An evaluation of an open group for depressed mood on a stroke rehabilitation ward: three years of clinical data

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Abstract

Purpose: An open group intervention for stroke inpatients, based on Acceptance and Commitment Therapy, is evaluated using retrospective clinical service data.

Materials and methods: Participants were included unless severely unwell or unable to provide informed consent. 117 participants attended at least two sessions in a non-controlled, repeated measures design. Two session protocols were delivered on alternating weeks by an Assistant Psychologist and Trainee Psychologist, covering values, committed action, and acceptance. Participants rated their mood each session using the Depression Intensity Scale Circles (DISCs).

Results: Attended sessions ranged from 1 to 11 (Md: 2). Significant reductions in DISCs scores with medium effect sizes were found among those scoring above the cut-off for depression at baseline, $X^2(3) = 20.87$, $p < .001$. The likelihood of scoring below the cut-off for depression did not change between participants' first and last sessions, $X^2(1, N=117) = 1.36$, $p=.24$. The number of sessions attended did not predict outcome, $rs(117) = .09$, $p = .33$.

Conclusions: Design limitations prevented inferences of clinical effectiveness, but the group met several clinical utility criteria by providing a flexible intervention on a rehabilitation ward with competing demands. We highlight the importance of contrasting findings of clinical trials with data from clinical services.

Acceptability of an open group for depressed mood on a stroke rehabilitation ward: three years of clinical data

Stroke is a life-threatening condition involving the disruption of blood supply to the brain, resulting in injury of brain tissue [1]. Stroke is one of the leading causes of adult disability in the UK [2] and can result in physical disability, cognitive impairment, fatigue and sensory disturbances [1].

Psychological distress, including depression and anxiety, is common in stroke survivors across the recovery span. Approximately one third of people experience depression following a stroke, compared with 7-12% in the general population [3–6]. Factors predictive of mental health difficulties following a stroke include level of disability, aphasia, mental health history and psychological adjustment [5,7–9].

Post-stroke depression can impede functional recovery, therefore increasing the chance of lifelong disability [10]. Accordingly, it is important that stroke patients have access to psychological interventions during their recovery [11]. Several authors emphasize the importance of psychological intervention in early stroke recovery, during the period when functional improvement is most significant [12,13]. Indeed, there is growing evidence to support the efficacy of psychological interventions in acute stroke recovery [13,14], and a Cochrane review indicated that they could be effective in a preventative capacity [15]. Despite its importance, there is evidence that only a minority of those with a diagnosis of a mental health condition after stroke report receiving psychological support, highlighting access as an issue [16].

Traditionally, group-based approaches to intervention delivery consist of a fixed schedule of sessions and content, with a defined cohort of attendees [17,18]. While this format enables the delivery of consistent and repeatable manualized interventions, there are associated limitations. In inpatient settings, immediate healthcare needs and variable lengths

of stay can make commitment to a fixed program difficult [19–21]. Missed sessions can result in a high rate of intervention drop-out and cause difficulty with understanding the content of later sessions that build on the content of earlier sessions [22]. Negative group dynamics can also contribute to high attrition rates and poorer intervention outcome [23].

Open-group approaches are, therefore, increasingly adopted in settings where closed groups are impractical [21,24,25]. Such formats have no attendance requirement or clinical cohort, instead supporting clients to attend an unspecified number of sessions flexibly. Typically, open-group sessions are designed to be standalone, so that missed sessions are less likely to detriment the learning outcomes of sessions that are attended [26]. There is emerging evidence that open-group clinical interventions are feasible in stroke settings, but more research is required [21,24].

There is increasing support for the use of Acceptance and Commitment Therapy (ACT) as a therapeutic paradigm in the context of stroke and acquired brain injury [27,28]. ACT may offer advantages, compared to traditional Cognitive Behavioral Therapy (CBT) for supporting the process of adjustment in physical health and long-term disability settings. For example, its focus on personal values and acceptance of distress may offer additional psychological resources for patients during a time of major personal transformation and adjustment. The evidence base for the effectiveness of ACT interventions in acute and post-acute stroke populations is currently still small, but two recent Randomized-Controlled Trials (RCTs) have reported promising results [18,29]. Both trials evaluated the efficacy of closed-group formats of intervention delivery on depression and other outcomes and were delivered in the community and on a ward, respectively. The interventions in these trials covered mindfulness, acceptance, values, and committed action [30]. A significant benefit from the intervention was found in both trials but only Majumdar and Morris (2019) reported effect

sizes, suggesting a medium-sized effect of the intervention on depression scores, relative to the treatment-as-usual control group.

It is important that evidence from clinical trials is supplemented by findings from routine outcome monitoring data because estimations of intervention outcome in studies using clinical service data have been found to substantially differ [31,32]. Furthermore, the group protocols used in clinical trials are often unfeasible in clinical contexts, where resource limitations, competing demands on participants' time, variability in functional ability, and other factors, are present.

No study has yet evaluated the clinical utility of an open group, using ACT principles, in inpatient stroke rehabilitation. The focus on separable processes contributing to psychological flexibility in ACT potentially makes the model an appropriate fit for an open group format [33,34]. The aim of this study was therefore to evaluate an ACT-based, open group for stroke rehabilitation inpatients, using routine clinical data collected over three years of delivery. We ask: a) is successive session attendance associated with a reduction in depression scores, b) is there a reduction in the proportion of those scoring above the cut off for depression between their first and last session, c) are reductions in depression associated with the number of sessions attended, and d) what is the clinical utility of the open-ACT group for stroke inpatients?

Methods

Design

Mood ratings were taken from participants each session, in an uncontrolled repeated-measures design, where changes in scores over time could be assessed. Because of the open-group format, participants were not clustered into cohorts. For example, a client attending their third session may share the group with participants attending their first session.

Participants

Data were collected from participants attending the group in a post-acute stroke rehabilitation ward in the East of England, between November 2016 and March 2020. Patients are referred to the ward upon reaching sufficient medical stability to engage in rehabilitation, following a radiologically confirmed stroke. The ward has an average length of stay of 33 days per patient, although the actual length of stay varies depending on individual circumstances.

Participants who met the criteria, outlined in Table 1, were invited to attend the group. Participants with moderate language or cognitive impairments, who were able to engage with reasonable adjustments and support from facilitators, were encouraged to attend. This includes moderate expressive and/or mild receptive aphasia, executive function difficulties, reduced processing speed or working memory capacity, or visual deficits. Data relating to the rates of non-participation were not collected, which limited the assessment of sampling bias.

- Table 1 near here -

A total of 244 participants attended at least one group session at the time of analysis, of whom 20 were excluded from the analysis because first-session data were missing. Because of the open-group format, an attrition rate would be expected where the number of sessions accumulated by each participant would gradually tail off due to discharge or if the patient decided to stop attending. For example, all participants would attend at least one session, fewer two sessions, fewer three sessions, and so on. Reasons for discontinuation were not recorded, but typically include discharge from the ward, changes to health status, improved mood, competing commitments, or declining to take part.

Procedure

Eligible participants were invited to attend the group during their ward stay. The size of the intervention groups varied with the number of available and eligible participants that day. Mean group size was 4.5 attendees (SD 1.5) and ranged between two and seven participants. Sessions lasted one hour and followed one of two protocols, an acceptance or values and

committed action session, which alternated weekly. The number of times each client received each respective session protocol was not recorded.

In the acceptance session, attendees were encouraged to share the emotions that they had experienced since the stroke. The quicksand metaphor [35] was used to illustrate the problems associated with fighting against emotions compared to the psychological process of acceptance [36]. Attendees were subsequently invited to engage in an experiential exercise, adapted from 'The Hexaflexercise' [35], where participants were guided to access the emotions discussed earlier in the session and respond to them with strategies designed to promote acceptance.

In the values and committed action session, attendees were asked to complete an adapted version of the Valued Living Questionnaire [37], which prompted participants to reflect on their respective values and how consistently they had been living by them. Attendees were asked to think of changes that could allow them to live more consistently with their values. Attendees ended the session by completing another experiential exercise adapted from a section of 'The Hexaflexercise' [35], that supports reflection on values and reinforces commitment to values-based action.

Significant session time was purposefully made available outside of the structured protocol. This additional group time was scheduled with the aim of supporting the accommodation of any cognitive or language difficulties among attendees, increasing contact between participants, reducing the sense of aloneness in the post-stroke experience, allowing participants to share suggestions about managing the effects of stroke, and supporting with the identification of those in need of one-to-one support.

Sessions were delivered by an assistant psychologist and, when available, a doctoral trainee clinical psychologist. A clinical psychologist provided absence cover and weekly supervision to the facilitators, separately, to monitor intervention delivery and clinical safety.

Separate time for training, outside of supervision, was not necessary, and so not a measurable cost; instead, the less experienced facilitator would shadow until competent to lead the session. The assistant and trainee facilitators changed over the three years, but the same clinical psychologist oversaw the entire period.

Measure

The DISCs is a single-item six-point visual analogue depression scale, where higher numerical scores indicate more severe depression.[38]. The scale is presented visually as six outer white circles with an inner grey circle; the relative size of the inner grey circle increases with increased ordinal hierarchy (i.e. depression severity), with the circle denoting 'most severe depression' being entirely grey and the circle indicating 'no depression' being entirely white.

The DISCs was formally incorporated into the session structure and administered to attendees mid-session. The mid-session positioning of the DISCs administration meant that any measurable changes to depression scores would only have been detectable in the DISCs scores of the following session.

Single-item ordinal measures, such as the DISCs, can be prone to poorer validity and sensitivity to change than multiple-item counterparts, because of their narrower scale of measurement [39–41], and are less likely to behave as interval measures, often warranting non-parametric statistics. At its optimal cut-off of ≥2, the DISCs was found to have inadequate sensitivity (60%) but high specificity (87%), indicating that it tends to miss a sizeable proportion depressed patients but is accurate in those that it identifies as experiencing depressed levels of mood [38]. Despite these weaknesses, the vertical presentation of the scale circles accommodates those with hemispatial neglect, and the minimal verbal loading supports those with acquired language difficulties. Its single-item length is preferable for those who cannot tolerate longer psychometric assessment, and for

swift collection of clinical outcomes in the context of a one-hour group session. These advantages make the DISCs favorable to use in the context of early stroke recovery.

Ethics

Data were collected and recorded by the service as part of routine outcome monitoring. The project was granted local approval from the host NHS trust for service evaluation purposes on 29th September 2020. Further ethical approval was provided by the University of East Anglia Faculty of Medicine and Health Sciences Research Ethics Committee (UEA FMH REC) on 12th November 2020. Data were handled and processed in accordance with GDPR and the Data Protection Act 2018.

Analysis

Because the DISCs is a single-item ordinal-level measure, non-parametric statistics were used**.** Participants scoring above the cut off for depression (scores ≥2) on the Depression Intensity Scale Circles (DISCs) in their first session were allocated to a 'depressed mood' group so that changes in those who were depressed could be evaluated [38]. To explore aim a of examining within-subjects changes in mood, Friedman's Test was conducted on data from the depressed group. Because of the proximity of the cut-off point to the floor of the measure and consequential risk of bias from floor effects, within-subjects changes were not evaluated in those who were classed as non-depressed at the start of group attendance. The Wilcoxon Signed-ranks Test was used for post-hoc analysis, to determine the sessions most associated with change. Bonferroni corrections were used to control for the family-wise error rate in post-hoc analyses.

McNemar's Test was used to explore aim b of examining changes in the proportion of participants scoring above the cut-off for depressed mood between the start and finish of group attendance. McNemar's Test determines paired-sample changes in marginal frequencies of depressed/non-depressed status pre- and post-intervention.

To explore aim c of examining an association between change scores and the number of sessions attended, Spearman's rho was used. The predictor variable was the number of attended sessions, and the dependent variable was the change in score between the first and last session attended.

To explore clinical utility, as per aim d, data submitted to the Stroke Sentinel National Audit Programme (SSNAP) was extracted to identify the total number of patients on the ward during data collection and, thus, the percentage that were able to attend the group. Furthermore, cost of delivery was calculated. To appraise utility, we referred to criteria outlined by Smart (2006), who suggests that utility includes dimensions of appropriateness, accessibility, practicability, and acceptability, each of which can be further subdivided [42]. Though some dimensions of utility in this model cannot be appraised with the available data, such as the clinical effectiveness aspect, SSNAP and clinical cost data can be used to appraise the accessibility, practicability, and relevance components. To be accessible, Smart (2006) suggests an intervention is recommended to have low resource implications, have available resources, and be financially easy to navigate; to be practicable, an intervention must have complete and deliverable materials/methods, and adequate training in the face of everyday constraints; and to be relevant, and intervention should have minimal impact on existing treatments or care, and be important for clinical decision-making.

Power Calculation

A medium effect size of 0.5 Cohen's *d* or 0.7 η^2 was used as the basis for power calculations, as these were found in Majumdar and Morris's ACT group study in stroke [18]. The number of within-subjects comparisons for the Friedman's test that could be made with the available dataset was calculated by adjusting repeated-measures ANOVA G*Power sample size estimates, using the asymptotic relative efficiency figures provided by Prajapati et al. (2010).

These are listed in table 3 below. Based on these figures, and the sample sizes shown in Table 3, data were sufficient to evaluate changes in up to four sessions.

- Table 2 near here -

For correlations between the number of sessions attended and the degree of pre-post change, an *r* value of 0.32 (r^2 = 0.1) was selected for power analysis because less than 10% shared variance with change scores is unlikely to be clinically meaningful and because an association of this magnitude has been reported in other psychotherapy studies analysing this effect [43]. These criteria would require a sample size of 74, which was achievable with the available dataset.

Results

DISCs data were inspected for normality using Q-Q plots and tests of skewness and kurtosis. Significant deviations from normality were found for DISCs scores in all sessions, substantiating the rationale for non-parametric statistics in analyses of changes to DISCs scores. Parametric statistics were used, where appropriate, for analysing demographic data below. Missing data were excluded pairwise, meaning a participant was included if an individual test did not require a specific missing datapoint, but otherwise excluded.

Sample characteristics

Only data on patient age and gender were available. Mean age of the remaining 224 participants was 74.5 years (*SD* = 11.4, range 28 to 94) and 51% (*n* = 115) of the sample were male. The median number of sessions attended was 2 with an interquartile range of 2. Table 2 outlines the sample size with increasing session attendance.

- Table 3 near here -

The number and percentage of people within each sample scoring ≥2 on the DISCs, stratified by the number of attended sessions, are also showed in Table 2. Those scoring above the cut-off on the DISCs in their first session, the 'depressed' group, were not found to differ in age from those scoring ≤ 1, the non-depressed group, $t(220) = .45$, $p = .65$. Equally in terms of gender, those in the depressed group (51.6% female) did not differ from those in the non-depressed group (46.6% female) $X^2(1, N=224) = .56$ $p=.46$.

Average session attendance for those in the non-depressed group (M = 2.11) was significantly lower than the depressed group, M = 2.80, $t(168) = -2.65$, $p=.009$, $d=.37$, with a small-to-medium effect size. Participants who attended a single versus multiple group sessions did not significantly differ by age, $t(220)$ = .47, p = .63, or by gender, X^2 $(1, N=224) = .14$, $p=.71$.

Is successive session attendance associated with a reduction in depression scores?

Friedman's Test was conducted on the depressed-only group for those who attended two, three and four sessions. Separate Friedman's tests were required for each level of session attendance because different participant populations are captured by each level; the two-session Friedman's test assesses changes in all participants who attended two *or more* sessions, while the four-session Friedman's test excludes those who attended fewer than four. Significant pair-wise changes to ranked DISCs scores were found on all levels in the direction of improvement, indicating a reduction in depressive symptoms amongst those in the baseline-depressed group (see Table 4).

- Table 4 near here -

Post-hoc analysis, using a series of Wilcoxon Signed-rank Tests with Bonferroni adjustments, indicated that first session DISCs scores (Mdn = 2) were significantly higher

than second-session (*Mdn* = 2), z = -3.281, p = .001, r = .31, third-session (*Mdn* = 2), z = -3.088, p = .002, r = .34, and fourth-session scores (*Mdn* = 1), z = -3.596, $p < .001$, r = .46, with medium effect sizes at each point of change.

While second-session DISCs scores were also significantly higher than fourth-session scores, z = -3.281, p = .004, r = .381, we did not find evidence for a significant reduction between the second and third, z = -1.840, p = .066, r = .20, and third and fourth sessions, z = -.406, p = .685, r = .05. Together, these analyses tentatively indicate a progressive decrease in depression scores with greater session attendance, for the depressed-only group.

Do those scoring above the cut-off for depression in their first session score below the cut-off in their last?

Data relating to change in the proportion of people above and below the cut-off for depressed mood at the start and end of group attendance are summarised in table 5. At baseline, 56 of the 117 (47.9%) attendees were above the cut-off of ≥2 and 47 (40%) participants met the criterion for depressed mood at the end of group attendance. Of the 56 participants who met the criterion for depression at baseline, 28 were no longer above the cut-off for depressed mood in their last session, indicating a 50% recovery rate. By contrast, 19 of the 61 (31%) non-depressed clients at baseline scored above the cut-off for depression at their last session.

- Table 5 near here-

McNemar's Test indicated that the change in percentage scoring above the cut-off between the start and end of the intervention did not reach statistical significance, $X^2(1, N=117) = 1.36, p=.24$, which indicates that there is no effect of session attendance on end DISCs classification. However, this finding should be interpreted with caution, as the depressed and non-depressed groups differed in the number of attended sessions and, therefore, the received intervention.

Are reductions in depression associated with the number of sessions attended?

Spearman's rho was used to evaluate the association between the number of attended sessions and the size of change scores. No statistically significant association was found between the number of sessions attended and the size of change scores, for either the overall sample, *rs(117) = .09, p = .33;* or for the depressed-only group, *rs(54) = .07, p = .56.* This indicates that attendance of more sessions is not associated with more favourable depression outcomes.

What is the clinical utility of the open-ACT group for stroke inpatients?

Using data submitted to SSNAP, an average of 157 patients per year were admitted to the sampled rehabilitation ward between 2016 and 2019. With 3.3 years of data collection, 244 of an estimated 518, nearly half (47.1%), attended at least one session, and 117 of an estimated 518 (22.6%) attended at least two. By contrast, only 30% of participants approached in the Niu et al. (2022) RCT proceeded to randomisation, and the vast majority (83%) that did not were excluded because of eligibility concerns. This, therefore, indicates a good relative uptake of the open ACT group, given that many patients with cognitive and communication difficulties after stroke may not be offered psychological therapies [44,45], and provides evidence of clinical utility in the accessibility and practicability dimensions [42].

Regarding the cost of the group, which is also important for appraising the accessibility component of clinical utility, we estimated that the intervention takes a approximately two hours per session of band four Assistant Psychologist and band six Trainee Clinical Psychologist time (including recruiting/transferring participants and writing clinical notes etc), plus approximately thirty minutes of combined weekly clinical psychologist supervision and training time, representing 4.5 hours of clinician time and a cost of £73.35 per session, with no significant cost for materials. With 4.6 patients per session, the per patient per session cost is £15.94. For comparison purposes, the cost of one-hour of individual therapy per patient per session by a band 8a clinical psychologist, would be £54.43, assuming two hours for planning, delivery, and administration, and ten minutes of supervision by an 8b psychologist. The

relatively low cost per patient may have contributed to the group's long-term stability, with a 3.3-year span of delivery and data collection. Such longevity is indicative of minimal impact on surrounding services and support from stakeholders such as members of the wider rehabilitation team. The ACT group is, therefore, a relatively inexpensive intervention with minimal resource implications on clinical services. indicating good clinical utility with respect to accessibility and relevance, which relate to low costs and minimal impact or disruption on existing services, respectively. We also note that training costs are low and were sustained over the 3.3-year intervention span, suggesting practicability according to Smart's (2006) criteria.

Discussion

In the current evaluation, we examined an open group intervention for stroke inpatients in a rehabilitation ward, using an uncontrolled, repeated-measures design. Group attendance was associated with reduced depression scores, with a medium effect size. However, we did not find evidence for an association between attendance and clinical recovery from depression, as indicated by percentage reductions in those scoring above the DISCs cut-off between the start and end of treatment. In other words, the likelihood of scoring below the cut-off for depression did not significantly change, overall, between the start and end of group attendance. The number of attended sessions did not predict clinical outcome in our sample, indicating that the benefits of the intervention may be reached after only a few attended sessions. The findings, therefore, provide mixed support for an association between group attendance and improvements in mood.

Even if all observed improvements could be causally linked to group attendance, and not extraneous factors, the findings of the current pre-post study appeared to be more modest than those in the previous group-based ACT RCT [18]. This disparity may be partially accounted for by differences in methodology, such as differences in the intervention, but changes to treatment protocols are often an inevitable consequence of translating intervention

manuals to clinical practice. This disparity in findings confirms the importance of supplementing clinical trial findings with analyses of routine service data [31,32].

Regarding clinical utility, the three-year span of group delivery, in the context of a busy rehabilitation ward with competing priorities and resource limitations, indicates longitudinal stability and acceptability of this weekly open-group intervention. The low cost of delivery and stakeholder support are hypothesised factors for this longevity. The large sample size, the inclusion of people with a wide range of stroke severities, and the use of materials carefully designed to accommodate cognitive impairments, are indicative of good accessibility. Improving the access of patients typically excluded from psychological intervention research is a substantial priority, as indicated by recent publications reporting limited aphasia inclusivity in psychological therapies and low access in general [16,44,45]. Our evidence, therefore, suggests that the group format possesses several components of clinical utility, including relevance, accessibility, and practicability [42].

There are several limitations to this study. First, the study design featured no control group, meaning that the benefits of intervention attendance cannot be separated from extraneous factors, such as recovery from stroke or the potential social and emotional benefits that may accompany spending time in a supportive context with people going through similar circumstances. Therefore, causality and clinical effectiveness of the intervention cannot be inferred.

Second, the DISCs only assesses non-specific state depression and is prone to floor effects, necessitating the use of non-parametric methodology with less statistical power and prohibiting analysis of the non-depressed group whose scores can only change in one direction. While the DISCs was selected because it could be quickly administered with minimal disruption to the session, a larger array of outcome measures would have reduced the issue of floor effects in the non-depressed group and supported a broader understanding

of the effects of intervention attendance, such as on anxiety, health-related quality of life, stroke recovery, and ACT processes[46].

Finally, there were limitations relating to the equivalency of the received intervention. For example, there was variance in the frequency of each session protocol received by each client and, the variable number of attended sessions means that participants represent slightly different populations for each number of sessions attended. That is, higher session attendance figures are more likely amongst patients with longer inpatient stays, and thus different clinical needs. These factors complicate the interpretation of the results. For example, the depressed and non-depressed groups were found to differ in the number of attended sessions, meaning the analysis of comparative trajectories of both groups may be biased by between-group non-equivalence in received intervention and extraneous population differences between groups.

Considering future research more broadly, different types of psychological therapy have traditionally been benchmarked against one another through RCTs to evidence efficacy. Although this practice is valuable, randomised controlled trials comparing, for example, cognitive behaviour therapy and ACT often demonstrate comparable improvements [47,48] and such comparisons may promote tribal practices that have the potential to inhibit progression [49]. Accordingly, we encourage future stroke psychological therapy research to also consider a process-based approach [34,50], where key mediators and moderators of psychological functioning are incorporated into testable formulations of psychological phenomena, irrespective of the therapeutic tradition that they originate. A greater process-based understanding of intervening with mood problems after stroke may enable the provision of more flexible, adaptable, time-efficient and feasible interventions in the context of competing demands and the limitations of ward-based work. Indeed, there were many potential ‘active ingredients’ that could have present in the current group, including change to

psychological flexibility, social support, emotional processing, and shared advice from peers, and identifying their relative importance is essential for optimising the effectiveness of psychological interventions.

In the case of clinical services recording similar data from routine outcome monitoring, we recommend the collection of qualitative information, possibly in the format of feedback forms, to ascertain aspects of acceptability, highlight benefits not captured by quantitative outcome measures, and evidence the advantages of open group formats. Clinicians may wish to consider establishing a waiting-list control in populations where their mood and medical condition are expected to be stable.

Conclusion

Overall, the findings of the current study appear to be mixed. We have shown evidence of several aspects of good clinical utility in the high attendance figures, the longitudinal sustainability of the intervention delivery, and suitability for people with moderate cognitive and language difficulties through the clinical materials used. Though we found evidence for pre-post reductions in DISCs scores, these scores did not appear to be substantial enough to elicit a statistically significant reduction in the proportion of people scoring above the cut-off for low mood. As such, there is partial support for the benefit of an open-group ACT intervention in stroke rehabilitation. Though the effectiveness of any intervention is impacted by many possible confounding factors, our findings suggest that intervention efficacy may, at times, be more modest than observed in clinical trials when translated into clinical practice. We recommend that stroke clinicians interested in implementing a low-cost flexible intervention in inpatient rehabilitation contexts consider the application of open group formats, so that patient access to psychoeducation of psychological concepts that have the potential to promote adjustment can be maximised.

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Data availability statement. Data are collected from patients as part of Routine Outcome Monitoring. The data is confidential and the property of Norfolk Community Health and Care NHS Foundation Trust. Accordingly, we are unable to make these data publicly available.

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Appendices

Appendix 1

ACT intervention protocol attached separately.

Appendix 2

Confirmation of ethical approval attached separately

Table 1 ACT intervention inclusion and exclusion criteria

Inclusion	Exclusion
Confirmed stroke	Acutely unwell or delirium
Willingness to attend the group intervention	Severe language impairment that precludes engagement; unable to verbalise even without clinician support
	Other cognitive impairments that preclude engagement, such as being unable to understand the content or engage in conversation with other group members
	Presenting behaviours judged to negatively impact group dynamics

Table 2 Sample size requirements for Friedman test, by number of comparisons

Number of comparisons (sessions attended)	Sample size requirement
3	32
4	26
5	23
6	20
7	18
8	15
9	15

Table 3 Cumulative sample sizes, by number of sessions attended

Cumulative session attendance	Total (*n*)	*n* above cut-off for depressed mood (%)
1 or more	224	93 (41%)
2 or more	117	56 (48%)
3 or more	73	41 (56%)
4 or more	47	30 (64%)
5 or more	27	16 (55%)
6 or more	19	11 (59%)
7 or more	10	6 (60%)
8 or more	5	3 (60%)
9 or more	3	2 (67%)
10 or more	1	1 (100%)
11	1	1 (100%)

Table 4 Depressed-only sample mean ranked DISCs scores, by cumulative sessions attended

		Mean ranked DISCs score: depressed-only sample				
Sessions	n	1	2	3	4	p
2	56	1.68	1.32			.001**
3	40	2.40	1.95	1.65		<.001**
4	27	3.19	2.69	2.19	1.94	<.001**

*** indicates significance to .01 level, after application of Bonferroni corrections. Ranked DISCs scores are not comparable between tests.*

Table 5 Contingency table of participants above and below the cut-off at their first and final session

	Final session score		
First-session score	Below cut-off	Above cut-off	Total
Below cut-off (non-depressed)	42	19	61
Above cut-off	28	28	56
Total	70	47	117