Improving Cardiac Patient Adherence to Exercise Regimens: A Clinical Trial of Health Education

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An educational intervention, consisting of telephone counseling for patient and spouse and a mailed pamphlet, was designed to improve cardiac patient attendance at six exercise programs. Attendance was tested against the pamphlet alone in a randomized, controlled trial with 174 patients and 134 spouses. Unadjusted results showed a nonsignificant increase (2%) in the first three months' attendance of the experimental group over the comparison group. However, adjustment for covariates showed that the intervention significantly increased attendance by 12% (P = .03) and had the strongest effect on subjects with a high school education or less. Evidence regarding the effect of spouse support was contradictory. Nonexperimental variables that significantly affected attendance, when controlling for all other variables, included exercise site, smoking, cardiac diagnosis, and patients' anticipation that their job would make them miss exercise sessions. Three quarters of the patients continued to exercise on their own after dropping out, but only one third of these people exercised at levels sufficient to maintain cardiorespiratory benefits.

INTRODUCTION

The present study was a controlled, randomized trial designed to assess the ability of an educational intervention (telephone counseling for patient and spouse, which was reinforced by pamphlets for each) to increase cardiac patient attendance in the first three months of prescribed exercise programs.

Medically supervised exercise is commonly prescribed for patients with coronary heart disease (CHD).[1,2] The typical program consists of exercise at 70% to 85% of the maximum heart rate achieved on an exercise test for 20 to 30 minutes three times per week.[3] However, low patient adherence rates may reduce the value of such programs. Oldridge[4] reviewed 12 published studies of cardiac rehabilitation exercise programs and found dropout rates ranging from 15% to 87%. Dropout rates were generally highest in the first three months of a program.

The cardiac exercise literature has few studies that are useful for designing an adherence-enhancing intervention. Heinzelman and Bagley[5] found perceived spousal support to be the best predictor of attendance at exercise sessions for coronary prone volunteers, and Andrew and co-workers[6] found that the exercise dropout rate for heart patients with little or no spouse support was three times that of those with positive family encouragement.

Reid and Morgan[7] tested the effect of education on exercise in a controlled trial with 124 healthy firefighters. Significantly more experimental subjects than control subjects exercised regularly at three months (55% versus 29%); however, the exper-

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imental group dropped back to control levels by six months. Martin and Dubbert[8] reviewed behavioral psychology approaches for increasing exercise program compliance and found evidence that stimulus control and reinforcement procedures can at least temporarily enhance adherence during the initial weeks of program participation. Oldridge and Jones,[9] in a trial of self-management techniques for 120 cardiac patients, found a nonsignificant 12% increase in attendance among experimental patients versus control subjects; contracting was indicated as having high potential for further research.

Other factors less amenable to modification strategies have been associated with low adherence to exercise regimens: smoking at entry,[10] lack of recreational exercise before the heart attack,[10] being a blue collar worker,[10,11] performing heavy work,[5] being recruited from hospital records versus volunteering or being referred by a physician,[12] living at a distance from the exercise site,[13,14] inability to relax,[6] and being overweight.[15] Kentala[13] found that poor results on the baseline stress test were associated with poor attendance, whereas Dishman[5] found a U-shaped distribution, with the best adherence among those with the highest and lowest exercise test results. Reports from multiple-center programs of large inter-site differences in patient attendance indicate that structural aspects of programs may be important.[10,16]

Given the lack of firm guidelines for developing an intervention in the cardiac literature, two social psychology models provided a theoretic base for the intervention design. Fishbein and Ajzen's[17,18] cognitive model of behavioral intention has been used extensively and successfully to predict voluntary behaviors, including jogging.[19] (cited in Fishbein[20]) used it to develop a persuasive message that raised the proportion of alcoholic patients signing up for a hospital detoxification program. Janis and Mann's[21] decision-counseling method has been used successfully to help patients making a variety of health decisions. Hoyt and Janis[22] used it over the telephone to double the average attendance of healthy women at an exercise program. Central to Janis and Mann's theory of decision-making is the notion that commitment to follow the course of action decided upon (i.e., adherence) may be strengthened by having the subject make a public declaration that he or she will perform the behavior.

MATERIALS AND METHODS

Development of the Educational Intervention. The primary goal of the educational intervention was to improve patient attendance at six cardiac exercise programs during the first three months by supplementing the existing educational efforts for patients and family members. These efforts varied among and within programs from a simple orientation session to a whole series of inpatient and outpatient discussions and lectures. Three programs were filled by former inpatients from single hospitals; the others were recruited wholly or in part through community physicians and newspaper advertising. Personnel and programs changed during the study year; thus, the exposure of individual patients to inhospital, public media, and exercise program education was impossible to determine.

To control for the effect of these variables, random assignment was stratified and balanced within programs. The comparison group (so designated because it was not a true control group) was given a pamphlet so that all study members would have the same information and so as to provide an inducement for participating in the program. It was expected to have little or no effect in that research indicates that single-method interventions do not sufficiently produce lasting behavioral change.[23]

Interviews with a prestudy cohort of 46 exercise-program patients (both successes and dropouts), their families, and program staff elicited the most commonly perceived benefits and drawbacks of regular attendance at the programs and the methods used by patients and spouses to overcome drawbacks. Examples of benefits included (1) I like the people in the group, (2) I can do more work around home or on the job, and (3) it improved my self-confidence. Examples of drawbacks included (1) it interrupts mealtime and (2) it is time-consuming. Discussion of the patient- and spouse-reported benefits, drawbacks, and methods for handling drawbacks to program attendance became the core of the educational intervention; the cognitive approach to changing voluntary behaviors as described by Fishbein and Ajzen[17,18] was used. Interestingly, almost all of the benefits and drawbacks were not health related. Thus, discussion of these points in the intervention may have touched an important area not covered in typical appeals to patients. The information was presented in both written (pamphlet) and telephone counseling formats. In the counseling, several techniques were adapted from Janis and Mann's decision-counseling method.[21]

In brief, the study intervention worked as follows. Subjects were asked to fill out a baseline questionnaire. After the questionnaires were returned to the investigator, experimental group patients received an oral persuasive communication on the telephone in a scripted counseling format that was designed to (1) convince them of the benefits of regular exercise

in the program, (2) warn them of likely drawbacks so that their expectations would be more realistic, (3) acquaint them with methods used by other patients to cope with the drawbacks so that they might be able to use the methods themselves, and (4) elicit an oral commitment to the interviewer that they would attend at least two classes per week for the first six weeks. In addition, each received a mailed, written persuasive communication that reinforced these points. The mailed communication was written to be readable at the fourth-grade level.[24]

During the telephone counseling, the counselor's discussion of the benefits and drawbacks of exercising in the program was tailored to each patient's perceptions of benefits and drawbacks as recorded on the baseline questionnaires. For example, patients who believed that regular exercise would not increase their energy were told that most patients reported that it did. Patients with unrealistically high expectations of weight loss were warned not to be disappointed if results were slow. Comparison patients received the mailed pamphlet after the baseline questionnaire was returned.

Telephone counseling for patients' spouses was similar to that of the patients and included discussion of benefits and drawbacks of participation, methods used by other patients' spouses to help the patient attend regularly, and an appeal to the spouse to encourage the patient to attend the exercise program regularly. A written persuasive communication was sent in the mail to reinforce these points. Spouses of comparison patients received only the written persuasive communication.

In summary, an experimental group patient was exposed to an oral persuasive communication coupled with educational counseling regarding his or her expectations of program participation, an oral commitment (with a written reminder) to exercise, increased spousal support, and a follow-up pamphlet. Comparison group members and their spouses received the pamphlet only.

Recruitment and Data Collection. Six Baltimore and Annapolis area medically supervised exercise programs for patients with CHD served as study sites. Patients met three times a week under a physician's supervision. Exercise conformed to the American Heart Association standards for training of individuals with heart disease[25]; each session included five to ten minutes of warm-up, 20–40 minutes of dynamic exercise designed to keep each patient's heart rate between 70% and 85% of maximum as determined by an exercise test, and five to ten minutes of cool-down.

Eligible patients included all patients with a history of myocardial infarction (MI), angina, or coronary artery bypass graft surgery (CABGS) who joined a site exercise program between January 5 and September 14, 1981 and who had not previously participated in a cardiac exercise program. Spouses of patients were recruited only if both the patient and the spouse consented. Of 192 patients eligible for participation, 166 (86.5%) enrolled. An additional five patients at high risk for CHD were inadvertently included in the study and randomized, as were three patients with CHD who had participated in previous programs. Eighty-one percent of subjects had a history of MI, 63.2% of angina, and 17.4% of CABGS. Time since last hospitalization for CHD was less than six months for 63.4% of the patients and six to 12 months for 16.8%. Thus, over a nine-month period, 174 cardiac patients and 134 family members (130 spouses) were recruited. Patients with their spouses were randomly assigned to the experimental or comparison groups, stratified by site and current cigarette smoking status, and balanced within each stratum. Eighty-four patients (with 66 spouses) were assigned to the experimental treatment group; 90 patients (with 68 spouses) were assigned to the comparison treatment group.

Baseline medical information was collected from study site records, and a written pretest was administered to all patients (98% response rate) and spouses (99% response rate) within two weeks after entry into the program. Attendance data were collected for 12 months; this provided three months information on those last enrolled and up to 12 months on those first enrolled. Attendance was recorded by staff, who were blinded to group status of the patients. A mailed questionnaire (92% response rate) was used to collect data on home exercise.

Statistical Methods. The primary outcome was percentage of sessions attended over the patient's first three months in the program, which was defined as the first 36 sessions offered for each patient regardless of calendar time. A difference of 5/36 sessions (14%) was set as the effect size for sample size calculations. With the sample size of 174 and a two-sided t test with alpha = .05, there was a 91% chance of detecting a 14% difference in attendance over the first three months.[26] A patient was categorized as a "dropout" if that person missed six consecutive weeks of class sessions (18 sessions) regardless of subsequent attendance. With alpha = .05, there was an 80% chance of detecting a 33% difference in dropout rates between the two groups using a two-tailed test based on the proportional hazard function.[27]

Statistical significance was set at .05 for all

Finding	Study Group	
	Experimental (%)	Comparison (%)
Men	85.7	91.1
White	91.7	97.8
Married	84.5	88.9
At least one year college	57.1	61.1
Employed	71.4	70.0
White collar (current/former)	75.9	73.3
Heavy work	43.3	41.3
Smokes cigarettes	15.5	12.2
Doctor said reduce alcohol	29.6	21.2
Taking beta blocker	55.1	50.0
History of myocardial infarction	81.0	81.1
History of angina	64.3	62.2
History of bypass surgery	21.7	13.5
	Mean ± SD	Mean ± SD
Age (years)	53.3 ± 8.7	54.2 ± 7.1
Weight (lbs)	176.2 ± 25.8	179.1 ± 26.1
Work capacity (METs)	7.3 ± 2.2	7.4 ± 2.1

TABLE I
SELECTED BASELINE FINDINGS BY STUDY GROUP

SD = standard deviation.

TABLE II
INTERVENTION ADMINISTRATION BY STUDY GROUP

Intervention Element	% Receiving Element	
	Experimental	Comparison
Patients	N = 84	N = 90
Telephone counseling	88.1%	—
Oral commitment to exercise	83.4%	—
Read pamphlet	72.6%	75.0%
Participating spouses	N = 66	N = 68
Telephone counseling	95.5%	—
Read pamphlet	53.0%	61.8%

N = number of subjects.

hypothesis tests. Variables for the multiple regression model were selected from baseline variables using backward elimination. Raw percentages were used as the outcome for hypothesis testing, because preliminary analysis indicated that their distribution was suitable for standard parametric approaches; the variability in percentage of sessions attended was primarily nonbinomial. Multiple regression and initial life-table analyses were performed by computer using *the Statistical Package for the Social Sciences* (SPSS).[28]

RESULTS

Baseline Comparability of Treatment Groups. The randomization produced two well-balanced groups. Tests for differences between experimental and control groups on 31 variables measured at baseline found none of the differences to be significant (smallest $P = .14$). Table I shows selected baseline findings by study group.

Intervention Administration. Table II shows exposure of patients to elements of the intervention package. Overall, it appears that enough patients received the various elements of the package to provide a fair test of its effectiveness when applied to a population of patients in the natural setting. When patients who failed to receive the intervention as scheduled were excluded from the analysis, the results did not differ meaningfully from those reported in Table III on the full set of subjects. Telephone counseling for the patients averaged 20 minutes, of which 12 minutes were necessary to complete the scripted communication tailored to the patients' responses on the baseline questionnaire. This left an average of eight minutes for more extended counseling on topics of particular interest to the patient (range 0–37 min). Only three patients refused to listen to the entire 12-minute scripted communication. Results for spouses were similar, with an average of 16 minutes in counseling, of which ten minutes was necessary for the scripted communication.

Variables Associated with Attendance. Table IV shows baseline variables that were significantly associated with the first three months attendance. All associations were in the expected direction.

In contrast with reports in the literature, occupational status was only weakly associated with attendance. However, patients who predicted job interference with class attendance and patients with high-intensity jobs (involving heavy lifting or walking at least half the time) were significantly less likely to have a high attendance rate.

Patient completion of at least one year of college (versus high school or less) was positively, although weakly, associated with attendance. However, among college-educated patients, experimental group subjects had a lower average attendance than did comparison group members (62.6% versus

TABLE III
MULTIPLE REGRESSION TEST OF HYPOTHESIS THAT EXPOSURE TO THE EDUCATIONAL INTERVENTION WILL INCREASE PATIENT ATTENDANCE IN THE FIRST THREE MONTHS

Dependent Variable: Percent Sessions Attended in First 3 Months

Independent Variable*	B*	SE (B)	Beta*	t	2-Tailed P Value
Experimental intervention (EI)	11.703	5.338	0.212	2.192	.030
Education (Ed)	5.485	5.172	0.098	1.061	.291
EI X Ed interaction	−9.460	7.032	−0.153	−1.345	.181
Site 4	12.487	6.022	0.138	2.073	.040
Site 5	21.630	5.268	0.275	4.106	.000
Site 6	24.940	4.219	0.422	5.911	.000
Myocardial infarction (MI)	19.491	7.929	0.277	2.458	.015
Angina pectoris (AP)	20.002	8.813	0.350	2.270	.025
MI X AP interaction	−26.291	9.762	−0.477	−2.269	.008
Smokes cigarettes	−12.448	5.188	−0.156	−2.399	.018
Told to reduce alcohol	−7.423	4.178	−0.114	−1.777	.078
Job make miss sessions	−6.580	1.701	−0.251	−3.868	.000
General psychologic well-being	1.693	0.878	0.128	1.928	.056
Constant	29.670	9.932		2.987	.003

Analysis of Variance

	DF	Sum of Squares	Mean Square
Regression	13	54166.965	4166.613
Residual	160	78169.657	488.560

F = 8.528
Multiple R = 0.640
SE of the estimate = 22.103
Significance of F <.001
R^2 = .409
DF = degrees of freedom

*The first 11 variables were all scored 0 or 1, where 1 equaled presence, high, or positive answer. Sites 1, 2, and 3 were pooled as the reference category for the other sites. "Job make miss sessions" was scored as (0) not working; (0.5) less than once a month; (1) once a month; (2.5) 2 to 3 times a month; (4) once a week or more. "General psychologic well-being" was scored from 0-10 by 0.25.
B = nonstandardized regression coefficient; SE = standard error of the estimate; Beta = standardized regression coefficient.

67.3%); among high-school educated patients, experimental group patients attended more often (65.3% versus 54.1%). A test for interaction yielded a probability of .06.

Baseline variables not associated with attendance included age, current employment status, marital status, sex, occupational status, METs on exercise test (both linear and quadratic expressions), use of beta blockers, history of bypass surgery, average weekly exercise in the year before the last acute episode of CHD, spousal participation in the study, patient attitude toward exercising regularly at the site programs, perceived spouse and physician support for exercise, perceived support for exercise from important others, time needed and distance traveled to attend the exercise sessions, and quality of decision-making (regarding joining a program) prior to joining a program. Direct measurement of financial cost of program attendance was impractical at baseline; patient reports at the end of the study showed that cost had little impact on most patients' attendance.

Attendance During First Three Months. A direct comparison of the two groups by the t test did not find any significant difference in percentage of sessions attended during the first three months (t = .38, P = .70, 172 DF). The 84 experimental patients attended an average of 63.8% of the sessions

TABLE IV
PERCENT OF PATIENTS ATTENDING AT LEAST TWO THIRDS OF THE SESSIONS IN THE FIRST THREE MONTHS BY SELECTED BASELINE VARIABLES

Variable	Total In Response Category (N)	Attended ≥2/3 Sessions (% of N)
Site		
1	24	45.8
2	30	26.7
3	21	38.1
4	18	50.0
5	25	56.0
6	56	83.9
Intention to exercise*		
Low	27	51.9
Medium	37	37.8
High	104	65.4
Job intensity		
Light work	66	68.2
Heavy work	90	47.8
How often job will make patient miss class*		
≤1/month	138	62.3
≥2/month	25	28.0
Smokes cigarettes		
Yes	24	25.0
No	150	60.7
Weight (lbs)*		
≤150	21	61.9
151-175	57	59.6
176-200	61	58.7
≥201	32	40.6
Told by doctor to cut down alcohol consumption		
Yes	42	35.7
No	124	64.5*

Variable	Total In Response Category (N)	Attended ≥2/3 Sessions (% of N)
General psychologic well-being*		
Poor	45	42.2
Moderate	86	58.1
Good	38	71.1
Ability to relax easily*		
Never, rarely, or sometimes	89	48.3
Usually or always	82	64.6
Medical history		
MI only	54	74.1
MI plus angina	87	51.7
Angina only	23	43.5
Neither MI nor angina	10	20.0
Race		
White	165	57.0
Other	9	33.3
Spouse support as stated by the spouse*		
Negative or neutral	20	40.0
Positive	113	60.2
Spouse estimate that patient will exercise*		
Low	16	25.0
Medium	24	45.8
High	93	64.5

Entered as continuous variables in analyses. N = number of subjects; MI = myocardial infarction.

(SD = 27.24); the 90 comparison patients attended an average of 62.2% (SD = 28.17). However, adjustment for baseline covariates in a linear multiple regression model altered the findings (Table III). After controlling for other baseline variables, exposure to the experimental intervention accounted for an 11.7% increase in attendance ($t = 2.19$, $P = .03$, 172 DF; 95% confidence interval = 1.2%–21.6%), which corresponds to an increase of 4.3 sessions attended out of the first 36. About 41% of the total variance was accounted for by the variables in the regression.

The unadjusted attendance scores had indicated that the experimental intervention was more effective for patients with high school education or less than for patients with at least one year of college. The same trend appeared, although less strongly, after adjustment in the regression analysis. Overall, college educated patients averaged 5.5% higher attendance than high school educated patients. Among high school patients, the experimental group subjects attended an average of 11.7% more sessions than did comparison group members; among college patients, those in the experimental group attended 2.2% more sessions than did comparison group members. Although probability values for education and the treatment-education interaction were not significant, an interaction between patient educa-

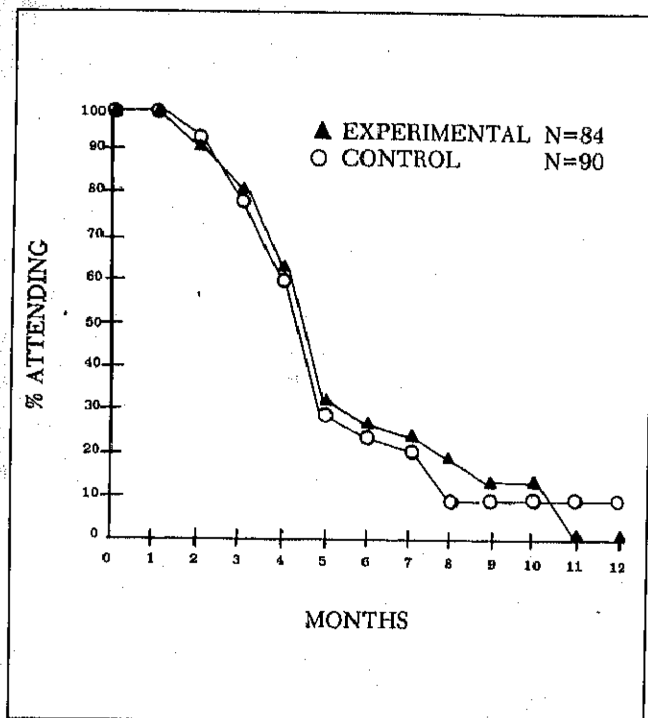

Figure. Percentage of patients still attending by month by study group. N = number of patients.

tional level and an educational intervention would not be surprising.

Dropout Rates. Data were available on patient attendance for varying lengths of time, ranging from 2 to 48 weeks with a median follow-up of 25.7 weeks. Patients who missed 18 consecutive sessions (i.e., six weeks) were defined as dropouts. Preliminary life-table analyses[28] indicated that the study site was the only baseline variable significantly associated with time until dropout (Lee–Desu's $D = 22.9$, $P \leq .001$, 5 DF). The median attendance time was 17.5 weeks (range over the sites was 13.5–31.4 weeks), thus indicating that half the patients had stopped coming to the exercise programs by the 12th week.

Log-rank analysis was used to compare the dropout curves of experimental and control groups, adjusting for site.[27] There was no significant difference in the length of time patients exercised at the sites before patients dropped out ($\chi^2 = .493$, $P = .5$, 1 DF). Dropout curves for the two groups are shown in the Figure.

Spousal Support. Seventy-seven percent of the patients had spouses who participated, with almost no difference between comparison group (75.6%) and experimental group (78.6%). Patients with spouses, regardless of spousal participation in the study, attended significantly better than did patients without spouses. For the 134 patients with participating spouses, the mean attendance was 63.6%; for the 31 patients with nonparticipating spouses, it was 66.4%; and for the nine patients without spouses, it was 41.8%. Spousal attitudinal support at baseline was measured by a seven-point scale on which spouses indicated whether they thought the patient should or should not attend the exercise sessions, which matched a question in which patients were asked if their spouse thought they should or should not attend the sessions. The correlation between patient attendance and spousal-report support at baseline was 0.203 ($P = .01$, 130 DF). Patient perception of spousal support was almost completely uncorrelated with attendance ($r = -.078$). Perceived spousal support and spousal-reported support were correlated only modestly ($r = .335$), although significantly ($P < .001$, 130 DF). This suggests that spousal-reported attitudinal support was more important to patient attendance than was patient-perceived spousal support.

The counseling and pamphlet intervention for spouses of experimental group patients was expected to increase their behavioral support of patient exercise over spouses of comparison group patients. Spousal behavioral support was measured by two actions as reported four to five weeks post-baseline: (1) spousal accompaniment of the patient to a nonorientation exercise session at least once, as reported by the patient, and (2) oral encouragement of the patient at least once, as reported by the spouse. Thirty-six percent of experimental group spouses accompanied the patients to exercise sessions versus 34% of comparison group spouses. Sixty percent of experimental group spouses gave oral encouragement versus 59% of comparison group spouses. Thus, a hypothesis of a difference in behavioral support must be rejected.

In Table V, attendance is classified by three variables (all measured four to five weeks post-baseline) that might be expected to measure the influence of others on patient attendance: spousal encouragement of the patient (specifically by saying something), spousal accompaniment of the patient to at least one exercise session, and a patient report that other patients or staff at the site had influenced him or her to attend regularly. In each case, patients who were exposed to these influences showed poorer attendance than did patients not so exposed, although differences were significant for oral spousal encouragement only.

What is the reason for these counter-intuitive

TABLE V
PERCENT OF PATIENTS ATTENDING AT LEAST TWO THIRDS OF THE SESSIONS IN THE FIRST THREE MONTHS BY INTERPERSONAL INFLUENCE AS MEASURED IN THE FOURTH AND FIFTH WEEKS POST-BASELINE

	Attended <2/3		Attended ≥2/3			
	N	Row %	N	Row %	χ^2	P Value
Spousal encouragement						
Yes	40	54.1	34	45.9	9.42	.002
No	13	25.0	39	75.0		
Spouse came to sessions						
Yes	24	48.0	26	52.0	0.62	.422
No	45	39.8	68	60.2		
Group or staff influence						
Yes	26	49.1	27	50.9	1.08	.300
No	43	39.1	67	60.9		

N = number of patients.

results? Perhaps there was some kind of reaction among the patients against being told what to do. However, of the 52 spouses who did not report saying something specific to encourage the patient to attend, 51 said they did not do so because it was not necessary. This implies that spousal encouragement might have been a reaction to early poor attendance rather than a cause of it. Data were not available for a similar analysis of the other two forms of influence.

Exercise Performed at Home. Patients were asked what exercise they had done on their own during the 11th month of the study. Most had dropped out of the supervised programs by this time. Of 160 patients who responded, 125 (78.1%) reported doing at least some exercise on their own, usually brisk walking, calisthenics, or indoor cycling. Analysis of 113 patients who started in months 1–5 showed that by month 11 only 35 (31.0%) were exercising at least 15 minutes three or more times a week with the heart rate usually maintained in the training zone. This indicates that for many patients, at-home exercise may have been insufficiently vigorous. The proportions of patients exercising regularly and vigorously at home ranged among sites from 9.1% to 45.7% (χ^2 = 10.84, 5 DF, P = .055), with site 6 at the high end. Site 6 also had the highest three-month in-program attendance rate (Table IV). Site 6 was unique in its policy of graduating patients at three months. This suggests that a planned graduation may encourage higher initial attendance (perhaps by providing a time-limited goal), while not lessening the amount of exercise achieved at home, compared with patients from sites without a planned graduation policy.

DISCUSSION

Overall, the educational intervention received a fair test in a clinical setting, with most of the experimental group patients receiving several elements of the intervention (telephone counseling, oral commitment, pamphlet, and spouse encouragement). A multiple regression approach demonstrated a significant increase of attendance of 11.7% in the first three months (P = .03) among experimental group members. The effect of the intervention appeared to be strongest for patients with a high school education or less. The data do not provide answers for this apparent interaction. Analyses did not indicate any biases in the administration of the intervention by level of patient education. It seems plausible that the intervention, which was targeted to the lower educational level, had the most effect at this level. Within the comparison group, both college and high-school educated patients who read the pamphlet attended about 9% more sessions than their education-level peers who had not read the pamphlet (through choice or nondelivery of mail). This indicates that the pamphlet alone might have had a positive effect on attendance, and that there was probably no negative reaction among college-educated patients to the simplicity of comprehension of the pamphlet.

An issue that could not be addressed in the present study was the relative worth of a 12% increase in attendance in patients who exercised at different levels. For instance, a person coming to half of the sessions might cross a conditioning threshold and thus be greatly benefited by exercising an additional four sessions out of 36, whereas a person coming to only a few or almost all of the sessions would probably receive little benefit from such an increase.

As predicted, spousal attitudinal support at baseline was positively correlated with patient attendance (r = .203), but patient perception of spousal support was completely unrelated to patient attendance. Paradoxically, patients whose spouses showed behavioral support were significantly less likely to have a good attendance rate. It is difficult to say whether their encouragement was a supportive response to initial poor attendance or whether the poor attendance was a patient reaction to perceived pressure. The issue is an important one for health

educators. It may be that increasing the favorableness of spousal attitude would constitute a more effective intervention than would stimulating overt, and possibly reactive, supportive acts.

Analysis of self-reported, unsupervised patient exercise indicated that a majority of patients continued to exercise on their own after dropping out but that most probably failed to exercise at levels sufficient to maintain cardiorespiratory benefits. The only program with a policy of planned graduation (site 6) had the highest average for both supervised and unsupervised exercise, thus indicating that a planned graduation of the majority of patients from an exercise program, with follow-up exercise prescriptions, might help keep more patients exercising safely, effectively, and longer on their own than programs without a set end point in which the majority of patients simply drop out with little preparation for exercising on their own.

The large differences in attendance among sites (see Tables III and IV) indicate that there is something about the relative quality of the various programs that greatly affects overall attendance. Whether it is a characteristic of the exercise facilities themselves, the medical and exercise staff, the location (hospital or college), the educational program offered, the graduation policy, or an interaction among all of these is not clear. A patient's prediction that his or her job would make him or her miss exercise sessions frequently was a good predictor of poor attendance. This suggests that staff should consider offering programs at more convenient times for those who work or instruct patients on how to supplement missed sessions with exercise on their own—if this is safe.

Limitations. The site programs and patient population in the present study seem typical of most of those reported in the literature on exercise programs for cardiac patients, and participation in the study was high (86.5%). However, generalizations about the educational intervention should be limited until results can be replicated. The random assignment of patients to experimental and control groups makes it likely that the effect of the intervention on patient attendance (shown in the adjusted analysis) was real. However, the apparent interaction of the intervention with educational level of the subjects signals caution in interpretation. There is also the possibility that experimental and control patients within sites interacted with each other; for example, experimental group patients could have encouraged comparison group patients to attend regularly or could have shared car rides with them. However, several questions directed at the issue failed to produce any evidence of this sort of contamination (which at worst would produce only a conservative bias in the comparisons of attendance).

Another potential limitation is that only one person counseled the study subjects. Other persons might have interacted differently with the subjects and had different results. Also, the educational intervention was added onto the education already being given at the sites; there is no way of telling what kind of additive or interactive effect it had with other education.

CONCLUSION

The findings of the present study suggest that attendance in service programs such as the six studied here can be significantly and reliably improved. First, the great variety in average attendance among sites (consistent over several years) indicates that there must almost certainly be structural variables that can be manipulated to improve attendance; other studies with multiple sites have shown similar differences.[10,16] Second, a pamphlet and telephone call to patients and spouses, even when added to other education programs being offered at many of the sites, was able to raise attendance significantly among patients with a high-school education or less. A combination of improved health education and structural changes should allow many service programs to improve their average patient attendance. Further research in improving exercise programs for cardiac patients should include structural variables and educational interventions. Policies for termination of supervised exercise should be examined for their impact on frequency and quality of subsequent unsupervised exercise. Strategies for effectively mobilizing spouse support also deserve further examination.

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