; Romero-Porras et al., 2018; Xavier, 2018) included underage participants. Therefore, our results can mainly be generalized to female young adults in terms of demographic characteristics. In terms of geographic areas, although studies were conducted in a variety of world regions, such as North and Latin America, Europe, and the Middle West, many of them were concentrated in just a few countries: the United States of America, Brazil, Spain, and Iran. This could limit the application of our findings to other countries/areas. Other factors that may limit the generalizability of the results is the high heterogeneity observed in most of the outcomes and, as we discuss further in the next section, the high risk of bias that we identified.

Limitations

Several factors may have limited the validity of our findings. Perhaps the most relevant was the paucity of evidence from more rigorous study designs, such as RCTs, which has been noted previously (Maitland & Gaynor, 2012). This is also reflected in the evidence of risk of bias observed in the evaluation of controlled trials, mainly due to inadequate information on the randomization process and the prevalent use of self-reported outcomes. This could affect the validity of the results as well as the generalizability of the observed effect in a clinical setting. Therefore, future studies should aim to address this by reporting the randomization process in more detail, including blinded clinician-reported outcome measures, and including larger sample sizes with active control comparators. Regarding SCDs, the validity of the results may be compromised by insufficient information on both procedural fidelity and blinding of outcome assessment. Future SCDs should improve this reporting deficiency by more clearly describing both aspects. In addition, given that the outcome measures in FAP typically involve recording

idiographically defined behaviors during therapy sessions, which can be resource-intensive, recent advances in artificial intelligence may offer promising opportunities to automate behavioral coding and potentially allow for larger sample sizes in FAP research. Another important methodological shortcoming that future research should properly address is the lack of information on how FAP was implemented, which is especially evident in the RCTs and SCDs. This could introduce a bias as to whether the interventions deviated from the FAP rules. An additional limitation of our sample is that three studies (Ansarypour et al., 2019; Etemadi et al., 2018; Foroutan et al., 2018) did not provide detailed descriptions of the participant samples. Regarding the mix of RCTs with nonrandomized controlled trials, although it may better predict outcomes in real clinical practice (Efthimiou et al., 2017), it may introduce some bias in the observed results. However, sensitivity analysis showed that the results were robust to the exclusion of nonrandomized controlled trials for most outcomes. It is also important to note that the mix of stand-alone FAP and FAP in combination with other interventions could be a source of significant heterogeneity in the results. Moreover, the results of Egger tests, trim-and-fill analyses, and subgroup analyses indicated that the ESs of studies published in peer-reviewed journals were significantly larger than those of doctoral or master's theses, suggesting the possibility of publication bias for some outcomes. Another potential limitation of our study was the imputation of missing data needed to calculate ESs for some of the SCDs. The results may be biased if the missing data are not completely missing at random, and the imputed values are estimates, so the uncertainty of these estimates must be considered. On the other hand, our sensitivity analyses showed that excluding studies with imputed data from the analysis did not significantly change the results. In addition, some data from the SCDs were extracted visually, which may have reduced accuracy. Moreover, it is important to note that for CRB2s and O2s, according to Pustejovsky (2018), LRR may be inappropriate when behavior is absent at baseline, as small increases in behavior may lead to overestimation of effect sizes. We recommend

considering τ_{BC} for these outcomes. Finally, some subgroup analyses may be limited by small sample sizes in some subgroups and should be interpreted with caution.

Conclusions

Notwithstanding these limitations, the results of this meta-analysis suggest that FAP, both as a stand-alone intervention and in combination with other behavioral interventions, has preliminary research support for the treatment of interpersonal and intrapersonal problem behaviors. In particular, the evidence is strongest for improving anxiety and depressive symptoms, and in clinically relevant idiographically defined behaviors. These symptoms may be representative of our increasingly rootless lifestyles. In light of this, FAP could be a source of natural reinforcement, providing grounding through the therapist's contingent responses aimed at cohering the client with a values-based life. Therefore, this could support its implementation in clinical practice to address various problems, placing the client-therapist relationship at the center of the mechanism of change. Similarly, the number of publications on the efficacy of FAP has increased considerably in recent years. However, these promising results must be interpreted considering the high risk of bias we found due to several methodological shortcomings we identified, such as inadequate description of the procedure and potential deviations from the intended interventions. It is, therefore, necessary to continue investigating the efficacy of FAP with less risk of bias, both in methodologically rigorous group designs such as RCTs and in SCDs, where the mechanism of change can be observed with much greater precision. It is also important to expand FAP research to other countries, younger and older age groups, and more male participants.

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Figures

Figure 1

PRISMA flow diagram

Figure 2a

Risk of bias summary for randomized controlled trials

Figure 2b

Risk of bias summary for group pre-post trials

Figure 2c

Risk of bias summary for single case designs

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