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DOI

[10.1007/s10802-022-00986-2](https://doi.org/10.1007/s10802-022-00986-2)

Publication date

2023

Document Version

Final published version

Published in

Research on Child and Adolescent Psychopathology

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Citation for published version (APA):

Roest, J. J., Welmers-Van de Poll, M. J., Van der Helm, G. H. P., Stams, G. J. J. M., & Hovee, M. (2023). A three-level meta-analysis on the alliance-outcome association in child and adolescent psychotherapy. *Research on Child and Adolescent Psychopathology*, 51(3), 275-293. <https://doi.org/10.1007/s10802-022-00986-2>

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A Three-level Meta-analysis on the Alliance-Outcome Association in Child and Adolescent Psychotherapy

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Accepted: 18 October 2022 / Published online: 17 November 2022

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Abstract

Previous meta-analyses have found small to moderate associations between child-therapist alliance and treatment outcomes. However, these meta-analyses have not taken into account changes in alliance (i.e., alliance shifts), alliance agreement (i.e., congruence or discrepancies between child-therapist ratings), and the role of alliance as a moderator in relation to treatment outcomes (i.e., an interaction effect of alliance and treatment condition on treatment outcomes). A series of multilevel meta-analyses of 99 studies was conducted to investigate several types of alliance-outcome associations in child and adolescent psychotherapy. Associations between child-therapist alliance and child outcomes ($r = 0.17$), changes in child-therapist alliance and child outcomes ($r = 0.19$), child-therapist alliance as a moderator of outcomes ($r = 0.09$), and parent-therapist alliance and child outcomes ($r = 0.13$) were small. Associations between child-therapist alliance agreement and outcomes ($r = 0.21$) and between parent-therapist alliance and parent outcomes ($r = 0.24$) were small to moderate. This meta-analysis provides the most updated and comprehensive overview of the alliance-outcome association in child and adolescent psychotherapy, showing that the alliance continues to show impact on treatment outcomes. Alliance research in youth psychotherapy has increasingly focused on several complex aspects of the alliance-outcome association, such as the role of changes in alliance, alliance discrepancies, client and therapist variability, and the reciprocal association between alliance and prior symptom change in relation to treatment outcomes. Implications for future research and clinical practice are discussed.

Keywords Meta-analysis · Therapeutic alliance · Working alliance · Child psychotherapy · Treatment outcomes

A Three-level Meta-analysis on the Alliance-Outcome Association in Child and Adolescent Psychotherapy

Researchers and clinicians both claim that therapeutic alliance is an important factor for successful psychotherapy, not only with adults, but also with children and their parents (Horvath et al., 2011; Karver et al., 2018; McLeod, 2011; Murphy & Hutton, 2018; Norcross &

Lambert, 2018). Therapeutic alliance has been defined in various ways (for a review, see Doran 2014; Horvath, 2018), but most definitions consist of three interdependent aspects; these include the personal bond between client and therapist, the agreement on therapy goals, and the agreement on tasks of therapy (Bordin, 1979). In recent years, alliance research in child and adolescent psychotherapy has received increasing attention, specifically changes in alliance during treatment and the role of the parent-therapist and child-therapist alliance. Also, methodological advances in meta-analysis, such as multilevel modelling, have become available, making it possible to investigate multiple effect sizes within studies, which is often the case in alliance research. Therefore, the purpose of the present meta-analysis was to synthesize the empirical literature on the alliance in child and adolescent psychotherapy to examine the relation between child- and parent-therapist alliance and treatment outcomes.

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The Alliance-Outcome Association in Child and Adolescent Psychotherapy

Research on the association between therapeutic alliance and treatment outcome of child and adolescent psychotherapy remained sparse until the late 1990s, but since then the quantity of research grew considerably. As a consequence, various meta-analyses have been conducted. Alliance-outcome studies in child and adolescent therapy typically use a correlational design, in which the alliance may be measured in the beginning of treatment (often in the 3rd session), at midtreatment, or concurrently at post-treatment. The treatment setting is rather heterogeneous when compared to adult psychotherapy, because treatment may be individual, family- or community based, or in a residential treatment setting.

Several meta-analyses on the alliance-outcome association in child populations using broad inclusion criteria found small to moderate effect sizes ($r = 0.20$, $k = 43$, Karver et al., 2019; $r = 0.14$, $k = 38$, McLeod, 2011; $r = 0.19$, $k = 29$, Shirk & Karver, 2011). A recent meta-analysis by Karver et al. (2018), focusing on the prospective association between alliance and outcomes, found a mean effect size of $r = 0.19$ ($k = 28$). Additionally, a recent meta-analysis specifically focusing on the association between self-report alliance ratings and outcomes in adolescent populations (mean age between 12 and 19) found a larger effect size ($r = 0.29$, $k = 27$, Murphy & Hutton, 2018).

Although previous meta-analyses have substantially increased knowledge on the alliance-outcome association in child and adolescent psychotherapy, several limitations remain. First, previous meta-analyses have used somewhat different inclusion criteria, making it difficult to conclude what variables have a moderating effect on the alliance-outcome association. Previous meta-analyses using broad inclusion criteria (e.g., concurrent alliance-outcome relations within a broad range of therapies; McLeod, 2011; Shirk & Karver, 2011) found several moderators of the alliance-outcome association such as child age (showing larger effects for children than for adolescents); type of problem behavior (showing larger effects for children with externalizing behavior than for those with other types of problem behavior); and timing of alliance (showing larger effects for alliance measured later in treatment than for alliance measured early in treatment). A more recent meta-analysis by Karver et al. (2018), using more stringent criteria (focusing on prospective alliance-outcome associations), found that the alliance-outcome association was stronger for outpatient treatment (vs. inpatient treatment), non-RCT studies (vs. RCT studies), and internalizing disorders (vs. substance abuse treatment and eating disorders). Also, behavioral treatment showed larger effects than mixed treatment approaches. Murphy and

Hutton (2018) focused specifically on the client-therapist ratio (number of therapists divided by number of clients) as a moderator of the alliance-outcome association, in order to investigate client and therapist contributions to the alliance-outcome association. However, no significant moderating effect was found.

Second, most studies have measured alliance at a single time point or several time points in relation to treatment outcome, using only these single time points or averaged scores. Some authors have suggested that changes in alliance may be more predictive of outcome than measurement of alliance at a single time point, considering that the alliance may be viewed as an ongoing dynamic process instead of a static characteristic at one point in time (Bickman et al., 2012; Chiu et al., 2009; Owen et al., 2016; Shirk & Karver, 2011) found only two studies in which alliance change was measured, which makes it difficult to draw firm conclusions about the effect of alliance change on outcomes. In the meta-analyses by McLeod (2011), Karver et al. (2018), and Murphy and Hutton (2018), no distinction was made between alliance change and alliance measured at a single time point. More recently, studies on the alliance in child and adolescent psychotherapy have increasingly focused on (early) changes in alliance in relation to outcomes (Chiu et al., 2009; Marker et al., 2013; Owen et al., 2016). Therefore, the growing number of studies on alliance change and outcomes makes it possible to meta-analytically examine whether (early) alliance change is related to treatment outcomes in child and adolescent psychotherapy, and to examine whether this association is stronger than the alliance-outcome association based on alliance measurements at a single time point.

Third, previous meta-analyses have not yet investigated the effect of alliance agreement on treatment outcomes. Some authors have suggested that the level of agreement on the quality of the alliance (also referred to as alliance congruence or lack thereof, i.e. alliance discrepancy) between client and therapist is a better indicator of their attunement to each other than the assessment of client and therapist perspectives of alliance separately, because the alliance is an interpersonal (dyadic) process (Marmarosh & Kivlighan, 2012; Zilcha-Mano et al., 2016). Client-therapist agreement has gained increased attention in alliance research in child and adolescent psychotherapy (Fjermestad et al., 2016; Ormhaug et al., 2015; Zandberg et al., 2015), which makes it possible to examine whether agreement between child and therapist alliance ratings is related to treatment outcomes.

Also, prior meta-analyses have not addressed the alliance as a potential moderator of treatment outcomes. A moderator of treatment outcomes can be seen as a variable that determines for whom or under which conditions an intervention has positive results (Kraemer et al., 2002). There are several definitions of moderation in the context

of intervention research: Kraemer et al. (2002) stated that a moderating variable should precede treatment, whereas Baron and Kenny (1986) stated that a third variable may affect the outcome of treatment, regardless of timing of the assessment. The alliance has generally been studied as a predictor of treatment outcomes instead of a moderator of outcomes (Barber et al., 2010). Nonetheless, by examining the alliance as a moderator of treatment outcomes, particularly in studies focusing on different treatments or several groups (e.g., experimental vs. control), the strength of the alliance–outcome association in a given treatment relative to another treatment can be determined, which has not yet been addressed in previous meta-analyses. Therefore, we also examined the interaction effect of alliance and treatment condition on treatment outcomes.

Further, the parent–therapist alliance has received increasing attention in alliance research in child and adolescent therapy. Previous meta-analyses have examined the association between parent–therapist alliance and child outcomes, indicating small to moderate effect sizes ($r = 0.24$; Karver et al., 2019; $r = 0.15$; McLeod, 2011). The parent–therapist alliance is recognized as an important factor in establishing positive outcomes in child and adolescent therapy, considering that parents may play a large role in the therapy process, including the referral of youth and transportation to treatment, and active participation in home-based or family-based treatment (McLeod, 2011; Welmers-Van de Poll et al., 2018). Therefore, the present meta-analysis also focused on studies reporting the parent–therapist alliance in child and adolescent therapy. Addressing the various alliance–outcome associations in separate meta-analyses allows for moderator analyses to gain an in-depth understanding of these alliance–outcome associations.

The Present Study

As the number of studies on the alliance in child and adolescent psychotherapy has substantially increased over the past decades, it is important to meta-analytically investigate important aspects of the alliance in relation to treatment outcomes, such as the changes in alliance and alliance agreement, as well as alliance as a moderator of treatment outcomes. In order to provide an accurate estimate of the alliance–outcome association, changes in alliance, alliance agreement, alliance as a moderator, type of alliance (child- or parent–therapist), and type of outcome (child or parent outcomes) need to be addressed in separate meta-analyses. Also, the increased number of studies makes it possible to test various moderators that could have an effect on these associations, and to test putative moderators of the alliance–outcome association that failed to reach significance in previous meta-analyses, possibly due to lack of statistical power.

In the present study, we examined the association between therapeutic alliance and treatment outcomes, taking into account associations between child–therapist or parent–therapist alliance, and child or parent outcomes. We examined studies examining single time points or averaged time point measurement scores of alliance, and studies on changes in alliance in separate meta-analyses. We also addressed the relation between child–therapist alliance agreement and child outcomes, and the moderating effect of alliance on treatment outcomes separately. Third, we investigated whether study characteristics, such as age, type of problem behavior, and timing of alliance, moderated the alliance–outcome association. It was expected that the relation between child- and parent–therapist alliance and treatment outcomes would be small to moderate, and that alliance changes as well as alliance agreement would be more strongly related to treatment outcomes than alliance measured at a single time point.

Methods

Sample of Studies

The inclusion criteria in the present meta-analysis were chosen to increase comparability with previous meta-analyses on the alliance–outcome association in child and adolescent psychotherapy using broad inclusion criteria (McLeod, 2011; Shirk & Karver, 2011). This strategy enabled us to also compare our results to recent meta-analyses using more stringent inclusion criteria (Karver et al., 2018; Murphy & Hutton, 2018). Inclusion and exclusion criteria for studies included in the separate meta-analyses are depicted in Table 1.

In an attempt to find all relevant published articles, various stepwise search procedures were carried out. First, an online database search up to June 1st 2021 was conducted on Scenedirect, Wiley Online Library, Taylor and Francis Online, SAGE Journals, Web of Science, and Springerlink. In doing so, the following search string was used in the database search: (“*therapeutic alliance*” OR “*working alliance*” OR “*helping alliance*” OR “*treatment alliance*” OR “*therapeutic relationship*”) AND (*child** OR *adolescen** OR *youth**) AND (*outcome* OR *change*). Also, a search was conducted a on the ProQuest databases in order to identify possibly overlooked unpublished dissertations.

The database search resulted in 2,175 references, which were screened by the first author based on title and abstract for relevance using inclusion criteria that would be apparent from the title or abstract: a, b, c, and g as depicted in Table 1. This process resulted in 156 studies that were assessed for eligibility. Second, relevant research reviews and prior meta-analyses (e.g., Elvins & Green, 2008;

Table 1 Inclusion and exclusion criteria for studies in the meta-analyses

	Inclusion	Exclusion
Alliance – outcome	a) the alliance construct operationalized as a collaborative relationship, consisting of a <i>bond</i> aspect and a <i>collaborative</i> aspect b) mean age of the child below 18 years c) the child and/or parents received a therapeutic intervention d) the child and/or parent had to be the unit of measurement (instead of the family as a whole) e) the therapist with whom an alliance was formed had to be clearly specified (as opposed to forming an alliance with multiple therapists) f) the study had to include a validated (formal) measure of the alliance between therapist and child, adolescent, or parent g) the study had to contain some assessment of treatment outcome h) the study had to report on the bivariate association between alliance and outcome	Treatment for a physical problem Alliance concept not clearly described Timing of alliance assessment not clearly described Timing of outcome assessment not clearly described
Alliance change – outcome	All of the criteria a – g <i>and</i> i) the study had to assess ‘alliance shifts’ (such as change scores between two time points) in relation to treatment outcome	
Alliance agreement – outcome	All of the criteria a – g <i>and</i> j) studies had to assess the level of alliance agreement between child and therapist alliance ratings (e.g., an interaction between informant ratings) in relation to outcome of treatment	
Alliance as a moderator	All of the criteria a – g <i>and</i> k) the study had to report on an interaction effect between alliance and treatment condition.	

Karver et al., 2006; Karver et al., 2019; McLeod, 2011; Murphy & Hutton, 2018; Shirk & Karver, 2003; Shirk et al., 2011) were used to identify studies that might have been overlooked in the search. Thirteen additional studies were identified by this method. In order to retrieve several studies and dissertations, authors were contacted. Google Scholar was used to retrieve studies that were found in reference sections of included studies or through review articles, which were not found in the database search.

In total, 168 studies were obtained of which the full text was reviewed. Studies were screened by the first, second, and last author, based on the in- and exclusion criteria depicted in Table 1. A total of 69 studies were excluded; primary reasons were that studies used a different operationalization of the alliance construct (e.g., Gatta et al., 2010; Smith et al., 2008). Also, some studies did not focus on a therapeutic outcome or a therapeutic intervention, or focused on the relation between alliance and other therapy process variables, or the outcome of a single session of therapy (e.g., DeVet et al., 2003; King et al., 2006; Langberg et al., 2016). Studies were also excluded if results regarding the same alliance-outcome associations were reported in another study using the same sample (e.g., Ayotte et al., 2016; Duppong Hurley et al., 2017; Fjermestad et al., 2018; Marcus et al., 2011; Shelef & Diamond, 2008; Zorzella et al., 2017).

If studies reported on outcomes of multivariate analyses, authors were contacted in order to obtain results of the bivariate association between alliance and treatment outcome. In total, 32 authors were contacted to obtain additional information regarding 38 studies. Thirteen authors responded to our inquiry. Five out of 13 authors who did respond, were able to provide the information requested. Thirteen out of 38 studies were excluded from the study sample, the remaining 25 studies could be included in at least one meta-analysis.

The search resulted in a total pool of 99 studies. Studies included in the meta-analysis are listed in Appendix A. A flow chart of our search strategy and screening process is depicted in Fig. B1 in Appendix B.

Coding of the Studies

We considered a number of variables to include in moderator analyses, which are divided into study characteristics, sample characteristics, alliance characteristics, outcome characteristics, and treatment characteristics. The purpose of coding these study characteristics was to test for substantive moderators on the one hand – such as type of problem behavior, type of treatment, and treatment setting) – and methodological moderators on the other hand, including timing of the alliance and outcome measurement, and type of

informant of alliance and outcome. Substantive moderators may provide information on whether the magnitude of the alliance-outcome depends on the type of problem behavior, which may be of interest to tailor treatment to certain populations. Methodological moderators provide information to help understand to what extent the alliance-outcome association is impacted by methods used in studying this association.

Study Characteristics

Information about the study was coded for each manuscript: author name(s), publication status, journal and journal impact factor, year of publication, the country in which the study was conducted (United States, Europe, other), and study quality.

Sample Characteristics

Information about the following variables was coded for each study: age of the child (studies were coded based on mean age of children above and below 12 years), client-therapist ratio (number of children or parents divided by the number of therapists), and type of problem behavior. The following information regarding child problem behavior was coded: target problem of the child for which the sample received treatment (internalizing problems, externalizing problems, mixed problems, substance abuse problems, and eating disorders).

Alliance Characteristics

Each alliance measure used in the studies was coded into the following categories: type of alliance (child-therapist alliance and parent-therapist alliance), alliance rater (child-report, parent-report, therapist-report, and observer-report), and timing of alliance assessment (early, middle, late, averaged, and posttreatment). For the meta-analysis on alliance change and outcomes, type of change was coded into changes across sessions, changes from early to midtreatment, early to late treatment, and changes from early to posttreatment. Of note, the use and interpretation of difference scores has been challenged, as these scores may not reflect any additional variance not already conveyed in the variables (i.e., time points of alliance assessment) from which the difference is calculated (Laird, 2020; Laird & De Los Reyes, 2013). We therefore coded the studies on alliance change as studies using change (difference) scores versus studies using more robust assessments of change to examine whether the parameterization of change scores moderated the association with outcomes.

Outcome Characteristics

Each outcome measure in the studies was coded into the following characteristics: outcome domain (symptom severity, functioning, treatment satisfaction, dropout/retention, and delinquency), outcome rater (child-report, parent-report, report by others, such as teacher, therapist, independent observer, or life event data), same rater of alliance and outcome, and outcome timing (midtreatment, posttreatment, and follow-up).

Treatment Characteristics

Treatments described in the studies were coded into several treatment characteristics: treatment setting (outpatient, inpatient, community/home-based, school, or not defined), use of a treatment manual, and treatment type (CBT and non-CBT).

The studies that met the inclusion criteria were coded by the first author. 20% of the studies were independently coded by the second author using a coding manual in order to calculate intercoder agreement. Reliability was computed with Cohen's kappa for categorical variables and intraclass correlations coefficients (ICCs) for continuous variables. The inter-rater reliability proved to be satisfactory, with kappas ranging from 0.78 to 1.00, and intraclass correlations (average measures) ranging from 0.73 to 1.00. Cohen's kappa was interpreted following guidelines by Byrt (1996): Values between 0.93 and 1.00 are considered excellent agreement; 0.81-0.92 very good agreement; 0.61-0.80 good agreement; 0.41-0.60 fair agreement; 0.21-0.40 slight agreement, and 0.01-0.20 poor agreement. ICC values were interpreted following guidelines by McDowell (2006): Values above 0.75 indicated excellent agreement, 0.60-0.74 showed good agreement, 0.40-0.59 indicated fair to moderate agreement, and below 0.40 was considered poor agreement.

Quality Appraisal

We used the quality of survey studies in psychology (Q-SSP checklist, Protogerou & Hagger, 2020). This quality appraisal tool comprises 20 items, and can be used to assess quality of psychological studies using survey designs, covering several quality domains, such as rationale, sampling, data collection and analyses, and ethics. Depending on the number of relevant items in the checklist, a score of 70% is rated as 'acceptable', whereas scores lower than 70% may indicate 'questionable' quality. A scoring guide of the Q-SSP is available at <https://osf.io/5aepd>. In the present meta-analysis, the total score was used to examine the effect

of study quality as a moderator of the alliance-outcome association.

Statistical Analyses

We calculated Pearson's r to estimate the association between therapeutic alliance and outcomes of therapy. For the meta-analyses on the alliance-outcome association using single time points, we only included effect sizes of bivariate alliance-outcome associations and excluded effect sizes from multivariate analyses. One study (Kirsch et al., 2018) reported Kendall's tau, which was transformed to Pearson's r based on the formula provided by Rupinski and Dunlap (1996). For studies that assessed alliance discrepancies or alliance changes, it was not possible to calculate bivariate associations. We chose to include these studies in our meta-analysis on the association between alliance change and outcomes in order to compare this effect size estimate to the association between alliance measured at single time points in relation to outcome. All calculations and conversions were done using the formulas of Rosenthal (1991), Rosenthal and DiMatteo (2001), Lipsey and Wilson (2001), and Borenstein et al. (2009).

If an association was in the expected direction (i.e., higher levels of therapeutic alliance were related to improved outcome), a positive r -value was assigned, whereas a negative r -value was assigned to associations that were not in the expected direction. For three studies reporting non-significant results without reporting test-statistics, we could not calculate an effect size, since the necessary information to calculate Pearson's r was not available. In these cases, we assigned the value zero, which is a conservative estimate of the true effect size (Mullen, 1989). Sensitivity analyses were conducted to test whether the studies which were assigned an effect size of zero had an effect on the overall effect size estimate, by excluding these effect sizes from the dataset.

We checked for outliers by calculating standardized scores of effect sizes in order to identify standardized scores larger than 3.29 or smaller than -3.29 following guidelines by Assink and Wibbelink (2016), based on Tabachnick and Fidell (2013). Regarding the meta-analysis on the alliance-outcome association, four effect sizes were identified as outliers. Two effect sizes exceeded the standardized value of -3.29 (both from Fernández et al., 2016), and two effect sizes exceeded the standardized value of 3.29 (also both from Fernández et al., 2016). Of note, the effect sizes from the study by Fernández et al. (2016) were also identified as outliers in a previous meta-analysis (Karver et al., 2019). To reduce the impact of these outliers, the raw r values of the outliers were substituted by a new r value equal to the highest (or lowest) effect size within the normal range. Sensitivity analyses were conducted to test whether the outliers had

an effect on the overall effect size estimates, by conducting the analyses including outliers.

Each correlation was transformed to Fisher's Z before combined effect sizes were calculated and moderator analyses were conducted (Mullen, 1989), and transformed back into Pearson r after analyses for reporting and interpretation. The resulting effect sizes were interpreted following Cohen's (1988) guidelines; r is a small effect when at least 0.10, r is a medium effect when at least 0.30, and r is a large effect when at least 0.50.

In most studies, more than one effect size was calculated, because these studies reported on multiple raters of alliance, multiple times of measurement, or multiple outcomes. Therefore, we used a three-level random effects model to account for statistical dependency of effect sizes (Cheung, 2014; Van den Noortgate et al., 2013, 2014), resulting in three sources of variance: sampling variance of the observed effect sizes (level 1), variance between effect sizes from the same study (level 2), and variance between studies (level 3). The three-level model was used to calculate an overall estimate of the difference between alliance ratings, the association between ratings, and the association between alliance and outcomes of psychotherapy. Also, it was used for moderator analysis to examine whether the observed variation was explained by theoretical or methodological characteristics of studies.

For the statistical analyses we used the function "rma.mv" of the metafor package (Viechtbauer, 2010, 2015) in the R environment (version 3.4.1; R Core Team, 2015). The R syntax and protocol (Assink & Wibbelink, 2016) were based on procedures outlined by Van den Noortgate et al. (2013, 2014), modeling three sources of variance. The t -distribution was used for testing individual regression coefficients of the meta-analytic models and for calculating the corresponding confidence intervals (Knapp & Hartung, 2003). This approach accounts for uncertainty of the amount of residual variance, which leads to a more accurate estimate of the standard errors and fewer type-I errors.

Iterative maximum likelihood estimation (MLE) procedures were applied to estimate unknown parameters. The intercept only model (without moderators) through MLE is equivalent to the traditional random-effects model by Hedges and Olkin (1985). In the overall model, covariates can be added to test potential moderators. Van den Noortgate and Onghena (2003) compared multilevel meta-analysis to the traditional meta-analytic approach and concluded that the results obtained by the maximum likelihood multilevel approach are not substantially different from the results of the traditional random-effects approaches for intercept only models.

For all associations, forest plots were generated based on the guidelines outlined by Fernández-Castilla et al. (2020) extended for use in three-level meta-analysis. Forest plots in traditional meta-analysis provide a visual representation of the effect size of a study based on the sample size and

confidence interval of effect sizes. The extended forest plot contains additional confidence intervals based on the sampling variance of individual observed effect sizes within the study and the number of effect sizes within the study. Therefore the forest plots provide information about the variability in effect sizes among studies and the relative contribution to the overall effect size estimate.

When models were extended with categorical moderators consisting of three or more categories, the omnibus test of the null hypothesis that all group mean effect sizes are equal, followed an *F*-distribution. To determine whether the variance between effect sizes from the same study (level 2), and the variance between studies (level 3) were significant, two separate one-tailed log-likelihood-ratio-tests were performed in which the deviance of the full model was compared to the deviance of a model excluding one of the variance parameters. The sampling variance of observed effect sizes (level 1) was estimated by using the formula of Cheung (2014). All model parameters were estimated using the restricted maximum likelihood estimation method and before moderator analyses were conducted, each continuous variable was centered around its mean and dichotomous dummy variables were created for all categorical variables (Tabachnick & Fidell, 2013). In multi-level regression analyses, the intercept is the reference category, while the dummy variables test if, and to what extent, the other categories deviate from the reference category. The log-likelihood-ratio-tests were performed one-tailed and all other tests were performed two-tailed. We considered *p*-values < 0.05 as statistically significant.

To increase comparability with prior meta-analyses, we tested whether the alliance-outcome effect sizes changed by applying more strict inclusion criteria, similar to the work of Shirk et al. (2011), such as exclusion of studies in which alliance and outcome were measured concurrently, and exclusion of studies in which treatment was not delivered individually. We also examined the overall effect size by excluding studies in which the mean age of children was below 12 years, excluding studies that did not use child self-report alliance ratings, and excluding studies in which the outcome was dropout, treatment satisfaction, and delinquency, following the method of Murphy and Hutton (2018).

Publication Bias

The tendency of selected publication by journals to accept papers that report significant associations – referred to as publication bias – can influence the overall estimates of effect sizes in a meta-analysis and therefore its conclusions (Rosenthal, 1979, 1991; Rothstein, 2008). We obtained all unpublished material as best as possible, which is the simplest solution to the problem of publication bias (Mullen, 1989; Rosenthal, 1991).

We applied three methods to address potential publication bias. First, we used Egger's regression test (Egger et al., 1997; Fernández-Castilla et al., 2021), which tests the degree of funnel plot asymmetry as measured by the intercept from regression of standard normal deviates (effect size divided by its standard error) against the estimate's precision (the inverse of the standard error). A significant Egger's test indicates funnel plot asymmetry. Following Fernández-Castilla et al. (2021), an adapted version of the Egger's test was used in which we investigated the relation between the effect size and the standard error in order to account for dependency of effect sizes. In doing so, the standard error of the effect size was included as a moderator in the regression model. Consequently, the degree of funnel plot asymmetry was interpreted via the moderator's regression weight and associated *p*-value.

A second method to address publication bias was the use of an extension of the funnel plot test for use in three-level meta-analysis (Fernández-Castilla et al., 2021). For all associations, both funnel plots of all effect sizes and plots of study effects are depicted, following guidelines by Fernández-Castilla et al. (2020). Funnel plots of all effect sizes are commonly used in meta-analysis to examine whether publication bias or selective reporting bias might be present (missing effect sizes at the lower-left part of the funnel plot). In a funnel plot of study effects, separate random-effects meta-analyses are conducted on each study, resulting in a dot based on the sample size and the number of effect sizes within the study.

We also performed a trim and fill procedure for all associations (Duval & Tweedie, 2000). Previous simulation studies have shown that effect size estimates based on imputation of effect sizes after the trim-and-fill procedure may not be accurate (Fernández-Castilla et al., 2021; Peters et al., 2007). Therefore, we used the trim-and-fill procedure as outlined by Fernández-Castilla et al. (2021) which estimates the number of effect sizes imputed at the right side or left side of the distribution, to examine whether the overall effect size estimates were sensitive to potential presence of publication bias. Fernández-Castilla et al. (2021) have proposed a method in which the estimated number of effect sizes on the left side of the funnel plot distribution is related to a cutoff value of the estimator of the trim-and-fill method, based on the population ES and power (number of effect sizes). If the number of imputed studies exceeds the cutoff value, this may be indicative of publication bias.

Results

Descriptive Statistics of the Study Sample

Table B1 (Appendix B) shows the characteristics of the study sample included in the meta-analyses. The sample

contains a total of 99 studies, which is divided into different subgroups. Sixty-two studies (66 independent samples, 399 ESs) were included in the meta-analysis on the association between child-therapist alliance and child outcomes. Twenty studies (23 independent samples, 62 ESs) focused on changes in child-therapist alliance and child outcomes. Seven studies (6 independent samples, 27 ESs) focused on the association between child-therapist alliance agreement and child outcomes. Eleven studies (11 independent samples, 67 ESs) reported on the child-therapist alliance as a moderator of child outcomes between treatment groups (e.g., experimental group vs. control group). Thirty-four studies (38 independent samples, 129 ESs) reported on the association between the parent-therapist alliance and child outcomes. Thirteen studies (14 independent samples, 86 ESs) focused on the association between the parent-therapist alliance and parent outcomes.

The studies were completed between 1992 and 2021. Eighty-one studies examined the association between child-therapist alliance and outcomes (including changes in child-therapist alliance, child-therapist alliance congruence and alliance as a moderator), 37 studies examined the association between parent-therapist alliance-outcomes (including child and parent outcomes), and 19 studies examined both child-therapist and parent-therapist alliance and outcomes. The majority of studies relied on self-report measures to assess alliance (child-therapist alliance, $n = 63$, 77.8%; parent-therapist alliance, $n = 33$, 89.1%). Thirty studies (37.0%) used therapist reports to assess child-therapist alliance and 13 studies (35.1%) used therapist reports to assess parent-therapist alliance. In 25 studies (30.9%), an observer measure was used to assess child-therapist alliance, and in 11 studies (29.7%) to assess parent-therapist alliance. Forty-six studies out of 99 alliance-outcome studies (46.4%) used multiple perspectives (i.e., client, therapist, or observer) to assess the alliance in relation to outcomes.

The studies reporting on the alliance-outcome association included a total of $N = 8,496$ children, and $N = 3,442$ parents. The mean sample size of children per study was $M = 111.79$ ($SD = 341.05$) and the mean sample size of parents per study was $M = 90.58$ ($SD = 143.03$). The mean age of the children was $M = 12.42$ ($SD = 3.42$; range 1.91–18.00).

With regard to the studies on alliance change and outcomes, studies used different methods to parameterize alliance change scores. The majority of studies relied on calculating change across two time points: mean difference scores between time points ($k = 7$), (residualized) change scores between time points ($k = 5$) or a reliable change index ($k = 1$). Other studies assessed alliance change in relation to outcome using a structural equation modeling (SEM) framework ($k = 2$), hierarchical linear modeling

(HLM) framework ($k = 2$), growth curve modeling ($k = 2$) or general estimation equation (GEE) ($k = 1$). Studies also differed in the period during which change in alliance was measured: difference between early treatment and post-treatment ($k = 5$), early to midtreatment ($k = 7$), early to late treatment ($k = 4$), or change from session to session ($k = 4$). Mean number of sessions between alliance measurements was $M = 3.93$, $SD = 2.15$, based on 14 studies. The remaining studies either reported pre-post scores with different number of sessions attended per youth, or change in alliance over several months.

Meta-analyses on Correlations Between Alliance and Outcomes

In Table 2 all mean effect sizes of alliance-outcome associations are presented. First, several meta-analyses were conducted on the association between child-therapist alliance and child outcomes. The overall mean effect size ($r = 0.17$, 95% CI = 0.13, 0.20, $p < 0.001$) for the association between the child-therapist alliance and child outcomes was significant (62 studies, 66 independent samples, 399 ESs). This indicates that when children have a more positive therapeutic alliance with their therapist, they have a better outcome of therapy. Sensitivity analysis including four outliers did not significantly change the overall ES ($r = 0.17$, 95% CI = 0.13, 0.21, $p < 0.001$). Also, sensitivity analysis excluding the effect sizes which were assigned a zero due to non-significant findings reported in primary studies, did not result in a significant change in the overall ES ($r = 0.20$, 95% CI = 0.16, 0.24, $p < .001$, $k = 58$, 351 ESs), based on the overlap of the confidence intervals.

Second, twenty studies (consisting of 23 independent samples, reporting 62 effect sizes) focused on changes in child-therapist alliance and child outcomes. The overall mean effect size for this association was significant ($r = 0.19$, 95% CI = 0.10, 0.27, $p < 0.001$), indicating that positive changes in child-therapist alliance during treatment are related to better outcomes of therapy. Third, we found an overall mean effect size ($r = 0.21$, 95% CI = 0.02, 0.38, $p = 0.036$) for the association between child-therapist alliance agreement and child outcomes (seven studies, six independent samples, 27 ESs). Notably, this analysis did not include changes in the level of agreement of the child-therapist alliance during treatment in relation to outcomes of child therapy. Fourth, we found a significant overall mean effect size ($r = 0.09$, 95% CI = 0.05, 0.13, $p < 0.001$) for the child-therapist alliance as a moderator of child outcomes between treatment groups (e.g., experimental group vs. control group, 11 studies, 11 independent samples, 67 ESs).

Fifth, we examined the influence of parent-therapist alliance on child and parent outcomes. Thirty-four studies

Table 2 Results for the Overall Mean Effect Sizes for the Association Between Child- or Parent-Therapist Alliance and Child- or Parent Outcomes

Type of association	# Studies ^a	# ES	Mean <i>r</i> (SE)	95% CI	<i>p</i>	σ^2_{level2}	σ^2_{level3}	% Var. level 1	% Var. level 2	% Var. level 3
Child alliance – child outcomes	66	399	0.17 (0.02)	0.13, 0.20	<0.001***	0.009***	0.015***	39.9	22.2	37.9
Child alliance change – child outcomes	23	62	0.19 (0.04)	0.10, 0.27	<0.001***	0.019***	0.025***	18.4	35.2	46.5
Child-therapist alliance agreement – child outcomes	6	27	0.21 (0.09)	0.02, 0.38	0.036*	0.013**	0.047***	17.7	17.5	64.8
Moderating effect of child alliance on child outcomes	11	67	0.09 (0.02)	0.05, 0.13	<0.001***	0.000	0.002	86.8	0.0	13.2
Parent alliance – child outcomes	38	129	0.13 (0.03)	0.08, 0.18	<0.001***	0.001*	0.011***	39.2	6.8	54.0
Parent alliance – parent outcomes	14	86	0.24 (0.07)	0.09, 0.37	0.002**	0.026***	0.052***	11.0	29.9	59.1

^a The number of studies reflects the number of independent samples

CI/confidence interval, ES effect size, σ^2_{level2} variance between effect sizes (within studies), σ^2_{level3} variance between effect sizes (between studies), % Var. percentage of variance explained
 *** $p < 0.01$, ** $p < 0.001$

(consisting of 38 independent samples, reporting 129 effect sizes) reported on the association between the parent-therapist alliance and child outcomes. The overall mean effect size for this association was significant ($r=0.13$, 95% CI=0.08, 0.18, $p < 0.001$). This indicates that when parents have a better therapeutic alliance with their child’s therapist, children have a better outcome of therapy. The overall mean effect size for the association between the parent-therapist alliance and parent outcomes (13 studies, 14 independent samples, 86 ESs) was significant ($r=0.24$, 95% CI=0.09, 0.37, $p=0.002$), suggesting that when parents have a better therapeutic alliance with their therapist, they have a better outcome of therapy. Forest plots of all associations are depicted in Appendix C.

Moderator Analyses

The results of the overall effect size estimates of the various types of alliance-outcome associations showed that there was significant variability in effect sizes within studies (level 2), as well as between studies (level 3). This variability stressed the need for moderator analyses to explain this variance. We considered various moderators related to study characteristics, sample characteristics, alliance characteristics, outcome characteristics, and treatment characteristics (Appendix D, Tables D1–D4). Although the results indicated significant within- and between-study variability for the association between alliance agreement and outcomes, this study sample contained only 5 studies, therefore moderator analyses were not carried out, given the small number of studies.

Moderator analyses on the association between child-therapist alliance and child outcomes indicated that problem type proved to be a significant moderator, indicating larger associations for youth with externalizing problems and mixed problems than for youth with internalizing problems. Alliance rater also was a significant moderator, indicating larger effects for child self-report alliance ratings compared to observer ratings. Regarding outcome characteristics, outcome domain proved to be a significant moderator; larger effects were found for treatment satisfaction than for symptom severity, functioning, or dropout. A significant moderating effect was also found for ratings of alliance and outcome by the same rater, showing larger effects for ratings of alliance and outcome by the same rater than for different raters. No significant moderating effects were found for study characteristics and treatment characteristics.

Next, moderator analyses were conducted on the association between change in child-therapist alliance and child outcomes. Type of alliance change proved to be a significant moderator, showing larger effects on outcome for change of alliance from early to posttreatment than for changes from

early to mid- or late treatment, and studies that measured alliance each session. Of note, the results indicated stronger associations for studies using change scores ($r=0.23$) compared to studies that used more robust methods to examine change ($r=0.12$), but this difference was non-significant ($p=.209$). Also, larger effects were found for adolescents (age > 13 years) than for children. Ratings of alliance and outcome by the same rater also was a significant moderator, showing larger effects for ratings of alliance and outcome by the same rater than for ratings by different raters. No moderating effects were found for study characteristics and treatment characteristics.

Further, moderator analyses of the association between parent-therapist alliance and child outcomes were conducted. A significant moderating effect was also found for client-therapist ratio, indicating larger effects on outcome when the client therapist ratio was higher (fewer therapists per client). Also, a significant moderating effect was found for timing of outcome assessment, indicating larger effects when outcome was measured posttreatment compared to follow-up. No moderating effects were found for study characteristics, alliance characteristics, and treatment characteristics.

We finally conducted moderator analyses on the association between parent-therapist alliance and parent outcomes. A significant effect was found for timing of alliance assessment; smaller effects were found for assessment of alliance early in treatment than for assessment of alliance at midtreatment, late in treatment, and compared to averaged alliance measurements. With regard to outcome characteristics, outcome domain proved to be a significant moderator, indicating larger effects were found for treatment satisfaction than for functioning or dropout. Also, a significant effect was found for ratings of alliance and outcome by the same rater, showing larger effects for ratings of alliance

and outcome by the same rater than for ratings by different raters. No significant moderating effects were found for study characteristics, sample characteristics, and treatment characteristics.

Publication Bias

Three methods were applied to address publication bias. First, we used extended versions of Egger's regression test (Egger et al., 1997) and the funnel plot test adapted from Fernández-Castilla et al. (2021), to test the degree of funnel plot asymmetry. We also conducted trim-and-fill analyses (Duval & Tweedie, 2000; Fernández-Castilla et al., 2021). The z-statistics and p-values of the Egger's tests, funnel plot tests, and trim-and-fill analyses for all meta-analyses are depicted in Table 3. Funnel plots of all associations are depicted in Appendix E.

The results of Egger's regression tests revealed no indications of funnel plot asymmetry and the funnel plot test proved significant for the association between parent-therapist alliance and child outcomes. Results of the trim-and-fill procedures indicated that for none of the associations, effect sizes were imputed on the left side of the plot. These findings showed no indication of the presence of publication bias (Table 3).

Comparing Our Findings with Prior Meta-Analyses

The main effect sizes for the alliance-outcome association were compared to main effect sizes found in previous meta-analyses, by using different inclusion criteria. After excluding studies in which alliance and outcome were measured

Table 3 Results for Publication Bias Analyses

Type of association	# k (# ES)	Funnel plot test	Egger's test	Trim-and-fill analyses		
				# ES imp. L	# ES imp. R	# ES > cutoff
Child alliance – child outcomes	66 (399)	$z = -0.50, p = .62$	$z = 0.19, p = .85$	0	6	no
Child alliance change – child outcomes	23 (62)	$z = 0.01, p = .99$	$z = 1.01, p = .31$	0	3	no
Child-therapist alliance agreement – child outcomes	6 (27)	$z = 0.13, p = .90$	$z = -0.37, p = .71$	0	1	no
Moderating effect of child alliance on child outcomes	11 (67)	$z = 1.35, p = .18$	$z = -1.26, p = .21$	0	0	no
Parent alliance – child outcomes	38 (129)	$z = 2.54, p = .01$	$z = 1.48, p = .14$	0	3	no
Parent alliance – parent outcomes	14 (86)	$z = 0.84, p = .40$	$z = -0.55, p = .58$	0	1	no

This estimate is based on the population ES and power (number of effect sizes)

#k number of studies, #ES number of effect sizes, *imp. L* number of effect sizes on the left side of the funnel plot distribution, *imp. R* number of effect sizes on the right side of the funnel plot distribution, #ES > cutoff the number of imputed ES on the left side in relation to the cutoff value of the estimator of the trim-and-fill method proposed by Fernández-Castilla et al. (2021)

concurrently in order to increase comparability with the meta-analysis of Karver et al. (2018), the mean effect size of the association between child-therapist alliance and child outcomes ($r=0.17$, 95% CI=0.13, 0.21, $p<.001$, $k=60$, 256 ESs), and the association between parent-therapist alliance and child outcomes did not change ($r=0.11$, 95% CI=0.07, 0.16, $p<0.001$, $k=34$, 90 ESs). In order to compare our results with findings of Murphy and Hutton (2018), we excluded studies that used other than child self-report alliance ratings, studies that focused on children (mean age below 12 years), and studies focusing on dropout, treatment satisfaction, and delinquency. The mean effect size for the alliance-outcome association did increase ($r=0.22$, 95% CI=0.16, 0.27, $p<0.001$, $k=32$, 125 ESs), however, based on the overlap of confidence intervals, this increase was non-significant.

Discussion

A series of meta-analyses was conducted to provide a comprehensive overview of the alliance-outcome association in child and adolescent psychotherapy by examining several types of alliance-outcome associations. We found that associations between child-therapist alliance and child outcomes, changes in child-therapist alliance and child outcomes, child-therapist alliance as a moderator of child outcomes, and parent-therapist alliance and child outcomes were small ($r=0.09$ to $r=0.19$). Associations between child-therapist alliance agreement and outcomes and between parent-therapist alliance and parent outcomes were moderate ($r=0.21$ and $r=0.24$, respectively). Ratings of alliance and outcome by the same informant was a consistent moderator across different alliance-outcome associations, showing larger effect sizes for studies in which alliance and outcomes were rated by the same informant. Also, alliance rater (larger effects for child self-report ratings compared to observer ratings), problem type (larger effects for externalizing behavior than for internalizing problems), and outcome domain (larger effects for treatment satisfaction compared to other types of outcome) were significant moderators of the association between child-therapist alliance and child outcomes.

The overall effect size estimate of the child-therapist alliance-outcome association ($r=0.17$) is comparable to previous estimates ($r=0.20$; Karver et al., 2019; $r=0.14$; McLeod, 2011; $r=0.19$; Shirk & Karver, 2011). A novelty of the present study is that alliance change and congruence of child-therapist alliance scores (as opposed to measurement of a single perspective) were examined in relation to outcomes. Based on the notion that positive alliance shifts throughout treatment would be associated with better outcomes, we expected this association to be stronger than alliance measured at a single time point. However, the overall

effect size was also small ($r=0.19$). Studies on the association between alliance congruence and treatment outcomes are currently few in number, although alliance congruence is viewed as an important and perhaps better indicator of child-therapist alliance. The overall effect size found in this study was small to moderate ($r=0.21$). The association between parent-therapist alliance and child outcomes found in the present study ($r=0.13$) was smaller than in the most recent meta-analysis by Karver et al. ($r=0.24$; 2019). Of note, the number of effect sizes for parent-therapist alliance included in our sample was substantially larger (128 ES in our study vs. 54 in the study by Karver et al.). To conclude, the present meta-analytic review shows that the alliance-outcome association in child and adolescent psychotherapy is generally small to moderate, depending on the type of alliance being measured (child-therapist vs. parent-therapist alliance), and whether the alliance was measured at a single time point, as alliance change during therapy, or as alliance congruence.

The effect size for the association between child-therapist alliance and child outcomes found in the present study was smaller than the alliance-outcome association found in adult psychotherapy (r ranging from 0.09 to 0.21 in the present study vs. $r=0.28$, Flückiger et al., 2019; $r=0.21$, Horvath & Bedi, 2002; $r=0.28$, Horvath et al., 2011; $r=0.22$, Martin et al., 2000, in adult populations). McLeod (2011) stated that comparing findings between meta-analyses on the alliance-outcome association in child and adult psychotherapy is difficult due to the variability in treatments in child and adolescent psychotherapy. Another problem regarding the comparison of meta-analyses on alliance and outcomes between child and adult psychotherapy are the different inclusion criteria that are used in meta-analyses. An important difference is that meta-analyses on the alliance-outcome association in adult populations have almost exclusively focused on individual therapy, and only included studies in which alliance was measured prior to outcome, whereas most meta-analyses in child populations included studies in which alliance and outcome were measured concurrently. Shirk et al. (2011) addressed this problem by conducting a meta-analysis based on inclusion criteria used in adult studies (i.e., focusing on individual therapy and prospective alliance-outcome studies), and found only a slightly larger mean effect size of $r=0.22$. In the meta-analysis of McLeod (2011), the overall mean effect size of $r=0.14$ did not change after applying more strict inclusion criteria. More recently, Karver et al. (2018) found an overall effect size of $r=0.19$, focusing on the prospective alliance-outcome association. We also tested whether the alliance-outcome effect sizes changed after applying more strict inclusion criteria, resulting in a mean effect size of $r=0.17$. In addition, although the same differences apply, our finding regarding the association between parent-therapist alliance and parent outcomes ($r=0.24$) is

comparable to the effect sizes found in adult populations. It can therefore be concluded that the association between child-therapist alliance and child outcomes is somewhat smaller than for the adult-therapist alliance.

The small range of alliance-outcome associations in child and adolescent psychotherapy may be partly explained by conceptual differences between the alliance concept in adult psychotherapy on the one hand and child and adolescent psychotherapy on the other hand. The child-therapist and parent-therapist alliance in the context of child therapy consist of different defining elements compared to the client-therapist alliance in adult psychotherapy. With regard to the operationalization of alliance in children and adolescents, several factor-analytic studies on youth self-report alliance measures have shown that Bordin's three-factor model could not be replicated, and that alliance may be best measured as a unidimensional construct (Cirasola et al., 2021; Ormhaug et al., 2015). Recent studies have also found that the alliance perceived by children may primarily be an affective instead of a cognitive construct (Ormhaug et al., 2015; Zack et al., 2007), and it has been proposed that children may not distinguish between the personal bond, task, and goal dimensions of the alliance (Shirk et al., 2010; Zack et al., 2007). Given that goal collaboration and goal consensus have been found to be important process factors in psychotherapy (Tryon et al., 2018), it is imperative to gain a more in-depth understanding of the alliance concept in child and adolescent psychotherapy in terms of the affective bond and collaboration in therapy. Of note, in child and adolescent therapy, it is of key importance that goal setting and consensus should not be restricted to the outcome of therapy, but placed in a broader social and developmental context with respect to long-term positive developmental outcomes.

Notably, Bordin (1979), cited in Horvath (2018) proposed that 'different forms of therapy will demand different kinds of alliances, goals, and tasks specific to the kind of therapy' (Horvath, 2018, p. 509). Thus, it seems important to better understand what aspects of the alliance are important for children, adolescents, parent, and therapists in youth therapy in a particular setting in which treatment is delivered, such as individual outpatient treatment, home-based or community treatment, family therapy or treatment in an inpatient setting.

Several moderators of the association between child-therapist alliance and child outcomes that we found are consistent with findings of previous meta-analyses (Karver et al., 2018; McLeod, 2011; Shirk et al., 2011). The finding that the alliance-outcome association is stronger in children with externalizing problems is consistent with previous meta-analyses (McLeod, 2011; Shirk & Karver, 2003, 2011), suggesting that children with externalizing behavior may benefit more from a positive alliance with their therapist. Single-source ratings of alliance and outcome proved to be

a significant moderator, showing larger effects when alliance and outcome were measured by the same informant, which is consistent with results from previous meta-analyses. However, these larger effects may result from common method variance (Hoyt, 2002; Podsakoff et al., 2003). It is therefore plausible that stronger associations for self-report ratings and treatment satisfaction are a result of same-informant bias, which should therefore be interpreted with caution.

Age was also found to be a moderator (larger effects for adolescents than for primary school-aged children), but only for the alliance change – outcome association. This finding differs from the studies by McLeod (2011) and Shirk and Karver (2011) in which larger associations were found for children compared to adolescents. However, Karver et al.'s (2018) updated meta-analysis also did not find a moderating effect of age, suggesting that findings of alliance-outcome studies are mixed with regard to differences between age groups (Karver et al., 2018).

Alliance timing was identified in previous meta-analyses to moderate the alliance outcome association, showing stronger associations when the alliance was measured later in treatment, which is line with our meta-analytic results for the association between parent-therapist alliance and parent outcomes. No significant moderating effect was found for client-therapist ratio of the child-therapist alliance and child outcomes, which is consistent with a recent meta-analysis by Murphy and Hutton (2018). The client-therapist ratio may be seen as an index of the therapist's contribution to the alliance, and these findings thus imply that therapist variability in the child-therapist alliance is not related to child outcomes of therapy.

Considering the ongoing discussion in psychotherapy literature about the relative importance of the alliance as a common factor in therapy versus specific factors, such as standardized treatment protocols, it is assumed that therapists are less focused on the alliance when using treatment protocols, whereas in usual clinical care therapists may be more prone to work on the alliance with their client (Flückiger et al., 2012). It is therefore suggested that stronger alliance–outcome correlations would be expected in non-protocolized care as usual compared to protocolized treatments. Several meta-analyses on the alliance-outcome association in adults have addressed this issue, concluding that research design, use of treatment manual, and type of treatment (CBT vs. non-CBT) did not moderate the alliance-outcome association (Flückiger et al. 2012, 2019). In the present meta-analysis, use of a treatment manual and whether treatment was CBT did not moderate the alliance-outcome association.

In youth psychotherapy literature, studies on the effectiveness of youth interventions, treatment-specific elements such as treatment protocols on the one hand, and therapy process factors such as therapeutic alliance on the other

hand, are usually not studied simultaneously, but as distinct mechanisms of change. This is also reflected in the large number of studies in the present review in which the sample was drawn from RCTs ($k = 41$), originally focusing on therapy outcomes. It should be noted that psychotherapy outcomes for both internalizing and externalizing problem behavior in child and adolescent therapy are generally small (Cuijpers et al., 2020; Weisz et al., 2017), and may even disappear in the case of comorbid and complex problems of youth (Weisz et al., 2013, 2017). This may indicate that an ‘upper limit’ may have been reached with respect to what could be expected in terms of positive effects of interventions in youth populations (Jones et al., 2019; Weisz et al., 2019). Given that effect size estimates in meta-analyses on the alliance-outcome association in youth are also generally small, it seems plausible to suggest that the alliance-outcome association in child and adolescent psychotherapy likewise indicates an upper limit with respect to the magnitude of this association.

This means that the attention in alliance and outcome research in child and adolescent psychotherapy should partly shift toward a more holistic understanding of change mechanisms that lead to positive outcomes in youth. Understanding the interplay between process factors on the one hand, and protocolized treatment and therapeutic techniques on the other hand, and how these factors work in concurrence with client and therapist factors could help tailor treatment to the individual needs of the child. Therefore, research on psychotherapy outcomes should focus on rigorous (multi-modal) interventions, carried out with high levels of treatment integrity (Goense et al., 2016), which target specific (multiple) psychological and social problems over extend periods of time under clinically representative conditions, taking into account factors related to the complex interaction between therapist and client(s), in particular their alliance, and the context of treatment, including relevant common and specific therapeutic factors that have been shown to affect treatment outcomes (contextual model; Wampold & Imel, 2015).

Limitations and Strengths

The current study has various limitations. In contrast to the association between alliance measured at a single time point and treatment outcome, it is difficult to parameterize alliance change as well as alliance congruence to assess the bivariate association with outcome. We chose to include these studies in our meta-analysis on the association between alliance change and outcomes in order to compare this effect size estimate to the association between alliance measured at single time points in relation to outcome. An important methodological limitation

of the present study is that the analyses on the associations between alliance congruence and child outcomes and the role of the alliance as a moderator of outcomes contained few studies, which makes it difficult to draw a valid and reliable conclusion from the results of these analyses. Another shortcoming pertains to the operationalization of alliance changes (or alliance ‘shifts’) in relation to outcomes. It seems that alliance change has not been consistently operationalized in literature, and that parameterization of alliance change varies across studies. We identified seven studies that used robust methods to examine alliance change in relation to outcomes, thus methodologically robust alliance change-outcome studies are currently few in number, which also limits the interpretation of the findings of our meta-analysis. Nonetheless, these findings offer a first insight in the present state of alliance-outcome research in child and adolescent psychotherapy with respect to the different operationalizations of the alliance in relation to outcomes.

Further, in the present study, 20% of the studies were double coded. While this endeavour resulted in acceptable inter-coder agreement, given the scale of the present meta-analysis, coding procedures might still be subject to coding discrepancies. Another limitation is that some categorical moderators contained relatively few studies, which resulted in insufficient statistical power to detect relatively small (significant) effects. Moderator analyses that were based on small numbers of level-2 and level-3 units need to be interpreted with caution, because the maximum likelihood estimation method, which is used in our meta-analysis, requires relatively large sample sizes. Also, testing multiple moderators can result in chance capitalization. Therefore, effects of moderator analyses that were just significant should be interpreted with caution.

To increase comparability with previous meta-analyses, we only included studies that assessed the alliance between child or parent and therapist and excluded other types of alliances. This means that the alliance between the therapist and the family as a whole was not included in our meta-analysis. A recent meta-analysis on this type of alliance found an overall small-to-medium ES of $r = 0.18$ (Welmers-Van de Poll et al., 2018). Also, in recent years, studies have been conducted on the alliance between child or parent and the treatment team as a whole – often found in residential treatment – and its relation to outcome (e.g., Lamers, 2016; Nevid et al., 2016; Riencke et al., 2016). These studies were also not included in the current meta-analysis, because these types of alliance are not comparable to the individual child-therapist or parent-therapist alliance.

We chose to include effect sizes of bivariate alliance-outcome associations, and exclude effect sizes that were the result of multivariate analyses. We contacted

authors of studies in which bivariate associations were not reported, but in most cases we were not able to obtain data from them to calculate an effect size, and thus were forced to exclude these studies from our sample. Another problem in general regarding the in- and exclusion of effect sizes pertains to the increased use of sophisticated analyses in primary studies, such as multilevel modeling and growth curve modeling to examine the alliance-outcome association. Karver et al. (2018) stated that while this development is of course beneficial for psychotherapy research, it may hinder meta-analytic studies with regard to calculation of effect sizes if primary studies do not include information on bivariate associations in a correlation table.

We used the client-therapist ratio as a moderator of the alliance-outcome association in order to examine the variability in clients and therapists in relation to treatment outcomes. However, the client-therapist ratio is an indirect indicator of client and therapist variability (Del Re et al., 2012, 2021). To properly address patient and therapist variability in relation to treatment outcome, studies should examine the alliance-outcome association using multilevel modeling to account for the within-therapist (i.e., client level) and between-therapist (i.e., therapist level) effects. In our study sample, relatively few studies reported analytic strategies to account for clustering of the data (multiple clients treated by the same therapist).

To investigate the robustness of our findings, we applied several methods to address publication bias. The results indicated no presence of publication bias, although these findings should be interpreted with caution, because specific methods to take into account dependency of effect sizes in the assessment of publication bias in three-level meta-analysis are still under development (Assink & Wibbelink, 2016; Fernández-Castilla et al., 2021; Rodgers & Pustejovsky, 2021).

Despite these limitations, several strengths can be noted. The present meta-analysis included a substantially larger number of studies than previous meta-analyses, 99 in total of which 62 were included in the child-therapist alliance – outcome association. We adopted broad inclusion criteria, while also applying more stringent inclusion criteria to compare our results to other meta-analyses. An important strength is that we addressed several types of alliance-outcome associations: alliance measured at a single time point, alliance changes, alliance congruence, and the role of alliance as a moderator in relation to treatment outcome in separate meta-analyses, resulting in the most comprehensive overview of the alliance-outcome association in child and adolescent psychotherapy that is currently available. We used an advanced method of three-level meta-analysis in which all available effect sizes of included studies could be used, increasing statistical power of the moderator analyses.

Implications for Future Research and Clinical Practice

Several directions for future research should be acknowledged. Research on the conceptual differences between the client-therapist alliance in adult and child psychotherapy is needed to advance alliance research. Specifically, the child-therapist and parent-therapist alliance in the context of child therapy may consist of different defining elements, especially compared to the client-therapist alliance in adult psychotherapy. Research found some evidence to suggest that child, parent, and therapist perspectives on alliance overlap to some extent, but can be seen as different perspectives (Roest et al., 2022). It is therefore important that in examining the alliance-outcome association, multiple perspectives should be included as well as the congruence in alliance scores.

In the present study sample, few studies focused on alliance agreement between child and therapist in relation to treatment outcomes, although in recent years there has been increased attention for this particular aspect of alliance (Fjermestad et al., 2016; Ormhaug et al., 2015; Zandberg et al., 2015). This is an important development, given that the alliance should be acknowledged as a dyadic and dynamic concept. In doing so, it is important to utilize robust and appropriate statistical methods beyond change scores to operationalize discrepancies (Laird, 2020; Laird & De Los Reyes, 2013). Recent studies on the alliance in adult psychotherapy have focused on the alliance at a dyadic level, taking into account the interdependency of alliance ratings between client and therapist, as well as within and between person variance (Friedlander et al., 2012; Zilcha-Mano, 2016; Zilcha-Mano et al., 2016). These relatively new analytic approaches in alliance research should be applied to child psychotherapy in order to better understand the child-therapist and parent-therapist alliance. In addition, relatively few studies in alliance research in child psychotherapy have used statistical methods to account for therapist effects (i.e., multiple children are treated by the same therapists), when addressing the alliance-outcome association (e.g., Fjermestad et al., 2016; Marcus et al., 2011; Owen et al., 2016). Future studies should benefit from the available statistical methods to disentangle the alliance-outcome association.

It is important to note that a score on a continuous scale measuring a general alliance construct in children provides limited information on the dynamic and interpersonal aspects of the alliance. Moreover, client self-report alliance measures are prone to ceiling effects (Meier & Feeley, 2022), which could also be problematic in establishing an association between the alliance and other therapy processes and outcomes of therapy. In line with the need for a comprehensive conceptual understanding of the

alliance in child and adolescent psychotherapy, research on alliance discrepancies or agreement may also benefit from conceptualizing distinctive qualitative discrepancies and similarities between child, parent, and therapist perspectives of the alliance. Consequently, more in-depth knowledge with respect to qualitative distinctions between the child-therapist and parent-therapist alliance, what factors constitute a ‘weak’, ‘suboptimal’, or ‘strong’ alliance in child psychotherapy, including possible discrepancies between different perspectives, may advance alliance research in child and adolescent psychotherapy, including research on the alliance-outcome association. Also, research on alliance ruptures and rupture-repair processes in child and adolescent psychotherapy has gained traction, which could also provide new perspectives on how the alliance develops during the course of treatment (Cirasola et al., 2022; Nof et al., 2019).

It is argued that measurement of alliance at a single time point is inadequate to measure its relation with outcomes (Crits-Christoph et al., 2011; Doran, 2014). Relatively few studies in the current study sample reported on alliance changes and alliance shifts in relation to outcomes using robust methods to operationalize alliance change and examine its relation to outcomes. Most studies in the present meta-analysis measured alliance at a single time point or did not measure alliance more than three times throughout treatment. Recent studies have focused on the development of alliance trajectories or alliance patterns (e.g., linear, quadratic), although their relation to treatment outcomes has been rarely assessed (Chu et al., 2014; Halfon et al., 2019; Hudson et al., 2014). Future studies should address alliance changes and alliance trajectories in relation to outcomes to increase knowledge on the alliance-outcome association.

Finally, future research on the alliance-outcome association should focus on the relation between early symptom change and early alliance. In studying the alliance-outcome association, it is important to take into account the impact of early symptom change on early alliance, because early symptom improvement might lead to a positive alliance, which may overestimate the impact that alliance has on treatment outcome (Barber et al., 2010, 2014; Hendriksen et al., 2014; Xu & Tracey, 2015). This ‘reversed causation’ poses a threat to the validity of alliance as a predictor of treatment outcomes (Barber et al., 2010; Doran, 2014; Kazdin, 2009; Norcross & Lambert, 2014; Zilcha-Mano et al., 2014). In a recent meta-analysis on the alliance-outcome association in eating disorders, a reciprocal relation was found between alliance and outcomes, such that early symptom improvement was related to subsequent alliance quality, and that alliance was also related to subsequent symptom change (Graves et al., 2017). Few studies in our study sample addressed the reciprocal relation between (early) alliance and symptom change in child and adolescent psychotherapy, producing

mixed results (Capaldi et al., 2016; Cirasola et al., 2021; Chiu et al., 2009; Keeley et al., 2011; Liber et al., 2010; Marker et al., 2013; Ormhaug et al., 2014; Reyes, 2013). At present, there is limited evidence that early symptom change predicts early alliance, and there is some evidence that early alliance predicts subsequent symptom change, controlling for previous symptom change.

Our findings have some implications for clinical practice. Despite the fact that the overall effect sizes of the alliance-outcome associations are in the small to moderate range, given that alliance quality is significantly associated with child outcomes, the alliance can be considered as an important factor in child and adolescent psychotherapy. Enabling children and parents to give feedback on the alliance with their therapist, and to actively discuss and reflect upon this process, could provide therapists and their clients insight into how children and parents perceive and experience the alliance. Also, discussing the alliance between all participants in therapy could prove helpful to deal with strengths and difficulties during the therapeutic process, and to prevent strains and ruptures in the alliance. Investing in the quality of the therapeutic alliance in child and adolescent psychotherapy should be considered at least as important as using a treatment protocol or specific well-established therapist techniques.

Supplementary Information The online version contains supplementary material available at <https://doi.org/10.1007/s10802-022-00986-2>.

Compliance with Ethical Standards

Funding The authors did not receive any funding or specific grants for the submitted work.

Conflict of Interest The authors have no relevant financial or non-financial interests to disclose.

Ethics Approval Not applicable.

Consent to Participate Not applicable.

Consent to Publish Not applicable.

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