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Therapist-Level Moderation of Within- and Between-Therapist Process–Outcome Associations

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Objective: Although higher quality patient–therapist alliance and more positive patient outcome expectation (OE) consistently predict symptomatic/functional improvement in psychotherapy, most research has failed to capture the nuance in these process–outcome relations by parsing them into *within-therapist* (i.e., differences between patients treated by the same therapist) and *between-therapist* (i.e., differences between therapists' average process/outcome ratings across all patients in their caseloads) components. Moreover, the few studies that have done so have produced mixed results, suggesting the possibility of systematic variability in these associations (i.e., moderators). One potential source of such variability could be providers themselves; that is, different therapists could use these processes to differing therapeutic benefit. This study tested the alliance– and OE–outcome associations at both the within- and between-therapist levels and explored therapist-level moderators of them. ***Method:*** Data derived from 212 adult outpatients treated naturalistically by 42 psychotherapists as part of a randomized trial that compared different case-assignment methods. Patients completed measures of alliance, OE, and outcome repeatedly throughout treatment. Therapist characteristics were assessed at baseline. ***Results:*** Multilevel structural equation models revealed that, at the between-therapist level, only higher alliance quality, but not more optimistic OE, was associated with greater caseload-level improvement. At the within-therapist level, only more optimistic OE, but not higher alliance quality, was associated with patient improvement. Finally, therapists' self-perceived alliance-fostering effectiveness and cognitive–behavioral orientation moderated the within- and between-therapist alliance–outcome associations, respectively. ***Conclusion:*** Results indicate that different therapists use common treatment processes to differing therapeutic benefit, which can inform more personalized clinical practices and trainings.

What is the public health significance of this article?

This study adds multilevel nuance to process–outcome associations in psychotherapy. Namely, the results indicate that different therapists use the therapeutic alliance and harness patient outcome expectation to differing therapeutic benefit, which can inform more personalized case assignments, clinical practices, and trainings. Preliminarily, the alliance has stronger *within-therapist* associations with outcome in the hands of therapists who are humbler in assessing their own alliance-fostering abilities and stronger *between-therapist* associations with outcome for therapists who do not identify strongly with a cognitive–behavioral orientation.

Keywords: within- and between-therapist effects, multilevel process–outcome associations, therapeutic alliance, outcome expectation, therapist-level moderation

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Voluminous research has established that some of the most consistent predictors of positive psychotherapy outcomes have been process variables (i.e., relational and participant variables that occur within the confines of a treatment course) that are largely transtheoretical and transdiagnostic or what have been termed "common" factors (Norcross & Lambert, 2018). Perhaps

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the quintessential *relational* process factor is the therapeutic alliance or the emotional and collaborative bond between a patient and therapist (Bordin, 1979). In the most recent meta-analysis of over 30,000 patients from 295 samples, a higher quality alliance was associated with greater patient improvement across different treatments and patient diagnoses ($d = .58$; Flückiger et al., 2018). Similarly, a prominent *patient* process factor is outcome expectation (OE) or the prospective belief about the likelihood that one's therapy will be effective. In the most recent meta-analysis of over 12,000 patients from 81 samples, more positive early treatment OE was associated with greater patient improvement across diverse treatments and patient problems ($d = .36$; Constantino et al., 2018). Although these findings compellingly demonstrate that alliance and OE influence psychotherapy outcomes, their *clinical* utility is somewhat limited due to a notable methodological issue. Namely, most research on the alliance and virtually all research on OE has failed to account for the complex structure of most psychotherapy data, whereby single therapists treat multiple patients (Baldwin & Imel, 2013). As a result of this nesting, all during therapy processes can be parsed into *patient* and *therapist* contributions, which can have meaningfully different clinical implications.

The patient component, or what is often termed *within*-therapist variability, reflects differences in a given process (e.g., alliance quality, OE) among different patients working with the *same* therapist. The therapist component, or what is often termed *between*-therapist variability, reflects differences in therapists' average given process (e.g., alliance quality, OE) across all patients in their caseloads. Importantly, if within-therapist differences in psychotherapy processes predict an outcome (e.g., as patients' alliance scores increase relative to their same therapist's average alliance score, it correlates significantly with, e.g., symptom reduction), it necessarily points to something about the patient or the individual patient–therapist dyad driving this effect, but *not* specifically the person of the therapist (who is held constant in this scenario; Baldwin & Imel, 2013). In contrast, if between-therapist differences in processes predict an outcome (e.g., as the average patient OE score that a therapist cultivates across all cases increases, relative to other therapists' average OE scores, it correlates significantly with, e.g., improved functioning), it necessarily points to something about the therapist (and how they influence all of their therapy relationships) driving this effect, as opposed to the individual patient or dyad (Baldwin & Imel, 2013). Of course, both components could uniquely and simultaneously influence patients' treatment outcomes. Thus, determining whether process–outcome associations operate at the within-therapist level, between-therapist level, or at both levels is necessary for illuminating the precise clinical meaning of these associations and for understanding the utility of different therapeutic processes. Yet, as noted, despite the voluminous literature on process–outcome associations (Constantino et al., in press), relatively few studies have parsed such associations into their patient and therapist contributions.

Of the rare existing studies that have parsed these components, almost all have focused on the alliance construct, producing somewhat mixed results. On the one hand, three studies have demonstrated that *both* within- and between-therapist alliance quality correlated significantly with patients' outcomes (Huppert et al., 2014; Marcus et al., 2011; Zuroff et al., 2010). On the other hand, five studies have demonstrated that only one of the alliance components correlated significantly with patients' treatment outcomes; specifically, whereas some found that greater between-, but not within-, therapist alliance quality was associated with better outcomes (Baldwin et al., 2007; Crits-Cristoph et al., 2009, 2018), others found that greater within-, but not between-, therapist alliance quality related to improvement (Crits-Christoph, Hamilton, et al., 2011; Falkenström et al., 2014). Thus, the existing research rather evenly supports the between- and within-therapist associations, though both can be considered somewhat variable across studies.

Such mixed results may suggest systematic variability in the size of these associations (i.e., moderators); that is, there may be some contexts that render one or both of these components more versus less relevant for treatment outcome. Plausibly, such variability may be due to differences between therapists in the extent to which they use, or harness, alliance quality to achieve positive outcomes. Put differently, similar to *patient-level* findings that higher quality alliance is more therapeutic for some patients than others (e.g., Constantino et al., 2017; Zack et al., 2015), it may be that higher quality alliance is more therapeutic in the hands of some *therapists* than others. Adding even more complexity, such therapist differences in the alliance–outcome association could theoretically exist at both levels of analysis.

At the within-therapist level, such variability could mean that even if two patients (treated by different therapists) both rated their alliance five points higher than their therapist's usual alliance, this rating might translate into a 4-point improvement (on some hypothetical outcome) for Therapist A's patient and a 10-point improvement for Therapist B's patient. At the between-therapist level, such variability could mean that even if Therapist A and Therapist B both achieved exactly the same average alliance level across all patients in their caseloads (i.e., they achieved similar relational "climates" across their patients), this might translate into an 8-point improvement for Therapist A's average patient's outcome and a 2-point improvement for Therapist B's average patient's outcome. Thus, if such variability exists at either (or both) levels, it could be important to uncover the therapist-level characteristics/practices that set the condition for using the alliance to its fullest therapeutic potential (or, alternatively, for failing to harness the alliance for therapeutic good, or even for having higher quality alliance relate to worse outcomes).

Notably, some preliminary support exists for this type of therapist-level moderation. In the one study of which we are aware on this topic, the authors found that the size of the within-therapist alliance–outcome association varied significantly across therapists in a large sample of patients with heterogeneous disorders receiving various forms of inpatient therapy (Dinger et al., 2008). The authors concluded that, whereas for some therapists, greater alliance quality for individual patients was strongly associated with improvement for those patients, for other therapists, the variation in alliance quality was unrelated to their patients' outcomes. However, none of the therapist-level variables the authors investigated (i.e., gender, age, clinical experience) explained for which therapists the alliance was more (or less) therapeutic.

Although research parsing *other* (beyond the alliance) process–outcome associations into their within- and between-therapist components is virtually nonexistent, a few studies have descriptively examined whether nonalliance processes, like OE, vary at both the patient (within-therapist) and therapist (between-therapist) levels. For example, two studies have found that therapists account for a significant proportion of the variability in OE (Constantino et al., 2020; Vîslă et al., 2019). Thus, research that accounts for such therapist contributions by parsing the OE–outcome association into its within- and between-therapist components is sorely needed. Moreover, as with the alliance construct, it seems plausible that certain therapists compared to others could harness patient OE to greater therapeutic benefit.

Stated differently, in addition to continuing to disaggregate process–outcome associations, it could behoove the psychotherapy research field to search for therapist-level *moderators* of within-therapist, and possibly between-therapist, process–outcome associations. Uncovering such moderators could have meaningful practice and training implications. For example, with respect to training, such knowledge could allow for the development of personalized therapist trainings tailored to the processes that each therapist is empirically likely to use to therapeutic effect—a type of "playing to strengths" or the presence of therapist-level facilitative factors (Smith-Hansen, 2016). For example, if research revealed that a particular therapist tends to use the alliance to achieve positive outcomes (either when the alliance is higher than their own average, or when compared to other providers), that therapist may be best suited to receive training (graduate level or via continuing education) on and deliver relationally oriented interventions. Alternatively, knowledge of therapist-level moderators could direct personalized trainings toward the presence of therapist-level risk factors—a type of remediation strategy—to have therapists improve their use of more common processes to therapeutic benefit. Such personalization, in either form, *could* enhance the effects of clinical training, which, at present, tend to be unrelated to therapist effectiveness (Tracey et al., 2014).

Underscoring the potential utility of this approach, one researcher reflected on the lack of significant effects of an alliance-focused training on patient outcomes by noting that such trainings should be tailored to therapists' unique styles and approaches (Smith-Hansen, 2016). Similarly, another researcher noted that different therapists appeared to be differentially able to benefit from alliance-focused trainings, with some (particularly those who reported that they did not generally view the alliance as a key change mechanism) becoming *less* effective following such training (Crits-Christoph et al., 2010). Thus, research that can aid in personalizing training (and practice in general) to the therapist is sorely needed.

In this vein, the first aim of the present study was to test the alliance– and OE–outcome associations at both the within- and between-therapist levels in a sample of therapists delivering naturalistic treatment in a community mental health system. Given the mixed nature of the existing alliance literature and the novelty of this question for the OE literature, both of these analyses were largely exploratory. However, given the well-established *total correlations* between these processes and treatment outcomes (Constantino et al., 2018; Flückiger et al., 2018), we expected that the association with outcome for both alliance and OE would be significant for at least one level of analysis. The second aim of this study was to test whether the size of the within-therapist alliance– and OE–outcome associations varied across therapists.[1] Given previous findings on the alliance (Dinger et al., 2008), we hypothesized that the within-therapist alliance–outcome association would show significant between-therapist variability. Given the lack of such research on OE, this question was exploratory.

Finally, the third aim of this study was to explore whether specific therapist-level variables moderated the within- and between-therapist alliance– and OE–outcome associations. Given the limited (alliance) or nonexistent (OE) research to date, we drew on theory and findings from related research areas to select variables that may be the most likely to moderate these associations. First, although the alliance has been shown to relate to outcome across different treatments, different theoretical orientations propose different roles for the alliance in treatment (Hatcher & Barends, 2006; Zilcha-Mano, 2017). For example, whereas psychodynamic (PD) traditions have historically viewed the alliance as a direct mechanism of change, cognitive–behavioral (CB) traditions have often framed the alliance as a facilitative platform that allows other techniques (which are thought to be the primary mechanisms) to have a greater effect on improvement (Zilcha-Mano, 2017). Thus, it is possible that therapists who identify with different theoretical orientations may use the alliance differently, as a result of the framework that guides their practice. Similarly, although less commonly discussed in the literature, with regard to OE, CB approaches arguably place more emphasis on certain OE-related strategies (e.g., reviewing research support for a given treatment; Constantino et al., 2018) than PD approaches. Thus, given that CB and PD traditions arguably hold the most distinct views of the alliance (and possibly of OE), we focused on these two orientations in the present study.

Second, drawing on the aforementioned comments of researchers who have conducted alliance-focused trainings (Crits-Christoph et al., 2010; Smith-Hansen, 2016), it is also plausible that the degree to which therapists typically use interventions that focus on alliance (or OE) could influence the extent to which these variables relate to those therapists' outcomes. For example, therapists who openly discuss the patient–therapist relationship may be better able to parlay the alliance into symptom change. Similarly, therapists who report typically using OE-fostering strategies (e.g., providing strong rationales for techniques) could be more aware of, and better able to channel, patient OE to achieve better outcomes. Finally, it is also possible that therapists are somewhat aware of the extent to which they use the alliance and/or OE to achieve positive outcomes. Thus, therapists' own perceptions of their ability to effectively foster alliance quality and OE could moderate their associations with outcome. That is, therapists who subjectively believe that they are effectively alliance- or OE-centered in their work may have stronger (or perhaps weaker) associations between these process variables and their patients' outcomes.

[1] Although it was also of interest to know whether the between-therapist alliance–outcome and OE–outcome associations varied across therapists, the nature of multilevel modeling precludes a direct test of this at the highest level of analysis (in this case, the between-therapist level). Thus, the only way to examine whether the therapist-level process–outcome association varied systematically based on other therapist-level variables (i.e., moderation) was to test the significance of specific moderators (which, as discussed next in the third aim, we did in this study).

Method

Data Set Overview

Data derived from a double-masked randomized trial that tested the efficacy of a measurement-based patient–therapist match system versus case assignment as usual (CAU; Constantino et al., 2021). The trial took place across six outpatient clinics within a single community mental health system in Cleveland, OH. As case assignment was the only experimental manipulation, subsequent treatment was delivered naturalistically; thus, its length and nature varied by patient. For the purposes of the parent trial, "posttreatment" was considered the point at which treatment terminated or after 16 weeks, whichever came sooner. In brief, results indicated that patients in the match condition experienced significantly greater global symptomatic/functional improvement than patients in the CAU condition ($d = 0.75$). Given this between-group effect, we included assignment condition as a covariate in all of the current analyses.

Participants

Therapists

Forty-eight therapists provided naturalistic treatment and were eligible to treat both match and CAU patients (this crossed design controlled for general between-therapist effects on outcome). Given the present study's focus on within- and between-therapist process–outcome associations, we included the subsample of 42 therapists who treated more than one study patient ($M = 5.05$; range = 2–11). Importantly, the excluded therapists did not differ significantly from those in the present subsample on any demographic or professional characteristics (all ps $> .05$). The subsample therapists were mostly White (81%) and female (71%), and they held a variety of professional degrees: 69% had a master's degree, 29% had a doctoral degree, and 2% had another type of degree. The subsample therapists were, on average, 49.17 years old ($SD = 13.81$ years) and had an average of 16.10 ($SD = 11.74$) years of post-licensure clinical experience. Although they endorsed a variety of *primary* theoretical orientations, 93% identified as at least "somewhat" integrative. Regarding the extent to which therapists' current practice was guided by different theoretical frameworks (on a scale from 0 to 6, with 0 = *not at all* and 6 = *very much*), subsample therapists identified most strongly with a "CB" orientation ($n = 42$, $M = 5.12$, $SD = 1.09$), followed by "interpersonal" ($n = 37$, $M = 3.92$, $SD = 1.48$), "humanistic/experiential" ($n = 39$, $M = 3.31$, $SD = 1.69$), "systems" ($n = 36$, $M = 2.86$, $SD = 1.20$), and "psychoanalytic (PA)/PD" ($n = 37$, $M = 2.24$, $SD = 1.77$).

Patients

Two hundred eighteen adults were randomly assigned to either the scientific match ($n = 99$) or CAU ($n = 119$) condition. Only patients who were *not* the primary decision maker for their health care were excluded, resulting in a heterogeneous sample with varied mental health problems. Given the aforementioned methodological requirement that therapists treat multiple study patients, the present subsample included the 212 patients ($n = 98$ match; $n = 114$ CAU) who were treated by the 42 subsample therapists. Importantly, the excluded patients did not significantly differ from those in the present subsample on any demographic or clinical characteristic (all ps $> .05$). Based on a normed, multidimensional outcomes measure (the Treatment Outcome Package [TOP], which is discussed below), the most common primary presenting problems were quality of life issues (21.2%), depression (19.8%), substance use (17.9%), and panic/anxiety (9.9%). Table 1 shows patient demographic and clinical characteristics by condition.

Treatment

As noted, psychotherapy was administered as usual. The parent trial capped data collection at 16 weeks after the start of treatment, though therapy itself lasted longer for some patients if clinically indicated. For the current subsample patients, active treatment through the 16-week study period lasted an average of 11.43 weeks ($SD = 6.09$), with patients attending an average of 5.67 ($SD = 3.36$) sessions. Finally, 21% of patients ($n = 44$) terminated treatment early (i.e., before Session 3).

Measures

Mental Health

To assess domains of mental health, patients completed the TOP (Kraus et al., 2005), a widely used routine outcome assessment tool. The TOP consists of 58 items rated on a 6-point scale (ranging from 1 to 6) that assess 12 symptom/functioning domains: panic/somatic anxiety, depression, suicidal ideation, violence, work functioning, sexual functioning, social conflict, substance use, sleep, mania, psychosis, and quality of life. Based on a series of confirmatory factor analyses, the TOP has excellent factor structure (Kraus et al., 2005). Moreover, with the exception of mania, the TOP subscales have demonstrated good internal consistency, test–retest reliability, strong convergent validity with other well-established outcome measures, and sensitivity to clinical change (Kraus et al., 2005, 2011).[2] Additionally, the TOP subscales are also summed to create an index of global psychological distress/impairment (theoretical range of 58–348), with higher scores indicating *better* functioning. Therefore, given this reverse scoring, the TOP total score is hereafter referred to as global psychological well-being/functioning. This total score has demonstrated excellent reliability, convergent validity with other measures of global symptom severity, and sensitivity to clinical change (Kraus et al., 2005; Zack et al., 2015). In the present sample, the TOP total score demonstrated good reliability throughout treatment (average Cronbach's $\alpha = 0.85$).

Alliance Quality

To assess alliance quality, patients completed the Working Alliance Inventory-Short Form (WAI-S; Tracey & Kokotovic, 1989). The WAI-S is a widely used measure based on Bordin's (1979) tripartite conceptualization of the alliance as consisting of patient–therapist agreement on treatment goals, agreement on treatment tasks, and emotional bond. The WAI-S consists of 12 items rated on a 7-point scale (ranging from 1 to 7), with higher total scores

[2] Despite the relatively poorer psychometric properties of the mania subscale (αs ranging from .55 to .70; test–retest intraclass correlation coefficient [ICC] = .76), its influence on the psychometrics of the TOP total score is unproblematic.

Table 1
Patient Baseline Demographic and Clinical Characteristics by Condition (N = 212)

Variable	CAU (n = 114)			Match (n = 98)		
	M	*SD*	*n* (%)	*M*	*SD*	*n* (%)
Age	34.44	11.67		33.09	10.50	
Sex						
Female			77 (67.5)			65 (66.3)
Male			37 (32.5)			33 (33.7)
Race/ethnicity						
Caucasian/White			101 (88.6)			86 (87.8)
Hispanic/Latino			3 (2.6)			3 (3.1)
African American/Black			6 (5.3)			7 (7.1)
Asian			2 (1.8)			1 (1.0
Other			2 (1.8)			1 (1.0)
Sexual orientation						
Heterosexual			92 (80.7)			88 (89.8)
Bisexual			10 (8.8)			6 (6.1)
Gay or lesbian			4 (3.5)			3 (3.1)
Not sure			5 (4.4)			0 (0.0)
Missing			3 (2.6)			1 (1.0)
Annual household income						
Less than 20,000			6 (5.3)			7 (7.2)
20,000–40,000			11 (9.7)			10 (10.2)
40,000–75,000			36 (31.6)			28 (28.5)
75,000–100,000			21 (18.4)			24 (24.5)
100,000 or more			37 (32.5)			28 (28.6)
Missing			3 (2.6)			1 (1.0)
Education						
High school or less			14 (12.3)			18 (18.4)
Business or trade school			6 (5.3)			8 (8.2)
2-year college			10 (8.8)			13 (13.3)
4-year college			41 (36.0)			29 (29.6)
Masters or doctorate			33 (29.0)			22 (22.4)
Missing			10 (8.8)			8 (8.2)
Marital status						
Single			53 (46.5)			45 (45.9)
Married/cohabiting			51 (44.7)			43 (43.9)
Divorced/widowed/separated			7 (6.1)			9 (9.2)
Missing			3 (2.6)			1 (1.0)
Previous therapists/courses of therapy[a]	1.72	1.89		1.56	1.51	
On psychiatric medication?						
Yes			34 (29.8)			26 (26.5)
No			55 (48.2)			52 (53.1)
Missing[b]			25 (21.9)			20 (20.4)
TOP-CS total score[c]	258.03	26.57		252.87	29.13	

Note. CAU = case assignment as usual; TOP-CS = Treatment Outcome Package-Clinical Scales.
[a] Note that n = 207 for this variable due to missing data. [b] The total sample size for the psychiatric medication item is 167 because of a technological error during data collection. [c] Note that n = 211 for this variable due to missing data.

reflecting a better quality alliance marked by more goal and task agreement and a closer bond (theoretical range of 12–84).[3] The total score for the 36-item original WAI (Horvath & Greenberg, 1989), from which the short-form derived, has excellent psychometric properties (Elvins & Green, 2008). Similarly, most relevant to this study, the WAI-S total score possesses excellent reliability and is highly correlated with the original measure (Tracey & Kokotovic, 1989). In the present sample, the WAI-S total score demonstrated excellent reliability throughout treatment (average Cronbach's $\alpha = 0.96$).

Outcome Expectation

To assess OE, patients completed the Credibility/Expectancy Questionnaire (CEQ; Devilly & Borkovec, 2000), a widely used measure of two distinct treatment beliefs. The OE subscale of the CEQ consists of three items: (a) "By the end of the therapy period, how much improvement in your presenting concerns/problems do you think will occur?"; (b) "At this point, how much do you really *feel* that therapy will help you to reduce your presenting concerns/problems?"; and (c) "By the end of the therapy period, how much improvement in your presenting concerns/problems do you *feel* will occur?" The first and third OE items are measured on an 11-point

[3] Total scores for the two process variables were created by averaging the items and multiplying by the total number of items. Although item-level missingness was rare, if a patient did not complete at least 80% of the items at a given timepoint, then the relevant process variable was treated as missing for this occasion (and this type of missingness was addressed in our analyses; see the Data Analysis section below).

scale (ranging from 0% to 100% improvement in 10-point intervals), whereas the second is measured on a 9-point scale (ranging from 1 to 9). Thus, prior to creating a total score for the OE subscale, the first and third items were rescaled to the same 9-point metric as the second item, with higher scores reflecting a more optimistic expectation of treatment success.[4] The OE subscale of the CEQ has shown good internal consistency, test–retest reliability, and predictive validity (Devilly & Borkovec, 2000). In the present sample, the OE subscale demonstrated excellent reliability throughout treatment (average Cronbach's $\alpha = 0.93$).

Therapist Characteristics

To assess the relevant therapist moderator variables (i.e., theoretical orientation, use of alliance- or OE-focused interventions, and self-perceived alliance- or OE-fostering effectiveness), therapists provided information about their personal and professional characteristics via the study-specific Provider Characteristics Form (PCF; Constantino et al., 2021), which included items adapted from several established measures. As reported previously, therapists rated the degree to which various theoretical orientations influenced their practice (see Orlinsky & Rønnestad, 2005), and our focus here was specifically on the PA/PD and CB orientations. To assess therapists' use of alliance- or OE-focused interventions, we drew on items from an augmented version of the Comparative Psychotherapy Process Scale (CPPS; Hilsenroth et al., 2005) for which therapists rated, from 0 = *not at all* to 6 = *very much*, "how characteristic" different interventions were of their "typical therapy practice." For alliance-focused interventions, we used the following CPPS item: "Focus discussion on the relationship between the clinician and client." For OE-focused interventions, we used the following CPPS item: "Explain the rationale behind your technique or approach to treatment."

Therapists also completed the study-specific Therapist Perceived Strengths (TPS) measure. Relevant to this study, we drew on items assessing therapists' perceptions of their ability to effectively foster and maintain positive alliances and to cultivate positive OE in their patients. Specifically, on a scale ranging from 1 = *always more ineffective* to 7 = *always more effective*, therapists completed the following alliance/OE item: "Compared to other clinicians, in establishing and maintaining a positive working alliance [instilling positive outcomes expectations] with my clients, I would say that I am . . . ."

Procedure

Therapists within the community mental health system were informed that the study would examine various case-assignment processes but were kept unaware of the specific experimental case-assignment manipulation. Consenting therapists completed a baseline survey packet that included the PCF and TPS. Patients were recruited to the trial after being referred (by self or another provider) for naturalistic mental health care. They were informed that the study was examining various case-assignment processes, and that both they and their therapists would be unaware of the specific processes. Consenting patients completed baseline study measures (including demographic and clinical information) and were then randomized to case-assignment condition (match vs. CAU).

Relevant to the present study, patients completed the TOP at baseline, after every odd numbered week, and at posttreatment (as noted, either their actual final session or Week 16 if they remained in longer term care). Patients also completed the WAI-S and CEQ after every even numbered week. Given previous research suggesting that at least four measurement occasions are required to reliably assess a process variable (Crits-Christoph, Gibbons, et al., 2011), we used the WAI-S and CEQ scores across the first four measurement occasions as our primary predictor variables. To maintain temporal precedence between our predictor and outcome variables, we used each patient's posttreatment TOP as the criterion variable. A university institutional review board approved the parent trial and secondary analysis of de-identified data.

Data Analytic Plan

We first examined descriptive statistics for the predictor, moderator, and outcome variables and transformed any variables that were not acceptably normally distributed (i.e., skewness value of greater than +2 or less than −2). Next, to create the primary predictors, we took the average of the first four OE and alliance measurements (typically at Weeks 2, 4, 6, and 8).[5] For the primary analyses, we used multilevel structural equation modeling (MSEM; Preacher et al., 2016), as facilitated by the Mplus 8.4 program (Muthén & Muthén, 1998–2017). Relevant to this study, MSEM is advantageous because it automatically parses the predictor (alliance, OE) and outcome (global psychological well-being/functioning) variables into their latent within- and between-therapist components. This latent variable approach adjusts for measurement uncertainty, resulting in unbiased within- and between-therapist estimates (Preacher et al., 2016).

Additionally, given that variance components and interactions (key foci of the present study) are typically not normally distributed, we used the Bayesian estimator to generate 95% credible intervals (CIs), which do not assume normality. Using this approach, 95% CIs that do not contain zero are considered to be statistically significant (Muthén & Asparouhov, 2012). Although Bayesian estimation allows for the use of specific priors (i.e., the hypothesized probability distribution of a parameter based on theory/previous data) to inform model estimates, because we were unaware of any previous studies testing therapist-level moderators of within- and between-therapist process–outcome associations, we used noninformative priors (which allowed the model to be estimated based on only the data).

All models were fit within a two-level framework with within-therapist (between-patient) differences at Level 1 (i.e., differences between patients seen by the same therapist) and between-therapist differences at Level 2 (i.e., differences between therapists across all patients in their caseloads). Additionally, missing data were handled

[4] Importantly, despite this transformation, the original and rescaled items remain perfectly correlated.

[5] Although the study measures were distributed to patients at standardized intervals, there was some variability in when patients completed the WAI-S and CEQ. On average, patients completed their first process rating at 2.58 weeks ($SD = 2.42$), suggesting relatively strong compliance with the expected measurement schedule. Additionally, across the four included measurement occasions, the average time in weeks that patients completed the process measures was 5.11 ($SD = 3.02$), which is squarely in the middle of the expected time frame (i.e., Weeks 2, 4, 6, and 8).

using the Bayesian corollary of full information maximum likelihood estimation. This method retains all participants who provide at least one rating of a study variable, which resulted in all 42 therapists and 212 patients being included in all analyses. Finally, effect sizes represent the average of the standardized associations across clusters for each parameter (Asparouhov & Muthén, 2021). These standardized associations can be interpreted similarly to partial correlation coefficients; that is, the number of *SD*s of expected outcome changes for every 1 *SD* change in the relevant predictor, controlling for the effect of all other predictors.

To simultaneously test research Aims 1 and 2, we fit two random slopes models (one for each process variable predictor) testing the within- and between-therapist components of the relevant process variable (alliance or OE) as predictors of within- and between-therapist outcome variance, respectively. To test whether each within-therapist process–outcome association varied across therapists, we allowed the relevant slope to be random and tested its significance. Thus, each process model (one for each process variable) yielded two primary fixed effect associations: (a) the within-therapist process–outcome association (i.e., the extent to which differences in the relevant process among different patients working with the same therapist related to differences in outcome among these different patients working with the same therapist) and (b) the between-therapist process–outcome association (i.e., the extent to which differences in therapists' average process levels across all patients in their caseloads related to differences in therapists' average outcomes across all patients in their caseloads). Additionally, as noted, each process model also yielded a random effect that quantified the extent to which the within-therapist process–outcome association varied across therapists (Aim 2). See the Online Supplement for the full equation.

Finally, for research Aim 3, we tested whether each relevant therapist-level variable (i.e., theoretical orientation, self-reported use of alliance- or OE-focused interventions, and self-reported perceptions of effectively using the alliance and OE during treatment) moderated the within-therapist process–outcome association (a cross-level interaction) and/or the between-therapist process–outcome association (a fully therapist-level interaction). To preserve power and parsimony, we fit with within- and between-therapist moderation models separately. However, across both models, we controlled for the effect of the relevant process variable at the other level of analysis (i.e., within or between).

The within-therapist moderation models were fit according to the random coefficient prediction (RCP) model for testing cross-level interactions (Preacher et al., 2016). Specifically, for each process variable, the relevant therapist-level moderators were included as Level 2 predictors of the relevant within-therapist (Level 1) process–outcome association. To enhance interpretability of the intercept (i.e., the value of the outcome when all predictors equal zero), the moderators were grand-mean centered. See the Online Supplement for the full equation.

The between-therapist moderation models were fit according to the latent moderated structural equations (LMS) model for same-level interactions in MSEM (Asparouhov & Muthén, 2021; Preacher et al., 2016). This model involves the generation of latent interactions among random coefficients (in this case, the latent between-therapist component of the relevant process variable and therapist-level moderator). Simulation research has shown that the LMS method results in less bias than other multilevel moderation approaches (e.g., those using observed rather than latent variables), including for sample sizes similar to the present study (Preacher et al., 2016). However, given that this model is computationally complex (Preacher et al., 2016), to preserve power and model parsimony, we fit separate models for each moderator and treated the sole continuous covariate of patient baseline global well-being/functioning as an observed (rather than latent) variable. Therefore, we disaggregated this covariate into its within- and between-therapist components using group-mean centering. The moderators were grand-mean centered prior to the creation of the latent interactions. See the Online Supplement for the full equation.

Results

Preliminary Analyses

Supplemental Table 1 shows the descriptive statistics and intercorrelations (i.e., total correlations) among all continuous study variables. All study variables were acceptably normally distributed (all skewness values less than +2 and greater than −2) and, therefore, no transformations were needed.

Within- and Between-Therapist Process–Outcome Associations

The full results of the two-level MSEM model testing the within- and between-therapist alliance–outcome associations are reported in Supplemental Table 2 and visually depicted in Panel a of Figure 1. Most relevant to our research questions, although within-therapist fluctuations around a given therapist's mean level of alliance quality were unrelated to within-therapist differences in patients' posttreatment global well-being/functioning, within-therapist association = 0.38; 95% CI [−0.07, 0.75], therapists who fostered higher quality average alliances across all patients in their caseloads tended to also achieve more patient improvement, on average, between-therapist association = 0.62; 95% CI [0.003, 1.10]. Expressed as a standardized association, every 1 *SD* increase in between-therapist alliance quality was associated with a .54 *SD* increase in average patient posttreatment well-being/functioning.

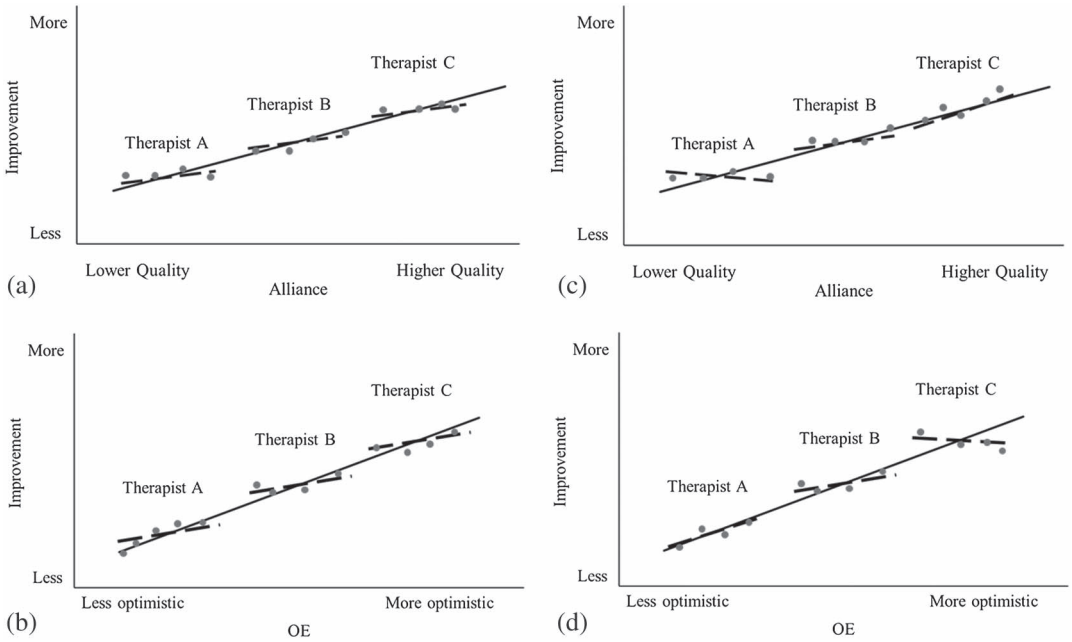
For OE, the full results of the two-level MSEM model testing the within- and between-therapist OE–outcome associations are reported in Supplemental Table 3 and visually depicted in Panel b of Figure 1. Most relevant to our research questions, at the within-therapist level, patients who had more optimistic OE compared to their therapist's mean level of OE also tended to experience more posttreatment improvement than their therapist's average patient, within-therapist association = 0.84; 95% CI [0.15, 1.53]. Expressed as a standardized association, every 1 *SD* increase in within-therapist OE was associated with a .15 *SD* increase in patient posttreatment well-being/functioning. In contrast, between-therapist differences in OE were unrelated to caseload-level differences in patients' global well-being/functioning, between-therapist association = 2.64; 95% CI [−0.67, 6.20].

Therapist-Level Variability in Within-Therapist Process–Outcome Associations

As hypothesized, random effects indicated that the within-therapist alliance–outcome association varied significantly among

Figure 1

Average Within- and Between-Therapist Process-Outcome Associations and Variability in These Associations



Note. Panel a depicts the sample average within-therapist (standardized association = 0.13) and between-therapist (standardized association = 0.54) alliance–improvement association. Panel b depicts the sample average within-therapist (standardized association = 0.15) and between-therapist (standardized association = 0.53) OE–improvement association. Panel c depicts variability in the within-therapist alliance–improvement association. Specifically, Therapist A's data depict a negative within-therapist alliance–improvement association that is 1 *SD* below the mean. Therapist B's data depict the average within-therapist alliance–improvement association. In contrast, Therapist C's data depict a strong, positive within-therapist alliance–improvement association that is 1 *SD* above the mean. Finally, panel d depicts variability in the within-therapist OE–improvement association. Specifically, Therapist A's data depict a strong, positive within-therapist OE–improvement association that is 1 *SD* above the mean. Therapist B's data depict the average within-therapist OE–improvement association. In contrast, Therapist C's data depict a negative within-therapist OE–improvement association that is 1 *SD* below the mean. OE = outcome expectation.

therapists, $\tau_{11} = 0.40$; 95% CI [0.10, 1.33]. Descriptively, therapists who were 1 *SD* above the mean had relatively strong, positive within-therapist alliance–improvement associations, slope = 1.01; 95% CI [0.54, 1.70], that were *more than double* the average association, average within-therapist association = 0.38; 95% CI [−0.07, 0.75], whereas therapists who were 1 *SD* below the mean had nonsignificant negative alliance–improvement associations, slope = −.25; 95% CI [−1.03, 0.16]. To determine whether some therapists in the sample had statistically significant negative alliance–improvement associations, we also calculated a slope for therapists who were 1.5 *SD*s below the mean; this analysis revealed a statistically significant and moderately sized, negative alliance–improvement relation, slope = −0.56; 95% CI [−1.56, −0.001]. See Panel c of Figure 1 for a visual depiction.

Also as expected, random effects indicated that the within-therapist OE–outcome association varied significantly across therapists, $\tau_{11} = 1.25$; 95% CI [0.36, 4.59]. Descriptively, therapists who were 1 *SD* above the mean had strong, positive within-therapist OE–improvement associations, slope = 1.95; 95% CI [1.07, 3.34], that were *more than double* the average positive association, average within-therapist association = 0.84; 95% CI [0.15, 1.53], whereas therapists who were 1 *SD* below the mean had nonsignificant negative OE–improvement associations, slope = −.27; 95% CI [−1.62, 0.46]. To determine whether some therapists in the sample had truly negative OE–improvement associations, we also calculated a simple slope for therapists who were 1.5 *SD*s below the mean; this analysis revealed a moderately sized, but still only marginally significant, negative relation, slope = −0.84, one-tailed $p = .05$; 95% CI [−2.62, 0.11]. See Panel d of Figure 1 for a visual depiction.

Therapist-Level Moderation of the Within- and Between-Therapist Process–Outcome Associations

The full results of the two-level RCP MSEM model testing therapist-level moderators of the within-therapist alliance–outcome association are reported in Model 1 of Table 2. Only therapist self-perceived alliance-fostering ability significantly moderated the within-therapist alliance–outcome association, $\gamma_{31} = -0.76$; 95% CI [−1.18, −0.28]. More specifically, whereas therapists who perceived themselves as having above average (+1.5 *SD*s) abilities to foster high-quality alliances had *negative* (though only marginally significant) alliance–improvement associations, simple slope = −0.57, one-tailed $p = .04$; 95% CI [−1.13, 0.14], therapists who

Table 2
Moderators of the Within-Therapist Alliance– and OE–Outcome Associations (N = 212)

Fixed effects	Alliance model			OE model		
	Coef. (*SD*)	95% CI	ES[a]	Coef. (*SD*)	95% CI	ES[a]
Posttreatment TOP-CS, γ_{00}	269.77* (2.09)	[265.60, 273.93]	—	270.80* (13.57)	[243.41, 301.62]	—
Baseline TOP-CS$_b$, γ_{01}	0.27 (0.89)	[−2.73, 1.15]	0.09	0.57 (8.79)	[−19.03, 16.75]	0.21
Alliance$_b$/OE$_b$, γ_{02}	0.72 (0.39)	[−0.22, 1.29]	0.45	1.40 (3.46)	[−5.80, 6.26]	0.23
CB orientation, γ_{03}	−1.67 (1.45)	[−4.62, 1.11]	−0.21	−1.97 (1.93)	[−5.65, 1.84]	−0.27
PA/PD orientation, γ_{04}	2.30* (1.08)	[0.13, 4.47]	0.49	1.40 (1.10)	[−0.76, 3.67]	0.31
Focus on alliance/rationale, γ_{05}	−0.20 (1.35)	[−2.92, 2.33]	−0.03	1.71 (1.55)	[−1.25, 4.85]	0.32
Self-perceived alliance/OE-fostering ability, γ_{06}	−3.88 (2.30)	[−8.14, 1.03]	−0.36	−1.55 (2.45)	[−6.89, 3.01]	−0.17
Match vs. CAU, γ_{10}	8.55* (2.70)	[3.52, 13.89]	—[b]	6.86* (2.92)	[0.96, 12.32]	—[b]
Baseline TOP-CS$_w$, γ_{20}	0.57* (0.06)	[0.47, 0.68]	0.58	0.57* (0.06)	[0.46, 0.68]	0.58
Alliance$_w$/OE$_w$-TOP-CS (slope), γ_{30}	0.27 (0.17)	[−0.01, 0.63]	0.28	0.67 (0.42)	[−0.17, 1.52]	0.39
CB orientation, γ_{31}	−0.04 (0.16)	[−0.37, 0.28]	−0.05	0.03 (0.38)	[−0.71, 0.77]	0.02
PA/PD orientation, γ_{32}	−0.14 (0.13)	[−0.34, 0.18]	−0.26	−0.44 (0.25)	[−1.01, 0.04]	−0.50
Focus on alliance/rationale, γ_{33}	0.18 (0.13)	[−0.09, 0.45]	0.30	−0.09 (0.32)	[−0.74, 0.49]	−0.09
Self-perceived alliance/OE-fostering ability, γ_{34}	−0.76* (0.24)	[−1.18, −0.28]	−0.62	−0.03 (0.54)	[−1.05, 1.08]	−0.01

Note. The main effects of each moderator on the outcome (γ_{00}) are represented by γ_{03}–γ_{06}. The effect of each moderator on the size/direction of the relevant within-therapist process–outcome association (γ_{30}) are represented by γ_{31}–γ_{34}. Coef. = coefficient; CI = credible interval; ES = effect size; TOP-CS = Treatment Outcome Package-Clinical Scales; $_b$ = between-therapist association; OE = outcome expectation; CB = cognitive–behavioral; PA/PD = psychoanalytic/psychodynamic; CAU = case assignment as usual; $_w$ = within-therapist association.
[a] Effect sizes represent the average of the standardized associations across clusters for each parameter. [b] Given that the assignment condition variable (match = 1, CAU = 0) is dichotomous, it does not make sense to present the association as the expected outcome difference for a 1 *SD* change in the predictor.
* indicates that the 95% CI does not include zero.

perceived themselves to have below average (−1.5 *SD*s) abilities to foster high-quality alliances had strong positive alliance–improvement associations that were approximately *three times* larger than the average within-therapist alliance–outcome association, simple slope = 1.14, one-tailed $p = .001$, 95% CI [0.52, 1.72]; see Online Supplemental Figure 1). Regarding OE, the full results of the two-level RCP MSEM models testing therapist-level moderators of the within-therapist OE–outcome association are reported in Model 2 of Table 2. None of the examined therapist-level variables significantly moderated the within-therapist OE–improvement association.

Regarding the completely between-therapist moderation models, the full results of the alliance and OE two-level LMS MSEM models are reported in Table 3. Across these models, results indicated that only identification with a CB theoretical orientation moderated the between-therapist *alliance*–improvement association, $\gamma_{04} = -0.46$; 95% CI [−0.96, −0.004]. As depicted in Online Supplemental Figure 2, for therapists with a more CB orientation (1 *SD* above the mean), the between-therapist alliance–outcome association was nonsignificant, simple slope = −0.27; 95% CI [−0.82, 0.34]. In contrast, for therapists who reported a lower-than-average CB orientation (1 *SD* below the mean), there was a strong positive between-therapist alliance–outcome association, simple slope = 0.75; 95% CI [0.20, 1.37].[6]

Discussion

The present study had three aims: (a) test the alliance– and OE–outcome associations at both the within- and between-therapist levels; (b) examine whether the within-therapist alliance– and OE–outcome associations varied among therapists; and (c) explore therapist-level moderators of the within- and between-therapist alliance– and OE–outcome associations. Regarding Aim 1, as hypothesized, both the alliance– and OE–outcome associations were significant at either the within- or between-therapist level. More specifically, although higher quality between-therapist alliances associated with greater average improvement, within-therapist (between-patient) differences in alliance quality were unrelated to within-therapist differences in improvement. In contrast, more optimistic OE was associated with greater improvement at the within- but not between-therapist level. Regarding Aim 2, as expected, the within-therapist alliance–outcome association varied significantly among therapists, such that some therapists had strong, positive alliance–improvement associations, others had negligible alliance–improvement associations, and still others had *negative* alliance–improvement associations. Similarly, the within-therapist OE–improvement association demonstrated variability among therapists. Finally, regarding Aim 3, therapists' self-perceived alliance-fostering ability and degree of CB orientation significantly moderated the within- and between-therapist alliance–outcome associations, respectively. In contrast, no significant moderators of either the within- or between-therapist OE–outcome associations emerged.

The finding that the between- but not within-therapist alliance–outcome association was significant may help to clarify the presently mixed literature on this topic. Regarding the between-therapist alliance–outcome association, out of now nine studies to date, seven

[6] Because treatment length varied across patients, we replicated all models with the total number of weeks each patient was in the study and the total number of sessions they attended (within the study period) as additional covariates. Across the three study aims, all research question relevant, statistically significant results remained significant and similarly sized. Full results from these supplementary analyses are available from the first author upon request.

Table 3
Moderators of the Between-Therapist Alliance– and OE–Outcome Associations (N = 212)

Alliance models								
Fixed effects	CB orientation		PA/PD orientation		Focus on alliance		Self-perceived alliance-fostering ability	
	Coef. (*SD*)	95% CI	Coef. (*SD*)	95% CI	Coef. (*SD*)	95% CI	Coef. (*SD*)	95% CI
Posttreatment TOP-CS, γ_{00}	173.73* (36.49)	[106.59, 246.05]	177.30* (43.12)	[111.46, 278.06]	171.90* (37.81)	[95.67, 245.51]	185.25* (36.18)	[121.84, 252.32]
Baseline TOP-CS$_b$, γ_{01}	0.30* (0.14)	[0.06, 0.58]	0.33 (0.17)	[−0.05, 0.57]	0.33* (0.16)	[0.03, 0.64]	0.29* (0.14)	[0.03, 0.51]
Alliance$_b$, γ_{02}	0.26 (0.16)	[−0.06, 0.55]	0.16 (0.20)	[−0.20, 0.46]	0.24 (0.22)	[−0.22, 0.70]	0.19 (0.16)	[−0.12, 0.47]
Moderator, γ_{03}	25.37* (11.90)	[1.48, 49.29]	−3.02 (4.87)	[−11.73, 6.45]	−1.54 (8.49)	[−18.90, 18.75]	−23.32 (15.48)	[−50.97, 14.44]
Interaction, γ_{04}	−0.46* (0.23)	[−0.96, −0.004]	0.06 (0.09)	[−0.11, 0.25]	0.01 (0.15)	[−0.33, 0.32]	0.31 (.27)	[−0.34, 0.82]
Match vs. control, γ_{10}	2.34 (2.82)	[−3.14, 8.97]	2.92 (2.83)	[−3.96, 8.04]	2.35 (2.64)	[−2.21, 8.15]	2.23 (2.82)	[−3.24, 8.86]
Baseline TOP-CS$_w$, γ_{20}	0.61* (0.07)	[0.49, 0.75]	0.61* (0.08)	[0.44, 0.73]	0.61* (0.07)	[0.48, 0.76]	0.61* (0.07)	[0.49, 0.75]
Alliance$_w$, γ_{30}	0.24* (0.08)	[0.08, 0.39]	0.28* (0.09)	[0.11, 0.46]	0.26* (0.08)	[0.10, 0.42]	0.24* (0.08)	[0.08, 0.39]
OE Models								
Fixed effects	CB orientation		PA/PD orientation		Focus on treatment rationale		Self-perceived OE-fostering ability	
	Coef. (*SD*)	95% CI	Coef. (*SD*)	95% CI	Coef. (*SD*)	95% CI	Coef. (*SD*)	95% CI
Posttreatment TOP-CS, γ_{00}	175.11* (39.31)	[99.69, 255.04]	174.66* (38.06)	[102.51, 244.18]	180.87* (38.85)	[114.86, 262.99]	191.41* (42.86)	[96.87, 267.96]
Baseline TOP-CS$_b$, γ_{01}	0.33 (0.16)	[−0.03, 0.63]	0.33* (0.16)	[0.03, 0.66]	0.34* (0.15)	[0.004, 0.58]	0.30 (0.17)	[−0.06, 0.66]
OE$_b$, γ_{02}	0.70 (0.67)	[−0.89, 1.79]	0.70 (0.79)	[−1.02, 2.40]	0.44 (0.85)	[−1.52, 2.02]	0.34 (0.77)	[−1.74, 1.74]
Moderator, γ_{03}	−6.65 (9.56)	[−28.99, 11.08]	8.13 (7.78)	[−7.79, 19.42]	1.48 (8.76)	[−14.03, 17.20]	2.52 (17.40)	[−28.47, 43.09]
Interaction, γ_{04}	0.59 (0.63)	[−0.78, 1.91]	−0.54 (0.49)	[−1.25, 2.40]	−0.09 (0.58)	[−1.03, 1.00]	−0.27 (1.08)	[−2.64, 1.88]
Match vs. control, γ_{10}	3.05 (2.64)	[−3.00, 6.89]	2.52 (2.70)	[−2.36, 8.23]	2.38 (2.37)	[−2.58, 6.63]	2.08 (2.58)	[−3.05, 6.99]
Baseline TOP-CS$_w$, γ_{20}	0.61* (0.07)	[0.49, 0.77]	0.61* (0.07)	[0.49, 0.77]	0.62* (0.08)	[0.46, 0.80]	0.62* (0.08)	[0.46, 0.75]
OE$_w$, γ_{30}	0.45 (0.27)	[−0.10, 0.96]	0.45 (0.26)	[−0.08, 0.99]	0.53 (0.28)	[−0.15, 0.97]	0.43 (0.24)	[−0.09, 0.84]

Note. The main effect of each moderator is represented by γ_{03}, and the between-therapist process by moderator interaction is represented by γ_{04}. CB = cognitive–behavioral; PA/PD = psychoanalytic/psychodynamic; Coef. = coefficient; CI = credible interval; TOP-CS = Treatment Outcome Package-Clinical Scales; OE = outcome expectation; $_b$ = between-therapist association; $_w$ = within-therapist association.

(including the present one) have found a significant between-therapist alliance–improvement association (i.e., Baldwin et al., 2007; Crits-Cristoph et al., 2009, 2018; Huppert et al., 2014; Marcus et al., 2011; Zuroff et al., 2010), with only two finding a null association at this level (i.e., Crits-Christoph, Hamilton, et al., 2011; Falkenström et al., 2014). Thus, the present results support a growing consensus that therapists who consistently foster more positive alliances across all patients in their caseload also tend to be more globally effective. In comparison, the within-therapist alliance–outcome association can now be regarded as more mixed, with five studies finding a significant alliance–improvement association (i.e., Crits-Christoph, Hamilton, et al., 2011; Falkenström et al., 2014; Huppert et al., 2014; Marcus et al., 2011; Zuroff et al., 2010) and four studies (including the present one) finding a null association at this level (i.e., Baldwin et al., 2007; Crits-Cristoph et al., 2009, 2018).

Clinically, this study's Aim 1 alliance results may suggest that what is most *consistently* therapeutic about the alliance is the portion that can be attributed to the therapist rather than the portion that can be attributed to the patient or the specific patient–therapist pairing. Although speculative, this finding squares with interpersonal theory positing that a quality alliance represents a novel and corrective in-session relational experience that can generalize to patients' extra-therapy relational functioning that can, in turn, facilitate broader symptom reduction (e.g., Coyne et al., 2019; Zilcha-Mano, 2017). From this perspective, it would make sense that the *novel* aspects of the therapeutic relationship would be more attributable to what the therapist and overall treatment context bring to the table than to what the patient contributes, which could include a person's preexisting, relationship-fostering abilities and/or interpersonal problems (Zilcha-Mano, 2017). Regardless of the exact reason for this finding, the present results suggest that therapists may wish to monitor their *general*, *caseload-level* alliances and engage in alliance-focused trainings if their average alliance levels tend to be lower than their peers.

Regarding the OE–outcome association, as this study was the first to parse this relation into its within- and between-therapist components, there are no prior studies with which to compare the present findings. Interestingly, though, in contrast to this study's alliance results, more optimistic OE was associated with greater improvement only at the *within-therapist* level. Although speculative, it seems plausible that in comparison to the inherently interpersonal alliance construct, the *patient*-"owned" nature of the OE variable could render individual, patient-level differences in this construct most potent for *patient-level* differences in outcome (Constantino et al., 2018). In other words, it may make sense that this inherently patient-focused construct operates primarily at the patient-level of analysis. Thus, therapists might consider monitoring their individual patients' *relative* OE. When a particular patient reports lower OE than usual, compared to the therapist's other patients, then that therapist could consider implementing OE-fostering strategies (e.g., providing personalized hope-inspiring statements, tailoring treatment strategies to match a person's beliefs; Constantino et al., 2018). Of course, the presently novel multilevel OE–outcome findings, and their preliminary clinical implications, require additional testing.

As expected, for Aim 2, both the within-therapist alliance– and OE–outcome associations varied significantly among therapists, providing proof of concept for the notion that different therapists may use different psychotherapy processes to differing therapeutic effects. Regarding OE, these results add nuance to the *average* within-therapist OE–improvement association revealed with Aim 1 of this study; that is, there appears to be a subgroup of therapists who do (those with strong, positive OE–improvement associations) and a subgroup who do not (those with negative or negligible OE–improvement associations) need to closely attend to differences in OE among the patients within their practices in order to maximize patient improvement. Additionally, given that OE can be assessed *prior* to the start of therapy (Constantino et al., 2018), the present results could support a new form of patient–therapist matching; that is, because therapists with *negative* OE–improvement associations are the ones who achieve better outcomes when patients report more pessimistic OE, it could be beneficial to assign the subgroup of patients with low *presenting* OE to these therapists. Similarly, it could also be beneficial to assign patients with more positive baseline OE to therapists who seem better able to capitalize on such optimism (i.e., therapists with strong positive OE–improvement associations). Therefore, although replication and further examination of specific moderators is needed (see our Aim 3 discussion below), these results preliminarily point to the importance of personalizing OE practice recommendations and case assignments *to the provider*. Regarding the alliance, this finding replicates the results of the one previous study of which we are aware on this topic (Dinger et al., 2008) and extends this finding to outpatient settings. Together, these two studies now preliminarily suggest that one reason for the variability in the size and significance of the within-therapist alliance–outcome association across studies could be that therapists differ in the extent to which they effectively use the alliance to achieve positive outcomes.

At least preliminarily, the present results further extend the literature by pointing to at least one therapist-level characteristic that can explain such *within-therapist* alliance–outcome variability (i.e., significant therapist-level moderation). Specifically, whereas therapists who perceived themselves as having above-average abilities to foster high-quality alliances had *negative* alliance–improvement associations, therapists who perceived themselves to have average or below-average abilities to foster high-quality alliances had strong, positive alliance–improvement associations that were approximately *three times* larger than the average within-therapist alliance–outcome association. Therefore, for therapists who hold humbler views of their relationship-fostering abilities, the alliance may represent a key ingredient for fostering change in their patients, whereas those who view themselves as more universally or consistently effective in building alliances may rely on *other* nonalliance processes to affect change in patient outcomes. Although the exact reason for such moderation remains unknown, we offer two possible explanations.

One possibility is that the therapist self-perception variable is largely a byproduct of how they view and use treatment processes. Those therapists using the alliance to the strongest effect may also be the ones who see the variable as being at the core of their work. And with such a relational focus, they may appreciate that alliance quality is neither always easily achieved nor constant, which is reflected in what they may see as an accurate account of their abilities; that is, regardless of their average relational skill, their alliances with individual patients will vary and can sometimes be difficult or suboptimal. In contrast, those therapists whose alliances do not associate strongly with outcome may also be the ones who

place less emphasis on the alliance as a central change agent. And with this secondary alliance focus, they may be less likely to perceive or concern themselves with fluctuations in relational quality, which is reflected in what they may see as an accurate account of their strong ability to cultivate a nonprimary change process.

Alternatively, this finding may square with previous research on the beneficial effects of therapist professional self-doubt or the degree to which therapists question their efficacy/competence in treating patients (Nissen-Lie et al., 2013). For example, several studies have found that greater levels of *global* therapist professional self-doubt associates with better between-therapist outcomes (Nissen-Lie et al., 2013, 2017), with the authors speculating that therapists who hold humbler views of their own competence may be more vigilant to cues that therapy may be offtrack, which could allow for more therapeutic responsivity. Applied to the *current* findings, it seems plausible that therapists who are humbler (and perhaps more accurate) about their own relationship-fostering abilities may be more attuned to variability in their individual alliances with patients, which could, at least in theory, facilitate effective responsiveness that renders the alliance a more personally potent change ingredient. In comparison, therapists who practice with the global assumption that all of their alliances are relatively strong could, at least in theory, promote potentially harmful relational misattunements in which their interventions (interpersonal or otherwise) are poorly aligned with the actual quality of a particular dyad's therapeutic relationship. For example, such confidence could result in therapists missing cues that a particular relationship is especially positive, which could represent a notable missed opportunity that ultimately results in poorer outcomes (and may explain the negative alliance–outcome correlation for these therapists). Alternatively, to the extent that very highly rated alliances can sometimes represent a patient's unrealistic idealization of the therapist, perhaps therapists who see themselves as particularly relationally effective may be at risk for missing this interpersonal nuance, which could ultimately lead to a recapitulation of a patient's maladaptive interpersonal patterns (in this case, to idealize others) and poorer ultimate outcomes (Benjamin, 2018).

Regardless of the exact reason for this finding, it could have preliminary implications for personalizing one's practice to the factors that are most important for a given therapists' outcomes (i.e., a *therapist-level* form of "what works for whom"). Specifically, it may be helpful for therapists to "know thyself" by reflecting on their own comparative alliance-fostering abilities. If therapists believe themselves to be roughly average compared to their peers (i.e., "sometimes more effective" or "inconsistently more effective"), it may behoove them to closely monitor their individual alliances with patients in the service of heightening effective responsivity to this personally important variable. That is, when patients rate the alliance as high, it could be a cue that things are going well and to continue with their treatment plan. In contrast, when the alliance with a given patient is relatively low, it could be important to recognize this *personal* risk factor for poorer outcomes and to respond accordingly (e.g., by incorporating explicit alliance-focused strategies; Flückiger et al., 2018).

For therapists who see themselves as above average relative to their peers, it could be less clinically important for them to attend to within-caseload fluctuations in their alliances, as such differences do not appear to be a key *personal* change agent. Naturally, deriving more specific implications for these therapists will depend on researchers and clinicians working together to identify *other* (beyond the alliance) personalized change processes, which could be theory-specific (e.g., effective use of cognitive interventions or interpretations) or theory-common (e.g., effective use of empathy, emotional expression). Alternatively, if such therapists were nevertheless motivated to view and use the alliance as a change process, then they may benefit from changing their self-perceptions to reflect a humbler stance about their alliance-fostering abilities. However, future research will need to investigate *whether* and *how* such changes can happen, and if they can indeed change the strength of a therapist's alliance–outcome relation.

Finally, therapist identification with a CB orientation moderated the between-therapist alliance–improvement association. Specifically, for *strongly* CB therapists, their caseload-level average alliance quality was generally unrelated to their average outcomes. In comparison, for therapists who did not identify with a CB orientation (or who identified with it less strongly than their peers), their average alliance quality was a relatively strong positive predictor of their average outcomes. This result squares with the theoretical role of the alliance in CB traditions; that is, the alliance is historically viewed as a facilitative platform that allows other theory-specific techniques (i.e., the more cognitive and behaviorally related putative change mechanisms) to have a greater effect on improvement (Hatcher & Barends, 2006; Zilcha-Mano, 2017). Therefore, it seems plausible that therapists who are *strongly* aligned with this theoretical perspective may place relatively less emphasis on trying to parlay their alliances *directly* into patient improvement, which could account for the observed null (and even slightly negative) between-therapist alliance–improvement association for these providers. It is worth noting that, somewhat counter to our expectations, therapist identification with a PA/PD orientation did not have the opposite impact on the between-therapist alliance–outcome association. Although speculative, this result could stem from the fact that a strong identification with a PA/PD orientation was quite rare in this sample, which limited our power to detect this association. Alternatively, this result could suggest that most of the other orientations (beyond CB) with which therapists in this sample identified (e.g., interpersonal, humanistic) could place relatively equal emphasis on the alliance as a change mechanism as compared with PA/PD. However, these speculations require additional testing, especially in samples in which more therapists identify strongly with a PA/PD orientation.

With additional regard to the CB moderator finding, though, it is worth reiterating that this result does not imply that therapists of certain theoretical orientations tend to be globally more versus less effective. Instead, this result simply suggests that, for highly CB therapists, other factors (beyond the alliance) explain between-therapist differences in outcome. Therefore, it may be helpful for therapists to "know thyself" in terms of their degree of CB orientation and to personalize their alliance practices accordingly. For example, it seems possible that highly CB therapists may be most effective when they attend to the change processes that they personally believe to be most facilitative of patient improvement (e.g., cognitive and behavioral interventions). Interestingly, this idea is consistent with the anecdotal observations of Crits-Cristoph et al. (2010) who noted that after an alliance-focused training, some therapists appeared to become *less effective*, suggesting that a focus on this construct may have detracted from their

ability to implement the strategies that *personally* help them to work more effectively with their patients. When these anecdotal results are interpreted in the context of the present findings, it seems likely that therapists' degree of identification with a CB orientation could represent one therapist-level characteristic that influences the efficacy of alliance-focused *trainings*. Whereas therapists with a low or moderate degree of CB identification may be wise to seek out alliance-focused trainings if or when their caseload-level alliances are relatively low in quality, therapists with a strong CB orientation may be better served by seeking *other* types of trainings to improve their outcomes. Of course, these speculations and preliminary training implications require direct testing in future studies.

Overall, it is worth noting that several of the investigated therapist-level variables did not moderate the alliance–outcome association at either level, and none explained variability in the OE–outcome associations. Although this proof-of-concept study was largely exploratory, these null results could have implications for future research. First, for *both* processes, the consistent lack of moderation for therapists' use of alliance- and OE-focused interventions could owe to the present study's reliance on self-report methods; that is, it is possible that therapists' self-perceptions of their use of these strategies could be somewhat unrelated to their *actual* use of such techniques. Therefore, it may be more fruitful for future studies to use observer-coding methods that can capture therapists' in-session provision, for example, of a compelling treatment rationale (our putative OE-focused practice). Second, with specific regard to OE, the present results may suggest that future therapist-level moderator research should look beyond the variables investigated in the present study. For example, given that explaining the rationale behind one's techniques can be considered *both* an alliance- and OE-fostering strategy (e.g., Constantino et al., 2018; Muran & Eubanks, 2020), perhaps more uniquely OE-focused techniques (e.g., providing personalized, hope-inspiring statements; Constantino et al., 2018) could allow therapists to parlay this belief into symptom change. Another possibility could be that effectively harnessing patient OE is less about *what* techniques therapists use and more about *how* they deliver or present them. In this vein, perhaps future research could investigate factors such as therapist facilitative interpersonal skill (e.g., persuasiveness, empathy, verbal fluency; Anderson et al., 2009), which might enable therapists to use OE-fostering strategies to the greatest therapeutic benefit.

The present study had several limitations. First, as noted, we did not have access to session recordings. Therefore, the assessments of all therapist-level moderators were based on self-report, which could be subject to bias (e.g., overly positive views of one's effectiveness; Walfish et al., 2012). Future research could use observer-coding methods to better capture the strategies that therapists use in their sessions. Second, the therapist-level moderators were each assessed with a single item, which could limit their reliability and validity. Third, despite this study having a relatively large sample size compared to many psychotherapy process–outcome studies, it only met the minimum sample size recommended for testing multilevel moderation (i.e., ˜50 upper-level units; Preacher et al., 2016). Therefore, we cannot rule out the possibility that some of the null results observed for the third aim could be a function of low power at the therapist level. Fourth, owing to the naturalistic context, there was patient-level variability in the length of treatment, and relatedly, the number of measurement occasions available for the process variables. Additionally, there was also variability in when patients rated the alliance and OE, which could have impacted the results. Fifth, although the present study included data from multiple clinics (within a single mental health network), the Constantino et al. (2021) trial did not collect data on which clinic each patient attended. Therefore, we were unable to test for possible clinic effects on the study outcome. However, because therapists were not nested within clinics in this sample (i.e., they could treat patients at multiple clinics), it seems unlikely that clinic effects could have solely or primarily accounted for therapist-level variability in processes and outcome. Finally, the sample was mostly White, with relatively high income; thus, replication is needed in more diverse samples.

Limitations notwithstanding, this study was one of the first to examine the potential utility of personalizing psychotherapy process and training to the *therapist*. Using two common processes, the results provided proof of concept for the idea that different therapists rely on different psychotherapy processes to affect clinical change. Preliminarily, clinicians may wish to attend to their own self-perceptions and theoretical orientations when attempting to parlay the alliance into therapeutic change. Additionally, if replicated, clinicians could use such information to select personally well-suited clinical trainings, whether to embolden strengths or redress weaknesses. Finally, the results also suggest that the time may be right for researchers to begin attending to *therapist-level* "what works for whom" questions when examining both theory-specific and common process–outcome associations, as such work has the potential to inform the development of more nuanced personalized case assignments, clinical practices, and trainings that take into account both participants in the psychotherapy endeavor.

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