

ORIGINAL ARTICLE

Analysis of a care planning intervention for reducing depression in older people in residential care

K. J. LYNE^{1,5}, S. MOXON¹, I. SINCLAIR², P. YOUNG⁴, C. KIRK³, & S. ELLISON²

¹Psychology Services, Selby and York Primary Care Trust, York, UK, ²Social Work Research and Development Unit, University of York, ³Department of Mental Health for the Elderly, Selby and York Primary Care Trust, York, UK,

⁴Institute of Medical Statistics and Epidemiology, Technical University of Munich, Munich, Germany, and ⁵Department of Psychology, York

(Received 24 March 2005; accepted 24 October 2005)

Abstract

Approximately 40% of older people in residential care have significant symptoms of depression. A training and care-planning approach to reducing depression was implemented for 114 depressed residents living in 14 residential care homes in North Yorkshire, UK. Care staff were offered brief mental health training by community mental health teams for older people. They were then assigned to work individually with residents in implementing the care-planning intervention, which was aimed at alleviating depression and any health, social or emotional factors that might contribute to the resident's depression. Clinically significant improvements in depression scores were associated with implementation of the care-planning intervention as evidenced by changes in scores on the Geriatric Mental State Schedule—Depression Scale. There was evidence of an interaction between the power of the intervention and degree of dementia. These improvements were not accounted for by any changes in psychotropic medication. The training was highly valued by care staff and heads of homes, and they considered that the care-planning intervention represented an improvement in quality of care for all residents, irrespective of levels of dementia. Staff also reported improvements in morale and increased confidence in the caring role as a result of their participation. The limitations of this study are discussed. On the basis of a growing body of evidence, it is argued that there is an urgent need for a suitably powered randomised controlled trial and economic evaluation, to test the cost-effectiveness of personalised care planning interventions aimed at reducing depression in older people in residential care.

Introduction

As many as 40% of residents in care homes for older people in the UK are depressed (Ames, Ashby, Mann, & Graham, 1988; Ames, 1993; Schneider, Mann, Levin, Mozley, & Abbey, 1997), a similar level to that found in hospitalised medically ill patients (Jackson & Baldwin, 1993) and constituting a serious health problem (Arvaniti et al., 2005). Non-pharmacological interventions have been recommended in professional and government policy statements, either as a first line of treatment or alongside medication (Turner, 2005). However, many health problems are missed in care homes for older people (Schneider, 1998) resulting in inferior standards of medical and mental health care (Denning & Bains, 2004; Department of Health, 2001a) and rates of active treatment of depression are low in this 'high-risk' group (Llewellyn-Jones et al., 1999). Ames (1990) found no evidence that psychiatric intervention, using a range of mainly social methods, reduced depression in residents in homes for older people over a period of three months.

Care staff in daily contact with older people in residential care provide a possible solution to some of these problems. They are well placed to detect when a resident is depressed (Lee, 2005). Brief mental health training programmes for care home staff have been shown to be popular and effective in improving knowledge of mental health problems (Walker & Osgood, 2000–2001). There is evidence that the ability of care staff to detect depression in residents can be enhanced, with some evidence of subsequent increase in treatment rates and improvement in the course of depressive illness (Eisses et al., 2005; Proctor et al., 1999). However, staff generally have limited or no specific training in recognising depression, understanding its causes, and the potential for effective treatment (Bagley et al., 2000; Jackson & Baldwin, 1993; Moniz-Cook et al., 1998). Lack of training, and management and peer pressure mean that care staff give priority to practical tasks rather than talking to residents (Lindeman et al., 2004; Penna, Paylor, & Soothill, 1995).

Appropriate training within the home might help care staff recognise depression, learn that it is capable of remediation in some of these residents and legitimise a role that goes beyond basic nursing and physical care.

A pilot study involving two local authority residential care homes, provided plausible evidence that care staff can be trained to detect residents' depression and intervene to reduce it (Moxon, Lyne, Sinclair, Young, & Kirk, 2001). The study provided training in a personalised care-planning intervention, adapted from Barrowclough & Fleming (1986) and in detecting other basic health problems, thereby enabling referrals to health care professionals for assessment and treatment. It was hypothesized that improvements in general health might be important in helping residents overcome depression. It was also believed that care staff could be directly instrumental in modifying depression, through their capacity to make meaningful relationships with residents, and provide a measure of therapeutic relief that would go beyond keeping the resident comfortable. The essence of the approach was to develop a bespoke care-plan, adapted to possible causes of the resident's depression, which in this client group are varied (Audit Commission, 2000; Katona, 1994).

The present study extends this experiment and tests whether the earlier results will be replicated in residents selected from a larger range of care homes for older people. The study was supported by a contextualisation study, which was included in the research for triangulation purposes. A variety of research designs were considered. Random allocation within homes was rejected because of likely staff resistance. There was also the likelihood that staff would apply their new training to the control as well as the experimental group (Llewellyn-Jones et al., 1999; Opie, Doyle, & O'Connor, 2002). Achieving adequate power by cluster randomisation at the level of homes and providing a suitable control intervention was calculated to be beyond the resources of the present study (Llewellyn-Jones et al., 1999). A multiple baseline design was not feasible because of problems of attrition over time in an older adult population. We therefore report a quasi-experimental analysis based on a natural occurrence that arose in the process of the project, as described in detail below.

Method

The study took place in North Yorkshire, England over 120 weeks during 1999 to 2002, and was approved by York Research Ethics Committee.

Settings and sample

All homes for older people with 30 or more residents registered in the York District were invited to take part, with the exception of two homes that

participated in the pilot study ($n=29$). Ten homes said that they could not afford to release staff for training and five refused for no reason. Fourteen homes (eight local authority, three private nursing, and three dual registered voluntary) agreed to be involved. Resident numbers in the homes varied from 30–80 with a mean of 36. The homes were entered into the study consecutively at five-week intervals to control for factors that might have affected all homes at the same time (such as seasonal effects, social care policy changes, media events). Each home was in the study for 33 weeks, from initial screening through four measurement points (T1–T4). Stratified randomisation controlled for the order in which the different types of home were entered into the study.

All residents aged 65 or over from the 14 homes who did not have a psychotic or serious physical illness, were approached by the researchers for informed consent to participate. They and, if appropriate, their close relatives were given an information leaflet about the purposes and details of the research.

All staff employed in the homes who did not have previous psychiatric training (all but one person) were sent an information sheet and invited to meetings with two researchers. Subsequently, they were given the opportunity to volunteer to participate in the training and research.

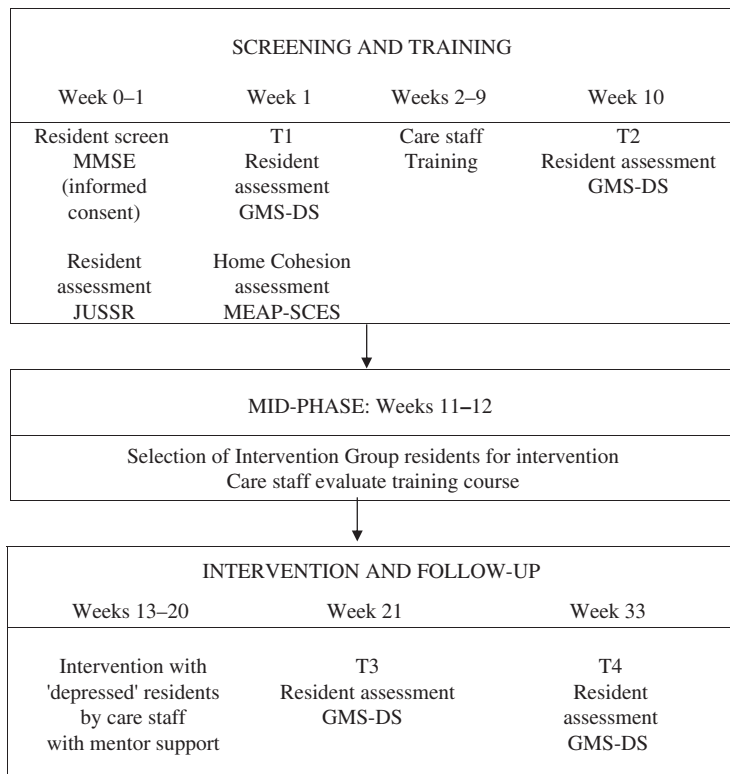
Measures

The primary outcome measure was change in the self-report Depression Scale of the Geriatric Mental State Schedule (GMS-DS: Ravindran, Welburn, & Copeland, 1994). This comprises 33 items with a scoring range 0–71, with 0 = no evidence of depression. Optimum sensitivity and specificity for sub-case level depression is achieved with a cut-off of ≥ 10 , and ≥ 14 for case-level depression (Mottram, 2000).

The study measured cognitive function, physical dependency, use of antidepressants and 'home cohesion' as possible moderating or mediating variables influencing depression, the ease of treating it, and the ease of implementing the intervention.

Cognitive function was assessed through the Mini-Mental State Examination (MMSE: Folstein, Folstein, & McHugh, 1975). This has a scoring range of from 0 (severely impaired) to 30 (intact). There are different views on suitable cut points when assessing dementia with this instrument. This study used: ≥ 21 mild or no dementia; 10–20 moderate dementia; < 10 severe dementia, (Department of Health, 2001b).

Physical dependency was assessed using the continence (two items) and self-care (six items) sub-scales of the Sheffield Joint Unit for Social Services Research Assessment Schedule for Residential Homes for the Elderly



Key:

<p><i>Assessment point:</i></p> <p>T1 Pre-training assessment</p> <p>T2 Post-training assessment</p> <p>T3 Post-Intervention assessment</p> <p>T4 Follow-up assessment</p>	<p><i>Scale:</i></p> <p>MMSE Mini-Mental State Examination</p> <p>GMS-DS Geriatric Mental State Schedule-Depression Scale</p> <p>JUSSR Assessment Schedule for Residential Homes for the Elderly</p> <p>SCES Sheltered Care Environment Scale: Cohesion Dimension</p>
---	---

Homes follow consecutively from Home 1 to 14, each starting five weeks after the start of the previous home

Figure 1. Procedure.

(JUSSR: Booth, Barritt, Berry, Martin, & Melotte, 1982). Each item has a range of response categories with scores from 1-3. High scores represent severe problems. The total scores for each scale are converted into dependency classifications of 'high', 'medium' or 'low' for each resident. The JUSSR does not require an experienced interviewer and is user-friendly (Booth & Philips, 1987).

Whether or not residents were using antidepressants was recorded at each of the four time points.

'Home Cohesion' was assessed through the Multiphasic Environmental Assessment Procedure (MEAP: Moos, Gauvain, Lemke, Max, & Mehren, 1979; Moos & Lemke, 1984). The scale generates scores out of nine for each member of care staff and these were averaged for each home, giving a home cohesion score. High scores signify 'more helpfulness'.

Procedure

The design was intended to track the depression scores of residents at selected points before and after

the intervention. The procedure followed in each home is shown in Figure 1.

Weeks 0-1. (Screening and initial assessment) All consenting residents were screened on the MMSE. Those scoring >9 formed the 'Study Group'. Heads of homes rated residents' dependency for self-care and continence using the JUSSR.

Week 1 (T1 first baseline). Residents were assessed using the GMS-DS. Participating staff rated the home on the MEAP-SCES cohesion scale.

Weeks 2-9. Care staff took part in a training programme, described elsewhere (Moxon, 1996; 2003), which was delivered in the homes in four three-hour weekly sessions by members of a Community Mental Health Team for older people. It covered definitions and recognition of depression, how to distinguish it from other problems, communicating with depressed older people, the effects of loss, the effects of depression on thinking, feeling and behaviour, understanding the principles and

practice of the care-planning intervention, and awareness of approaches that are used to reduce depression in older people.

Week 10 (T2 second baseline). All residents in the Study Group were reassessed on the GMS-DS.

Weeks 11–12. Residents who scored >9 on the GMS-DS at T1 or T2 were identified as members of the 'Intervention Group', a sub-set of the overall 'Study Group'.

Weeks 13–20. Care staff who had completed the training programme conducted the care-planning intervention with the Intervention Group residents. Randomised allocation within homes was not practicable in most cases for operational reasons so usually, where the resident's key worker had been trained, s/he carried out the care-planning intervention. Each care staff was allocated one or two Intervention Group residents. They conducted a structured interview with their resident(s) covering physical health, likes/dislikes/habits, work and family history, past and present leisure interests, quality of relationships in the home, and significant past experiences if volunteered. The resident and member of care staff worked together in an attempt to formulate three or four individualised goals and procedures for the care-planning intervention. Care plans typically involved activities such as resuming a hobby, re-establishing contact with friends, relatives, or the Church, reassurance, supportive listening (for example, about a loss), or arranging physical health check-ups and treatments. Care-planning interventions were documented by care staff using a proforma adapted from Barrowclough and Fleming (1986). Activities agreed in the care-planning intervention were in addition to home-based activities such as Church services, bingo sessions, and Christmas and birthday celebrations. For the duration of the care-planning intervention, care staff had access to weekly one-to-one contact with a mentor, who was experienced in working with older people and familiar with the training programme. Seven mentors (six psychiatric nurses and an occupational therapist) assisted care staff in implementing the care-planning intervention, but did not meet the resident.

Week 21 (T3 post-intervention). All residents in the Study Group were reassessed on the GMS-DS.

Week 33 (T4 follow-up). All Study Group residents were reassessed for the final time on the GMS-DS.

Statistical power and procedures

A within subjects, repeated measures, quasi-experimental analysis was used and applied to the Study Group as a whole and then separately to the

Intervention sub-group. Not all depressed residents in the Intervention Group received an intervention, mainly because of lack of care staff. This gave an opportunity to measure the impact of the intervention by comparing change in GMS-DS scores for depressed residents who received the intervention with scores for those who did not. The following null hypotheses were tested:

1. For the Study Group as a whole and for the Intervention Group sub-set, scores on the GMS-DS at T1 would not be significantly different from those at T2.
2. For the Study Group as whole, scores on the GMS-DS at T3 and T4 would not be different to those at T1 and T2.
3. For the Intervention Group, scores on the GMS-DS at T3 and T4 would not be different to those at T1 and T2, whether or not they actually received an intervention.

Sample size estimation was based only on the test of whether or not individual older people selected for intervention on the basis of their level of depression would be less depressed after receiving the care-planning intervention (hypothesis 3). Based on the results of the pilot study and assuming that the GMS-DS is approximately normally distributed, it was calculated that 104 residents would be required using a paired *t*-test ($p < 0.05$; two tailed test) at the standard 80% level of power. Estimates of case detection rates and attrition were kept under review, subsequently indicating that in practice all 14 homes were required.

The Bonferroni-Holm criterion for guarding against type I error in multiple testing (Holm, 1979) was used. In order to avoid excessive multiple testing it was planned to test if the scores at T1 and T2 were significantly different. If not, as would be expected, then the mean of the scores at T1 and T2 would be used as the single pre-intervention score. However, if the T1 and T2 scores were to be significantly different then they would be compared to the T3 and T4 scores separately and additional adjustments for multiple testing would be needed.

If a significant intervention effect was found, then exploratory linear least-squares regression analyses were also planned to determine which factors other than the intervention influenced any observed effects, in particular the effect of dementia at baseline. Logistic regression was used to assess if the intervention had an effect on the secondary outcome of subsequent anti-depressant prescription. For the purposes of these exploratory analyses, missing data were replaced using multiple imputation (expectation-maximization method-EM; Patrician, 2002). However for analyses of changes in depression across time points, list-wise data deletion was employed (that is, only residents with complete data were used). This was because it was assumed that attrition, which was expected to be

approximately 20%, would be significantly confounded with the target variable.

Inter-rater reliability

An occupational therapist and a psychiatric nurse, both trained to use the GMS-DS by a consultant psychiatrist, conducted all resident assessments. They worked independently of the researchers and were blind to other aspects of the research. Inter-rater reliability was calculated for the GMS-DS, with scores categorised as ‘no depression; <10’, ‘sub-case; 10–13’ and ‘case; >13’.

Contextualisation study

The contextualisation study explored the degree to which the intervention was implemented as planned, staff perceptions of the experience of participating in the research, staff perceptions of outcomes for residents both at completion and over time, and possible implications for the wider application of the research. It consisted of a convenience sample of

eight homes. Interviews were guided, but otherwise unstructured to optimise the flow of data. One to one interviews were carried out with the head of the home and focus groups were conducted with care staff who were encouraged to interact freely. The interviews and focus groups were taped, written up and analysed thematically.

Results

Resident selection

Figure 2 shows details of resident selection into groups and attrition rates. Of 499 residents aged 65 or over, 81% consented to take part but a further 10% were screened out through the MMSE. This left 360 residents who formed the full Study Group. Their mean age was 86.4 years (median 88; range 65–103) and 70% were female. To minimise the effects of attrition in this population of older people, residents were considered to have completed the study if they participated in one or both of T3 and T4 assessments.

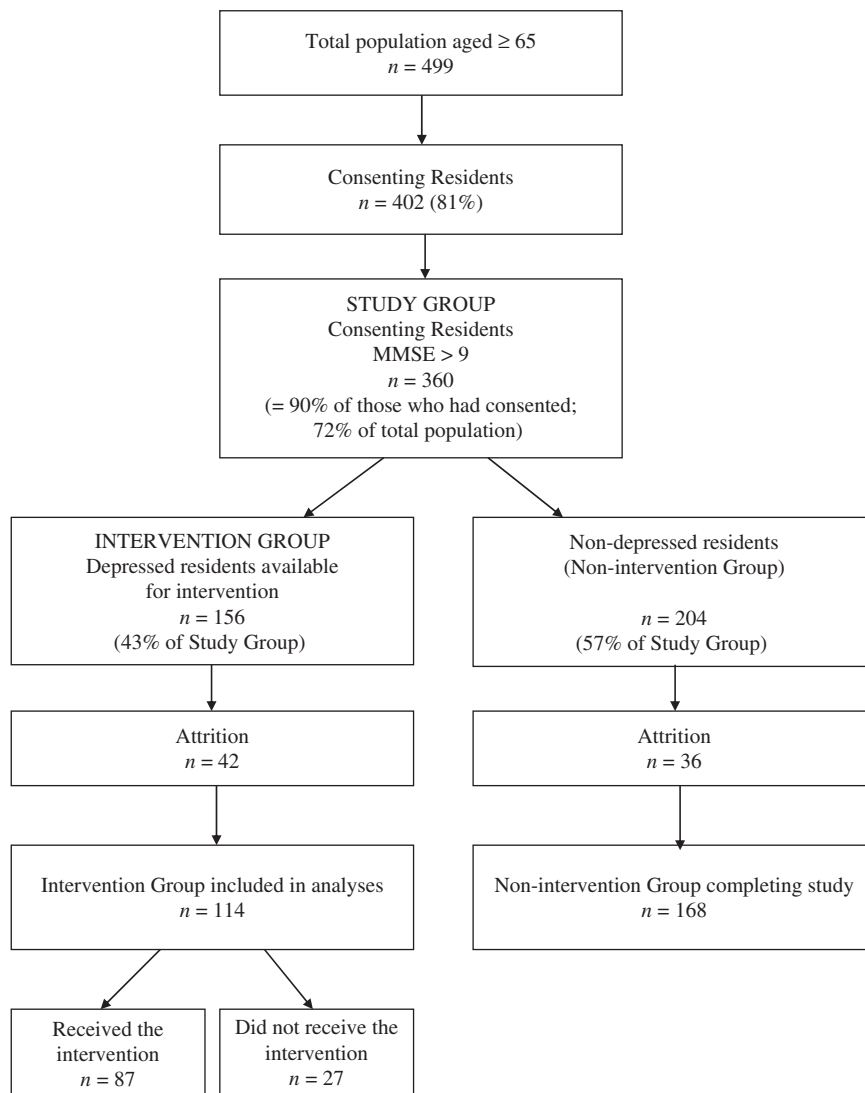


Figure 2. Selection of residents.

Of the 360 residents, 76 did not complete assessment at either T3, or T4 and so were lost to the study, due to sickness ($n=27$), death ($n=24$), communication problems ($n=2$), withdrawal of consent ($n=23$), or incomplete records (2). This left 282 residents, four with missing data at T3, and 29 with missing data at T4, (22% attrition rate).

Staff volunteers

A total of 166 (out of 287; 58%) care staff (151 women; 15 men) volunteered to participate in the training. Reasons for not participating included current involvement in other training, plans to leave the home, not wishing to consider a role beyond basic care, and anticipation of maternity leave. They had been employed as care workers for a mean of 76 months (SD 78.21; range one month to 28 years). The mean age of the women (41.62 years, SD 12.39) was significantly greater than that of the men (31.25 years, SD 10.46; $t=-3.12$; $p<0.01$).

Resident Intervention Group

Of the 360 residents in the Study Group, 156 had raised scores on the GMS-DS at T1 or T2 and were identified as members of the Intervention Group. Of these, 42 were lost to the study due to attrition.

Timing and content of care-planning interventions

Of the 87 interventions that were implemented, only 45 were completed by T3; the remaining 42 were completed between T3 and T4, due to staff sickness, annual leave and administrative problems in the homes. Between one and three physical/medical/sensory care-plans were attempted or carried out for 59 of the residents, specifically to do with mobility ($n=22$ care-plans), pain management ($n=7$), sleep problems ($n=2$), health anxiety or illnesses ($n=11$), incontinence or personal hygiene ($n=6$), diet ($n=5$), dental ($n=1$), hearing ($n=6$), vision ($n=5$) and medication review ($n=12$).

Psychosocial care-plans were attempted or carried out for all but three residents.

Comparison of Intervention Group residents who did and did not receive the care-planning intervention

The intervention could not be attempted for 27 members of the Intervention Group, mainly because of lack of availability of care staff. A conservative approach was taken to defining what counted as an intervention, so that even the few residents for whom the intervention was attempted superficially were included as having received it. The reasons care staff were not available after allocation were absence ($n=7$), leaving to work elsewhere ($n=1$), withdrawal due to personal problems or loss of interest ($n=12$). For six residents, insufficient trained care staff were available in their particular home; in such cases selection for intervention was randomised. One resident was in hospital at the time of allocation. Residents from the Intervention Group who received the care-planning intervention were compared with those who did not and there were no significant differences across all study variables: home cohesion; MMSE score; T1 GMS-DS score; T2 GMS-DS score; self-care; continence; age; sex; T1 antidepressants.

Inter-rater reliability on the GMS-DS

Thirty-six residents contributed data for the 43 inter-rater reliability GMS-DS checks and agreement was found to be high (Kappa = 87, $p<0.001$).

Whole Study Group

Scores at T1 and T2 were not significantly different (difference -0.13 ; $t=-0.396$; n.s.) and it was therefore decided to use the mean of the GMS-DS scores at T1 and T2 as the single pre-intervention score. There was a significant improvement in GMS-DS scores at T4, but not at T3 (Table I).

Table I. Study Group and Intervention Group residents who received or did not receive the intervention: Comparison of mean GMS-DS scores over time (list-wise missing data deletion).

Change between	Mean GMS-DS score			Difference (95% CI; two-tailed)
	Mean T1/T2 (SD)	T3 (SD)	T4 (SD)	
Whole Study Group $n=246$ $df=245$				
T1/2 and T3	8.0 (7.4)	7.8 (7.1)		0.24 n.s.
T1/2 and T4			7.2 (6.8)	0.80 [0.20–1.42] $t=2.61$; $p<0.025$
Intervention Group residents who received intervention $n=80$ $df=79$				
T1/2 and T3	15.3 (8.1)	12.8 (7.7)		2.59 [1.30–3.89] $t=3.99$; $p<0.001$
T1/2 and T4			11.8 (8.8)	3.58 [2.20–4.96] $t=5.16$; $p<0.001$
Intervention Group who did not receive intervention $n=21$ $df=20$				
T1/2 and T3	14.4 (7.1)	14.4 (8.4)		0.00 ns
T1/2 and T4			14.0 (8.5)	0.43 ns

Time between assessments: T1 and T2, nine weeks; T1/2 and T3, 11 weeks; T1/2 and T4, 23 weeks.

Table II. Regression analysis of factors influencing change in GMS-DS scores between T1/T2 and T4.

	Unstandardized coefficients			
	B	Std. error	t	Sig.
Model 1. All members of the Intervention Group ($n = 114$)				
(Constant)	18.802	10.131	1.856	0.066
MMSE score	-1.046	0.452	-2.315	0.023
Intervention	-9.729	5.415	-1.797	0.075
Interaction	0.558	0.243	2.300	0.023
T1/T2 GMS-DS	0.224	0.072	3.122	0.002
T1 Antidepressants	-2.704	1.236	-2.187	0.031
Model 2. Residents with mild or no dementia ($n = 62$)				
(Constant)	-4.727	7.361	-0.642	0.523
MMSE score	-0.256	0.274	-0.934	0.354
Intervention	4.960	1.663	2.983	0.004
T1/T2 GMS-DS	0.412	0.097	4.225	0.000
T1 Antidepressants	-3.287	1.719	-1.913	0.061
Model 3. Residents with moderate dementia ($n = 52$)				
(Constant)	4.932	5.932	0.831	0.410
MMSE score	5.125E-02	0.260	0.197	0.845
Intervention	-1.267	2.127	-0.596	0.554
T1/T2 GMS-DS	1.487E-02	0.103	0.145	0.886
T1 Antidepressants	-1.913	1.718	-1.113	0.271

Intervention Group

Scores on the GMS-DS for the 114 residents in the Intervention Group were not significantly different between T1 and T2 (difference -0.24 ; $t=0.326$), and so again the mean of T1 and T2 was used as the pre-intervention score. As can be seen in Table I, for Intervention Group residents who received the intervention there was a significant improvement in scores between T1/T2 and T3 (difference = 2.59; $p < 0.001$) and a larger improvement by T4 (difference = 3.58; $p < 0.001$). For Intervention Group residents who did not receive the intervention there were no significant changes between T1/T2 and T3 or T4. Differences in GMS-DS scores between the Intervention Group sub-groups were not statistically significant at any time point.

An improvement in scores for the depressed sub-set of residents (Intervention Group) could be explained by regression to the mean in residents selected for high scores on the GMS-DS. No such explanation is possible for the reduction in scores for the entire Study Group.

Interaction between dementia and the intervention

As a result of feedback from care staff (below), of particular interest was whether improvement for members of the Intervention Group who had received the intervention had been modified by the extent to which they had dementia.

A linear regression model was fitted to predict change in scores on the GMS-DS between T1/T2 and T4 for all residents in the Intervention Group using: whether the intervention was attempted, T1/T2 GMS-DS score, MMSE score, whether on antidepressants at T1, and the interaction between MMSE score and the intervention.

The overall model was significant, as was the interaction between MMSE score and the intervention ($p = 0.023$). After accounting for the interaction, improvement in depression scores was associated with not being on anti-depressants at T1 ($p = 0.03$), greater extent of dementia ($p = 0.02$), starting with higher levels of depression at T1/T2 ($p = 0.002$) and a trend in favour of not having the intervention ($p = 0.075$; Table II, model 1).

These results can be explained by a beneficial effect of the intervention on residents with less dementia, or a negative effect on residents with more dementia. We therefore divided the Intervention Group into two: those with MMSE scores >20 ($n = 62$, mild or no dementia) and those with MMSE scores <21 ($n = 52$, moderate dementia). None of the variables were significantly associated with change in GMS-DS scores for the moderately demented group (Table II, model 3), whereas for the mild or no dementia group, positive change was significantly associated with having the intervention ($p = 0.004$), and higher GMS-DS scores at T1/T2 ($p < 0.001$; Table II, model 2).

The effect of other covariates: sex, whether the intervention was attempted, SCES cohesiveness scores, self-care score, and continence score were also tested but none of these had a significant effect in any of these analyses. All of the variables in the equations are within subject measures with the exception of the SCES cohesiveness score, which is home rather than resident specific. Therefore a multi-level regression analysis was undertaken using MLWIN (Goldstein, 1995). This confirmed that the SCES cohesiveness score did not have a significant effect on depression scores at T4.

Antidepressant prescriptions

Of the 114 residents included in the study, 29% were on antidepressant medication for all four assessment points, whereas 52% were not on antidepressants at any point. A logistic regression analysis was used to test whether being selected for the intervention had any effect on antidepressant prescribing at T3 (missing data for 10 residents) or T4 (missing data 18 residents). Variables entered into the model were age, sex, continence, self-care, MMSE score, GMS-DS score at T1 and T2, whether the intervention was undertaken, and antidepressant use at T1 or T2. For both T3 and T4 the models were highly significant: Chi-square 109.0; $df=11$; $p<0.0001$, and Chi-square 78.4; $df=11$; $p<0.0001$, respectively. However, the significant effects reflected prior anti-depressant use at T1 and T2, and GMS-DS scores at T2; none of the other variables were significant.

Contextualisation study

Staff in six of the eight homes reported difficulties in finding the required amount of time with residents for conducting the care-planning intervention due to time pressures and a lack of adequate cover. For the most part, staff reported overcoming these problems by a combination of informal arrangements with colleagues and the use of personal time, neither of which was thought to be sustainable once the research was completed.

Of the 42 residents reviewed in interviews and focus groups, 23 were perceived to have improved in mood at completion. The amount of one-to-one contact time with care staff was then substantially reduced for 18 of those residents, although the benefits were perceived to be sustained for eight residents, either because contact time was found from alternative sources such as Age Concern (two residents), or because a social, emotional or physical cause of the depression had been resolved and improvement was therefore no longer dependent on intensive contact time with care staff.

Most care staff reported that the experience of participating in the research was a positive one. Typically, the benefits of the training, such as an understanding of the sources and signs of depression, and of how to engage more therapeutically with residents, led to improved levels of self-confidence in the staff, which encouraged them to be more proactive. In five homes this effect was reported to have filtered through to staff who did not participate in the research, through staff meetings and supervision, as well as informal staff interaction. Following the completion of the research, when it was more difficult to maintain levels of individual contact time, staff in these homes nevertheless reported the development of a more emotionally supportive culture and a sharper, more perceptive practice

leading to earlier and more efficient recognition of depressive symptoms.

Care staff were doubtful that the intervention could produce a reduction in depression in residents with moderate dementia. However, they felt strongly that, as a model of best practice in caring for the well-being of residents, the training they received and the care-planning intervention could contribute significantly towards the quality of care of *all* residents.

Discussion

Of 256 residents who completed this study, 44% were assessed as having case level or sub-case level depression as measured by the GMS-DS, an incidence that is similar to that found in other studies mentioned in the Introduction. It was intended that all of these 'depressed' residents ($n=114$) would receive the care-planning intervention. Over a period of 33 weeks in each home the attrition rate was 22%, which is not dissimilar to that in other studies of residential care for older people (for example, Moniz-Cook et al., 1998).

The primary null-hypothesis, that the care-planning intervention would not be instrumental in recovery from depression, could not be tested definitively, because a randomised controlled trial design was not used. However, since 27 of the 114 depressed residents in the Intervention Group did not actually receive the intervention, because of the unavailability of a member of care staff, there was an opportunity to test the plausibility of the primary hypothesis.

Comparisons between the 87 depressed residents who received the care-planning intervention, and the 27 who did not, indicated that they did not differ significantly on any of the measures used in the study. This does not rule out the possibility of selection bias, for example, residents with the most intractable depression might have been the most difficult for care staff to relate to, and this might be why some staff pulled out of the study at the intervention phase. However, there is evidence that care staff characteristics and availability of care staff was the main reason that these residents did not receive the intervention: some care staff were unavailable due to personal circumstances that were unrelated to particular residents (including holidays, pregnancy, and leaving the home) and in six cases where there were insufficient trained care staff in the particular home. In these situations the decision about which residents would receive an intervention was randomised.

There was a clinically and statistically significant within group improvement in GMS-DS scores for Intervention Group residents that received the intervention by T3 as had been expected, but the effect was stronger by T4. This is consistent with the

finding that in practice the interventions got off the ground quite slowly for half of the Intervention Group and were not completed until after T3. However, there was no significant difference in scores between the Intervention Group members that received the intervention from the Intervention Group residents that did not receive the intervention at T3 or T4.

A subsidiary analysis indicated that severity of dementia had a significant impact on the effect of the intervention in those residents that received it, supporting the impression formed by care staff, that the intervention was less effective in reducing depression in residents with more severe dementia. Since for most residents, antidepressant prescribing did not change during the period of the study, these findings cannot be explained by the effects of medication.

There are three main reasons for thinking that the intervention had an effect in depressed residents. Firstly, for the whole Study Group sample of 246 residents with complete data including depressed and not depressed, there was a significant drop in depression scores between T1/T2 and T4. This was probably not explained by a trend over time (there was a slight rise between T1 and T2), by regression to the mean (which would tend to increase as many scores as it decreased in the whole Study Group) or by selection effects (the analysis was carried out on residents who were present at all four time points). Residents who were eligible and who actually received an intervention experienced a significant improvement by T3 and this effect had increased by T4. There was no evidence of improvement for residents who did not receive an intervention. Thirdly, whilst staff were undoubtedly critical of aspects of the research they nevertheless were quite convinced that in a number of cases it had 'worked' for residents whose dementia was not so severe as to interfere with the impact of the intervention. A regression analysis lends support to this conviction.

Given the combination of different kinds of evidence there are grounds for considering that the intervention was instrumental in the improvement in depression scores for the residents, but that this improvement was modified by dementia severity.

There are several plausible explanations for why this might be so. Care staff reported that the interviews underpinning their assessment of residents' strengths and needs allowed them to develop a greater appreciation of the resident as a person with a personal history. This could have increased empathy on the part of care staff, and it may have been important for some residents to have their past acknowledged in the context of their present relationship with care staff and the home. It was important for the mental health of many residents that the intervention included the assessment of physical health needs. The intervention resulted in increased social contact and resumption of previous hobbies for

some residents, which might also have had beneficial effects on their mental health (Banerjee et al., 1997). For those residents who started out with high scores on the *GMS-DS*, the intervention might have compensated for a history in which they became depressed because they were not able to make important transitions, such as moving into the home, physical deterioration, or loss of contact outside the home, in a personally meaningful way (Arvanti et al., 2005; Krout & Wethington, 2003).

Care-staff also suggested that the presence of a meaningful and interactive relationship between the care worker and the resident was important in the implementation of the care-planning intervention. A key factor in establishing such a relationship was time; without the increase in contact time that occurred in the study it is probable that already depressed residents would not have improved. Staff also thought that the absence of organisational or cultural barriers or both to the proper implementation of the training and care-planning intervention was essential.

Conclusion

This research adds to a growing body of evidence (Eisses et al., 2005; Llewellyn-Jones et al., 1999; Moniz-Cook et al., 1998; Moxon et al., 2001; Opie et al., 2002; Proctor et al., 1999) that personalised care planning, conducted by suitably trained and supported care staff, might be an effective intervention for detecting and reducing depression in residential care for older people. It has been emphasized, however, that for reasons of cost and practicality, depressed residents were not randomly allocated into treatment and no-treatment conditions in the present study. Those receiving an intervention were not different from those that did not on a number of measures, but the possibility of selection bias cannot be definitively ruled out. For this reason, a cluster randomised controlled trial (RCT) comparing experimental and control groups, in which depressed residents within particular residential care facilities form the clusters, along with an economic evaluation, is urgently needed. A cluster RCT design was adopted in part in a study similar to the present one (Proctor et al., 1999) and is therefore a feasible design in residential care for older people, although the target variable of interest in that particular trial was challenging behaviour. In the meantime, the case for such interventions rests not only on the plausibility of their effectiveness, but also on their obvious relevance to good practice.

Acknowledgements

This research was funded by the National Health Service Executive under the Biomedical Health

Services Research Programme (Grant No. P0051), Wyeth Laboratories, Sir Halley Stewart Trust, Purey Cust Trust and Jack Brunton Charitable Trust. The authors gratefully acknowledge the contributions of Professor Anthony Mann, Professor Ian Russell, Maggie Browne, Eryk Grant, Caroline Mozley, the training and assessment teams, and above all, the residents and care staff who took part. The views expressed are attributed to the authors.

References

- Ames, D. (1990). Depression among elderly residents of local-authority residential homes. Its nature and efficacy of intervention. *British Journal of Psychiatry*, *156*, 667–675.
- Ames, D. (1993). Depressive disorders among elderly people in long-term institutional care. *Australian and New Zealand Journal of Psychiatry*, *27*(3), 379–391.
- Ames, D., Ashby, D., Mann, A. H., & Graham, N. (1988). Psychiatric illness in elderly residents of Part III homes in one London borough: Prognosis and review. *Age and Ageing*, *17*(4), 249–256.
- Arvaniti, A., Livaditis, M., Kanioti, E., Davis, E., Samakouri, M., & Xenitidis, K. (2005). Mental health problems in the elderly in residential care in Greece: A pilot study. *Aging & Mental Health*, *9*(2), 142–145.
- Audit Commission. (2000). *Forget-me-not: Mental health services for older people*. London: HMSO.
- Bagley, H., Cordingly, L., Burns, A., Mozley, C. G., Sutcliffe, C., Challis, D., & Huxley, P. (2000). Recognition of depression by staff in nursing and residential homes. *Journal of Clinical Nursing*, *9*, 445–450.
- Banerjee, S., Blanchard, M., Drinkwater, C., Kirk, C., Mann, A., & Tylee, A. (1997). *Old age depression: A practical approach to treating depression in residential homes*. Milton Keynes: Lundbeck Ltd.
- Barrowclough, C., & Fleming, I. (1986). *Goal planning with elderly people: Making plans to meet individual needs*. Manchester: University Press.
- Booth, T., Barritt, A., Berry, S., Martin, D. N., & Melotte, C. (1982). A reliable assessment schedule for use in homes for the elderly. *Social Work Service*, *29*, 42–53.
- Booth, T., & Phillips, D. (1987). Group living in homes for the elderly: A comparative study of the outcome of care. *British Journal of Social Work*, *17*, 1–20.
- Denning, T., & Bains, J. (2004). Mental health services for residents of care homes. *Age & Ageing*, *33*, 1–2.
- Department of Health. (2001a). *National Service Framework for Older People*. London: Department of Health. <http://www.doh.gov.uk/nsf/pdfs/nsfolderpeople.pdf>.
- Department of Health. (2001b). *Guidance on the use of donepezil, rivastigmine and galantamine for the treatment of Alzheimer's disease*. In NICE Technology Appraisal Guidance-No. 19. London: HMSO.
- Eisses, A. M. H., Kluiters, H., Jongenelis, K., Pot, A. M., Beekman, A. T. F., & Ormel, J. (2005). Care Staff training in detection of depression in residential homes for the elderly. *British Journal of Psychiatry*, *186*, 404–409.
- Folstein, M. F., Folstein, S. E., & McHugh, P. R. (1975). Mini-mental state: A practical method for grading the cognitive state of patients for the clinician. *Journal of Psychiatric Research*, *12*, 189–198.
- Goldstein, H. (1995). *Multilevel statistical models*. London: Edward Arnold, New York: Wiley.
- Holm, S. (1979). A simple sequentially rejective multiple test procedure. *Scandinavian Journal of Statistics*, *6*, 65–70.
- Jackson, R., & Baldwin, B. (1993). Detecting depression in elderly medically ill patients: The use of the Geriatric Depression Scale compared with medical and nursing observations. *Age and Ageing*, *22*(5), 349–353.
- Katona, C. L. E. (1994). *Depression in old age*. Chichester: Wiley.
- Krout, J. A., & Wethington, E. (Eds.) (2003). *Residential choices and experiences of older adults*. New York: Springer.
- Lee, S. M. (2005). Innovative practice: The effectiveness of a stress management training intervention for care assistants working in a residential home for the elderly. *The International Journal of Social Research & Practice*, *4*(2), 312–315.
- Llewellyn-Jones, R. H., Baikie, K. A., Smithers, H., Cohen, J., Snowdon, J., & Tennant, C. C. (1999). Multifaceted shared care intervention for late life depression in residential care: Randomised controlled trial. *British Medical Journal*, *319*, 676–682.
- Lindeman, M. A., Black, K., Smith, R., Gough, J., Bryce, A., Gilson, B., et al. (2004). A. Changing practice in residential aged care using participatory methods. *Change in Learning & Practice*, *16*(1), 22–31.
- Moniz-Cook, E., Agar, S., Silver, M., Woods, R., Wang, M., Elston, C., et al. (1998). Can staff training reduce behavioural problems in residential care for the elderly mentally ill? *International Journal of Geriatric Psychiatry*, *13*, 149–158.
- Moos, R. H., Gauvain, M., Lemke, S., Max, W., & Mehren, B. (1979). Assessing the social environment of sheltered care settings. *The Gerontologist*, *19*(1), 74–82.
- Moos, R. H., & Lemke, S. (1984). *Multiphasic Environmental Assessment Procedure (MEAP) Manual*. Palo Alto, CA: Stanford University, Social Ecology Laboratory.
- Mottram, P. (2000). Validation of rating scales of treatment-controlled trials of depression in the elderly. Liverpool: PhD Thesis, University of Liverpool.
- Moxon, S. (2003). The role of care staff in identifying and managing depression in residential homes: An investigative study. York: PhD Thesis, University of York.
- Moxon, S. (1996). *Promoting mental health in residential homes: A pilot project*. Report for the National Health Service Executive (Northern and Yorkshire Research and Development Directorate). York: Evaluation, Research and Support Unit, York Health Services.
- Moxon, S., Lyne, K., Sinclair, I., Young, P., & Kirk, C. (2001). Mental health in residential homes: A role for care staff. *Ageing and Society*, *21*, 71–93.
- Opie, J., Doyle, C., & O'Connor, W. O. (2002). Challenging behaviours in nursing home residents with dementia: A randomised controlled trial of multidisciplinary interventions. *International Journal of Geriatric Psychiatry*, *17*, 6–13.
- Patrician, P. A. (2002). Multiple imputation for missing data. *Research in Nursing & Health*, *25*(1), 76–84.
- Penna, S., Paylor, I., & Soothill, K. (1995). Job satisfaction and dissatisfaction amongst residential care workers. *Social Care Research*, *69*. London: National Institute of Social Work.
- Procter, R., Burns, A., Stratton Powell, H., Tarrrier, N., Faragher, B., Richardson, G., et al. (1999). Behavioural management in nursing and residential homes: A randomised controlled trial. *The Lancet*, *354*, 26–29.
- Ravindran, A. V., Welburn, K., & Copeland, J. R. M. (1994). Semi-structured depression scale sensitive to change with treatment for use in the elderly. *British Journal of Psychiatry*, *164*, 522–527.
- Schneider, J., Mann, A. H., Levin, E., Mozley, C., & Abbey, A. (1997). *Quality of care: Testing some measures in homes for elderly people*. PSSRU Discussion Paper 1245. Canterbury: University of Kent, Personal Social Services Research Unit.
- Schneider, J. (1998, January 15). Home truths. *Health Service Journal*, *108*(5587), 30–31.
- Turner, S. (2005). Behavioural symptoms of dementia in residential settings: A selective review of non-pharmacological interventions. *Aging & Mental Health*, *9*(2), 93–104.
- Walker, B. L., & Osgood, N. J. (2000–2001). Preventing suicide and depression: A training program for long-term care staff. *Journal of Death & Dying, (Spec. Elderly Suicide)*, *42* (1), 55–69.