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Effects of adult inpatient group psychotherapy on attachment characteristics: An observational study comparing routine care to an untreated comparison group

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Effects of adult inpatient group psychotherapy on attachment characteristics: An observational study comparing routine care to an untreated comparison group

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Abstract
This study investigated the changes in attachment characteristics of patients undergoing inpatient group psychotherapy in routine care. We collected data from 265 consecutively recruited patients and 260 non-clinical control persons using self-report measures of attachment, depression, and socio-demographic characteristics. The effects of treatment on patients were analyzed using propensity score techniques (propensity strata and logit-transformed propensity scores) in combination with a generalized analysis of covariance. A moderate increase of attachment security was found which could be attributed to a decrease both in attachment anxiety and avoidance. Pre-post improvements in attachment with regard to romantic partnerships were stable after a 1-year follow-up. Furthermore, we found significant treatment-covariate interactions indicating that subjects with particularly high treatment propensities (propensities were highly correlated with depression and attachment anxiety) improved the most in terms of attachment security. Our results are encouraging for psychotherapeutic practice in that they provide evidence that long-term attachment improvements can be reached via psychotherapy. Our results will also provide a sound basis for future studies in the field of clinical attachment research, e.g., studies examining whether improved attachment security is correlated to symptom improvements in different psychological disorders.

Keywords: attachment; inpatient psychotherapy; group psychotherapy; propensity analysis; generalized analysis of covariance

Introduction

In the past two decades attachment theory has become increasingly important in clinical psychology and psychotherapy research. Attachment characteristics have been analyzed as pretreatment characteristics which influence both the psychotherapeutic process and the outcome of therapies. Several studies have shown that insecure attachment characteristics could impair the therapeutic alliance with a single therapist (e.g., Goldman & Anderson, 2007; Reis & Grenyer, 2004) and could limit the cohesion within group psychotherapy (e.g., Kirchmann et al., 2009; Mallinckrodt & Chen, 2004). However, results with regard to the relationship between attachment characteristics and therapy outcome have so far been inconsistent. Although most of the studies showed that more securely attached patients benefited more from a psychotherapy (e.g., Meredith, Strong, & Feeney, 2007; Meyer & Pilkonis, 2001), others indicated a better outcome for avoidant patients (Fonagy et al., 1996) and ambivalent subjects (Sachse & Strauss, 2002). There are also studies that did not find any significant differences (Cryanowski et al., 2002; Strauss et al., 2006).

Another issue of clinical attachment research relates to the association between insecure attachment and psychopathology. Several studies have consistently found cross-sectional associations between insecure attachment and various psychiatric diagnoses (e.g., Bakermans-Kranenburg & van Ijzendoorn, 2009) or symptom ratings (Riggs et al., 2007). Longitudinal associations have also been

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found between insecure attachment and various variables from developmental psychopathology models such as decreased affect regulation capabilities (e.g., Bosquet & Egeland, 2006), social skills (Belsky & Fearon, 2002), negative self-evaluations (Easterbrooks & Abeles, 2000), or reduced problem-solving competencies (Schieche & Spangler, 2005).

Although insecure attachment characteristics are not equivalent to psychopathological symptoms (about 40–50% of the individuals in non-clinical samples fall into insecure attachment categories; e.g., Bakermans-Kranenburg & van Ijzendoorn, 2009; Höger, Stoebel-Richter, & Braehler, 2008), insecure attachment may be an important risk factor for the development of different psychopathological problems. Along this line, more recent studies have considered changes in attachment characteristics (i.e., an increase in attachment security) to be a criterion of a positive psychotherapeutic outcome.

Researchers examining the changes in attachment characteristics assume that individual attachment patterns *can* change – i.e., they are created based on interpersonal childhood experiences but can be altered later in life. These changes are possible by either making alternative/corrective interpersonal experiences or by reframing one's own attachment history—both of which are possible focus points of psychotherapy.

It has not yet been empirically well established to what extent individual attachment patterns can be modified. This might be because different concepts and research traditions exist in the field of adult attachment research (e.g., Steele, 2002). One tradition, rooted in developmental psychology, focuses on memory representations and the inner working models (expectations) of attachment and bases their data on interviews. Another tradition, rooted in social/personality psychology, uses predominantly self-reports in order to measure emotional experiences and behavior in attachment relationships. The empirical convergences between measures of these two traditions are in general weak and often statistically insignificant. Studies which simultaneously used attachment measures from both traditions have often revealed varied findings depending on the measure (e.g., Roisman et al., 2007; Strauss et al., 2006).

The literature on attachment characteristics' changes contains three lines of research: (a) longitudinal panel studies; (b) studies examining the impact of transitions and critical life events on attachment; and (c) intervention studies.

So far, there have been at least six longitudinal panel studies which repeatedly measured attachment characteristics from infancy to early adulthood (for an overview see Grossmann, Grossmann, & Waters, 2005). Only two of these studies were able to find statistically significant convergences between infant and adult attachment characteristics (Hamilton, 2000; Waters, Merrick, Treboux, Crowell, & Albersheim, 2000), whereas the remaining studies did not find any significant associations (Grossmann & Grossmann, 2004; Lewis, Feiring, & Rosenthal, 2000; Weinfield, Sroufe, & Egeland, 2000). However, critical life events could be consistently identified (e.g., loss of a parent; separation/divorce of the parents; severe somatic or psychological disease of a parent) that were predictive of attachment changes.

In these studies, which focused on the changes in attachment characteristics with regard to transitions or critical life events, attachment characteristics were usually assessed at two different occasions (with a time span between 5 months and 24 months) around the life circumstances that were thought to challenge the existing attachment patterns. Such life circumstances were: the transition into early adulthood (16–18 years of life; Allen, McElhaney, Kuperminc, & Jodl, 2004; Zimmermann & Becker-Stoll, 2002), the beginning or the end of a university education (Lopez & Gormley, 2002; Scharfe & Cole, 2006), marriage (Crowell, Treboux, & Waters, 2002; Davila, Karney, & Bredbury, 1999), the birth of the first child (Simpson, Rholes, Campbell, & Wilson, 2003), separation from a partner (Ruvolo, Fabin, & Ruvolo, 2001), or an abortion (Cozzarelli, Karafa, & Collins, 2003). Test-retest convergences in these studies were consistently lower than cross-sectional reliability estimates like inter-rater convergences or internal consistency coefficients, which suggests that true attachment changes occurred. However, the lack of control groups in these studies means that the role of serial effects is ambiguous and that the changes in attachment cannot be clearly attributed to a specific transition or critical life event.

These limitations could be ruled out by intervention studies if a control group design was used. By including an untreated group in the research design there would be a control of historical and test-retest effects. Furthermore, if more than one well-defined treatment condition was included, then factors causing the changes in attachment could be identified. Clinical intervention studies of attachment change are therefore important not only for the clinical or practical issues (i.e., improvement of interventions, e.g., psychotherapy), but also for the general research of attachment (understanding the causal mechanisms of attachment change processes).

Fonagy et al. (1995) were the first to publish data regarding the changes in attachment characteristics in patients who were undergoing psychodynamic

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therapy. They found at pretreatment that none of the 35 borderline patients, who were assessed with the Adult Attachment Interview (AAI), belonged to the secure attachment category. However, at post-treatment 40% of the patients were categorized as securely attached.

Travis, Binder, Bliwise and Horne-Moyer (2001) used the Bartholomew Attachment Rating on 84 participants in outpatient psychotherapy with different diagnoses. There was a subsample of 29 patients for whom posttreatment assessments were available after time-limited psychodynamic therapy (with a mean of 21 sessions). Within this group they found dimensional pre–post increases in attachment security and decreases in several insecure dimensions. At the beginning of the treatment none of the patients was categorized as securely attached, and by post-treatment seven fell into this category.

Lawson, Barnes, Madkins, and Francois-Lamonte (2006) collected pre post self-ratings of attachment using the Adult Attachment Scale (AAS) from 33 males being treated for domestic violence. The treatment was an integrated cognitive-behavioral/psychodynamic outpatient group therapy which lasted 17 weeks. On a categorical level, the authors found increases in the number of securely attached participants (14 versus 25); however, the means of the attachment dimensions did not show any significant changes.

All results mentioned so far must be interpreted cautiously because of the small sample sizes and the lack of comparison groups.

Levy et al. (2006) were the first who investigated the changes in attachment as a result of psychotherapy using a comparison group design. They collected Adult Attachment Interviews from outpatients with Borderline Personality Disorder before and after treatment. Initially 90 patients were randomly assigned to one of three treatment conditions each of which lasted 1 year. They found (based on data available from 60 patients at post-treatment) that there was a statistically significant increase in the number of securely attached participants only in the transference-focused psychotherapy group (1 pre- versus 7 posttreatment), but not in the dialectical behavioral or psychodynamic supportive therapy groups.

Tasca, Balfour, Ritchie, and Bissada (2007) analyzed pre–post attachment changes among inpatients with a binge eating disorder. They randomly assigned 95 patients to one of the treatment groups: cognitive-behavioral or psychodynamic-interpersonal therapy. Their results, which were based on pre–post attachment self-report data (Attachment Style Questionnaire) from 66 patients, indicated a decrease in attachment insecurity in both treatment groups.

Muller and Rosenkranz (2009) used a waitlist control group. They collected attachment self-reports (Relationship Scales Questionnaire, RSQ) from inpatients with Posttraumatic Stress Disorder undergoing the Program for Traumatic Stress Recovery. Data were available for $n = 101$ (pre post) and $n = 61$ patients (6-month follow-up). Individuals in the waitlist control group ($n = 46$) also filled out the RSQ at pre- and posttreatment. The authors' findings were inconsistent. Contrasted to the controls, the patients showed pre post improvements on only two of the six attachment scales. Within-group statistics indicated that the attachment improvements had disappeared by follow-up; however, there weren't any data from the control group available for follow-up. The results were also difficult to interpret because of the lack of randomization and because covariates were not taken into account, despite the existence of pretreatment differences between the patients and the controls.

Altogether, the existing studies suggest that attachment characteristics may be improved through different psychotherapeutic interventions in different diagnostic groups independent of the attachment measure used. However, attachment changes were predominantly investigated using diagnostically specific samples that may not be very common in psychotherapeutic routine care. Moreover, long-term attachment improvements have not yet been adequately examined.

The goal of the present study was to investigate whether inpatients undergoing group psychotherapy in routine care show changes in attachment characteristics. More specifically, we expected that the patients' attachment (increase in security, decrease in anxiety and avoidance) would improve in comparison to the untreated controls with regard to both pre–post changes and changes from pretreatment to a 1-year follow-up.

Method

Participants

We designed this study as a naturalistic observational study including a non-randomized control group. A total of $N = 265$ psychotherapy inpatients were recruited from five psychotherapeutic institutions in Germany. Two of the psychotherapy wards were from the University Hospital in Jena; one ward was psychodynamically oriented ($n = 38$) and the other ward uses CBT ($n = 35$). The other three psychodynamically oriented hospitals were from different cities: Rhein-Clinic, Bad Honnef ($n = 45$);

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Asklepios Hospital, Stadtroda ($n = 97$); Department of Psychotherapy and Psychosomatic Medicine, University Hospital Dresden ($n = 50$). The control group subjects ($N = 260$) were patients from a general practice in Weimar ($n = 112$) as well as psychology undergraduate students ($n = 47$) and medical students ($n = 101$) from the Friedrich Schiller University in Jena. Initially we tried to use a control group that was exclusively made up of physically ill patients but due to the limited attendance we decided to also include students in the control group.

This study was part of a research project which examined changes in attachment and depression. All participants received an invitation to participate in "a study of the changes of depressive complaints" and all provided informed consent as well as their mailing address. This study was approved by the local ethics committee.

In the cooperating hospitals, all admitted patients were asked to participate in the study. The inpatient sample, therefore, consisted of patients with different primary diagnoses (comorbidity was not considered in this study), which were made on the basis of clinical judgment (unipolar affective disorders 30%, anxiety disorders 28%, eating disorders 21%, personality disorders 12%, somatoform disorders 5%, others 4%). The general practitioner in Weimar asked all patients older than 18 years to participate. As compensation for their participation, patients received feedback information about their depression scores and three patients received €100 as part of a random drawing. The same was offered to the medical students, whereas participating psychology students only received course credits for their participation.

Data were collected at three different occasions: pretreatment, posttreatment, and 1-year follow-up. The patients filled out questionnaires at the beginning of their treatment, at the end (mean treatment duration: $Md = 9$ weeks; range: 3–18 weeks), and a year after finishing treatment (via mail). The questionnaires were sent to the control group individuals at identical intervals. At posttreatment, data were available from $n = 188$ patients (drop-out due to treatment attrition) and $n = 219$ controls. At follow-up, data were available from $n = 152$ patients and $n = 200$ controls.

Measures

For the assessment of adult attachment characteristics we used three different multi-item questionnaires.

Bielefeld Partnership Expectations Questionnaire (BFPE). The BFPE (Höger et al., 2008; Pollak, Wiegand-Grefe, & Höger, 2008) consists of 30 items (plus one warm-up item) that are rated on a 5-point Likert scale. The items of the BFPE evaluate the participants' expectations regarding an intimate partnership. The items are assigned to three subscales, *Fear of Rejection* (10 items, e.g., "I am afraid that my partner will withdraw from me if he/she knows the truth about my inner feelings"), *Readiness for Self-Disclosure* (12 items, e.g., "It is generally easy for me to talk to my partner about my innermost feelings"), and *Conscious Need for Care* (7 items, e.g., "When I feel neglected by my partner, I can become very depressed").

For the assignment to attachment categories, the participants' scale values can be compared to the values of the secure, ambivalent, and avoidant clusters of a large reference sample ($N = 1506$; Höger et al., 2008). This therefore allows for an empirically based assignment to one of the classical attachment categories. Cronbach's alphas for the three scales ranged in our study from $\alpha = .77$ to $\alpha = .89$.

Evidence for convergent validity (associations with other attachment questionnaires) and theoretically expected associations with other concepts (such as coping or relationship quality) has been comprehensively presented by Pollak et al. (2008). The BFPE is especially suitable for a categorical assessment of attachment and we used it accordingly.

Grau's Attachment Questionnaire (GAQ). The GAQ was developed by Grau (1999). It also assesses attachment characteristics in intimate partnerships. The GAQ consists of 20 items which are rated on a 5-point Likert scale. The items are assigned to two subscales, *GAQ-Anxiety* (10 items, e.g., "I often worry that my partner could not like me enough") and *GAQ-Avoidance* (10 items, e.g., "If my partner gets too close to me, I withdraw"). The GAQ corresponds to the results of studies using exploratory factor analysis, which were based on a larger number of attachment questionnaires. In these studies orthogonal two-dimensional solutions were consistently found indicating the dimensions anxiety and avoidance (Brennan, Clark, & Shaver, 1998; Fraley, Waller, & Brennan, 2000; Stein et al., 2002). Convergent validity of the GAQ was confirmed by high associations with other attachment questionnaires, for example the BFPE (Grau, Clashausen, & Höger, 2003) and the German version of the Experiences in Close Relationships Scale (ECR; Neumann, Rohmann, & Bierhoff, 2007). Furthermore, hypothesized associations of the GAQ scales with partner relationship quality/satisfaction and

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feelings of equity were validated (Grau & Doll, 2003). The reliability estimates were high in our study with internal consistencies of $\alpha = .92$ (*GAQ-Anxiety*) and $\alpha = .88$ (*GAQ-Avoidance*). By computing (*GAQ-Anxiety* + *GAQ-Avoidance*) (-1), we provided a continuous measure of *GAQ-Security*.

Relationship Scales Questionnaire (RSQ). The RSQ (Griffin & Bartholomew, 1994; German version from Steffanowski et al., 2001) consists of 30 items (5-point Likert scale) which assess attachment-related feelings, expectations, and motivations, not specifically for romantic partners, but for more general relationships. Originally, the items were assigned to four highly intercorrelated subscales with a fairly low reliability (Griffin & Bartholomew, 1994). However, Kurdek (2002) found the best model was a two-dimensional solution (anxiety and avoidance) which was based on a subset of only 13 of the 30 items. For our present study we administered the German version of the RSQ and used a two-dimensional factor solution with the dimensions *Anxiety* and *Avoidance* resulting from a preceding German study (Kirchmann, Fenner, & Strauss, 2007). This factor solution, which we used in the current study, consists of six items that were assigned to the dimension *RSQ-Anxiety* (e.g., "I worry that others don't value me as much as I value them") and 12 items that were assigned to *RSQ-Avoidance* (e.g., "I am somewhat uncomfortable being close to others"). The correlation of *RSQ-Anxiety* with *ECR-Anxiety* was $r = .62$, the correlation of the respective avoidance scales was $r = .71$. In our study, Cronbach's alpha for anxiety was $\alpha = .86$ and for avoidance $\alpha = .82$. We provided a continuous measure *RSQ-Security* by computing (*RSQ-Anxiety* + *RSQ-Avoidance*) (-1).

Additionally, at the beginning of treatment, participants were asked to complete the Center for Epidemiological Studies Depression Scale (CES-D: Radloff, 1977; German version: Hautzinger & Bailer, 1993) which is a commonly used 20 item Likert-type questionnaire. The CES-D is a depression self-report with high internal consistency ($\alpha = .91$ in our study) and high correlations with other depression measures such as the Beck Depression Inventory and the Hamilton Depression Scale (Hautzinger & Bailer, 2002).

Furthermore, participants answered socio-demographic questions (age, gender, education, marital status, employment status, partner relationship).

Statistical procedures

Only in the randomized controlled trial can one expect that in a treatment and a control group pretreatment characteristics show similar distribution statistics (frequencies, means, variances, etc.) and, therefore, that group differences in the outcome reflect unbiased treatment effects. In quasi-experimental studies confounding variables (variables related to both treatment assignment and outcome) may bias the estimates of treatment effects. Therefore it is crucial to measure and statistically control such confounders (e.g., Rosenbaum, 2002; Shadish, Cook, & Campbell, 2002; note that we call a measured confounder a *covariate*). In our present study, we used propensity score techniques for covariate modeling in combination with a generalized analysis of covariance (g-ANCOVA) provided by the statistics program *EffectLite 3.1.2*. For bivariate statistics and the computation of propensity scores we used *PASW 18.0*.

Propensity score modeling. Propensity scores (*PS*) are the conditional probabilities of a participant being in a treatment condition given a vector of covariates (for an overview see Guo & Fraser, 2010). Usually, these conditional probabilities are estimated by logistic regression or discriminant analysis. Numerous covariates can be taken into account simultaneously, yielding one single propensity score for each participant, provided that there are only two treatment conditions. A group of treated participants with the same *PS* as a group of controls also show similar distribution statistics of all the included covariates used for computing the propensity scores (Rosenbaum & Rubin, 1983). Hence, by accurately constructed *PS* a multitude of covariates (potentially all confounders) can be balanced simultaneously using only the *PS* in a statistical model. If there are differences in the outcome between treated participants and untreated controls with the same *PS*, these outcome differences can be attributed to the treatment and not to the covariates and, therefore, reflect the treatment effect.

A crucial and controversial issue in *PS* analysis is which pretreatment characteristics should be selected for *PS* computation. In the empirical literature, there are very different inclusion strategies (e.g., see the review by Weitzen, Lapane, Toledano, Hume, & Mor, 2004). There are two criteria for a proper *PS* model: the estimates should be unbiased and they should be efficient. For an unbiased estimation the inclusion of all potential confounders would be advantageous. On the other hand, the inclusion of unimportant covariates is detrimental to efficiency and, therefore, parsimonious models with only a few covariates would be preferable. In order to increase efficiency, Brookhart et al. (2006) recommended that the covariate selection for the *PS* computation should be based on the associations

between pretreatment variables and the outcome even in cases in which these pretreatment variables are not associated with the treatment variable. The authors used simulation studies to show that variables that are related to treatment but not to the outcome increase the variance of the estimated treatment effect (adding noise to the estimated *PS*) without decreasing bias and should therefore be omitted. Furthermore, in another simulation study, Austin, Grootendorst, and Anderson (2007) showed that the inclusion of pretreatment characteristics that are only related to the treatment variable and not to the outcome did not reduce bias. Instead, the inclusion of such pretreatment variables reduced the number of pairs (treatment case/control group case) that could be matched with respect to the *PS*.

We did not want to carry out different *PS* computation models for the different attachment outcome variables considered in this study (attachment post and follow-up scores). We therefore decided to select all those pretreatment variables (raw-scales, interaction and quadratic terms) for the *PS* computation which showed a statistically significant association with at least one outcome variable (R^2 difference of $p < .05$ in a series of linear regression models including pretreatment variables as predictors and each attachment outcome as a dependent variable; categorical pretreatment characteristics were dummy coded in these analyses). It is well-established that bias is reduced only by those covariates in a propensity model that are associated with outcome *and* the treatment variable (e.g., Shadish et al., 2002). Hence, in order to have a manageable number of covariates, we included only those covariates for the *PS* computation that were also associated with the treatment variable at least by trend (Nagelkerke's R^2 of $p < .10$ in logistic regression analysis including the treatment variable as a dependent variable).

In more detail, our covariate selection strategy comprised four steps: In step one we included all pretreatment raw-scales (see Table I) which showed a statistically significant bivariate association with at least one outcome as independent variables in a *PS* computation model (discriminant analyses with the treatment variable as a group variable).

In step two, we combined each of the depression-related interaction terms (e.g., depression × *GAQ-Anxiety*; depression × gender) with the logit-transformed *PS* obtained from step one as independent variables in a series of linear regression analyses in which the attachment outcomes were included as dependent variables. (We started the successive inclusion of interaction terms with depression-related interactions, because we viewed depression as a specifically relevant psychopathological

Table I. Pretreatment attachment and socio-demographic characteristics with regard to the patient ($n = 265$) and control group ($n = 260$)

Pretreatment characteristics		Patient group	Control group	Test statistics
GAQ-Anxiety	*M* (*SD*)	1.68 (1.01)	.94 (.81)	$F(1, 513) = 82.81$, $p < .001$
GAQ-Avoidance	*M* (*SD*)	1.27 (.86)	.89 (.67)	$F(1, 513) = 31.44$, $p < .001$
RSQ-Anxiety	*M* (*SD*)	2.15 (.94)	1.35 (.88)	$F(1, 506) = 97.27$, $p < .001$
RSQ-Avoidance	*M* (*SD*)	2.05 (.72)	1.67 (.55)	$F(1, 506) = 44.43$, $p < .001$
Depression score	*M* (*SD*)	1.55 (.58)	.70 (.46)	$F(1, 523) = 342.68$, $p < .001$
Age	*M* (*SD*)	32.40 (10.59)	26.92 (10.44)	$F(1, 522) = 35.67$, $P < .001$
Gender (female)		72.1%	74.9%	$\chi^2 = .537$; $df = 1$; $p = .463$
Education				
Still at school/lowest level		15.6%	10.0%	
Secondary school		45.6%	23.9%	$\chi^2 = 65.35$; $df = 3$; $p < .001$
High-school diploma		25.1%	59.8%	
University degree		13.7%	6.2%	
Marital status				
Unmarried		64.5%	75.4%	$\chi^2 = 9.99$; $df = 2$; $p = .007$
Married		23.4%	19.2%	
Divorced/widowed		12.1%	5.4%	
Employment status				
Student		26.6%	66.9%	$\chi^2 = 91.12$; $df = 3$; $p < .001$
Employed		33.8%	22.0%	
Unemployed		30.0%	6.3%	
Retired/otherwise unemployed		9.7%	4.7%	
Partner relationship				
No relationship (short-term)		14,3%	15,2%	$\chi^2 = 3.57$; $df = 3$; $p = .312$
No relationship (long term)		24.5%	18.4%	
Changing partners		4.5%	3.5%	
Long term partner relationship		56.6%	62.9%	

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characteristic for self-selection to inpatient psychotherapy.) All significant interaction terms which were also associated with the treatment assignment ($p < .10$) were then combined together with the logit-*PS* from step one as independent variables in a further discriminant analysis including the treatment variable as a group variable. By this step we yielded a new *PS*.

In step three we reran step two, but this time we replaced the depression-related interaction terms with the attachment-related interaction terms and used the logit-*PS* resulting from step two instead of those from step one.

In step four we applied the same procedure to the quadratic terms but could not find any significant associations among the attachment outcomes. We therefore based our final *PS* estimates on step three.

The highest Pearson correlations, respectively, the highest multiple correlations between the final *PS* and the pretreatment characteristics were found with regard to depression ($r = .873$), employment status ($R = .578$), *RSQ-Anxiety* ($r = .564$), and *GAQ-Anxiety* ($r = .504$).

PS estimates can be used for different data balancing strategies (e.g., pair-matching, stratification, weighting procedures, or as a continuous covariate). Each of these strategies has advantages and limitations depending on the sample characteristics and the theoretical assumptions. The *PS* differed considerably between the treatment and the control group in our study and we assumed that there could be non-linear dependencies between the propensity scores and the outcome variables. We therefore decided to follow a double robustness strategy and used *PS* in two different ways for covariate modeling: first in the form of discrete *PS* strata (stratification) and secondly as a continuous covariate.

In the creation of the stratification we followed the recommendations of Rosenbaum and Rubin (1984) and others (Bartak et al., 2009; Luellen, Shadish, & Clark, 2005) and divided the sample into five groups. First, we inspected the range of the *PS* in the patient group ($PS = .033$ to $PS = .999$) and the control group ($PS = .001$ to $PS = .978$). We then excluded all cases ($n = 56$ patients; $n = 83$ controls) outside the overlap. Using the quintiles of the *PS* distribution, we then created five subclasses (strata, *St*) with approximately the same number of cases. Each stratum thus included treatment and control group participants with similar *PS* (*St1*: range from $PS = .033$ to $PS = .140$; *St2*: $PS = .145$ to $PS = .412$; *St3*: $PS = .414$ to $PS = .774$; *St4*: $PS = .776$ to $PS = .900$; *St5*: $PS = .901$ to $PS = .978$).

For the continuous covariate modeling, we logit-transformed the *PS* in order to obtain normally distributed values. The logit-*PS* variable was then used as a covariate in g-ANCOVA. By using this procedure we were able to include the complete data set.

Figure 1 shows the frequencies of patients and controls in the five *PS* strata as well as in 10 intervals of logit-*PS*, each with the same length. With respect to logit-*PS* there was almost no overlap between patients and controls within the three lowest and the three highest logit-*PS* intervals. In contrast, each *PS* stratum comprised at least 10 participants of the control as well as the patient group. To achieve a minimum of 10 controls/patients per stratum, we expanded *St1* and *St5*, which led to slightly different frequencies in each of the strata.

Figure 2 presents *p* values indicating the associations between the treatment variable and the

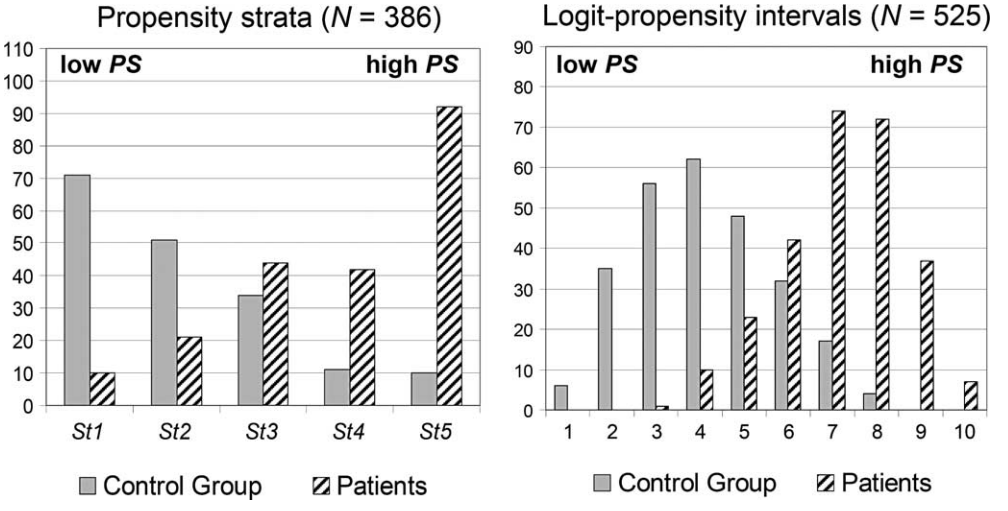

Figure 1. Frequencies of patient and control group participants based on propensity score intervals (five propensity strata and 10 same-sized logit-transformed propensity score intervals).

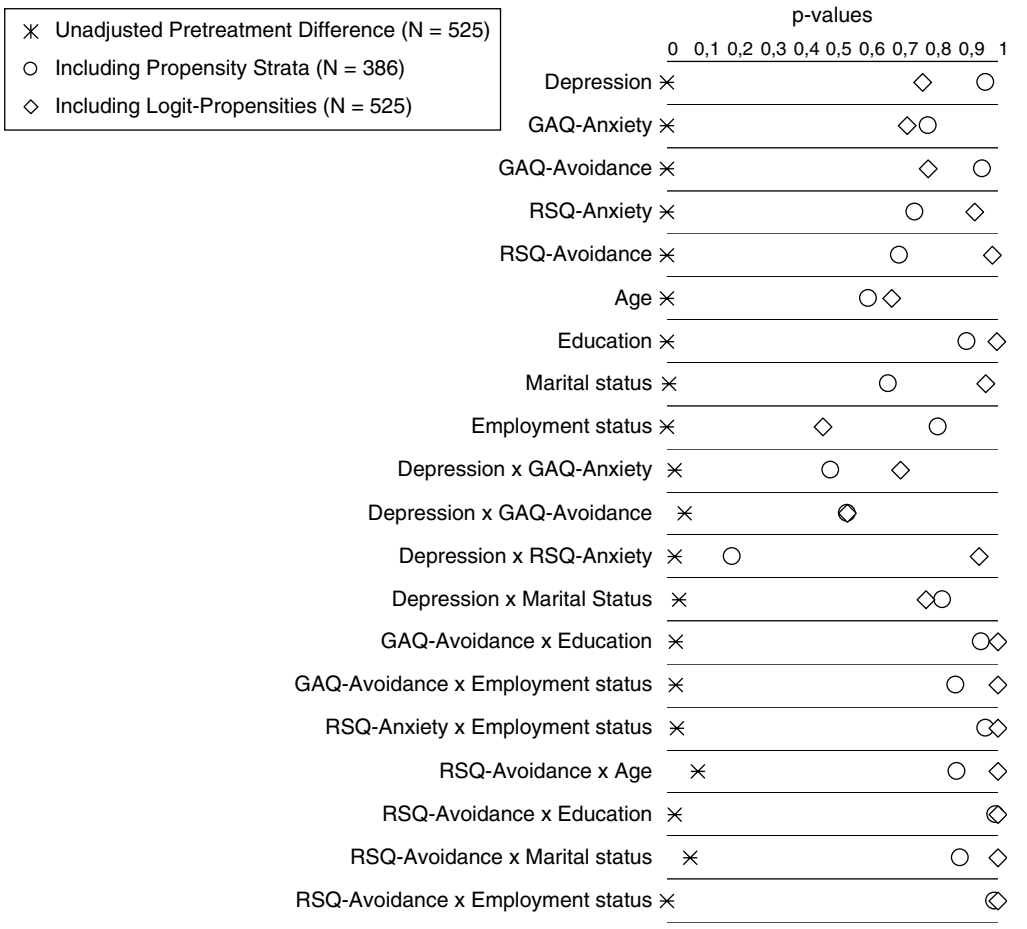

Figure 2. Associations between the treatment variable and covariates that were selected for propensity computation before and after propensity adjustment (after statistical control of propensity strata respectively logit-propensities).

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covariates that were selected for *PS* computation before and after balancing on the basis of *PS* strata and logit-*PS*. In these analyses each covariate was used as a dependent variable either in a multiple linear regression (continuous covariate) or in a multinomial logistic regression (categorical covariate). Unbalanced associations were computed including the treatment variable as a single independent variable. In contrast, the p values with regard to *PS* strata and logit-*PS* balancing resulted from regression analyses in which not only the treatment variable was used as an independent variable but also the PS strata or the logit-PS respectively were included as independent variables.

As can be seen below, when *PS* strata or logit-*PS* were additionally included, there were no longer any significant associations between the treatment variable and the covariates. Both *PS* techniques adequately balanced the observed pretreatment differences.

Generalized ANCOVA with EffectLite 3.1.2. *EffectLite* (Steyer & Partchev, 2008) is a software application that uses the structural equation programs *LISREL* or *Mplus* for the computation of a generalized analysis of covariance (g-ANCOVA). In traditional ANCOVA a covariate (Z) or a vector of covariates ($\boldsymbol{Z}$) is taken into consideration by estimating the conditional mean difference of an outcome variable between the treatment groups given the values of the covariates (instead of simply analyzing the mean pre–post differences between the groups or the mean post differences as done in ANOVA with or without repeated measurements). Note that in traditional ANCOVA it is assumed that the conditional treatment effects are the same for all values of the covariate. G-ANCOVA provided by *EffectLite* has several advantages over traditional ANCOVA: (i) Covariate-treatment interactions are modeled, i.e., depending on the values of the covariates, treatment effects can be larger or smaller. (ii) Treatment effects can be estimated not only on the basis of the total sample but also on the basis of subsamples consisting of specific treatment categories. (iii) Covariates as well as dependent variables can be modeled as latent variables. (iv) Homogeneity of variances between

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groups is not necessary for the validity of the statistics.

In *EffectLite* treatment effects are estimated by contrasting one or several treatment conditions to a reference condition (such as an untreated control). In a simple case with a single covariate Z and a dichotomous treatment variable X (control: $X=0$; treatment: $X=1$) the regression $E(Y|X,Z)=g_0(Z)+g_1(Z)\cdot X$ is considered. In this equation, $g_0(Z)$ is the intercept function and $g_1(Z)$ is the slope or effect function. The values of $g_1(z)$ are the effects of the treatment (compared to the control) on the outcome.

In *EffectLite*, both the intercept and the effect function are parameterized as linear functions of the considered covariates. Hence, if there is only a single covariate, then $g_0(Z)=\gamma_{00}+\gamma_{01}Z$ and $g_1(Z)=\gamma_{10}+\gamma_{11}Z$. Inserting these two functions into the formula for the regression $E(Y|X,Z)$ yields: $E(Y|X,Z)=(\gamma_{00}+\gamma_{01}Z)+(\gamma_{10}+\gamma_{11}Z)\cdot X$.

The meaning of the parameters in the effect function is straightforward: $\gamma_{10}$is the mean difference of the outcome variable between the control and the treatment group if $Z=0$ (provided that this value exists; otherwise γ_{10} is simply the intercept in the effect function $g_1(Z)$); γ_{11} is the difference in the conditional treatment effects comparing two values of the covariate (Z) that differ by one unit. Hence γ_{11} quantifies the covariate-treatment interaction. Finally, the expectation $E[g_1(Z)]=E(\gamma_{10}+\gamma_{11}Z)=\gamma_{10}+\gamma_{11}E(Z)$ is the average treatment effect over the distribution of Z.

In *EffectLite* it is possible not only to estimate the average treatment effect on the total sample (i.e., $E[g_1(Z)]$), but also to estimate the conditional treatment effects given specific treatment conditions. In a simple case with a dichotomous treatment variable X (control: $X=0$; treatment: $X=1$) and a single covariate Z, the conditional treatment effect on the treated $E[g_1(Z)|X=1]=\gamma_{10}+\gamma_{11}E(Z\,|X=1)$ is estimated by inserting the mean of the covariate in the subsample of the treated participants into the estimated effect function. Hence, $E[g_1(Z)|X=1]$ is the average treatment effect that would be estimated if everyone in the treated group received the treatment versus if no one in the treated group received the treatment. Accordingly, by $E[g_1(Z)|X=0]$ we estimate the average treatment effect on the controls (i.e., if everyone in the control group received the treatment versus if no one in the control group received the treatment).

Note that in a randomized experiment the three treatment effects, $E[g_1(Z)]$, $E[g_1(Z)|X=1]$, $E[g_1(Z)|X=0]$ are identical, because randomization implies that the theoretical distributions (not the sample distributions) of the covariates are identical between the treatment and the control group. In a quasi-experiment, however, these distributions may (and usually do) differ and we must decide which of those treatment effects is of interest. Since we were interested in the treatment effects on patients under naturalistic conditions, and in our study the distributions of many pretreatment variables varied considerably between the patients and the control group, we focused on the average treatment effects on the treated, $E[g_1(Z)|X=1]$.

As mentioned before, we included the *PS* in the framework of g-ANCOVA in two different ways. (1) The five *PS* strata were included via covariates as dummy variables representing strata membership. In this procedure *St3* (participants of the mean *PS* quintile) was chosen as a reference, omitting the respective dummy variable. (2) Logit-transformed *PS* were included in g-ANCOVA as a single covariate.

Results

Table I shows the pretreatment characteristics of the patient and the control group. The mean values and standard deviations of the patients′ attachment characteristics as well as their depression scores were considerably higher than those of the control group. This indicates that there was not only higher attachment insecurity and depression but also higher variability in the patient group than in the control group. There were also significant group differences with respect to several socio-demographic characteristics (age, education, marital status, employment status). In contrast, no differences were found with respect to gender and partner relationship.

Categorical changes

Figure 3 shows the frequencies of the attachment categories assessed by the BFPE. In the control group there were only marginal changes in the frequencies from pre- to posttreatment ($\chi^2(2,n=525)=1.39$, $p=.499$) and from posttreatment to follow-up ($\chi^2(2,n=525)=.77$, $p=.680$). The results from the Bowker tests of internal symmetry were statistically insignificant, changes from one category to another (e.g., from secure to ambivalent category) did not occur more often than changes in the opposite direction. This was true for both comparisons, pre- to posttreatment and posttreatment to follow-up (Bowker test: $\chi^2(3,n=525=2.36$, $p=.501$, respectively $\chi^2(3,n=525)=.86$, $p=.835$).

In the patient group, the frequencies of the attachment categories fluctuated between pre- and post-treatment ($\chi^2(2,n=525)=47.75$, $p<.001$). As Figure 3 shows, there was a pre–post increase in the frequency of the secure category and a decrease in

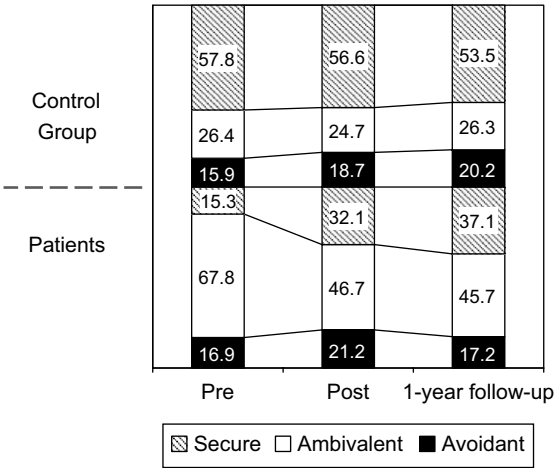

Figure 3. Attachment categories changes (%, BFPE).

the ambivalent category; accordingly symmetry of changes could not be held (Bowker test: $\chi^2(3,n=525)=28.71$, $p<.001$). In contrast, frequencies of the attachment categories did not change from posttreatment to follow-up in the patient sample ($\chi^2(2,n=525)=2.35$, $p=.309$), correspondingly we did not find significant differences with regard to the symmetry of changes (Bowker test: $\chi^2(3,n=525)=2.07$, $p=.557$).

Dimensional changes

Changes in the attachment characteristics on a dimensional level (means) are shown in Figure 4. The means of the patients were higher than those of the control group at all three measurement occasions. However, there were considerable pre–post decreases of attachment anxiety in the patient group in both the GAQ and RSQ, whereas decreases in avoidance were larger in the RSQ.

The raw-scale means and standard deviations for pretreatment attachment anxiety and avoidance variables are presented in Table I. For the patients, the post-treatment raw-scale means were: *GAQ-Anxiety* $M=1.22$ ($SD=1.02$), *GAQ-Avoidance* $M=1.12$ ($SD=.76$), *RSQ-Anxiety* $M=1.61$ ($SD=1.03$), *RSQ-Avoidance* $M=1.83$ ($SD=.68$); and at follow-up: *GAQ-Anxiety* $M=1.04$ ($SD=1.17$), *GAQ-Avoidance* $M=1.22$ ($SD=.81$), *RSQ-Anxiety* $M=1.50$ ($SD=1.09$), *RSQ-Avoidance* $M=1.86$ ($SD=.64$). For the control group the post-treatment raw-scale means were: *GAQ-Anxiety* $M=.92$ ($SD=1.00$), *GAQ-Avoidance* $M=.87$ ($SD=.68$), *RSQ-Anxiety* $M=1.24$ ($SD=.98$), *RSQ-Avoidance* $M=1.65$ ($SD=.61$); and at follow-up: *GAQ-Anxiety* $M=.91$ ($SD=.96$), *GAQ-Avoidance* $M=.94$ ($SD=.74$), *RSQ-Anxiety* $M=1.25$ ($SD=.95$), *RSQ-Avoidance* $M=1.66$ ($SD=.56$).

We calculated *reliable change indices* (RCI; according to Jacobson & Truax, 1991) and assigned participants to RCI categories in order to analyze dimensional attachment changes on an individual level. Three RCI categories were used for both pre – post and pre – follow-up changes: *deteriorated* ($RCI > |1.96|$ in direction of insecure attachment), *unimproved* ($-1.96 < RCI < +1.96$), and *improved* ($RCI > |1.96|$ in direction of secure attachment).

Table II shows that only a few patients were classified as deteriorated (about 3–8%) with respect to pre–post changes. Most of the patients (about 60–90%) were categorized as unimproved with regard to each attachment variable. There were especially high rates of unimproved patients for attachment

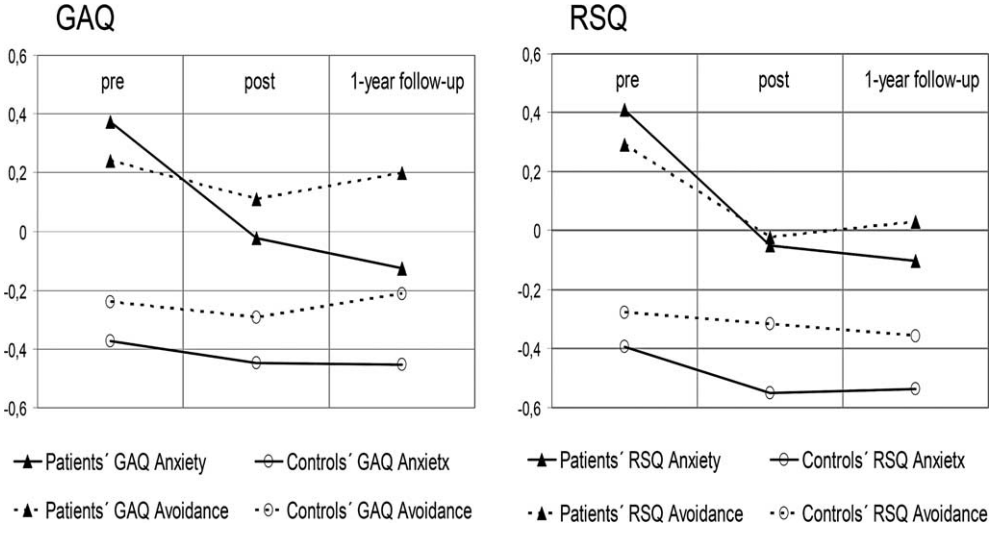

Figure 4. Changes of attachment scales (GAQ and RSQ means); for an easier comparison, all values were transformed in accordance with z-transformation by subtracting the respective total sample pretreatment mean and dividing the difference by the pretreatment standard deviation.

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Table II. Percentages of RCI categories with regard to pre – post and pre – follow-up changes in attachment characteristics for the patient and control group (in brackets)

	Pre–post						Pre–1-year follow-up					
Attachment scales	Deteriorated		Unimproved		Improved		Deteriorated		Unimproved		Improved	
GAQ-												
Anxiety	5.6	(4.6)	69.4	(88.0)	25.0	(7.4)	8.5	(7.0)	58.6	(82.0)	32.9	(11.0)
Avoidance	5.6	(1.8)	87.2	(95.4)	7.2	(2.8)	8.1	(6.1)	80.3	(90.3)	11.6	(3.6)
Security [a]	7.9	(7.8)	58.9	(81.6)	31.2	(10.6)	14.3	(15.4)	46.3	(67.2)	39.5	(17.4)
RSQ-												
Anxiety	2.9	(1.8)	75.1	(90.9)	22.0	(7.3)	4.4	(4.5)	67.1	(86.5)	28.5	(9.0)
Avoidance	3.5	(2.3)	89.6	(96.8)	6.9	(0.9)	4.4	(2.0)	84.7	(95.0)	10.9	(3.0)
Security [a]	4.0	(4.6)	64.2	(84.0)	31.8	(11.4)	8.0	(6.5)	56.2	(82.0)	35.8	(11.5)

Note. Sample sizes for pre–post RCI categories were: $n = 180$ (patients) and $n = 217$ (controls); for pre–follow-up RCI categories: $n = 152$ (patients) and $n = 200$ (controls).

[a]Attachment security variables were computed by $(Anxiety + Avoidance) \cdot (-1)$.

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avoidance (about 90% unimproved). However, there was a relatively high proportion of improved patients with regard to attachment anxiety (about 25% improved) in contrast to attachment avoidance (about 7% improved).

The pattern was similar with regard to pre–follow-up changes although the proportion of unimproved patients was generally lower (50–85%) and the frequency of deteriorated and improved patients was higher.

The frequencies of the RCI categories for the control group were similar to the patients only in that there was also a very small proportion of participants classified as deteriorated (maximum difference between patients and controls: 3.8%). The proportion of unimproved controls was considerably higher than that of the patients (about 20% higher with regard to attachment anxiety, 10% to avoidance). Accordingly, the control group had a lower proportion of improved participants than the patient group (difference of percentages with respect to anxiety about 15–20%, avoidance 5–10%).

So far, the differences in attachment changes (categories or means) between the patient and the control group have not yet been adjusted for covariates. For example, the differences in attachment changes between the groups may be due to the regression to the mean (attachment pre-scores) or to depression. Hence, in order to balance relevant covariates, propensity scores were included in the following computations of treatment effects.

The main results of the respective *EffectLite* analyses are presented in Table III and Table IV. Table III shows the results that were obtained on the basis of dummy-coded propensity strata as covariates (stratum three was chosen as a reference in these analyses). Table IV shows the results based on logit-transformed propensities modeled as a continuous covariate. In both procedures we examined the treatment effects on the treated, i.e., we present the estimates for the average treatment effect that would be seen if everyone in the treated group received the treatment versus no one in the treated group received the treatment.

As can be seen in Table III, we found small to medium average treatment effects with regard to each attachment outcome variable following Cohen's classification (e.g., Cohen, 1992). This indicates that the patients' attachment characteristics improved by inpatient psychotherapy. The most pronounced treatment effects were for *GAQ-Anxiety* and the attachment security variables (*GAQ-Security*, *RSQ-Security*). The pre–post treatment effects on attachment measured by the GAQ were essentially the same as the pre–follow-up measurements. However, the attachment improvements measured with the RSQ had decreased by follow-up, although small treatment effects still existed.

We furthermore found statistically significant covariate-treatment interactions. This can be seen in that the patients with the highest propensities (stratum 5) showed considerably larger pre–post attachment improvements than the patients in the reference group (stratum 3). Along this line there was also observably higher improvement in stratum 4 (the group with second highest propensities).

Table IV presents the treatment effects including only logit-propensities as a covariate. In these analyses, the total sample could be used. The treatment effects for patients with higher propensities than those in the control group were extrapolated in these analyses by the linear functions which were computed using the available data. The results displayed the same pattern as those from the propensity strata analyses (with an average effect size difference of $\triangle ES = .05$). In other words, there were also small to medium treatment effects for each attachment outcome; the improvements were stable for the GAQ variables but tended to get smaller on the RSQ scales; covariate-treatment interaction effects indicated that

Table III. Treatment effects on the treated, estimated by a generalized analysis of covariance (*EffectLite*) using propensity score stratification (five strata with $n = 386$)

	Pre–post						Pre–1-year follow-up					
	Average treatment effect		Numeric signs of covariate-treatment interactions [d]				Average treatment effect		Numeric signs of covariate-treatment interactions [d]			
Attachment scales	Effect [b] (*SE*)	*ES* [c]	*St1*	*St2*	*St4*	*St5*	Effect [b] (*SE*)	*ES* [c]	*St1*	*St2*	*St4*	*St5*
GAQ-												
Anxiety	−.445 (.154) **	−.51	+	+	−	**−**	−.425 (.160) **	−.50	+	−	−	**−**
Avoidance	−.174 (.124)	−.27	−	−	−	**−**	−.184 (.138)	−.26	−	−	**−**	−
Security [a]	.621 (.222) **	.49	+	−	+	**+**	.618 (.241) **	.48	−	+	+	**+**
RSQ-												
Anxiety	−.297 (.149) *	−.38	+	+	+	**−**	−.236 (.163)	−.28	+	−	−	−
Avoidance	−.239 (.105) *	−.45	−	+	−	−	−.143 (.101)	−.26	+	+	−	−
Security [a]	.528 (.194) **	.50	−	−	+	**+**	.374 (.196) *	.38	+	+	+	+

[a]Attachment security variables were computed by (*Anxiety* + *Avoidance*)·(−1).
[b]The estimations refer to conditional treatment effects on the treated, i.e., $E[g_1(\boldsymbol{Z})|X = 1]$.
[c]Effect size was computed by dividing the estimated treatment effect by the standard deviation of the outcome variable in the control group.
[d]The five propensity score strata—*St1* (lowest *PS*), *St2* (low *PS*), *St3* (mean *PS*), *St4* (high *PS*), *St5* (highest *PS*)—were dummy coded. *St3* was chosen as a reference.
Numeric signs indicate positive (+) and negative (−) aberrations from the treatment effect with regard to *St3*.
Bold numeric signs indicate statistically significant differences ($p < .05$) with regard to *St3*.
* $p < .05$ (one-tailed); ** $p < .01$ (one-tailed).

patients with higher propensities reached higher treatment effects (attachment improvements).

Changes with respect to post–follow-up treatment usage groups

One hundred and twenty-eight of the 152 patients (84.2%) for whom follow-up data were available underwent psychotherapeutic or pharmacological treatment during the 12 months between their discharge from the hospital and the follow-up assessment (44.1% psychotherapy, 7.2% pharmacotherapy, 32.9% combined therapy). At follow-up, 74 of the patients (48.7%) were still in treatment (15.8% psychotherapy, 5.3% pharmacotherapy, 27.6% combined therapy). In contrast, only 16 of the 200 control group participants (8.0%) received a psychotherapeutic or pharmacological treatment between the posttreatment assessment point and the follow-up assessment, and only six (3.0%) were currently undergoing a psychotherapeutic or pharmacological treatment at follow-up.

It is of course possible that pre–follow-up attachment improvements may have been due to the

Table IV. Treatment effects on the treated, estimated by a generalized analysis of covariance (*EffectLite*) using logit-transformedpropensity scores ($n = 525$)

	Pre–post			Pre–1-year follow-up		
	Average treatment effect		Covariate-treatment interaction	Average treatment effect		Covariate-treatment interaction
Attachment scales	Estimate [b] (*SE*)	*ES* [c]	Estimate [d] (*SE*)	Effect [b] (*SE*)	*ES* [c]	Estimate [d] (*SE*)
GAQ-						
Anxiety	−.346 (.128) **	−.42	−.159 (.039) **	−.396 (.137) **	−.49	−.116 (.042) **
Avoidance	−.221 (.105) *	−.36	−.110 (.034) **	−.217 (.121) *	−.33	−.109 (.039) **
Security [a]	.583 (.186) **	.48	.274 (.057) **	.630 (.207) **	.50	.220 (.064) **
RSQ-						
Anxiety	−.304 (.127) **	−.40	−.147 (.039) **	−.177 (.147)	−.23	−.083 (.047)
Avoidance	−.217 (.095) *	−.40	−.049 (.028)	−.123 (.094)	−.26	−.048 (.029)
Security [a]	.530 (.166) **	.49	.182 (.050) **	.319 (.180) *	.31	.131 (.056) *

[a]Attachment security variables were computed by (*Anxiety* + *Avoidance*) (−1).
[b]The estimations refer to conditional treatment effects on the treated, i.e., $E[g_1(\boldsymbol{Z})|X = 1]$.
[c]Effect size was computed by dividing the estimated treatment effect by the standard deviation of the outcome variable in the control group.
[d]Parameter estimate refers to *EffectLite* parameter γ_{11}.
* $p < .05$ (one-tailed); ** $p < .01$ (one-tailed).

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therapeutic interventions that occurred after the inpatient therapy. Moreover, the type of treatment received after the inpatient psychotherapy may have depended on the patients' posttreatment characteristics.

To test these assumptions, we first analyzed whether posttreatment characteristics (attachment anxiety/avoidance, depression) were associated with treatment categories received between post-treatment and follow-up. The two main treatment categories were *type of treatment* (no treatment, psychotherapy, pharmacotherapy, combined psychotherapeutic-pharmacological therapy) and *treatment usage* (no treatment, treatment between post and follow-up, treatment at follow-up). We did not find any differences in the posttreatment characteristics with regard to the type of treatment received after discharge from the hospital, or the treatment usage categories. The Kruskal-Wallis-Tests results for type of treatment received were as follows: *GAQ-Anxiety*: $\chi^2(3, n = 152) = 3.25$, $p = .354$; *GAQ-Avoidance*: $\chi^2(3, n = 152) = 1.99$, $p = .575$; *RSQ-Anxiety*: $\chi^2(3, n = 152) = 1.58$, $p = .664$; *RSQ-Avoidance*: $\chi^2(3, n = 152) = 6.66$, $p = .083$; Depression: $\chi^2(3, n = 152) = 6.42$, $p = .093$. The Kruskal-Wallis-Tests results for the treatment usage categories were: *GAQ-Anxiety*: $\chi^2(3, n = 152) = .75$, $p = .688$; *GAQ-Avoidance*: $\chi^2(3, n = 152) = 2.87$, $p = .239$; *RSQ-Anxiety*: $\chi^2(3, n = 152) = .18$, $p = .913$; *RSQ-Avoidance*: $\chi^2(3, n = 152) = 1.68$, $p = .433$; Depression: $\chi^2(3, n = 152) = 1.63$, $p = .442$).

Table V shows the changes in attachment characteristics from posttreatment to follow-up depending on the treatment usage categories. The treatment effect sizes presented in Table V were derived from *EffectLite* analyses comparing each treatment usage category to the control group. Each attachment characteristic was analyzed separately with the respective follow-up variable as an outcome and the post-treatment variable as a covariate. These analyses were performed with manifest variables and listwise deletion due to misfit of the measurement models. To analyze the post–follow-up changes on an individual level we computed reliable change indices (RCI) using the same cut-off criteria described above.

As it can be seen in Table V, the group of patients who did not receive any treatment after their discharge from the hospital showed consistently small to medium post–follow-up improvements in attachment characteristics compared to the controls (negative algebraic signs of the effect sizes for anxiety and avoidance, positive for security). The group of patients who were treated after inpatient psychotherapy but had already finished treatment by the follow-up assessment showed a relatively stable pattern:

Table V. Post–follow-up attachment changes (effect sizes and RCI groups) based on treatment usage categories

	No treatment after discharge from the hospital ($n = 24$)				In treatment between post and follow-up ($n = 54$)				In treatment at follow-up ($n = 74$)			
		RCI%				RCI%				RCI%		
Attachment scales	*ES* [b]	Deteriorated	Unimproved	Improved	*ES* [b]	Deteriorated	Unimproved	Improved	*ES* [b]	Deteriorated	Unimproved	Improved
GAQ-												
Anxiety	−.24	13.6	50.0	36.4	−.16	2.0	78.0	20.0	.25*	17.6	64.9	17.5
Avoidance	−.25	9.1	72.7	18.2	−.11	4.0	88.0	8.0	.35*	13.5	81.1	5.4
Security [a]	.29	18.2	40.9	40.9	.18	12.0	64.0	24.0	−.34*	27.0	54.1	18.9
RSQ-												
Anxiety	−.42*	4.5	68.1	27.4	.11	11.1	83.3	5.6	.17	9.5	83.8	6.7
Avoidance	−.15	0.0	72.7	27.3	.24	7.4	87.0	5.6	.48*	8.1	87.8	4.1
Security [a]	.41*	13.6	50.0	36.4	−.20	13.0	66.7	20.3	−.33*	12.2	71.6	16.2

[a]Attachment security variables were computed by $(Anxiety + Avoidance) \cdot (-1)$.
[b]The estimations refer to conditional treatment effects on the treated, i.e., $E[g_1(\mathbf{Z})|X = \text{treatment usage category}]$; i.e., we compared each treatment usage category to the control group given the covariate characteristics of the respective treatment usage category. Effect size was computed by dividing the estimated treatment effect by the standard deviation of the outcome variable in the control group.
*$p < .05$ (two-tailed).

they had improved slightly with regard to the GAQ variables but had slightly worsened with respect to RSQ characteristics—both results were statistically insignificant. In contrast, the group of patients who were still undergoing psychotherapeutic or pharmacological treatment at follow-up showed consistently small to medium deteriorations with regard to the treatment effect sizes: attachment anxiety and avoidance increased (positive algebraic signs), attachment security decreased (negative algebraic signs).

On an individual level (RCI), a more differentiated pattern of the differences between the treatment usage categories became apparent.

The "in treatment at follow-up" group showed a considerably lower proportion of patients who had further improved from posttreatment to follow-up than the "no treatment" group did (differences between 13% and 23%). However, the percentage of deteriorated patients was similar in both groups (differences between 1.5% and 8.5%).

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General Discussion

Empirical evidence indicates that insecure attachment characteristics are risk factors for the development of different mental disorders. Accordingly, it can be assumed that increases in attachment security are important for long-lasting improvements of different psychopathological complaints. Increasing attachment security should therefore be a goal in psychotherapy. Whether or not long-term changes in adult attachment characteristics can be accomplished via psychotherapeutic interventions is a significant question both from a theoretical point of view (stability of attachment related internal working models), as well as for clinical purposes (increase of attachment security as a treatment goal).

In the present study, we focused on the theoretical issue (psychotherapy-induced improvement of attachment security) instead of the more practical issue (long-term decreases of psychopathological complaints due to psychotherapy-induced improvement of attachment).

We collected data from 265 consecutive psychotherapy patients with different diagnoses from five inpatient units at three different time points: pretreatment, post-treatment, and 1-year follow-up. In addition, we collected data from 260 non-clinical controls. Attachment characteristics were assessed by three well-validated multi-item self-reports (BFPE, GAQ, RSQ). The BFPE allows patients to be assigned to one of the three classical attachment categories (secure, ambivalent, avoidant). The GAQ and the RSQ include individual scores on the attachment dimensions anxiety and avoidance. Both questionnaires provide a continuous measure of "attachment security" computed by (anxiety + avoidance) $\cdot(-1)$. This comprehensive measurement of attachment characteristics was conducted in order to obtain comparability to other studies. We were interested in routine care patients and long-term changes and therefore used a quasi-experimental study design. Although treatment effects can be best tested using randomized trials, we tried to at least overcome some limitations of previous studies by including a large sample, a control group, and a 1-year follow-up in our study design. In order to balance the treatment and control group on all potentially relevant covariates we used propensity score techniques in combination with a generalized ANCOVA.

On a descriptive level, about 20% of the patients improved from the insecure to the secure category from pretreatment to posttreatment according to the BFPE, which assesses attachment with respect to romantic partnerships. From pretreatment to follow-up this rate was even higher (about 25%). These categorical pre–post changes are similar to the results from Travis et al. (2001), who also examined a clinical sample including different diagnoses. They were also similar to Levy et al.'s (2006) findings regarding patients with Borderline Personality Disorder treated with transference-focused psychotherapy. In both studies attachment was assessed via interviews, in contrast to our use of self-reports.

The percentages of categorical improvement measured with the BFPE were also comparable to the results from our reliable change index (RCI) analyses. Here the treatment group was compared to the controls on the basis of the dimensional attachment measures GAQ (attachment with respect to romantic partnerships) and RSQ (attachment with respect to "others"). Both the categorical analyses with the BFPE and the RCI analyses with the GAQ and RSQ indicated that the patients' increase of attachment security could mainly be attributed to a decrease in attachment anxiety/ambivalence. This could also be seen on a descriptive level with regard to the means of the GAQ and RSQ scales. The patient group did not, however, attain an attachment security level comparable to the controls, either at posttreatment or at follow-up. There were, as hypothesized, numerous statistically significant differences in the pretreatment characteristics between the patients and the controls. Therefore, the results referred to so far only indicate to what amount attachment changes can be expected from patients in an inpatient psychotherapy group of mixed diagnoses. These results, however, cannot be clearly attributed to the treatment and must therefore not interpreted causally.

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In order to balance the covariates between the treatment and the control group we computed propensity scores based on a careful selection of relevant covariates. We used these propensity scores, on the one hand, for the generation of five propensity strata and on the other hand, we used the logit-transformed propensity scores as a continuous covariate. The treatment effect analyses were carried out with a generalized ANCOVA model using the statistics program *EffectLite 3.1.2.* The pretreatment characteristics were considerably different between the patients and the controls and therefore we examined the conditional treatment effects on the treated (i.e., treatment effects that would be observed if everyone in the patient group received the treatment versus if no one in the patient group received the treatment).

The same treatment effect pattern with very similar effect size estimates was obtained using both propensity score modeling procedures, indicating the robustness of our results. The patients showed small to medium treatment effects with regard to both attachment dimensions (anxiety, avoidance) and both attachment measures (GAQ, RSQ). The attachment improvements were stable relating to romantic partnership (measured with the GAQ). The treatment effects for attachment relating to "others" (measured with the RSQ), in contrast, tended to diminish over time, although small treatment effects were still observable. We have a good reason for a causal interpretation, i.e., for attributing the effects to the inpatient group psychotherapy: In these analyses relevant covariates like pretreatment depression and attachment pre-scores were statistically controlled; patients were only compared to controls showing similar pretreatment characteristics (propensities).

A further finding, which resulted from both of the propensity score modeling procedures, was that covariate-treatment interactions existed. We found that treatment effects in terms of improved attachment were especially pronounced in patients with high treatment propensities (i.e., high pre-scores in depression and attachment anxiety). This result offers the optimistic perspective that particularly subjects with the highest need for treatment benefited the most from treatment with regard to attachment-related changes. Moreover, the psychotherapeutic interventions from this study did not explicitly focus on improving attachment patterns, which means that psychotherapeutic approaches that specifically focus on attachment characteristics may provide larger attachment improvements.

It is crucial to evaluate whether long-term attachment improvements can be achieved by psychotherapy, i.e., if increases in attachment security (specifically decreases in attachment anxiety and avoidance) will remain stable or continue after the completion of a psychotherapy. In our naturalistic design, about 85% of the patients received either psychotherapeutic or pharmacological treatment after being discharged from the hospital and about 50% were still in treatment at the 1-year follow-up. We were unable to experimentally vary the usage of treatment from discharge from the hospital to follow-up within our study design. We did, however, analyze whether post–follow-up changes in attachment were associated with the self-selected usage of treatment after discharge from the hospital (no treatment; in treatment between post and follow-up; in treatment at follow-up). If there was a positive association between post–follow-up attachment changes and treatment after discharge from the hospital then the interpretation that long-term attachment improvements are caused by inpatient group psychotherapy would be challenged (so long as undergoing outpatient treatment is not considered a treatment effect). Contrary to this assumption, we found the worst post–follow-up attachment changes for the group of patients who were still undergoing treatment at follow-up. This group showed statistically significant deteriorations in attachment characteristics on a dimensional level compared to the control group whereas the other treatment usage groups (especially the "no treatment" group) showed post–follow-up attachment improvements.

Even though post-treatment attachment characteristics and depression scores were not different between the treatment usage groups, it is unreasonable and methodologically unjustified to assume that treatment after discharge from the hospital may be harmful or detracting from further improvement. Presumably patients who deteriorated after discharge from the hospital were more likely to require posttreatment care. This means that the amount of long-term attachment improvements attributed to the inpatient group psychotherapy could have been overestimated in Table III and Table IV, because patients who had deteriorated after their discharge from the hospital may have been more likely to undergo further treatment and receive thereby further support.

In summary, we conclude that under naturalistic conditions in Germany—including posttreatment care if desired—undergoing inpatient group psychotherapy appears to be a good starting point for long-term improvement of attachment characteristics. With regard to pre–post changes we found small to moderate treatment effect sizes and individual improvements for approximately 20% more patients than controls. From posttreatment to

follow-up the patients' attachment patterns remained fairly stable.

Methodological Discussion

In psychotherapy research it is not always possible to perform a randomized study in order to estimate treatment effects, especially if the focus is on long-term improvements or if one is interested in the effects under naturalistic conditions (effectiveness). Such research questions are, however, important and researchers, therefore, have to consider alternatives to the randomized controlled trial in order to answer the question: What would be the average outcome if patients were treated versus not treated?

In quasi-experimental studies, the distribution of pretreatment variables which influence the outcome can be very different between the treatment groups. Hence, comparability between the treatment groups cannot be assumed but must be tested and if necessary generated using the sample data. The researcher's main function then in this process is to measure and to "balance" all relevant confounders (i.e., covariates). The process of "balancing" covariates presents two challenges: First, all relevant confounders have to be measured and all covariates exerting an empirically significant influence on the outcome must be taken into account (covariate selection); second, only participants with the same covariate values may be compared using an appropriate adjustment technique (covariate adjustment).

Change processes in social and behavioral sciences are influenced by multiple factors. Covariate selection and adjustment can therefore seem to be a never ending story. However, there have been several studies in the field of educational research that have investigated which covariates are relevant and what covariate adjustment procedures are appropriate by comparing the results of randomized studies to quasi-experiments (e.g., Pohl, Steiner, Eisermann, Soellner, & Cook, 2009; Steiner, Cook, Shadish, & Clark, 2010).

In these within-study comparisons participants were randomly assigned to two groups, one group in which the persons were later randomly assigned to treatment conditions (randomized experiment), and another group for which the same treatment conditions were offered, but the subjects were allowed to select a treatment themselves (quasi-experiment). It was then determined which covariate selection and adjustment procedures resulted in treatment effects that were similar to the results of the randomized experiment. The general finding of these studies was that if a set of covariates from different domains was included in the computation then the estimated treatment effects based on the non-randomized group were less biased and a very good approximation of the random experiment.

These covariates should be well measured and plausibly related to both the selection process and the outcome (i.e., pretreatment scores of the outcome variable, socio-demographic characteristics, motivation for treatment conditions). The details of data analyses, on the other hand, were less important (e.g., inclusion of propensity strata vs. logit-transformed propensities).

In our study we took pre-test measurements of each outcome variable (attachment scales) as well as of depression. (We viewed depression as a relevant psychopathological characteristic for self-selection to inpatient psychotherapy. In our study about 80% of the patients showed clinically relevant depression scores, compared to 15% in the control group.) Additionally, a set of socio-demographic characteristics was assessed. In order to take a maximum of relevant covariates into account we computed propensity scores based on the raw scales as well as on interaction and quadratic terms.

Different covariate modeling procedures have specific advantages as well as limitations and might potentially lead to different results. Therefore we followed a double robustness strategy. We utilized the propensity scores on the one hand for creating five propensity strata and on the other hand, we logit-transformed the propensity scores and used them as a continuous covariate. The advantage of stratification is that non-linear relationships between propensities and outcomes are taken into account; however, participants outside the overlap of the propensities between the treatment groups must be excluded from the data analyses. In contrast, if logit-propensities are used as a covariate, treatment effects for participants outside the overlap can be extrapolated, e.g., based on linearity assumptions. Of course these extrapolations may be biased, if these assumptions do not hold.

In our study successful covariate balancing with regard to pretreatment variables could be shown for both procedures. Furthermore the estimated treatment effects based on the two methods were almost identical, indicating the robustness of our results.

For the estimation of treatment effects we used the statistical software *EffectLite 3.1.2*, which provides a generalized ANCOVA which includes several advantages not available with the standard ANCOVA from other statistical packages. The most important advantages for our data analysis was the inclusion of covariate-treatment interactions (treatment effects could be larger or smaller for different covariate values) and the possibility to consider conditional treatment effects on the treated (in our study the patients).

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Psychotherapy researchers are usually interested in the conditional treatment effect on the treated if treatment effects under naturalistic conditions are the focus in a quasi-experimental study and there are pronounced pretreatment differences between the control and the treatment group (e.g., due to self-selection to treatment on the basis of psychopathological complaints). In such a case the control group includes subjects who would not or are less likely to seek and undergo treatment (subjects with little or no psychopathological complaints). In such a design, researchers must take into account that if covariate-treatment interactions do exist (e.g., if the individual treatment effects are larger on persons with more complaints) then the estimate of the average treatment effect depends on the covariate values included in the computation. Accordingly, if researchers intend to estimate the average treatment effect under naturalistic conditions (i.e., for subjects who will in real life probably seek and undergo the treatment), they should include the covariate values of the subsample which consists only of treated participants (instead of the values of the total sample).

In our study, when we did not consider the conditional treatment effects on the treated in the *EffectLite* output but instead considered the treatment effects on the total sample (including treatment effects on control group participants who had frequently at pretreatment already showed high levels of attachment security), we did not find any significant treatment effect. Accordingly, when we analyzed our data using the standard ANCOVA (including treatment effect estimates for control group participants and, additionally, omitting covariate-treatment interactions), we also found no significant treatment effects. These analyses, however, do not address our research question: What would be the average effect if all patients in the sample were treated versus not treated. If we had used these analyses, then we would have drawn the wrong conclusion, i.e., the patients did not benefit from the treatment.

Although we recommend the use of propensity techniques in combination with a generalized ANCOVA from *EffectLite* as an appropriate covariate modeling procedure for quasi-experiments, our study has limitations with respect to internal and external validity.

Even though we tried to include a variety of pretreatment characteristics that may have influenced participation and the effect of inpatient group psychotherapy, we cannot be certain that we took all relevant confounders into account. To our knowledge there are no within-study comparisons (experiment vs. quasi-experiment) in the field of psychotherapy research. Future research identifying important confounders and appropriate data analysis procedures would not only strengthen the accuracy of treatment effect estimates in quasi-experimental psychotherapy research, but it would also strengthen clinicians' and politicians' confidence in the results of this research area.

A further limitation regarding internal validity refers to the small overlap of propensity scores between the treatment group and the control group. Within the propensity strata analyses, treatment effect estimates for the lowest and the highest propensity strata (*St1*, *St5*) were based only on 11 treatment participants, respectively, 10 control group subjects, which we consider to be the lowest limit for the estimation of means. In the logit-transformed propensity analyses, there were no controls available who corresponded to the $n = 56$ patients with highest propensity scores and the treatment effects had to therefore be extrapolated by a linear function. However, since both propensity modeling procedures yielded almost identical treatment effect estimates, our results should not be considered severely biased. Nevertheless, replication studies using more analogous control groups would be useful.

With regard to external validity, our findings cannot be generalized to interview-based assessments of attachment since the convergences between self-reports and interview-measures of attachment are weak. In our view, test-retest effects would be especially important for interview-based attachment measures, because the coherency of narratives may tend to improve when a person is interviewed a second time (see e.g., findings indicating attachment improvements related to transitions: Crowell et al., 2002; Zimmermann & Becker-Stoll,, 2002). Future research is needed to clarify whether long-term changes of mental representations of attachment (assessed on the basis of narratives) can be proved using a comparison to a control group and therefore be attributed to psychotherapeutic interventions.

Future research should also address which kinds of psychotherapy or treatment components are more or less successful in inducing attachment-related improvements. As described in the introduction, some research has already been done but with rather specific clinical samples (e.g., Levy et al., 2006; Tasca et al., 2007) and replication studies are also needed. In our study, which included one cognitive-behaviorally oriented and four psychodynamically oriented psychotherapy sites, we found that the patients from one of the psychodynamically oriented sites showed rather weak attachment improvements whereas the cognitive-behavioral site seemed to be especially beneficial for the improvement of attachment avoidance. However, since the sites were

specialized for different patients, these results must be regarded as explorative.

In conclusion, our results should be encouraging for psychotherapists since we provided evidence that significant improvements of adult attachment can be achieved through psychotherapeutic interventions. Moreover, in view of the various associations between insecure attachment and psychopathological complaints, our results are encouraging for psychotherapy research. We believe an example of an important future study would be the examination of whether the improvement of attachment is a proper treatment goal as it could guard against long-term symptom deteriorations for different psychological disorders.

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