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Effect of Structured Couple Communication Training With and Without Emotion Regulation Skills on Marital Conflict and Psychological Distress Couple Communication Training and Emotion Regulation

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ABSTRACT: Background

Couple communication training addresses interaction patterns associated with marital distress. The additional effect of emotion regulation skills on marital adjustment and psychological symptoms warrants evaluation.

Methods

This prospective quasi-experimental study with three parallel intervention groups at Pacific Institute of Medical Sciences, Udaipur, India included 150 married couples assigned sequentially in a repeated A-B-C cyclic sequence to usual counselling, structured couple communication training or communication training with emotion regulation skills, with 50 couples in each group. Each spouse was assessed independently at baseline, week 12 and week 24 by assessors kept unaware of intervention group. The primary comparison was the baseline adjusted week 12 Revised Dyadic Adjustment Scale (RDAS) difference between the active programmes. Analyses used couple means with multiple imputation of missing spouse scores. Twenty nine secondary comparisons underwent Holm correction.

Results

The adjusted primary difference favoured the combined programme by 3.95 points (95% CI 2.00 to 5.91; $p < 0.001$). The corresponding week 24 difference was 4.81 points (95% CI 2.81 to 6.81; Holm $p < 0.001$). Week 24 depressive symptoms, anxiety and emotion regulation difficulties were lower with the combined programme after correction. Demand and withdrawal did not differ significantly between the active programmes after

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correction. Assessments were available for 274 spouses at week 12 and 245 at week 24. Adverse events were recorded for 15 spouses from 14 couples; one couple discontinued treatment for safety but retained consent for data use.

Conclusion

Couples receiving the combined programme showed greater marital adjustment than those receiving communication training alone. Because group assignment was non-randomised and predictable, residual confounding cannot be excluded; additional treatment contact also limits attribution to emotion regulation content. Study registration: Not registered. Funding: No external funding.

INTRODUCTION

Marital distress involves difficulties in relationship adjustment and communication that may coexist with depressive symptoms and anxiety. Couple interventions seek to improve relationship functioning through changes in interaction, emotional responsiveness and shared problem solving. Lebow and Snyder (2022) described the contemporary evidence base for couple therapy and the diversity of approaches used in clinical practice. Markman et al. (2022) reviewed developments in couple relationship education and identified growing evidence across delivery formats and populations. [1,2]

Joseph et al. (2025) reported a between group effect of d = 0.85 for couple interventions in marital distress, although heterogeneity and publication bias limited interpretation. Roddy et al. (2020) found substantial improvement during couple therapy across several outcomes; their within treatment estimate does not establish the expected effect of one active programme compared with another. Relationship adjustment and demand and withdrawal are related constructs but require separate measurement. Schrodt et al. (2014) synthesised associations between demand and withdrawal and individual and relational outcomes. [3-5]

Communication skills may help spouses state concerns and respond constructively during disagreement. Azarnik et al. (2024) reviewed communication and sexual skills interventions among Iranian couples and described improvements across heterogeneous marital outcomes. Emotion regulation provides another plausible treatment target. Bloch et al. (2014) linked regulation of negative emotion with marital satisfaction over time and Aldao et al. (2010) identified associations between regulation strategies and psychopathology. These associations do not establish the effect of adding regulation skills to communication training. White VanBoxel et al. (2024) found that baseline regulation difficulties were associated with lower relationship satisfaction in a small therapy trial but did not reliably modify subsequent change. Voldstad et al. (2025) reported uncertain evidence for mindfulness interventions; an effect of g = 0.60 decreased to 0.21 after removal of outliers. [6-10]

Indian studies provide evidence that couple communication can be addressed in clinical and community settings. Raj et al. (2022) evaluated the CHARM2 family planning intervention among 1201 rural couples in Maharashtra and reported improvements in contraceptive communication. Dixit et al. (2023) analysed marital practices and communication within the same baseline cohort. These reports concern reproductive decision making rather than treatment of marital distress. Rathod et al. (2024) assessed depression and marital adjustment among 120 men with diabetes. Gayathri et al. (2026) studied a combined couple intervention among 60 couples with primary infertility in Chennai. The published changes within each treatment group do not provide an adjusted between group estimate for the present comparison. [11-14]

International communication and relationship education trials provide closer intervention comparisons. Deylami et al. (2021) evaluated online Gottman psychoeducation among 72 Iranian couples and reported improvements in marital communication. Adler-Baeder et al. (2022) examined ELEVATE and Couples Connecting Mindfully among 929 couples with assessments extending to one year. Le et al. (2023) compared levels of coach support for the OurRelationship programme among 740 couples. Differences in intervention content, comparator contact and outcome definitions limit direct transfer of their estimates to a clinic based comparative study of communication training with an additional emotion regulation module. [15-17]

The present study compared usual counselling, structured couple communication training and communication training with emotion regulation skills. The primary objective was to compare week 12 marital adjustment between the combined

programme and communication training alone after adjustment for baseline scores. Secondary objectives were to examine marital adjustment at week 24, demand and withdrawal, depressive symptoms, anxiety and emotion regulation difficulties. Attendance and recorded adverse events were described. Exploratory analyses examined associations between changes in psychological measures and marital adjustment.

METHODS

STUDY DESIGN AND SETTING

This was a prospective quasi-experimental study with three parallel intervention groups. Recruitment occurred through the psychiatry outpatient service at Pacific Institute of Medical Sciences, Udaipur, Rajasthan, India. The protocol was dated 3 May 2024, 31 days before study commencement on 3 June 2024. Baseline assessments extended from 4 June 2024 to 31 January 2025. The final recorded week 24 assessment occurred on 26 July 2025 and formal study closure was on 9 September 2025, 45 days later. The study period was approximately 15 months; each couple was followed for 24 weeks.

PARTICIPANTS AND ELIGIBILITY

Participants were legally married heterosexual couples who shared a household and had been married for at least one year. Men were at least 21 years old and women at least 18 years old. Marital distress was defined for eligibility as an RDAS score of 48 or less in at least one spouse at screening. Both spouses identified marital conflict as a concern and sought help for it. Each spouse provided written informed consent and agreed to sequential assignment to any of the three study groups. Both were available for joint sessions and the 24 week assessment period and lived within a reasonable travel distance of the study site. They could complete the questionnaires through reading or interviewer administration and could participate in the language used for treatment. Any current psychotropic medication dose had been stable for at least eight weeks before baseline.

Couples were excluded for current or recent severe physical violence within the preceding 12 months, violence that caused injury, sexual coercion or fear that made joint sessions unsafe. Each spouse was screened separately and privately. Other safety exclusions were suicidal ideation with intent or a plan, a psychiatric condition that needed priority individual treatment and moderate to severe alcohol or substance use disorder. Mild verbal conflict alone was not an exclusion. [Specify the screening instruments actually administered.]

Couples were also excluded for filed separation or divorce proceedings, a firm decision to end the marriage, an ongoing extramarital relationship that the involved spouse was unwilling to end, living apart or planned relocation during the assessment period. Couple therapy or structured communication training in the preceding 12 months or planned during the study, current or planned individual therapy focused on emotion regulation and participation in another interventional study were exclusion criteria. Cognitive, intellectual, hearing or speech impairment that prevented participation, serious medical illness expected to interfere with attendance and employment by or dependency on the study team also precluded enrolment. [Confirm whether the proposed exclusion for couples from the same close family network was enforced.]

SAMPLE SIZE

Sample size planning addressed the baseline adjusted week 12 RDAS comparison between Group C and Group B. Molajafar et al. (2015) studied 45 married individuals with marital problems in Iran in a quasi experimental study with 15 participants each in emotion regulation, mindfulness and untreated control groups. Postintervention Marital Conflicts Questionnaire scores were 100.47 ± 15.80, 107.33 ± 18.54 and 118.60 ± 16.77 respectively. These values yield standardised differences of d = 1.11 for emotion regulation versus control and d = 0.40 versus mindfulness with pooled standard deviations of 16.29 and 17.22. Differences in population, outcome instrument, unit of participation and comparator limited direct transfer of these effects. An assumed planning effect of d = 0.65 was selected with reference to these contrasts. Standardisation avoids dependence on raw score units but does not establish equivalence between instruments or populations. [19]

For two independent means, $n = 2(Z_{1-\alpha/2} + Z_{1-\beta})^2/d^2$. A two sided significance level of 0.05 and 80% power give approximately 37.15 evaluable couples per group using full precision normal quantiles, rounded to 38. The calculation using the noncentral t distribution required 39 evaluable couples per group. Allowing for 20% loss to follow up gave $39/(1 - 0.20) = 48.75$, rounded to 49 and then to 50 couples per group. A target of 50 couples in each of three groups yielded 150 couples or 300 spouses. The couple was the unit of group assignment and primary analysis, so a further adjustment for clustering of

spouses within couples was unnecessary. The calculation addressed one prespecified primary comparison without multiplicity adjustment and did not assume a variance reduction from baseline adjustment.

SEQUENTIAL GROUP ASSIGNMENT AND ASSESSOR BLINDING

After both spouses completed written informed consent, eligibility assessment and baseline questionnaires, the couple was registered with a unique identifier and assigned sequentially to Group A, Group B or Group C using a predetermined A-B-C cyclic sequence. The sequence was repeated throughout recruitment until 50 couples had been assigned to each group. Group A received usual counselling, Group B structured couple communication training and Group C communication training with emotion regulation skills. Assignment was non-randomised and predictable; no random-number sequence or allocation concealment was used.

Outcome assessors did not deliver treatment and were kept unaware of intervention group. Participants were asked not to disclose session content and assessments took place in rooms separate from treatment sessions. Participants and treating clinicians were aware of the programme received. The protocol proposed assessor guesses at week 24 but these were not recorded; the success of assessor blinding was therefore not assessed.

TREATMENT AS USUAL AND COMMON PROCEDURES

Ordinary supportive or medical follow up could continue across groups and changes in concurrent care were recorded. The workbook recorded psychotropic medication and outside psychological treatment at baseline and subsequent changes with dates. Structured couple therapy outside the study and individual treatment focused on emotion regulation were excluded to limit contamination. Concurrent treatment changes and session attendance were not used as covariates in the primary treatment comparison.

STUDY INTERVENTIONS

Group A was scheduled to receive up to six fortnightly usual counselling sessions. Groups B and C were scheduled to receive up to 12 weekly joint communication training sessions. Group C received an additional module of six emotion regulation sessions during weeks 9 to 11 after the initial communication training phase and before the week 12 assessment. The codebook indicates that spouses sometimes differed in attendance by one session. The additional sessions in Group C produced greater planned treatment contact than in Group B.

USUAL COUNSELLING DELIVERY

Usual counselling comprised joint supportive sessions that explored current marital concerns and identified practical coping strategies. Psychoeducation addressed relationship stress; sessions facilitated respectful discussion and shared problem solving. Delivery followed the semi structured Supportive Therapy Manual for Marital Distress. Each session lasted 45 minutes. Licensed couples therapists or clinical psychologists with at least two years of experience after qualification delivered the intervention. Providers completed a one day introductory workshop and received supervision once every two weeks. No formal structured homework was assigned. Couples were encouraged to apply the strategies discussed during sessions in daily life. [Provide the manual edition or version and its bibliographic or access details.]

COMMUNICATION TRAINING DELIVERY

Communication training was adapted from the Prevention and Relationship Education Program (PREP) manual. Sessions addressed active listening, clear expression of needs and validation of the partner's perspective. Clinician demonstration and guided role play addressed criticism, defensiveness and withdrawal. Couples developed agreed methods for discussing disagreements. Each session lasted 60 minutes. Certified relationship counsellors delivered the programme after three days of intensive training in behavioural couples therapy techniques. Trainers were required to pass a fidelity checklist using a mock training recording. Couples completed 20 minute communication exercises twice weekly between sessions, including practice of the Speaker Listener technique. Written practice logs were submitted and reviewed at the beginning of each subsequent visit. [Provide the PREP manual edition or version and describe the adaptations used in this study.]

EMOTION REGULATION DELIVERY

The additional emotion regulation module comprised six sessions of 75 minutes each during weeks 9 to 11. All sessions were scheduled before the week 12 assessment. Delivery used a joint couple format with individual exercises when needed.

Sessions addressed recognition of physiological and emotional cues and identification of triggers during marital disagreement. Couples practised a time out before responding, paced breathing using the 4-7-8 technique, cognitive reappraisal of distressing interpretations and distress tolerance exercises. Exercises explicitly linked physiological calming to the communication skills. The module followed the initial communication training phase to support de escalation before advanced conflict resolution practice.

OUTCOME MEASURES AND ASSESSMENT SCHEDULE

Each spouse completed questionnaires independently at baseline, week 12 and week 24 in English or Hindi. Language was chosen individually at screening according to reading comfort and confirmed using a sample item. Each spouse used the same language across visits; spouses could choose different languages. Assessors who were kept unaware of intervention group administered the measures separately from intervention sessions. Spouses completed forms in separate rooms or sufficiently far apart without discussion.

When literacy was limited, the assessor read each item verbatim and recorded the response privately. Questionnaire order was consistent across visits and missed items were checked before departure. Responses were not shown to the partner or therapists. Language and administration mode were not recorded in the analysis dataset, so language specific analyses and adjustment for administration mode were not possible.

Hindi RDAS, CPQ-SF and DERS-16 versions underwent two independent forward translations, reconciliation and a blind back translation into English. A panel of psychiatrists, clinical psychologists and a language expert reviewed meaning equivalence. Comprehension piloting was completed in 24 couples before recruitment and the final versions were frozen before study use. These couples subsequently entered the main study and completed fresh baseline assessments with the final versions before sequential group assignment; only the fresh assessments were used in the study analysis. Internal consistency was not assessed. The workbook contained scale totals rather than item responses, which prevented retrospective calculation of reliability coefficients.

The Revised Dyadic Adjustment Scale (RDAS) measures relationship consensus, satisfaction and cohesion. Scores range from 0 to 69 and higher scores indicate better adjustment. The primary outcome was the couple mean week 12 RDAS score, adjusted for baseline couple RDAS, for Group C compared with Group B. RDAS at week 24 and the other treatment comparisons were secondary outcomes. The original RDAS publication supports the construct and scale development but does not establish a clinically important difference for this study population. [20]

Demand and withdrawal were assessed using the original six item total demand and withdrawal subscale of the 11 item Communication Patterns Questionnaire Short Form (CPQ-SF). Items 3, 4, 8, 9, 10 and 11 were rated from 1 to 9 and summed to give a score from 6 to 54; higher scores indicate more demand and withdrawal. The outcome was this subscale score rather than a total across all 11 items. Futris et al. (2010) describe this scoring and evaluate the short form and alternative factor structures. [21]

The Patient Health Questionnaire (PHQ-9) assesses depressive symptom severity from 0 to 27 and the Generalized Anxiety Disorder scale (GAD-7) assesses anxiety severity from 0 to 21. Higher scores indicate greater symptom burden. The Difficulties in Emotion Regulation Scale (DERS-16) measures regulation difficulties from 16 to 80 with higher scores indicating greater difficulty. The original validation reports support these instruments. [22-24] Couple averages were interpreted as symptom burden rather than individual diagnostic classifications. Any positive response to PHQ-9 item 9 prompted a same day clinical risk assessment. [Specify the sources of the English and Hindi PHQ-9 and GAD-7 versions used.]

The intended week 12 visit was day 84 with a window from day 81 to day 91. The intended week 24 visit was day 168 with a window from day 165 to day 178. One couple's week 12 assessment occurred on day 92. The main analysis retained these scores and a sensitivity analysis treated both spouse assessments as missing.

INTERVENTION FIDELITY AND SAFETY MONITORING

Adverse events were defined as untoward medical, psychological or relational occurrences after intervention group assignment, irrespective of their relationship to study participation. Events included clinically meaningful worsening of depression or anxiety, suicidal ideation, self harm, new or escalating intimate partner violence, psychiatric admission or

emergency presentation, escalation of substance use, marked relationship deterioration and acute distress during or after a session or assessment. A standard checklist was administered to each spouse separately and privately at baseline, week 12 and week 24. Baseline inquiry documented existing concerns; only occurrences after group assignment or subsequent worsening qualified as study events. Therapists in all groups reported observed or disclosed events and predefined questionnaire triggers prompted clinical review. Fixed assessment inquiries were the same across groups but the greater number of treatment contacts in the active groups could create additional opportunities for detection. Attendance was recorded separately for each spouse. Harms reporting distinguished affected spouse records from affected couples. [25]

The protocol required an event log with the affected spouse, onset, description, severity, seriousness, relatedness, expectedness, action, outcome and resolution date. Serious events included death, life threatening events, hospital admission or prolongation, persistent disability and important events that required urgent intervention to prevent these outcomes. The latter category included suicide attempts, self harm that needed medical attention, violence causing injury, sexual violence, strangulation, threats to kill, police involvement and child protection concerns. Severity was rated separately from seriousness. Relatedness used five categories from not related to definitely related and expectedness was judged against events listed in the protocol. Initial classification by the site clinician was reviewed by an independent clinician kept unaware of intervention group through removal of group identifying information from narratives; disagreements were referred to the chair of the Data and Safety Monitoring Board (DSMB). Serious events were reported immediately to the principal investigator and to the ethics committee according to its procedures. The DSMB reviewed cumulative events and could recommend modification or stopping. [Specify the actual DSMB review intervals and ethics reporting timelines.]

A violence disclosure prompted a private clinician risk assessment and suspension of conjoint sessions and couple homework. Risk assessment addressed escalation, injuries, strangulation, weapons, threats, sexual violence and children in the home. Safety planning and referral were provided with the participant's agreement; information was shared outside the team according to consent and the specified safeguarding exceptions. Further support was individual or through referral. Assessments continued only when safe and private and contact avoided shared addresses or phones. The protocol provided for a clinician check within one week, risk reassessment at later contacts and staff supervision. Discontinuation of treatment did not itself withdraw consent for data use or remove the couple from analysis according to the initially assigned intervention group.

Usual counselling providers received supervision once every two weeks. Communication trainers were required to pass a fidelity checklist using a mock training recording before delivery. Written communication practice logs were reviewed at the beginning of subsequent visits. [Describe any assessment of adherence during actual treatment sessions, supervision of the communication and emotion regulation modules and independent fidelity review, if undertaken.]

DATA COLLECTION AND QUALITY CONTROL

Patient confidentiality was maintained. The eight couples who withdrew before baseline assessment had their data deleted. The retained dataset was deidentified and the manuscript analysis used serial numbers without participant names. Couples were linked through consecutive spouse records in accordance with the codebook. The source workbook was preserved and audited for partner linkage, group assignment, dates, score ranges and missing assessments. Audit findings were recorded separately from source values. Safeguarding records were held in a restricted safety file separate from the case record forms. [Describe original data entry and source verification procedures.]

Statistical analysis

The primary comparison, outcome hierarchy and statistical methods were specified before recruitment and were not changed. The manuscript analysis document was dated 4 October 2026 and recorded the computational implementation after data collection and descriptive auditing but before outcome comparisons in this manuscript workflow. The original protocol was dated 3 May 2024. Exploratory analyses are identified separately from the primary and secondary outcome comparisons.

Treatment comparisons used the mean of both spouse scores within each couple. The primary ANCOVA included week 12 couple RDAS as the outcome and baseline couple RDAS and treatment group as covariates. All 150 couples were included in the primary analysis according to their initially assigned intervention group. Missing spouse scores were imputed before calculation of couple means; a single observed partner score was not treated as a complete couple mean.

Chained equations used predictive mean matching with five observed donors, 50 burn in sweeps and five sweeps between retained datasets. One hundred datasets were retained for each outcome. Models incorporated baseline measures, treatment group and relevant observed couple and spouse information. The assumptions and sensitivity analyses followed established guidance for multiple imputation. [26]

Custom routines implemented QR based predictive mean matching and ANCOVA with HC3 standard errors. Estimates were pooled using Rubin's rules and Barnard and Rubin degrees of freedom. The primary test used a two sided significance level of 0.05.

The remaining 29 treatment comparisons across five outcomes and two visits formed one Holm correction family. Secondary confidence intervals were pointwise 95% intervals and were not adjusted for multiplicity. Adjusted group means were evaluated at the overall mean baseline score. The primary standardised difference used the pooled baseline couple RDAS standard deviation in Groups B and C; it was not interpreted as an established clinical importance threshold.

Sensitivity analyses used observed complete couples, exclusion of the late week 12 visit and shifts in originally missing week 12 RDAS scores. Common deterioration scenarios subtracted two, four or six points. Differential scenarios subtracted up to six points from missing Group C scores and added up to six points to missing Group B scores. A supporting repeated assessment model used profiled Gaussian restricted maximum likelihood with random intercepts for couple and spouse, baseline adjustment and group, visit and spouse role terms. Its confidence intervals were approximate Wald intervals.

Exploratory regressions related improvement in RDAS to improvement in each psychological or communication measure at each visit. Models used complete couples and adjusted for baseline RDAS, baseline explanatory score and treatment group. Percentile confidence intervals used 2000 bootstrap samples at couple level. HC3 tests underwent Benjamini and Hochberg correction across eight comparisons. These analyses assessed association rather than mediation. Analyses used Python 3.12.14, NumPy 2.3.5, SciPy 1.17.0 and pandas 2.2.3.

Ethical considerations

The study was approved by [Ethics committee name], approval number [Ethics approval number], dated [Ethics approval date]. Both spouses provided written informed consent individually before study procedures and could withdraw at any stage and request deletion of their data. Limits to confidentiality for imminent risk to life and child safeguarding concerns were explained during consent. Patient confidentiality was maintained. The study was not registered.

RESULTS

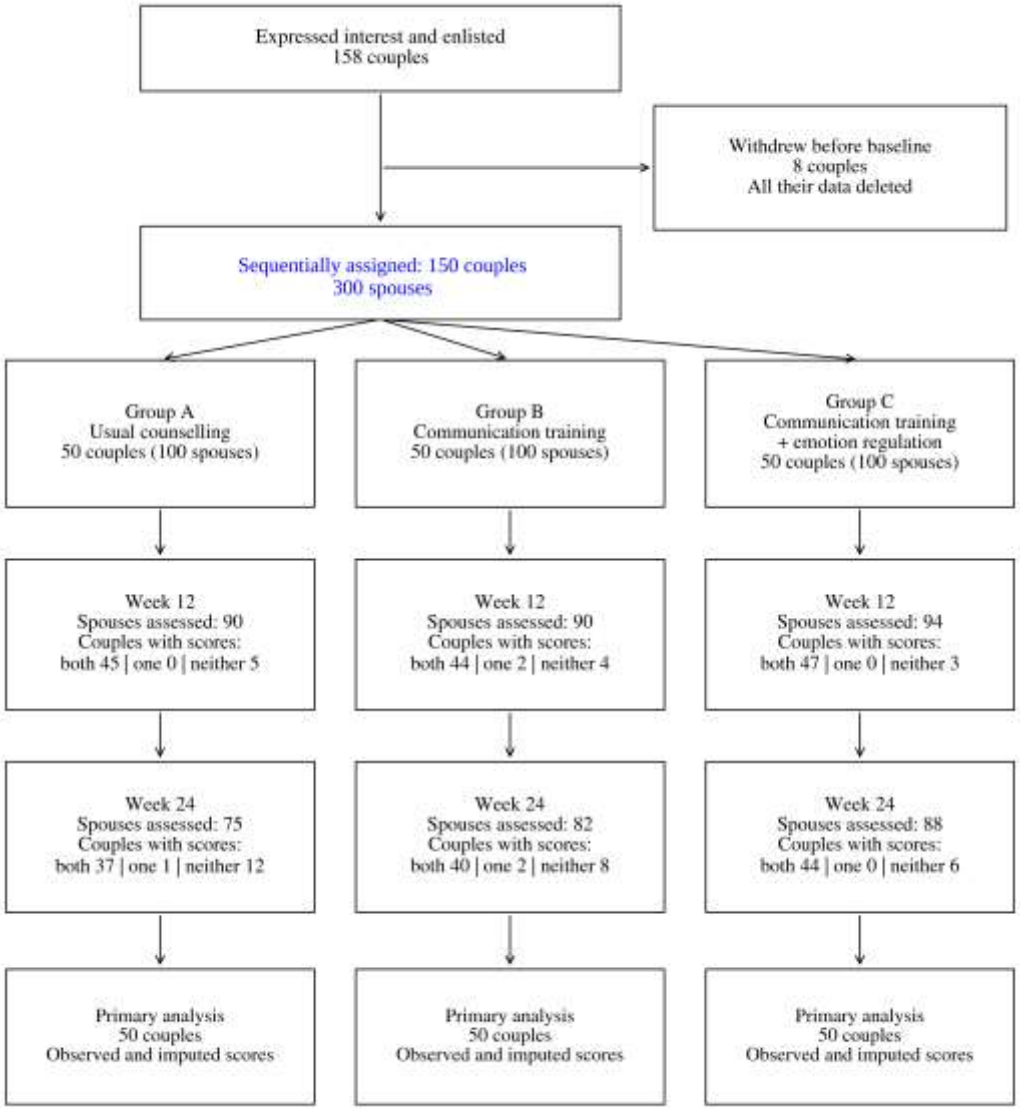
Participant disposition and data completeness

A total of 158 couples expressed interest and enlisted. Eight withdrew before baseline assessment and their data were deleted. The remaining 150 couples comprising 300 spouses were sequentially assigned in a repeated A-B-C cyclic sequence, with 50 couples and 100 spouses in each group. All spouses had baseline scale scores. At week 12, assessments were available for 90 spouses in Group A, 90 in Group B and 94 in Group C; both spouses had scores in 45, 44 and 47 couples respectively. At week 24, assessments were available for 75, 82 and 88 spouses and complete couple scores for 37, 40 and 44 couples respectively. Two spouses missed week 12 and returned at week 24.

No couple subsequently withdrew consent or requested data deletion. One couple discontinued treatment for safety. All 150 couples were retained in the primary analysis according to their initially assigned intervention group. Figure 1 and Supplementary Table S1 show visit specific availability.

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Figure 1. Participant flow



Eight of 158 interested couples withdrew before baseline and their data were deleted. The remaining 150 couples were sequentially assigned using the predetermined A-B-C cyclic sequence. Assessment counts distinguish couples with both scores, one score or neither score at each visit and do not indicate withdrawal of consent. All 50 couples per group were included in the primary analysis through observed or imputed scores. One Group C couple discontinued treatment for safety and retained consent for data use. Two spouses missed week 12 and returned at week 24.

Baseline characteristics

Mean spouse age ranged from 35.24 to 35.78 years across groups and mean marriage duration ranged from 11.08 to 11.12 years. Each group contained 50 female and 50 male spouses. The index spouse was female in 35 couples in Group A, 34 in Group B and 25 in Group C. Baseline couple RDAS means were 38.87, 39.66 and 39.50 respectively. Baseline psychotropic medication was recorded for 44 spouses and outside psychological treatment for seven. Table 1 provides demographic and clinical characteristics with the appropriate spouse or couple denominators.

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Table 1. Baseline characteristics

Characteristic	Group A	Group B	Group C
Spouse age (years)	35.58 ± 5.22	35.24 ± 4.82	35.78 ± 5.67
Marriage duration (years)	11.08 ± 4.34	11.12 ± 3.95	11.12 ± 5.05
Number of children	1.54 ± 0.81	1.62 ± 0.73	1.34 ± 0.72
Female spouses	50/100 (50.0%)	50/100 (50.0%)	50/100 (50.0%)
Female index spouses	35/50 (70.0%)	34/50 (68.0%)	25/50 (50.0%)
Joint family	20/50 (40.0%)	18/50 (36.0%)	17/50 (34.0%)
Any children	44/50 (88.0%)	46/50 (92.0%)	45/50 (90.0%)
Baseline psychotropic medication	12/100 (12.0%)	18/100 (18.0%)	14/100 (14.0%)
Baseline outside psychological treatment	4/100 (4.0%)	2/100 (2.0%)	1/100 (1.0%)
RDAS score (points)	38.87 ± 5.26	39.66 ± 6.92	39.50 ± 5.85
CPQ-SF score (points)	25.71 ± 8.85	24.41 ± 8.57	23.72 ± 8.82
PHQ-9 score (points)	8.37 ± 4.07	8.95 ± 2.77	9.02 ± 3.61
GAD-7 score (points)	6.84 ± 3.31	6.67 ± 2.26	7.01 ± 2.68
DERS-16 score (points)	42.96 ± 8.33	41.52 ± 7.00	41.59 ± 8.70

Values are mean ± SD or n/N (%). Age, female spouses and baseline treatment use spouse denominators; other characteristics use couple denominators. Index spouse means the member recorded as spouse A and is independent of sex. Scale values are means of both spouses. Baseline significance tests were not performed.

Observed outcome profiles

Observed complete couple RDAS means increased from baseline to week 12 in all groups. Week 12 values were 40.19 in Group A, 43.65 in Group B and 47.09 in Group C. Corresponding week 24 values were 39.14, 41.89 and 46.82. Table 2 presents observed scale summaries. Different couples contributed complete data at each visit, so these marginal summaries were not used to calculate paired change estimates. Figure 2 displays the observed RDAS profile.

Table 2. Observed outcome scores across assessments

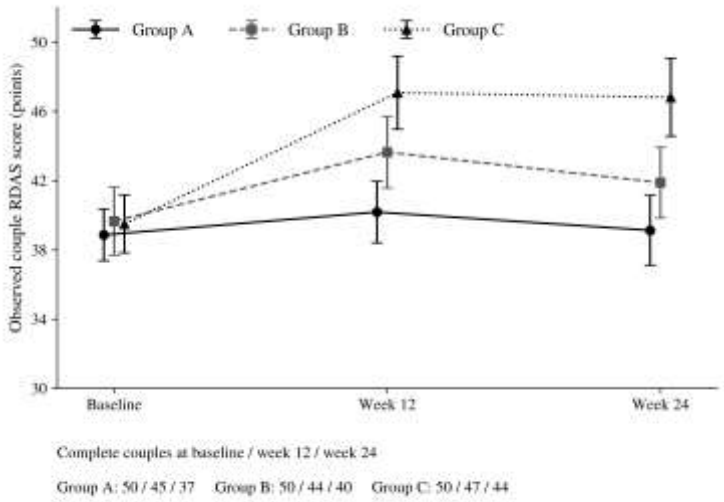
Measure	Assessment	Group A	Group B	Group C
RDAS	Baseline	38.87 ± 5.26	39.66 ± 6.92	39.50 ± 5.85
RDAS	Week 12	40.19 ± 5.90	43.65 ± 6.81	47.09 ± 7.21
RDAS	Week 24	39.14 ± 6.05	41.89 ± 6.38	46.82 ± 7.38

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Measure	Assessment	Group A	Group B	Group C
CPQ-SF	Baseline	25.71 ± 8.85	24.41 ± 8.57	23.72 ± 8.82
CPQ-SF	Week 12	25.22 ± 8.50	20.55 ± 7.28	19.27 ± 7.80
CPQ-SF	Week 24	25.35 ± 6.83	22.26 ± 7.96	19.32 ± 7.92
PHQ-9	Baseline	8.37 ± 4.07	8.95 ± 2.77	9.02 ± 3.61
PHQ-9	Week 12	7.82 ± 3.87	8.08 ± 3.54	6.62 ± 3.20
PHQ-9	Week 24	7.91 ± 3.30	8.86 ± 3.13	6.64 ± 3.02
GAD-7	Baseline	6.84 ± 3.31	6.67 ± 2.26	7.01 ± 2.68
GAD-7	Week 12	6.40 ± 3.10	6.20 ± 2.63	5.13 ± 2.81
GAD-7	Week 24	6.66 ± 2.95	6.69 ± 2.31	4.90 ± 2.64
DERS-16	Baseline	42.96 ± 8.33	41.52 ± 7.00	41.59 ± 8.70
DERS-16	Week 12	41.60 ± 10.17	37.74 ± 9.51	34.66 ± 9.06
DERS-16	Week 24	41.42 ± 9.06	40.25 ± 8.33	34.48 ± 9.74

Values are observed mean ± SD in points and require both spouse scores. A/B/C couple counts were 50/50/50 at baseline, 45/44/47 at week 12 and 37/40/44 at week 24. These descriptive values use no imputation. Higher RDAS indicates better adjustment; lower values on the other scales indicate lower symptom or communication difficulty. RDAS: Revised Dyadic Adjustment Scale; CPQ-SF: six item total demand and withdrawal subscale of the short form; PHQ-9: Patient Health Questionnaire; GAD-7: Generalized Anxiety Disorder scale; DERS-16: Difficulties in Emotion Regulation Scale.

Figure 2. Observed marital adjustment scores



Points are observed means of both spouse scores for complete couples. Error bars are descriptive Student t 95% confidence intervals based on couple standard deviations. Couple counts appear below the plot. Symbols are offset for readability. The vertical axis is restricted to 30 to 52 points; the full RDAS range is 0 to 69. These descriptive intervals are not adjusted treatment effect intervals.

PRIMARY OUTCOME

Adjusted week 12 RDAS means were 40.38 (95% CI 39.02 to 41.74) in Group A, 42.76 (41.38 to 44.15) in Group B and 46.72 (45.35 to 48.09) in Group C. The primary Group C minus Group B difference was 3.95 points (95% CI 2.00 to 5.91; exact p = 0.000101). The standardised difference was 0.62 using the pooled baseline couple RDAS standard deviation of 6.41 points in the two active groups. Table 3 presents the adjusted means and primary comparison.

Table 3. Primary adjusted marital adjustment outcome at week 12

Group	Adjusted RDAS mean (points)	95% CI
A	40.38	39.02 to 41.74
B	42.76	41.38 to 44.15
C	46.72	45.35 to 48.09

Adjusted means are in RDAS points and evaluated at the overall baseline couple mean.

Primary treatment comparison

Contrast	Difference (points)	95% CI	P
C versus B	3.95	2.00 to 5.91	<0.001

The ANCOVA included all 150 enrolled couples across 100 imputations and adjusted for baseline couple RDAS. A positive difference favours Group C. The single primary comparison was not corrected for multiplicity. Exact p = 0.000101.

Secondary outcomes

The week 24 RDAS difference for Group C compared with Group B was 4.81 points (95% CI 2.81 to 6.81; Holm p < 0.001). Group C also had higher RDAS than Group A at both visits. The Group B versus Group A RDAS comparison did not pass Holm correction at either visit.

At week 24, Group C had lower PHQ-9 than Group B by 1.97 points (95% CI -3.11 to -0.83; Holm p = 0.020), lower GAD-7 by 1.57 points (95% CI -2.49 to -0.66; Holm p = 0.020) and lower DERS-16 by 4.72 points (95% CI -7.73 to -1.70; Holm p = 0.049). The corresponding week 12 psychological comparisons did not pass Holm correction.

The additional Group C benefit for demand and withdrawal was not significant after correction at either visit. Group C had lower demand and withdrawal than Group A at week 12 (difference -4.37; 95% CI -7.13 to -1.62; Holm p = 0.044) but the week 24 comparison did not pass correction (Holm p = 0.051). Group C had lower DERS-16 than Group A at both visits and lower GAD-7 at week 24. No Group B versus Group A secondary comparison passed Holm correction. Table 4 reports all 29 secondary comparisons. Failure to reject a difference was not interpreted as equivalence.

Table 4. Secondary treatment comparisons at week 12

Measure	Contrast	Difference	95% CI	Raw P	Holm P
RDAS	B versus A	2.39	0.44 to 4.34	0.017	0.252
RDAS	C versus A	6.34	4.40 to 8.29	<0.001	<0.001

Measure	Contrast	Difference	95% CI	Raw P	Holm P
CPQ-SF	C versus B	-0.83	-3.51 to 1.85	0.540	1.000
CPQ-SF	B versus A	-3.54	-6.14 to -0.94	0.008	0.144
CPQ-SF	C versus A	-4.37	-7.13 to -1.62	0.002	0.044
PHQ-9	C versus B	-1.49	-2.72 to -0.26	0.018	0.252
PHQ-9	B versus A	0.20	-1.30 to 1.69	0.795	1.000
PHQ-9	C versus A	-1.30	-2.69 to 0.10	0.068	0.681
GAD-7	C versus B	-1.30	-2.30 to -0.31	0.011	0.183
GAD-7	B versus A	-0.03	-1.17 to 1.10	0.953	1.000
GAD-7	C versus A	-1.34	-2.47 to -0.20	0.021	0.277
DERS-16	C versus B	-3.32	-6.42 to -0.22	0.036	0.435
DERS-16	B versus A	-2.20	-5.51 to 1.10	0.189	1.000
DERS-16	C versus A	-5.52	-8.69 to -2.36	<0.001	0.019

Differences are baseline adjusted scale points; the first group minus the second. Positive RDAS and negative differences on other scales favour the first group. All models included 150 couples. Holm correction covers all 29 comparisons across both visits; the displayed intervals are pointwise and unadjusted.

Table 4. Secondary treatment comparisons at week 24 continued

Measure	Contrast	Difference	95% CI	Raw P	Holm P
RDAS	C versus B	4.81	2.81 to 6.81	<0.001	<0.001
RDAS	B versus A	1.79	-0.21 to 3.78	0.079	0.713
RDAS	C versus A	6.59	4.52 to 8.66	<0.001	<0.001
CPQ-SF	C versus B	-2.78	-5.60 to 0.03	0.053	0.581
CPQ-SF	B versus A	-1.27	-4.00 to 1.45	0.357	1.000
CPQ-SF	C versus A	-4.06	-6.68 to -1.43	0.003	0.051
PHQ-9	C versus B	-1.97	-3.11 to -0.83	<0.001	0.020
PHQ-9	B versus A	0.48	-0.80 to 1.75	0.462	1.000
PHQ-9	C versus A	-1.49	-2.70 to -0.29	0.015	0.247
GAD-7	C versus B	-1.57	-2.49 to -0.66	<0.001	0.020
GAD-7	B versus A	-0.20	-1.25 to 0.85	0.712	1.000

Measure	Contrast	Difference	95% CI	Raw P	Holm P
GAD-7	C versus A	-1.77	-2.82 to -0.72	0.001	0.026
DERS-16	C versus B	-4.72	-7.73 to -1.70	0.002	0.049
DERS-16	B versus A	-0.82	-3.85 to 2.20	0.591	1.000
DERS-16	C versus A	-5.54	-8.63 to -2.45	<0.001	0.014

The week 12 primary C versus B RDAS comparison is in Table 3. Rounded p values do not replace the correction calculated at full precision. The week 24 CPQ-SF C versus A comparison did not pass correction (Holm p = 0.051).

Treatment attendance and concurrent care

Mean recorded counselling attendance was 4.35 ± 1.62 sessions in Group A. Communication attendance was 9.76 ± 2.52 sessions in Group B and 9.60 ± 2.34 in Group C. Group C emotion regulation attendance was 4.65 ± 1.17 sessions. These summaries describe spouse attendance records rather than independent session counts. Subsequent medication or outside treatment changes were recorded for nine spouses in Group A, three in Group B and five in Group C.

EXPLORATORY ASSOCIATIONS

At week 12, improvement in RDAS was associated with improvement in demand and withdrawal, PHQ-9, GAD-7 and DERS-16 after false discovery rate correction. At week 24, the associations with demand and withdrawal, PHQ-9 and DERS-16 passed correction. The week 24 GAD-7 association had an adjusted p value of 0.058. Its bootstrap interval excluded zero because the interval and p value used different inference methods. Supplementary Table S4 reports these exploratory associations; they do not establish a mediating mechanism.

SAFETY AND TREATMENT DISCONTINUATION

Adverse events were recorded for two spouses in Group A, seven in Group B and six in Group C. These records represented two, six and six affected couples respectively. Recorded severities were mild in 11 spouse records, moderate in three and severe in one. The severe Group C record described disclosure of physical violence and both members of that couple discontinued treatment for safety. Consent for data use was retained and no postbaseline data deletion was requested. The analysis workbook did not contain adjudicated relatedness or formal seriousness classifications. Table 5 distinguishes spouse and couple counts. [Enter the adjudicated seriousness, relatedness and outcome of the violence event from the restricted safety record.]

Table 5. Attendance, additional treatment and safety

Measure	Group A	Group B	Group C
Counselling or communication sessions	4.35 ± 1.62	9.76 ± 2.52	9.60 ± 2.34
Emotion regulation sessions	Not applicable	Not applicable	4.65 ± 1.17
Spouses with an adverse event	2/100 (2.0%)	7/100 (7.0%)	6/100 (6.0%)
Couples with an adverse event	2/50 (4.0%)	6/50 (12.0%)	6/50 (12.0%)
Spouses with additional treatment changes	9/100 (9.0%)	3/100 (3.0%)	5/100 (5.0%)
Spouses who discontinued treatment for safety	0/100 (0.0%)	0/100 (0.0%)	2/100 (2.0%)

Measure	Group A	Group B	Group C
Couples who discontinued treatment for safety	0/50 (0.0%)	0/50 (0.0%)	1/50 (2.0%)
Spouse records with mild events	2/100 (2.0%)	5/100 (5.0%)	4/100 (4.0%)
Spouse records with moderate events	0/100 (0.0%)	2/100 (2.0%)	1/100 (1.0%)
Spouse records with severe events	0/100 (0.0%)	0/100 (0.0%)	1/100 (1.0%)

Attendance is mean ± SD of spouse records. Scheduled maxima were six counselling sessions in A, 12 communication sessions in B and C and six additional emotion regulation sessions in C. The emotion regulation sessions were delivered during weeks 9 to 11 before the week 12 assessment. Other values are n/N (%). Adverse events affected 15 spouses from 14 couples; these are affected records rather than independently adjudicated event counts. Severity does not establish seriousness or causation.

MISSING DATA AND SENSITIVITY ANALYSES

Week 12 data were incomplete for at least one spouse in 14 couples. Lower baseline RDAS was observed among couples with incomplete week 12 scores in every group. At week 24, 29 couples lacked at least one spouse assessment. These patterns do not establish the missing at random assumption. Supplementary Table S5 presents baseline RDAS by later availability.

The observed complete couple primary difference was 3.99 points (95% CI 2.02 to 5.96). Treating the late visit as missing produced a difference of 3.86 points (95% CI 1.91 to 5.81). The differential scenario that subtracted six points from missing Group C scores and added six to missing Group B scores produced a difference of 2.99 points (95% CI 0.92 to 5.06). All 16 differential scenarios favoured Group C with intervals above zero. These shifts examine specified departures from the imputation assumption and do not cover all possible missing data mechanisms. The supporting mixed model estimated a week 12 difference of 4.07 points (95% CI 2.18 to 5.97) and a week 24 difference of 4.76 points (95% CI 2.84 to 6.68). Table 6 and Supplementary Tables S2 and S3 provide the sensitivity results.

Table 6. Sensitivity of the primary RDAS comparison

Analysis	Couples	C minus B	95% CI	P
Main analysis	150	3.95	2.00 to 5.91	<0.001
Observed complete couples	136	3.99	2.02 to 5.96	<0.001
Late visits treated as missing	150	3.86	1.91 to 5.81	<0.001
All missing scores minus 2	150	4.04	2.06 to 6.01	<0.001
All missing scores minus 4	150	4.12	2.10 to 6.14	<0.001
All missing scores minus 6	150	4.20	2.12 to 6.29	<0.001
C minus 6 and B plus 6	150	2.99	0.92 to 5.06	0.005

Differences are in RDAS points. The complete couple model used 136 couples; other analyses included 150. Shifts apply only to originally missing week 12 spouse scores. The late visit sensitivity treats both assessments on day 92 as missing. Sensitivity p values are unadjusted. The full differential grid is in Supplementary Table S2.

DISCUSSION

Couples receiving the combined communication and emotion regulation programme had higher marital adjustment than those receiving communication training alone at week 12. The adjusted difference was 3.95 RDAS points and persisted at week 24. Later differences in depressive symptoms, anxiety and regulation difficulties were also observed in the combined programme group. The additional difference in demand and withdrawal did not pass multiplicity correction. The principal finding therefore concerns marital adjustment and does not establish improvement in every aspect of marital conflict.

The relational benefit is consistent with the broader couple intervention literature. Joseph et al. (2025) reported a between group effect of d = 0.85 across interventions for marital distress. Roddy et al. (2020) reported a within treatment effect of g = 1.12 for relationship satisfaction. The present standardised difference of 0.62 uses baseline couple RDAS variability and compares two active programmes. These estimates have different denominators and comparators and should not be ranked as directly equivalent treatment effects. [3,4]

Adler-Baeder et al. (2022) evaluated two relationship education programmes among 929 couples and found improvements in relationship skills, quality and family harmony. ELEVATE also improved mental health and sleep. Le et al. (2023) reported satisfaction effects of d = 0.46, 0.40 and 0.47 against a waitlist for three OurRelationship support formats among 740 couples. Their active formats did not differ reliably in change. Doss et al. (2016) reported a satisfaction effect of d = 0.69 in a trial of 300 couples. These trials support relationship interventions but differ from the present additional module comparison in population, contact and delivery. [16,17,27]

Demand and withdrawal findings deserve separate consideration. Deylami et al. (2021) reported reductions in demand and withdrawal after online Gottman psychoeducation with changes of 51% for husbands and 65% for wives. Those percentages are reported changes rather than adjusted effects between active programmes. Trillingsgaard et al. (2025) compared OurRelationship with bibliotherapy among 39 Danish parent couples and reported a conflict difference of b = -4.01 ($p < 0.001$; d = 0.79). Their comparison also included coaching in only one group. Differences in measure construction and treatment contact limit comparison with the present six item communication composite. [15,28]

The Indian trial by Gayathri et al. (2026) provides a clinically relevant comparison because it addressed marital quality and psychological wellbeing in 60 couples with primary infertility. Dyadic Adjustment Scale scores increased from 90.8 to 97.4 in the combined intervention group and from 91.5 to 92.3 with standard counselling. General Health Questionnaire scores decreased from 26.0 to 19.6 and from 25.8 to 24.7 respectively. The displayed tests assessed changes within each group. A significant change in one group and a nonsignificant change in another do not establish a treatment difference. Infertility and the use of the full adjustment scale also distinguish that trial from the present study. [14]

Hatch et al. (2022) compared OurRelationship and ePREP with a waitlist among 671 couples and found relational benefits for both programmes without reliable differences between them. Darwiche et al. (2023) compared two brief therapies among 101 parent couples with six sessions in each programme. Both groups improved across marital and other domains without significant between group differences. These active comparator findings support careful interpretation of incremental effects. The absence of corrected differences between communication training and usual counselling in this study does not demonstrate that the interventions are equivalent. [29,30]

The psychological symptom findings were more evident at week 24 than at week 12. Group C had lower PHQ-9, GAD-7 and DERS-16 than Group B at the later visit after correction. Doss et al. (2016) reported depression and anxiety effects of d = 0.71 and 0.94 among participants symptomatic at baseline; those estimates did not apply to the whole sample. Wittenborn et al. (2019) examined emotionally focused therapy and usual care for depression in a small couple trial and identified different temporal patterns of relationship and depressive change. These findings support joint assessment of relational and individual outcomes but do not establish a uniform sequence of improvement. [27,31]

White VanBoxel et al. (2024) examined baseline regulation difficulties in the parent trial underlying the Wittenborn analysis. Higher baseline difficulties were associated with lower relationship satisfaction but the association with subsequent rate of improvement was not significant. The two publications are analyses of the same trial rather than independent replications. They used a longer DERS measure and a different adjustment subscale. The present associations between RDAS improvement and regulation improvement are also exploratory. They do not establish that regulation change mediated the treatment effect. [9,31]

The additional six sessions in Group C are a central limitation. Each emotion regulation session lasted 75 minutes, which added 450 minutes of planned therapist contact. At the scheduled maximum, communication training alone involved 720 minutes and the combined programme involved 1170 minutes. More contact may provide additional attention, rehearsal and opportunities to discuss difficulties. The observed between group difference reflects the combined programme as delivered and cannot isolate the regulation techniques from additional treatment exposure. Attendance records describe session counts rather than actual minutes received. A future comparison with matched contact would address whether regulation content provides a benefit beyond additional therapeutic time.

The Indian context adds relevance but requires cautious generalisation. Raj et al. (2022) reported adjusted ratios of odds ratios for contraceptive communication of 1.9 at nine months and 2.7 at 18 months in CHARM2. Dixit et al. (2023) analysed communication within the same cohort and Rathod et al. (2024) assessed marital adjustment alongside depression in men with diabetes. These studies support attention to couple communication and clinical context. They do not provide a direct estimate for the present communication and regulation intervention. The present sample was drawn from a psychiatry outpatient service and does not represent all married couples. [11-13]

The primary estimate was statistically convincing within the completed analysis but its clinical importance is uncertain. No validated minimal important RDAS difference was supplied for this population. Couple mean symptom scores also obscure individual variation and should not be interpreted as diagnostic remission. The week 24 DERS-16 comparison had a Holm p value of 0.049 and requires replication. Pointwise intervals for secondary outcomes can exclude zero even when the corresponding corrected test is not significant; interpretation should follow the stated multiplicity procedure.

Intervention group assignment followed a fixed, predictable A-B-C cyclic sequence rather than randomisation or concealed allocation. Although this avoided assignment based explicitly on participant preference or clinician choice, selection effects, temporal recruitment effects and residual confounding from measured or unmeasured baseline characteristics cannot be excluded. Adjustment for baseline RDAS does not provide the protection against confounding afforded by concealed random assignment. [18] Analysis at the couple level addressed dependence between partners. Multiple imputation preserved all 150 enrolled couples and sensitivity analyses examined specified departures from the missing data assumption. Week 24 complete couple data were available for 121 of 150 couples. The sensitivity grid did not reverse the primary result but it cannot exclude bias under every possible missingness mechanism. Outcome totals without item responses limited independent scoring verification. The Hindi RDAS, CPQ-SF and DERS-16 underwent translation, expert review and comprehension piloting but internal consistency was not assessed. Language and administration mode were not recorded, which precluded language specific analyses. The 24 pilot couples entered the study only after fresh baseline assessments using the final frozen versions; the pilot responses were not used as study outcomes. The pilot process did not establish full psychometric validation of the translated measures. [21,26]

Outcome assessors were kept unaware of intervention group, but the success of assessor blinding was not formally assessed because group assignment guesses were not recorded. The study was not registered, which limits independent verification of prospective outcome specification. The investigator confirmed that the primary comparison, outcome hierarchy and statistical methods were specified before recruitment without changes; the computational analysis document for this manuscript was prepared after collection. Independent assessment of adherence during actual treatment sessions also requires documentation.

The violence disclosure and subsequent treatment discontinuation show the importance of safeguarding within couple interventions. The couple retained consent for data use and remained in the primary analysis according to its initially assigned intervention group. Affected spouse and couple counts provide descriptive safety information but do not establish treatment relatedness or the rate of formally adjudicated serious adverse events. Equal scheduled safety inquiries reduced differences in assessment opportunity; additional treatment contacts could still affect detection. Interpretation of benefit should therefore consider the safety procedures and limitations of the available event records. [25]

Further research should compare programmes with matched treatment contact and verified language versions of all measures. Assessment of individual partner outcomes alongside couple means could clarify whether both spouses benefit. A prospectively specified assessment schedule for potential mediators would provide stronger evidence about how changes in regulation and communication relate to marital adjustment.

CONCLUSION

Couples receiving communication training with additional emotion regulation skills showed greater marital adjustment at week 12 and week 24 than those receiving communication training alone. Week 24 differences in depressive symptoms, anxiety and regulation difficulties were also observed in the combined programme group. An additional difference in demand and withdrawal was not established after correction. Because intervention assignment was non-randomised and predictable, residual confounding cannot be excluded. The greater treatment contact in Group C also limits attribution to emotion regulation content alone.

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DECLARATIONS

FUNDING

No external funding was received for this study.

CONFLICTS OF INTEREST

The authors declare no conflicts of interest.

DATA AVAILABILITY

Deidentified study data can be shared by the corresponding author on reasonable request. The study protocol, analysis and intervention materials are available from the authors on request. Patient confidentiality will be maintained.

Supplementary material

Supplementary Table S1. Assessment availability

Visit	Group	Spouses assessed	Both spouses	One spouse	Neither spouse
Baseline	A	100/100	50	0	0
Baseline	B	100/100	50	0	0
Baseline	C	100/100	50	0	0
Week 12	A	90/100	45	0	5
Week 12	B	90/100	44	2	4
Week 12	C	94/100	47	0	3
Week 24	A	75/100	37	1	12
Week 24	B	82/100	40	2	8
Week 24	C	88/100	44	0	6

Both, one and neither refer to numbers of couples out of 50 in each group. At least one spouse assessment was available in 138 couples at week 12 and 124 at week 24. Available spouse counts were 274 and 245 respectively.

Supplementary Table S2. Differential missing score sensitivity

C points subtracted	B plus 0	B plus 2	B plus 4	B plus 6
0	3.95 (2.00 to 5.91)	3.75 (1.80 to 5.71)	3.55 (1.58 to 5.52)	3.35 (1.35 to 5.35)
2	3.83 (1.87 to 5.80)	3.63 (1.66 to 5.60)	3.43 (1.44 to 5.42)	3.23 (1.21 to 5.25)

C points subtracted	B plus 0	B plus 2	B plus 4	B plus 6
4	3.71 (1.73 to 5.70)	3.51 (1.52 to 5.51)	3.31 (1.30 to 5.32)	3.11 (1.07 to 5.15)
6	3.59 (1.57 to 5.62)	3.39 (1.37 to 5.42)	3.19 (1.15 to 5.23)	2.99 (0.92 to 5.06)

Cells show the C minus B RDAS estimate and 95% CI. Row values are points subtracted from originally missing Group C scores; column values are points added to originally missing Group B scores. All models include 150 couples. Observed scores are unchanged.

Supplementary Table S3. Supporting repeated assessment model

Visit	C minus B	95% CI	P
Week 12	4.07	2.18 to 5.97	<0.001
Week 24	4.76	2.84 to 6.68	<0.001

The model used 519 postbaseline assessments from 276 spouses in 138 couples and included random intercepts for couple and spouse. Fixed effects included baseline RDAS and group, visit and spouse role terms. Intervals are approximate Wald intervals.

Supplementary Table S4. Exploratory associations with RDAS improvement

Measure	Week	Couples	Coefficient	Bootstrap 95% CI	BH P
CPQ-SF	12	136	0.13	0.02 to 0.26	0.039
PHQ-9	12	136	0.67	0.47 to 0.87	<0.001
GAD-7	12	136	0.67	0.38 to 0.97	<0.001
DERS-16	12	136	0.30	0.20 to 0.40	<0.001
CPQ-SF	24	121	0.21	0.10 to 0.32	<0.001
PHQ-9	24	121	0.61	0.32 to 0.91	<0.001
GAD-7	24	121	0.40	0.02 to 0.78	0.058
DERS-16	24	121	0.26	0.15 to 0.36	<0.001

Coefficients are RDAS improvement per point improvement in the explanatory measure after adjustment for baseline RDAS, baseline explanatory score and group. Intervals use 2000 couple bootstrap samples. HC3 tests underwent Benjamini and Hochberg correction across eight comparisons. The week 24 GAD-7 bootstrap interval and corrected p value use different inference methods. These associations do not establish mediation.

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Supplementary Table S5. Baseline RDAS and later missing scores

Week	Group	Both spouses observed	At least one score missing
12	A	39.33 ± 4.96 (n=45)	34.70 ± 6.62 (n=5)
12	B	40.34 ± 7.02 (n=44)	34.67 ± 3.33 (n=6)
12	C	39.65 ± 5.96 (n=47)	37.17 ± 3.69 (n=3)
24	A	38.55 ± 5.03 (n=37)	39.77 ± 6.00 (n=13)
24	B	40.09 ± 7.19 (n=40)	37.95 ± 5.71 (n=10)
24	C	39.85 ± 6.01 (n=44)	36.92 ± 3.94 (n=6)

Values are baseline couple RDAS mean ± SD with couple counts. The association between baseline score and later availability does not establish the missing at random assumption. No additional significance tests were performed.