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Research Article

Effect of Mental Health Training on Behavioral Intentions Among Primary Healthcare Workers in Iraq: A Randomized Controlled Trial

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Background

In Iraq, an estimated 20% of the population lives with a mental disorder, yet approximately 90% do not receive treatment. To address this treatment gap, primary healthcare workers (PHCWs) are essential; however, their involvement is hindered by stigma and a lack of training.

Objective

This study aims to assess the effect of brief mental health training on the behavioral intentions of PHCWs.

Methods

We conducted a randomized controlled trial in a conflict-affected region between November 2024 and August 2025. A total of 174 PHCWs were randomly assigned to either receive four days of intensive mental health training ($n = 87$) or continue their usual clinical work without additional training ($n = 87$). We measured participants' behavioral intentions (willingness to engage with people experiencing mental health problems) using the validated Reported and Intended Behaviour Scale at baseline and three months after the intervention.

Results

By the end of the study, 162 participants remained ($n = 81$ per group; 6.9% dropped out). Those who received training reported significantly stronger intentions to support people with mental health conditions (median = 16) compared to the control group (median = 15; $p = 0.007$; moderate effect size [$r = 0.30$]). Within-group analysis revealed large improvements in the intervention group ($p < 0.001$, $r = 0.52$) but no significant change in controls ($p = 0.218$). The proportion achieving high intended behavior increased from 30.9% to 56.8% in the intervention group versus 33.3% to 46.9% in controls.

Conclusion

This randomized controlled trial provides preliminary evidence that brief mental health training significantly improves behavioral intentions among PHCWs in post-conflict settings. Because behavioral intentions explain approximately 30–50% of behavioral variance, these findings support the need for larger trials evaluating actual clinical practice changes and patient outcomes.

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1. INTRODUCTION

Mental disorders have become a common health burden. During the COVID-19 pandemic, the incidence of depression and anxiety worldwide exceeded 50 million cases.[1] The pressure on healthcare workers has increased substantially. They experience an excessive workload, emotional burden, and repeated exposure to trauma.[2] The pandemic revealed underlying failures in mental health support for the healthcare workforce. Primary healthcare workers (PHCWs), in particular, face a distinct situation. They often deliver psychosocial care at the point of need, and they must also endure significant personal psychological distress.[3] Because PHCWs may be both caregivers and psychologically vulnerable, interventions should go beyond competency-building and proactively influence behavioral intentions as well as the psychological processes that support sustained changes in practice.

In Iraq, this need is particularly critical. Decades of war, instability, and a fragmented healthcare system have left the country with one of the highest burdens of mental health disorders in the Eastern Mediterranean region.[4] About one-fifth of Iraqis live with a diagnosable mental health condition, yet fewer than one-tenth receive any form of treatment,[5] indicating that approximately 90% remain untreated. According to recent studies, one-third of primary healthcare employees in Baghdad report moderate to severe depression or anxiety symptoms,[6] and similar findings have been reported in the Kurdistan region.[7] In the absence of empowering, stigma-reducing interventions alongside evidence-based treatment strategies, the mental health treatment gap in Iraq will persist, further exacerbating population-level suffering.

One solution to such workforce shortages has been task-shifting. The strategy shifts some of the mental health care responsibilities of specialists to non-specialists and increases access to mental health services in low- and middle-income countries (LMICs).[8] It has been shown that structured training programs can raise knowledge levels, reduce stigma, and have a positive impact on practice.[9] Nevertheless, the majority of such evaluations have been in high-income or stable LMIC settings, and very few have been undertaken in fragile or post-conflict environments. This limits our understanding of the effectiveness of these strategies in contexts where health systems experience chronic instability and limited resources.[10]

According to Ajzen,[11] the theory of planned behavior can be used to understand how training may influence practice. It states that attitudes, social norms, and perceived control are the primary determinants of actual behavior, with intentions mediating their influence. Meta-analyses indicate that intentions predict approximately 30–50% of behavioral outcomes.[12] Therefore, measuring changes in intentions represents an effective and affordable way to estimate the likelihood of achieving subsequent practice changes following an intervention.[13] Although training tends to increase knowledge and attitudes, fewer studies have examined how training influences behavioral intentions, a validated intermediate outcome that predicts, but does not guarantee, real-world behavior change.

This gap restricts our understanding of whether evidence-based training methods can be applied to post-conflict settings. Current Iraqi mental health capacity-building initiatives are mainly led by specialists and often fail to offer rigorous evaluation frameworks that target primary care providers, a gap that remains critical for effective implementation.[4]

A critical evidence gap therefore remains: no randomized controlled trial has assessed whether structured mental health training interventions can enhance PHCWs' intended clinical behaviors in Iraq or comparable conflict-affected settings. Addressing this gap is important because, while improvements in knowledge and attitudes are valuable, behavioral intention is a well-established, theory-based predictor of actual clinical practice,[14] and measuring intention change represents a necessary step before resource-intensive observational studies of practice translation. This study evaluated whether a culturally adapted mental health training program could improve the behavioral intentions of PHCWs in Iraq using a randomized controlled trial design, hypothesizing that PHCWs receiving the intervention would demonstrate significantly greater improvements in intended clinical behaviors compared with those receiving usual practice.

2. MATERIALS AND METHODS

2.1. DESIGN

This study employed a parallel-group, two-arm randomized controlled trial design with a 1:1 allocation ratio. The study was conducted in Kirkuk, Iraq, where PHCWs were recruited through governmental primary healthcare facilities that included psychosocial health units, health promotion teams, and school health programs. Recruitment and baseline assessments were conducted in November 2024. The intervention was delivered in December 2024, and participants were assessed at three months post-intervention (February–March 2025). Final data analysis and study closeout were completed in August 2025. All training sessions were standardized and conducted at the local university college of nursing.

2.2. STUDY ETHICS

Good Clinical Practices and the principles of the Declaration of Helsinki were adhered to during the study. The study protocol, informed consent procedure, and all relevant documentation were reviewed and approved by Hawler Medical University Ethics Committee (No. 173, June 2024) in June 2024. Oral informed consent, approved by the ethics committee, was obtained from all participants prior to enrollment. The trial was prospectively registered at ClinicalTrials.gov (NCT06655792).

2.3. PARTICIPANTS

Inclusion criteria included government-employed primary care providers who are currently involved in providing clinical services in the participating units. Exclusion criteria included participation in the pilot study, non-clinical/administrative roles, unavailability during the training period, and prior formal, multi-day mental health training within the past year.

Health sector supervisors used a systematic review of administrative records to identify eligible providers (n = 293). All participants were informed verbally about the study procedures, objectives, risks, and benefits, and provided oral informed consent prior to enrollment. The oral consent methodology was employed to accommodate

different literacy levels and cultural considerations, in accordance with ethics provisions set by the institutional review committee.

2.4. PARTICIPANT FLOW

Out of 293 providers screened, 180 were selected using simple random sampling (Randomizer.org). In total, 119 were excluded before baseline assessment (82 not selected, 31 ineligible, and 6 lost), leaving 174 participants who were randomly assigned to the intervention (n = 87) or control (n = 87) group. During follow-up, 12 participants were lost (n = 6 per group), resulting in a final analytic sample of 162 (n = 81 per group; 6.9% attrition). Figure 1 presents a detailed description of the flow of participants.

2.5. INTERVENTION

The intervention included eight 50–60-minute sessions on four consecutive days (December 2024) and covered mental health awareness, common disorders, stigma reduction, therapeutic communication, case vignettes based on the Iraqi context, and resource navigation. The culturally adapted materials were used to deliver the content through lectures, discussions, role-plays, and videos.

The training was delivered by two qualified facilitators, both holding Doctor of Philosophy degrees in psychiatric nursing. One served as an assistant professor, while the other served as a lecturer and was World Health Organization (WHO) Mental Health Gap Action Programme (mhGAP)-certified with 15 years of clinical training experience.

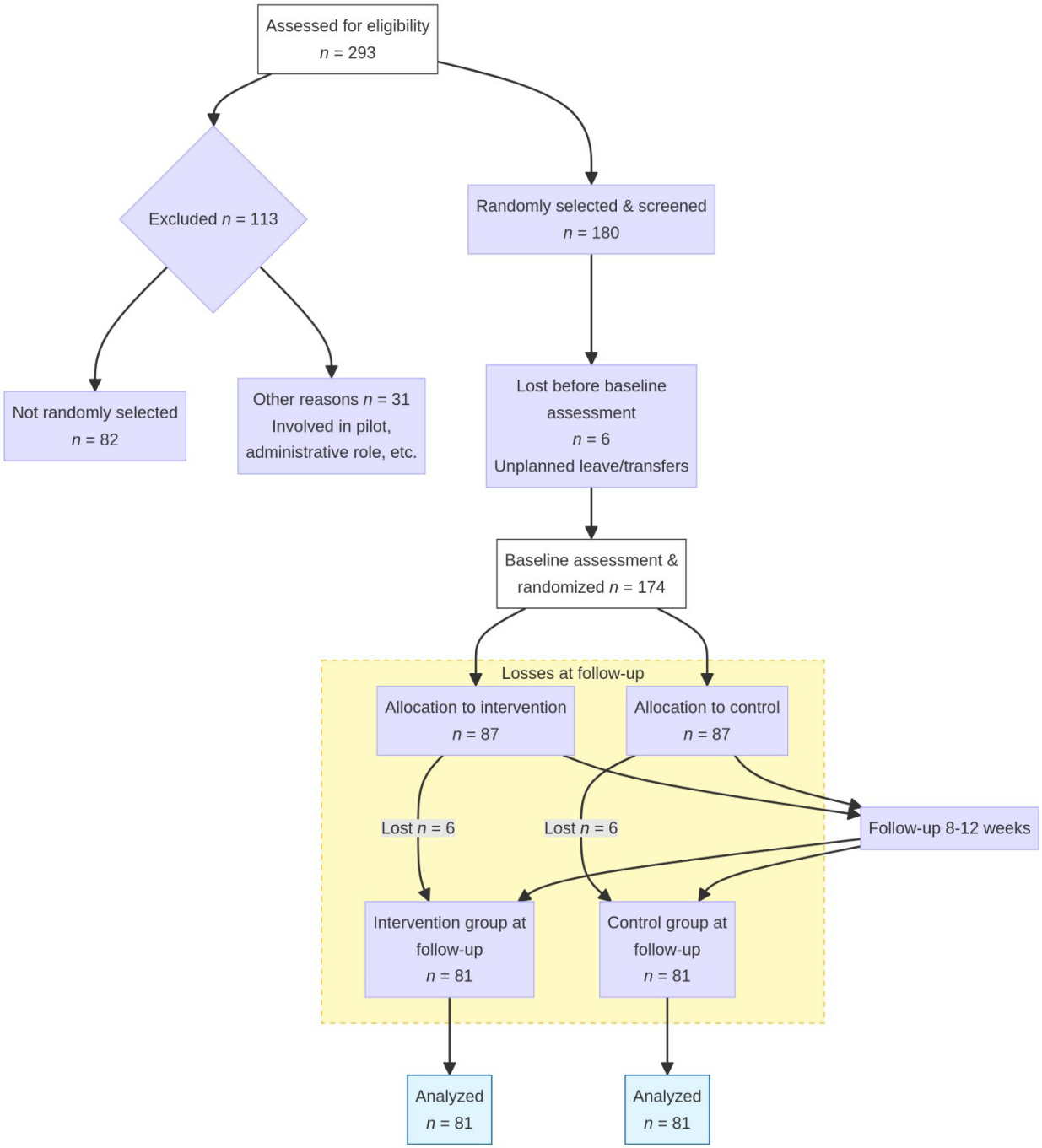

Figure 1. CONSORT flow diagram illustrating the flow of participants through each stage of the randomized controlled trial, from eligibility assessment to final analysis

The methods of cultural adaptation included: (i) all materials were translated and back-translated by bilingual mental health professionals; (ii) all materials went through an expert panel of 21 members (consisting of psychiatrists, primary care physicians, and cultural advisors); and (iii) integration of Iraqi clinical scenarios that reflected local help-seeking patterns and beliefs about mental illness.

The sessions were held offsite, and participants were instructed not to share training materials or discuss session content with colleagues. Control participants continued their usual clinical practice with access to general medical resources but did not receive any organized mental health training or supervision during the study. Control participants were assessed during follow-up to determine whether they had received any mental health training; there were no reports of contamination. There were no negative incidents in either group.

2.6. OUTCOME

The major outcome was mental health-related behavioral intentions, measured using the Reported and Intended Behaviour Scale (RIBS), a validated and widely used instrument to evaluate stigma-related behavioral intentions.[15] The scale includes eight questions—four questions measure reported past contact with individuals experiencing mental health issues (yes, no, or uncertain answers, scored descriptively), and four questions measure planned actions using a five-point Likert scale (1 = *strongly disagree* to 5 = *strongly agree*).

(i) Scoring: Intended-behavior scores range 4–20, with higher scores indicating stronger behavioral intentions. For descriptive purposes, the scores were categorized as low (4–10), moderate (11–15), and high (16–20). There was sufficient internal consistency in the sample (intervention: $\alpha = 0.82$; control: $\alpha = 0.81$).

(ii) Timing: The pre-test was administered in November 2024, and the post-test was administered three months after the intervention (February–March 2025). This interval allowed participants to integrate learning and minimize attrition in the conflict-affected environment and is consistent with previous studies showing that behavioral intentions do not change significantly over this period.[11]

The present study focused on behavioral intentions as the primary outcome, rather than direct observation of workplace behavior or measures of professional effectiveness, for several important reasons:

(i) According to the theory of planned behavior, intentions are validated intermediate outcomes that predict 30–50% of behavioral variance and represent a necessary precursor to practice change.[11,12]

(ii) Measuring actual clinical behavior requires resource-intensive methods (standardized patients, direct observation, and chart audits), which were not feasible in this conflict-affected setting during the study timeframe.

(iii) The intervention was designed to primarily address mental health knowledge, stigma reduction, and attitudinal change, with limited opportunities for skills-based training, supervision, or workplace reinforcement, which may influence intention more readily than observable clinical behavior.

(iv) The three-month follow-up period was insufficient to reliably capture sustained behavior change, given organizational constraints and staff turnover patterns in this context.[16]

2.7. SAMPLE SIZE AND POWER ANALYSIS

G*Power 3.1 (Heinrich-Heine-Universität Düsseldorf, Düsseldorf, Germany) was used to calculate the sample size for a two-tailed *t*-test analyzing two independent means. We used a medium effect size ($d = 0.5$), which represents a clinically meaningful change in behavioral intention scores that would be considered sufficient to drive implementation on a scale. We determined that 172 participants (n = 86 per group) would be sufficient to achieve $\alpha = 0.05$ and power = 0.90. Accounting for an expected 5–10% attrition, we recruited 180 participants. The final sample analyzed included 162 participants (n = 81 per group), representing a 6.9% attrition rate.

2.8. RANDOMIZATION AND BLINDING

(a) Randomization

Of 293 eligible providers, 180 were selected by simple random sampling; 174 met eligibility criteria at baseline and were randomized (1:1) using a computer-generated sequence prepared by an independent statistician. Allocation concealment was ensured using sequentially numbered, sealed, opaque envelopes that were identical in appearance and weight. Envelopes were opened only after confirming eligibility and completing baseline assessments. This procedure minimized selection bias.

(b) Blinding

It was not possible to blind participants or trainers due to the overt nature of the intervention. Nevertheless, outcome analysts were kept blind during data analysis. Datasets were coded numerically (Group 1/2). Group codes were revealed only after completion of the primary analyses. Code review and statistical script verification were conducted prior to unblinding to pre-specify analyses.

2.9. STATISTICAL METHODS

Baseline equivalence was assessed using independent *t*-tests (continuous variables) and chi-square/Fisher's exact tests (categorical variables). Given the ordinal nature of RIBS data and significant departures from normality (Shapiro–Wilk tests: all $p < 0.01$; positive skewness confirmed via Q–Q plots), non-parametric tests were employed:

(i) Between-group comparisons: Mann–Whitney *U* tests at baseline and follow-up.

(ii) Within-group changes: Wilcoxon signed-rank tests.

(iii) Effect sizes: Rank-biserial correlation (r: small ≈ 0.10, medium ≈ 0.30, large ⩾0.50).

Categorical distributions (low, moderate, or high) were examined descriptively. No statistical tests were applied to categorical distributions, as the continuous analysis represented the primary hypothesis test.

No adjustment for multiple comparisons was applied, as analyses addressed distinct a priori hypotheses: baseline equivalence verification (confirmatory), between-group comparison at follow-up (primary outcome), and within-group changes (exploratory mechanistic questions).

For missing data, a complete-case analysis was appropriate, as participants lost to follow-up (n = 12; 6.9%) did not

differ from completers in age (p = 0.564), gender (p =0.709), education (p = 0.681), or baseline RIBS (p = 0.447), supporting the missing-at-random assumption. Attrition was balanced between groups with similar reasons (unplanned leave/transfers).

All statistical analyses were performed using SPSS version 29.0 (IBM, USA). Statistical significance was set at α =0.05 (two-tailed).

3. RESULTS

A total of 162 providers participated in the study (n = 81 per group). The dropout rate was 6.9% and was equal in both groups. Table 1 shows that the two groups were similar in age, gender, education, and work experience, indicating successful randomization.

At baseline, both groups demonstrated equivalent RIBS scores (median = 13.00; interquartile range [IQR]: 11.0–16.0; Mann–Whitney U test: Z = −0.163, p = 0.870, r = 0.12), confirming successful randomization (Table 1) and comparable baseline RIBS score (Table 2). Categorical analysis showed that most participants exhibited low intended behavior (control: 53.1%, n = 43; intervention: 55.6%, n = 45), with fewer demonstrating moderate (control: 13.6%, n = 11; intervention: 13.6%, n = 11) or high (control: 33.3%, n = 27; intervention: 30.9%, n = 25) intentions (Figure 2).

Following the intervention, significant differences were observed (Table 2). The intervention group achieved higher median RIBS scores (16.00, IQR: 13.0–16.0) than the control group (15.00, IQR: 12.0–17.0), a statistically significant difference (Mann–Whitney U test: Z = −2.685, p = 0.007) with medium effect size (r = 0.30). The intervention group's narrower IQR suggests greater consistency in positive behavioral intentions.

Table 3 provides intra-group analyses. The training curriculum for the intervention group is presented in Table 4. Significant improvement in the intervention group (Wilcoxon: Z = 4.256, p < 0.001, r = 0.52) was observed, with 64.2% (n = 52) showing positive changes, 19.8% (n =16) showing negative changes, and 16.0% (n = 13) showing no change. Conversely, the control group showed no significant change (Z = 1.232, p = 0.218, r = 0.21), with 27.2% (n = 22) positive, 17.3% (n = 14) negative, and 55.6% (n = 45) unchanged.

The changes in the intervention groups are illustrated in Figure 3: high intended behavior increased from 30.9% (n = 25) to 56.8% (n = 46), while low intended behavior decreased from 55.6% (n = 45) to 7.4% (n = 6). The control group showed modest changes: high intended behavior increased from 33.3% (n = 27) to 46.9% (n = 38), with low intended behavior decreasing from 53.1% (n = 43) to 12.3% (n = 10).

4. DISCUSSION

This randomized controlled trial offers initial evidence that intensive mental health training condensed into short periods can provide significant benefits to behavioral intentions among PHCWs in a post-conflict environment, and effect sizes indicate that the observed changes could have practical implications for practice, pending observational research. Median follow-up RIBS scores were higher in the intervention group (median = 16, IQR: 13–16) compared to the control group (median = 15, IQR: 12–17; p = 0.007, r = 0.30, medium effect size), consistent with the significant between-group difference. Intra-group analysis revealed substantial intervention effects (r = 0.52, p < 0.001), which were significantly higher than levels of intervention effects typically considered practical in meta-analyses of healthcare worker social norms intervention.[17]

Nevertheless, there is a critical distinction: while behavioral intentions improved, they are not sufficient for practice change. As noted by Gupta, Li, Dong and Acri,[18] the majority of training assessments are based on self-reported intentions; therefore, there is a significant gap between intentions to translate the gained knowledge and actual behavior change. Behavioral intentions only predict 30–50% of behavioral variance.[19] Organizational barriers—such as time constraints, lack of supervision, and resource limitations—often prevent even motivated providers from applying new skills.[18] The transfer of these intentions to clinical practice is yet to be demonstrated through future observational studies, using standardized patients, direct observation, or chart review.[20]

As an exploratory observation, the intervention produced not only upward changes in median intentions but also a reduction in score variability: the intervention IQR decreased from 5 points at baseline (11–16) to 3 points post-intervention (13–16), whereas the control IQR remained unchanged at 5 points (12–17). This pattern suggests increased convergence in behavioral intentions following training; however, in the absence of direct measures of social norms or group processes, it should not be interpreted as definitive evidence of norm change.

Alternative explanations, including ceiling effects, regression to the mean, or measurement artifacts, cannot be excluded and warrant further investigation.[11,14] When credible local trainers demonstrate what is perceived as new professional norms in cohort groups, variation drops as outliers are aligned with consensus norms. Such differences between the level of norm change in groups and in self-selected individuals have important implications for scalability, as interventions that diminish variation in homogeneous provider groups are more predictable than those whose benefits are limited to motivated self-selected individuals, which is also relevant to implementation science frameworks.[8]

This effect has been achieved by systematically targeting three behavioral determinants:[21]

(i) Capability: Developed through role-play simulations that provided procedural competence in therapeutic communication and mental health assessment, where high completion rates (93.1%) and high positive score change (64.2%) indicated effectiveness at this level, in line with the principles of adult learning.[9]
(ii) Opportunity: Using local psychiatric nursing faculty as trainers, leveraging the "credible source" technique to enhance engagement.
(iii) Motivation: Provided via intensive immersion and positive peer norms.

Although we did not directly measure mediation, these design features likely explain why our effects exceeded those seen in other social norms interventions;[17,19] these proposed mediators were, however, not formally measured using mediation analysis and should be a priority in future research to establish the mechanistic pathways.[22]

There are several implementation benefits of the strategic selection of local psychiatric nursing faculty as trainers. First, it increased the intervention credibility by increasing source legitimacy, as the trainers understood the professional background of participants, were aware of the local

Table 1. Sociodemographic characteristics of the participants

No.	Characteristics		Control group (*n* = 81)	Intervention group (*n* = 81)	*p*-value
			n (%)	*n* (%)	
1	Gender	Male	34 (42.0)	36 (44.4)	0.751[a]
		Female	47 (58.0)	45 (55.6)	
2	Age (years)	21–30	26 (32.1)	24 (29.6)	0.832[b]
		31–40	23 (28.4)	20 (24.7)	
		41–50	19 (23.5)	24 (29.6)	
		>50	13 (16.0)	13 (16.0)	
		Mean ± standard deviation	37.68 ± 11.1	38.59 ± 10.9	
3	Level of education	High school	9(11.1)	10 (12.3)	0.640[a]
		Diploma	49 (60.5)	47 (58.0)	
		Bachelor	19 (23.5)	16 (19.8)	
		Postgraduate	4 (4.9)	8 (9.9)	
4	Marital status	Single	14 (17.3)	14 (17.3)	0.756[a]
		Married	62 (76.5)	59 (72.8)	
		Widowed	4 (4.9)	5 (6.2)	
		Divorced	1 (1.2)	3 (3.7)	

Notes: [a] Chi-square ($\chi 2$) test; [b]Independent *t*-test.

Table 2. Pre- and post-test comparison of Reported and Intended Behaviour Scale scores between control and intervention groups (Mann–Whitney *U* test)

Factor	Groups	*n*	Median (interquartile range)	Mean rank	Z	*p*-value	Effect size (*r*)
Baseline	Control	81	13.00 (11.0–16.0)	82.10	−0.163	0.870	0.12; small
	Intervention	81	13.00 (11.0–16.0)	80.90			
Three-month follow-up	Control	81	15.00 (12.0–17.0)	71.66	−2.685	0.007	0.30; medium
	Intervention	81	16.00 (13.0–16.0)	91.34			

Note: Effect size was calculated using rank-biserial correlation.

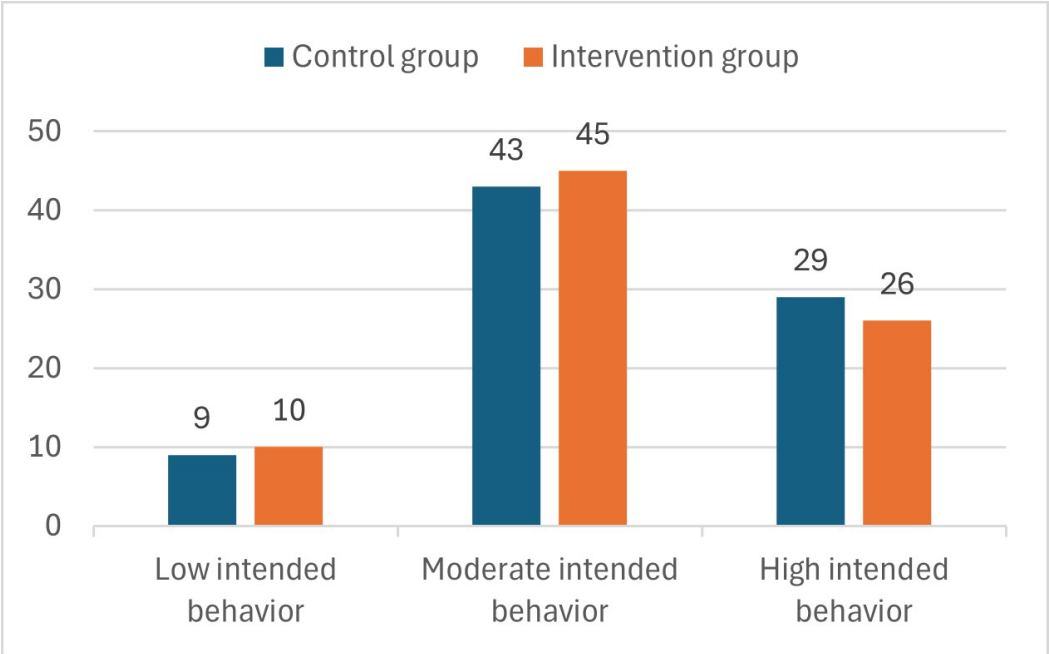

Figure 2. Pre-intervention distribution of intended behavior scores toward individuals with mental health disorders among intervention and control groups

Table 3. Within-group changes in Reported and Intended Behaviour Scale scores following intervention: A Wilcoxon signed-rank test

Variable	Ranks	*n*	*Z*	*p*-value	Effect size (*r*)
Control group	Negative ranks	14	1.232	0.218	0.21; small effect
	Positive ranks	22			
	Ties	45			
Intervention group	Negative ranks	16	4.256	<0.001	0.52; large effect
	Positive ranks	52			
	Ties	13			

Note: Effect size was calculated using rank-biserial correlation.

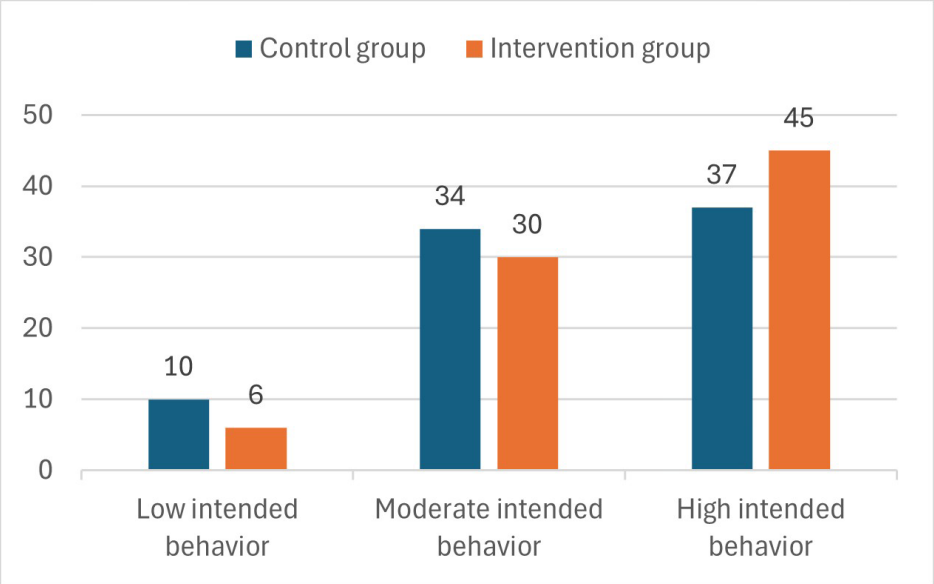

Figure 3. Post-intervention distribution of intended behavior scores toward individuals with mental health disorders among intervention and control groups

healthcare challenges, and could provide context-specific examples.[23] Second, it proved to be feasible: the intervention did not require international consultants or other specialized personnel, which contributes to scalability in resource-limited settings.[8] Third, it was consistent with evidence that locally provided training yields better results in post-conflict contexts due to cultural resonance and sustained access to follow-up support.[22]

The principle behind this approach—leveraging local capabilities rather than bringing in foreign instructors—represents an implementable model of capacity-building in weak health systems that can be generalized, as it adheres to WHO guidelines on task-shifting in LMICs.[24,25] Regional evidence also supports cross-context validity in post-conflict settings. For example, Syrian aid workers were trained to enhance the psychological first aid in interactive, practice-based formats,[22] whereas WHO-provided mhGAP training substantially enhanced healthcare workers' confidence and diagnostic skills, resulting in higher levels of mental health service utilization.[26] These overlapping observations in similar environments indicate that peer-based norm-setting, credible local instructors, and intensive immersion can be generalizable principles for capacity-building in weak healthcare systems.[8]

The post-conflict context of this study may have paradoxically enhanced training effectiveness through multiple mechanisms. The visible acute mental health needs (20% prevalence, 90% untreated) created clinical urgency and reflective motivation, potentially absent in well-resourced, stable healthcare system settings.[4,5] Additionally, Hasan *et al.*[27] found that advancing age was correlated with more open-minded attitudes in the study region, suggesting that our sample of practicing, likely older PHCWs (mean = 38 years; Table 1) possessed developmental receptivity. These contextual factors align with evidence from post-conflict mental health programs showing that acute visible need and peer engagement can enhance training engagement.[22,23] However, these contextual advantages may not generalize universally across post-conflict settings with different demographic profiles, conflict timelines, or organizational contexts, consistent with implementation science literature noting substantial context dependence in global health interventions.[8]

However, the response was not universal. Based on Table 3, 19.8% (n = 16) of intervention participants showed negative rank changes. This heterogeneity reflects multiple moderators requiring investigation in future research, consistent with meta-analytic findings showing high variability in training intervention effectiveness (I^2 = 85.4%).[17] First, some providers experienced "learned pessimism"—recognizing through training the profound gap between what they learned and what they could feasibly deliver given

Table 4. Eight-session mental health training curriculum

Session number	Session title	Content
1	Introduction to mental health	Definitions of mental health/illness; mental health determinants; facts about global mental health burden; awareness days
2	Overview of disorders	Depression, bipolar disorder, anxiety, schizophrenia, eating disorders, grief disorder, and substance use disorders; symptoms and impacts
3	Understanding stigma	Definition of stigma types (public, self, structural, associative); cognitive, emotional, and behavioral components; stereotyping and labeling mechanisms
4	Stigma in the healthcare setting	Manifestations of stigma in healthcare; provider attitudes' impact on trust and care; diagnostic overshadowing; clinical objectivity
5	The impact of stigma	Effects on diagnosis, treatment delays, and service access; consequences for health outcomes; barriers to effective treatment
6		Individual, provider, and systemic barriers; personal and systemic effects; local context (Iraq/Kirkuk challenges)
7	Overcoming public and self-stigma in healthcare	Good practices; respectful language; reducing public and self-stigma; communication strategies; empowerment
8	Anti-stigma campaigns and training	Role of education; designing campaigns and staff training; global campaign examples; implications for intervention

system constraints.[18] Second, conflicting professional priorities (overwhelming patient loads, competing health programs) framed mental health as an additional burden, as documented in resource-limited settings where workload compression reduces training adoption.[16]

Control group dynamics (Table 3) also warrant interpretation: 27.2% improved (likely reflecting secular trends, peer discussion, or regression to the mean), 17.3% worsened (possibly stigma exposure without counter-narratives), and 55.6% remained unchanged—confirming effective wait-list control functioning, consistent with prior meta-analyses showing 20–30% improvement in controls without structured intervention.[19]

5. IMPLICATIONS AND FUTURE DIRECTIONS

Intensive in-person training is feasible in post-conflict settings. With approximately 1.6 billion people in fragile settings globally facing severe mental health treatment gaps, this scalable model—requiring only local faculty and minimal equipment—can be deployed through existing infrastructure. Task-shifting to non-specialists is viable when training addresses capability, opportunity, and motivation simultaneously, supporting WHO mhGAP strategies.[8] However, training must be embedded within broader system-strengthening efforts; without concurrent improvements in supervision, workload, and retention support, training gains are lost to attrition.

Three future research priorities incorporating explicit effectiveness criteria and evaluation methods are proposed:

(i) Behavioral translation studies with 12–24 months follow-up using direct observation, standardized patients, and patient-reported measures to evaluate whether improvements in intention translate into core clinical behaviors, including mental health screening, appropriate referral, and quality of therapeutic communication.

(ii) Sustainability and implementation studies with longer follow-up (⩾24 months) examining persistence of practice change and key organizational moderators (e.g., supervision, workload, and institutional support), as well as cost-effectiveness and equity outcomes, using established implementation science frameworks.

(iii) Cluster-randomized trials across post-conflict primary care settings to assess scalability, contextual adaptation, and patient-level outcomes, including access to care and symptom improvement.

5.1. STRENGTHS OF THE STUDY

Study strengths include rigorous randomization with allocation concealment, minimal attrition (6.9%) balanced between groups, blinded outcome assessment, validated instrument (RIBS, $\alpha = 0.81–0.82$), prospective registration without protocol modifications, culturally adapted intervention, and transparent effect size reporting.

5.2. LIMITATIONS

Several limitations remain in this study and warrant consideration. First, we measured intentions rather than observed practice. While intention predicts behavior adoption, the gap is well-documented,[19] particularly in resource-constrained and high-pressure healthcare settings, necessitating observational follow-up. The training intervention primarily emphasized mental health knowledge, stigma reduction, and attitudinal change, with limited opportunities for skills-based practice, supervision, or workplace reinforcement, which may have contributed to improvements in intention without guaranteed translation into clinical behavior.

Second, the three-month follow-up period is insufficient

to assess the sustainability of behavioral intention changes, particularly given documented high staff turnover rates in Iraqi primary healthcare settings (annual turnover 18–35%). Longitudinal studies of 12–24 months are needed to determine whether intention changes persist, translate into sustained practice, and survive organizational disruptions, including staff attrition, workload fluctuations, and supervision gaps that characterize post-conflict health systems,[16] as the current follow-up is insufficient to evaluate long-term training effects.

Third, single-site recruitment limits direct generalizability, though focus on post-conflict settings enhances relevance for similar contexts. Fourth, absence of active attention control means observed effects may reflect both specific content and non-specific factors (peer interaction, trainer attention), though wait-list design was ethically appropriate and post-conflict constraints precluded active controls. Lastly, self-report measurement carries social desirability risk, though anonymous administration and 20% negative responses suggest reasonably candid reporting.

6. CONCLUSION

This trial demonstrates that brief, culturally adapted mental health training significantly improves PHCWs' behavioral intentions in post-conflict Iraq. The intervention achieved an upward shift in median intentions and a compression of variability, evidencing group-level norm change through systematic targeting of capability, opportunity, and motivation. However, training represents a necessary but insufficient condition for practice change. For populations in conflict-affected regions facing severe treatment gaps, sustainable impact requires concurrent organizational strengthening, including:

(i) The establishment of structured post-training supervision and mentoring within primary care facilities.
(ii) Allocation of protected time for mental health consultations within existing workload structures.
(iii) Implementation of motivation and retention strategies, such as continuing professional development credits, certification, and career advancement pathways.
(iv) Integration of mental health training into existing primary care and public health programs rather than creating parallel systems.

To translate behavioral readiness into accessible care and determine whether improvements in intention lead to patient-level benefits, future research should prioritize longitudinal observational studies with direct assessment of clinical practice and extended follow-up periods of 12–24 months.

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CONFLICT OF INTEREST

The authors declare they have no competing interests.

AUTHOR CONTRIBUTIONS

Conceptualization: All authors
Data curation: Nashwan Nadhim Shwan
Formal analysis: Nashwan Nadhim Shwan
Investigation: Nashwan Nadhim Shwan
Methodology: All authors
Writing – original draft: Nashwan Nadhim Shwan
Writing – review & editing: All authors

ETHICS APPROVAL AND CONSENT TO PARTICIPATE

Good Clinical Practice and the principles of the Declaration of Helsinki were followed throughout the conduct of this study. The study protocol, informed consent procedure, and all relevant documentation were reviewed and approved by Hawler Medical University Ethics Committee (No. 173, June 2024). Oral informed consent, approved by the ethics committee, was obtained from all participants prior to enrollment. This trial was prospectively registered at ClinicalTrials.gov (NCT06655792).

CONSENT FOR PUBLICATION

Informed consent was obtained from all individual participants included in the study for the publication of de-identified data. Because this study did not include any individual images, videos, or highly sensitive personal identifiers, the oral consent obtained prior to enrollment covered the dissemination of aggregated and de-identified research findings.

DATA AVAILABILITY STATEMENT

The data and trial protocol are available upon reasonable request from the corresponding author.

ADDITIONAL DISCLOSURE

The authors used QuillBot (an online paraphrasing and grammar tool) only for language editing and improving clarity.

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