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PSYCHOLOGY

EVALUATING THERAPEUTIC ALLIANCE LONGITUDINALLY: DESCRIBING THERAPEUTIC ALLIANCE GROWTH AND ITS IMPLICATIONS FOR OUTCOMES

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Dissertation under the direction of Professor Leonard Bickman

Therapeutic alliance, the helping relationship that develops between client and therapist, is the most commonly studied process variable in mental health treatment. In child/adolescent treatment, the relationship that develops between therapist and the youth's caregiver has also been argued as important. This is due to the routine involvement of caregivers in treatment tasks (e.g., learning new parenting skills), as well as their involvement in dictating the youth's treatment goals and treatment agenda. Alliance is theorized not to remain static; it deteriorates or grows stronger over the course of treatment. Yet despite alliance's theorized volatility and instrumental role in predicting eventual treatment response, very little research has investigated whether longitudinal alliance change within client relates to outcome. Using data from the Multimodal Treatment Study of Children with AD/HD, caregivers participating in the parent-training arm of the study were utilized to investigate how alliance growth affects outcome using statistical methods that can model appropriately within-client change

(hierarchical linear modeling, survival analysis). Consistent with hypotheses, caregiver-reported alliance was found to be significantly volatile within-clients and has different trajectories in early versus late treatment. These findings call into question the utility of the alliance literature's traditional means of summarizing therapeutic alliance (e.g., average overall alliance, one-session snapshots of alliance over phases of treatment). Contrary to the other hypotheses, treatment engagement variables did not consistently predict eventual treatment response, patterns of missing alliance data, or early termination from the study. The only consistent predictor of outcome (i.e., treatment response and early termination), was treatment group assignment, which is consistent with the main findings of the study. Explanations for the results of this study are generated, and the conclusion of the study outlines the new directions that can move the therapeutic alliance literature forward using advances in longitudinal statistics.

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INTRODUCTION

Therapeutic alliance, the helping relationship that develops between client and therapist, is the most commonly studied process variable in mental health treatment (Horvath & Bedi, 2002). Although it was discussed as an important component of the therapeutic relationship by prominent psychodynamic authors (e.g., Freud 1913/1966; Greenson, 1965; Zetzel, 1956), it remained a theoretical curiosity until the mid 1970s. It was during this period that therapeutic alliance was first investigated as a predictor of outcome (e.g., Luborsky, 1976; Strupp & Hadley, 1979). After 30 years of research, it is now considered by many as the best therapy process predictor of therapeutic outcome in adult populations (e.g., Horvath & Luborsky, 1993; Orlinsky, Grawe, & Parks, 1994). Besides "traditional" psychotherapy settings (i.e., outpatient services with a therapist), alliance has been predictive of outcomes in approaches as diverse as couples therapy, behavioral medicine, and pharmacotherapy (Bourgeois, Sabourin, & Wright, 1990; Gavin et al, 1999; Krupnick, Sotsky, Simmens, & Moyer, 1996).

Since the operational definitions of alliance were developed in the 1970s, over 2000 research publications have been published regarding therapeutic alliance (Horvath & Bedi, 2002). A larger review of process variables found therapeutic bond to be the best predictor of outcome (Orlinksy, Grawe, & Parks, 1994). However, this review only used vote-counting methods of significant findings, which is highly dependent on factors such as measurement reliability and sample size. Furthermore, vote counting methods do not quantitatively address the strength of the relationship between alliance and outcome.

Parametric measures of effect size (e.g., *r* or Cohen's *d*) can both describe the size of the relationship between two variables as well as if the relationship is significant. Horvath and Symonds (1991) was the first meta-analysis to evaluate the relationship between outcome and therapeutic alliance. Based on 24 studies, the average effect size was $r = 0.26$. Small, medium, and large effects for *r* are .10, .30, and .50, respectively (Cohen, 1992). Two other major meta-analyses on adult client therapeutic alliance have been recently conducted (Horvath & Bedi, 2002; Martin, Garske, & Davis, 2001). Both of these reviews had larger study samples (i.e., 89 and 79 studies, respectively) use more sophisticated meta-analytic procedures (e.g., homogeneity of variance tests) and had similar effects ($r = .21$ & $r = .22$, respectively). Tests for homogeneity of variance indicated that there were no significant moderator variables. This finding is interesting, because it appears that the average alliance relationship to outcome in adult studies does not depend on type of rater, time of alliance assessment, type of treatment provided, or the publication status of the study.

Empirical Evidence of Therapeutic Alliance in Child Studies

Despite decades of investigation in adult populations, therapeutic alliance has only gained increased attention in child[1] clinical outcome studies in the last 10 years (Eltz, Shirk, & Sarlin, 1995; Florsheim, Shotorbani, Guest-Warnick et al, 2000; Green, Kroll, Imrie et al, 2001; Hogue, Dauber, Stambaugh, Cecero, & Liddle, 2006). One estimate indicates that less than 3% of child therapy studies have examined treatment processes like therapeutic alliance (Kazdin, Bass, Ayers, & Rodgers, 1990). This has led some to

[1] References to child studies includes studies where the primary clients are either children or adolescents.

describe treatment process research (i.e., research into within-treatment variables/interactions) as the "ignored stepchild of child psychotherapy investigators" (Russell & Shirk, 1998). Although alliance in child community practice has not been evaluated with the rigor that it has had in adult psychotherapy, emerging evidence in inpatient adolescent services, youth day treatment, and the child welfare system indicate a similar relationship between alliance and outcome as in adult research clinics (Dore & Alexander, 1996; Colson, Cornsweet, Murphy et al, 1991; Eltz, Shirk, & Sarlin, 1995; Florsheim, Shotorbani, Guest-Warnick et al, 2000; Green, Kroll, Imrie et al, 2001). An argument can be made that it may be more important in child mental health settings. Since most children enter therapy involuntarily and could be experiencing social deficits, the ability for the clinician to form and maintain a therapeutic relationship in child therapy is paramount (Shirk & Saiz, 1992). Failure to establish a therapeutic alliance early in treatment could lead to dropout. Data from child and family therapy studies suggest that as much as 30%-60% of participants terminate prematurely (Armbruster & Kazdin, 1994). In cases where children/adolescents involved in residential treatment cannot decline treatment, low therapeutic alliance can presumably attenuate treatment effectiveness (Shirk, 2001). Recently, a meta-analysis was conducted investigating the relationship between therapeutic alliance and outcome in child studies (Shirk & Karver, 2003). In a sample of 23 studies, the relationship was quite similar to adult studies of alliance (r=.21). In contrast to the adult studies, several moderators were found. Alliance was more related to outcome in externalizing children versus internalizing children (M= .30 vs. M= .10). On methodological factors, alliance was more related to outcome on therapist- and observer-reported alliance than in the child (M = .29-.26 vs. M= .18).

Also, late session reports versus early session reports were more related to outcome (*M* = .27 versus *M*= .12). Although alliance in child studies may have a similar overall relationship with outcome, it appears that some moderators of alliance that affect its relationship to outcome.

Caregiver Therapeutic Alliance

There is growing evidence that process research in youth treatment must also recognize the alliance between caregiver and therapist. A survey of nearly 1200 child psychologists and psychiatrists cite parental cooperation as the only factor more important than the therapist-child relationship (Kazdin, Siegel, & Bass, 1990). Furthermore, theoretical papers on this topic have suggested that a therapeutic alliance with the parents or primary caregiver may in some cases be more important for treatment adherence and outcomes than the child's relationship to the therapist (DiGiuseppe, Linscott, & Jilton, 1996). Since the primary caregiver is often primarily in charge of providing/arranging transportation, payment, and establishing treatment goals, the caregiver could be considered more the "client" than the child (e.g., Weisz & Jensen, 1999). Attrition in child psychotherapy has been found to be more related to characteristics of the parent than the child (Gould, Schaffer, & Kaplan, 1985). Recent empirical research has indicated that factors related to parental alliance affect engagement, attendance, and outcome (Kabuth, DeTychey, & Vidailhet, 2005; Morrissey-Kane & Prinz, 1999). In the largest study to date of child/adolescent treatment dropouts in community outpatient settings, the most cited reason for dropping out of treatment by parents was therapeutic relationship problems (Garcia & Weisz, 2002). This study illustrated that therapeutic alliance was more important for treatment engagement

and preventing premature dropout than if the child had improved or whether the child still needed treatment.

Definition and Empirical Structure of Therapeutic Alliance

The numerous therapeutic alliance measurement systems are currently based on only a few theoretical models. All of these alliance models share two major factors in common: agreement on the agenda of therapy and the presence of a client emotional bond. Luborsky's (1976) theory of alliance was the first to discriminate how alliance develops over time. The first phase of alliance, *Type I Alliance*, involves the client's belief that the therapist is helpful and provides a warm, supporting relationship. *Type II Alliance* develops later, and it consists of the client's commitment to the therapeutic process. Bordin (1979) described therapeutic alliance as a three factor model: (1) the emotional bond between the client and therapist, (2) the agreement of the two parties on the therapeutic tasks, and (3) the agreement on the goals/expectations of therapy. Gaston (1990) attempted to reconcile these previous theoretical models and current empirical therapist/client characteristic literature by proposing a four factor model: (1) the patient's capacity to purposefully work in therapy, (2) the client's emotional bond to the therapist, (3) the therapist empathetic understanding and involvement, and (4) the patient-therapist agreement of tasks and goals.

These modern conceptualizations of alliance have generated several measurement scales. Luborsky (1976) developed a series of alliance measurement systems known collectively as the Penn Scales (HAcs; Luborsky, 1976; HAr; Luborsky, Crits-Christoph, Alexander, Margolis, & Cohen, 1983; HAq; Luborsky, McLellan, Woody, O'Brien, & Auerbach, 1985). These were the first scales to be commonly used to measure

therapeutic alliance. These instruments operationalized his Type I and Type II alliance constructs into therapist-, client-, and observer-rated alliance measures. The Working Alliance Inventory, with factors tapping into bond, agreement on tasks, and agreement on goals is now the most widely used alliance measure today (WAI; Horvath & Greenberg, 1989). The California Psychotherapy Alliance Scales is also widely used, has versions for all three types of raters, and is based on Gaston's (1990) four-factor theory (CALPAS; Marmar, Gaston, Gallagher, & Thompson, 1989). Other measurement scales used to a lesser extent are the Vanderbilt Psychotherapy Process Scale (VPPS; Suh, Strupp, & O'Malley, 1986), Therapeutic Alliance Rating Scale (TARS; Marziali, 1984), and Therapeutic Bond Scales (Saunders, Howard, & Orlinsky, 1989).

Psychometric evaluation of the therapeutic alliance construct has not consistently supported multiple factors in the most widely used alliance measures. High correlations between dimensions in these studies would indicate that therapeutic alliance would be best considered a unidimensional construct (e.g., Gaston, 1991; Morgan, 1982; Salvio, Beutler, Wood, & Engle, 1992). However, other authors have argued elsewhere that a priori classical test theory methods of maximizing reliability and inter-item correlations in small validation samples could prevent viewing multidimensional patterns found in larger, more representative samples (Henry, Strupp, Schacht, & Gaston, 1994; Horvath & Greenberg, 1989). Confirmatory factor analyses studying this issue have yielded both unidimensional and multidimensional factor solutions. Tracey & Kokotovic (1989) compared the fit of the traditional Bordin (1979) model (i.e., 3 factors), one general factor, and a hierarchical 2-level factor on two samples that totaled 207 subjects. The hierarchical two-level model consisted of the three first-order factors as well as a

second-order general alliance factor, which loaded on each first-order factor. None of the models fit the data well, but the hierarchical two-factor model had the best fit and was reported as the most adequate. However, the authors failed to point out that more complicated models would always approximate the sample's variance/covariance matrix better. With a larger sample (i.e., 308 subjects), Gaston, Sabourin, Hatcher, & Hansell (1992) found additional evidence of a two-level model. Four mildly correlated alliance instruments (i.e., resembling Gaston's therapeutic alliance model) were imbedded in a larger alliance factor. Sapyta (2001) investigated the factor structure of the therapeutic alliance instrument used in this study. Although the hierarchical two-factor model was the best fitting model, the item loadings suggest each item loads highest on the general factor. In everyday practice, it was suggested that clinicians use the overall mean, and not each individual factor.

Theories Describing How and Why Alliance Affects Outcomes

Many theorists have considered how the therapeutic relationship contributes to treatment engagement and therapeutic change. Interestingly, very few of these individuals could be considered therapeutic alliance theorists per se. Instead they have used the concept of alliance as part of larger perspective on psychotherapy. Some approaches have included concepts that are basic to other fields such as social psychology. *Social influence theory* describes counseling and the therapeutic relationship as a process of interpersonal influence (Strong, 1968). Through charismatic qualities of the therapist (client-perceived expertness, trustworthiness, and attractiveness), the therapist reframes the client's issues and attempts to change their behavior patterns or attitudes about the world. This sometimes causes psychological discomfort or *cognitive*

dissonance, which then motivates the client to reduce the dissonance in several ways. Ideally under this model, the counselor can successfully persuade the client to accept the therapist's model of change and dismiss their own objections. This may take a few sessions of conflict, but eventually the client will come to agree and participate in the therapist's model for change. However, the client can also alleviate the dissonance of changing thoughts/behavior by essentially disengaging from the therapeutic process (e.g., reject the therapist's claim entirely, discount the importance of alleviating this particular problem, or seek other sources or experts with a philosophy closer to their own).

The *phase model for psychotherapy* is another theory that describes how the therapeutic relationship may affect therapeutic outcome (Howard, Lueger, Maling, & Martinovich, 1993). Based on previous models of healing, they describe how therapists must provide a plausible rationale for the client's distress (Frank & Frank, 1993; Luborsky, Singer, & Luborsky, 1975). The phases of therapy include *remoralization*, *remediation*, and *rehabilitation*. This is a stage model, which specifies that a client cannot go into remediation until the client meets criteria of remoralization. It is in the remoralization phase, where it is argued; developing the therapeutic relationship is paramount. Based heavily on previous work by Frank & Frank (1993), the phase model of psychotherapy argues that clients seek therapy when they are no longer able to cope with their current strategies and begin to feel powerless and hopeless (i.e., demoralized). Clients then seek the services of a therapist who will build a therapeutic relationship with them. In this relationship, the therapist will convey an aura of an expert, clarify the client's presenting problem, and instill hope in the client for treatment success. Once the client perceives the therapist as trustworthy/competent and has developed an emotional

bond with the therapist, the client will be willing to form goals based on the now reframed problems as well as participate in therapeutic tasks. Thus, the initial therapeutic relationship fosters an allegiance to the therapist's treatment model, which then leads to the activities of reducing the client's symptoms, issues, etc. Only after the establishment of a strong alliance, can the client work on relieving current symptoms (i.e., *remediation*) and perhaps changing maladaptive habits or personality characteristics after symptoms are reduced (i.e., *rehabilitation*).

Besides the formal theories of psychotherapy process, there have been other attempts to explain how alliance relates to outcome based on empirical processes. The role of client expectancies relationship with alliance has recently been gaining greater attention (Connolly-Gibbons et al, 2003; Joyce & Piper, 1998; Morrisey-Kane & Prinz, 1999). Client expectancies have been defined as the anticipatory beliefs that clients have regarding the procedures, outcomes, and any other aspect about the treatment rationale or its delivery (Nock & Kazdin, 2001). Pretreatment expectancies have been found to be significantly associated with alliance (e.g., Joyce & Piper, 1998). Although pretreatment expectancies are also related to outcome, it appears that this relationship is partially mediated by therapeutic alliance (Meyer et al, 2002). These findings suggest that client expectancies may play a significant role in how the alliance is developed. If a client has optimistic expectancies about treatment, the process of "persuading" the client of the treatment rationale may be easier than in clients with lower expectations. Along with expectancies, some alliance theorists have described how within-client variation may predict treatment success. A reasonably high but gradually increasing alliance through treatment has been indicated as predictive of better outcomes (Florsheim et al, 2000;

Joyce & Piper, 1998). Others argue that the typical pattern of successful therapy is initially high, followed by lower alliance when the honeymoon period ends. But in successful therapy cases, the alliance will be restored to its initial level. Thus, successful treatment cases will resemble a "U-shaped" high-low-high pattern (Gelso & Carter, 1994).

Common Features of Theories Explaining the Alliance-Outcome Relationship

The theories that describe how the therapeutic relationship affects outcome have several components that compliment each other. Most notably, they all describe a temporal process regarding how alliance develops and fluctuates, with particular alliance development courses leading to improving or declining negative outcomes. This is also a main tenet in one of the prominent alliance theories (Luborsky, 1976). The expectancies literature also fits nicely in this approach, because expectancies of the client may affect how well the alliance can initially be developed. A client already feeling optimistic about therapy success could have fewer reservations of establishing a good alliance early. Once therapy begins, the ability of a therapist to be persuasive (through being perceived as expert, trustworthy, and attractive) and the client's expectations of therapy will determine if the therapist can successfully redefine the client's problem, establish a treatment rationale, and instill hope that causes an emotional reaction in the client (i.e., bond to therapist). Once this bond is formulated, it motivates the client to engage in the collaborative goals and tasks of therapy. If this emotional bond is not adequately in place; the client will reject the therapist's treatment rationale and will not improve due to the therapy.

These conceptualizations also leave room for the client to question the therapist and therapeutic approach in the early stages of therapy. In this early phase, there may be

a degree of volatility to the therapeutic relationship as the therapist both attempts to persuade the client in reframing their issues and simultaneously introducing the treatment rationale. The few alliance articles that assess within-client variation longitudinally indicate that there is significant volatility of alliance, at least in the early phase of treatment (e.g., Kivlighan & Shaunessy, 2000). Other researchers have described how alliance ruptures may happen occasionally at any point in treatment (Safran & Muran, 1996). Therefore, based on the theories described, clinicians and researchers should concentrate to both the level of alliance and the degree of volatility throughout treatment.

The Great Disconnect: The Study of Alliance Longitudinally

Despite the large attention given to alliance's role in outcome and the theories describing alliance volatility, there is very little research investigating whether longitudinal alliance change within-client relates to outcome. According to alliance theorists, therapeutic alliance develops gradually and does not remain static throughout treatment. Yet despite the assumed volatility of therapeutic alliance, only a few researchers in the extensive alliance literature have attempted to explain alliance volatility's impact on outcome. Bordin (1979) argued that the strength of the alliance depends on both the personal characteristics of the client and therapist and how these interact with the specific tasks they engage in throughout treatment. Others have argued that the therapeutic relationship must be first grounded in trust of the therapist, which leads to commitment in the tasks and goals of therapy (Frank & Frank, 1993; Luborsky, 1976). Others hypothesize how different patterns of alliance development may impact eventual outcome (Gelso & Carter, 1994; Mann, 1973). For instance, Mann's (1973) description of clients truly engaged in time-limited therapy will begin treatment with a

level of optimism, followed by frustration as the therapist begins challenging their life patterns, and finally rebound to the level of initial engagement when they incorporate the therapist's messages. Finally, the research program at Beth Israel Medical Center currently has the most descriptive model of therapeutic alliance volatility: therapeutic alliance ruptures (e.g., Safran & Muran, 2000; Safran, Muran, & Samstag, 1994). They have developed taxonomy of different alliance ruptures and a stage-process model in identifying, attending, and repairing alliance ruptures. Ruptures in alliance are argued as not only common but expected. It is how the therapist handles a therapeutic alliance rupture, not the rupture itself, which eventually determines how responsive a client is to treatment.

Despite the basic theoretical tenet that alliance development is a volatile process; the empirical approaches to investigating alliance have mostly neglected alliance as a longitudinal process. Many alliance studies measure the alliance at only one point of time (Horvath & Marx, 1990). Alliance in this way is typically operationalized as a one session snap shot (i.e., typically around the third session) or the average alliance throughout treatment (Horvath & Symonds, 1991). Others have examined the alliance over time by breaking the treatment into phases (e.g., thirds; early, middle, or late treatment). They will then select randomly one session from each phase or average the alliance within a phase as one score (e.g., Hartley & Strupp, 1983). Although these approaches yield group averages that are useful in identifying alliance-outcome relationships, they inform nothing about how within-person changes of alliance over time affect future treatment engagement or eventual clinical outcome (Henry, Strupp, Schacht,

& Gaston, 1994; Kivlighan & Shaughnessy, 2000). To best understand how therapeutic alliance operates, you must not only know where the clients are but how they got there.

Future Directions in Therapeutic Alliance Research

Although therapeutic alliance has been consistently linked to outcome, most of the studies have only used autoregressive methods of linking a specific session, phase, or average alliance to outcome. However, recent advances in longitudinal analyses can be utilized in therapeutic alliance research. Hierarchical linear modeling (HLM) has the ability of simultaneously modeling a person's initial level on a variable of interest as well as the patterns of change that occur within subject. This approach has many benefits over previous methods of studying change such as repeated measures ANOVA (Nich & Carroll, 1997). Some reports have studied how alliance change over time (i.e., growth) is related to subsequent outcome (Kivlighan & Shaughnessy, 1995, 2000). The findings of these two studies were mixed, but a pattern of increasing alliance throughout treatment was consistently linked with positive outcome. Replicating these initial longitudinal findings in other populations and settings are crucial for generalizing the effect of alliance longitudinal growth on therapy outcomes. Another longitudinal method being used more commonly for treatment engagement research is survival analysis (Corning & Malofeeva, 2004; Woodside, Carter, & Blackmore, 2004). For studying time until an event (e.g., treatment dropout), survival analysis is superior to traditional methods of studying longitudinal data such as OLS regression (Singer & Willett, 2003). In fact, some have argued that most of psychotherapy termination research to date has been flawed due to the use of common use of statistical methods that do not correctly account for termination as a longitudinal process (Corning & Malofeeva, 2004). For example, as

opposed to ANOVA or OLS regression, only survival analysis can accurately account for censored data (i.e, an event that has not yet occurred due to an arbitrary cause such as the data collection period ending).

Another proposed method of improving the research on the effect of treatment engagement on outcome is to expand the scope of treatment engagement variables. Although therapeutic alliance has been the most often-studied process variable, the construct is mostly observed as client-reported data, which can contribute some traditional biases associated with self-report questionnaires (e.g., social desirability, recency effect). Furthermore, studying alliance exclusively may be problematic due to growing evidence that many alliance instruments have ceiling effects (i.e., most patients have high alliances with therapist), which can lead to biased analyses due to truncated samples. Therefore, treatment engagement should move toward involving several discrete variables that may all reflect treatment engagement in different ways. Including variables such as pretreatment expectancies to treatment, homework completion, and treatment attendance collectively may give a more robust picture of treatment engagement's impact on outcome.

Another interesting application for studying change in therapeutic alliance is developing benchmarks for how alliance typically develops. Since the growth of alliance through the course of treatment has not been adequately addressed, we simply do not know how alliance is established or fluctuates normatively. Finding the typical growth of alliance can be useful in flagging clinicians when fluctuations of alliance are out of the ordinary and require additional attention. This approach has been shown promising effects in systematically providing feedback to clinicians in symptoms/functioning

assessments. Known as *patient-focused research*, this approach has shown to improve outcome and reduce premature termination in clients who are not doing well in therapy (Lambert et al, 2001). A meta-analysis of providing feedback in this way has shown to significantly affect clinician behavior, treatment engagement, and to a lesser extent clinical outcome (Sapyta, 2004). In light of these promising findings, finding the typical course of alliance in treatment is an important question by itself. The following study will investigate how alliance growth typically occurs in treatment as well as its relationship to outcome.

Study Purpose

Using data from the 14-month long Multimodal Treatment Study of Children with AD/HD (MTA), this study will address several questions regarding studying longitudinally therapeutic alliance over the course of treatment: (1) does therapeutic alliance fluctuate significantly over the course of treatment and to what extent does the volatility vary depending on the phase of treatment (i.e., early or late)? According to the theories describing the therapeutic relationship, large volatility should be expected early in treatment as the client is being socialized into the treatment rationale described by the therapist. It is hypothesized that the volatility in the sample should follow these theories with more volatility early in treatment. The amount of volatility should then decline as therapy progresses. (2) Does therapeutic alliance growth predict outcome over and above average alliance and does this relationship vary based on the phase of treatment (i.e., early or late)? Although alliance will be related to outcome throughout treatment, early positive alliance is hypothesized as the most crucial for successful treatment response. Clients with high or growing alliance toward the end of the early phase of treatment are

predicted to have the best outcomes. Previous process-outcome studies on therapeutic alliance typically assessed alliance at one time point (e.g., average alliance in treatment, one session in a large section of treatment). We will compare this approach with incorporating each client's alliance slope as well. (3) Do different patterns of missing alliance data predict treatment or alliance outcomes? It is hypothesized that participation in therapeutic alliance procedures is a proxy to treatment engagement in general and thus should have a similar relationship to outcome. Lack of participation in measurement procedures, especially after initial cooperation, could be an indicator of low treatment engagement. (4) Are predictors such as therapeutic alliance, missing data patterns, and treatment group related to other treatment engagement variables such as premature dropout? It is hypothesized that early therapeutic alliance growth will be a better predictor of treatment dropout than other alliance summary variables such as average TA. In particular, early engagement markers such as declining early alliance or sudden lack of participation in measurement procedures could be indicators that a client is at risk of terminating prematurely. Not benefiting from the treatment assigned may also make one more at risk for termination, which may make those assigned to the less effective treatment (i.e., parent-training only) more at risk to dropout of treatment.

CHAPTER II

METHOD

Population Sample

Participants for this study were from the Multimodal Treatment Study of Children with AD/HD (MTA). In the MTA, 579 children with AD/HD and at least one caregiver for each child participated in a randomized controlled trial comparing four treatment strategies. The children, aged 7.0-9.9 years of age, were assigned to 14 months of medication management (titration followed by monthly visits with a pharmacotherapist); intensive behavioral treatment which includes 35 parent-training sessions; the two combined; or routine community care. The sample was 80% male and 61% Caucasian. Outcome measurement[2] was collected during treatment at baseline, 3 months, 9 months, and 14 months. Follow-up outcome measurement was also collected 10 months after the termination of treatment (i.e., 24 months after randomization). For further details on the study methodology, see Arnold, 1997a, 1997b and MTA Cooperative Group, 1999.

Sampling and Data Collection

All families that were randomized to an experimental treatment (i.e., not community care) were eligible to complete the adapted Working Alliance Inventory-Short for caregivers (CWAI-S). After their scheduled treatment appointment, family

[2] In practice, outcome measurement time intervals are approximate, not exact. However, this is not problematic in HLM, due to its ability to model precisely the time between baseline and the respective data collection point.

members had the opportunity to complete a CWAI-S, place it in a sealed envelope, and give it to a research assistant on-site. Of the 433 families eligible to complete a CWAI-S, 334 families completed at least one. Analyses indicate that families who participated had significantly lower teacher-reported baseline symptoms as measured by the SNAP Inattention mean item score (t (431)= -2.14, $p < .05$), SNAP ODD mean item score (t(431) = -2.04, $p < .05$), and SNAP Total mean score (t(431)= -3.24, $p < .001$). In contrast, there were no significant differences on caregiver-reported baseline symptoms. Teachers also reported significantly greater symptom reduction in participators as measured by mean item SNAP Inattention (t(431)= -2.45, $p < .05$) and SNAP Total mean score (t(431)= -3.03, $p < .003$). There were no caregiver-reported symptom reduction differences between participators and non-participators.

Similar to the published papers on the MTA, only measures reported by biological mothers will be utilized for these analyses. This was primarily done in the MTA because outcome measurement was mostly completed by biological mothers (MTA Cooperative Group, 1999). In the parent-training and medication-management sessions, sometimes additional family members would complete alliance measures with the primary caregiver (e.g., Grandmother). However a vast majority (68%) of the 1841 valid alliance measures were completed by biological mothers. There were 439 biological father-reported measures (24%) and the rest were completed by grandparents, stepparents, or other family members.

Only alliance measures from parent-training sessions were utilized for this longitudinal study. The first reason for this was lack of more medication management participation. While families in the parent-training and combined treatment groups had a

participation rate averaging 80 percent (81% and 79%, respectively), only 40% of families assigned to medication management-only treatment participated including two of the six treatment sites with no medication management participation at all. More importantly the design of the study specified that medication management sessions had significantly fewer alliance measurement points, which is problematic for analyses studying longitudinal change. Medication management session had only three "preferred" visits as opposed to six sessions preferred for parent-training sessions. In practice only 50% of families with medication management sessions had three or more time points. In contrast, 81% of parent-training sessions that had three or more time points. Finally, a preliminary growth model found no differences on slope or intercept between parent-training and medication management sessions ($t(286) = 1.18$, $p = .24$; $t(286) = -1.10$, $p = .28$). Therefore, due to the above reasons and to maximize power to detect differences using families in the combined treatment group, only parent-training sessions will be used. The final sample size utilized for this study with mother-reported, parent-training sessions with linking outcome measurement was 229.

Measures

Caregiver Working Alliance Inventory- Short Version (C-WAI)

The short version of the Working Alliance Inventory (WAI-Short; Tracey & Kokotovic, 1989) was adapted for use in the MTA study for caregiver-therapist therapeutic alliance. It consists of 12 items on a 7-point likert scale with three subscales mapping directly on to the Bordin (1979) model of therapeutic alliance (i.e., agreement on tasks, agreement on goals, and bond). The original WAI has consistently been

reported as highly reliable (i.e., α = .84-.92) and possessing adequate convergent (e.g., other alliance measures) and discriminant validity (e.g. client perceptions of therapist expertness, attractiveness, etc.) (Horvath, 1994; Tichenor & Hill, 1989**).** As mentioned previously, confirmatory analyses of this measure indicate one general factor is the most parsimonious representation of the factor structure. For this reason, mean item score will be used in all analyses**.**

Swanson, Nolan, and Pelham AD/HD Checklist from the DSM-IV (SNAP-IV)

The SNAP-IV was the primary AD/HD symptom scale used in MTA studies (The MTA Cooperative Group, 1999). The SNAP-IV has shown adequate reliability and validity in psychometric studies (Swanson, 1992). The 39-item SNAP-IV in this sample has good reliability (α = .94). Mother- and teacher-reported SNAP-IV will be utilized for the study. The AD/HD composite score will be utilized as the outcome variable for all analyses, which is consistent with the analytical procedures used for the main MTA studies.

Data Analysis

Hierarchical Linear Modeling

The present study uses hierarchical linear modeling, otherwise known as random-coefficients regression or individual growth modeling (Francis et al, 1991; Raudenbush & Bryk, 2002). This family of statistical procedures analyzes nested data (e.g., students within schools) or longitudinal data by generating growth curves (i.e., individualized change over time). The estimation procedures used in HLM are superior to random effects ANOVA and other longitudinal data approaches. Each level-2 unit (e.g., multiple

time points nested within subjects) has a unique intercept and slope, and HLM models can also include data that has significant missing data. This is in contrast to repeated measures ANCOVA, which can only fix a single growth parameter for all cases and typically requires no missing data.

Allowing all available data to be included is a major advantage of HLM because maximizing the amount of subjects allowed leads to better estimates and increased statistical power. Even subjects with only one time point can be included in longitudinal models. Although these individuals would provide no information about within-person variation, HLM still incorporates these data into the estimates of fixed effects when appropriate (Singer & Willett, 2003). Another advantage over ANOVA procedures is that HLM handles irregularly spaced time points well. Since time is modeled precisely in HLM, the data schedule does not have to be equally spaced. Finally, HLM has the ability to accurately estimate variance and covariance components, even with unbalanced, nested data (Raudenbush & Bryk, 2002). This gives the analyst the ability to decomposition the level-2 variation into within- and between-unit components, which cannot be done accurately with repeated measures ANOVA in unbalanced designs.

The typical protocol for conducting HLM model comparisons will begin by generating an unconditional model, which is a model that lacks any level-2 predictors. Then, a model including level-2 predictors will be generated and then compared to the unconditional model. Both of these models variance components will be estimated using full maximum likelihood. Only full maximum likelihood deviance statistics can compare nested models that differ on both fixed and random components, as is planned in the current study.

In order to test our hypotheses regarding differential therapeutic alliance growth in different phases of treatment, a piecewise linear model will be used to generate separate slopes for the early and late phase of treatment. Piecewise models have been used in other applications such in mental health services, where specifying particular phases of treatment better reflect individual trajectories or ease interpretation (Lambert, Wahler, Andrade, & Bickman, 2001). These types of models have been used with success to model phenomena with distinct early and late longitudinal trajectories, such as patient response to HIV/AIDS medication (Littell, Milliken, Stroup, & Wolfinger, 1996). This study will test whether a piecewise model specifying "early" alliance development and "late" alliance development is more representative of the overall trajectory. If so, the specified trajectories of alliance in "early" and "late" treatment will be compared to traditional summaries of therapeutic alliance (e.g., average TA) on their ability to predict treatment outcome in the MTA.

Pattern-Mixture Model Analysis

As mentioned previously, HLM can provide valid parameter estimates in the presence of missing data. The caveat to this assertion is that the missing data must be considered ignorable nonresponse (Laird, 1988). Ignorable nonresponse has been described as meeting Rubin's (1976) *missing at random* (MAR) criteria, which is achieved when the factors contributing to the missing data can be attributed to both observed covariates and previous observed values of the missing data in question (Hedeker & Gibbons, 1997). However, in cases where the MAR criteria may not be met due to missing data patterns, a class of models called pattern-mixture models are often used to address missing data in longitudinal designs (Hedeker & Gibbons, 1997; Little,

1995). Subjects are grouped based on their missing data pattern and then the relative impact of these categorical classes of missing data can be tested. When significant differences between different missing data patterns occur in outcomes (e.g., treatment completers show better treatment benefit than those that attrite), the pattern-mixture approach can model corrected estimates based on the missing data pattern of the sample.

In order to determine if patterns of missing alliance data impacts alliance growth or eventual outcome, the pattern-mixture model approach will be utilized. Although previous studies indicate that missing *outcome* data patterns have no effect on the major MTA outcome findings (e.g., The MTA Cooperative Group, 1999), similar analyses have not been conducted with patterns of missing alliance data. It's possible that certain missing data patterns (e.g., few valid alliance measurements collected) may be indicative of disengagement from the study procedures or recent alliance ruptures with the therapist. We will evaluate how the most common data patterns (e.g., missing vs. complete data) impact alliance, premature termination, and outcome.

The procedures used to conduct a pattern-mixture analysis are similar to those that test models in standard hierarchical linear modeling. The unconditional model will be compared to pattern-mixture models that correspond to subject-level missing data patterns. Dummy codes reflecting these patterns will be modeled as level-2 predictors and compared with the unconditional model. The variance components will also be estimated using full maximum likelihood to compare nested models.

Assessing Treatment Dropout Using Survival Analysis

Treatment dropout is also an important variable when assessing treatment engagement. Survival analysis offers advantages over other forms of regression (e.g.,

OLS regression, ANOVA) because of several properties specific to premature termination research (Corning & Malofeeva, 2004; Singer & Willett, 2003). The ability for survival analysis to accurately account for censored data, time-varying covariates, and multiple end states simultaneously (e.g., premature termination, mutual termination, or censored termination) make this approach superior to traditional methods. Survival analysis also provides more information than traditional methods because it can model both *whether* a premature termination occurred and *when*, which provide more powerful tests for covariates.

The impact of treatment engagement covariates (e.g., therapeutic alliance growth) on premature dropout was analyzed using Cox regression survival analysis. Similar to HLM conditional model comparisons, an estimate of a covariate's goodness of fit is evaluated by the amount of additional variance it can account for over and above the unconditional model. The goodness of fit test is distributed as a chi-square with degrees of freedom equal to the number of additional covariates being modeled. After initially modeling premature termination with no predictors, several models with therapeutic alliance, patterns of missing data, and treatment group status will be entered as covariates will be generated.

CHAPTER III

RESULTS

Preliminary Analyses

A total of 229 families that were involved in parent-training sessions were utilized based on the criteria described above. Table 1 highlights the descriptive statistics, reliability and counts for number of waves caregiver alliance data by treatment group. The CWAI-S in this sample had acceptable reliability ($\alpha = .84$) and acceptable numbers of observations per case for longitudinal analyses (approximately 2/3 of sample has 4 or more repeated measures per client).

Table 1. Descriptive data for the caregiver working alliance inventory item mean value and reflected log transformation.

						Highest # of Observations			
Measure	n	Mean	SD	Range	Cronbach's α	1	2	3	4+
CWAI-S (item mean)	229	6.11	.52	4.14-7.00	.84	25	23	34	147
Parent Training	118	6.14	.51	4.42-7.00	.84	16	9	22	71
Combined	111	6.09	.52	4.14-7.00	.84	9	14	12	76
CWAI-S (transformed)	*	0.25	.12	0.57-0.00	*	*	*	*	*
Parent Training	*	0.25	.12	0.55-0.00	*	*	*	*	*
Combined	*	0.26	.11	0.57-0.00	*	*	*	*	*

Note: * indicates the same value in the transformed portion of the table as in the non-transformed CWAI-S section

The CWAI-S was evaluated for normality. The distribution was evaluated by using normal quantile plots to determine normality and identify outliers. Outliers are problematic because they often have disproportionate influence on mean and variance estimates (e.g., Cohen, Cohen, West, & Aiken, 2003). Examination of the distribution indicated a large negative skew with several outliers. Tabachnik and Fidell (1989) provide formulas to transform skewness values to *z* scores. Alliance ratings had significant skewness and kurtosis scores (i.e., all univariate z-transformed scores higher than 3.0 and 10.0 respectively), which indicated a large negative skew to both measures. In these cases, a reflected log transformation is strongly recommended (Tabachnik & Fidel, 1989). The alliance ratings were transformed, which significantly improved its skewness and kurtosis. As can be seen in Table 1, the reflected transformation reversed the interpretation of values (e.g., 0.0 = very high alliance after transformation). Despite the transformation, there were still 6 cases that were considered outliers. These cases were kept in the study, but they were windsorized. Windsorizing recodes an outlier to a less extreme value, which is typically a z-score of 2.0 (Lipsey & Wilson, 2001). This was done for all outliers.

In order to assess early vs. late therapeutic alliance's impact, the parent training sessions had to be categorized into "early" and "late" groups. Data collection in the MTA data set had intended for therapeutic alliance on the "preferred visits" of parent training Session 4, 6, 12, 18, 24, and 27. Figure 1 displays the historgram for therapeutic alliance data collected by session number. As can be seen, data was collected most frequently on the preferred visits (e.g., 4, 6, 12, etc.) but at least some data was collected on each session. To maximize power for all comparisons preferred visits 4, 6, and 12

were considered “early” sessions and visits 18, 24, and 27 were considered “late”. In practice, subjects may have missing data on several of these preferred visit dates. In order to systematically code for early and late sessions, session number was used to determine the early and late session categories. Session 14 was selected because it approximated the midpoint of both the session number and cumulative frequency of the data collected. Using this designation, 51% of the sample would be considered an “early” treatment session and 49% would be considered a “late” treatment session.

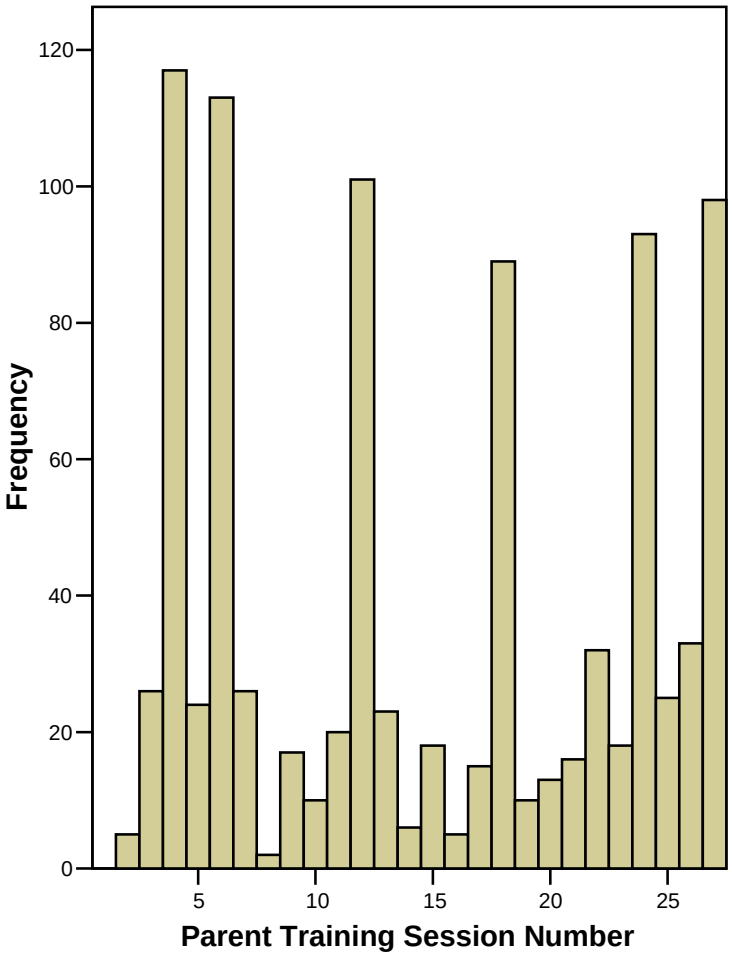

Figure 1. Frequency of completed alliance data by session number.

The mother- and teacher-reported SNAP-IV data available from the primary caregiver alliance sample were also utilized. Descriptive statistics for each reporter are included in Table 2. All reliabilities are adequately high. Exploratory analyses of each measure also indicate no problems with distribution or outliers.

Table 2. Descriptive data for the SNAP-IV total scale and subscales

Measure	n	Mean	SD	Range	Cronbach's α	Highest # of Observations 1	2	3	4+
Caregiver SNAP-IV (item mean)	229	1.31	.58	0.00-3.00	.94	2	8	16	203
Parent Training	118	1.42	.57	0.00-3.00	.93	0	7	10	101
Combined	111	1.19	.58	0.00-3.00	.95	2	1	6	102
Teacher SNAP-IV (item mean)	216	1.27	.85	0.00-3.00	.93	3	5	31	177
Parent Training	112	1.41	.56	0.00-3.00	.93	0	4	21	87
Combined	104	1.11	.50	0.00-3.00	.92	3	1	10	90

Caregiver Alliance Growth

Inspection of the alliance slope trajectories prior to modeling indicated that a linear growth function would represent growth best across the entire duration of treatment. A two-level, unconditional hierarchical model of caregiver therapeutic alliance growth was generated. The reason for modeling an unconditional growth model first is to determine on average if there is a significant slope, determine if growth and initial status should be considered fixed or random effects, and also to serve as a baseline for testing nested models. Prior to modeling, the time variable (i.e., session number) was centered at

Session 2 because that was the earliest session an alliance rating was observed. Table 3 describes the findings for the unconditional growth model.

The summary statistics for the unconditional model suggest that both the initial status and growth rate parameters are significant. It should be noted that due to the reflected transformation described previously, a negative growth rate actually indicates increasing alliance. The mean growth rate is significant and changing at -.003 logits (+.013 untransformed CWAI units) per session. In untransformed units, this would indicate a change of 0.325 untransformed units across the span of the study. Both initial status and growth also indicate significant variance, so both parameters can continue to be modeled as random parameters (i.e., initial status and slope that can be modeled uniquely to each subject). One note of concern is that the overall reliability of the growth rate is at 0.21. This indicates that only 21% of the growth of therapeutic alliance can be predicted by level-2 predictors.

Table 3. Linear growth model of alliance throughout treatment (unconditional model)

Fixed Effect	Coefficient	SE	*t* Ratio (*df*=228)	*p* Value
Mean initial status, β_{00}	0.289	.01	30.74	<0.001
Mean growth rate, β_{10}	-0.003	.00	-6.50	<0.001
Random Effect	**Variance**	***df***	**χ^2**	***p* Value**
Initial status, r_{0i}	0.01126	202	503.94	<0.001
Growth rate, r_{1i}	0.00001	202	247.82	<.015
Level-1 error, e_{ti}	0.00882			
Random level-1 coefficient				
Initial status, π_{0i}	.54			
Growth rate, π_{1i}	.21			
Deviance Statistic (6)	-1379.14			

To test our hypothesis regarding the improved benefit of modeling an alliance piecewise model (i.e., early and late slope) versus one alliance slope for the entire study, the piecewise model was compared with the unconditional model. Table 4 summarizes the unconditional model of the 138 subjects with sufficient data to model both an early and late slope, which is also displayed in Figure 2. Inspection of Table 4 indicates that coefficients and variance components have similar values as in the larger sample**.** The piecewise model was then generated and compared with the unconditional model of the smaller sample with sufficient data.

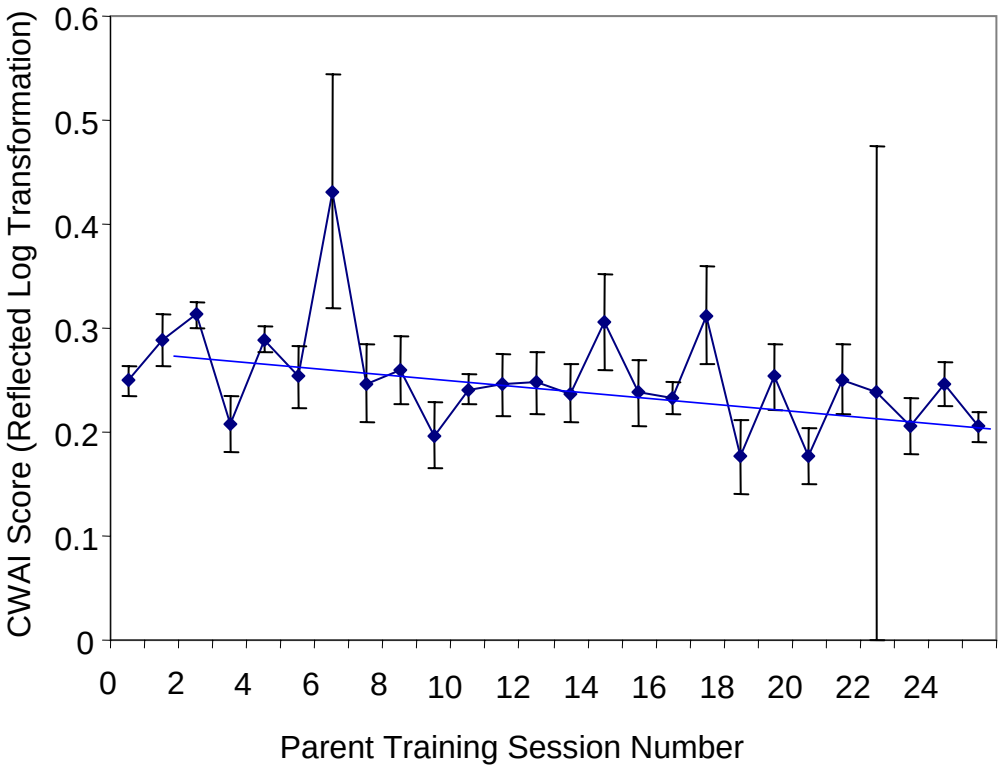

Figure 2. Linear Growth Model of Therapeutic Alliance by Training Session

Table 4. Linear growth model of alliance over the course of treatment (unconditional model) for subjects with sufficient data to construct a piecewise growth model

Fixed Effect	Coefficient	SE	*t* Ratio (*df*=137)	*p* Value
Mean initial status, β_{00}	0.292	0.011	25.59	<0.001
Mean growth rate, β_{10}	-0.003	0.001	-5.82	<0.001
Random Effect	**Variance**	***df***	χ^2	***p* Value**
Initial status, r_{0i}	0.012	137	441.19	<0.001
Growth rate, r_{1i}	0.000	137	210.70	<0.001
Level-1 error, e_{ti}	0.009			
Random level-1 coefficient				
Initial status, π_{0i}	.68			
Growth rate, π_{1i}	.34			
Deviance Statistic (6)	-1024.82			

Table 5 displays the results of the alliance piecewise model. Inspection of the piecewise model parameters indicates that the early slope was found to be significant, which is in contrast to the nonsignificant overall slope of the late sessions. The variance components of both early and late slope were significant, indicating each have significant random effects. As predicted, the early slope is both more steep and variable than the slope later in treatment. Descriptively, this indicates that alliance fluctuations are larger early in treatment, but then become more gradual (i.e., no significant change on average) and less variable later in treatment. When compared with the unconditional model, the piecewise model predicts a statistically significant amount of additional variance (i.e., 13.4% of Level-1 variance) to justify the more complicated, piecewise model ($\chi^2(4) = 16.76, p < .01$).

Table 5. Linear piecewise model of growth of alliance over the course of treatment (early slope and late slope)

Fixed Effect	Coefficient	SE	*t* Ratio (*df*=137)	*p* Value
Mean initial status, β_{00}	0.310	0.01	23.75	<0.001
Mean early growth rate, β_{10}	-0.007	0.001	-4.34	<0.001
Mean late growth rate, β_{10}	-0.002	0.001	-1.85	0.067

Random Effect	Variance	*df*	χ^2	*p* Value
Initial status, r_{0i}	0.01375	137	328.66	<0.001
Early growth rate, r_{1i}	0.00010	137	206.96	<0.001
Late growth rate, r_{2i}	0.00003	137	219.04	<0.001
Level-1 error, e_{ti}	0.00741			

Random level-1 coefficient	
Initial status, π_{0i}	.55
Early growth rate, π_{1i}	.30
Late growth rate, π_{2i}	.34

Deviance Statistic (10) -1041.580083

Caregiver Alliance Growth Relationship to Outcome

Using the sample with sufficient data to model both an early and late therapeutic alliance slope, the relationship between outcome and fluctuations in therapeutic alliance was evaluated. Using the residual file from the generated piecewise therapeutic alliance model, the respective early and late slope was calculated by adding the ordinary least squares residual of the subject to the fitted model value. Traditional conventions to summarize therapeutic alliance were also utilized including average overall TA, average early TA, and average late TA.

The distributions for early alliance slope, late alliance slope, and the conventional TA summary variables were all evaluated for normality. Significant outliers were

identified for both of the slope variables (i.e., three and five, respectively). These outliers were truncated by windsorizing them. All distributions were then assessed for normality using both Kolmogorov-Smirnov and Shapiro-Wilk tests (Chakravarti, Laha, & Roy, 1967; Shapiro & Wilk, 1965). All were non-significant indicating the distributions reasonably adhere to a normal distribution for their respective sample size.

The relationship between therapeutic alliance growth and outcome was then evaluated. The primary outcome variable for the MTA, the SNAP-IV AD/HD composite score, was utilized as the dependent variable as reported by both caregivers and teachers. As was practiced in the primary MTA analyses, a log transformation was performed on the Level-1 time variable due to the curvilinear trajectory of outcome growth. The practice of transforming curvilinear trajectories with a log transformation aids both estimating parameters and interpretation of the coefficients (Cohen, P., Cohen, J., West, S., & Aiken, L., 2002).

Therapeutic alliance growth was first evaluated with the caregiver-reported AD/HD composite score up to the end of the treatment phase (i.e., 14-month timepoint). Table 6 summarizes the unconditional model for caregiver-reported outcome with a log-transformed Level-1 time covariate. Inspection of the growth coefficient indicates that the slope is significant (i.e., AD/HD symptoms improve over time). The variance components for both growth and initial status are also significant, justifying their model specification as random effects.

Table 6. Unconditional model for Caregiver-reported SNAP-IV AD/HD Composite over the course of treatment (i.e., through 14-month timepoint)

Fixed Effect	Coefficient	SE	*t* Ratio (*df*=137)	*p* Value
Mean initial status, β_{00}	1.89	0.06	33.17	<0.001
Mean SNAP growth rate, β_{10}	-0.33	0.02	-15.89	<0.001

Random Effect	Variance	*df*	χ^2	*p* Value
Initial status, r_{0i}	0.301	137	420.57	<0.001
Growth rate, r_{1i}	0.022	137	222.47	<0.001
Level-1 error, e_{ti}	0.151			
Random level-1 coefficient				
Initial status, π_{0i}	.67			
Growth rate, π_{1i}	.38			
Deviance Statistic (6)	860.21			

The effect of treatment group (i.e., Combined Treatment vs. Parent Training Only) on the AD/HD composite score was then modeled. Table 7 summarizes the effect of treatment group on caregiver-reported outcome at the end of the treatment phase. When compared with the unconditional model, the model including treatment group predicts a significant amount of additional variance to justify its inclusion ($\chi^2(1) = 30.72$, $p < .001$). Consistent with the major findings of the MTA study, the combined treatment group improved at a significantly faster rate when compared to parent training alone. These are illustrated in Figure 2.

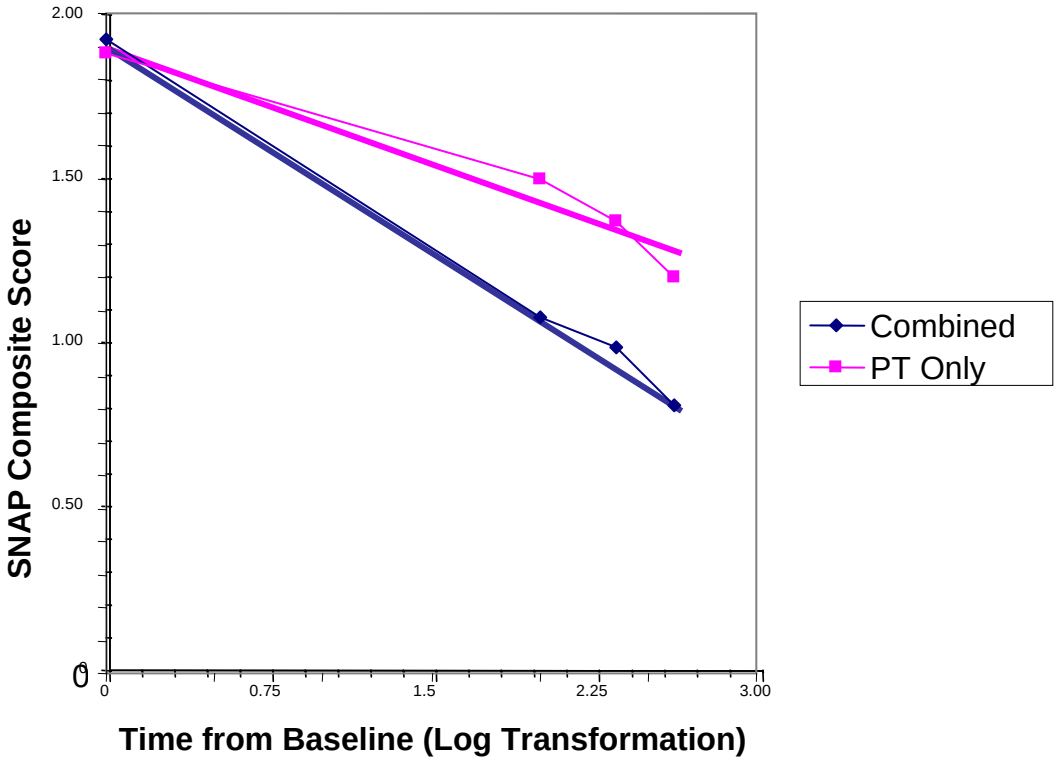

Figure 3. 14-month caregiver-reported AD/HD symptoms by treatment group

Both early and late therapeutic alliance slope was then evaluated by entering it in the model that included treatment group. Table 8 summarizes the model with treatment group, early TA slope, and late TA slope. When the TA slopes were entered, they werefound not to predict any additional variance $\chi^2(2) = 0.86$, $p < .65$). Similar null findings were found when average early TA ($\chi^2(1) = 0.32$, $p < .57$), average late TA ($\chi^2(1) = 0.08$, $p < .78$), and average overall TA ($\chi^2(1) = 0.02$, $p < .89$) were entered respectively with a model including treatment group. It appears that, contrary to our hypotheses, that therapeutic alliance did not contribute to predicting eventual treatment outcome over and above participation in combined treatment.

Table 7. 14-month caregiver-reported AD/HD composite model including treatment group

Fixed Effect	Coefficient	SE	*t* Ratio (*df*=137)	*p* Value
Mean initial status, β_{00}	1.89	0.06	33.23	<0.001
Mean SNAP growth rate, β_{10}	-0.33	0.02	-17.25	<0.001
Combined treatment, β_{11}	-0.18	0.03	-5.90	<0.001

Random Effect	Variance	*df*	χ^2	*p* Value
Mean initial status, β_{00}	0.300	137	419.66	<0.001
Growth rate, r_{1i}	0.014	136	189.39	0.002
Level-1 error, e_{ti}	0.152			
Random level-1 coefficient				
Initial status, π_{0i}	.67			
Growth rate, π_{1i}	.27			
Deviance Statistic (7)	829.490318			

Variance explained	Var (π_{0i})	Var (π_{1i})
Unconditional	N/A	0.02232
Conditional on Treatment	N/A	.01356
Proportion of Variance Explained	N/A	39.2

Table 8. 14-month caregiver-reported AD/HD composite model including treatment group and piecewise alliance slopes

Fixed Effect	Coefficient	SE	*t* Ratio (*df*=137)	*p* Value
Mean initial status, β_{00}	1.89	0.06	33.21	<0.001
Mean SNAP growth rate, β_{10}	-0.33	0.02	-17.29	<0.001
Combined treatment, β_{11}	-0.18	0.03	-5.77	<0.001
Early TA Slope, β_{12}	-0.55	0.90	-0.61	0.55
Late TA Slope, β_{13}	0.70	1.88	0.37	0.71

Random Effect	Variance	*df*	χ^2	*p* Value
Mean initial status, β_{00}	0.300	137	419.64509	<0.001
Growth rate, r_{1i}	0.013	134	187.46679	0.002
Level-1 error, e_{ti}	0.152			
Random level-1 coefficient				
Initial status, π_{0i}	.67			
Growth rate, π_{1i}	.27			
Deviance Statistic (9)	828.625328			

Therapeutic alliance was also found to not be related to caregiver-reported outcome when long-term effects were evaluated (i.e., 24-month time point). Consistent with the previous findings, the model including treatment group accounted for a significant amount of additional variance ($\chi^2(1) = 18.07$, $p < .001$) and clients in the combined treatment group improved at a faster rate. However, a model with treatment group, and both TA slope parameters did not account for additional variance when compared with the model with only treatment group ($\chi^2(2) = 0.78$, $p < .68$). The traditional TA conventions of average early TA ($\chi^2(1) = 0.00$, $p < .99$), average late TA,

and average overall TA ($\chi^2(1) = 0.47$, $p < .49$) also failed to account for additional variance when entered in model including treatment group. It appears that therapeutic alliance is not related to outcome on any of the primary caregiver-reported outcome measures.

Therapeutic alliance growth was also evaluated using the teacher-reported AD/HD composite of the SNAP-IV in a similar fashion. Beginning with the data available up to the end of treatment (14-month timepoint), treatment group was entered first and yielded a model that accounted for a significant amount of additional variance when compared to the unconditional model ($\chi^2(1) = 14.33$, $p < .001$). When the TA slopes were then added to the model with treatment group, they did not account for a significant amount of variance ($\chi^2(2) = 2.10$, $p < .35$). Similar non-significant findings were found for average overall TA ($\chi^2(1) = 0.15$, $p < .70$), average early TA ($\chi^2(1) = 0.98$, $p < .32$), and average late TA ($\chi^2(1) = 0.11$, $p < .74$). Similar to the primary caregiver-reported outcome measures, therapeutic alliance did not predict outcome over and above treatment assignment at the end of treatment (14-months).

Some significant relationships between outcome and therapeutic alliance did emerge when the teacher-reported long term outcomes were included. As before, treatment group was found to be a significant predictor of treatment outcome when compared to the unconditional model ($\chi^2(1) = 15.81$, $p < .001$). When the TA slopes were added to the model including treatment group, the new model including the TA slopes accounted for a significant amount of variance ($\chi^2(2) = 6.85$, $p < .03$), and it is summarized in Table 9. Inspection of Table 9 indicated that the Early TA slope

parameter was not significant, and so it was removed from the model. The model including only treatment group and Late TA slope was then generated but not significant ($\chi^2(1) = 3.48$, $p < .06$). Similar to all previous outcome analyses, the traditional TA conventions of average TA ($\chi^2(1) = 0.08$, $p < .78$), average early TA ($\chi^2(1) = 0.61$, $p < .44$), and average late TA ($\chi^2(1) = 1.84$, $p < .17$) were all non significant. Similar to the other analyses, the traditional summaries of therapeutic alliance did not predict outcome in the 24-month teacher-reported outcome measures. However, entering both early and late therapeutic alliance slope did predict treatment outcome over and above treatment assignment (i.e., 15.1% explained error variance). However this trend did not hold once early alliance slope was removed due to lack of a significant Early TA slope coefficient.

Pattern Mixture Analysis

The degree that therapeutic alliance missing data patterns affects outcomes were evaluated using pattern mixture analysis procedures. In order to accurately evaluate patterns of missing data, a dummy coding strategy was utilized to reflect different patterns of missing data. Subjects were divided by these missing data patterns and entered as level-2 variables. As mentioned previously, therapeutic alliance ratings had six "preferred" timepoints where alliance data was to be collected (i.e., Sessions 4, 6, 12, 18, 24, 27). Due to the number of permutations of missing data in six timepoints (i.e., $2^6 = 64$ separate patterns), the groups were combined to early, middle, and late. From these major categories, the frequency table for existing data patterns was generated and is displayed in Table 10.

Table 9. 24-month caregiver-reported AD/HD composite model including treatment group and piecewise alliance slopes

Fixed Effect	Coefficient	SE	*t* Ratio (*df*=133)	*p* Value
Mean initial status, β_{00}	1.95	0.07	29.20	<0.001
Mean SNAP growth rate, β_{10}	-0.37	0.02	-15.20	<0.001
Combined Treatment, β_{11}	-0.12	0.03	-3.88	<0.001
Early TA Slope, β_{12}	1.83	1.07	1.72	0.088
Late TA Slope, β_{13}	4.67	1.77	2.65	0.010

Random Effect	Variance	*df*	χ^2	*p* Value
Mean initial status, β_{00}	0.285	133	255.54	<0.001
Growth rate, r_{1i}	0.015	130	165.06	0.02
Level-1 error, e_{ti}	0.331			
Random level-1 coefficient				
Initial status, π_{0i}	0.475			
Growth rate, π_{1i}	0.185			
Deviance Statistic (9)	1185.223060			

Variance explained	Var (π_{0i})	Var (π_{1i})
Conditional on Treatment	N/A	0.01750
Conditional on Treatment and Alliance Slope	N/A	0.01485
Proportion of Variance Explained	N/A	15.1

Table 10. Frequency table of missing data patterns over early, middle, and late parent-training sessions

Missing Data Pattern	Frequency	Percentage
Observed./Observed./Observed.	121	52.8
Observed./Observed./Missing.	26	11.4
Observed./Missing./Missing.	24	10.5
Missing./Missing./Observed.	20	8.7
Missing./Observed/Observed	17	7.4
Observed/Missing./Observed	16	7.0
Missing/Observed/Missing.	5	2.2

Few missing data patterns had enough data to warrant separate analyses for each category. When this is the case, Hedecker & Gibbons (1997) recommend testing missing data patterns that both incorporate adequately represented categories and can model complete/incomplete data patterns. For this reason two major comparisons were utilized that represented the most frequent missing data patterns. The first compared those with complete data in all three phases in treatment to those with any missing data (i.e., complete vs. incomplete data). This comparison included all data available and compared groups of relative equal groups (i.e., Completers = 53% vs. Non-completers = 47%). The second pattern that utilizes the most frequent missing data patterns compares completers vs. those that begin initial data collection and then subsequently drop out of all further alliance data collection. This analysis compared the three most common missing data patterns and 75% of the sample. This comparison also highlights our hypotheses regarding the importance of early alliance and subsequent dropout could serve as a proxy for low treatment engagement. Hedecker & Gibbons describe this comparison as a monotone pattern of dropout and is represented by two dummy coded variables. The dummy coding strategies for both the incomplete and monotone coding schemes are illustrated in Appendix II.

The impact of various patterns of missing alliance data was tested on several outcome variables. Similar to our previous procedures, an unconditional model must first be generated before the pattern mixture models can be compared to it. For comparing subjects with incomplete data collection vs. complete data collection, the full unconditional model for therapeutic alliance displayed in Table 3 was used. Using the "incomplete" coding strategy illustrated in Appendix 2, the impact of a subject having incomplete data was tested on their overall therapeutic alliance growth. The pattern mixture model was found to be nonsignificant, suggesting that the effect of having incomplete data does not significantly alter the growth or intercept of therapeutic alliance ($\chi^2(2) = 0.53$, $p < .77$). Similarly using the "monotone" coding strategy, no significant differences were found on alliance development between subjects with complete data versus initial participators who "dropout" in future waves ($\chi^2(4) = 4.22$, $p < .38$).

Similar procedures tested the impact of missing alliance data on treatment outcome. Consistent with the lack of effects missing data has on alliance growth, missing data patterns had no significant impact on outcome. When using the incomplete coding strategy, missing data patterns had no effect on either caregiver-reported 14-month ($\chi^2(2) = 2.26$, $p < .33$) or 24-month outcome ($\chi^2(2) = 1.54$, $p < .47$). Incomplete data also had no effect for 14-month ($\chi^2(2) = 0.66$, $p < .72$) and 24-month ($\chi^2(2) = 0.28$, $p < .87$) teacher-reported outcome. When using the monotone coding strategy (i.e., initial participators who dropout in all subsequent data collection), no effects were found on teacher-reported (14-month- $\chi^2(4) = 1.50$, $p < .83$; 24-month- $\chi^2(4) = 3.53$, $p < .47$) or

caregiver-reported (14-month- $\chi^2(4) = 1.21$, $p < .88$; 24-month- $\chi^2(4) = 3.00$, $p < .56$) outcome.

It appears that patterns of missing therapeutic alliance data do not predict either therapeutic alliance growth or subsequent clinical outcome. Although hypothesized to be a proxy for treatment engagement, it did not predict outcome on either caregiver- or teacher-reported outcome measures.

Survival Analysis

The effect of treatment engagement variables on premature termination was evaluated using survival analysis procedures. In particular, Cox (1972) regression procedures were used to generate both the survival functions and test the goodness of fit of added covariates to the baseline hazard function. Because Cox regression assumes that time is on a continuous scale, the number of days since baseline was used as opposed to assessment point for the time variable. Also due to the skew in the time distribution typically found in survival analysis, time was modeled after a log transformation. Figure 2 illustrates the baseline cumulative survival function. To ease interpretation, the x-axis was transformed back to raw number of days since baseline. Inspection of the function indicates that a gradual decline in treatment participation occurs during the 3-month (median= 104 days) and 9-month time points (median = 229 days). A more drastic decline occurs at the 14-month timepoint (median = 412 days), where a larger proportion

of the sample attrite.

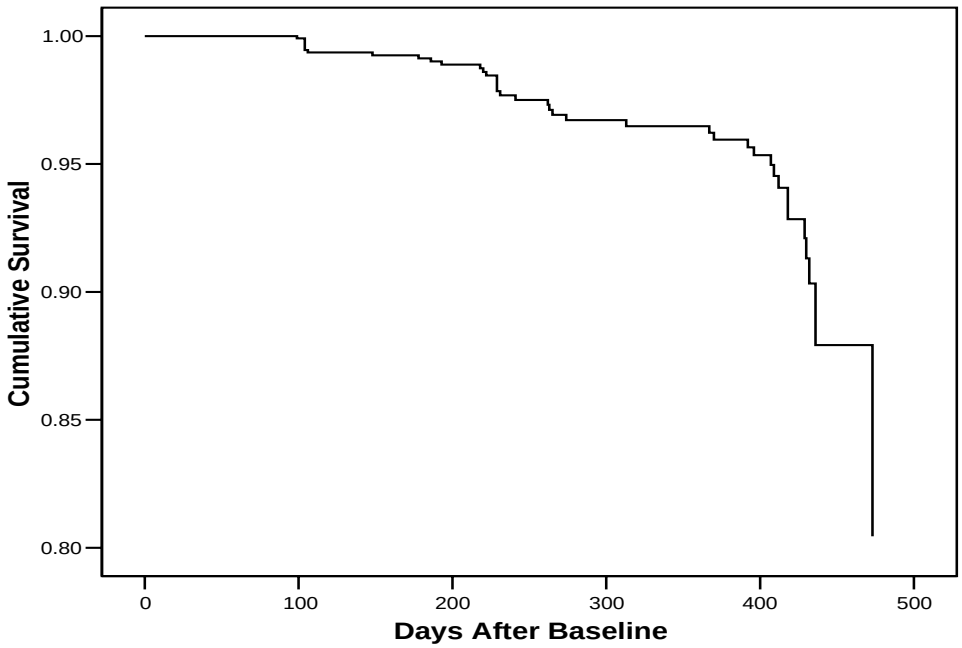

Figure 4. Cumulative survival function of treatment dropout

Covariate analyses were conducted using the primary therapeutic alliance variables, missing data patterns, and treatment assignment. Each static covariate (i.e., average overall alliance, alliance missing data pattern, and treatment assignment) was tested separately by entering it as a predictor and comparing the difference in goodness of fit , which is distributed as a chi-square with one degree of freedom. In addition, alliance slope (early and late) and average slope (early and late) were modeled as time-varying covariates. Based on the concordance between parent-training session 14 (i.e., the midpoint of the treatment protocol) and the corresponding outcome measurement point, the time-varying covariate was considered "early" until the 9-month outcome measurement point when the covariate became "late".

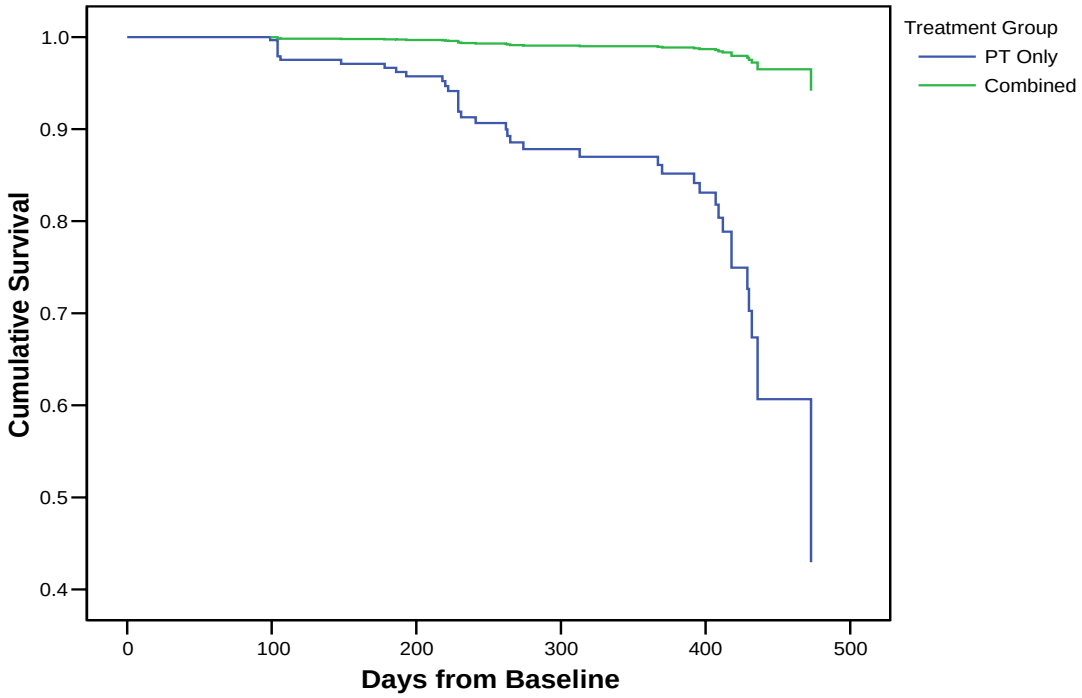

Figure 5. Cumulative survival by treatment group

The results of covariate effects on survival are displayed in Table 11. Consistent with previous analyses, none of the alliance variables or missing data patterns were related to premature termination. The only significant predictor of premature termination was treatment assignment. Figure 3 illustrates the survival function by treatment group. Once again, in contrast to treatment group assignment, it appears that engagement variables are not related to outcome in the MTA data set.

Table 11. Cox regression analyses with covariates predicting premature termination.

Covariate	*B*	*SE*	χ^2_{dif}	*p* Value	Effect Size (*r*)
Treatment Group	-2.64	0.60	38.39	<0.001	0.41
Overall Average TA	0.48	1.39	0.12	0.73	0.02
Early Average TA	.08	1.45	0.00	0.96	0
Late Average TA	.03	1.54	0.00	0.99	0
Early TA Slope	-10.08	12.94	0.61	0.44	0.06
Late TA Slope	12.78	21.50	0.35	0.56	0.05

CHAPTER IV

DISCUSSION

This study generated several findings related to therapeutic alliance and outcome:

1. Therapeutic alliance is volatile within-clients and has different trajectories in early versus late treatment. This brings into question the utility of traditional means of summarizing therapeutic alliance such as average overall alliance and one-session snapshots of alliance over phases of treatment.
2. Assignment to the combined treatment (i.e., medication management and behavioral treatment) significantly predicted therapeutic outcome. Inconsistent with our hypotheses, therapeutic alliance did not consistently predict treatment outcome over and above treatment assignment.
3. Patterns of missing therapeutic alliance data, hypothesized to be a proxy for overall treatment engagement, was not predictive of therapeutic alliance growth or treatment outcome.
4. Premature treatment dropout, as analyzed using survival analysis, was only predicted by treatment group assignment. Treatment engagement variables such as therapeutic alliance and missing alliance data patterns did not predict premature treatment termination.

This study incorporated several different statistical techniques to investigate the role of longitudinal treatment engagement on treatment outcome. The treatment engagement literature, with a few recent exceptions, has not addressed adequately the role that longitudinal changes in constructs such as therapeutic alliance can affect eventual treatment outcomes. As opposed to the majority of studies that use one-session snapshots to represent stretches of treatment, this study utilized piecewise HLM growth models to represent therapeutic alliance trajectories representative of the larger course of treatment engagement over time.

This study utilized the Multimodal Treatment Study of Children with AD/HD to address the role of longitudinal treatment engagement for several reasons. First, the MTA systematically collected therapeutic alliance data up to seven times over the course of a 14-month treatment span. There is no other published study that parallels the scope or frequency of therapeutic alliance data collection over such a long period of treatment. Second, the MTA collected therapeutic alliance data from the most commonly utilized measure (i.e., WAI). The fact that this study utilizes the state-of-the-art measure adds to the generalizability of the findings. Third, the study also was a first to evaluate the role that caregiver therapeutic alliance could have on clinical outcome of child mental health services. The role that caregivers play in both the financing and execution of child treatment plans are well documented (Weisz & Jensen, 1999). Therefore, the impact of the caregiver's bond to the therapist on outcome is important to evaluate.

First, this study described the pattern of therapeutic alliance growth during the course of therapy. The theories describing therapeutic alliance development typically conceptualize alliance to be a volatile process, particularly in the early stage of treatment.

Therefore for this sample, it was hypothesized that higher fluctuations in therapeutic alliance (i.e., more drastic growth) and the overall volatility (i.e., higher variability) would be most likely found in the early stage of treatment. Once the therapeutic alliance is established and the client internalizes the treatment rationale in later stages, the alliance should stabilize (i.e., small subsequent alliance growth) and the frequency of treatment alliance ruptures should dissipate (i.e., less variability).

In order to adequately describe the possibility that growth may be different or more variable in different phases of treatment, a therapeutic alliance piecewise growth model was generated. When the piecewise model was compared to a single slope model, the findings were consistent with all of the initial hypotheses. First, the piecewise model represented therapeutic alliance growth significantly better than a typical linear growth model. This suggests that alliance growth could vary significantly depending on the phase of treatment. Second, alliance growth exhibited more drastic growth trends early in treatment. Inspection of the alliance growth coefficients indicated that alliance generally increased during the early phase of treatment but then typically leveled off (i.e., no significant growth) during the late phase of treatment. Finally, the variability of alliance was more pronounced in early treatment. The variance in early treatment was also larger than in late treatment suggesting that the volatility also decreased as treatment progressed.

These descriptive findings from the alliance piecewise growth model can be explained by either theoretical or methodological factors. Most of the theories relevant to therapeutic alliance suggest that treatment engagement would be the most volatile early in the treatment process. For example, the *phase model for psychotherapy* describes that

a client seeking treatment must first be socialized to the rationale and procedures of treatment in the early phase of *remoralization*. During this phase the client must reframe their mental condition and begin to trust the therapist as an agent for change. Due to the number of factors that could affect the outcome of this initial phase (e.g., the client's motivation to change, the match between pretreatment expectancies and the therapist's treatment rationale, the therapist's ability to persuade the client), the period would be expected to be highly volatile and could perhaps lead to the most client-initiated unilateral treatment termination. The descriptive picture of MTA therapeutic alliance growth would be consistent with this theoretical explanation. However, the methodological nuances of the MTA sample cannot be entirely ruled out. The distribution of therapeutic alliance ratings was skewed, indicating that a majority of caregivers reported high alliance with their therapist throughout the study. As therapeutic alliance increased during the study, the already high alliance may have exhibited a ceiling effect in late sessions for some participants. In fact, 17% of the sample that had a late session at all (i.e., 31 of 186) had the highest score possible on their last recorded late session. However, despite the overall high alliance ratings in late sessions, 42% of the late TA slopes in the piecewise model were decreasing. These findings indicate that although overall high TA led to truncation and ceiling effects in a portion of the sample, nearly half of the participants were demonstrating decreasing alliance in late treatment. This indicates that although alliance growth is affected by consistently high alliance ratings, ceiling effects cannot completely explain the descriptive findings.

The second question this study addressed was the relationship between therapeutic alliance growth and treatment outcome. In addition to evaluating the

predictive utility of alliance growth, the study also evaluated how alliance growth compares to the literature regarding traditional methods of summarizing therapeutic alliance. Consistent with previous studies, the average early, late, and overall alliance were all included as possible predictors of treatment outcome. It was hypothesized that all therapeutic alliance indicators would be related to therapy outcome, but therapeutic alliance growth would be a better predictor due to its ability to represent treatment engagement trends. Furthermore, early alliance growth was hypothesized to be the most predictive indicator of eventual outcome, because it represented the alliance trend during the relationship-building phase of treatment.

Contrary to our hypotheses, every therapeutic alliance indicator failed to predict treatment outcome over and above the effect of treatment group. In both caregiver- and teacher-reported outcome, none of the therapeutic alliance indicators significantly predicted outcome. Similar results were found in both 14-month and 24-month outcome. There was a trend for late therapeutic alliance growth to be a significant predictor over and above treatment assignment in the 24-month teacher reported data. However, little weight can be attributed to this finding due to the number of a priori comparisons conducted.

The third major research question of this study was the degree that patterns of missing alliance data could predict alliance or treatment outcomes. Although therapeutic alliance ratings can directly assess a client's level of treatment engagement, this study hypothesized that families who participate sporadically in alliance data collection may also not be engaging in the overall treatment. Conceptualizing participation in alliance data collection as a proxy to therapeutic alliance itself, caregivers who participate in most

or all data collection procedures were expected to have higher alliances as well as better outcomes. Two different coding strategies were used to assess the impact of missing data as outlined by Hedecker & Gibbons (1997). The "incomplete" coding scheme directly compared those who completed alliance data in all phases of treatment to those who missed any along the way. Although this can illustrate the general impact of missing data on outcome, it does not address our hypotheses regarding the impact of dropout after initial early treatment engagement. If clients participate initially and then subsequently dropout, it is hypothesized that these clients will have initially poor alliance (i.e., in early treatment) and subsequent poor outcome.

As with the ratings of alliance, the patterns of missing alliance data were not related to outcome. For both the incomplete and monotone coding schemes, missing data patterns were related to neither alliance nor outcome. Although it may have been the case that missing alliance data collection could be a proxy for poor alliance (particularly in this generally high alliance sample), the results do not support our hypotheses. Perhaps other proxies for treatment engagement may be more related to alliance development and outcome. Proxies such as parent-training session attendance or medication management adherence (e.g., pill counts, prescription records) may be better indicators of treatment engagement because they are directly related to participation in specific components of treatment.

Our final research question investigated whether treatment engagement variables or treatment selection was related to premature dropout. Similar to our previous research questions, it was hypothesized that those with low overall therapeutic alliance, declining alliance growth, and poor alliance measurement attendance would be associated with

premature termination. Since combined treatment was found to a more effective treatment for AD/HD, the study also hypothesized that treatment group was related to outcome. It was argued that not being exposed to the most effective treatment would be similarly related to premature termination as treatment engagement.

Consistent with our previous analyses, none of the therapeutic alliance indicators predicted premature termination. The only significant predictor was treatment group. Overall, not a large portion of the sample prematurely dropped out (40 out of 229). However, 38 of those who did dropout were from the Parent Training Only treatment group. At first glance, it would seem the difference between premature treatment termination between the combined and parent-training groups is vastly different. However, it should be noted that the MTA defined dropout as both participants who literally dropped out of the study and/or participants who crossed treatment arms (i.e., began another treatment that was not initially assigned). Although the distinction between these types of "dropout" were not coded directly, it was systematically easier to dropout of the parent training only group because adding any additional treatment constituted dropping out of this group. In contrast, nothing could be added to the combined treatment group. Nevertheless, significantly more parent-training only participants dropped out, most likely due to experiencing less benefit from the parent training only treatment. When comparing these results with the lack of association with therapeutic alliance variables, it is interesting to speculate why alliance was not related to any outcome in most comparisons.

Despite the large literature indicating that therapeutic alliance is a significant predictor of outcome, some aspects particular to the MTA study may have contributed to

alliance's non-findings. For example, this is the first outcome study to date that has evaluated therapeutic alliance's relationship to outcome of AD/HD treatment. There may be factors specific to AD/HD (e.g., primarily a neurological disorder) that may stymie a strong therapeutic relationship from having additional benefit over and above highly effective treatments. Just as it would be unreasonable to suggest that having a strong bond with your neurosurgeon could improve the outcome of having a tumor over and above the latest surgical procedures, the same may be the case for disorders that are primarily not affected by psychological factors such as social influence.

The fact that the most effective treatment for AD/HD is a combination of medication and psychological treatment could also be related to alliance's non-findings. Although alliance has been found to be related to outcome in pharmacotherapy trials before (e.g., Krupnick, Sotsky, Simmens, & Moyer, 1996), alliance may not be adequate to predict outcome consistently in interventions that require little more of the client than take medication daily. Alliance may be a better predictor of outcome in interventions that require more time-consuming/involving therapeutic tasks (e.g., doing daily mood grids, thought records several times a day). It could also be a better predictor in interventions that have a larger variability in treatment adherence. For example, a client with adequate alliance could attend weekly sessions to vent but do very little outside the therapy hour to address their issue. However, a highly engaged client may journal, complete, daily mood records, implement the therapist's suggestions for behavior change, or other activities in addition to attending the treatment session. For these reasons, alliance may be a better predictor of treatment outcome in interventions where the variability in exposure to a treatment's "specific effects" is greater or involve large

amounts of time/energy to be compliant with the treatment. Even among medication treatment regimens, treatment engagement variables may be better related to outcome in regimens that require extensive monitoring/dosing (e.g., diabetes control) than taking a dose once or twice a day, as is typically the case in AD/HD treatment.

Another possible factor for the non-findings specific to the MTA data was that therapeutic alliance was reported by the client's caregivers. The MTA study is the first study to date that directly addresses the role of caregiver therapeutic alliance on client outcome. Although there is previous literature citing the importance of caregiver therapeutic alliance in child studies, most of these studies were conducted using retrospective surveys or were theoretical papers. Several factors could explain why caregiver therapeutic alliance would not be as predictive of outcome as it is in typical mental health treatment modalities. First, although the "therapy" is caregiver-centered the eventual outcome is based on their child's symptoms. It could be that the therapeutic relationship that the caregiver develops with the therapist may be too far removed to helping with the child's symptoms. Theories relevant to alliance's impact on outcome describe how the therapeutic relationship is important to remoralizing the client by reframing their problems in the context of the therapist's treatment rationale and instilling hope (e.g., *remoralization*) (Frank & Frank, 1993; Howard et al, 1993). Motivating the client through processes such as *remoralization* may improve symptoms via positive expectancies or other psychological effects that may be difficult to transfer to an individual not directly involved in the therapeutic process.

Finally, therapeutic alliance's inability to predict outcome or premature termination may be related to the lack of variability in the MTA alliance data. As

mentioned previously, the majority of caregivers in the MTA sample had high alliances with their therapist. It may be that high overall alliance ratings should be expected in efficacy trials that include highly trained clinicians using state-of-the-art treatments with intense supervision. In fact, high alliance ratings in randomized control trials are not uncommon, particularly with self-report alliance measures (Kendall et al, 1997; Schelef, Diamond, Diamond, & Liddle, 2005). However, the fact remains that truncated distributions leading to ceiling effects limit the ability to detect true associations between variables (Cohen, P., Cohen, J., West, S., & Aiken, L., 2002). Further troubling is the fact that families who did not participate in alliance data collection had poorer treatment outcomes on teacher-reported measures. Having a more representative sample from all families involved in treatment may have increased the power enough to detect differences.

The following study evaluated the impact of longitudinal therapeutic alliance development on outcome and other treatment engagement variables. Based on the available data using appropriate longitudinal analytic techniques, caregiver-reported therapeutic alliance does not appear to be strongly related to treatment outcome for child AD/HD treatment. Although this study advanced the scope of how alliance can be investigated in child studies, much more work is needed to determine the impact that treatment engagement variables affect outcome. This study utilized hierarchical linear modeling that can flexibly model sample trends despite missing data or irregularly spaced time points. Although this approach is flexible to account for missing data, samples that can minimize the amount of missing data and include additional treatment engagement variables can address more specific theoretical and methodological longitudinal questions than in the current study.

One way to advance alliance literature would be to actively test competing theories why therapeutic alliance is related to treatment outcome. For example, there are two competing hypotheses explaining alliance's relationship to outcome based on the general- vs. specific-effects debate (Feeley, DeRubeis, & Gelfand, 1999; Wampold, 2001). One main theory explains that therapeutic alliance is a necessary predicate of future treatment benefit, and improvement in symptoms/functioning can be predicted by previous gains in the therapeutic relationship (e.g., Klein et al, 2003; Frank & Frank, 1993). A competing theory explains that high therapeutic alliance is based on experiencing prior symptom relief based on the specific effects of treatment (e.g., CBT techniques). Although these competing theories have been actively tested in previous studies (e.g., DeRubeis, Brotman, & Gibbons, 2005), these studies have used OLS regression techniques that are simply not adequate to address these longitudinal questions. A possible future study could utilize structural equation modeling (SEM) that can specifically test the two competing models using cross-lag designs. However, due to SEM requirement of equal time intervals and complete data, this study may be more likely to be conducted in treatment efficacy trials than in community settings.

Another way to move the literature forward is to stop focusing exclusively on therapeutic alliance and expand the scope of treatment engagement to other variables. Although alliance has been found to be a key predictor of outcome, it is only one indicator of treatment engagement. Specifically, alliance is a summary of attitudes toward the therapist and treatment procedures. But it does not inform about what a clients *does* in the context of the therapy procedures. Adding engagement variables such as homework completion rates or weekly session attendance in addition to regular

therapeutic alliance ratings may represent a better indication of overall treatment engagement. Additional variables such as pretreatment expectancies have also been getting increased attention (e.g., Dew & Bickman, 2005). Promising research in this field suggests that therapeutic alliance mediates the relationship between expectancies and eventual therapeutic outcome (Abouguendia, Joyce, Piper, & Ogrodniczuk, 2004). Investigating further the relationship between pretreatment variables, alliance, and eventual outcome may lead to better understanding of the longitudinal processes that affect outcome.

A final approach to improving the therapeutic alliance literature is to develop better measures that have more suitable distributions for analyses. The distributions for both the therapeutic alliance ratings were both highly skewed and positive. More attention needs to be focused on developing alliance scales that are less likely to have ceiling effects (i.e., more "difficult" in content). Generating items that may tap into higher degrees of alliance (e.g., "I implement what my therapist challenges me to do everyday") may further differentiate alliance on the high side. New measurement methods, such as *item response theory*, can be implemented to determine items that may discriminate between individuals with moderate alliance and high alliance (Embretson & Reise, 2000). By continuing to use measures with truncated variance, researchers may be generating spurious findings and attenuated correlations between alliance and other variables.

APPENDIX A

CONSTRUCTING PIECEWISE MODELS

Piecewise models are useful to model nonlinearity in data. Although a linear model seems the most parsimonious when looking at the entire span of treatment (i.e., 14 months), it is sometimes useful to investigate different phases of treatment. Inspection of the growth plots indicated generally linear, but curvilinear patters were evident in many of the plots for a short period of time. Most of these inflections were transient, and the lines would then become linear again. For alliance researchers, an interesting question is whether these varying patterns are more pronounced in a particular phase of therapy? In order to test this, one could construct a piecewise model.

In order to accomplish this in the MTA data set, we essentially construct two separate slopes for an early phase (through Session 14) a then one for later treatment (Session 15 through Session 27). The level-1 model would look like this:

$$Y_{ti} = \pi_{0i} + \pi_{1i}(\text{Early Phase}) + \pi_{2i}(\text{Late Phase}) + e_{ti}$$

Once this is constructed, the unconditional model can assess the slope and volatility for each phase of treatment. If both phases are significant, we can then identify how much variance is predicted by using this approach over the simple linear model.

In order to construct the piecewise model, each observation has a coded variable that relates to each of the slope parameters. Since the time variable of the linear model is based on the session number, the coding scheme for the separate slopes follows from that. However, it is difficult to describe concisely the coding scheme, because the three variables are coded differently depending on the particular measurement time point. For this reason, we will illustrate with an example from the data set.

The following is a case from the MTA alliance data. The dummy codes early and late signify which phase of treatment each repeated measure was collected in. The variable "Session" is the parent-training session that therapeutic alliance was observed. Since no alliance data was collected prior to Session 2, session number was centered on Session 2 (i.e., becoming the session representing the intercept). From this centered variable, the piecewise model is constructed by constructing two slope parameters that will selectively estimate a particular phase of treatment. These slope variables are called early_slp and late_slp. The variables are shown below from subject 3420:

id	early	late	Session	Centered	Early_slp	Late_slp
3420	1	0	2	0	0	0
3420	1	0	5	3	3	0
3420	1	0	11	9	9	0
3420	1	0	12	10	10	0
3420	0	1	14	12	12	0
3420	0	1	16	14	12	2
3420	0	1	17	15	12	3
3420	0	1	23	21	12	9
3420	0	1	24	22	12	10
3420	0	1	25	23	12	11

As this illustrates, early_slp is coded exactly the same as unconditional time variable for measurement points in that phase. For subsequent measurement points, it is coded as the last recorded alliance point in the early phase. Subsequent time points are coded similarly except they must subtract the last measurement session of the previous time point from their respective days from baseline.

APPENDIX B

PATTERN MIXTURE ANALYSIS DUMMY CODING

Hedecker & Gibbons (1997) describe several dummy coding systems that can be used to model missing data patterns. Many times differences between all possible missing data pattern cannot be evaluated because some categories are not realized adequately in the sample. In these cases, coding schemes that can both model incomplete vs. complete data patterns and represent a large proportion of the sample should be implemented. The two coding schemes that were used for this pattern mixture analysis compared either complete vs. incomplete data or compared complete data vs. those that initially participated and then dropped out for all subsequent data collection periods. The dummy coding strategies for both schemes is described below. The incomplete coding scheme adds a dummy code in the model that will represent the effect having anything but complete data. The monotone coding scheme adds two dummy codes (i.e., M1 and M2) that are entered simultaneously. When these two are added to the level-2 model, each missing data pattern is compared respectively to subjects with complete data. Both the "incomplete" and "monotone" coding schemes can represent large proportions of the overall alliance sample (100% and 75%, respectively) and address the question if differential missing data patterns can predict the course of alliance or treatment outcome.

Missing Pattern	D1	D2	D3	D4	D5	D6	M1	M2	Incomplete
Obs./Obs./Obs.	0	0	0	0	0	0	0	0	0
Obs./Obs./Msg.	1	0	0	0	0	0	0	1	1
Obs./Msg./Msg.	0	1	0	0	0	0	1	0	1
Msg./Msg./Obs.	0	0	1	0	0	0			1
Msg./Obs./Obs.	0	0	0	1	0	0			1
Obs./Msg./Obs.	0	0	0	0	1	0			1
Msg./Obs./Msg.	0	0	0	0	0	1			1

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