Therapist effects in the therapeutic alliance-outcome relationship: A restricted-maximum likelihood meta-analysis.

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Abstract

Objective: Although the relationship between the therapeutic alliance and outcome has been supported consistently across several studies and meta-analyses, there is less known about how the patient and therapist contribute to this relationship. The purpose of this present meta-analysis was to (1) test for therapist effects in the alliance-outcome correlation and (2) extend the findings of previous research by examining several potential confounds/covariates of this relationship.

Method: A random effects analysis examined several moderators of the alliance-outcome correlation. These included (a) Patient-Therapist Ratio (Patient *N* divided by therapist *N*), (b) Alliance and Outcome Rater (patient, therapist, observer), (c) Alliance Measure, (d) Research Design and (e) DSM IV Axis II diagnosis.

Results: The Patient-Therapist Ratio (PTR) was a significant moderator of the alliance-outcome correlation. Controlling for several potential confounds in a multi-predictor meta-regression, including rater of alliance, research design, percentage of patient axis II diagnoses, rater of outcome and alliance measure, PTR remained a significant moderator of the alliance-outcome correlation.

Conclusion: Corroborating previous research, therapist variability in the alliance appears to be more important than patient variability for improved patient outcomes. This relationship remains significant even when simultaneously controlling for several potential covariates of this relationship.

Keywords: therapeutic alliance; therapist effects; alliance-outcome correlations; random effects meta-analysis

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The therapeutic alliance is considered to be an important aspect of successful treatment and has been found to be a consistent predictor of therapy outcomes in over 30-years of psychotherapy research (Horvath & Bedi, 2002; Horvath, Del Re, Flückiger, & Symonds, 2011; Horvath & Symonds, 1991; Martin, Garske, & Davis, 2000). A recent meta-analysis that examined the impact of the therapeutic alliance found that the alliance was a robust, albeit moderate (r = .275) predictor of treatment outcome, accounting for about eight percent of variability in outcomes (Horvath, et al., 2011). However, significant variability in this overall alliance-outcome relationship was found, likely due to examined (i.e., moderators, such as time of alliance measurement) and unexamined (e.g., multiple measures of alliance with lack of consensus on alliance operationalization) factors. The therapeutic alliance refers to the collaborative relationship between patient and therapist (Bordin, 1979; Hatcher & Barends, 2006), which is impacted by in-session responsiveness (i.e., process where behavior is influenced by emerging information in the therapy session; see Stiles, 2009; Stiles et al., 1998) between the therapeutic dyad. Although the field has yet to settle on a uniform definition of the concept (Fitzpatrick, Iwakabe, & Stalikas, 2005; Horvath, 2005), there seems to be convergence empirically and theoretically that the central aspects of the therapeutic alliance construct involve the bond between the therapist and the patient as well as agreement about the therapeutic goals and tasks (Hatcher & Barends, 1995, 2006; Horvath, 2002).

The influence of the alliance has also been examined in a variety of disorders. For example, its impact has been demonstrated in patients with depression (Krupnick et al., 1996; Raue, Goldfried, & Barkham, 1997), anxiety (Piper et al., 1995), PTSD (Cloitre, Chase, Miranda, & Chemtob, 2004), eating disorders (Constantino, Arnow, Blasey, & Argas, 2005), personality disorders (Andreoli et al., 1993; Strauss et al., 2006, Klein et al., 2003), and a number of other

disorders (for an overview see e.g., Castonguay & Beutler, 2006). As well, the alliance and outcome have shown a remarkably robust association (although perhaps not linear; see Stiles et al., 2004), with treatment outcomes across different moderating variables such as measures of the alliance, measures of outcomes (primary symptom measure and non-targeted measures), rating perspectives, type of treatment (e.g., evidence-based, manualized, focused on specific ingredients), and context in which treatment was delivered (e.g., RCT or not; Flückiger, Del Re, Wampold, Horvath, & Symonds, 2012; Horvath et al., 2011)

Despite the robust relationship with outcome across a number of different contexts, establishing causality is difficult as the alliance cannot be experimentally manipulated. As DeRubeis, Brotman, and Gibbons (2005) have argued (see also Strunk, Brotman, & DeRubeis, 2010), the alliance-outcome correlation may be due to (a) contributions of the patient, (b) contributions of the therapist, (c) the interaction of therapist and patient (i.e., the match), or (d) early change in functioning. For example, patients with good attachment histories and well developed social skills might well form better alliances and have better prognoses; consequently, the alliance-outcome correlation would be due to the patient's characteristics and not something that the therapist offers in treatment. Indeed, there are a number of studies that have shown that patients with better attachment histories or more adaptive attachment styles report better alliances with their therapists (Mallinckrodt, 1991; Mallinckrodt, Coble, & Gantt, 1995; Strauß & Schwark, 2007).

In contrast, others have argued that the alliance-outcome correlation is due, to a large extent, to the therapist. That is, therapists who consistently form better alliances with their patients generally achieve better outcomes. Several recent studies have investigated the impact of therapist versus patient variability in the alliance using multilevel methods and found evidence

suggesting that the therapist contribution is more critical than the patient contribution to the alliance-outcome correlation (Baldwin, Wampold, & Imel, 2007; Dinger, Strack, Leichsenring, Wilmers, & Schauenburg, 2008; Marcus, Kashy, Wintersteen, & Diamond, 2011; Zuroff, Kelly, Leybman, Blatt, & Wampold, 2010). For example, Dinger et al. (2008) studied an inpatient population and found significant therapist effects in the alliance-outcome correlation. Similarly, Baldwin et al. (2007) used multilevel modeling to examine the impact of the therapist on the alliance-outcome correlation and found that the therapist contribution to the alliance was a statistically significant predictor of outcome, whereas the patient contribution was not. The relationship between therapist and patient contributions to the alliance and outcome found by Baldwin et al. is presented in Panel (a) of Figure 1. In this figure, it is clear that those therapists who generally formed better alliances had better outcomes (correlation between the average alliance scores for a therapist and outcome adjusted for pretest scores was -.33). On the other hand, variability in alliances among patients within therapists was unrelated to outcome ($r = 0$). It is important to keep in mind that total alliance-outcome correlation in this study was -.24, which misrepresents both the within therapist and between therapist correlation (less than the between therapist correlation and greater than the within therapist—i.e. the patient contribution—correlation).

Although these recent findings that support the importance of therapist effects in the alliance-outcome correlation are informative, the evidence cannot be characterized as conclusive at this point in time for several reasons. Most importantly, the studies that used multilevel modeling to disentangle patient and therapist contributions have not been extensively replicated and may be idiosyncratic to the study characteristics (e.g., populations, methods) or simply may be instances of Type I errors (i.e., falsely rejecting the null hypothesis). For example, Baldwin et

al. (2007) used a sample derived from college counseling centers and the alliance and outcome measures were from the same perspective (viz., the patient), raising the possibility of mono-method bias (Hoyt, 2002). Dinger et al.'s (2008) sample consisted of inpatients receiving psychodynamic therapy, along with other services, and only the patient rated alliance was used, and thus it is not clear how much of the treatment impact can be attributed to the therapist factors. Zuroff et al. (2010) was a reanalysis of the National Institute of Mental Health Treatment of Depression Collaborative Research Program; these data has been analyzed in a variety of ways, and many of these re-analyses have resulted in contradictory conclusions (cf., Kim, Wampold, & Bolt, 2006; Elkin, Falconnier, Martinovich, & Mahoney, 2006).

A preferred method to synthesize primary studies and investigate aggregate effects, including the impact of moderators, is meta-analysis (Hunt, 1997; Mann, 1994). However, appropriately conducting a meta-analysis can be challenging, as the method requires a sufficient number of studies and for those studies to provide the relevant data to synthesize the results. Unfortunately, until only fairly recently have there been appropriate methods (and knowledge to implement these methods) to examine the relative contributions of therapist and patient variability in the alliance on outcome (i.e., multilevel modeling). Therefore, we could only identify a limited number of studies that permitted direct evaluation of the therapist contributions vis-à-vis the patient's contribution to the alliance-outcome correlation. Because of this paucity of studies, we developed an alternative meta-analytic strategy that enabled us to use a more broad selection of investigations providing a better representation of the universe of studies that have examined the correlation of the alliance with outcome.

The meta-analytic strategy in the present study involved examining the ratio of the number of patients to the number of therapists (i.e., patient N divided by therapist N, denoted by

PTR) as an indicator of research design properties of the included studies, which in turn allowed for examination of therapist and patient contributions to the alliance-outcome correlation. The PTR refers to the design of the studies that investigated the alliance-outcome correlation, yet is an index of the degree to which therapist and patient variability in the alliance was related to outcome. In Figure 1, the Baldwin et al. (2006) results (panel a) is illustrated as well as hypothetical studies with high and low PTRs, as illustrated in panels (b) and (c) of Figure 1. If the ratio is large (many patients and few therapists), then most of variability in the alliance would be due to the patients. The extreme example of a study with a high PTR is illustrated in panel (b) where all N patients are seeing the same therapist. In such a study, there is no therapist variability in the alliance because there is only one therapist; all of the variability in the alliance in such a study is between patient variability (i.e., all variability is within a single therapist). If the Baldwin et al. (2007) result that within therapist variability is not related to outcome is replicated, then the correlation of the alliance and outcome (i.e., the total alliance/outcome correlation) in this study would be zero because all the variability in the alliance is due to the patients. On the other hand, if the ratio is small (i.e., few patients per therapist), then a greater proportion of the variability in the alliance will be due to the therapists. A study with a PTR of 1 (viz., one patient per therapist) is illustrated in panel (c) and variability in the alliance is due to between therapist differences (no variability in the alliance within therapists)[1]. If the therapists' contribution to the alliance is critical, as Baldwin et al. found, then it would be expected that the total alliance-outcome correlation in studies with relative small ratios would be relatively large because the variability in the alliance in such studies is mostly due to therapist differences. According to Baldwin et al., in a study in which the PTR approaches 1, the total correlation should approach the value of -.33. In this way, the PTR reflects a decomposition of the total alliance-outcome

correlation into within- (patient-level correlation) and between-therapist (therapist-level correlation) components.

The purpose of the current meta-analysis was to test therapist effects in the alliance meta-analytically by using the ratio of patients to therapists as a moderator of the strength of the alliance-outcome correlation. It is hypothesized that there will be an inverse relation between the PTR and the size of the alliance-outcome correlation.

Because there are several study characteristics that might be related to the ratio, we attempted to rule out these confounding variables. Specifically, the following variables were also examined to see if they moderated the hypothesized relationship between the patient-therapist ratio and the alliance-outcome correlation: (a) alliance rater (patient, therapist, observer), (b) alliance measure (Working Alliance Inventory [WAI], California Psychotherapy Alliance Scale [CALPAS], Helping Alliance Questionnaire [HAQ], Vanderbilt Psychotherapy Process Scale [VPPS]), (c) research design (randomized controlled trial [RCT] vs. naturalistic design), (d) percentage of DSM IV Axis II diagnosis, and (e) outcome rater (patient, therapist, observer). The RCT variable as a potential moderator was included because an investigation using an RCT design will likely have a smaller number of therapists and more patients per therapist in contrast to naturalistic settings, which introduces a potential confound because the significance of the PTR variables might be due to study characteristics.

Methods

Study selection and effect size calculation. The present analysis utilized a subset of data *(k*=69) from a meta-analytic study conducted by Horvath et al. (2011) (*k*=190; see Horvath et al., for the search strategy, inclusion/exclusion criteria, and a precise description of the primary studies). These 69 studies provided enough information about the sample description to compute

PTR. For each alliance-outcome correlation reported in the original source, one correlation (Pearson's *r*) ES was computed. These correlations were converted to Fisher's Z (correlation coefficients are known to have asymmetric distributions at larger values) for all analyses and then converted back for interpretive purposes. If multiple alliance-outcome relations were reported, the ESs within studies were aggregated to obtain one ES for each independent sample. This aggregation procedure accounted for the inter-correlation among within-study outcome measures; when the value of the correlation among the outcome measures was not reported, it was imputed to be *r*=.50 (see Wampold et al., 1997; for details on this procedure see Hunter & Schmidt, 2004; Del Re & Hoyt, 2010). Accounting for the within-study correlation among ESs is a recommended practice (Hunter & Schmidt, 2004; Hedges & Olkin, 2009) but is rarely conducted in published meta-analytic research. One reason for this is that primary studies typically do not report the correlation between measures used in the study. Therefore, most researchers will aggregate within-study measures by taking a mean, thus ignoring the correlation between measures within a study. However, this practice is problematic as it assumes the correlation between measures is 0. The consequence of this assumption is reduced precision in the ES estimate; that is, it results in inaccurate ES estimates (mean aggregated ES) and larger ES variance. Although availability of all between-measure correlations within each study is ideal, an imputation procedure for missing values has been recommended (see Wampold et al., 1997) and utilized in several meta-analyses (e.g., Horvath et al, 2011; Wampold et al., 2012; Flückiger et al., 2012; Farber and Doolin, 2011). The imputed value of r=.50 used to in this meta-analysis was based on the average correlation between psychotherapy measures in the literature and that which had been used in several previous published meta-analyses. This procedure was preferred (along with being a more conservative approach) to simply ignoring the within-study correlation between measures.

In our data, several studies contributed more than a single level of potential categorical moderators (more than a single source of alliance and outcome rater, and/or more than one alliance measure). The use of more than one categorical variable based on the same sample introduced the problem of dependencies, which could potentially bias the findings. Therefore, to avoid dependencies in such cases, we randomly selected one within-study level for each categorical moderator to base our analyses on a fully independent sample.

Variables Coded

Patient-therapist ratio (PTR*).* The primary predictor variable in this study was the ratio of patient N divided by therapist N (P_N / T_N), which was calculated for each study. From the Horvath et al. (2011) study, 69 studies provided information that enabled us to calculate this ratio and were included in the present meta-analysis.

The following possible confounding variables were coded:

Alliance rater (*k*= 65) was coded on three categorical levels: (1) patient-rated, (2) therapist-rated, (3) observer-rated. If a study reported effect sizes for multiple raters, one level was randomly chosen to be included in the analysis to eliminate non-independent data in the dataset; thus we used a fully independent sample for alliance rater in the analyses.

Research design (*k*=69) was coded on two levels (Randomized controlled trial; *RCT*): (1) Randomized controlled trial, (0) Not a randomized controlled trial. We recognize there is a large amount of variability in the "Not a randomized controlled trial" moderator level, from tightly controlled non RCT designs to a range of naturalistic designs. However, this variable was used primarily as a control variable for PTR, as RCT designs generally have a smaller patient to

therapist ratio. Therefore, modeling this moderator simultaneously with PTR will determine the unique contribution of PTR while controlling for RCT designs.

Axis II (*k*= 69) was coded as the percentage of DSM-IV Axis II disorders (e.g., antisocial, narcissistic or histrionic personality disorders) in the study sample. However, if studies did not report relevant information to compute a percentage it was imputed as 0 *(k*=52). Mean values over all studies was also imputed and made no substantial difference in coefficient or model significance.

Outcome rater (*k*= 62) was coded on three categorical levels: (1) patient-rated, (2) therapist-rated, (3) observer-rated. If a study reported effect sizes for multiple raters, one level was randomly chosen to be included in the analysis. This eliminated any potential dependencies among outcome raters in the dataset thus producing a fully independent sample.

Alliance measure (*k*=51) was coded into four categorical levels: (1) WAI, (2) CALPAS, (3) HAQ, (4) VPPS. These four measures are the most commonly used alliance measures in the literature. Studies not using one of these measures either did not report the alliance measure or reported measures that were developed for the study (or are infrequently used measures in the literature). If a study reported effect sizes for multiple measures, one level was randomly chosen to be included in the analysis. This eliminated any potential dependencies among alliance measures in the dataset thus producing a fully independent sample.

Two of the authors coded the independent variables; Inter-rater agreement across all categories was satisfactory (Kappa $>$ .85 for all moderators).

Statistical analysis. A random effects restricted maximum-likelihood estimator was used (Viechtbauer, 2005; 2010), which assumes that the studies in this meta-analysis were sampled

from a population of studies. All analyses were conducted using the R statistical software package for meta-analysis 'MAc' (Del Re & Hoyt, 2010). The first analysis involved an unconditional model (not conditioned on study level variables, i.e., moderators), as follows:

$$r_j = \theta_j + v_j^*,$$

where r_j is the estimate of the effect size for study j, θ_j is the true effect for study j, and $v_j^* = v_j + \tau^2$, where the variance of the within-study errors v_j is known and the between-study errors τ^2 are unknown and estimated based on the studies included in the analysis. Homogeneity was tested with the *H*-statistic and indexed as a percentage of variance with the I^2-statistics (Hedges & Olkin, 1985, Raudenbush & Bryk, 2002). *H* has an approximate χ^2 distribution with *k* - 1 degrees of freedom, where *k* is the number of studies aggregated; *H*-values of above the critical value result in rejection of the null hypothesis of homogeneity. I^2 is an index of heterogeneity computed as a percentage that reflects the proportion of variability in effects sizes that are due to true differences among the studies.

A conditional model examining the effects of the primary moderator variable (PTR moderator) was then examined. The PTR moderator is a study level variable, yielding

$$\theta_j = \gamma_0 + \gamma_1(PTR) + v_j^*$$

where γ_0 is the expected effect for a study when the moderator is zero or at the grand mean and γ_1 is the expected difference in effect size per unit change of the moderator. If a moderator variable accounts for the effects detected, the fixed effect γ_1 will be significantly different than zero (p-values < .05) and the variance, v_j^*, will be reduced. Heterogeneity of this model was also assessed.

Similarly, when the other potential moderators (i.e., potentially confounding variables) were included in a multi-predictor meta-regression, the formula yielded

$$\theta_j = \gamma_0 + \gamma_1(PTR) + \gamma_2(alliance\ rater) + \gamma_3(alliance\ measure) + \gamma_4(research\ design) + \gamma_5(axis\text{-}II) + \gamma_6(outcome\ rater) + v_j^*$$

where γ_0 is the expected effect for a study when all the moderators are at zero or at the grand mean and $\gamma_1 \ldots \gamma_6$ are the expected differences in effect size per unit change of the moderator, while holding each γ_j constant. If γ_1 remains statistically significant in this model, it can be inferred that the ratio (i.e., PTR) is robust moderator in the sense that it is not confounded with the other moderators.

Results

Unconditional Model. The overall effect of the unconditional model analysis (K = 69) was $r_+ = .274$ (95% CI = .230, .318) which, not surprisingly, closely approximates the correlation found in previous meta-analyses of alliance-outcome correlation. This also suggests that the subset of data used in this analysis closely mirrors the larger universe of studies from which these data were drawn (i.e., is not biased). There was significant heterogeneity in the effect sizes (H = 159.09, $p < .0001$; $I^2 = 57\%$), indicating that one or more study level variables might explain the variability of the effect sizes. More complex models were then fit to account for this variability, which involved inclusion of the proposed moderators of alliance-outcome correlation with a focus on the PTR in both single and multi-predictor meta-regressions.

To ensure the studies included in this analysis were free from publication bias, we created a funnel plot (available from first author upon request) and conducted rank correlation (Begg & Mazumdar, 1994) and regression tests (Egger, Smith, Schneider, & Minder, 1997) to determining

funnel plot asymmetry using the R package 'metafor' (Viechtbauer, 2010). Significance tests indicated that publication bias was not present in the included studies (*ps* > .26).

Moderator analyses. The PTR was a highly significant moderator of the alliance-outcome correlation (beta = -0.011, *p* = .002, 95% CI [0.017, -0.004]), which is in the expected direction if the therapist contribution to the alliance accounts for the alliance-outcome correlation, as found by Baldwin et al. (2007). That is, the smaller the PTR, the larger the alliance-outcome correlation.

Significant heterogeneity in the effect sizes remained in this model (Q (67) = 105.42, *p* = .002, I^2 = 35%). However, under a Maximum Likelihood estimator (MLE, to compare model fit), the PTR moderator alone accounted for 25.6% of the (residual) heterogeneity in the alliance-outcome correlation (MLE empty model Q(error) = 118.92 and τ^2 = .0107 vs. ML PTR model Q(error) = 105.42 and τ^2 = .0079; p = .0028), indicating that including PTR as a moderator significantly improved the model fit.

To determine whether the PTR effect might have been spurious, an analysis with several possible confounding variables was conducted. None of the other potentially confounding moderators, by themselves, significantly moderated the alliance-outcome correlation (all *p*s > .30, see Table 1). When all moderators were run simultaneously in a multi-predictor meta-regression, the PTR moderator remained a significant moderator of alliance-outcome correlation (see Table 2). In this model, the HAQ alliance measure was also significant, indicating that the alliance-outcome correlation was larger for the HAQ than for other measures of the alliance after controlling for the effects of PTR and the other moderators.

Comparison of meta-analytic results to Baldwin et al. (2007). We also attempted to meta-analytically replicate the Baldwin et al. study, using a simultaneous multi-predictor meta-

regression with a restricted maximal likelihood estimator. This was done by analyzing (a) total correlation, (b) patient contribution, and (c) therapist contribution to the alliance-outcome relationship. The moderators included PTR, Alliance Rater and Alliance Measure moderators in the model. As the Balwin analysis used patient-rated WAI, we were interested to see if we could not only replicate but extend their findings by including multiple perspectives and measures of the alliance.

First, we estimated the total correlation (aggregate of within and between therapist contribution) of the Baldwin et al. study, which had a PTR of 4.14 to 1, used patient rated WAI, and found a total correlation of was -.240. Using the meta-analytic regression, the predicted r for these parameters was -.353, $p < .0001$, 95% CI[-.437, -.27] which is somewhat higher than Baldwin's observed correlation.

Next, we estimated the patient contribution to the alliance-outcome relationship. This was estimated by examining our estimate when PTR was large. Our meta-regression estimates (PTR beta = 0.016, p =.0002, 95% CI [0.006, -0.025]) indicated that the regression line between PTR and the alliance/outcome correlation intersects the horizontal axis when PTR = 29, indicating that the alliance/outcome correlation is predicted to be zero when there are 29 patients per therapist. That is, when there are many patients per therapist (i.e., mostly patient variability), the alliance/outcome correlation approaches zero, consistent with Baldwin et al's finding.

Finally, we examined the therapist contribution to the alliance-outcome correlation. This was estimated in our meta-regression when PTR = 1. Our results indicated that the between therapist alliance-outcome correlation (i.e., when PTR = 1) was equal to -.402 (r = -.402, p =.0002 ; 95% CI[-.305, -.500]. Baldwin's between-therapist estimate (r =-.330) falls within our 95% CI. and provides a meta-analytic corroboration of Baldwin et al.'s findings. Moreover,

because the meta-analytic results were robust across many variables (e.g., rating perspective, alliance measure, type of design), these results also extend the Baldwin et al. findings.

Discussion

Recent studies have found that therapist's capacity to develop alliances with their patients (i.e., the therapist effect) is associated with outcome (Baldwin et al., 2007; Dinger et al., 2008; Zuroff et al., 2010). Specifically, some therapists seem to be consistently better at forming alliances with their patients than others and these therapists' patients have better treatment outcomes. Based on these recent findings, it appears that the quality of the alliance between therapist and patient is more a result of therapist actions or characteristics and therefore the therapists' role is the most important for achieving beneficial outcomes. However, the relative paucity of studies examining the therapist effect in relation to the alliance-outcome links, and the characteristics of available studies make this conclusion tenuous.

The purpose of the current meta-analysis was to assess the degree to which therapists account for the effects on the alliance-outcome correlation using a broad cross section of research data and controlling for various study level confounds. Using a random effects (restricted maximum likelihood) meta-analytic estimator (Del Re & Hoyt, 2010; Viechtbauer, 2010), the influence of the therapist contributions to the alliance — assessed by the patient-therapist ratio — on the alliance-outcome correlation were examined in single and multi-predictor meta-regression models.

The results of this meta-analysis indicated that the therapist's contribution to the alliance was a statistically significant predictor of outcome, even when accounting for patient Axis II diagnosis, research design, rater of alliance and outcome and alliance measure used in the study. The findings suggest that some therapists consistently do better in developing the therapeutic

alliance than others and that the patients of these therapists tend to achieve better outcomes. With a ratio of therapist to patient at 1:1, the average alliance-outcome correlation was equal to -.40; that is, the correlation between the average therapist alliance with his or her patients and outcome is -.40, a relatively large correlation. Further, the results of the present meta-analysis indicated that variability of the alliance of patients within therapists seems to be unrelated to outcome. That is, the patient's contribution to the alliance appears to be unrelated to outcome.

That being said, in the meta-analytic context it was not possible to examine the interaction of therapist and patient. The within-therapist variance involves not only the patient contribution but also the *interaction* between therapist and patient. Some therapists might well form an alliance with some types of patients whereas other therapists might form an alliance with other types of patients. That is to say, therapists may be more responsive to some types of patients than others (Stiles, 2009; Stiles, et al., 1998). Therefore, the non-significant relationship at the within-therapist level (i.e., high PTR) is masking the transactional nature of the alliance. However, Baldwin et al. (2007) was able to directly examine the interaction of therapists and patients and found that the variability in the alliance due to the interaction was not related to outcome.

Nevertheless, the results of the present meta-analysis, based on nearly 70 studies, confirms and extends the results of Baldwin et al. (2007), Dinger et al. (2008), and Zuroff et al. (2010), all of which found the therapist variability in the alliance to be a statistically significant predictor of outcome, whereas patient variability was not. The earlier studies and this meta-analysis indicate that the total correlation misrepresents the association between the alliance and outcome. The alliance-outcome correlation within therapist appears to be nonexistent, whereas the between therapist correlation seems relatively large (magnitude $\geq$ - .40). The average total correlation found in this and previous meta-analyses, which is in the neighborhood of -.25 (6% of explained

variance) is significantly lower than the between therapist alliance-outcome correlation (in Baldwin et al. -.33; here estimated to be -.40 which is 11% and 16%, respectively, of the explained variance). Thus, the literature may be underestimating the alliance-outcome correlation, a conclusion reached by Crits-Christoph, Connolly Gibbons, Hamilton, Ring-Kurtz, and Gallop (2011), albeit for a very different reason.

Assuming the results of this and previous studies continue to be replicated, they have important clinical and research implications. It appears that therapists differ significantly in their ability to form alliances with a range of patients and this capacity to build effective alliances is related to outcome (Baldwin, Wampold, & Imel, 2007; Dinger, Strack, Leichsenring, Wilmers, & Schauenburg, 2008; Zuroff, Kelly, Leybman, Blatt, & Wampold, 2010) .

Although the results of this study, as well as others (Baldwin, Wampold, & Imel, 2007; Dinger, Strack, Leichsenring, Wilmers, & Schauenburg, 2008; Marcus, Kashy, Wintersteen, & Diamond, 2011; Zuroff, Kelly, Leybman, Blatt, & Wampold, 2010) establish the important role of therapists in the alliance-outcome relationship, they tell us little about *how* therapists contribute to the alliance. These findings provide a further rationale to examine specific therapist behaviors and perhaps institutional working conditions of "good alliance" therapists, so that psychology training programs can implement programs to address and teach these behaviors (for an overview see Muran & Barber, 2010) and psychology clinics can foster working conditions most conducive to improving patient alliance and outcome (see Donabedian, 1966/2005). However, this might be more complex that it appears because although the alliance is a common factor, the process that lead to the alliance and how the alliance creates change may differ depending on the type of therapy being delivered (Ulvenes et al., in press).

Fostering optimal working conditions for improving patient outcomes, may include integrating feedback systems to monitor patient progress and tracking patient outcomes (Flückiger, Del Re, Wampold, Znoj, Caspar, Jörg, 2011; Lambert & Shimokawa, 2011; Miller, Duncan, Brown, Sorrell, & Chalk, 2006). These tracking systems can then be used monitor therapist progress on process (e.g., alliance) and outcome variables and provide feedback to therapists' on their treatment efficacy For practicing clinicians, it seems important to regularly monitor their alliance with patients and, if their alliances are consistently low, consider how to remedy this problem with additional supervision, training, or other strategies (e.g. Anderson, Lunnen, & Ogles, 2010), such as being more responsive to the patient (Stiles, 2009; Stiles, et al., 1998). Responsiveness refers to the therapeutic process where therapist and patient behavior is influenced by emerging information during the therapy session. Good therapist's are likely more responsive with their patients and this responsiveness then influences the alliance which in turn impacts outcome. Nevertheless, alliance-building behaviors may be difficult to teach, in which case it might be beneficial for clinical administrators and training programs to screen therapy trainees for these relevant behaviors and the ability to form alliances across a range of patients.

This investigation highlights the benefits of disentangling the contributions of patients and therapist to important process-outcome relations. We recommend that future research exploring process variables related to outcome, utilize mixed-effect models (patients nested within therapists) to disentangle the within- and between-therapist contributions to the process variable. Identifying the source of variance influencing these relations will allow researchers to build a more precise model of the dynamics of change inducing events and enhance clinical training programs ability to focus on the most effective ways to strengthen new therapist’s capacity to be

effective with their patients. Meta-analytically, the use of PTR could be used to disentangle patient and therapist contributions to outcomes.

In addition to this procedure (P_N/ T_N) being useful for identifying the unique therapist and patient contributions to the alliance-outcome correlations, a similar meta-analytic strategy (i.e., Within-Cluster/ Between-Cluster = W_C/B_C) is (hypothetically) plausible with a variety of naturally nested variables. This variable would then be treated as a moderator in a meta-regression and the beta coefficient would indicate whether there is significant within- and between clustering variance. Some examples of B_C and W_C include time (W_C) within patient (B_C), patients (W_C) within clinics (B_C), and clinics (W_C) within hospital (B_C). Other examples include student (W_C) within family or school (B_C) and employee (W_C) within worksites (B_C). There are several possibilities to disentangle sources of variance using this meta-analytic strategy and future studies could benefit from considering a similar strategy.

There are a number of limitations to the present study. As with any meta-analysis, the conclusions are dependent on the quality of the primary studies. In this current meta-analysis, the quality of reporting study methodology varied. For example, the majority of studies did not report the correlation between outcome measures, which reduces the overall precision when meta-analytic researchers aggregate within and between study effects. Further, several studies failed to report any information about the type of alliance measure used or the severity of their patient sample (or diagnostic classification). As well, typically alliance is measured at one point in therapy, whereas there is evidence that the pattern of alliance over time is critical to producing psychotherapy benefits (Kivlighan & Shaughnessy, 2000). In addition, the PTR moderator itself is a synthetic meta-analytic variable, which is an indirect indicator of the relative importance of therapist and patient contributions to the alliance. Nevertheless, the results using the PTR are

consistent with previous studies (viz., Baldwin et al., 2007; Dinger et al., 2008; Zuroff et al., 2010) and provide estimates very similar to Baldwin et al. (2007), while also integrating multiple raters of both the alliance and outcome and controlling for several potential confounds. The studies examined were not designed to disentangle the therapist and patient contributions to the alliance. Although several confounding variables were examined, which reduced the heterogeneity of the effects, some heterogeneity remained, indicating that there were unobserved factors related to the variability among effects.

In summary, therapist variability in the alliance appears to be more important than patient variability for improved patient outcomes (as assessed with the PTR moderator). This relationship remained significant even when simultaneously controlling for several potential covariates of this relationship. These results suggest that some therapists develop stronger alliances with their patients (irrespective of diagnosis) and that these therapist's patients do better at the conclusion of therapy.

Footnote

[1]In panel c (PTR = 1), there remains patient variability, as some patients will be better able to form alliances with their therapists than others, affecting the observed alliance for each therapist. However, these deviations would constitute random error around the regression line and thus the correlation obtained in such instances would be an unbiased estimate of the true correlation of therapist contribution to the alliance with outcome.

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Tables

Table 1. Single- and Multi-Predictor Random Effects Meta-Regression Model Fit

Model	*k*	QE	QE	QE(p)	QM	QM(df)	QM(p)
PTR	69	105.4201	67	0.0019	9.1384	1	0.0025
Alliance Rater	65	122.9075	62	0	1.9752	2	0.3725
Research Design	63	137.0573	61	0	0.1314	1	0.717
Axis II	69	154.0894	67	0	0.8425	1	0.3587
Outcome Rater	62	115.4621	59	0	3.2043	2	0.2015
Alliance Measure	51	124.0901	47	0	4.2833	3	0.2325
All Moderators	38	51.7145	38	0.0029	26.1926	10	0.0035

Note: *k* = number of studies; QE = Model Error (residual variance) Q-statistic; QE(df) = QE degrees of freedom;

QE(p) = QE p-value; QM = model fit Q-statistic; QM(df) = QM degrees of freedom;

QM(p) = QM p-value (significance of model); * =p> .05

Table 2. Coefficient Values for Multi-Predictor Meta-Regression *(k* = 38)

	Estimate	SE	CI (Upper)	CI (Lower)	p-value
Intercept	-0.2605	0.0951	-0.080	-0.425	.0051 *
PTR	0.0119	0.006	0.0236	.0001	.0479 *
Alliance Rater (Observer)	-0.113	0.1589	0.1954	-0.4011	.4424
Alliance Rater	0.1032	0.0837	0.2615	-0.0604	.2127
Research Design (RCT)	-0.1099	0.0768	0.0401	-0.255	.1495
Axis II	-0.0025	0.0030	0.0033	-0.0084	.3757
Outcome Rater	0.1586	0.0882	0.3210	-0.0129	.0696
Outcome Rater	-0.2359	0.1574	0.0679	-0.4997	.1259
Alliance Measure (HAQ)	-0.4856	0.1528	-0.2267	-0.6803	.0005 *
Alliance Measure (VPPS)	0.1561	0.2226	0.5326	-0.272	.4459
Alliance Measure (WAI)	-0.1122	0.0956	0.0745	-0.2913	.2340

Note: All models utilized random-effects procedures. Estimate = coefficient value;

SE = standard error; CI (Lower) = lower 95% confidence interval of the estimate;

CI (Upper) = upper 95% confidence interval of the estimate; * =p> .05.

Heterogeneity: QE=51.7145; Qep=0.0029; QM=26.1926; QMp=0.0035 *.

QE = Model Error (residual variance) Q-statistic;QE(p) = QE p-value;

QM = model fit Q-statistic; QM(p) = QM p-value (significance of model).

Figures

Figure 1.