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A Minimal-Contact Intervention for Cardiac Inpatients: Long-Term Effects on Smoking Cessation[1]

Catherine Bolman, Ph.D.,*[,2] Hein de Vries, Prof. Dr.,† and Gerard van Breukelen, Ph.D.‡

*Department of Social Sciences, Netherlands Open University, Heerlen, The Netherlands; and †Department of Health Education, and ‡Department of Methodology and Statistics, Maastricht University, Maastricht, The Netherlands

Background. **This study examined the 1-year effects of a minimal-contact smoking cessation intervention for cardiac inpatients.**

Methods. **The multicenter study included cardiac inpatients who had smoked prior to hospitalization. A pretest–posttest quasi-experimental design was used. Patients' experimental condition depended on the hospital they were assigned to. The design was partially randomized: 4 of the 11 hospitals selected the experimental condition themselves (2 experimental, 2 control), while the remaining 7 hospitals were randomly assigned. The experimental group consisted of patients of 5 hospitals (N = 388). Patients of 6 other hospitals served as the control group (N = 401). The intervention included stop-smoking advice by the cardiologist, brief counseling by the nurse, the provision of self-help materials, and aftercare by the cardiologist.**

Results. **Logistic regression analyses controlling for baseline differences and covariates did not show significant intervention effects on point prevalence and continuous abstinence. The study also showed that the outcomes were not significantly related to the way hospitals were assigned to the experimental condition.**

Conclusions. **While short-term effects were found, the minimal-contact intervention did not result in significant effects after 12 months, at least if patients lost to follow-up were treated as posttest smokers. Efforts should be made to improve the intervention, especially the aftercare.**

Key Words: **smoking cessation intervention; cardiac patients; coronary heart disease; smoking cessation.**

INTRODUCTION

Smoking cessation after the development of coronary heart disease (CHD) improves a patient's prognosis more than any other treatment [*1–5*]. It is therefore desirable to assist cardiac inpatients in attempts to stop smoking. The hospital setting presents an excellent opportunity for cessation interventions. During hospitalization, cardiac patients are more aware of their personal vulnerability to the dangers of smoking, and their serious illness makes them more motivated to quit and more receptive to interventions [*6–10*]. Moreover, patients have often been smoke-free for some days and may therefore be more willing to continue to refrain from smoking. Several randomized controlled studies have examined the efficacy of in-hospital smoking cessation interventions for patients with CHD [*6,11–15*]. One year posttest measurements in these studies revealed point prevalence abstinence rates ranging from 35 to 61% in experimental groups, compared with rates ranging from 28 to 54% in control groups [*6,11–15*]. However, these interventions were characterized by at least 1 h of behavioral counseling and several brief follow-up contacts after discharge, which made them very laborious.

A needs assessment by The Netherlands Society of Cardiovascular Nursing, however, revealed that cardiac nurses could spend only a maximum of 30 min on a smoking cessation intervention. Therefore, a minimal-contact smoking cessation intervention was developed and tested, an adaptation of the Minimal Intervention Strategy (MIS) that had already proved its efficacy in general practice [*16*]. The adapted version (called C-MIS) requires a time investment of 15–30 min. A search of the literature revealed no evaluation of such a minimal-contact intervention for hospitalized CHD patients.

The C-MIS was developed in order to stimulate smoking patients to quit and to prevent relapse among patients who have not smoked since hospital admission. Since the efficacy of the MIS had already been

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[2] To whom correspondence and reprint requests should be addressed at current address: Department of Social Sciences, Netherlands Open University, Postbox 2960, 6401 DL Heerlen, The Netherlands. Fax: 31(0)-45-5762939. E-mail: catherine.bolman@ou.nl.

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tested, the present study focused on assessing the effectiveness of the protocol in actual clinical circumstances [*17*]. For this purpose, the behavioral effects of the intervention after 12 months, as well as the predictors of quitting, were assessed. Furthermore, the main findings are described for the process evaluation that was conducted together with the 3 months of follow-up of the effectiveness study [*18*], in an attempt to improve our understanding of why the intervention was successful or unsuccessful.

METHODS

Patient Recruitment and Design

Twenty-one hospitals, selected at random, were approached for participation in the study by contacting the managers of the cardiac wards. Ten hospitals refused to participate for various reasons, including a ward merger, current participation in other time-consuming studies, staff turnover, and personnel shortage. Initially, the remaining 11 hospitals were to be randomly assigned to the treatment and control conditions. However, 2 hospitals wanted to serve only as controls, while 2 other hospitals wanted to be involved only in the experimental condition. This situation resulted in self-selected assignment of 4 hospitals and random assignment of the remaining 7 hospitals. Five hospitals served as the experimental group and 6 as the control group. Assignment of patients consequently depended on the hospital to which they had been admitted. Hospitals rather than patients were thus used as the units of assignment in order to avoid between-group contamination [*19*], since the latter would threaten the internal validity of the study [*20*]. The units of analysis were patients, nested within hospitals.

The study included three university and eight general hospitals. The number of beds in the hospitals ranged from 440 to 1174, while the number of beds on the coronary care wards ranged from 8 to 41. The cardiac treatments offered in the hospitals were comparable.

From November 1995 to May 1997, all patients who had smoked in the week prior to admission to the cardiac wards were recruited by ward nurses. During the enrollment period, patients' smoking behavior in the 7 days prior to admission was routinely assessed by nurses at intake. In emergency admissions, the patients' smoking behavior was assessed later, but within 96 h from admission. Every day during the briefing at shift change it was communicated whether a patient was eligible for inclusion in the study or whether his or her smoking behavior still needed to be assessed. Ward representatives[3] coordinated and supervised the process of inclusion. Ward representatives and nurses were explicitly instructed to ask all patients who were eligible for inclusion and to register refusals. Nurses informed patients about the study and asked them to volunteer for a project on smoking cessation, including the possibility of being a control group member. Nurses also handed patients a letter containing the same information as that provided orally by the nurse. Patients who were willing to participate had to give informed consent. Since nurses in the experimental condition could not be blinded for the treatment status (as they implemented the intervention), they were explicitly instructed by the researcher and ward representatives not to inform patients about the content of the control and experimental treatments. In the control group, only the ward representatives were informed about the treatment condition to which their ward had been allocated. In order to blind the control ward nurses for the condition to which their ward had been allocated, ward representatives were instructed to give their ward nurses only a minimum of information about the study design and not to inform them about the treatment condition to which the ward had been allocated.

A pretest was conducted by means of a questionnaire administered by ward nurses at the hospital after patients had signed their informed consent forms; posttests were conducted by means of questionnaires that were mailed to patients' home addresses at 3 and 12 months after hospitalization. Results of the posttest after 3 months have been reported elsewhere [*18*].

Intervention

Control patients received the usual care, indicating that no systematic attention was given to smoking. The experimental group received C-MIS, which consisted of stop-smoking advice by the cardiologist, followed by 15–30 min of standardized individual counseling and the provision of self-help materials (i.e., a self-help manual for smoking cessation and a brochure focusing on the relationship between smoking and the progression of CHD) by the ward nurse, and aftercare by the cardiologist. Ward nurses informed the cardiologists which patients needed cessation advice. The cardiologist provided this advice when seeing a patient during his daily ward visit. After this advice, the nurse visited the patient and provided more in-depth counseling, which was attuned to the patient's stage of change [*21*].

Counseling by the nurse included the following: standardized assessment of the patients' smoking behavior, their degree of addiction, and their motivation to quit; addressing the positive and negative consequences of quitting, especially in the case of patients who were unmotivated to quit; addressing the barriers to cessation (expected problems and high-risk situations) and patients' self-efficacy expectations concerning smoking cessation; and encouraging patients to set a date for quitting.

[3] Those who supervised and coordinated patient enrollment. In the experimental group they also supervised the implementation of the protocol.

Aftercare was provided by the cardiologist 4 to 6 weeks after hospital discharge, during the first check-up at the cardiology outpatient clinic. He or she addressed various aspects of smoking, depending on the new assessment he or she had made of the patient's smoking behavior. To remind the cardiologist, patients' hospital charts were stamped with a prompt by the nurse and included an intervention card briefly reporting the content of the in-hospital part of the intervention. This card was developed specifically for the present study and was used by nurses to record relevant smoking-related issues addressed by a patient and to record what aspects of the intervention had been completed. In addition, a copy of the intervention card and a letter in which the protocol was explained were sent to the patient's general practitioner. The letter asked the general practitioner to pay attention to the patient's smoking behavior if he or she visited the general practice after discharge from the hospital.

The intervention incorporated the principles of Bandura's Social Learning Theory [*22*] combined with the ASE model [*23*], theories on relapse prevention [*24*], the Stage of Change Theory [*21,25*], and motivational interviewing strategies [*26*].

Training on the Use of C-MIS

Prior to the start of the study, the nurses in the experimental group received a standardized 2-h training from the research team. The training included information on the rationale of and the theory behind the intervention. The steps of the protocol were explained and practiced in role-playing sessions. In addition, every ward received a training manual with instructions on how to implement the protocol and a poster with a description of the protocol, while every nurse received a pocket-size reminder chart including an overview of the protocol and standardized messages on the pros of quitting and on coping with high-risk situations. To ensure quality control, each ward appointed two ward representatives who supervised the interventions and to whom nurses could turn with questions. Moreover, meetings between the ward representatives and the researcher were held every 3 months to discuss progress, difficulties, and experiences. Cardiologists and general practitioners received the protocol and written instructions on how to use it.

Measurements

The posttest assessed point prevalence abstinence by asking: "Have you smoked during the last 7 days (even if only one puff)?" (yes/no). Second, continuous abstinence was assessed by asking: "Have you smoked since your hospital discharge (even if only one puff)?" (yes/no). The words "even if only one puff" were included in the question to indicate that absolute abstinence from smoking was required for both outcomes to be categorized as abstinent. Ethical and financial restraints precluded biochemical validation, so only self-reports were used. To enhance the validity of these self-reports, a bogus pipeline procedure was used [*27–29*]. According to this procedure, patients were informed that they could be invited for a saliva test to validate their self-report. Simultaneously, they were asked to give permission for such a test.

Baseline measurements included age, gender, educational level, prior admission for cardiovascular diseases (CVD), reason for admission, 24-h quitting attempts in the year prior to admission, age of smoking onset, hospital type, and nicotine dependence. The latter was measured by means of the Fagerström test for nicotine dependence (FTND), which includes the daily number of cigarettes [*30*]. Cognitive factors measured at baseline included an assessment of perceived positive (pros) and negative consequences (cons) of quitting, social support, modeling, self-efficacy expectations, intention to quit, and motivational phases of behavioral change. Cognitive factors were measured according to the ASE model of behavioral determinants [*31,32*]. Reliability of all cognitive scales was sufficient (Cronbach's $\alpha > 0.60$), as has been reported elsewhere [*33*]. Motivational phases were assessed by using an adaptation of the Stage of Change typology [*25*]. Some alterations were necessary to make this typology fit the situation of Dutch cardiac inpatients, since some patients could be forced into action by nonsmoking regulations in force at the ward or by physical constraints, while others acted because they were motivated to quit. Therefore, a distinction was made between externally and internally motivated actors. Furthermore, the study did not include maintainers, since only patients who had smoked during the week prior to admission were included. Finally, the category of preparators was combined with that of contemplators, due to the small group size ($N = 11$). These alterations resulted in the following motivational phases at baseline:

Smokers in the precontemplation phase had smoked since hospital admission and were not planning to quit within 6 months.

Smokers in the contemplation phase had smoked since hospital admission and were planning to quit within 6 months.

Externally motivated actors had been abstinent since hospital admission due to ward regulations or because of physical constraints and intended to start smoking again as soon as possible.

Internally motivated actors had been abstinent since hospital admission and intended to continue to refrain from smoking [*33*].

Analyses

To check the comparability between the groups at baseline, χ^2 statistics were used for categorical and

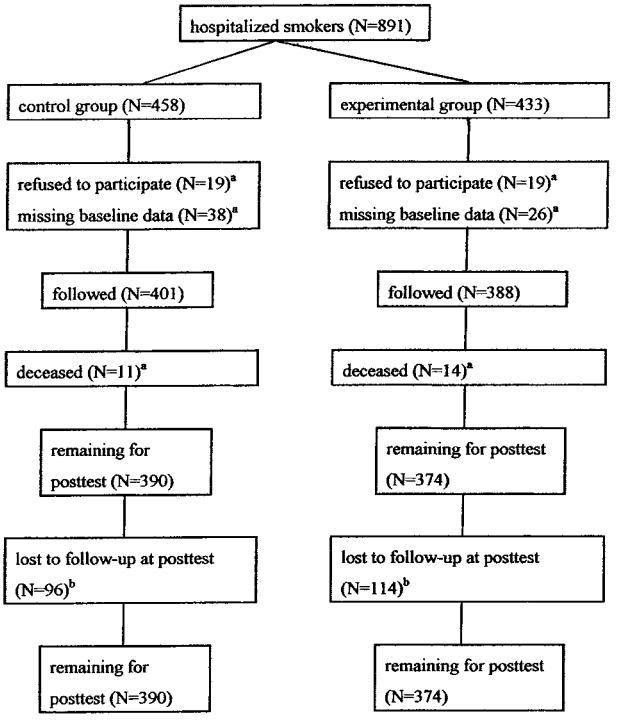
dichotomous variables (gender, educational level, prior admission for CVD, reason for admission, 24-h quitting attempts in the year prior to admission, motivational phase, hospital type), while *T* tests were used for continuous variables (age, intention to quit, pros, cons, self-efficacy expectations, social support, modeling, FTND score, age of smoking onset). A significance level of 0.05 was used for these analyses.

In order to determine whether the follow-up population showed selective loss to follow-up,[4] attrition between pretest and posttest was assessed by logistic regression with attrition as the dependent variable and baseline characteristics as predictors.

The effect of the intervention was tested by means of logistic regression analyses on point prevalence abstinence and continuous abstinence. Patients lost to follow-up were classified as smokers (last observation carried forward procedure). Pretest variables were always included as covariates to correct for baseline differences and to increase the statistical power by reducing unexplained variance [*34*]. Furthermore, since 4 of the 11 hospitals had selected their own treatment instead of being randomized to the experimental or control condition, a dummy-coded indicator of self-selection of treatment was constructed as an additional covariate to correct for self-selection in the analyses. The "treatment" factor and the covariates were entered first as one block, followed by possible interactions between (a) treatment and reason for admission, (b) treatment and motivational phase, and (c) treatment and self-selection of treatment. The rationale for the first two interactions was derived from the literature [*11,15*]. The last interaction assessed whether the treatment effect differed between the self-selected and randomized hospitals. To avoid the problem of multiple testing, we did not test for any other interactions. Nonsignificant interaction terms were removed using a backward elimination procedure ($\alpha = 0.10$, two-tailed). One-tailed tests ($\alpha = 0.05$) were used to test the treatment (intervention) effect because it was hypothesized that the intervention would be superior to usual care.

Logistic regression assumes that outcomes are statistically independent. However, this study had a nested design with patients nested within hospitals, which could lead to intraclass correlation. Since entire hospitals, rather than individuals within hospitals, were assigned to treatment conditions, ignoring intraclass correlation might cause underestimation of the standard error of the treatment effect, as well as too narrow confidence intervals and possibly type 1 errors [*35–37*]. Therefore, final logistic regression models were rerun using EGRET random effect logistic regression [*38*], treating patients as nested within hospitals and hospital as a random effect.

Since the regression analysis of treatment condition on abstinence was adjusted for all baseline patient variables recorded, we were also able to examine which patient characteristics were predictive of quitting, while controlling for treatment. They were tested with two-tailed significance. In order to decrease the probability that significant differences would occur merely by chance due to multiple testing, a significance level of 0.01 was used for this examination.



[a]excluded from all analyses (N=127), [b]included as smokers

FIG. 1. Numbers of patients.

RESULTS

Response and Attrition

We identified 891 hospitalized smokers. Registration by ward nurses of patients who were not willing to participate showed that 38 patients (4%) refused. Reasons for refusal could not be obtained since medical ethics policies did not allow this. Furthermore, 64 patients (8%) were excluded because the baseline questionnaire had not been filled out. Of the remaining patients ($N = 789$), 401 (51%) were control group members, while 388 (49%) belonged to the experimental group.

As shown in Fig. 1, 14 experimental patients (4%) and 11 control patients (3%) were deceased after 1 year

[4] Patients lost to follow-up included those who did not return the follow-up questionnaire. No distinction was made between those who were no longer willing to cooperate with the follow-up and those who could not be reached.

TABLE 1

Baseline Characteristics for Experimental and Control Cardiac Inpatients (N = 789)

	Experimental group (N = 388)	Control group (N = 401)	P value[a]	P value[b]
Prior admission for CVD, %	25%	39%	<0.01	N.S.
Educational level, up to secondary vocational school, %[c]	86%	78%	<0.05	N.S.
Reason for admission, %			<0.01*	N.S.
Myocardial infarction	37%	30%		
Angina pectoris	23%	25%		
Unknown[d]	24%	11%		
Other[e]	16%	35%		
Intention to quit, mean ± SD (1 = very weak, very strong = 10)	7.74 ± 2.2	6.36 ± 2.9	<0.01	<0.05
Motivational phase, %			<0.01*	<0.05
Precontemplation	1%	10%		
Contemplation	7%	12%		
External motivation phase	29%	35%		
Internal motivation phase	63%	43%		
Pros, mean ± SD (0 = no pros, 3 = many pros)	1.78 ± 0.5	1.68 ± 0.6	<0.05	N.S.
Cons, mean ± SD (0 = no cons, −3 = many cons)	−1.28 ± 0.7	−1.34 ± 0.7	N.S.	N.S.
Social support, mean ± SD (−3 = much discouragement, 3 = much support)	1.38 ± 0.9	1.04 ± 1.0	<0.01	N.S.
Modeling, mean ± SD (−1 = no one smokes, 1 = everyone smokes)	0.09 ± 0.4	0.06 ± 0.4	N.S.	N.S.
Self-efficacy expectations, mean ± SD (−3 = very difficult, 3 = very easy)	−1.55 ± 1.3	−1.66 ± 1.3	N.S.	N.S.
Age at smoking initiation, mean ± SD	17.2 ± 4.5	17.1 ± 4.3	N.S.	N.S.
Level of addiction, Fagerström, mean ± SD (−1 = no physical addiction, 10 = serious addiction)	4.3 ± 2.3	4.4 ± 2.4	N.S.	N.S.
Previous quit attempt, %	45%	55%	N.S.	N.S.
Age, mean ± SD	56.2 ± 10.6	57.3 ± 10.9	N.S.	N.S.
Self-selection of treatment (yes), %	65%	46%	—[f]	N.S.
Type of hospital (general hospital), %	79%	54%	—[f]	N.S.
Men, %	78%	78%	N.S.	N.S.

[a] Patients were the unit of analysis.
[b] Hospitals were the unit of analysis.
[c] Educational level up to lower level vocational training degree.
[d] Includes those patients whose reason for admission was missing.
[e] Includes those patients whose reason for admission was coronary angioplasty, cardiac dysrhythmias, or decompensatio cordis; these reasons for admission were combined because of the rarity of their occurrence.
[f] Not tested assuming patients as units of analysis.
* P values concern overall χ^2 tests (df = 3).

and therefore excluded. One hundred fourteen experimental patients (30%) and 96 control group members (25%) were lost to follow-up, resulting in a study sample of 554 of the 764 living responders (73%). Logistic regression analysis of these 764 patients, with attrition as the dependent variable and baseline variables and treatment as predictors, revealed that patients lost to follow-up were more often from the experimental group (odds ratio (OR) = 1.81, 95% confidence interval (CI) = 1.26–2.61, $P < 0.01$), had made more attempts to quit prior to the study (OR = 1.52, 95% CI = 1.06–2.18, $P < 0.05$), had experienced more pros of quitting at the pretest (OR = 1.44, 95% CI = 1.04–1.98, $P < 0.05$), had experienced fewer nonsmokers in their environment (modeling) (OR = 0.48, 95% CI = 0.30–0.75, $P < 0.01$), and had weaker intentions to quit (OR = 0.86, 95% CI = 0.80–0.92, $P < 0.001$).

In view of the nested nature of our study, attrition was also examined with hospitals instead of patients as the unit of analysis. The rate of patients lost to follow-up ranged from 28 to 38% for the experimental hospitals and from 19 to 28% for the control hospitals (excluding one hospital with only N = 7 patients and no patients lost to follow-up). So the rate of patients lost to follow-up was higher in each of the experimental hospitals than in each of the control hospitals, excepting one tie, i.e., an equal dropout rate of 28% (see Table 4 for details).

Baseline Characteristics

Since hospitals were the unit of assignment to the treatment condition, baseline comparisons of hospital characteristics, i.e., type of hospital (general vs university) and self-selection of treatment (yes vs no), must evidently use hospitals as the unit of analysis (see Table 1). For baseline comparisons of patient characteristics, Table 1 presents two significance levels, one using patients as the unit of analysis (which is too liberal) and one using hospitals as the unit of analysis (which is too conservative).

Using patients as the unit of analysis, we found a

TABLE 2

Predictors of Point Prevalence Abstinence ($N = 735$)[a]

Variable	β	SE	Odds ratio	P
Experimental (1) versus control (0)	−0.03	0.19	0.97	0.89
Prior admission (1) versus no prior admission for CVD (0)	−0.42	0.21	0.66	0.05
Up to secondary vocational school (1) versus higher education (0)	0.16	0.23	1.18	0.47
Reason for admission[b,c]				
Admission for myocardial infarction (1) versus other (0)	0.54	0.24	1.71	0.03
Admission for angina pectoris (1) versus other (0)	0.51	0.26	1.66	0.05
Admission for unknown (1) versus other (0)	0.18	0.28	0.53	0.53
Pretest intention to quit	0.16	0.04	1.18	0.001
Motivational phase[d,e]				
Contemplation phase (1) versus precontemplation phase (0)	−0.38	0.63	0.68	0.54
External motivation phase (1) versus precontemplation phase (0)	0.41	0.53	1.50	0.44
Internal motivation phase (1) versus precontemplation phase (0)	0.71	0.54	2.04	0.19
Mean pretest score on pros (0/+3)	−0.04	0.17	0.96	0.83
Mean pretest score on cons (0/−3)	−0.001	0.14	0.99	0.99
Mean pretest score on social support (−3/+3)	0.14	0.10	1.15	0.16
Mean pretest score on modeling (−1/+1)	0.83	0.23	2.28	0.001
Mean pretest score on self-efficacy expectations (−3/+3)	0.11	0.08	1.12	0.14
Age at smoking initiation	0.01	0.02	1.01	0.65
Level of addiction, FTND score (1–10)	−0.09	0.04	0.91	0.03
Previous quit attempt, yes (1), no (0)	−0.31	0.19	0.73	0.11
Age (28/86)	0.01	0.01	1.01	0.45
Self-selection of treatment, yes (1), no (0)	0.14	0.20	1.16	0.46
Type of hospital, general (1), university (0)	0.03	0.21	1.03	0.87
Gender, female (1) versus male (0)	−0.29	0.21	0.75	0.17
Constant (intercept)	−2.38	0.84		0.01

[a] N differs from N according to Fig. 1 due to missing values on several variables.
[b] Reference category is other.
[c] Wald test = 6.68, $df(3)$, $P = 0.08$.
[d] Reference category is precontemplation phase.
[e] Wald test = 9.02, $df(3)$, $P = 0.03$.
−2 log likelihood empty model = 978.8; −2 log likelihood final model = 836.9.

number of baseline differences between the experimental and control groups (see Table 1). An overall χ^2 test showed a significant difference in the reasons for admission. Although the groups differed only slightly with respect to admission for myocardial infarction and angina pectoris, substantial differences were found in the two remaining categories. Motivational phase also differed significantly between the groups. Most importantly, the experimental group consisted of more patients in the internal motivation phase and fewer patients in the precontemplation phase. The experimental group also included significantly more patients who had a low educational level, i.e., up to secondary vocational school, and fewer patients who had previously been admitted for CVD. As regards the cognitive factors, experimental patients reported significantly stronger intentions to quit smoking after hospital discharge, reported more pros of quitting, and perceived more social support.

Using hospital as the unit of analysis, we only found two baseline differences significantly related to treatment condition. Intention to quit was significantly stronger in the experimental hospitals. Furthermore, in the experimental hospitals, significantly more patients were situated in the internal motivation phase. All baseline variables were included as covariates in the regression analysis.

Effects on Smoking Cessation

Logistic regression of both outcomes revealed no interaction between treatment and reason for admission, motivational phase, or self-selection of treatment (all $P > 0.40$ using likelihood ratio tests).

The logistic regression analysis for point prevalence abstinence (Table 2) did not show a significant intervention effect (OR = 0.97, 90% CI = 0.71–1.34, $P = 0.89$) nor a main effect of the indicator of self-selection of treatment on the outcome.

Neither was there any intervention effect on continuous abstinence (Table 3) (OR = 1.17, 90% CI = 0.85–1.61, $P = 0.43$). As shown in Table 3, the main effect of the indicator of self-selection was not significant either.

Reanalysis of the regression models in Tables 2 and 3 with random effect logistic regression in EGRET [*38*] revealed treatment effects similar to those found with SPSS. The EGRET analysis on point prevalence abstinence showed an odds ratio of 0.98 ($P = 0.92$) for treatment, while the EGRET analysis on continuous abstinence showed an odds ratio of 1.17 ($P = 0.43$) for

TABLE 3

Predictors of Continued Abstinence ($N = 735$)[a]

Variable	β	SE	Odds ratio	P
Experimental (1) versus control (0)	0.16	0.20	1.17	0.43
Prior admission (1) versus no prior admission for CVD (0)	−0.57	0.22	0.57	0.01
Up to secondary vocational school (1) versus higher education (0)	−0.52	0.24	0.59	0.03
Reason for admission[b,c]				
Admission for myocardial infarction (1) versus other (0)	0.64	0.25	1.89	0.01
Admission for angina pectoris (1) versus other (0)	0.42	0.27	1.53	0.12
Admission for unknown (1) versus other (0)	0.06	0.30	1.06	0.83
Pretest intention to quit	0.13	0.05	1.14	0.005
Motivational phase[d,e]				
Contemplation phase (1) versus precontemplation phase (0)	−0.70	0.71	0.50	0.33
External motivation phase (1) versus precontemplation phase (0)	0.46	0.58	1.58	0.43
Internal motivation phase (1) versus precontemplation phase (0)	0.70	0.59	2.01	0.24
Mean pretest score on pros (0/+3)	−0.07	0.18	0.93	0.70
Mean pretest score on cons (0/−3)	−0.18	0.15	0.84	0.22
Mean pretest score on social support (−3/+3)	0.13	0.10	1.14	0.20
Mean pretest score on modeling (−1/+1)	0.79	0.24	2.20	0.001
Mean pretest score on self-efficacy expectations (−3/+3)	0.14	0.08	1.15	0.07
Age at smoking initiation	0.02	0.02	1.02	0.44
Level of addiction, FTND score (1–10)	−0.05	0.04	0.95	0.24
Previous quit attempt, yes (1), no (0)	−0.23	0.20	0.79	0.25
Age (28/86)	−0.01	0.01	0.99	0.52
Self-selection of treatment, yes (1), no (0)	0.16	0.20	1.17	0.44
Type of hospital, general (1), university (0)	0.01	0.22	1.01	0.95
Gender, female (1) versus male (0)	−0.47	0.23	0.63	0.04
Constant (intercept)	−2.28	0.88		0.01

[a] N differs from N according to Fig. 1 due to missing values on several variables.
[b] Reference category is other.
[c] Wald test = 9.06, $df(3)$, $P = 0.03$.
[d] Reference category is precontemplation phase.
[e] Wald test = 9.51, $df(3)$, $P = 0.02$.
−2 log likelihood empty model = 925.7; −2 log likelihood final model = 797.9.

treatment. The random hospital effect was not significant in either of the two analyses ($P > 0.40$). Furthermore, the residual variance between hospitals was estimated to be 0.01 for point prevalence abstinence and 0.00 for continuous abstinence, yielding an intraclass correlation of 0.003 and 0.000, respectively [*39*].

The absence of interactions between (a) treatment and motivational phase and (b) treatment and reason for admission was contrary to our expectations. The absence of an interaction between treatment and motivational phase could have been caused by a lack of power, since the variable consisted of four categories, which necessitated the use of $df(3)$ to test the interaction, and had a skew distribution with the first two categories underrepresented (see also Table 1). To check whether the absence of significant interaction was due to a lack of power, we reanalyzed the final regression models, including the interaction between phase of change and treatment with phase of change dichotomized. Based on our expectations that internally motivated patients would profit significantly more from the intervention than patients in the other motivational phases, precontemplators, contemplators, and externally motivated actors were categorized as one group (reference group, $N = 351$) and internally motivated actors ($N = 391$) as the comparison group. These analyses showed no interaction either (both $P > 0.51$). Dichotomization of reason for admission was deemed unnecessary because the sample was equally distributed among the various reasons for admission and both $P > 0.45$ for the interaction between reason and treatment.

Viewing the percentages of quitting for each individual hospital, Table 4 shows that the experimental hospitals generally showed higher point prevalence and continuous abstinence cessation rates than the control hospitals. Since the raw abstinence percentages represent abstinence rates in which patients lost to follow-up were considered as smokers, differences of 9 and 11% were found for point prevalence abstinence and continuous abstinence, respectively. This table is only descriptive, however, since its contents are raw percentages, unadjusted for baseline differences, and without significance test.

Additional application of the final regression models obtained from the total sample ($N = 764$) to only the patients situated in the seven hospitals that were randomized ($N = 343$) confirmed the absence of effects on point prevalence abstinence (OR = 0.74, 90% CI =

TABLE 4

Smoking Cessation per Hospital (Raw %)

	Point prevalence abstinence (%)			Continuous abstinence (%)		
	Abstinent[a]	Smoking	Patients lost to follow-up	Abstinent[a]	Smoking	Patients lost to follow-up
Experimental condition						
Hospital 1[b] (N = 80)	44	27	29	40	31	29
Hospital 2[b] (N = 162)	48	23	29	42	29	29
Hospital 3 (N = 39)	46	26	28	41	31	28
Hospital 4 (N = 48)	38	29	33	31	36	33
Hospital 5 (N = 45)	33	29	38	33	29	38
Control condition						
Hospital 1[b] (N = 117)	32	40	28	26	46	28
Hospital 2[b] (N = 62)	21	55	24	18	58	24
Hospital 3 (N = 62)	26	48	26	16	58	26
Hospital 4 (N = 78)	49	32	19	41	40	19
Hospital 5 (N = 64)	26	47	27	19	55	26
Hospital 6 (N = 7)	43	57	0	43	57	0
Mean in five experimental hospitals (%)	42	27	31	38	31	31
Mean in six control hospitals (%)	33	46	21	27	52	21

[a] Since all percentages are based upon the total sample size including dropouts, the percentage abstinent is equal to the abstinence rate according to the last observation carried forward analysis with dropouts as smokers.
[b] Self-selected hospital.

0.41–1.31, P = 0.28) as well as on continuous abstinence (OR = 1.02, 90% CI = 0.57–1.37, P = 0.94).

Predictors of Quitting

Tables 2 and 3 show which baseline characteristics were significantly (α = 0.01) associated with patients' smoking status at follow-up. Tables 2 and 3 show that point prevalence abstinence and continuous abstinence were positively associated with patients' intention to quit smoking, as well as with the perception of more nonsmokers in the social environment (modeling).

A check of assumptions of logistic regression gave satisfactory results: no independent variables showed collinearity, and none of the patients could be considered an outlier or influential case.

Main Findings of Process Evaluation

The process evaluation that was conducted together with the 3-month follow-up of the present effectiveness study has been described elsewhere [*18*]. The main findings, however, are briefly included below.

The process evaluation conducted among experimental patients revealed that 84% reported having spoken about smoking cessation with the nurse during their hospitalization, while 75% reported that the cardiologist had assessed their smoking behavior during the aftercare. Furthermore, 76% of the patients had received the self-help manual and 65% had received the brochure. They reported that the conversation(s) with the nurse had helped them to perceive more pros and fewer cons of quitting ($\bar{x}$ = 7.0; SD = 2.89; scale range: 1 (not at all) to 10 (to a large extent)). As regards the extent to which the conversation had enabled patients to cope with high-risk situations and to obtain support in their immediate environment (using similar answering scales), patients were less positive, with average scores of 5.7 (SD = 3) and 5.9 (SD = 2.99), respectively. Self-help materials were read moderately well: patients scored averages of 6.39 (SD = 3.64) and 6.23 (SD = 3.85) for the self-help guide and brochure, respectively (scale range: 1 (not read it) to 10 (read it all).

DISCUSSION

The present study examined the 1-year effectiveness of C-MIS on smoking cessation in which patients lost to follow-up (27%) were included as smokers. The latter procedure has been recommended by various researchers as a protection against biased treatment effects due to patient selection [*40–43*]. On the other hand, treating all dropouts as posttest smokers may yield a too conservative estimate of the effect if there is substantial loss to follow-up, as in the present study [*42–44*].

No effects were found for both outcomes. Because randomization failed in 4 hospitals, this part of the study became quasi-experimental or observational. However, no evidence was found that the differences in the randomization procedure (i.e., self-selection by 4 of the 11 hospitals), resulting in a quasi-experimental part (for 4 hospitals) and an experimental part (for 7 hospitals), had influenced the treatment effect (as the interaction effect) or the smoking cessation outcomes (as the main effect). But even if there was a main effect of self-selection on the smoking outcomes, this was adjusted for by retaining the indicator for self-selection in the regression models. The regression analyses among a subgroup of patients (N = 343) who were

admitted to the 7 randomized hospitals confirmed the absence of treatment effects as found in the analyses for the total sample. Furthermore, the random effect logistic regression analyses within the total sample failed to show a random hospital effect and confirmed the SPSS analyses. Since no interaction was found between the treatment condition and either the reason of admission or the motivational phase of change, there is no evidence that the intervention effect depended on the reason for admission or the motivational phase.

Because the present study had several shortcomings that might have threatened its internal validity (e.g., confounding may not have been under complete control due to unmeasured or inadequately measured baseline variables), conclusions concerning causality must be formulated and interpreted with caution. With this in mind, we carefully conclude that, in contrast to the results after 3 months of follow-up [*18*], the present study failed to show effectiveness at 12 months after the intervention. Since a large proportion of patients were included as smokers because they were lost to follow-up, a real effect of the treatment could have been masked. This is suggested by the fact that our results were not confirmed in similar regression analyses which excluded patients lost to follow-up. These analyses indicated significant intervention effects for both outcomes (point prevalence abstinence: OR = 1.63, 95% CI = 1.13–2.34, $P = 0.03$; continuous abstinence: OR = 1.86, 95% CI = 1.30–2.66, $P = 0.005$).

Comparison of our results with those of other studies among cardiac inpatients is hindered by differences in the methodologies used. Three of the five studies reported in the literature included biochemical validation [*13–15*]. Furthermore, all interventions were more intensive than ours, while two of the five studies measured the effects at 6 and 18 months [*11,45*], and most studies used a truly randomized controlled design [*11,13–15*]. Finally, our loss to follow-up rate was higher, which may have led to more conservative estimates in our study. In line with our study, all studies mentioned here regarded patients lost to follow-up as smokers. As in the present study, two other studies also failed to find an effect [*14,45*]. Johnson and colleagues measured 6-months effects by self-reports in a quasi-experimental study. They attributed the absence of a significant effect to a type II error (46% smoking abstinence in the intervention group, 31% in the control group) [*45*]. However, their rate of loss to follow-up was only 11%, compared to 27% in our study. In a randomized controlled trial, Rigotti and colleagues found identical 12-months continuous nonsmoking rates in the intervention and control groups (51%) and a small, nonsignificant difference in point prevalence abstinence (61% in the intervention group, 54% in the control group) [*14*]. They reported no loss to follow-up, attributed the absence of effects to low strength of the intervention and bad timing, and suggested targeting patients after discharge from the hospital. In contrast to our findings, Taylor and colleagues [*13*] found a significant intervention effect in their biochemically validated analysis of point prevalence abstinence (61% versus 32% in the intervention and control groups, respectively). Their intervention was more intensive and used a long period of aftercare. Ockene and colleagues also reported an intervention effect, but only for patients with severe MI [*15*]. This effect disappeared at the 18-months follow-up measurement [*11*]. Miller and colleagues showed a significant intervention effect on point prevalence abstinence (OR = 1.6 compared to usual care) [*6*]. Their repeated telephone support after discharge (four calls) increased the cessation rate significantly after 1 year. Two meta-analyses showed that high cessation rates can be achieved if health care providers collaborate and use behavioral counseling, nicotine replacement therapy, and aftercare [*41,46*].

In conclusion, comparison of our study with the successful studies described above revealed that our effect estimations were more conservative, so the effect may have been underestimated. The studies also suggest, however, that our intervention could be improved by including intensive aftercare with several follow-up telephone calls and nicotine replacement therapy, while the in-hospital counseling might also need to become more intensive. Furthermore, process evaluation results obtained at our 3-month follow-up indicate that the implementation of the intervention needs improvement, since the present results were obtained under conditions of moderate intervention coverage.

Both point prevalence and continuous abstinence were related only to the number of smokers perceived in the environment (modeling) and the intention to quit at the pretest. The influence of pretest intention has also been reported in other studies among CHD patients [*13,14,47*], as has the influence of modeling [*47,48*]. Most studies that reported on the predictors of smoking cessation among CHD patients, however, did not correct for multiple testing, which impedes comparison.

The present study was subject to several weaknesses. First, loss to follow-up was high despite three postal reminders, a telephone reminder, and the provision of an incentive by making patients eligible to win a $25 prize. Many patients had changed their addresses without informing us. Future studies are recommended to use a tracking system based on hospital records. Follow-up assessments by telephone are recommended, since most studies among cardiac inpatients that used this procedure reported attrition rates of 10% or less [*6,12,14,15*]. Second, selective loss to follow-up occurred; i.e., loss to follow-up was related to variables that are known to be predictors of smoking

abstinence among CHD patients [*14,15,45*]. Therefore, the study suffered from information bias (due to the possibility of differential misclassification of smoking status between the two conditions). This affects the internal and external validity of the study and therefore the validity of our conclusions. Third, no biochemical confirmation was used. Efforts were undertaken to overcome this deficiency by using an identification coding system, by ensuring patients' anonymity and confidentiality, and by applying a bogus pipeline procedure, which has been justified in a number of studies [*27–29*]. However, while several intervention studies in the general population [*27,49,50*] as well as for cardiac patients [*6,13,15,51*] have reported that treatment groups were not more likely to misrepresent their smoking behavior than controls, patients may have underreported or falsely reported their smoking behavior. Fourth, the sample may not have been representative. This especially might have been the case in the experimental group because nurses in this group could not be blinded for treatment condition. Although the recruitment procedure was supervised by ward representatives and nurses were explicitly instructed to ask every eligible patient, nurses may have been selective in their recruitment (selection bias). The long period needed to include sufficient numbers of patients suggests that not all smoking patients were approached. Nurses confirmed this, though mentioning time constraints and forgetfulness as the most important causes. We were not able to assess the potential magnitude of this bias since, despite our request, neither the numbers of all smoking patients admitted to the hospital nor nurses' adherence to the intervention was monitored by the ward representatives. This might be due to the fact that ward nurses rather than research nurses were responsible, as is common in The Netherlands. We therefore recommend that future smoking cessation effectiveness studies should record data of all patients admitted to cardiac wards. Fifth, there were few follow-up contacts by telephone. Initially, nurses were supposed to call patients once after discharge from hospital. This was ultimately refused by ward managers due to time constraints. Because high-quality aftercare is essential [*6,12,52–54*], efforts should be made to make telephone follow-up feasible. An alternative to phone calls may be computer-generated personalized letters [*55–57*]. Sixth, our assessment of factors related to smoking cessation among CHD patients may have been incomplete. For example, we did not assess the influence of severe symptoms like shortness of breath. Seventh, variations in the dosage of the intervention (i.e., incomplete intervention coverage) were not accommodated for in the statistical methods since adjusting for postrandomization variables such as process data disrupts the randomization and may introduce serious bias in the treatment effect estimation and testing [*58*]. Therefore, the results of the effect evaluation reflect average effects under moderate intervention coverage. Because this effectiveness study was conducted under actual hospital conditions, it was difficult to avoid differences in dosage. This might well occur more often in effectiveness studies, which underscores the importance of conducting simultaneous process evaluations. Eighth, it might be argued that adjustment for baseline differences by including them as covariates is sufficient only to the extent that those covariates have been measured validly and reliably. This is a reason for concern primarily for those covariates that are associated both with treatment condition (i.e., show a baseline difference) and with the outcome (i.e., are predictive of abstinence). Fortunately, the latter was only the case for the pretest intention to quit. With respect to this factor, the assumptions for adjustment for covariates were met, because it was measured in accordance with an operationalization used by Ajzen and Fishbein [*59*]. A decade of research with this measurement instrument has supported its reliability and validity. In our opinion, other covariates have been measured validly and without measurement error (continuous variables) or misclassification (categorical ones) as well. Finally, our study did not include a cost-effectiveness assessment. Reviews on the treatment of hospitalized smokers suggest, however, that brief treatments, including interpersonal counseling and self-help, are cost-effective [*7,8,60*].

Despite these limitations, the study provided important insight into the effectiveness of the intervention in real clinical circumstances. Future effectiveness studies should, however, in the planning phase and also in the implementation phase, take lessons from our study to make sure that they will not be plagued by the same problems that we did. The study suggests that the intervention is eligible for improvement in order to achieve long-term effects. The study furthermore contributed to the theoretical insights into the factors associated with smoking cessation in CHD patients, which is also relevant for clinical practice.

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