Effect of Local Medical Opinion Leaders on Quality of Care for Acute Myocardial Infarction

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A Randomized Controlled Trial

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Context.—The effectiveness of recruiting local medical opinion leaders to improve quality of care is poorly understood.

Objective.—To evaluate a guideline-implementation intervention of clinician education by local opinion leaders and performance feedback to (1) increase use of lifesaving drugs (aspirin and thrombolytics in eligible elderly patients, β-blockers in all eligible patients) for acute myocardial infarction (AMI), and (2) decrease use of a potentially harmful therapy (prophylactic lidocaine).

Design.—Randomized controlled trial with hospital as the unit of randomization, intervention, and analysis.

Setting.—Thirty-seven community hospitals in Minnesota.

Patients.—All patients with AMI admitted to study hospitals over 10 months before (1992-1993, N=2409) or after (1995-1996, N=2938) the intervention.

Intervention.—Using a validated survey, we identified opinion leaders at 20 experimental hospitals who influenced peers through small and large group discussions, informal consultations, and revisions of protocols and clinical pathways. They focused on (1) evidence (drug efficacy), (2) comparative performance, and (3) barriers to change. Control hospitals received mailed performance feedback.

Main Outcome Measures.—Hospital-specific changes before and after the intervention in the proportion of eligible patients receiving each study drug.

Results.—Among experimental hospitals, the median change in the proportion of eligible elderly patients receiving aspirin was +0.13 (17% increase from 0.77 at baseline), compared with a change of −0.03 at control hospitals (P=.04). For β-blockers, the respective changes were +0.31 (63% increase from 0.49 at baseline) vs +0.18 (30% increase from baseline) for controls (P=.02). Lidocaine use declined by about 50% in both groups. The intervention did not increase thrombolysis in the elderly (from 0.73 at baseline), but nearly two thirds of eligible nonrecipients were older than 85 years, had severe comorbidities, or presented after at least 6 hours.

Conclusions.—Working with opinion leaders and providing performance feedback can accelerate adoption of some beneficial AMI therapies (eg, aspirin, β-blockers). Secular changes in knowledge and hospital protocols may extinguish outdated practices (eg, prophylactic lidocaine). However, it is more difficult to increase use of effective but riskier treatments (eg, thrombolysis) for frail elderly patients.

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THE INFLUENCE OF local medical opinion leaders in the diffusion and adoption of new medical treatments has been recognized for almost half a century.[1,2] Opinion leaders are not necessarily innovators or authority figures, but are trusted by their colleagues to evaluate new information and assess the value of new medical practices in the context of local group norms[3]; are approached frequently for clinical advice; have good listening skills[4]; and are perceived as clinically competent and caring.[5] Many researchers and policymakers advocate recruiting opinion leaders in ongoing quality improvement efforts, in part because of the potential efficiency of capitalizing on local volunteers skilled in changing practice patterns.[6] Yet, evidence supporting such interventions is limited,[4,7,8] including only 1 randomized controlled trial (RCT) that enlisted local opinion leaders to reduce unnecessary cesarean deliveries in Canada.[4] No well-controlled study has examined the effectiveness of recruiting opinion leaders to influence the adoption of underused, lifesaving interventions for major acute illnesses, such as acute myocardial infarction (AMI).

See also pp 1351 and 1392.

The selection of treatments for AMI patients represents one of the most critical decisions in medical practice.[9] Coronary heart disease is the leading cause of death in the United States.[10] Large RCTs and national guidelines strongly support the early administration of aspirin, β-blockers, and thrombolytic agents for AMI because they substantially reduce mortality and morbidity in eligible patients.[9,11-18] However, a meta-analysis of 14 RCTs of lidocaine prophylaxis to reduce ventricular fibrillation during AMI indicates that this practice may lead to increased mortality, especially in uncomplicated MI.[19] The national guidelines also

recommend avoidance of lidocaine prophylaxis.[17,18] Yet, recent studies of actual practice have found substantial nonadherence to these recommendations, resulting in potentially avoidable morbidity and mortality.[20,21] Our previous report of baseline data at the 37 hospitals participating in this study indicated that only 53% of eligible patients received β-blockers.[21] Although aspirin use and thrombolysis were high (87% and 81%, respectively) among eligible nonelderly patients, only 76% and 69% of eligible elderly patients (aged 65 years or older) received aspirin and thrombolytic agents, respectively. About 20% of patients received prophylactic lidocaine.

In this article we report the results of a large statewide RCT that combined identification and involvement of local opinion leaders with performance feedback to improve quality of care for AMI. Specifically, we sought to increase adherence to the national (American College of Cardiology/American Heart Association [ACC/AHA]) guidelines recommending (1) increased use of highly effective drugs for eligible AMI patients, ie, β-blockers in all patients and aspirin and thrombolysis in the elderly, and (2) reduced use of an ineffective treatment, ie, prophylactic lidocaine.

METHODS

Setting

The Minnesota Clinical Comparison and Assessment Program (MCCAP), an ongoing quality improvement collaborative of the Healthcare Education and Research Foundation, collected medical record data and coordinated the experimental intervention throughout Minnesota. At the time of the study, 45 participating MCCAP hospitals accounted for 60% of all hospital admissions and over 80% of all community hospital admissions statewide. Of these previously described hospitals,[21] 37 agreed to participate in this study. Of these, 20 (54%) were in urban areas, and 35 (95%) were community hospitals.

Overall Design

The study design was an RCT with the hospital as the unit of intervention and analysis. Outside St Paul and Minneapolis, hospitals were stratified by size and randomized from within each of 9 strata to experimental or control (usual care) conditions. To minimize contamination of control hospitals, large cities (ie, St Paul [7 hospitals, 1430 patients] and Minneapolis [11 hospitals, 2536 patients]) were randomized as clusters, resulting in a statewide sample of 20 experimental and 17 control hospitals (Figure 1). While this randomization plan may have reduced baseline comparability somewhat, it avoided extensive contamination of controls that would have been caused by physicians working in multiple hospitals within each city.

We collected baseline data on use of study drugs for AMI patients admitted from October 1, 1992, to July 31, 1993, in all 37 study hospitals. Randomization occurred on August 1, 1994. The experimental intervention occurred from December 1, 1994, to June 30, 1995. We collected postintervention data in 20 experimental and 16 control hospitals for AMI patients admitted from July 1, 1995, to April 30, 1996. All hospitals completed the study except one that closed before the intervention (Figure 1). All hospital medical directors were informed that they would receive feedback on AMI guideline adherence rates (see below). Hospital administrators, physicians, AMI patients, and nurse abstractors were blinded with respect to study hypotheses and experimental assignment of each hospital.

This study was exempt from human subject committee review and informed consent requirements by the National Institutes of Health and Harvard Medical School because it used chart reviews to evaluate educational activities aimed at increasing adherence to accepted standards of care.

Study Patients

Because we sought to improve clinical decision making during the acute phase of illness, we identified patients with suspected AMI at the time of hospital presentation. This avoided the problem of evaluating care for patients presenting atypically and not diagnosed as having AMI until later in the hospitalization. As described elsewhere,[21-23] patients were included in the study if they had an admission diagnosis of "AMI," "rule out AMI," or "suspected AMI" and met 2 of the following criteria: (1) clinical symptoms typical of AMI (chest discomfort, arm or shoulder pain, diaphoresis, dyspnea, nausea or vomiting, and neck or jaw pain); (2) explicit medical record documentation by a physician that electrocardiographic (ECG) findings were considered compatible with AMI (ie, new Q-wave or ST-segment depression or elevation ≥1 mm); and (3) elevated serum creatine kinase and MB isoenzyme levels above the upper limit of normal (as specified by the laboratory at each participating hospital). Agreement between medical record ECG findings and those of 2 independent cardiologist-reviewers of ECG data was high.[21] Patients were excluded from study if they died before admission, were transferred from a nonstudy hospital, or had suffered an AMI in the 2 weeks before the index admission.

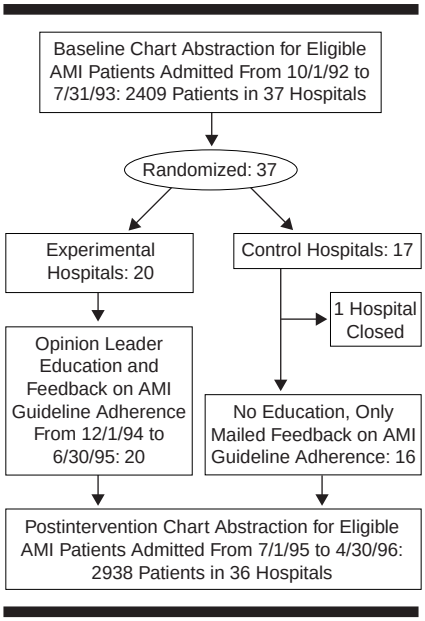

Figure 1.—Flowchart of trial. AMI indicates acute myocardial infarction.

Eligibility for Study Drugs

We determined eligibility for the 4 study drugs based on the 1990 ACC/AHA guidelines in effect during the observation period.[17,21] We defined eligibility for treatment with aspirin, β-blockers, and thrombolytics as having all indications for each treatment and no absolute or relative contraindications (Table 1). We defined eligibility for lidocaine liberally (Table 1) to more clearly identify prophylactic use (without the listed indications). Data on key clinical variables necessary to define eligibility (eg, ST-segment elevation, time from symptom onset to presentation, sustained ventricular tachycardia) were recorded for more than 98% of patients.

Data Collection and Integrity

As previously described,[21] trained nurses with cardiology experience retrieved detailed medical record data on AMI inclusion and exclusion criteria; admission data, including first medical contact and time to presentation; inpatient procedures; ECG and laboratory evidence of AMI; medical history and comorbidities at admission[24]; clinical findings at presentation and during the first 24 hours of hospitalization; identity and time of administration of all drugs in the first 48 hours, including during emergency transport and in the emergency department; and study drug indications and contraindications. Twenty-four nurses collected preintervention data; and 23 abstracted postintervention charts of hospitals close to their residence. Only 6 abstractors collected data in both periods. Abstractors were required to demon-

Table 1.—Eligibility and Contraindications for Study Drugs*

Oral Aspirin
Eligible population: All patients with AMI and no contraindications
Absolute contraindications: History of allergy to aspirin; serious gastrointestinal bleeding
Relative contraindications: Asthma; nasal polyps (aspirin could lead to anaphylaxis); history of bleeding and/or significant risk of bleeding; history of peptic ulcer disease
β-Blockers
Eligible population: All patients with AMI and no contraindications
Absolute contraindications: Heart rate too low (<60/min); low systolic blood pressure (<100 mm Hg); severe left ventricular failure (rales >10 cm from base of lungs [10 cm = one third from base]); severe bronchospastic lung disease; signs of peripheral hypoperfusion; atrial ventricular conduction abnormalities; history of adverse reaction to β-blockers
Relative contraindications: Systolic blood pressure <110 mm Hg; history of asthma; severe peripheral vascular disease; difficult-to-control, severe, insulin-dependent diabetes
Thrombolytics
Eligible population: All patients with AMI or suspected AMI presenting within 12 h of onset of symptoms; ST-segment elevation ≥1 mm; no contraindications
Absolute contraindications: Active internal bleeding; suspected aortic dissection; prolonged or traumatic cardiopulmonary resuscitation; recent head trauma (≤2 wk); intracranial neoplasm; hemorrhagic ophthalmic conditions; pregnancy; previous allergic reaction to the thrombolytic agent; sustained systolic blood pressure >180 mm Hg; or diastolic blood pressure >110 mm Hg; any recorded blood pressure >200/120 mm Hg on admission; trauma or surgery ≤2 wk; AMI onset >24 h
Relative contraindications: Major bleeding; recent trauma or surgery >2 wk and <2 mo; history of chronic severe hypertension with or without drug therapy; history of CVA; current use of warfarin anticoagulants; prior use of streptokinase or APSAC (if they are the agents of choice); significant liver dysfunction; active peptic ulcer; AMI onset >12 h
Lidocaine
Eligible population: Patients with AMI (ACC/AHA class I) or suspected AMI (ACC/AHA class IIa) with frequent (>6/min) ventricular premature beats; nonsustained or sustained (>30 s) ventricular tachycardia at a rate >100/min; ventricular fibrillation
Absolute contraindications: Allergy to lidocaine

*AMI indicates acute myocardial infarction; CVA, cerebrovascular accident; APSAC, anisoylated plasminogen streptokinase activator complex (anistreplase); and ACC/AHA, American College of Cardiology/American Heart Association.

strate ongoing interrater agreement with the criterion reviewer of 95% or higher. The MCCAP reviewers audited random samples of 10% of each abstractor's completed cases to ensure that this standard was met.

Experimental Conditions

Approximately 2 years before the collection of baseline data, MCCAP had disseminated local AMI guidelines (virtually identical to the ACC/AHA guidelines) to the administrator, medical director, and directors of quality management and nursing at all Minnesota hospitals, as well as to cardiologists' and generalist physicians' offices.[25] Based on previous evidence,[26,27] we assumed that such dissemination of printed materials alone might predispose clinicians to change but would not cause practice changes by itself. Two of us (cardiologists, F.G. and R.A.) who were chairs of the Minnesota guideline panel agreed to lead the opinion leader meetings described below and to invite local opinion leaders to participate in the educational interventions.

Control Hospitals.—Seventeen hospitals were randomized to control or usual care conditions. During the intervention at the experimental hospitals, we mailed data feedback books to the medical director of control hospitals. The books included data on baseline rates of use of study drugs in eligible patients (by age category and sex) for each hospital, allowing comparison of rates of guideline adherence with the other 36 participating hospitals using anonymous identifiers. This feedback book also included comparative utilization rates of nonstudy drugs and procedures. The mailing of such a large volume of data approximated usual care feedback conditions and was unlikely to influence use of study drugs at control hospitals.[28]

Identification of Opinion Leaders at Experimental Hospitals.—We used a previously validated 1-page questionnaire to identify local opinion leaders at each hospital from whom other physicians regularly obtained advice on AMI cases and whose personal attributes were most similar to key characteristics of opinion leaders.[5] The sample consisted of all 772 physicians who were the most frequent prescribers of cardiac medications for each study patient at baseline. The mailed questionnaire was followed by up to 2 reminders. Two hundred ninety-four questionnaires were returned. At 17 of the 20 experimental hospitals, the physicians chosen as local opinion leaders received more than 70% of votes at their hospitals. First-ranked opinion leaders at 16 of the 20 experimental hospitals agreed to participate in the intervention. At the remaining hospitals, the physicians receiving the second-highest scores agreed to participate. None of the opinion leaders (see below) worked in both an experimental and a control hospital.

Experimental Intervention.—During the first intervention phase, the chairs of the MCCAP AMI guideline panel led a 1-day meeting of opinion leaders that promoted consensus and commitment to voluntary practice changes and identified common barriers to change and promising interventions to surmount them. The meeting began with a review and discussion of the evidence from large RCTs regarding use of study drugs that supported guideline practice recommendations. Subsequently, feedback was provided on individual hospitals' comparative performance (eg, proportion of eligible elderly patients receiving aspirin). The most common stated barriers were concerns about the risks of bradycardia and hypotension due to β-blockade and fears of causing thrombolytic-related bleeding in patients older than 75 years. Several clinicians reported observing patients suffering hemorrhagic strokes soon after administration of thrombolytics and reported that such events were more salient to them than the larger number of lives saved.

We did not provide training in communication and behavioral change[29] because opinion leaders were selected for their ability to influence peers. However, we provided several tools and resources to be used by opinion leaders. These included (1) slides (with "talking points") covering the main results of RCTs of study drugs, practice recommendations, and comparative hospital-specific performance at baseline; (2) administrative support (including scheduling educational interventions, providing handouts and slides, and answering questions about baseline data); and (3) illustrated 2-sided educational brochures entitled "Updates in Clinical Care: Treatment of Acute MI," which were used in all educational interactions. Each brochure covered use of 1 study drug, featured bold headlines and clear behavior-change messages, and contained clear, easy-to-interpret graphs and charts summarizing the efficacy of study drugs and opportunities for improvement in study hospitals. Drafts of all educational materials were reviewed, edited, and approved by opinion leaders before final printing to increase their acceptability.

The second phase of the intervention occurred over the next 7 months when the opinion leaders carried out local interventions. They adapted the content and methods of the statewide opinion leader meeting to the needs of nurses and physicians at their hospitals. Educational interactions occurred less frequently in formal lectures (eg, grand rounds) and more frequently in small groups and formal and informal consultations with colleagues. Small groups included department or committee meetings, such as emergency department, medicine department, cardiology section, nursing department, clinic departments, emergency medical services staff, and quality improvement teams.

Because of broad agreement that some deficits in practice could be attributable to system barriers, all opinion leaders worked to institute system changes, such as revising protocols, clinical pathways, and standing orders for evaluating of chest pain, diagnosing or ruling out MI, using the cardiac care unit, prescribing thrombolytics, and treating arrhythmias. In general, these changes encouraged rapid assessment of eligibility for and use of aspirin, β-blocker, and thrombolytic therapy and avoidance of lidocaine prophylaxis.

Statistical Analysis

The hospital was the unit of analysis in evaluations of intervention effects. We compared control and study patients with regard to baseline use of study drugs and several variables that predicted use of study drugs in previous analyses (eg, age, sex, presence of severe comorbidity, and time from initial symptoms to presentation).[21,22] The main study outcome measures were hospital-specific changes before and after the intervention in (1) the proportion of eligible patients receiving each of the effective drug categories, and (2) the proportion of patients without indications who received lidocaine. For drug-specific analyses, we measured changes in hospitals with at least 7 eligible patients before and after the intervention period; this cutoff excluded some hospitals with very small numbers of eligible patients and unstable hospital-specific estimates of changes in use of specific drugs. We used a conservative, nonparametric test, the Wilcoxon rank sum test,[30] to determine the statistical significance of differences between the experimental and control group changes in the outcome measures (eg, proportion of eligible patients receiving each effective drug). One-sided tests were used because of our a priori hypothesis that education would increase use of recommended therapies.

RESULTS

Table 2 presents demographic and clinical characteristics of experimental and control patients before and after the intervention. Both groups were comparable overall and with respect to several characteristics that predicted use of study drugs at baseline, namely, old age (≥75 years), female sex, severe comorbidity, recent symptom onset (<6 hours), and heart failure.[21,22,31] Community hospitals represented 19 of 20 experimental hospitals and 16 of 17 control hospitals. Eight experimental hospitals and 9 control hospitals were in rural areas. The median number of AMI study patients at baseline was 43 for experimental hospitals and 36 for control hospitals.

Intervention Effects

There were no significant differences in baseline rates of use of study drugs between experimental and control hospitals (Table 3). Overall, the intervention resulted in a significant increase in aspirin and β-blocker use (Table 3). Among experimental hospitals, the median change in the proportion of eligible elderly patients receiving aspirin was +0.13 (17% increase over the baseline proportion of 0.77) compared with the median change of –0.03 at control hospitals (P=.04). Fourteen of 17 experimental hospitals with at least 7 eligible patients both before and after the intervention exhibited a positive change in aspirin use compared with only 5 of 13 control hospitals.

The median change in the proportion of eligible patients receiving β-blockers at experimental hospitals was +0.31 (63% increase over the baseline proportion of 0.49) compared with a median change of +0.18 (30% increase over baseline proportion of 0.60) at control hospitals (P=.02; Table 3). Because rates of β-blocker use at control and experimental hospitals were somewhat different at baseline (difference in proportion, 0.11; P=.19), we conducted a stratified analysis to determine if the larger increase in use of β-blockers at experimental hospitals was related to their lower baseline utilization. Figure 2 indicates positive and consistent effects of the experimental intervention on β-blocker use in hospitals both above and below the median baseline level of β-blocker use, suggesting that the larger increase in use of these agents in experimental hospitals was unrelated to experimental-control differences at baseline.

There was a strong secular decline in nonindicated lidocaine use at all hospitals that overwhelmed any potential intervention effects. We observed an approximate 50% reduction in the proportion of ineligible patients receiving lidocaine to about 10% after the intervention in both experimental and control hospitals (Table 3).

Although the number of hospitals and thrombolytic-eligible elderly patients in the control group was too low to yield stable estimates of changes in thrombolysis, the intervention did not increase thrombolytic use in eligible elderly patients at experimental hospitals (Table 3). The median proportion of eligible elderly patients receiving thrombolysis at experimental hospitals was 0.73 at baseline; the median before and after change was –0.03 compared with a median change of +0.12 at 5 control hospitals (P=.44).

Characteristics of Nonrecipients of Thrombolytics

During intervention meetings, opinion leaders suggested that the approximate 70% adherence rate for thrombolysis in eligible elderly patients at baseline may already have been close to the attainable ceiling. They suggested that, although some elderly patients may be eligible for thrombolysis, their advanced age, severe comorbidities, or late arrival would in-

Table 2.—Characteristics of Experimental and Control Patients Before and After Intervention*

Characteristic	Before, % (N = 2409)	After, % (N = 2938)
Age ≥75 y		
Experimental	31	31
Control	35	34
Female		
Experimental	38	35
Control	39	40
Severe comorbidity†		
Experimental	26	24
Control	22	25
History of AMI		
Experimental	27	24
Control	30	25
Diabetes mellitus		
Experimental	20	20
Control	24	20
Anterior AMI		
Experimental	21	21
Control	21	23
Presented with CHF		
Experimental	21	22
Control	21	20
Symptom onset <6 h		
Experimental	60	66
Control	60	62
First contact		
Emergency department		
Experimental	43	44
Control	43	45
EMS transport		
Experimental	41	42
Control	42	42
Median length of stay, d		
Experimental	7	6
Control	6	5
Cardiologist consult or attending		
Experimental	78	80
Control	68	72
Medicaid or uninsured		
Experimental	4	4
Control	2	4
Urban location		
Experimental	84	86
Control	79	76
Eligible for aspirin		
Experimental	65	70
Control	66	65
Eligible for β-blockers		
Experimental	20	18
Control	25	20
Eligible for thrombolytics		
Experimental	30	33
Control	28	28
Eligible for lidocaine		
Experimental	37	33
Control	38	36

*AMI indicates acute myocardial infarction; CHF, congestive heart failure; and EMS, emergency medical services.

†Severe comorbidity or impairment at admission, based on the Greenfield Index of Coexistent Diseases.[24]

Table 3.—Distribution of Hospital-Specific Changes in Proportion of Eligible Patients Receiving Study Drugs

Drug Category	No. of Hospitals*	Median Proportion at Baseline	Absolute Before-After Change in Proportion: Median (% Change)	25th Percentile	75th Percentile	Wilcoxon *P* Value†
Aspirin, elderly patients						
Experimental	17	0.77	0.13 (17)	0.04	0.22	.04
Control	13	0.80	−0.03 (−4)	−0.09	0.13	
β-Blockers						
Experimental	11	0.49	0.31 (63)	0.20	0.38	.02
Control	7	0.60	0.18 (30)	0.06	0.27	
Lidocaine, ineligible patients						
Experimental	18	0.19	−0.09 (47)	−0.02	−0.19	.29
Control	15	0.25	−0.13 (52)	−0.04	−0.22	
Thrombolytics, elderly patients						
Experimental	9	0.73	−0.03 (−4)	−0.13	−0.02	. . .
Control‡	5	0.67	0.12 . . .	. . .	. . .	

*Number of hospitals with at least 7 eligible patients both before and after intervention. Sample sizes of patients were 1807 for aspirin; 862 for β-blockers; 3342 for lidocaine (ineligible patients); and 544 for thrombolytics.
†One-sided test.
‡Control group changes not interpretable because of the very low number of eligible patients (noncomparable) and hospitals (total of 84 patients at follow-up).

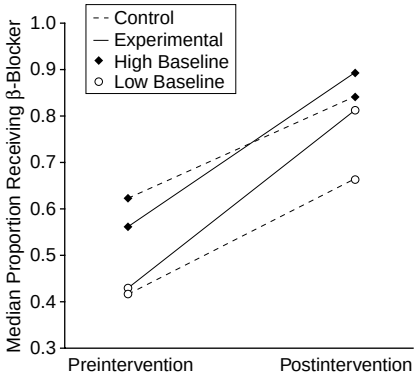

Figure 2.—Change in proportion of eligible patients receiving β-blockers, stratified by hospitals above and below median baseline proportion. Median proportion was 0.49 (experimental group). The number of hospitals was 11 for the experimental group and 7 for the control group. Before and after change represents median change in proportion receiving β-blockers in each of the 4 strata.

crease the risk-to-benefit ratio. The data suggest that study group physicians were using such characteristics to help determine who, among otherwise eligible elderly patients, would receive thrombolysis. Thirty-six percent of nonrecipients of thrombolytics were very old (≥85 years) compared with only 5% of eligible recipients of thrombolytics. Also, nonrecipients were 3 times more likely to have a severe comorbidity or physical impairment (38% vs 12%), and 44% more likely to present late (6 to 12 hours) (23% vs 16%) compared with eligible recipients of thrombolytics. Almost two thirds of the 30% of eligible elderly nonrecipients had one of these characteristics.

COMMENT

Discovering effective ways to measure and improve the quality of medical care continues to challenge health delivery systems and insurers worldwide.[32-37] Recruitment of local medical opinion leaders into systematic quality improvement initiatives is a promising strategy to achieve this goal because these individuals are already changing their peers' clinical practice through informal provider networks[1-3]; however, only limited data are available on the effectiveness of this approach.[4,6-8,38] This RCT, involving 37 community hospitals and 5347 AMI patients, is, to our knowledge, the only controlled study evaluating the effectiveness of combining recruitment of opinion leaders and performance feedback in a nonacademic setting. We believe it is also the only large RCT of any educational intervention to improve therapeutic decision making in AMI, despite extensive evidence of underuse of effective and lifesaving therapies for this condition[20,21,39,40] and the existence of numerous national and local practice guidelines.[17,18]

Several conclusions can be drawn from this study. First, opinion leaders were easily identifiable and enthusiastic about joining systematic efforts to improve the quality of care. Second, the intervention was successful in increasing the use of 2 highly effective AMI therapies that were promoted in national and local guidelines, namely, aspirin and β-blockers. Third, the intervention did not increase already high thrombolysis rates in eligible elderly patients; and nonrecipients were likely to be much older and more frail than recipients of thrombolytics. Finally, the large changes in use of most study drugs in control hospitals illustrates once again the importance of control group designs in such research.[27,28] Without controls, we would have incorrectly attributed all of the reduction in lidocaine use and about half of the increase in β-blocker use to the intervention.

We believe that different characteristics of the study practices and their respective barriers played an important role in determining intervention responses.[41] The strong secular decline in lidocaine use between 1992 and 1996 suggests that there were few barriers to following increasing numbers of recommendations to abandon prophylactic use of this agent.[17,19,42] Moreover, postintervention interviews with opinion leaders at both control and intervention hospitals confirmed that many hospitals simply removed prophylactic lidocaine from their protocols or standing orders. Hospitals would have an obvious interest in reducing use of "routine" medications, especially if they posed risks to patients. However, we cannot rule out the possibility that the minimal mailed feedback and opinion leader intervention were both effective in reducing lidocaine use. We encountered moderate but surmountable barriers to β-blocker use—such as fears of bradycardia or hypotension—that could be addressed through opinion leader education. One possible reason for the large improvements in β-blocker use was that baseline use was low and opportunities for improvement were high. Also, in a separate analysis, we found that the intervention did not have any spillover effect on β-blocker use among patients with absolute or relative contraindications.

The perceived risks of thrombolysis in the elderly represented a substantial barrier to increased use. Previous research indicates that experience of rare catastrophic outcomes associated with a medical treatment, such as thrombolytic-related hemorrhagic stroke, can have a deleterious effect on subsequent utilization of that technology.[43] Experience with such adverse events seemed to outweigh the statistical evidence of survival benefit in the elderly. Furthermore, our data suggest that there was little room for improvement in use of thrombolytics. Eligible elderly patients who did not receive thrombolytic agents (27%) were 7 times more likely to be older than 85 years, 3 times as likely to have a severe comorbidity, and almost 50% more likely to present late (≥6 hours) compared with elderly recipients of thrombolytics. Previous conclusions regarding "underuse" of thrombolytics in the elderly[20,21,39] may be overstated in the context of these new findings,[44] and achievable goals for rates of use of thrombolytics in eligible elderly patients may need to be lowered substantially, perhaps to 80% or less.

A limitation of this study is that we cannot isolate the independent effects of feedback, opinion leaders, or system changes (eg, protocols) on changes in drug use. However, the intervention included several interrelated components that predis-

pose, enable, and reinforce changes; effective practice change strategies usually require 2 or more of these elements.[27] For example, detailed and credible feedback data may have provided initial motivation to increase use of β-blockers, but opinion leaders enabled their colleagues to overcome their excessive concerns about adverse effects of β-blockers; they were also instrumental in translating drug treatment recommendations into clinically acceptable system changes (eg, protocols) that reinforced use of these effective agents over time. These protocol revisions were a voluntary and natural consequence of opinion leader education since the study did not require such changes.

This study was not designed to have sufficient power to detect changes in mortality. Such an outcome study would need to be prohibitively large, because RCTs have demonstrated that 2 to 3 lives are saved per 100 patients treated with aspirin or β-blockers[12,14,16]; eligible patients represent, in some cases, only a fraction of study subjects; and unlike drug trials, changes in medication use are voluntary and incremental.

The overall effectiveness and generalizability of this quality improvement strategy merit discussion. One other well-controlled study that examined the effects of obstetrical opinion leader education in 4 Canadian community hospitals found a 20% proportional increase in trials of labor in eligible women who had had previous cesarean deliveries.[4] Taken together with previous data,[4,7,8] our study suggests that, when best practices are clearly defined by national consensus guidelines and rigorous evidence, guided quality improvement interventions using local opinion leaders can accelerate adoption of effective treatments in community practice. Such changes are especially likely when there is substantial room for improvements (eg, β-blockers following AMI). However, we do not know whether such interventions can affect different kinds of treatments, such as those without national consensus and good evidence. Future research should compare the cost-effectiveness of opinion leaders and alternative interventions and identify the types of practices most amenable to change using this approach.

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