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EMPIRICAL PAPER

## Does it make a difference to be more “on the same page”? Investigating the role of alliance convergence for outcomes in two different samples

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### Abstract

**Objective:** To better understand the complexity of dyadic processes, such as the mechanisms of the working alliance, researchers recommend taking advantage of innovations in data analytic procedures when studying the interactions between therapists and patients that are associated with favorable therapeutic outcomes. Inspired by a recent line of alliance research using dyadic multilevel modeling, the present study investigated the hypothesis that convergence in the patient-therapist working alliance (i.e., increased similarity in ratings of the alliance across treatment) would be associated with better outcomes. **Method:** Data were retrieved from two samples: 1. A randomized controlled trial for treatment resistant depression ( $N = 96$  dyads), and 2. An archival dataset of naturalistic psychotherapies from public health care ( $N = 139$  dyads). Multilevel growth curve analysis was employed to investigate the degree of change in session-to-session agreement of global WAI ratings between therapists and patients (i.e., alliance convergence) as a predictor of symptom reduction in the BDI-II and the SCL-90R. **Results:** Contrary to our expectations, alliance convergence did not predict outcome in either sample, but was negatively associated with symptom severity in Study 2. Implications for understanding the complexity of dyadic processes and alliance work in psychotherapy are discussed.

**Keywords:** alliance; outcome; alliance convergence; dyadic multilevel modeling

**Clinical or methodological significance of this article** It has been suggested that in addition to a strong dyadic alliance, a more convergent perspective between therapist and patient in their view of the working alliance across treatment is reflective of a constructive therapy process. We tested the assumption that alliance convergence would be related to more positive therapy outcomes in two studies. However, contrary to our expectation, alliance convergence was not related to symptom reduction in either sample but it was related to more symptoms in one of the studies. It may be that idiosyncratic fluctuations between therapist and patient stand in the way of a beneficial effect of alliance convergence and/or that the severity of clinical distress of the patients studied precluded an increase in alliance agreement over time. As such, we cannot propose that therapists should make sure to foster increased agreement about the alliance over time with their patients. Instead, our study further promotes a conception of the therapy relationship and collaboration as a highly complex phenomenon that evades simple inferences.

Having established that psychotherapy works well for most common mental health problems (Norcross & Lambert, 2018), and better than many other evidence-based medical practices (Wampold & Imel, 2015), we are also getting closer to understanding

why psychotherapy works across different approaches. Some of the (pan-theoretical) process factors with the most promising evidence thus far are; a sound alliance between therapist and client (Flückiger et al., 2018); collecting and giving

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feedback on client progress using routine outcome monitoring (Lambert et al., 2018); therapist empathy (Elliott et al., 2018), facilitative interpersonal skills (Anderson et al., 2009) and cultivating client positive expectations (Constantino et al., 2018).

Of these processes, the working alliance has been subjected to the greatest volume of empirical investigations. Recent meta-analytic work has reconfirmed that the alliance is a robust and moderately strong predictor of outcomes, and that this relationship holds across rater perspectives, types of alliance and outcome measures, treatment approaches, patient diagnoses and clinical settings (Flückiger et al., 2018). Moreover, using powerful analytical procedures, researchers have demonstrated that the alliance is not just a byproduct of symptomatic improvement (e.g., Falkenström et al., 2013) but has itself a therapeutic effect in the relief of psychological distress through psychotherapy (Zilcha-Mano, 2017).

Through the gradual shift towards a more (two-person) relational theory of psychotherapy, it is now commonly accepted to see all therapeutic interactions as interdependent, dynamic systems in which “both client and therapist are mutually but asymmetrically shaped and transformed over time as a function of their responsibilities and roles” (Atzil-Slonim & Tschacher, 2020, p. 555). Coupled with recent technical and methodological innovations this has brought to the fore the study of dyadic processes, such as emotional synchrony, affective co-regulation, and perspective congruence between client and therapist.

Even if the working alliance in its original conceptualization (Bordin, 1979; Greenson, 1965) was seen as an inherently dyadic construct, with two subjects perceiving and constructing their mutual relationship and collaboration (as a contrast to more intrapsychic phenomena such as transference and countertransference reactions), it has typically been studied from a single perspective (see Atzil-Slonim & Tschacher, 2020). That is, measures of the working alliance obtained by client or therapist are correlated with various pre-treatment characteristics, in-session experiences and outcome, rather than studied simultaneously taking into account the different perspectives as a combined measurement reflecting the state of the dyad.

As a response to this critique, one could argue that any measure of the therapeutic alliance is always in reference to the dyad, and hence is always—at a conceptual level—a dyadic construct. When the alliance is studied dyadically, the measure represents in fact a kind of “meta-agreement” (i.e., an agreement about an agreement). The question is whether this

construct empirically (or theoretically) adds to our understanding of the alliance-outcome dynamic or the working mechanisms of psychotherapy.

There are theoretical and empirical reasons to see this question from both a confirming and a disconfirming stance. On the one hand, the perception of alliance quality in the dyad is clearly influenced by more or less stable characteristics of both client and therapist (e.g., Constantino & Smith-Hansen, 2008; Heinonen et al., 2013; Nissen-Lie et al., 2014). As such, it always contains an intrapersonal (or even transference) or trait-like dimension (Zilcha-Mano, 2017). Moreover, the two perspectives of the shared alliance (i.e., patient-rated and therapist-rated alliance) are only moderately related to each other (Tryon et al., 2007), underscoring the intrapsychic or subjective element in alliance ratings. Thus, in any conceptualization of the alliance we need to acknowledge that it is always perceived by two (or more) individuals whose perception is influenced by the participants’ inner working models guiding their perception of others, as well as the distinctly different roles they have in the therapy relationship as clients and therapists/health care professionals, respectively. The latter point was illustrated in the classic study on the dual perspective of Yalom and Elkin in 1974, showing that the therapist (Yalom) and his client (Elkin) had fundamentally different perspectives, valuing very different aspects of their relationships and collaboration (see Chui et al., 2020). A number of later empirical studies also suggest that patients and therapists emphasize different aspects of their relationship when scoring the alliance (for a discussion, see Bachelor, 2013).

Despite this, it might be argued that if the therapist’s perception of the alliance is similar to the patient’s, this indicates that the therapist is more attuned to the patient’s inner states and that the two are more “on the same page”—which purportedly should lead to better therapy results (Coyne et al., 2018). Indeed, it was early argued by Pepinsky and Karst (1964) that a lessening of discrepancies in judgments, perspectives, beliefs, and behaviors (i.e., increased alignment) between therapists and their clients would be an important predictor of a beneficial therapy process. Horvath and Bedi (2002) saw consensus between the client and therapist as one of the *defining features* of the working alliance.

There is now a growing body of research suggesting that perspective congruence is a meaningful contributor to outcome. For example, more agreement between clients and therapists in their recall of important session events has been found to be related to session effectiveness (Cummings et al., 1992) as well as to final outcome (Kivlighan & Arthur, 2000). More recently, in two studies using response

surface analysis, Marmarosh and Kivlighan (2012) found that as the therapist and client had more positive agreement on the perceived alliance at the beginning of the treatment, clients experienced greater symptom change. Similarly, Zilcha-Mano et al. (2017) observed a pattern of more alliance agreement between therapists and patients at one time in treatment being associated with lower symptoms one month later. In a study of the bond aspect of the alliance, Atzil-Slonim et al. (2015) found that when clients reported more symptoms, therapist and client ratings of the emotional bond were more divergent. Moreover, Jennissen et al. (2020), using multi-level polynomial regression with response surface analysis, found that congruence in therapist-patient alliance predicted less symptom severity five sessions later.

In this rather novel empirical field, a potentially important distinction is made between patient-therapist alliance *congruence* (i.e., degree of patient-therapist agreement of alliance ratings at one point in time) and patient-therapist alliance *convergence* (i.e., increasing agreement over time). Given that the alliance is dynamic in nature and not a fixed characteristic of a dyad but rather tends to change over the course of treatment through an ongoing negotiation of rupture and repairs (Safran & Muran, 2006), one could argue that to study changes in congruence over time is a more important test of the dyadic alliance hypothesis. At any given moment, alliance congruence may reflect as much how clients (or therapists) are influenced by their inner representations and prior experiences of “the other” (that is, a more trait-like disposition) rather than the actual dynamic with the therapist. By tracking agreement or disagreement across time, we may test whether convergence—as an addition to perceived alliance quality—reflects an independent aspect of the process.

However, only a few studies have investigated the convergence hypothesis. Taking advantage of recent analytical innovations (i.e., dyadic multilevel growth curve modeling), Coyne et al. (2018) studied the influence of dyadic convergence (growth in agreement across treatment) in global alliance scores on psychotherapy outcomes for patients with severe generalized anxiety. Their findings indicated that dyadic convergence in the first part of treatment (rate of change in alliance discrepancy from the 2nd through the 8th session of a total of 15 sessions) was a predictor of less worry and general distress in the second part of the treatment (change in symptoms from the 9th or 10th through the 15th session). In this study, effect size estimates (pseudo- $R^2$ ) of convergence ranged from around 4% to 18% explained variance in post-treatment scores

(depending on type of outcome) with a mean effect size across measures of 10.75%, indicating a moderate effect of the convergence coefficient on outcome.<sup>1</sup>

In another study using a similar methodology, Laws et al. (2017) examined if convergence in the patient-therapist alliance was related to symptom reduction in a 12-week treatment program (CBT or brief supportive therapy) of chronic depression. They operationalized convergence as growth curve estimates of the discrepancy in patient-therapist alliance ratings across the entire treatment as predictor of changes in depression measured by external observers. They found that higher alliance convergence across treatment was associated with greater reduction in a pharmacotherapist-rated measure of depression (QIDS-C),<sup>2</sup> but was not predictive of declines in depression as rated by the more conventional interviewer-rated Hamilton rating scale for depression (i.e., HAMD). Instead, alliance convergence was positively related to lower HAMD scores at follow-up<sup>3</sup> (i.e., 3 months after the end of treatment) but it is difficult to determine whether or how these measures were (causally) linked. Based on these findings, the authors state that they only found partial support for the convergence hypothesis.

To sum, we now have a number of studies (Atzil-Slonim et al., 2015; Marmarosh & Kivlighan, 2012; Zilcha-Mano et al., 2017) suggesting that agreement in alliance scores (i.e., *alliance congruence*), as an addition to a strong mutual alliance *level*, is indicative of a more aligned, attuned and fruitful therapy process. Furthermore, there are two notable studies on the even stronger hypothesis that increased congruence over time is also a marker of therapeutic success. Inspired by these two, we add two more studies to this knowledge base examining whether alliance convergence is an independent predictor of more favorable therapy results.

It is important to replicate findings across types of patients, clinical severity, treatment approaches, and research designs—even countries, to bring forth generalizable knowledge about constructive therapy processes. We cannot draw definitive conclusions from single studies. Instead, we need to accumulate results from several studies before we can feel confident in conclusions. In the present work, we used data from two different studies that had gathered therapist and patient alliance ratings as well as measures of symptomatology regularly during treatment. Inspired by Coyne et al. (2018) and Laws et al. (2017) using a similar dyadic MLM approach with global alliance scores and specific types of outcome, our goal was to explore the relationship between patient-therapist alliance convergence (rate of change of agreement on

global alliance) across treatment and patient outcomes in one RCT and one naturalistic process-outcome study that were both conducted in Scandinavia. In Study 1, two evidence-based treatments, Cognitive Behavioral Therapy (CBT) and Interpersonal Psychotherapy (IPT) were delivered to patients with persistent major depression as part of an RCT (Ekeblad et al., 2016). Study 2 involved a naturalistic process-outcome study of psychotherapies delivered as part of ordinary clinical practice, treating a range of adult outpatients (see Nordmo et al., 2020). In our analyses predicting outcomes from a convergence parameter, we sought to identify the unique contribution from convergence on outcome by controlling for dyadic alliance levels and change across treatment as well as the direction of alliance convergence (if therapist and patient agreed on whether the alliance was improving or deteriorating). In accordance with the findings mentioned above, we posed the hypothesis that increased agreement between therapists and patients in their ratings of the alliance over the course of treatment would positively predict distress reduction in both samples.

## Method

### Study 1

**Treatment and context.** In this study, patients were randomized to one of two evidence-based treatments, Cognitive Behavioral Therapy (CBT) and Interpersonal Therapy (IPT) in a secondary care setting in Scandinavia (Ekeblad et al., 2016). The CBT treatment condition followed two CBT manuals for treating depression (i.e., Beck et al., 1979; Martell et al., 2010). The therapists had been trained in both manuals and adherence was ensured via ratings of videotaped sessions. Some therapists also included mindfulness-based interventions in their CBT treatment (Segal et al., 2013) when deemed appropriate as part of the CBT treatment. IPT was delivered according to the standard manual by Weissman et al. (2000). Treatment adherence was assessed and ensured using the Collaborative Study Psychotherapy Rating Scale (Evans et al., 1984) from videotaped therapy sessions. In this trial, 14 sessions of therapy were provided in both treatments. The randomization procedure was done by a psychologist at the clinic not otherwise involved in the project. There was no difference in treatment response between the two conditions (Ekeblad et al., 2016). The study was approved by the local Regional Ethical Review Board (reference = 2010/348-31).

### Participants

**Patients.** The patients were all referred to psychiatric outpatient care for treatment of depression, mostly from primary care but some from inpatient treatment. They were all diagnosed with major depressive disorder (MDD) by experienced psychiatrists or clinical psychologists using the Structured Clinical Interview for DSM-IV (SCID; First et al., 2002). All patients had previously received treatment for depression, most commonly through antidepressant medication in primary care, with insufficient treatment response. According to medical records, 59.4% of the sample was on stable antidepressant medication at the start of the study. Note that ongoing medication was not an exclusion criterion, but the recommendation was to avoid changes in medication during therapy. The inclusion criteria were ages between 18 and 65 years and a primary MDD diagnosis or recurrent depression with a current MDD. Exclusion criteria were psychosis, ongoing substance addiction, serious neuropsychiatric disorder, or active self-harming behavior. To be included in the study patients had to accept random allocation to the therapy methods and video filming of all sessions. A total of 96 patients were included out of 99 who were invited to participate. Patients who met the inclusion criteria were informed about the study and gave their written consent.

**Therapists.** A sample of 34 psychotherapists participated in this study. They all worked at the psychiatric outpatient clinic from which these data were collected. Twenty-five therapists provided CBT: 14 psychologists (eight women and six men), four psychiatric nurses, two social workers (all women) and three physicians (one woman and two men). There were nine therapists providing IPT: Six psychiatric nurses (five women and one man), one nurse assistant, one occupational therapist and one social worker (all women). The IPT therapists were older (mean 57.9 years, range 50–65 years) and had more years of professional experience in their basic profession. The CBT therapists were younger (mean 37.6 years, range 28–60) and thus did not have as much professional experience. All therapists had basic training in psychotherapy and a specialist training in the treatment method they provided (CBT or IPT). During the trial, therapists in both conditions received regular supervision (1–2 times per month) from expert supervisors.

### Measures

**Working alliance Inventory.** For assessing therapist-rated alliance, the Short Form of the

Working Alliance Inventory (WAI; Horvath & Greenberg, 1989), WAI-S (Tracey & Kokotovic, 1989) was used, and for assessing patient-rated alliance the revised short form, WAI-SR (Hatcher & Gillaspay, 2006) was used.<sup>4</sup> In both versions, four items are designed to measure each of the three aspects of the alliance (agreement on goals, tasks, and the emotional bond), making a total of 12 items. Several studies have demonstrated excellent psychometric properties for the WAI (Horvath & Greenberg, 1989, 1994). In the current study, the session ratings of all 12 alliance items were collapsed for both therapist- (WAI-T) and patient-rated (WAI-P) alliance. Both versions of the WAI were rated on a scale from 1 (“Never”) to 7 (“Always”) and were completed after each session by the patient and the therapist independently from each other. As is typical with the WAI, the intercorrelations between the three subscales (bond/task/goals) were high ( $r_s = .76-.93$ , all  $p_s \leq .001$ ) so we analyzed the global alliance scores (the mean of the 12 bond/task/goal items) and did not analyze the subscales separately.

**Beck Depression Inventory—II (BDI-II; Beck et al., 1996).** The BDI-II, which is a widely used self-rated instrument to assess depressive symptoms, was used as outcome variable. The BDI-II has demonstrated good psychometric properties such as high reliability, capacity to discriminate between depressed and non-depressed individuals as well as different subtypes of depression, and has demonstrated good to excellent concurrent, content and structural validity (Beck et al., 1996; Wang & Gorenstein, 2013). The scale consists of 21 items, rated on a 4-point scale Likert scale reflecting statements with increased severity on different depression symptoms (problems with sleep, self-esteem, energy level, irritability, suicidality). In this sample, the mean BDI score at pre-treatment was 35.8 ( $SD = 9.5$ , range = 14–58) indicating a severe level of depression. The patients completed the BDI-II before each therapy session.

**Procedure.** Patients were referred to the psychiatric clinic from their general physician or other mental health care professionals for treatment of major depression. The recruitment started in the fall of 2010 and ended in November 2013. The patients who fulfilled the inclusion criteria were invited to participate and those who accepted, who were eligible and gave their written informed consent, were then randomized to either CBT or IPT. Ethical approval and informed consent were collected from all participants. All therapy sessions

were videotaped in order to assess treatment adherence.

## Study 2

**Design and treatment.** Data were retrieved from an archival naturalistic study of psychotherapy treated at outpatient clinics within the public mental health care system, organized at eight different research sites in Norway (e.g., Nordmo et al., 2020). The treatments were influenced by different therapeutic models and could be classified as “treatment-as-usual” in ordinary psychiatric care. That is, no protocols or special supervision were used (except that two of the eight sites provided Affect Consciousness treatment; Monsen & Monsen, 1999). All sessions were audio-taped. The treatment lengths in the study as a whole were fairly long, with a mean of 51 sessions ( $SD = 59$ ; 25% percentile = 16; 50% percentile = 34; and 75% percentile = 56), ranging from 1 to 364. The study was approved by the local Regional Ethical Review Board (reference = S-950109).

## Participants

**Patients.** The patients were referred to public outpatient clinics, mostly by their physicians, for assessment and treatment of a wide range of clinical symptoms and disorders. To ensure a typical and representative outpatient sample reflecting both breadth and depth of mental health problems, the inclusion policy was liberal. Only patients with serious substance abuse problems, acute crises requiring hospitalization, and psychoses were excluded from the study. Around 50% of the total sample comprised patients suffering from at least one personality disorder (Nordmo et al., 2020). The analyses of patient-therapist dyads presented below were conducted on a subsample of patients ( $N = 139$ ) who had both patient and therapist rated WAI from sessions 3, 12 and 20 and outcome measures from the same assessments along with final session outcome (see Statistical analyses below). This subsample included 74.4% women and 25.6% men, whose ages ranged from 20–62 years, with a mean of 35.7 ( $SD = 9.52$ ). The most frequent Axis I DSM-IV diagnoses were anxiety disorders (67%) (e.g., Social phobia or Generalized anxiety) and affective disorders (55.9%) (e.g., Major depression or Dysthymia). More than half (54.2%) of the patients met the criteria for at least one personality disorder (PD).

**Therapists.** This subsample of patients ( $N = 139$ ) was treated by 52 psychotherapists (28 clinical

psychologists, 12 medical doctors or psychiatrists, 8 physiotherapists specializing in psychodynamic body therapy, a variant of the Affect Consciousness psychotherapy model, 2 psychiatric nurses and 2 social workers). Their level of experience in practicing psychotherapy ranged from 0 to 28 years, with the mean being 10.0 years ( $SD = 6.57$ ). The majority (over 2/3) of therapists reported a psychoanalytic/psychodynamic *salient orientation* (= ratings of 4 or more on a five-point Likert scale on the major theoretical orientations), but a substantial portion of therapists in the sample also reported having a salient orientation in the humanistic and/or cognitive treatment models.

### Measures

**Working alliance.** The 12-item version of the Working Alliance Inventory (WAI-S) (Tracey & Kokotovic, 1989) in both the patient-rated (WAI-P) and therapist-rated (WAI-T) version was used to assess the working alliance in this study. See description of the WAI-S for Study 1. In Study 2, the WAI was completed after session 3, 12 and 20 (and each 20th session, when applicable) by the patient and the therapist. As in Study 1, the WAI was rated on the traditional Likert scale from 1 (“Never”) to 7 (“Always”) and were completed after each session by the patient and the therapist independently from each other.

**Symptom distress.** The outcome variable was general symptom distress as measured by the revised Symptom Checklist-90 (SCL-90-R) (Derogatis, 1994). The SCL-90-R is a self-report questionnaire composed of 90 items tapping nine different symptom dimensions. The 90 items are rated from 0 (*not at all*) to 4 (*very much*). The responses to the items were averaged in the standard Global Severity Index (GSI). The GSI is regarded a suitable measure to reflect patients’ general psychopathology and psychological distress and has demonstrated sensitivity to change through psychotherapy (e.g., Ogles et al., 1996). The mean Global Severity Index (GSI) at pre-treatment was high (mean = 1.39,  $SD = 0.62$ , range = 0.16–3.34) indicating a relatively severe level of distress in this sample. The patients completed the SCL-90-R after session 3, 12, 20 and each 20th session (when applicable) and at the end of treatment (T2).

**Procedure.** Patients were recruited between 1996 and 2000 from eight sites across the country. By the end of 2005 all treatments in this project had been terminated. All patients were screened by expert clinicians (clinical psychologists or psychiatrists). Those who fulfilled the inclusion criteria and had provided full informed consent were assigned to a

psychotherapist at the individual treatment site based on availability of the therapists. No other criteria were used in allocating patients to therapists. All sessions were audiotaped.

**Statistical analyses (both studies):.** For descriptive purposes we report means, standard deviations and range of patient-rated (WAI-P) and therapist-rated (WAI-T) alliance at all measurements. A convergence score was computed as the *rate of change* in discrepancy between growth-curve estimated WAI-scores of patients and therapists throughout the alliance measurements (from session 1 through 14 in Study 1 and 3 through 20, in Study 2, see below). Hence, we estimated a regression line through the patient-therapist discrepancy scores using OLS regression. Using growth curve estimated changes in congruence as the operationalization of convergence allowed us to avoid problems with reduced reliability of difference scores and presumably is the best analytic option available for estimating alliance convergence.

Multilevel modeling was applied in the main analyses using the linear mixed models option in the IBM/SPSS version 26.0. For longitudinal data in which assessments are nested within individuals, repeated measurements represent units at the first level and individuals represent units at the second level while therapists represent a third level, and so on. Application of multilevel modeling for the analysis of such nested data is the recommended option (Hox, 2010; Singer & Willett, 2003).

In the present study, assessments were treated as fixed occasions. We thus combined models of individual patterns of longitudinal change, while also maintaining statistical power of the multilevel models to estimate and predict the overall magnitude of change in therapy. We also modeled the therapist level (level 3) to account for potential differences in outcomes (BDI scores or GSI) resulting from therapist effects (that patients treated by the same therapist share variability and thus are not independent observations), but this model was later dropped (see below).

To do a proper power analysis for these multilevel models, we would have needed to find plausible values for several parameters that were unknown, e.g., random effects and residual variances/covariances. These were deemed difficult to obtain due to the scarcity of prior studies with similar samples and designs. However, the main research question was tested by a regression coefficient on a between-patient level (i.e., Level-2), which means that estimation performance is determined mostly by the between-level sample size (Maas & Hox, 2005). Therefore, we calculated statistical power for a linear

regression model in Stata (Statacorp, 2019), using a cross-sectional design as a rough approximation of the between-level part of our two-level model. Following these calculations, we found that to obtain 80% power at  $\alpha = .05$ ,  $N = 44$  would be needed to find a standardized regression slope of  $\beta = .40$ ,  $N = 82$  for  $\beta = .30$  and  $N = 191$  for  $\beta = .20$ . Thus, our samples should be sufficiently powered for obtaining a medium-sized regression slope representing the effect of alliance convergence on outcomes.

**Preparatory data analyses.** Visual inspection of raw score- and individual ordinary least squares (OLS) plots were conducted to determine whether linear or nonlinear models best fitted the outcome data. A linear trajectory (as compared to a loglinear curve) of change in symptoms was best suited in both samples as indicated by a deviance score (Aikakes Information Criterion, AIC). Individual growth-curve models were then computed for the alliance ratings of corresponding therapists and patients in each dyad. Based on these growth curves we calculated an initial alliance level-score and an overall convergence score (i.e., increased agreement in WAI ratings over time) for each dyad to be used as predictors of outcome. The convergence score was not normally distributed and was thus log-transformed to counter this. In line with Laws et al. (2017) and Coyne et al. (2018), we then controlled for a number of parameters so as to account for the strength of the alliance, development over time and who rated the alliance as higher than the other (see specification below). All variables were grand mean centered for interpretability and to avoid problems with multi-collinearity.

**Multilevel modeling.** The multilevel models initially contained three levels of analysis representing repeated measurements over time nested within patients who were nested within therapists. However, in the null models (“intercept and slope only” models), therapist variability in the outcome scores (BDI; GSI) were negligible and non-significant, hence we dropped this level in the final analyses presented here. Also, when we added the therapist level with the alliance predictors, the models had problems converging. All predictor and control variables (such as baseline symptom severity) were entered at level 2 (i.e., the dyadic/patient level).

The analyses investigating convergence as a predictor of change in the outcome variables (GSI, BDI) began by estimating a null model that only contained the fixed effects of intercept and time, along with random effects of intercept and time (i.e., allowing both the intercept and slopes to vary across patients) (Model 1). This was done to test whether there was

significant variability in change trajectories of symptom distress, which is a precondition for carrying on with predictor analyses (e.g., Hox, 2010). In the next step, a “Convergence only” model (Model 2) (see Coyne et al., 2018) including fixed and random intercept and slope along with the dyadic convergence parameter and initial symptom severity was estimated to address the convergence hypothesis (whether convergence was associated with the reduction of symptoms). This model represented a test of the marginal (unique) effect of convergence on outcome, while accounting for baseline symptom distress.

However, since this model did not distinguish between convergence with regards to varying alliance levels at the dyadic level or whether the alliance was perceived to improve or worsen over time (which would reflect two different kinds of processes clinically), in Model 3, we also entered additional parameters to control for: (1) early patient-rated and therapist-rated alliance (so that any effect associated with covariance between the initial level of the dyad’s alliance scores and subsequent convergence could be taken into account); (2) early alliance discrepancy (patient-rated minus therapist rated alliance at the first measurement), to ensure that potential effects of convergence would not be conflated with effects due primarily to highly discrepant alliance ratings early on in treatment; (3) mean alliance change of the dyad across treatment (i.e., dyadic alliance development) to ensure that obtained effects of convergence would not be conflated with general improvement in the level of the alliance. Finally, we added the (4) interaction between dyadic alliance development and alliance convergence, so that any specific effects of convergence dependent upon the magnitude and direction of mean alliance development in the dyad could be identified.

Hence, the full model tested patient-therapist alliance convergence while holding the other alliance parameters constant, in line with recommendations from recent studies (Laws et al., 2017) since convergence may have a differential impact in atypical cases of alliance level and change through treatment.

These variables were entered as predictors of both BDI and GSI intercept and slopes in two separate sets of models. In Study 2, all the same models were analyzed and the model testing involved assessing the effect of alliance convergence across sessions 3, 12 and 20 as predictor of symptom change across the same sessions and including the final session (T2). Hence we tested the following model in both studies:

#### **Model 1 (Null-Model):**

Level-1 Model:

$$\text{Outcome}_{ij} = b_{0j} + b_{1j}(\text{Time}_{ij}) + r_{ij}.$$



Level-2 Model (patient level):

$$b_{0j} = g_{00} + u_{0j}$$

$$b_{1j} = g_{10} + u_{1j}$$

**Model 2 (“Convergence only”):**

Level-1 Model:

$$\text{Outcome}_{ij} = b_{0j} + b_{1j}(\text{Time}_{ij}) + r_{ij}.$$

Level-2 Model (patient level):

$$b_{0j} = g_{00} + g_{01}(\text{initial symptoms}_j) \\ + g_{02}(\text{alliance convergence}_j) + u_{0j}$$

$$b_{1j} = g_{10} + g_{11}(\text{initial symptoms}_j) \\ + g_{12}(\text{alliance convergence}_j) + u_{1j}$$

**Model 3: (Convergence and covariates model):**

Level-1 Model:

$$\text{Outcome}_{ij} = b_{0j} + b_{1j}(\text{Time}_{ij}) + r_{ij}.$$

Level-2 Model (patient level):

$$b_{0j} = g_{00} + g_{01}(\text{initial symptoms}_j) \\ + g_{02}(\text{alliance convergence}_j) \\ + g_{03}(\text{patient} - \text{rated WAI}_j) \\ + g_{04}(\text{therapist} - \text{rated WAI}_j) \\ + g_{05}(\text{alliance development}) \\ + g_{06}(\text{early WAI discrepancy}) \\ + g_{07}(\text{alliance convergence} \\ \times \text{alliance development}_j) + u_{0j}$$

$$b_{1j} = g_{10} + g_{11}(\text{initial symptoms}_j) \\ + g_{12}(\text{alliance convergence}_j) \\ + g_{13}(\text{patient} - \text{rated WAI}_j) \\ + g_{14}(\text{therapist} - \text{rated WAI}_j) \\ + g_{15}(\text{alliance development}) \\ + g_{16}(\text{early WAI discrepancy}) \\ + g_{16}(\text{alliance convergence} \\ \times \text{alliance development}_j) + u_{1j}$$

Estimation was done with Restricted Maximum Likelihood (RML) and using an unstructured error covariance matrix (UN) for the random effects which allowed for correlated residuals. All models were compared using an indicator of model fit (AIC) and significance of predictors using the conventional significance level of  $p \leq .05$  (two-tailed).

## Results

### Study 1

As can be inspected from [Figure 1](#) of the patient-rated and therapist-rated WAI raw scores (with error bars around the mean, and with lines indicating  $\pm 1$  SD), the alliance was seen by both therapist and patient to increase over time (see also the supplementary material, S1, with means and SDs of patient-rated and therapist WAI at all measurements). However, on average, therapist-patient alliance ratings did not converge as treatment progressed (i.e., they did not approach each other over time). Moreover, in the beginning the therapists (not the patients) tended to rate the alliance as higher, while this changed as therapy progressed (when patients rated the alliance as higher). Also, when the alliance was rated by patients, there was more distribution of scores compared to when it was rated by the therapists who used a more narrow range of the scale when scoring the alliance.

Model 1 (“null model”) indicated a significant reduction of symptoms across treatment. Also, patients had significantly different intercepts and slopes (random effects model). In Model 2 (“convergence only”), predicting change in BDI by a convergence parameter and controlling for initial symptoms, the convergence parameter was not related to the BDI intercept (coeff. = 1.19,  $p = .63$ ) nor to the BDI slope (coeff. =  $-.08$ ,  $p = .83$ ).

In the full model (Model 3), controlling for BDI at pretreatment, as well as early patient-rated and therapist-rated alliance levels, early discrepancy and dyadic alliance development, the pattern remained the same (i.e., convergence was still not a predictor of outcome; slope coeff. =  $-.33$ ,  $p = .484$ ). Of the covariates in this model, early patient-rated alliance was not associated with the BDI intercept. However, when rated by the therapists, it was significantly associated with levels of depression to the effect that the higher the therapist-rated WAI, the lower the patients’ level of depression (coeff. =  $-4.42$ ,  $p = .032$ ), but none of these parameters were associated with the average BDI slope (and hence was not related to outcome). Dyadic alliance development was a positive predictor of outcome (coeff. =  $-.87$ ,  $p = .004$ ), to the effect that an increase of the patient-therapist alliance level over time (note that this is not an indicator of alliance convergence but rather of an increase in dyadic alliance), was associated with a reduction of depressive symptoms during treatment. Finally, we also tested the three-way interaction between the convergence variable, dyadic alliance development and the slope. This test indicated that the effect of convergence on BDI-change did not depend upon variations in the change of dyadic alliance scores (coeff. =  $.11$ ,  $p$

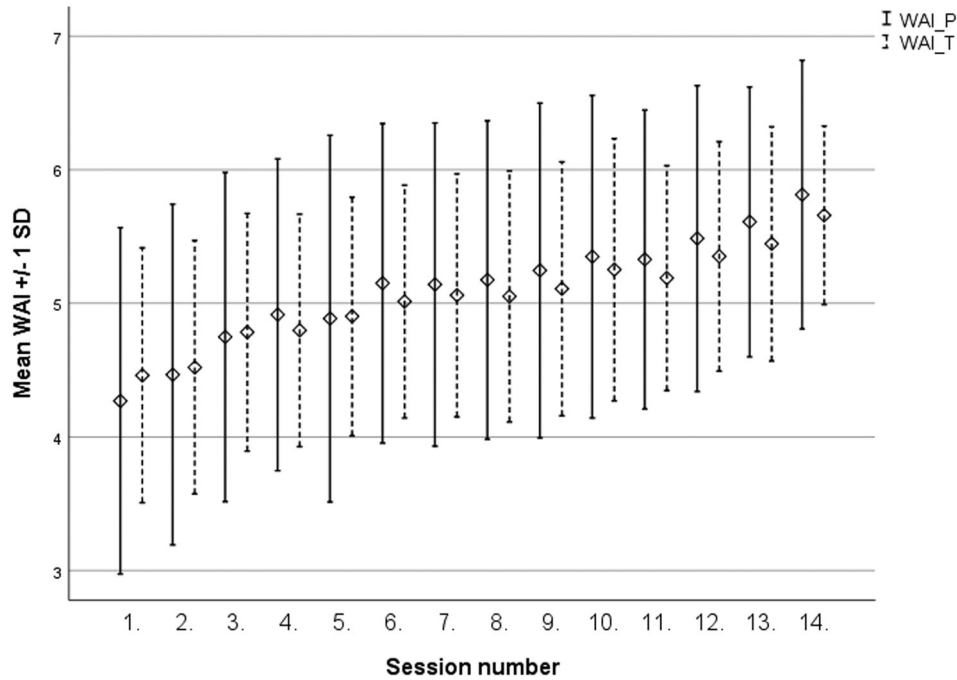


Figure 1. Raw scores of WAI-P and WAI-T across treatment (Study 1).

Table I. Results of dyadic multilevel growth curve modeling, Study 1.

Fixed effects	Model 1 Null model			Model 2 Convergence only			Model 3 Convergence and covariates model		
	Est.	S.E	p-value	Est.	S.E	p-value	Est.	S.E	p-value
Intercept	33.4	1.16	<.001	32.78	.84	<.001	32.43	1.50	<.001
Slope	-.79	.13	<.001	-.82	.14	<.001	-.94	.25	<.001
Initial symptoms				.88	.08	<.001	.77	.10	<.001
Converg.				1.19	2.44	.626	-.05	2.87	.986
Converg. × Time				-.08	.39	.831	-.333	.46	.484
Early WAI-P							-.69	1.45	.635
Early WAI-T							-4.42	2.03	.032
Alliance dev.							.66	1.61	.683
Early P-T discrep.							1.05	3.33	.753
Early WAI-P × Time							-.08	.23	.745
Early WAI-T × Time							-.16	.32	.615
Alliance develop. × Time							-.87	.29	.004
Early P-T discrep. × Time							.37	.55	.504
Alliance dev. × Converg. × Time							.11	.73	.881
<b>Random effects</b>	<b>Est.</b>	<b>S.E</b>	<b>p-value</b>	<b>Est.</b>	<b>S.E</b>	<b>p-value</b>	<b>Est.</b>	<b>S.E</b>	<b>p-value</b>
Intercept	49.84	9.35	<.001	50.78	1.30	<.001	49.10	9.22	<.001
Cov.	-1.85	1.11	.096	-1.81	1.10	.099	-1.94	1.13	.086
Slopes	1.24	.26	<.001	1.31	.26	<.001	1.22	.26	<.001
AIC	6953			6500			6449		

Note. Dependent variable is BDI-II (longitudinal) from sessions 1 through 14. S.E = Standard error. WAI = Working Alliance Inventory. Convergence = Multilevel growth coefficient representing reduction in discrepancy (i.e., increased agreement) between P and T across WAI measurements obtained after sessions 1 through 14. Early WAI-P = patient-rated WAI at first measurement. Early WAI-T = therapist-rated WAI at first measurement. P-T discrep. = (WAI-P – WAI-T) at first measurement. Alliance develop. = Longitudinal estimate of change in dyadic alliance across sessions. Cov. = Covariance between random intercepts and random slopes. Predictors were centered for interpretability. **Bold** = Significant at either  $p < .05$ ,  $p < .01$  or  $p < .001$ . AIC = Aikakes Information Criterion.  $N = 96$ .

= .881). This further suggests that convergence did not have a differential effect on outcome depending on whether the alliance was improving or worsening. See Table I with the results of the dyadic growth curve modeling (Models 1–3).

## Study 2

As can be inspected from Figure 2 depicting the raw scores of patient-rated and therapist-rated WAI (with error bars around the mean and lines indicating  $\pm 1$  SD), the alliance scores increased over time (see also S2 with means and SDs of patient-rated and therapist-rated WAI at all measurements), and the patient-therapist alliance seemed to converge (i.e., approached each other) across time. Patients tended to rate the alliance as higher than therapists and they used a broader range of the scale when scoring the alliance (just as in Study 1).

Since the models with all three levels had problems converging (like in Study 1), we only retained two levels (repeated measurement as level 1 and patients/dyads as level 2) excluding the therapist level (i.e., therapist variability in random GSI growth curve scores). We tested the same parameters as in Study 1, but we modeled GSI (outcome) only across sessions 1, 3, 12 and 20, and then at treatment termination (T2), in order to see if alliance convergence predicted symptom change across the same sessions, including the final session. All patients included had a

minimum of 20 sessions of psychotherapy, but some had longer treatments (thus we also controlled for number of sessions in the MLM analyses, see below).

As in Study 1, in the “convergence only” model (Model 2), the convergence parameter was not a significant predictor of outcome; that is, the product term  $\text{Convergence} \times \text{Time}$  was not significant (coeff. = .03,  $p = .648$ ). In the full model (Model 3), also controlling for early dyadic alliance level, early alliance discrepancy and dyadic alliance development, convergence was still not a predictor of outcome (coeff. =  $-.04$ ,  $p = .775$ ). However, it was related to GSI intercept, indicating that more symptoms were negatively related to increased patient-therapist agreement in alliance over time (coeff. =  $-.34$ ,  $p = .050$ ). The results showed that none of the other alliance parameters were significant predictors of intercept and slope of GSI. As in Study 1, the effect of convergence on the GSI slope did not depend upon variations in the development of dyadic alliance scores.

We also tested the three-way interaction between convergence, change in dyadic alliance scores and the GSI slope. This product term was not significant, indicating that the effect of convergence on GSI-change did not depend upon variations in the development of dyadic alliance scores (coeff. = .04,  $p = .590$ ). See Table II with the results of the dyadic growth curve modeling from Study 2.

In order to see if the varying treatment lengths impacted on these relationships (or lack thereof),

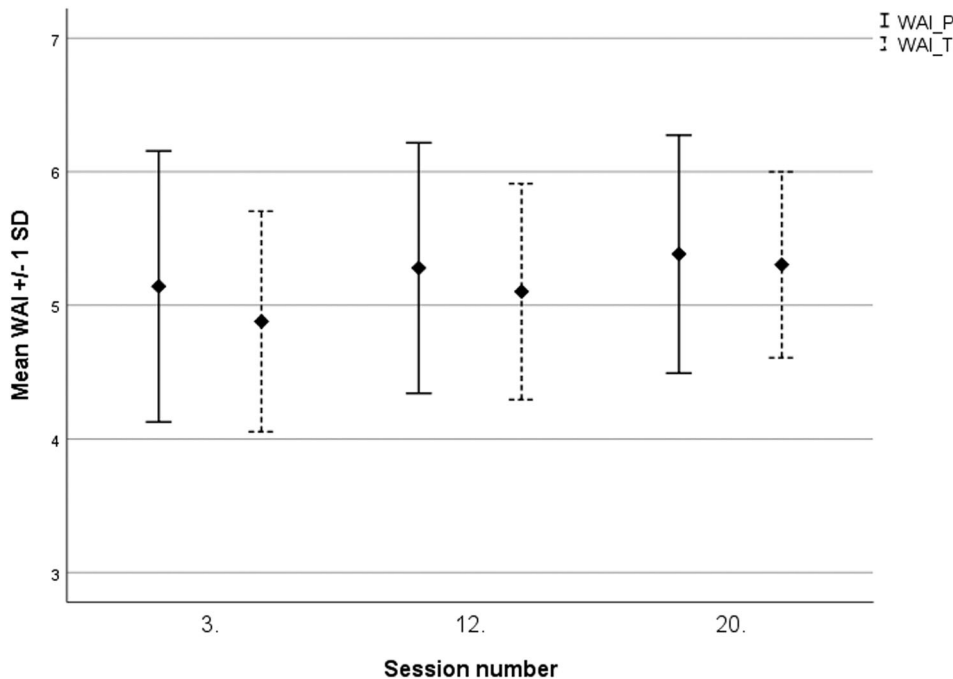


Figure 2. Raw scores of WAI-P and WAI-T across treatment (Study 2).

Table II. Results of dyadic multilevel growth curve modeling, Study 2.

Fixed effects	Model 1 Null model			Model 2 Convergence only			Model 3 Convergence and covariates model		
	Est.	S.E	<i>p</i> -value	Est.	S.E	<i>p</i> -value	Est.	S.E	<i>p</i> -value
Intercept	1.28	.04	<.001	1.28	.02	<.001	1.28	.02	<.001
Slope	-.10	.01	<.001	-.08	.01	<.001	-.09	.01	<.001
Initial symptoms				.92	.03	<.001	.88	.03	<.001
Converg.				-.12	.08	.153	-.34	.18	.050
Converg. × Time				.03	.06	.648	-.04	.12	.775
Early WAI-P							.02	.03	.568
Early WAI-T							-.02	.04	.562
Alliance develop.							-.01	.04	.856
Early P-T discrep.							.09	.06	.143
Early WAI-P × Time							-.003	.02	.888
Early WAI-T × Time							-.02	.02	.430
Alliance develop. × Time							-.01	.02	.422
Early P-T discrep. × Time							.02	.04	.709
Alliance develop. × Converg. × Time							.04	.08	.590
<b>Random effects</b>	<b>Est.</b>	<b>S.E</b>	<b><i>p</i>-value</b>	<b>Est.</b>	<b>S.E</b>	<b><i>p</i>-value</b>	<b>Est.</b>	<b>S.E</b>	<b><i>p</i>-value</b>
Intercept	.34	.04	<.001	.002	.001	<.001	.002	.000	<.001
Cov.	-.017	.007	.010	.0005	.002	.060	.004	.003	.095
Slopes	.01	.002	<.001	.01	.002	<.001	.011	.002	<.001
<b>AIC</b>	1379			461			509		

Note. Dependent variable is SCL90-R, GSI (longitudinal) based on sessions 1, 3, 12, 20 and termination (T2). S.E = Standard error. WAI = Working Alliance Inventory. Convergence = Multilevel growth coefficient representing reduction in discrepancy (i.e., increased agreement) between P and T across WAI measurements obtained after sessions 3, 12 and 20. Early WAI-P = patient-rated WAI at first measurement. Early WAI-T = therapist-rated WAI at first measurement. P-T discrep. = (WAI-P – WAI-T) at first measurement. Alliance develop. = Longitudinal estimate of change in dyadic alliance across sessions. Cov. = Covariance between random intercepts and random slopes. Predictors were centered for interpretability. **Bold** = Significant at either  $p < .05$ ,  $p < .01$  or  $p < .001$ . AIC = Aikakes Information Criterion.  $N = 139$ .

we controlled for number of sessions in a final model in Study 2. Number of sessions did not have an effect on the results, hence we do not report this.<sup>5</sup>

### Discussion

To better understand the complexity of dyadic processes in psychotherapy, such as the mechanisms of the working alliance, researchers recommend taking advantage of innovations in data analytic procedures when studying the interactions between therapists and patients that are associated with favorable therapeutic outcomes (Atzil-Slonim & Tschacher, 2020). Inspired by a recent line of alliance research using dyadic multilevel modeling to assess the role of convergence in the patient-therapist working alliance, the present study investigated the hypothesis that increased similarity in ratings of the alliance across treatment (alliance convergence) would be associated with better outcomes (Coyne et al., 2018).

A distinction has been made between alliance *congruence* (agreement in scores at one point in time) and alliance *convergence*, i.e., a gradual development toward a more convergent view on the state of the

alliance (see Coyne et al., 2018). Not surprisingly, there are indications to suggest that when two participants (i.e., infant-caregiver, members of a romantic couple) develop to become more similar in their emotions and viewpoints, their relationship is found to be more satisfying and profound (see Beebe & Lachman, 1998; Coyne et al., 2018). Correspondingly, do more similar views on the working alliance lead to more fruitful therapy results?

Using two data sets from different treatment contexts in Scandinavia (one RCT comparing CBT and IPT for treatment resistant depression, and one naturalistic study involving a breadth of clinical diagnoses who were treated with open-ended therapies of different kinds), we tested this hypothesis. Contrary to some recent studies (Coyne et al., 2018) suggesting that convergence plays a unique role in the therapeutic process, we did not find that alliance convergence predicted outcome in symptom distress. This was the case in both samples and treatment contexts, also when controlling for alliance level, dyadic alliance development as well as direction of convergence (whether the participants agreed on an improving or deteriorating alliance). However, we did find that convergence was related to a higher level of

symptom severity in the naturalistic study, such that more symptoms were negatively related to increased agreement in alliance over time.

To our knowledge, only two prior studies have investigated the convergence hypothesis (Coyne et al., 2018; Laws et al., 2017). While Coyne et al. (2018) found support for the hypothesis (with a moderate effect size), in the sense that when patient-therapist agreement across the first half of treatment increased, symptoms of generalized anxiety were reduced in the second half of treatment, Laws et al. (2017) only found partial support for the convergence hypothesis, and the effect size of convergence was small (see Laws et al., 2017). It is noteworthy that Laws et al. found that convergence in alliance ratings was not predictive of declines in depression as rated by HAMD across therapy, but was predictive of lower HAMD scores at 3-months follow-up. It is less plausible that increasing patient-therapist consensus on alliance quality *during* therapy was responsible for the scores at follow up, given the lack of association between the constructs when rated closer in time. Instead, a more viable interpretation of this association could be that some unmeasured third variable accounted for both the increased convergence and the beneficial follow-up scores. One candidate might be a more stable interpersonal propensity, for example degree of interpersonal hostility/affiliation on the part of the patient (see also Constantino & Smith-Hansen, 2008) which may color the perception of the dyadic alliance as well as influence levels of depression in the longer run, but further research is needed to understand the individual and dyadic influences in these relationships.

Indeed, since we found that our multilevel convergence parameter predicted symptom severity (i.e., the GSI intercept), we may infer that there is a relationship between how much dyadic agreement one can achieve, on the one hand, and (presumably rather stable) patient characteristics on the other. Prior research has also found certain therapist variables (i.e., a more “distressing practice pattern”) to predict divergence of patient-therapist alliance ratings (Hartmann et al., 2015) indicating that convergence is influenced by characteristics of the therapist too.

One explanation for the lack of consistency in findings between the now four studies exploring the convergence hypothesis, may have to do with (subtle) differences in statistical procedures or operationalization of constructs. For example, while Coyne et al. investigated the effect of alliance convergence during the first half of treatment as predictor of change in symptoms during the second half of treatment, Laws and colleagues (like us), investigated convergence as predictor of simultaneous change in symptoms

across treatment. We believe the convergence hypothesis is best tested across treatment for these samples, as it may take some time to establish a working alliance with these patients (see Laws et al., 2017). However, this difference in choice means that results are not entirely comparable across studies.

Other differences regard the measurement of mental health disturbance i.e., whether one relies on observer-ratings or self-report to capture the clinical severity of an individual. The current two studies are the only ones basing outcome on the self-report of the patients themselves. The patients in our two samples suffered from high levels of symptom distress and had relatively severe diagnoses. For example, the average pre-treatment score of BDI-II in Study 1 was almost 36, indicating severe depression (Beck et al., 1996), while the average level of depression severity in the sample investigated by Laws et al. (2017) was less than 20 on the HAMD, indicating a more moderate symptom severity (e.g., Zimmerman et al., 2013). Moreover, the samples in both studies had extensive Axis II comorbidity (Ekeblad et al., 2016; Nordmo et al., 2020). Additionally, in Study 1, the majority (almost 60%) of the patients used some kind of antidepressant medication. Cumulatively, these factors may have limited the potential for increased dyadic alliance agreement over time, and thus impeded our chances of observing a beneficial effect of alliance convergence on the outcomes of the patients seen.

In fact, contrary to some other studies, agreement about the alliance did not increase as treatment progressed in the RCT study of treatment resistant depression (Study 1), but it did increase on average in the naturalistic study (Study 2), even if this in turn was not related to better outcomes. This may indicate that the dyads in our samples may have struggled more to get “on the same page,” even if the treatments were mostly successful in terms of outcome (see Ekeblad et al., 2016; Nordmo et al., 2020). Our results suggest that alliance convergence may not represent a meaningful process variable in all treatments, and that getting “more on the same page” in terms of increasingly similar viewpoints of the working alliance may not be reflective of more helpful work.

The absence of a relationship between alliance convergence and outcome aligns, albeit indirectly, with other notable findings in the field. The results of one study indicated that agreement on alliance ratings did not influence the clients’ evaluations of session impact, and agreement between the two rater perspectives did not increase as therapy progressed (Fitzpatrick et al., 2005). The authors claimed that lack of convergence in the perception of the working alliance is an expected scenario of

the therapeutic relationship and “does not necessarily represent a hindrance to constructive treatment” (Fitzpatrick et al., 2005, p. 69). The results of our studies seem to suggest the same. Applying a mixed-method design, Chui et al. (2020) found that agreement between therapists and clients about what was helpful in therapy was related to treatment outcome, whereas agreement about what they *wished had happened* in therapy, was not.

We are also reminded of research indicating that therapists and patients emphasize different aspects of their mutual work and relationship when rating the alliance. For example, clients emphasize *collaboration*, while therapists underscore *client involvement* when they score the alliance (Bachelor, 2013). This may reflect the therapist’s idea that a fruitful therapeutic relationship involves active participation of the client, including commitment to the tasks of therapy and willingness to disclose information (Bachelor, 2013). Indeed, Horvath (2000) suggests that therapists view the relationship through a “theoretical lens” while patients compare it with other relational experiences, which could lead to an epistemological difference between the two, but this does not necessarily imply that their collaboration is not working.

One of the logical clinical implications of the relatively strong association between alliance and outcome is that therapists should be advised to ensure an *explicit* agreement of tasks and goals with every client early in treatment. Interestingly, a line of studies (e.g., Oddli et al., 2014), investigating processes and outcomes of highly experienced psychotherapists show that despite high mutual alliance ratings, in-depth qualitative analyses of transcripts from initial sessions yielded that the therapy dialogues did not contain many instances of explicit negotiations of the tasks or goals of the treatment. The authors noted that: “Processes regarding hope, motivation and engagement rather than explicit goal agreement” (Oddli et al., 2014, p. 245) were observable in these sessions.

Such findings indicate that there might not be a direct, one-to-one relationship between high alliance and evidence of explicit agreement on tasks and goals in the actual therapy dialogue (see also Jennissen et al., 2020). Instead, high alliance ratings may reflect an underlying sense of commitment and understanding rather than the active agreement on what to do and what to achieve in treatment.

According to the work of Safran (1993), “mis-meetings” or alliance ruptures between therapists and their clients provide opportunities to explore barriers to relatedness that may occur for the client in everyday life. Essentially, to learn as a client that one can achieve both a mutual alliance and be “on

the same page”—yet also experience and tolerate divergence and difference of perception, may be therapeutic in and of itself and can work as a template for other relations the client engages in. As such, alliance divergence might be a particular type of alliance rupture in which one member of the therapeutic dyad rates the alliance as weak while the other member of the therapeutic dyad is unaware of any strains in the therapeutic relationship and therefore rates the alliance as strong(er). While some divergence is inevitable—even constructive, Safran and colleagues argue that substantial ruptures are not, and their idea is that breaches in the alliance produce a chance for re-coordination, re-attunement and eventually repair which in turn leads to productive change. This negotiation process is assumed to be a central change process throughout treatment (Jennissen et al., 2020). There are empirical indications to suggest that alliance ruptures and subsequent repair can benefit the change process (Safran et al., 2013). These dynamics may outweigh a potentially favorable effect of alliance convergence in the current study. That is, productive alliance work may not take the form of a linearly increasing agreement between therapist and patient but rather as idiosyncratic fluctuations in the dyadic alliance obscuring the relationship between convergence and outcome (at least in our study).

Also, a fair proportion of patients have severe difficulties in forming trustful relationships; indeed believing that they can be *liked by the therapist* (as assessed by the WAI) due to a deep shame about who they are (typical for complex, relational trauma and higher levels of clinical disturbance; see for example Halvorsen et al., 2016). Such a vulnerability may lead to atypical alliance trajectories which may further complicate the relationship between convergence in alliance and outcome.

### Clinical Implications and Conclusion

Since only one of four studies on alliance convergence unambiguously supports the idea that convergence is a promising process variable, we may speculate that an increased patient-therapist alignment in more fundamental experiential or relational dimensions, such as non-verbal synchrony, emotional attunement and affective co-regulation, are more essential than an increasing consensus in dyadic alliance or alliance convergence (see Atzil-Slonim & Tschacher, 2020; Jennissen et al., 2020).

Clinicians often hear that they should make sure to “foster a shared perspective on the alliance through treatment with every client.” However, this may be an oversimplification of the complex nature of the

therapy relationship and likely a too literal translation of the positive alliance-outcome association (such as findings on alliance congruence and/or convergence over time) into practical guidelines for therapeutic action. As Jennissen et al. (2020) recently put it:

it should be noted that in clinical practice, high consensual alliance ratings may *not* be a result of explicit discussions on the tasks and goals of therapy. Instead, an automatic process of *attunement* and a *sense of communion* between the patient and the therapist might create high agreement in the alliance ratings. (p. 334, emphasis added)

Along those lines, we hope our findings may further stimulate a conception of the therapy relationship and collaboration as a highly complex phenomenon that evades simple inferences.

### Limitations

With our  $N = 96$  (Study 1) and  $N = 139$  (Study 2) dyads, we have comparable sample sizes to those of similar studies (e.g., Coyne et al., 2018), however we had a lower number of dyads than Laws et al. (2017), and it may be that our samples were not large enough to detect a relationship between convergence and outcome in our patient populations. Note, however, that Laws et al., who had a large sample size, only found partial support for the convergence hypothesis. With regard to statistical power, we had estimated that for 80% power at an alpha of .05, we would need 44 dyads to find a standardized regression slope of  $\beta = .40$  (a medium to large effect) while 82 dyads would suffice to find a  $\beta = .30$  (a medium effect). To find a smaller effect size of a  $\beta = .20$ ,  $N = 191$  dyads would be needed. Thus, our samples should be sufficiently powered for obtaining a medium-sized regression slope, but we did not have sufficient power to obtain a small-sized effect of convergence. This is a limitation that should be taken into consideration.

In Study 2 the treatments were predominantly open-ended, hence the treatment lengths were individualized and varied considerably, while the treatments of Study 1 were short-term and time-limited. This may have differentially impacted on the alliance-outcome relationship in the two studies. To remedy this potential problem (of comparing different relationships), we restrained the analysis of convergence (in the naturalistic study) to dyadic alliance of sessions 3, 12 and 20 to ensure we modeled dyadic alliance development similarly in all cases and also restrained the trajectory of symptom distress to those same sessions, but added symptom level at the end of treatment so as to test convergence as a predictor of symptom development

including final outcome. However, we did not study alliance convergence across the entire course of treatment for all patients, like we did in Study 1. To compensate further for this limitation, we also controlled for number of sessions in our final analysis and conducted exploratory analysis on those patients who had received less than 40 sessions (thus making their treatment lengths more uniform), but this did not change the finding. To us, this consistency across samples and analyses strengthens the idea that convergence is not a substantial contributor to outcome, at least not in our data.

Even though there were relatively few missing data in these studies, it is possible that convergence scores for dyads with missing data may have been more unreliable. However, in Study 1 there were numerous alliance assessments for each dyad to compensate for that, and in Study 2, all dyads had complete data of alliance measures (only those dyads with complete data from sessions 3, 12 and 20 and post-treatment were included in Study 2), which reduces the impact of this problem.

In Study 1 only patients with (treatment resistant or persistent) major depression were included, thus results about the (lack of a) relationship of convergence and outcome may not generalize to other patient populations. However, in Study 2 all kinds of mental health problems were included with the exception of acute psychosis and major substance abuse, and the same results were found which may increase generalization of our findings.

Lastly, both our studies were conducted in Scandinavian treatment contexts. This is not regarded as a limitation but rather a strength. We would argue that we need studies and replications from outside of the Anglo-American sphere. Due to the developed public mental health systems of these countries with good access to mental health care for all patients, Scandinavian countries are also well suited for studying relationships between process and outcome for patients with varying mental health problems and disorders.

### Supplemental data

Supplemental data for this article can be accessed <https://doi.org/10.1080/10503307.2020.1823030>.

### Notes

<sup>1</sup> However, such pseudo- $R^2$  estimates are unreliable and context specific, so caution should be applied in concluding from the results (Singer & Willett, 2003).

<sup>2</sup> Information was lacking to calculate effect size of this association but the authors indicated that the effect size was “small” (see Laws et al., 2017, p. 420).

- <sup>3</sup> Information was lacking to calculate effect size of this association.
- <sup>4</sup> Even if the therapists and patients in this study rated the alliance using slightly different short forms of the WAI, we have examined the overlap between the two versions and it turned out to be high, Hatcher, Lindqvist, & Falkenström, (2020).
- <sup>5</sup> We also did exploratory analysis on a subsample ( $N=67$ ) of patients who had received up to 40 sessions (= min of 20 sessions and a maximum of 40 sessions, for which we had complete dyadic alliance and outcome data) in Study 2. Convergence did not play a role in predicting outcome in this subsample where treatment length was more uniform.

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