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Alliance in Group Therapy: A Meta-Analysis

Cameron T. Alldredge, Gary M. Burlingame, and Chongming Yang
Brigham Young University

Jenny Rosendahl
Jena University Hospital, Jena, Germany

Objective: Alliance is considered to be one of the oldest, most well-researched themes in psychotherapy. This article presents a brief history of alliance, its most common definitions and measures used in group treatment, and a meta-analytic review of the alliance-outcome relationship in group therapy. ***Method:*** Major databases were searched for articles published between 1969 and 2019. We conducted a random effects meta-analysis of published articles that reported the relation between alliance and treatment outcome within the group format. ***Results:*** Results from 29 studies including 3,628 patients indicate that the weighted average correlation between alliance and treatment outcome is significant $r = .17$ ($p < .001$), with a small effect size (corresponding to $d = 0.34$). Heterogeneity of effect sizes was low ($Q = 36.47$, $df = 28$, $p = .131$, $I^2 = 23.2$). Two variables were found to moderate the alliance-outcome association (treatment orientation and reporting perspective of alliance). ***Conclusion:*** With a substantial link between alliance and outcome established, the importance of therapeutic alliance rupture and repair in group therapy is discussed and treatment guidelines are outlined.

Highlights and Implications

- In reviewing 29 articles looking at the member–leader therapeutic alliance-outcome relation in group therapy, evidence was found to suggest that alliance is a significant predictor of outcome.
- Compared to alliance in individual therapy, alliance in group has a smaller effect on therapeutic outcome. This may be due to the complex dynamics found in group, whereas alliance is the only relationship in the individual format.
- Two variables were found to be significant, positive moderators of the alliance-outcome relation: treatment orientation and reporting perspective of alliance.
- Because there is a substantial link between alliance and outcome in group therapy, it is important for therapists to routinely measure it and repair ruptures as they occur.

Keywords: alliance, member–leader alliance, rupture and repair, group therapy, group treatment

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Although group treatment contains various forms of relationships unique to the format (e.g., member-to-member, member-to-group), group and individual therapy share alliance as a common, therapeutic relationship. Broadly speaking, alliance (also called working alliance,

Cameron T. Alldredge and Gary M. Burlingame, Department of Clinical Psychology, Brigham Young University; Chongming Yang, College of Family Home and Social Sciences, Brigham Young University; Jenny Rosendahl, Institute of Psychosocial Medicine and Psychotherapy, Jena University Hospital, Jena, Germany.

Correspondence concerning this article should be addressed to Cameron T. Alldredge, Department of Clinical Psychology, Brigham Young University, 238 Taylor Building, 1190 North 900 East, Provo, UT 84604. E-mail: cam.alldredge@byu.edu

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therapeutic alliance, or helping alliance) refers to the relationship between a patient and therapist. This relationship is considered to be one of the most well-researched and oldest themes in psychotherapy since its initial use in Zetzel's (1956) article describing the relationship between patients and psychoanalysts to accomplish tasks (Norcross & Lambert, 2018). The large empirical base for alliance, its ubiquity, and its robust association with treatment outcomes have contributed to its establishment as a common factor in psychotherapy (Flückiger, Del Re, Wampold, Symonds, & Horvath, 2012; Frank & Frank, 1991; Horvath, 2011; Wampold & Imel, 2015).

Past meta-analyses concerning alliance in individual therapy have consistently shown alliance to be predictive of therapeutic outcome (Flückiger, Del Re, Wampold, & Horvath, 2018; Horvath, Del Re, Flückiger, & Symonds, 2011). More specifically, Flückiger et al.'s most recent meta-analysis (2018) suggests that the alliance-outcome relation accounts for approximately 8% of the variability of treatment outcomes. In the individual literature, high levels of heterogeneity ($I^2 = 70.8$) have been prospectively attributed to various moderators such as publication year of the study, patient diagnosis, alliance measure, treatment type, source of outcome data, reporting perspective of alliance, time of the alliance assessment, outcome measure, type of research design, country of study, and specificity of outcome.

The question of whether alliance causes symptom change or if changes in symptoms lead to greater alliance is an important one which past research has investigated and found evidence to support a causal effect of alliance on symptom change (Zilcha-Mano, 2017). This finding increases the concern of measuring and monitoring therapeutic alliance throughout therapy. With a discrete and temporal link between alliance and outcome, it is important to consider ruptures in therapeutic alliance as potential threats to treatment success. The classic definition of rupture characterizes it as a strain in the alliance stemming from bonding issues or inconsistency between the therapist and patient on the goals and tasks of therapy (Safran & Muran, 2003). Building on Safran's extensive research on rupture and repair, Lo Coco, Tasca, Hewitt, Mikail, and Kivlighan (2019) outlined the complexity of this concept in group therapy. Eubanks, Muran, and Safran's (2018) findings in their meta-analysis on the association of alliance rupture-repair and outcome illustrate both the importance of tracking alliance and repairing ruptures as they occur to increase the likelihood of favorable treatment outcomes.

In the context of the group therapy literature, alliance has been used to describe various group relationships (e.g., member-group alliance). The majority of studies, however, tend to use alliance to represent the relationship between the individual members and the group leader or coleaders (Burlingame, MacKenzie, & Strauss, 2004). For the purpose of this meta-analysis and to maintain clarity, we will refer only to member–leader alliance.

Past research has implicated that relationship climate factors are determined to be more important to clients in group therapy compared to those in individual therapy (Holmes & Kivlighan, 2000). Additionally, alliance has been suggested to be one of the strongest predictors of group effectiveness (Burlingame, Fuhriman, & Johnson, 2004). Despite a strong foundation in individual therapy research, it seems member–leader alliance has been slower to gain traction within the group literature when compared to other dynamics such as group cohesion. Past research findings have suggested, however, that pretherapy training improves the quality of the therapeutic relationship throughout the duration of group treatment (Couch, 1995). Specifically, pretherapy training tends to provide greater ease with group therapy, promotes realistic expectations, and fosters a collaborative relationship with the group leader. Additionally, better therapist-rated improvement has been associated with members' experience of therapist warmth (Sexton, 1993). Despite some evidence pointing to alliance being an important predictor in group therapy outcomes, very little research has been done concerning effective ways to repair ruptures in group that may occur during the course of treatment.

Racial and ethnic differences in the development of alliance have also been examined for group therapy (Walling, Suvak, Howard, Taft, & Murphy, 2012). Specifically, the researchers found that race and ethnicity were not significantly associated with therapist ratings of alliance. Member ratings indicated that White group members reported a significant increase of alliance throughout therapy while minorities

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reported no change. Additionally, improvements were observed in both White and minority clients who reported an increase in therapeutic alliance during treatment. Whether or not the clients' race/ethnicity matched the leader or coleader did not affect client ratings of alliance.

A recent meta-analysis by Burlingame, McClendon, and Yang (2018) concluded that group cohesion was positively linked to therapeutic outcomes in both inpatient and outpatient settings. Their article implies that alliance and cohesion are frequently investigated in tandem, and they state that seven studies were excluded from their meta-analysis for being focused more on alliance than cohesion. Some have explored whether member–leader alliance in group is a different therapeutic relationship from cohesion (Bakali, Baldwin, & Lorentzen, 2009). Consistent with past findings, one recent study demonstrated the reality of group members having differential levels of bonding between the therapist and the other members in the group, which supports the idea of multilayered relationships within group that may or may not interact with their overall change and attitude toward the group (Kivlighan et al., 2017).

To our knowledge, a meta-analytic review of the alliance-outcome relation has not been conducted for group therapy. It is particularly relevant for this special issue to summarize what is known about the predictive nature of member–leader alliance with outcome that creates the empirical support for the importance of addressing alliance ruptures in group therapy. In this article, we discuss the main definitions and measures of alliance. We then present an original meta-analytic review of alliance's relation with outcome and discuss possible moderators. Treatment guidelines regarding alliance in group therapy will be provided. Our goal is to discuss alliance in its current state within the group literature, present the meta-analytic conclusions, and outline treatment practices to improve therapeutic outcomes.

Definitions and Measures

Since its debut in the 1950s, alliance was usually conceptualized in a psychodynamic framework and didn't gain much attention until the mid-1970s when Luborsky and Bordin began to suggest that it may be a common element to all helping relationships (Horvath, 2011). During that time, however, Greenson (1965) mentioned "working alliance" as a collaboration between client and therapist. Luborsky et al. (1996) suggested that alliance is dynamic and that there were two types of alliances found between a therapist and patient: one in the beginning of the relationship and the other emerging after some time of working together. The first is "based on the patient's experiencing the therapist as supportive and helpful with himself as a recipient" (p. 94) and the second is ". . . a sense of working together in a joint struggle against what is impeding the patient . . . on shared responsibility for working out treatment goals" (p. 94).

A few years later, Bordin (1979) suggested that the pantheoretical, working alliance is a combination of three main components: (a) an agreement between the therapist and patient on the goals of change, (b) the collaboration and assignment of tasks to be completed to work toward these goals, and (c) the development of bonds within the therapeutic relationship. Bordin (1989) speculated that the function of alliance was to help patients faithfully accept and follow treatment rather than owning its own therapeutic properties.

As implicated in Flückiger et al.'s (2018) meta-analysis, alliance is a living, evolving, and dynamic construct that can be perceived and reported differently throughout the course of therapy. Horvath (2011) indicates that the abundance of alliance measures that have emerged without consensus concerning what alliance is and is not has facilitated various conceptualizations of alliance. He implies that, "these alliances are pan-theoretical—in the plural, but unique to each assessment—in singular" (p. 128). Thus, differences in definition, lack of precision, and a plethora of measures have contributed to the difficulty of accurately pinpointing alliance's effect on therapeutic outcomes. Although most conceptualizations and measures have relatively fair amounts of overlap, it is difficult to clearly understand alliance's impact without broader consensus.

Past research and meta-analyses have indicated a strong presence of certain "core measures" for measuring alliance (Flückiger et al., 2018; Horvath & Bedi, 2002; Horvath et al., 2011). These core measures consist of the California Psychotherapy Alliance Scale–Patient (CALPAS-P; Marmar, Horowitz, Weiss, &

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Marziali, 1986), the Helping Alliance Questionnaire (HAQ; Alexander & Luborsky, 1987), the Working Alliance Inventory (WAI; Horvath & Greenberg, 1989), and the Vanderbilt Psychotherapy Process Scale (VPPS; Suh, Strupp, & O'Malley, 1986). These measures have over three decades of use and have shown internal consistency ranging from .81 to .87. When compared to each other, however, shared variance has been suggested to be less than 50% (Horvath, 2009). This has been a focus of speculation concerning high levels of heterogeneity found in the meta-analyses.

Using these alliance measures, Hatcher and Barends (1996) found their common theme to be that of a "confident collaborative relationship." More specifically, this can be thought of as confidence in and commitment to a process which patients perceive has the potential to help them. To further illustrate the overlap between these measures, Flückiger et al. (2018) present specific items from each questionnaire concerning quality of collaboration such as "My therapist and I respect each other" (WAI-patient), "I feel I am working together with the therapist in a joint effort" (Helping Alliance Questionnaire—II patient), and "Did you feel that you were working together with your therapist, that the two of you were joined in a struggle to overcome your problems?" (CALPAS-patient).

When it comes to measuring alliance specifically in groups, the literature is a combination of studies using measures designed to assess alliance in groups (or multiperson therapy) and studies using measures originally designed to assess alliance in an individual format that were modified to be used in a group. For example, the CALPAS is a scale that has a group-specific measure modeled after it (CALPAS-G Kivlighan, Lo Coco, & Gullo, 2015). The CALPAS-G (Gaston & Marmar, 1993) items are grouped into four subscales including Patient Working Capacity (patients' ability to work actively and purposefully in treatment), Patient Commitment (patients' attitude toward therapy such as commitment or trust), Working Strategy Consensus (degree of agreement between members and therapist about the process of therapy), and Member Understanding and Involvement (members' empathic understanding and active involvement in the group). Despite having some group-specific alliance measures available, the majority of studies (90%) included here were comprised of either modified or unmodified measures originally developed for alliance in individual therapy. The frequent adaptation of such measures in a group context is not surprising and has been reported in past studies (Johnson, Burlingame, Olsen, Davies, & Gleave, 2005).

Meta-Analytic Review

For the present study, we relied upon past group therapy meta-analyses of cohesion in group to develop inclusion criteria (Burlingame et al., 2018; Burlingame, McClendon, & Alonso, 2011). These criteria included groups that were comprised of at least three members; met for the purpose of counseling, psychotherapy, or personal growth; used at least one quantitative measure of alliance and outcome; met at least four times (face-to-face); produced data that allowed the calculation of effect sizes as weighted correlations; and were reported in the English language.

Search Strategy

We identified potential articles by searching PsycINFO, MEDLINE, and Google Scholar for publications between January, 1969 and January, 2019. We also obtained the articles not included in Burlingame, McClendon, and Yang's (2018) meta-analysis for being too focused on alliance. Taken together, a total of 1,029 unduplicated abstracts were retrieved using the following search terms: group psychotherapy, group therapy, support groups, group counseling, alliance, working alliance, and therapeutic alliance. Each abstract was reviewed according to the inclusion criteria at which point the article was retrieved and underwent a full text review if deemed promising (see Figure 1). A total of 74 articles were included using this method. From there, we eliminated 45 articles. Reasons for eliminating the 45 papers included the following: no statistical comparison between alliance and outcome (21), the absence of a formal alliance measure (15), not meeting the predetermined criteria for group therapy (5), incomplete data to compute effect size (3), and measuring member–group alliance rather than member–leader alliance (1).

Taken together, the 29 included studies had a total of 3,628 patients and focused on the asso-

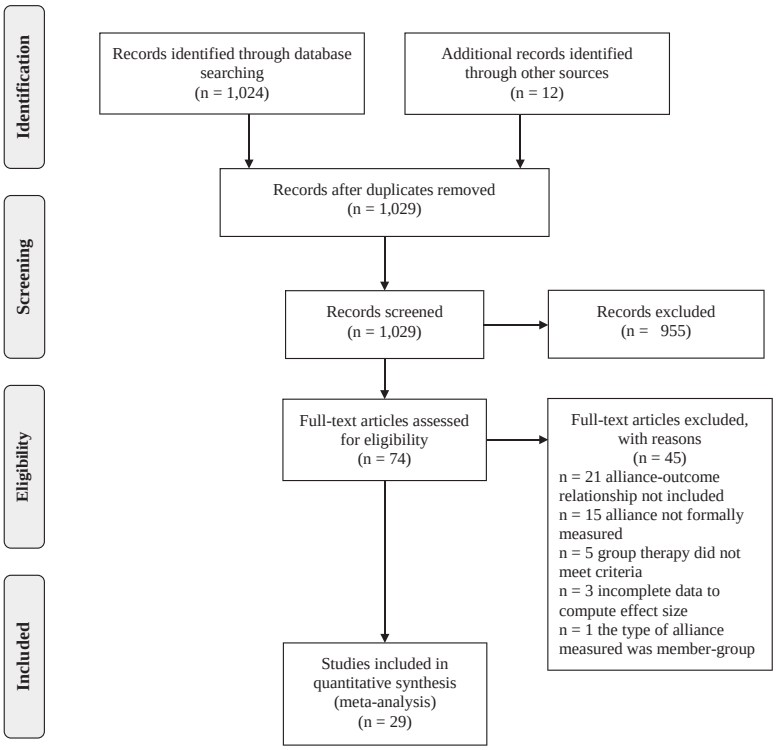


Figure 1. Flow chart of the inclusion/exclusion decisions.

ciation between therapeutic alliance and outcome. Study characteristics are presented in Table 1. Only two studies were published before 2000, with the median year of publication being 2011. The average number of sessions held by the groups in the studies was 15.6. Over half ($n = 16$) of the studies indicated that the groups followed a cognitive–behavioral approach. The majority of studies ($n = 23$) used groups in an outpatient setting.

Coding and Analysis

We selected and coded 38 variables stemming from four main categories: study characteristics, leader characteristics, member characteristics, and group characteristics. Some of these variables have been found to moderate outcome in previous group therapy meta-analyses (Burlingame et al., 2016). More specifically, we used the same moderator variables reported in a recent study that tested the relationship between cohesion and outcome (Burlingame et al., 2018). Most of the variables were concerned with characteristics of the therapy groups. We were mainly interested in understanding the structure of the groups, how they were conducted, and demographic information of the individuals in them. Although not all variables were included in the final analyses due to too much missing data, the categories were as follows:

- Nine study characteristics as follows: year of publication, types of outcome measures used, which outcome measures were used, alliance measures used, rater perspective, how frequently the alliance measures were administered, when the alliance measures were administered, and whether treatment integrity was monitored via fidelity checks.
- Five leader characteristics as follows: number of therapists, therapist gender, clinical experience, therapists' professional degree, and single leader versus co-led groups.
- Seven member characteristics as follows: sample size, gender, age, race, diagnosis, whether they were concurrently in other

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Table 1
Characteristics of Studies Included in the Meta-Analysis

Variable	%	*N*
Number of studies		29
Year of publication median		2011
Average number of sessions		15.6
Theoretical orientation		
Cognitive behavioral	55%	16
Supportive/humanistic	14%	4
Interpersonal	7%	2
Psychodynamic	7%	2
Other	3%	1
Unknown	14%	4
Member gender		
Single-gender	17%	5
Mixed-gender	76%	22
Unknown	7%	2
Primary diagnosis		
Informal	28%	8
Anxiety disorder	17%	5
Mood disorder	7%	2
Eating disorder	3%	1
Substance disorder	11%	3
Other disorders	34%	10
Location		
Clinic or private practice	31%	9
University counseling center	17%	5
Community mental health	10%	3
Other	21%	6
Unknown	21%	6
Setting		
Outpatient	76%	22
Inpatient or mixed	14%	4
Other	10%	3
Leadership of Group		
Single leader	31%	9
Co-led by two or more leaders	45%	13
Mixed	10%	3
Unknown	14%	4

treatments (e.g., individual therapy), and attrition.

- Seventeen group characteristics as follows: orientation, whether a manual or model was used, format, type, open or closed membership, composition, size, number of different groups included, session length, session frequency, number of sessions, average number of sessions attended by members, entrance into treatment, setting, and location.

The present study used 10 raters (two graduate students and eight undergraduate students) trained on a codebook using studies not included in the analysis. They achieved an 85% criterion level of agreement with high interrater reliability. After achieving this criterion, raters were paired and independently coded identical articles in the meta-analysis so that each article was double-coded by two raters. Complete agreement was required, with discrepancies resolved by the graduate students and coauthors.

If groups were cofacilitated, the original studies usually averaged the ratings between the different leaders to create a single composite score of alliance. Because of this, we treated these averages the same way as when groups were led by only one therapist. We computed weighted average correlations and 95% confidence intervals for the association between alliance and outcome. If a study reported data on more than one association (e.g., for various outcome measures) data were first aggregated on single study-level. Dependency of data (Borenstein, Hedges, Higgins, & Rothstein, 2009) was accounted for and set at $r = .50$ (Wampold et al., 1997). Thus, only one effect size estimate per study was added to the analysis. All correlations were transformed to Fisher's (1924) *z* for the analyses. Since we expected a heterogeneous effect size distribution, a random-effects model was used for data aggregation. For interpretative purposes, *z*-values were transformed back to *r*. Effect size can be interpreted according to Cohen (1988) with 0.1, 0.3, and 0.5 as small, medium, and large effect sizes, respectively.

Between-study heterogeneity was tested with the χ^2-test (Cochrane's Q) and quantified using I^2, which gives the amount of between-study variability that cannot be explained by chance alone. I^2 values of 25%, 50% and 75% are commonly interpreted as low, moderate, and high levels of heterogeneity, respectively (Higgins, Thompson, Deeks, & Altman, 2003). We further calculated prediction intervals representing the possible underlying effect in a new study that is similar to the studies in the meta-analysis (Deeks, Higgins, & Altman, 2019; Riley, Higgins, & Deeks, 2011).

Risk of Bias Across Studies

Publication bias was assessed via visual examination of the funnel plot (see Figure 3), looking for asymmetry that might suggest specific omission of nonsignificant results. When

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funnel plot asymmetry was detected, a trim-and-fill analysis was carried out that added potentially missing studies to the meta-analytic model and gave an adjusted effect estimate including the studies added (Duval & Tweedie, 2000). Egger′s regression test was run to statistically analyze the relationship between study effect size and standard error (Egger, Davey Smith, Schneider, & Minder, 1997). Additionally, we computed Rosenthal's (1979) Fail-Safe *N* to estimate the number of unpublished studies with effect sizes of zero and similar sample sizes that would be needed to bring the mean effect size to nonsignificance. Effect sizes can be considered robust if the required number of unpublished studies is greater than or equal to $5n + 10$, where n is the number of studies in a meta-analysis (Rosenberg, 2005).

Additional Analyses

We conducted sensitivity analyses in order to test the robustness of findings, examining if meta-analytic results change when excluding outliers (defined as effect sizes with confidence intervals not overlapping with the confidence interval of the pooled effect; Cuijpers et al., 2014). Exploratory subgroup analyses and metaregression analyses were run to explain statistical heterogeneity (Thompson & Higgins, 2002). Differences between subgroups were tested only if $n \geq 5$. In cases of smaller subgroups, those groups were merged to contrast them against the largest subgroup or were otherwise excluded from analysis. All analyses were performed using Comprehensive Meta-Analysis software (Biostat. Inc., Version 3).

Results

The results of the current meta-analysis indicate complexity in the alliance–outcome association. Nearly half ($n = 14$) of the studies produced a statistically significant correlation between alliance and patient improvement, with four studies having p values in the trend range ($p < .10$). The weighted average correlation of the 29 studies including 3,628 patients was statistically significant, $r = .17$ (95% CI [.13, .21], $p < .001$), with a small effect (corresponding to $d = .34$) between alliance and outcome. Heterogeneity of effect sizes was low ($Q = 36.47$, $df = 28$, $p = .131$, $I^2 = 23.2\%$). The 95% prediction intervals representing the possible underlying effect in a new study that is similar to the studies in the meta-analysis was [0.06, 0.28].

We have included a typical forest plot (see Figure 2) that can assist readers in determining whether an individual study favored a positive or negative relationship between alliance and outcome. This plot is also useful in identifying outliers on either side of the weighted aggregate correlation of $r = .17$. Two statistical outliers were identified with confidence intervals not overlapping with the confidence interval of the pooled effect: positive outlier = Shechtman and Katz (2007); negative outlier = Petry, Weinstock, and Alessi (2011); results do not change when outliers are excluded, but heterogeneity is largely reduced (Supplementary Table 1).

Risk of Bias Across Studies

Visual inspection of the funnel plot provided an asymmetry of studies (see Figure 3). trim-and-fill analysis revealed that 11 studies are missing which indicates a risk for publication bias. The adjusted value including the "missing" studies is somewhat smaller ($r = .13$ [0.08; 0.17]) than the observed value, but conclusions do not change (adjusted value is still significantly larger than 0). Egger′s regression test also indicates a risk for publication bias ($\beta = 1.62$; $t(27) = 3.70$; $p < .001$). Classic fail-safe *N* analysis shows that despite missing studies, the result is robust as 746 new studies would be needed to bring the p value above 0.05.

Moderator Analyses

Subgroup analyses revealed differences in size of alliance-outcome correlations with respect to treatment orientation and reporting perspective of alliance (see Table 2). Correlations between alliance and outcome were significantly smaller for patients treated with cognitive–behavioral group approaches compared to those being treated with other approaches, and larger for patient-reported alliance in comparison to mixed (both therapist and patient) and outside observer.

All other tested moderator variables (based on the $n \geq 5$ criteria) did not result in significant subgroup differences (Supplementary Table 2). Notable nonsignificant study characteristics include variables such as alliance measure type, whether the alliance measure was modified for group, tracking fidelity to a treatment protocol,

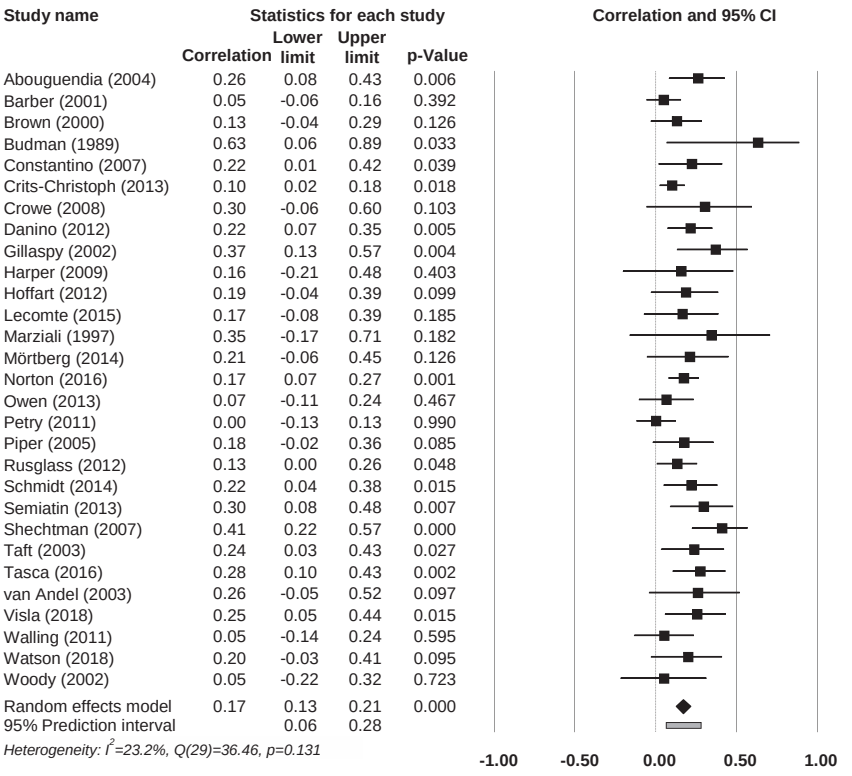


Figure 2. Forest plot of study effects and total effect.

and how often alliance was measured and when. Nonsignificant member variables included client age, race, diagnosis, and attrition rate. Leader variables such as gender, level of experience, and whether coleaders led the groups were also found to be nonsignificant. Nonmoderating group variables included leadership type, whether small group process was emphasized, whether the groups were model-based or manual-based, entrance into the groups, the group format, the number and length of sessions, and location.

Discussion

We believe that our meta-analysis is the first systematic attempt to estimate the correlation between member–leader alliance and outcome in group treatment. A statistically significant correlation between alliance and patient improvement was produced in nearly half of the studies with four additional studies having *p* values in the trend range. This pattern of significant and trend studies supporting a significant alliance-outcome relationship is nearly identical to the findings of Burlingame et al.'s (2018) cohesion meta-analysis which had a much larger sample size. The pattern herein suggests that more often than not, alliance is associated with treatment outcome in group therapy, which empirically urges the group leader to monitor and detect alliance ruptures.

We found a weighted average alliance-outcome correlation of $r = .17$ representing a significantly positive, small effect which has been proven to be fairly robust. Since cohesion is a similar group dynamic relationship construct, we compared our weighted average alliance-outcome correlation ($r = .17$) with the findings of Burlingame et al.'s (2018) meta-analysis that provides a weighted aggregate correlation for cohesion-outcome. Our weighted alliance correlation's confidence interval (.13-.21) overlaps with the confidence interval (.20–.31) of the cohesion-outcome meta-analysis. Concerning effect sizes, the reported cohesion-outcome effect size was medium ($d =$

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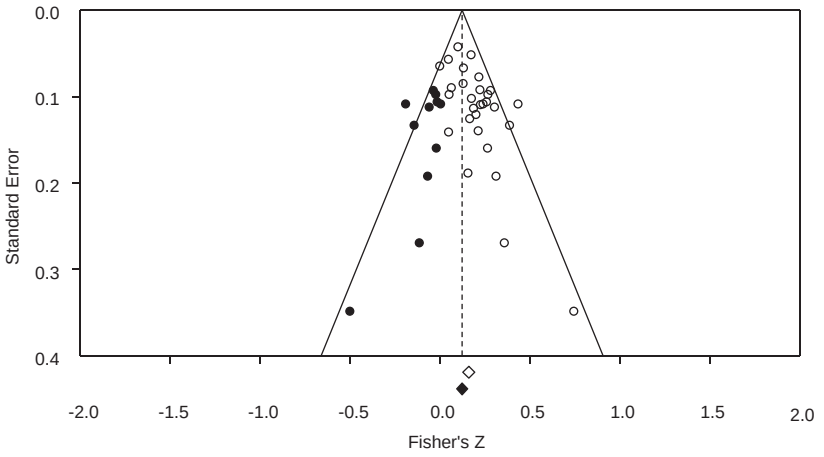


Figure 3. Funnel plot of standard error by Fisher's Z. Filled dots (11) represent "missing" studies.

.56) while the current study found that the alliance-outcome effect size is small (d = .38). From this, we see that the member–leader relationship and group relationship (member-member and member-group) are both predictive of outcome although group cohesion appears to be stronger than alliance. This is noteworthy for several reasons.

First, cohesion's long history of empirical inquiry in the group literature and has simply produced more studies over time than any other group dynamic construct (Burlingame et al., 2018). Stated differently, we have more empirical support regarding the importance of member–member and member–group relationships predicting treatment outcome than we do about the member–leader relationship. The fact that the alliance-outcome and cohesion-outcome confidence intervals overlap provides empirical support that they are both important, which provides an empirical reason for the value of a special issue on alliance rupture.

Second, these findings potentially clarify mixed alliance results published over the past few decades. In a recent paper on alliance rupture and repair in group therapy, Lo Coco and colleagues (2019) noted the complexity inherent in alliance rupture and repair due to the multifaceted nature of relationships in group treatment—member–member, member–group and member–leader. Further complexity is added when some studies (Marziali, Munroe-Blum, & McCleary, 1997) suggest that the member–leader relationship is a better predictor of member improvement and others show the opposite pattern (Crowe & Grenyer, 2008). Meta-analytic findings are often considered to

Table 2
Alliance-Outcome Correlations for Different Subgroups

Subgroups	*n*	*r*	[95% *CI*]	*p* (*r*)	Heterogeneity *Q*	Heterogeneity *p* (*Q*)	Heterogeneity I^2	p_{diff}
Theoretical orientation								.013
CBT	16	0.14	[0.10, 0.18]	< .001	14.52	.486	0%	
All others reported	9	0.23	[0.17, 0.29]	< .001	7.75	.459	0%	
Reporting perspective[a]								.049
Patient	19	0.19	[0.15, 0.24]	< .001	25.79	.105	30%	
Mixed[b] or observer	10	0.11	[0.03, 0.18]	.005	6.38	0.604	0%	

Note. CBT = cognitive– behavioral therapy; n = number of studies.
[a] studies with missing information on the respective variable were excluded. [b] both patients and therapists rated alliance.

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produce more reliable findings than single studies and the overlapping confidence intervals between group (member–member, member–group) and member–leader relationships suggests that both are important.

Further clarity may be added by comparing the results of this meta-analysis with findings from 295 individual therapy alliance studies (Flückiger et al., 2018). More specifically, their meta-analysis reports an alliance-outcome estimate ($r = .29$) and confidence interval (.26–.30) that does not overlap with findings from our study. While there are obvious limitations in directly comparing these two meta-analyses due to confounds inherent in between-study comparisons (Burlingame et al., 2016; Shadish & Sweeney, 1991), it is important to note that both meta-analyses used identical alliance measures and overlapping outcome instruments. One might expect alliance to be more predictive in individual therapy compared to group therapy because it is essentially the only relationship possible for that format. Given the multifaceted nature of group therapy relationships, it may be that the alliance between member and leader while significant, simply accounts for less outcome variance than in individual therapy.

Another explanation for the lower alliance-outcome value is that the majority of studies herein used cognitive–behavioral therapy (CBT). In Burlingame et al.'s (2018) cohesion meta-analysis, which also showed that CBT studies produced a smaller effect size compared to other approaches, only 35% of the studies were CBT. Possible reasons for this are discussed in the moderator section below. However, quantitatively, we simply had a higher proportion of CBT studies in the present study compared to Burlingame et al. (2018), and since they post a lower average alliance-outcome correlation, this "confound" may have lowered our study average compared to the average that Burlingame et al. report for the cohesion-outcome relationship.

Finally, it's important to note that similar between-study limitations exist when alliance-outcome and cohesion-outcome meta-analytic results are compared. Stated differently, the same potential for between-study confounds noted above exist; however, additional limitations may be introduced by the comparison of different relationship constructs. Cohesion measures assess group therapy relationships (member–member, member–group) while the alliance measures typically assess the member–leader relationship. One way to control for these potential between-study and construct confounds is to identify if there is a sufficient number of published studies that assess multiple relationship constructs (member–member, member–group, and leader–member). A meta-analysis that estimates alliance-outcome and cohesion-outcome effects simultaneously examined in the same study (i.e., within-study comparisons) is the most direct method for replicating the above between-study effects and controlling for unwanted between-study and measure confounds. Future research should explore the feasibility of this analysis.

Alliance Rupture-Repair

We find it important to report on the presence of alliance rupture references among the studies included in this meta-analysis. Of the 29 studies, only three mentioned alliance rupture. Specifically, two studies (Tasca, Compare, Zarbo, & Brugnera, 2016; Vîslă et al., 2018) briefly mention alliance ruptures in the introduction and/or conclusion sections. The other study (Watson, Daffern, & Thomas, 2018) was the only study included that formally measured ruptures. They used the Postsession Questionnaire (PSQ; Muran, Safran, Samstag, & Winston, 1992), which contains a self-report measure of ruptures using a five-point Likert scale. In their analyses, they used these self-report ratings to create a variable with three classifications: no rupture, minor rupture, and major rupture. In their study, contrary to their hypothesis, they found that major ruptures were not associated with treatment gains. They concluded that although poor therapeutic alliance typically affects treatment outcomes, the presence of ruptures may not fully implicate a poor, overall therapeutic relationship. It may be that the multiple relationships inherent in group therapy—member–member, member–group and member–leader—create a buffer for rupture in a single relationship structure. Clearly, the sparse research on alliance rupture-repair in group therapy lends to the necessity of this special issue as well as the importance of future studies that compare ruptures that occur singly and simultaneously across the multiple relationships

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in group therapy and how this affects efforts of repair as well as outcome.

Moderator Analyses

We view the significant moderators with caution for a few reasons. First, the number of studies herein is approximately 10% of the those tested in the alliance individual therapy meta-analysis, making our analysis comparatively underpowered. Second, only one moderator in the original cohesion-outcome meta-analysis (Burlingame et al., 2011) was replicated when the sample size was increased by 40% (Burlingame et al., 2018). This suggests that the identification of significant moderators is susceptible to sample characteristics and size, and the study herein has a relatively small sample ($n = 29$). Indeed, an example of this was the disappearance of moderators in our analysis when we dropped an ineligible study, so this adds to our caution.

The only moderator that replicated the moderator findings from the cohesion meta-analysis was theoretical orientation and we found the same pattern—cognitive–behavioral studies produced smaller alliance-outcome correlations. We were surprised by this finding since CBT tends to be more leader-centric, with less emphasis on the interpersonal and interactive relationship between members. Indeed, the highest cohesion-outcome values were found with leaders who tried to foster interactive cohesive relationships, so we expected the leader-centric approach of CBT to place greater emphasis on alliance. Might the more structured, manualized characteristic of CBT put greater emphasis on the informational and skill-based interventions, thus neutralizing, to some extent, the member–leader relationship? Alternatively, might the other theoretical orientations herein put a greater emphasis on the member–leader relationship, leading to higher alliance-outcome effects? Clearly, these are speculative and await research, preferably from a single study to produce direct comparisons and ideally where leader effects are held constant (i.e., leaders fully crossed by treatment).

Although it was not significant in Burlingame et al.'s (2018) meta-analysis, we have some confidence in reporting perspective as a significant source moderator because there is simply more research to support evidence-based explanations. In the individual literature (Flückiger et al., 2018), a trend was found for lower alliance-outcome correlations when independent observers rated the alliance versus self-report ratings of alliance. Our findings partially replicate this trend in the lower alliance-outcome correlation found for the observer and mixed category compared to self-report (see Table 2). Further support for this moderator comes from therapeutic relationship research (Chapman et al., 2012), which shows group leader ratings of alliance only correlating with group member ratings toward the end of group treatment. It may simply take more time for a therapist or independent observer to accurately rate this construct, making member's ratings a more reliable and accurate index across an episode of group treatment. Finally, since most outcome measures also rely upon client self-report, the alliance-outcome correlation may be inflated by the use of a common source (Burlingame et al., 2004).

We believe that it is important to note moderators that were not significant, particularly those that replicate past meta-analytic findings from the cohesion-outcome and individual therapy alliance literature. The alliance-outcome correlation does not appear to vary by measure used, date of publication, age, diagnosis, single versus co-led groups, size and treatment dose. A cautionary note to this observation is the small sample size herein that may lead to underpowered moderator analyses. Nevertheless, the fact that these same moderators replicate past meta-analytic results are noteworthy.

Limitations

Our results have to be interpreted with caution since publication bias cannot be ruled out. There was substantial funnel plot asymmetry with 11 unpublished studies missing. Despite indications for publication bias, the alliance-outcome effect estimation seems to be quite robust as indicated by the trim-and-fill analysis and classic fail-safe *N* analysis. The most significant limitation, however, is the small sample size noted above—particularly when we compare our sample to the individual therapy meta-analysis. On the other hand, the overlapping confidence intervals with the recent cohesion-outcome meta-analysis provides some support for the accuracy of our alliance-outcome estimate. The small number of studies for each alliance measure lowers our confidence in our instrument moderator analysis. However, neither the individual alliance-outcome or

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cohesion-outcome meta-analyses found instrument moderation which attenuates our sample size concern. Sample size is clearly a concern in the moderator analysis of the leader and group characteristics (e.g., group type, homogeneity, dose, setting, size, etc.). Indeed, the recently updated cohesion-outcome meta-analysis (Burlingame et al., 2018) identified several new moderators compared to a previous meta-analysis (Burlingame et al., 2011) when 15 new studies were added. Finally, we realize the studies herein focus on establishing the relationship between alliance-outcome and that very few specifically focused on alliance-rupture. However, the findings of our study, when coupled with the clinical literature, provide foundational empirical support for monitoring alliance and engaging in alliance rupture repair.

Conclusions

Our results suggest that member–leader (alliance) therapeutic relationships and group cohesion (member–member and member– group relationships) have a similar correlation with outcome. Thus, rupture and repair in any of these relationships seems supported as an empirically sound strategy to improve member outcomes. Alliance-outcome correlations for group therapy were stronger when the member was the reporting source, which finds partial support in trends noted in individual therapy (Flückiger et al., 2018). Additionally, the alliance-outcome relation was weaker when CBT was the therapy orientation.

Other findings from related, single studies portend greater clarity regarding the multifaceted relationships is group therapy. As noted by Lo Coco et al. (2019), an important consideration in understanding the alliance-outcome relationship may be the convergence of member, group-as-a-whole, and leader ratings (Compare, Tasca, Lo Coco, & Kivlighan, 2016; Lo Coco, Gullo, & Kivlighan, 2012). Furthermore, when the group leader's perception of a member's alliance is independent of the group alliance, greater benefit may result (Kivlighan et al., 2017). Finally, there is considerable evidence in the individual therapy literature and modest research in the group therapy literature to suggest that therapist accuracy in predicting clients' perception of the therapeutic relationship is poor (Chapman et al., 2012). Thus, using measure-based interventions to identify alliance ruptures may be a more reliable method than relying upon therapist perception (Burlingame, Whitcomb, et al., 2018).

Taken together, these conclusions lend reason for group practitioners to develop, measure, and monitor therapeutic alliance in group treatment. Using self-reports of therapeutic alliance seem to be a more reliable way to gauge the strength of the member–leader relationship. Because of the predictive nature of alliance on treatment outcomes, ruptures in alliance are important to detect and repair. Actively striving to repair ruptures as they occur may lead to a better sense of alliance, which, in turn, will contribute to favorable treatment outcome.

Treatment Guidelines

Our meta-analysis of 29 studies involving over 3,600 group therapy clients has led to the following guidelines for group treatment:

- Alliance is reliably associated with and predictive of group therapy outcome ($r = .17$, $d = .34$). This finding lends empirical support to the importance of group clinicians to regularly monitor each group member's therapeutic alliance throughout treatment to detect ruptures and engage in repair, thereby increasing the likelihood of favorable outcomes.
- The alliance-outcome association is different depending on the theoretical orientation. Orientations that typically emphasize interpersonal interaction (e.g., interpersonal, supportive, psychodynamic) post a stronger alliance-outcome relationship when compared to a cognitive–behavioral orientation. Nonetheless, similar to Burlingame et al.'s (2018) cohesion meta-analysis, alliance is an empirically supported factor in each.
- Alliance is linked to outcome in both inpatient and outpatient settings. Additionally, there is no evidence concerning differential effects across diagnoses. Because of this, group leaders should actively strive to develop and maintain alliance with each group member regardless of setting and presenting problems.

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