

Effects of Emotion-Oriented Care on Work-Related Outcomes of Professional Caregivers in Homes for Elderly Persons

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Objectives. A randomized controlled trial was conducted on the effects of emotion-oriented care on professional caregivers in homes for elderly persons. Emotion-oriented care is used in the care for cognitively impaired elderly persons and is mainly based on the validation approach.

Methods. Sixteen homes for the aged were randomly allocated to an intervention or control group. Three hundred professional caregivers were included in the study. The eight intervention homes received a training program in emotion-oriented care. In the eight control homes, usual care was continued. Measurements about job satisfaction, burnout, and sick leave were performed at baseline and after 3, 6, and 12 months of follow-up.

Results. Modest positive effects were found in favor of the intervention group. These related to various aspects of job satisfaction and burnout. The effects were not very consistent over time. No differences in sick leave were found.

Discussion. From this study, as well as other studies in this field, no strong effects were found of emotion-oriented care on work-related outcomes in professional caregivers. Additional research is therefore needed. In these new studies, special attention should be given to the optimization of the implementation process of emotion-oriented care programs.

INCREASING numbers of elders are living in long-term care facilities. In the Netherlands, these facilities include so-called homes for elderly persons, of which there are approximately 1,400, with more than 100,000 residents and about 49,000 professional caregivers (CBS, 2000). Until the 1980s, these homes were mainly sheltered living facilities. From then on, they became more and more residential care facilities. At the moment, the care needs of residents resemble those of residents in nursing homes. Many residents in homes for elderly persons are highly dependent, partly because of psychogeriatric problems. This has resulted in an increased workload and increased complexity of work for the professional caregivers involved (in particular, nursing assistants and geriatric assistant nurses). In general, these caregivers have received little training in psychogeriatric care, entailing a need for extra education and instruction on how to approach psychogeriatric patients.

Several approaches or therapies have been developed in the care for these patients. One potentially promising approach to care management of disoriented elders and to the management of problem behavior is so-called "validation" (Feil, 1967, 1984, 1985, 1989, 1990, 1992; Feil & Wetzler, 1979). This approach has been developed by Feil, who described it as a process of communicating, both verbally and nonverbally, with disoriented elderly people by validating and respecting their feelings. Validation, or validation therapy, can be used for moderately to severely disoriented elderly people. It strives to accept the disoriented elderly per-

son as he or she is, to be nonjudgmental, and to share feelings that are freely expressed. Disoriented persons are not forced into "our reality," instead their perception of the environment is validated. In addition to positive effects for disoriented elders, Feil claims that validation increases job satisfaction, reduces frustration, and prevents burnout in professional caregivers (Feil, 1994).

A review of the literature on the effectiveness of validation (Schrijnemaekers, Duijnhouwer, te Wierik, & Frederiks, 1995) revealed that only a few studies addressed the effects on work-related outcomes of caregivers (Alprin, 1980; Blanchard et al., 1991; Nooren Staal, Frederiks, & te Wierik, 1995; Ronaldson & Savy, 1991). Besides being small, none of these four studies included a control group. We therefore concluded that there was insufficient evidence to support the claimed effects of validation on caregivers and that additional research, using more rigorous methods, was needed.

In the Netherlands, there is a tendency to apply validation in combination with other psychosocial care approaches, such as sensory stimulation and reminiscence. This combined approach, mainly based on the validation approach, is called emotion-oriented care (Finnema, 2000; Finnema et al., 1998). In a randomized controlled trial, we studied the effects of emotion-oriented care on disoriented elderly persons with behavioral problems and on professional caregivers in homes for elderly persons. In this article, we present the results for professional caregivers.

METHODS

The study was performed among caregivers in 16 homes for elderly persons in The Netherlands (province of Limburg) from the years 1997 to 1999. After prestratification, the 16 homes were randomly allocated to an intervention or control group, and subsequently a selection of professional caregivers was made. The eight intervention homes received emotion-oriented care training, whereas the eight control homes continued usual care. Measurements were performed at baseline and after 3, 6, and 12 months of follow-up. The study was approved by the Medical Ethical Committee (institutional review board approval) of Maastricht University Hospital.

Homes for Elderly Persons

To select homes for elderly persons, a postal questionnaire was sent to all 77 homes in the province of Limburg. Of those responding ($n = 59$), 31 homes fulfilled the eligibility criteria and were interested in the study. These homes were invited for an informative meeting about the trial. Next, interviews were held with staff members in the homes that were still interested in our study. Finally, 16 homes were selected. None had yet implemented an emotion-oriented care approach in the daily care of their residents, and they all had a day-care unit for psychogeriatric residents. These day-care units offer a structured day-care program to (usually 10–15) psychogeriatric residents.

Prestratification and Randomization

Randomization was performed on the level of homes. The homes were prestratified before randomization on two prognostic characteristics: the capacity of the day-care unit for psychogeriatric residents and the degree of care innovation in the homes (presence of resident-oriented care plans and a system of resident allocation). On the basis of these two characteristics, eight pairs of homes were formed. Next, within each pair, one home was randomly assigned to the intervention or control group, and the other home was assigned to the alternate state.

Study Population

A selection of caregivers was made for the study. They had to have been working for at least half a year in the homes and employed for at least 18 hours per week. Caregivers who were expected to be absent during the study period for a longer time (e.g., pregnancy leave) were excluded from the study. First, all caregivers of the day-care units were included in the study population. Next, a random sample of caregivers in the wards where the residents live was included.

To calculate the number of caregivers needed for the study, a difference of 1.5 points between the study groups on the job satisfaction subscales (primary endpoint, theoretical range from 0 to 12 points) was judged to be of clinical relevance. Based on a power of 90% (1-beta), an alpha of 5% (two-sided), and an estimated loss to follow-up of 25%, this led to a minimum of 80 caregivers per group. To obtain enough power for the subgroup analyses (see "Analyses"), the number of caregivers per group was increased to 150.

Interventions

The experimental intervention (intervention homes) was compared with standard care (control homes). The intervention offered to the eight intervention homes consisted of three successive elements: clinical lessons, a training program, and supervision meetings spread over a total period of 8 months. All three elements of the intervention were organized in-company and given by the same qualified and experienced teacher of a professional training organization.

Clinical lessons.—Every intervention home first received two identical lessons of approximately 1 hr. These lessons were offered to all employees (e.g., receptionist, domestic staff, nurses) and aimed at informing them about the study and the general ideas behind emotion-oriented care.

Training program.—Eight caregivers in each intervention home received a training program in emotion-oriented care. They had to be key figures in the daily care for cognitively impaired residents and had to be able to implement the emotion-oriented care approach in their home. In addition, they had to meet the following inclusion criteria: (1) working for at least half a year in the home for elderly persons; (2) being employed for at least 18 hours per week; (3) working for at least 1 year in geriatric care; and (4) having at least a secondary vocational training. On average, five caregivers from the day-care unit and three from the wards were selected for the training in each intervention home.

These caregivers received a 6-day training program. The first 4 days were given at intervals of 2 weeks, and the last two training days had an interval of 4 weeks. The training program had various goals. First, participants were taught about the dementia syndrome and various care models for communicating with elderly persons with dementia (e.g. reality orientation, validation, and reminiscence). Next, much attention was paid to the inequality of the resident-caregiver relation, the importance of going deeply into and trying to understand the residents' perceptions of the environment, and the attitude and (non-) verbal communication of staff toward the resident (including the use of sensory perceptions such as touch, smell, vision, and hearing). Finally, the implementation of practical skills received much attention. Various didactic methods, such as lecturing, homework, class assignments and exercises, role-playing, and video presentations, were used during the training.

Supervision meetings.—Three supervision meetings (half-a-day each) were offered to support the implementation of emotion-oriented care in daily care. They were held over a period of 4 months after training. The meetings were "tailor-made" for each intervention home. Depending on the problems described by the participants in the first supervision meeting, goals, agreements, and evaluations were discussed with those involved.

The eight control homes continued their usual care. To increase willingness to participate, the control homes were offered the training program after the end of the study.

Table 1. Overview of Outcome Measures Used for the 300 Professional Caregivers in 16 Homes for Elderly Persons

Outcome Measures	Theoretical Range ^a	Measurement Occasions (in months)
Primary Outcome Measures		
Job satisfaction (MAS-GZ)		
Head of the ward	0- <u>12</u>	0, 3, 6, 12
Quality of care	0- <u>12</u>	
Contact with colleagues	0- <u>12</u>	
Contact with residents	0- <u>12</u>	
Opportunities for self-actualization	0- <u>12</u>	
Secondary Outcome Measures		
Job satisfaction		
Short version MAS-GZ	0- <u>28</u>	0, 3, 6, 12
Two study-specific satisfaction-items		
Contact with residents	0- <u>4</u>	
Good care residents	0- <u>4</u>	
One general satisfaction-item	0- <u>4</u>	
Burnout (MBI)		
Depersonalization	0- <u>30</u>	0, 3, 6, 12
Emotional exhaustion	0- <u>48</u>	
Personal accomplishment	0- <u>42</u>	
Sick leave in last 3/6 months (no. of days)	0-92/183	0, 3, 6, 12
Opinion work situation	0- <u>6</u>	3, 6, 12

Note: MAS-GZ = Maastricht Work Satisfaction Scale for Healthcare; MBI = Maslach Burnout Inventory.

^aThe underlined score indicates the most favorable score for each scale/question.

Outcome Measures and Timing of Measurements

Table 1 gives an overview of the outcome measures, as well as the different measurement occasions.

The *primary outcome measure* was the job satisfaction of the professional caregivers. For this, we used some specific subscales of the Maastricht Work Satisfaction Scale for Healthcare (MAS-GZ; Landeweerd, Boumans, & Nissen, 1996a, 1996b). This questionnaire was specifically developed for health care workers and has been used in various Dutch health care settings, including homes for elderly persons. The MAS-GZ consists of seven subscales with three items, each of which have to be answered on a 5-point scale ranging from 0 (very dissatisfied) to 4 (very satisfied). In our study, 5 of the 7 subscales were selected: satisfaction with head of the ward, quality of care, contact with colleagues, contact with residents, and opportunities for self-actualization. The other two subscales (possibilities for promotion and clarity of tasks and rules) were considered to be less relevant for our study. The psychometric properties of the MAS-GZ are adequate. The internal consistency (Cronbach's alpha) of the five subscales used range from .67 to .87. Its validity (factorial and construct) also appears to be satisfactory (Landeweerd et al., 1996a, 1996b).

Secondary outcome measures were additional job satisfaction measures, burnout, sick leave, and the general opinion of the professional caregivers about their work situation. Additional job satisfaction outcomes were the short version of the MAS-GZ (an overall job satisfaction score; Landeweerd et al., 1996a, 1996b): two study-specific job satisfaction items (on actual contact with and being able to provide good care to cognitively impaired residents) and one general satisfaction item (relating to pleasure in one's work). Burnout was measured with the Dutch version of the Maslach

Burnout Inventory (MBI-NL; Schaufeli, Maslach, & Marek, 1993; Schaufeli & Van Dierendonck, 1994), a scale specially developed for evaluating burnout in the human services sector. The MBI consists of three subscales with a total of 20 items: emotional exhaustion (eight items), depersonalization (five items), and personal accomplishment (seven items), which have to be rated on a 7-point scale ranging from never (0) to daily (6). The internal consistency of the three MBI scales has been extensively tested in many samples with Cronbach's alpha values ranging from .71 to .91. The convergent validity, as well as the factorial validity, seems quite acceptable (Schaufeli et al., 1993).

Caregivers were asked to inform us about their sick leave before and during the intervention period (pregnancy leave excluded) and to rate the number of calendar days of absenteeism from sickness. At every time point, we also asked the caregivers to give their general opinion about their work situation compared with the baseline measurement. They had to rate their answer on a 7-point scale, ranging from extreme improvement (0) to extreme deterioration (6).

Analyses

Handling of missing data.—Missing values on items that were part of a scale or subscale were replaced according to the "mean value of valid subtests" principle (i.e., replacement by the mean value calculated from the valid item scores of the [sub-] scale obtained for the same subject at the same time point). This replacement strategy was only used if less than 25% of the items of a scale or subscale had missing values. Missing values on questions in single-item scales were not replaced.

Data analysis.—The hierarchical structure of the data (measurement occasions nested with caregivers, who are nested within homes for elders) results in dependencies among measurements, which makes multilevel analysis an appropriate tool for analyzing the data (Bryk & Raudenbush, 1992; Snijders & Bosker, 1999). This technique not only accommodates for dependencies, but also handles missing data in an adequate way (Snijders & Bosker, 1999). All analyses were performed using the MLwiN program (Rasbash, Browne, Healy, Cameron, & Charlton, 1999).

Assuming a linear trend for the outcome measures across time, we compared the intervention and control group. Besides unadjusted analyses, adjusted analyses were conducted in which the following covariates were added: age, workplace (ward vs day-care unit), work experience, number of sick reports, and hours of employment. In the linear trend analyses, the linear trend starting from the measurement at baseline is analyzed, which implies that possible differences on the outcome measures at baseline were also accounted for. In addition to the linear trend analysis, the effects of the intervention at short- (3 months), medium- (6 months), and long-term intervals (12 months) were examined. In the latter analysis, no specific relation was assumed between the outcome measure and time of measurement.

In these "overall" analyses, differences between intervention and control homes were tested without considering potential effect modifiers. Therefore, additional subgroup

analyses were performed, based on five characteristics measured at baseline: age, workplace (ward vs day-care unit), years of working experience, hours of employment per week, and number of sick reports.

All statistical analyses were conducted following the "intention-to-treat" principle. In addition, per protocol analyses (unadjusted and adjusted linear trend analyses, analyses per time period as well as subgroup analyses as described previously) were performed to examine the effect of the intervention as a function of the success of its implementation. These additional analyses were restricted to the primary outcomes. To conduct these analyses, we asked the teacher of the course to give an overall judgment of the success of the implementation of emotion-oriented care (good, moderate, or poor) for each of the eight intervention homes. The teacher was blinded with respect to the outcomes. She rated the implementation in three homes as "good," in three others as "moderate," and in two as "poor." In the per protocol analyses, the control homes ($n = 8$) were compared with intervention homes with good implementation ($n = 3$), and to intervention homes with moderate-to-poor implementation ($n = 5$).

RESULTS

Three hundred and eight caregivers were selected and invited to participate. Eight of them refused to participate, resulting in a study population of 300 caregivers: 155 in the intervention group and 145 in the control group. Ninety-four percent of the study population was female, with an average age of 36 years. The average work experience of these caregivers was 12 years. On average, they were employed for 27 hours per week.

Table 2 shows that the intervention and control groups were, to a large extent, comparable at baseline. As far as differences were present (e.g., regarding age and two subscales of job satisfaction), these were accounted for in the analyses.

Table 3. Numbers (Percentages) Response and Loss-to-Follow-up Per Group Professional Caregivers

Measurement	Loss to Follow-Up							
	Response		Structural Dropout		Discharge or Transfer		Incidental Nonresponder ^a	
	I	C	I	C	I	C	I	C
Baseline	154 (99)	139 (96)	—	—	1 (1)	1 (1)	5 (3)	
3 months	143 (92)	126 (87)	1 (1)		1 (1)	2 (1)	10 (6)	17 (12)
6 months	141 (91)	120 (83)	1 (1)		3 (2)	2 (1)	10 (6)	23 (16)
12 months	126 (81)	116 (80)	1 (1)	1 (1)	12 (8)	4 (3)	16 (10)	24 (17)

Notes: I = Intervention ($n = 155$); C = control ($n = 145$).

^aMainly from sickness or vacation.

Table 3 shows the response and loss to follow-up over time per study group. The response rate ranged from 98% at baseline to 81% after 12 months for the total population. The response was at every time point somewhat higher in the intervention group. Loss-to-follow-up from structural dropout was almost equal for the two study groups. The number of discharged or transferred employees was comparable up to 6 months, but was higher after 12 months in the intervention group. (Five of the 12 discharged or transferred caregivers in intervention homes worked in one home with organizational changes.) The numbers of incidental nonresponders were higher in the control group at every time point, probably because of a lower commitment with our study. None of these differences, however, was statistically significant (Fisher's exact test, two-tailed, $p > .05$).

Table 4 presents the unadjusted and adjusted estimated differences on the outcome measures between the two groups per month (assuming a linear trend), as well as the differences after 3, 6, and 12 months of follow-up (not assuming a particular trend). The adjusted analyses included

Table 2. Comparison Between the Intervention ($n = 154$) and Control ($n = 139$) Group Professional Caregivers on Baseline Characteristics

Characteristic	Empirical Range in Study Population ^a	Intervention Group ($n = 154$) ^b	Control Group ($n = 139$) ^b
Gender: female ($n, \%$)	—	147 (96)	129 (93)
Workplace: day-care unit ($n, \%$)	—	42 (27)	37 (27)
Age (mean years, SD)	20–58	35.2* (9.3)	37.7* (8.6)
Work experience (mean years, SD)	0–36	11.7 (7.1)	13.3 (7.2)
Hours employment per week (mean hours, SD)	18–40	27.6 (7.2)	26.6 (7.6)
Numbers of sick reports last half year (mean numbers, SD)	0–4	0.7 (0.8)	0.5 (0.8)
Job satisfaction (MAS-GZ; mean, SD)			
Head of the ward	0–12	7.5** (2.5)	8.2** (2.3)
Quality of care	0–12	6.0 (2.5)	6.1 (2.4)
Contact with colleagues	3–12	9.0 (1.6)	9.3 (1.3)
Contact with residents	5–12	9.6 (1.3)	9.6 (1.1)
Opportunities for self-actualization	1–12	7.3** (2.3)	8.0** (1.8)

Note: MAS-GZ = Maastricht Work Satisfaction Scale for Healthcare.

^aThe underlined score is the most favorable score.

^bSeven caregivers were unable to respond to the questionnaires at baseline (also see Table 3).

* $p < .05$; ** $p < .01$; *** $p < .001$.

Table 4. Estimated Differences Between the Intervention and Control Groups of Professional Caregivers on the Primary and Secondary Outcome Measures

Outcome Measures	Linear Trend Analyses ^a		Analyses/Time Period ^b		
	Estimated Difference/Month		Estimated Difference After 3 Months	Estimated Difference After 6 Months	Estimated Difference After 12 Months
	Unadjusted	Adjusted ^c			
Job Satisfaction (MAS-GZ)					
Head of the ward ^d	-0.08	-0.08	-0.28	-0.04	-1.08*
Quality of care ^d	-0.04	-0.05	-0.52	-0.45	-0.62
Contact with colleagues ^d	-0.01	-0.01	-0.16	-0.03	-0.23
Contact with residents ^d	+0.01	+0.01	0.10	+0.10	+0.06
Opportunities for self-actualization ^d	-0.05*	-0.05*	-0.28	-0.49	-0.73
Job Satisfaction					
Short version MAS-GZ ^d	-0.09*	-0.10*	-0.77*	-0.49	-1.28*
Two study-specific satisfaction items					
Contact residents ^d	-0.02*	-0.02*	-0.05	-0.15*	-0.22
Good care residents ^d	-0.02	-0.02	-0.20	-0.11	-0.22*
One general satisfaction item ^d	-0.01	-0.01	-0.10	-0.01	-0.16
Burnout (MBI)					
Depersonalization ^e	+0.05	+0.05	+0.40	+0.40	+0.55
Emotional exhaustion ^e	+0.14	+0.17	+1.58	+2.17*	+1.85
Personal accomplishment ^d	-0.11*	-0.11	-0.87	-0.61	-1.46*
Sick Leave (no. of days) ^f	-0.01	-0.14	-0.86	+0.70	-0.60
Opinion Work Situation ^e	-0.00	-0.00	+0.23	-0.07	+0.00

Notes: MAS-GZ = Maastricht Work Satisfaction Scale for Healthcare; MBI = Maslach Burnout Inventory.

^aThe estimated difference is the difference in deterioration or improvement per month between the control and intervention groups.

^bThe estimated difference is the difference in deterioration or improvement after 3, 6, and 12 months between the control and intervention groups.

^cAdjusted for age, workplace (ward vs unit), work experience, number of sick reports, and hours of employment.

^dNegative difference implies a better score for the intervention group.

^ePositive difference implies a better score for the intervention group.

* $p < .05$; ** $p < .01$; *** $p < .001$.

the baseline covariates age, workplace, work experience, number of sick reports, and hours of employment.

The interpretation of the results for the linear trend analyses is as follows. An estimated difference of, for example, -0.08 per month on the "head of the ward" subscale (job satisfaction, second column), indicates that the control group deteriorates on average 0.08 points more (or improves less) on this dimension, compared with the intervention group, assuming a linear trend between outcome and time. The estimated differences after 3, 6, and 12 months (fourth to sixth columns) do not assume a specific relationship between the outcome measure and time, and simply reflect the difference between the two groups after 3, 6, and 12 months, adjusting for baseline differences.

Table 4 shows that the majority of the differences in the linear trend analyses are in favor of the intervention group. Of these, four are statistically significant ($p < .05$): the job satisfaction subscale "opportunities for self-actualization," the short version of the job satisfaction scale, the study-specific satisfaction item "contact with residents," and the subscale "personal accomplishment" of the burnout questionnaire. Hardly any differences in results were found between the adjusted and unadjusted analyses (Table 4).

The analyses per time period also show modest effects in favor of the intervention homes. The differences between the groups are not very consistent over time, and as far as

they are statistically significant, they mainly appear after 12 months of follow-up.

Figure 1 presents graphically the average change over time for the three time points (3, 6, and 12 months) on the five job satisfaction subscales. These graphs show that the differences in favor of the intervention homes are most consistent (but not statistically significant) on the subscales "quality of care" and "opportunities for self-actualization."

The results of the estimated differences between the two groups per month (assuming a linear trend) and the effects after 3, 6, and 12 months of follow-up (not assuming a particular trend) yield more or less the same results for the primary and secondary outcome measures.

Next, subgroup analyses were conducted. These analyses did not reveal subgroups of caregivers with specific characteristics, which benefited clearly more, or less, from the emotion-oriented care approach. The effect of the intervention apparently does not depend on the age of the caregiver, his or her workplace (ward vs day-care unit), years of working experience, hours of employment per week, and the number of sick reports before the start of the study.

Finally, per protocol analyses were conducted for the primary outcomes. In these analyses, the teacher's overall judgment of the success of the implementation was used. In the per protocol analyses, the control homes ($n = 8$) were compared with intervention homes with good implementa-

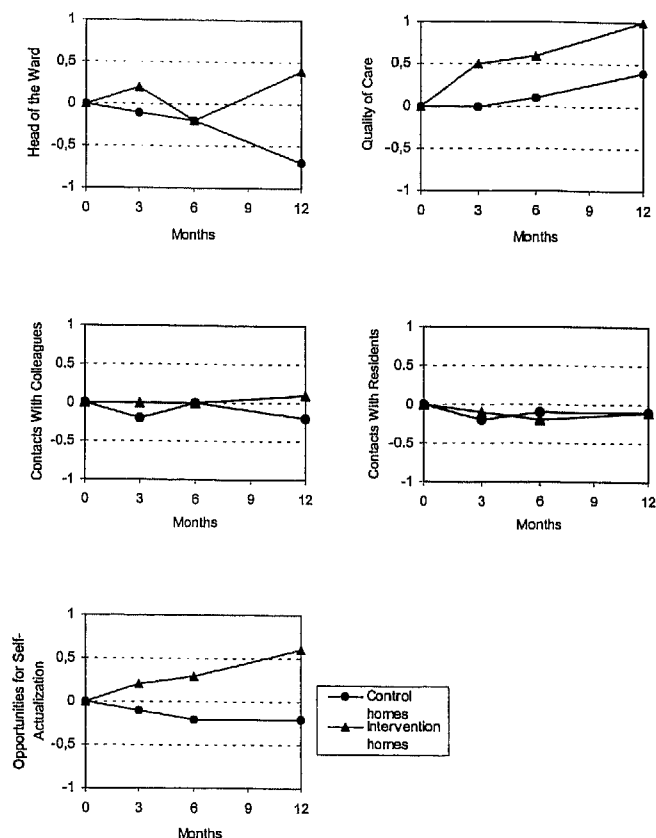


Figure 1. The average deterioration and/or improvement on five job satisfaction subscales for different time points in the intervention and control groups.

tion ($n = 3$), and with intervention homes with moderate-to-poor implementation ($n = 5$).

The linear trend analyses (unadjusted as well as adjusted) showed no differences between the control homes and the homes with good implementation on the primary outcome

measures. However, the intervention homes with moderate-to-poor implementation had better scores on three job satisfaction subscales (satisfaction with head of the ward, quality of care, and opportunities for self-actualization) than the control homes (Table 5).

These intervention homes also had a statistically significant better score in the analyses per time period after 12 months of follow-up on four of the five job satisfaction scales, compared with the control homes (data not shown).

DISCUSSION

One of the goals of emotion-oriented care is the improvement of job satisfaction and reduction of burnout and sick leave in (professional) caregivers. We found only modest positive effects in favor of the intervention group on some aspects of job satisfaction and burnout, and no differences in sick leave. These findings give rise to some questions. These will be addressed later, in which we will discuss specifically implementation of emotion-oriented care in the intervention homes, the results of the per protocol analyses, possible sources of bias, and the results of other studies on the effectiveness of validation or emotion-oriented care.

Implementation of Emotion-Oriented Care

The relatively intensive intervention was organized and given by a highly experienced and motivated teacher. An evaluation at the end of the 6-day training showed that the participants were (very) positive about the training and the teacher. The absence rate at training was only 9%, mainly because of illness.

To obtain some insight into the implementation process of emotion-oriented care in the intervention homes, an additional qualitative study using observations and interviews was performed. The observations (restricted to 1 day at the day-care unit in four intervention and four control homes) indicated that the practice of emotion-oriented care was not clearly observable in the homes 11 months after baseline. No clear differences in communication and interaction with the residents between the intervention homes and control

Table 5. Unadjusted and Adjusted Differences Between the Intervention and Control Groups of Professional Caregivers on the Primary Outcome Measures in Case of the Per Protocol Analyses

Primary Outcome	Estimated Difference ^a /Month in Linear Trend Analysis			
	Intervention+ vs Control ^b		Intervention- vs Control ^b	
	Unadjusted	Adjusted ^c	Unadjusted	Adjusted
Job Satisfaction (MAS-GZ)				
Head of the ward	-0.03	-0.03	-0.11*	-0.10*
Quality of care	+0.03	+0.02	-0.09*	-0.09*
Contacts with colleagues	+0.01	+0.01	-0.03	-0.03
Contacts with residents	+0.01	+0.01	+0.01	+0.01
Opportunities for self-actualization	-0.03	-0.04	-0.07*	-0.08*

Note: MAS-GZ = Maastricht Work Satisfaction Scale for Healthcare.

^aThe estimated difference is the difference in deterioration or improvement per month between the control and intervention group. A negative difference implies a better score for the intervention homes.

^bIntervention+ represents the three intervention homes with good implementation, whereas Intervention- relates to the 5 intervention homes with moderate-to-poor implementation.

^cAdjusted for age, workplace (ward vs unit), work experience, number of sick reports, and hours of employment.

* $p < .05$; ** $p < .01$; *** $p < .001$.

homes became visible. Interviews with management and caregivers in half of the intervention homes indicated that the caregivers had become more aware of their own actions and that their attitude toward the residents had changed. The interviewees also mentioned that organizational problems may have hindered the implementation of the intervention (van Heusden, Widdershoven, Schrijnemaekers, & van Rossum, 1999).

Results of the Per Protocol Analyses

In conducting the per protocol analyses, we anticipated a clear relation between the quality of implementation of emotion-oriented care and study outcomes. However, there were no differences between the intervention homes with good implementation and the control homes. Instead, some positive effects were found for the intervention homes with moderate-to-poor implementation. A possible explanation is that difficulty in implementing emotion-oriented care reflects organizational problems in the homes involved, leading to lower job satisfaction scores at baseline. Additional analyses indeed showed lower job satisfaction scores at baseline in intervention homes with moderate-to-poor implementation, compared with the intervention homes with good implementation. Therefore, the training program may have been most effective for caregivers with relatively low job satisfaction scores at the start. The fact that caregivers with low scores improved more may also be attributed to regression toward the mean.

Sources of Bias

Although much precaution was taken to prevent bias, our study may still have been subjected to it. Selection bias does not seem obvious. After prestratification and randomization, the homes and caregivers in the two groups appeared to be comparable. Also, the response rate was almost equally distributed over the study groups during the 1-year follow-up period.

The outcome measurements could not be blinded. Usually nonblinded observations lead to an overestimation of effects. Given the modest effects found, it does not seem very likely that information bias masked real existing effects. It is, however, possible that the effects found were caused by nonblinding.

The study showed some statistically significant ($p < .05$) effects of the intervention. When a Bonferroni correction is made for multiple testing ($\alpha/5 = .01$), none of these effects were significant. This demands an evaluation of the power of the tests performed. Therefore, we conducted post-hoc power analyses to clarify the power of this study, given the data collected. These analyses were based on an assumed clinically relevant difference of 1.5 points between the two groups on the job satisfaction subscales (ranging from 0 to 12 points), linear trend analyses accounting for clustering, five primary endpoints, a Bonferroni adjusted α of .01, and two-sided testing. The post-hoc power ranges from .69 to 1.0 on the five primary endpoints, implying that the study had sufficient power to detect meaningful differences between the two groups.

It was difficult to choose a suitable "placebo" or control intervention. Consequently, the intervention was compared

with standard care. This implies that the outcomes contain specific and nonspecific elements. As a result, it is unclear whether the modest effects result from nonspecific elements, such as increased social interaction and attention or from the emotion-oriented care training.

Comparison With Other Studies

Only a few studies about the effectiveness of validation or emotion-oriented care have addressed the effects on caregivers. Four small-scale studies without a control group on the effects of validation do not allow firm conclusions (Alprin, 1980; Blanchard et al., 1991; Nooren Staal et al., 1995; Ronaldson & Savy, 1991). In a recent Dutch trial by Finnema (2000), the effects of emotion-oriented care were examined. This trial studied, among other things, the effects on the general health of nursing assistants ($n = 99$) in nursing homes. General health was measured by feelings of stress, stress reactions, feelings of competence, and illness. For the total population, no statistically significant effects were found. Fewer stress reactions were found for one subgroup: nursing assistants in the experimental group who felt that they had obtained more emotion-oriented care skills during the study showed less stress reactions than comparable assistants in the control group.

Because the outcome measures differ considerably, the findings of Finnema (2000) are only to a small extent comparable with our results. The Finnema study and our study showed no differences in sick leave.

From this study, as well as other studies in this field, no strong effects were found of emotion-oriented care on work-related factors in professional caregivers. Therefore, additional research is needed. In these new studies, special attention should be given to the optimization of the implementation process of emotion-oriented care programs.

ACKNOWLEDGMENTS

This study was supported by grants from the National Center for Nursing & Care (LCVV), the Province of Limburg, and the VSB Fund.

We are grateful to the caregivers and their staff for their commitment to our study. We also thank Jos Slangen and Marijke Moll (Department of Epidemiology, Maastricht University) for their help in data collection and data management, and Hilde Ham (CEREIN) for organizing and performing the intervention in our study.

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Received February 6, 2001

Accepted October 8, 2001

Decision Editor: Fredric D. Wolinsky, PhD