

The Urgent Need to Improve Health Care Quality

Institute of Medicine National Roundtable on Health Care Quality

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Objective.—To identify issues related to the quality of health care in the United States, including its measurement, assessment, and improvement, requiring action by health care professionals or other constituencies in the public or private sectors.

Participants.—The National Roundtable on Health Care Quality, convened by the Institute of Medicine, a component of the National Academy of Sciences, comprised 20 representatives of the private and public sectors, practicing medicine and nursing, representing academia, business, consumer advocacy, and the health media, and including the heads of federal health programs. The roundtable met 6 times between February 1996 and January 1998. It explored ongoing, rapid changes in health care and the implications of these changes for the quality of health and health care in the United States.

Evidence.—Roundtable members held discussions with a wide variety of experts, convened conferences, commissioned papers, and drew on their individual professional experience.

Consensus Process.—At the end of its deliberations, roundtable members reached consensus on the conclusions described in this article by a series of discussions at committee meetings and reviews of successive draft documents, the first of which was created by the listed authors and the Institute of Medicine project director. The drafts were revised following these discussions, and the final document was approved according to the formal report review procedures of the National Research Council of the National Academy of Sciences.

Conclusions.—The quality of health care can be precisely defined and measured with a degree of scientific accuracy comparable with that of most measures used in clinical medicine. Serious and widespread quality problems exist throughout American medicine. These problems, which may be classified as underuse, overuse, or misuse, occur in small and large communities alike, in all parts of the country, and with approximately equal frequency in managed care and fee-for-service systems of care. Very large numbers of Americans are harmed as a direct result. Quality of care is the problem, not managed care. Current efforts to improve will not succeed unless we undertake a major, systematic effort to overhaul how we deliver health care services, educate and train clinicians, and assess and improve quality.

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A complete list of the members of the National Roundtable on Health Care Quality appears at the end of this article.

All members are expressing their individual views and not necessarily those of agencies or organizations with which they may be affiliated.

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FEW ISSUES are more central to the ongoing debate about health care in the United States than quality of care. The Institute of Medicine (IOM), a component of the National Academy of Sciences, Washington, DC, convened the National Roundtable on Health Care Quality to bring together a wide variety of individuals to engage in a series of discussions about health care quality, a process that took place over a 2-year period. The roundtable solicited presentations from experts, convened conferences, and initiated a parallel set of detailed discussions about managed care and quality. (Additional information about the roundtable's work is available at <http://www2.nas.edu/hcs/>.)

See also p 1006.

The roundtable, which met 6 times between February 1996 and January 1998, reached consensus on the conclusions delineated here by a process of examining the information it received from these processes and the experience of its members. The consensus evolved during the final meetings of the roundtable. The first draft of a document reflecting these conclusions was created by the listed authors and the IOM project director. Revisions were made in accordance with discussion at roundtable meetings and comments from individual members. The final document was approved following the formal report review process of the National Research Council of the National Academy of Sciences.

The roundtable concluded that, following a period of appropriate and in-

tense concern about health care costs, a national focus on improving the quality of health care is imperative. The roundtable reached this conclusion by the following reasoning:

1. The quality of health care can be precisely defined.^{1,2} In many instances, quality measures have the same degree of accuracy as the majority of measures used in clinical medicine to make vital decisions about patient care. These quality measures have been used in a wide array of scientifically valid studies to assess the nature and magnitude of specific quality problems.

2. At its best, health care in the United States is superb. Unfortunately, it is often not at its best. Problems in health care quality are serious and extensive; they occur in all delivery systems and financing mechanisms. Americans bear a great burden of harm because of these problems, a burden that is measured in lost lives, reduced functioning, and wasted resources. Collectively, these problems call for urgent action.

3. A few health plans, hospitals, and integrated delivery systems have made impressive efforts to improve their quality of care, and a number of successes in improving quality for specific patient groups have been documented.³⁻⁵ However, many more institutions have made little, if any, effective effort to improve, and major obstacles lie in the way of rapid, systemwide progress. There are no available data identifying individual health plans, hospitals, or health care systems that deliver care that is uniformly and consistently of the highest quality. Therefore, there are no clear role models of exemplary delivery systems.

4. Taken together, these circumstances require a major effort to rethink and reengineer how we deliver health care services and how we assess and try to improve the quality of care.

QUALITY CAN BE DEFINED AND MEASURED

The IOM council addressed these quality-of-care issues in 1994.¹ The roundtable concurs with the council's view that the IOM's definition of quality, developed in 1990, has been widely accepted and is still robust today: "Quality of care is the degree to which health services for individuals and populations increase the likelihood of desired health outcomes and are consistent with current professional knowledge."²

Several ideas in the definition deserve elaboration. The term *health services* refers to a wide array of services that affect health, including those for physical and mental illnesses. It includes services aimed at preventing disease and promot-

ing health and well-being as well as acute, long-term, rehabilitative, and palliative care. Furthermore, the definition applies to many types of health care practitioners (eg, physicians, nurses, various other health care professionals) and to all settings of care (from hospitals and nursing homes to physicians' offices, community sites, and even private homes).

Including both *individuals* and *populations* draws attention to the different perspectives that need to be addressed. On one hand, we are concerned with the quality of care that individual health plans and clinicians deliver to individuals in specific episodes of care. On the other hand, we must direct attention to the quality of care across the entire system. In particular, we must ask whether all parts of the population have access to needed and appropriate services and whether their health status is improving.

The phrase *desired health outcomes* refers to health outcomes that patients desire and highlights the crucial link between how care is provided and its effects on health, as well as the need to ensure that patients and their families are well informed about alternative health care interventions and their expected outcomes. It underscores the importance of being mindful of people's ability to function as well as possible in their daily lives in addition to attending to more narrowly defined medical outcomes of disease. It also includes a consideration of patient and family satisfaction with health care services.

The definition emphasizes that high-quality care *increases the likelihood* of beneficial outcomes. It reminds us that quality is not identical to positive outcomes. Poor outcomes occur despite the best possible health care because disease often defeats our best efforts. Conversely, patients may do well despite poor quality care because humans are resilient. Assessing quality thus requires attention to both processes and outcomes of care.

Current professional knowledge emphasizes that health care professionals must stay abreast of the dynamic knowledge base in their professions and use that knowledge appropriately. No matter how good our understanding or measures of quality are today, we must always be prepared to revise them as new knowledge is generated about what works and what does not in health care to produce positive outcomes for patients. Although the knowledge and practices of individual clinicians are important for high-quality care, today we realize that no health care professional can deliver high quality alone. Increasingly, health care professionals practice

within groups and systems of care. The functioning of those systems in preventing and minimizing errors and the harm such errors may cause, coordinating care among settings and various practitioners, and ensuring that relevant and accurate health care information is available when needed are critical factors in ensuring high-quality care.

For more than 25 years, experts have worked to create reliable and valid measures with which to assess the quality of health care over a wide range of diagnostic and therapeutic services and for a broad array of health and medical problems. For some health care fields, such measurement tools can be put to immediate, widespread use, but in others, the science of quality measurement is in an early stage of development. There have been many advances as well as refinements in the field of quality measurement. As the acceptance of these measures has increased, so has the audience for them. With this wider attention has come the need to broaden the domain of measures to include outcomes as well as processes of care and to speak to the concerns of consumers by developing outcome measures that go beyond immediate morbidity and mortality to include various kinds of functional status.

In general, either processes or outcomes may be valid measures of quality. For an outcome to be a valid measure, it must be closely related to processes of care that can be modified to affect the outcome. For example, the proportion of patients with inoperable lung cancer who develop metastases within 6 months of diagnosis is an important outcome measure but not a valid quality measure, because no known processes of care can influence this outcome. For a process to be a valid measure, it must be closely related to an outcome that we care about. Thus, controlling hypertension is a process that is a valid measure of quality because it has been shown to reduce the occurrence of strokes and death.

A number of specific examples of different types of quality measures and their uses were discussed at the September 1996 IOM conference, *Measuring the Quality of Health Care: State of the Art*. As this experience made clear, quality of care for a great variety of specific clinical conditions and procedures can be measured with sufficient precision to make judgments and take needed actions to bring about improvement. The inventory of useful measures continues to grow. The Joint Commission on Accreditation of Healthcare Organizations, Oakbrook Terrace, Ill, and the National Committee for Quality Assurance, Washington, DC, have stimulated interest in developing quality measures and in quality measure-

ment. A large number of valid measures have been used to assess the magnitude of various quality problems.

QUALITY PROBLEMS ARE SERIOUS AND EXTENSIVE

Health care quality problems may be classified into 3 categories, underuse, overuse, and misuse. *Underuse* is the failure to provide a health care service when it would have produced a favorable outcome for a patient. Missing a childhood immunization for measles or polio is an example of underuse. *Overuse* occurs when a health care service is provided under circumstances in which its potential for harm exceeds the possible benefit. Prescribing an antibiotic for a viral infection like a cold, for which antibiotics are ineffective, constitutes overuse. *Misuse* occurs when an appropriate service has been selected but a preventable complication occurs and the patient does not receive the full potential benefit of the service. Avoidable complications of surgery or medication use are important misuse problems. A patient who suffers a rash after receiving penicillin for strep throat despite having a known allergy to that antibiotic is an example of misuse. Evidence from careful research studies demonstrates a large number of serious problems in each of these categories. A recent review of quality research published from 1993 to 1997 reached the same conclusion,^{6,7} as did the report of the President's Advisory Commission on Consumer Protection and Quality in the Health Care Industry.⁸

Underuse of proven effective interventions leads to major foregone opportunities to improve health and function. Undetected and untreated hypertension or depression, failure to immunize children, and prenatal care begun too late in pregnancy are examples of important underuse problems. The magnitude of these problems is considerable. Failure to use effective treatments (eg, thrombolytics, β -blockers, aspirin, and angiotensin-converting enzyme inhibitors) for acute myocardial infarction for all patients who could benefit from these interventions may lead to as many as 18 000 preventable deaths each year in the United States.⁹ One recent study showed that in one group of elderly acute myocardial infarction patients, 79% of eligible patients did not receive β -blockers; their subsequent mortality at 2 years was 75% greater than those who had received β -blockers.¹⁰

Underuse is by no means confined to managed health care plans, which have financial incentives to reduce the amount of care they provide. Several studies have shown that between 40% and 60% of patients in selected health maintenance

organization and fee-for-service populations do not receive needed care for specific effective services. One study, for example, showed that 59% of hypertensive patients did not have controlled blood pressures in fee-for-service plans compared with 46% in managed care plans.¹¹ The same study also documented that 65% of women treated in fee-for-service settings did not receive scheduled mammograms compared with 45% of those in managed care plans. Another study showed a failure to detect and treat depression by general medical clinicians in 58% of managed care patients compared with 46% of fee-for-service patients.¹² These data and others like them led roundtable members to conclude that quality is the problem, not managed care.

Underuse problems are exacerbated when people lack health insurance, a problem that is faced by more than 41 million Americans. The net health effect of the barrier to access to care that results from being uninsured is measured in shortened lives and increased disability. One study found that those without health insurance had a 25% greater chance of dying within 12 years, controlling for age, race, education, income, and comorbidity.¹³ Other work has confirmed these findings and extended them to show that lack of insurance is associated with poor functional status and that loss of health insurance, particularly Medicaid, can be associated with deterioration in chronic disease secondary to reduced access to effective care.¹⁴⁻¹⁷

Overuse is also common in US medicine. Two recent studies showed that 21% of all antibiotic prescriptions (a total of 23.8 million prescriptions) given to ambulatory adults or children in 1992 were used to treat colds, other upper respiratory tract infections, or bronchitis, conditions for which antibiotics are ineffective and pose the risk of life-threatening adverse drug reactions and an increase in antibiotic resistance.^{18,19} The RAND Health Services Utilization Study, the results of which are now 17 years old, is the largest study of overuse and, to our knowledge, the only one that examined multiple regions of the country. It showed that 17% of coronary angiographies, 32% of carotid endarterectomies, and 17% of upper gastrointestinal tract endoscopies were performed for clearly inappropriate indications in a nationally representative sample of Medicare beneficiaries in 1981.²⁰ No data have been published subsequently that suggest significant improvements have occurred. Other studies have found that 16% of hysterectomies in a group of 7 health maintenance organizations were inappropriate, with individual plan rates

varying between 10% and 27%²¹; that 23% of children were proposed for tympanostomy tube insertion (the most common surgical procedure in childhood) for inappropriate reasons²²; and that 20% of cardiac pacemakers were inserted for clearly inappropriate indications.²³

Misuse problems (that is, the preventable complications of treatment) also occur with great frequency. Misuse is not the same as error because not all errors result in adverse events or injury. Many errors, such as the wrong dose of medication or misdiagnosis, may be identified before harm occurs. If not identified and corrected, however, many errors do cause injury. Recent research indicates that patient injuries resulting from the administration of medications occur at the rate of about 2000 per year in each large teaching hospital; about 28% are preventable given current knowledge.²⁴ Each of these preventable injuries adds nearly \$5000 to the cost of the hospital stay during which it occurs.²⁵ The Harvard Medical Practice Study estimated that more than 27 000 patient injuries due to negligent care occurred among patients hospitalized in New York State in 1984.²⁶ The RAND study of prospective payment for hospitals showed that Medicare patients who received poor-quality care while hospitalized for congestive heart failure, as judged by adherence to objectively defined criteria, experienced a 74% greater mortality rate within 30 days of hospital admission compared with patients who received good-quality care.²⁷

This tripartite classification of quality problems illuminates the relationship between quality and cost. It also helps answer the question of whether improving quality leads to increased or decreased costs. Reducing overuse improves quality (by sparing patients the unnecessary risk that attends to inappropriate health services) and reduces costs at the same time. Solving misuse problems also improves quality (by reducing the number of complications) and decreases costs (by eliminating the cost of treating complications). Fixing underuse problems, however, nearly always results in both increased costs and increased quality. This relationship arises from the fact that, except for immunizations and prenatal care, effective health care services generally do not save money.²⁸ If they are effective, they improve health and result in increased quality, but only at increased cost. The principal exception to this rule arises when services are narrowly targeted at very high-risk subgroups of people for whom expensive complications of disease are prevented with high frequency.²⁹ Such circumstances are un-

usual because we typically cannot predict with accuracy which individuals will suffer particular complications in the short term (eg, which patients with hypertension will suffer strokes in the next year).

These relationships also identify the most effective ways to improve the value of health care services, which may be defined as the health benefit per dollar spent. The largest improvements in value occur when the same action increases the numerator of the ratio while decreasing the denominator. If we improve quality by fixing overuse or misuse problems, we have exactly this impact on value. The impact on value of remedying underuse problems is less clear because both the numerator and the denominator of the ratio increase.

The evidence is compelling. Millions of Americans are not reached by proven effective interventions that can save lives and prevent disability. Perhaps an equal number suffer needlessly because they are exposed to the harms of unnecessary health services. Large numbers are injured because preventable complications of medical treatment are not averted. These problems exist in managed care and fee-for-service systems, in large and small communities, and in all parts of the country. Substantial opportunities exist to increase quality and decrease cost simultaneously by ameliorating problems of overuse and misuse.

OTHER QUALITY-OF-CARE ISSUES

In discussing quality problems in terms of underuse, overuse, and misuse, this statement does not attempt to address all the issues that might relate to quality. Such issues include geographic variations in the rates of use of health services, generalist and specialist physician training, the makeup of the nonphysician health care workforce, and the effect of organization of medical services as a determinant of quality, for which there is an emerging literature.³⁰⁻³² These and other relevant issues may be causal or explanatory factors leading to a better understanding of quality problems; that is, they will be related to specific underuse, overuse, or misuse problems.

CURRENT APPROACHES TO QUALITY IMPROVEMENT ARE INADEQUATE

The statement that our health care system faces quality problems of serious magnitude should not be taken as an indictment of the skill or motivation of the men and women who provide those health care services. Indeed, these people, who represent a host of different disciplines, are among the most highly trained, technically proficient, and best

motivated of professionals. In the vast majority of specific instances of problems in health care quality, individuals are not to blame. The answers are not simple and often involve shortcomings in the complex systems in which health care is delivered.

In part, the problems we face represent the obverse side of an extraordinary success story. In the past 25 years, we have generated an immense amount of new knowledge about what works to improve health and what does not. One crude index of the pace of this change is illuminating. The randomized controlled trial has become the "gold standard" for evaluating the efficacy of health care interventions of all sorts. Yet it is a relatively recent phenomenon; the first one was published in 1952. In the 30 years from 1966 through 1995, more than 76 000 journal articles were published from randomized controlled trials (as registered in the automated database MEDLINE). The first 5 years of that period contributed less than 1% of the total, whereas the last half decade contributed more than the previous 25 years combined. In the face of this avalanche of rigorous data on efficacy, our methods of training physicians and other clinicians and our systems for supporting them in the delivery of health care services have not kept pace. Their rigorous clinical training has not equipped them to make maximal use of a variety of methods to assess and improve their own practices. Principles of quality measurement and improvement could be included in the education and training of future practitioners to better prepare them for this ongoing responsibility.

Whether they are organized in solo practice, in small single-specialty partnerships, or in large multispecialty groups, too few physicians have ready access to all the data that would be useful to them as they care for patients. Too few hospitals take maximum advantage of all of their data in facilitating efficient patient care while systematically avoiding preventable complications. One hospital has given us a glimpse of what may be possible. Researchers at LDS Hospital in Salt Lake City, Utah, published their experience in reducing the frequency and impact of adverse drug events due to antibiotics. They assisted physicians in prescribing prophylactic antibiotic regimens in surgery and therapeutic treatments using a powerful set of computer-assisted guidelines. The results were impressive: a 30% decrease in the frequency of patient injuries due to antibiotics, a 27% decrease in the mortality of antibiotic-treated patients, and a 58% decrease in antibiotic costs per treated patient.³ However, this

example stands out starkly because it is so exceptional compared with the experience of the vast majority of other institutions.

A notable constraint to quality improvement is posed by the lack of an information infrastructure to support it in almost all health care delivery settings and the substantial investment needed to build such an infrastructure. Engaging clinicians actively and enthusiastically in quality improvement requires providing them with timely and detailed clinical information they believe and can use to judge quality of care. Collecting and analyzing these data, whether manually by record review or by sophisticated automated systems, are extremely expensive.³³

At present, quality improvement efforts are sporadic at best. They are typically limited to single, large institutions, usually hospitals. Long-term, multi-institutional quality improvement programs are infrequent, and regional attempts to improve quality across an entire delivery system are very rare. However, the exceptions are noteworthy. New York State's program of collecting standardized clinical data for coronary artery bypass surgery (CABS) patients, producing and publishing risk-adjusted mortality rates for hospitals and surgeons, and using these data to facilitate quality improvement efforts has resulted in lower statewide mortality following this procedure.^{34,35} This ongoing program now also produces risk-adjusted mortality data on percutaneous transluminal coronary angioplasty. The 5 hospitals in northern New England at which CABS is performed have used continuous quality improvement techniques to achieve reductions in mortality as well.⁵ Pennsylvania has published data on risk-adjusted mortality following CABS and acute myocardial infarction, but their impact on inducing improvement is not clear.^{36,37} Some other states are beginning to experiment with compiling and publishing less complicated data on hospital performance.³⁸ The large majority of these efforts, including a few regional efforts to publish performance data for managed care plans, consist only of compiling and reporting data. Improvement is left to individual hospitals or plans and is rarely documented.

The Joint Commission on Accreditation of Healthcare Organizations, the National Committee for Quality Assurance, and the Peer Review Organizations of the Health Care Financing Administration are encouraging organizations to use methods of continuous improvement but the effectiveness of these efforts remains to be documented.

Four major strategies have been advocated to move the health care delivery system toward improving quality. Whether one believes in regulation, continuous quality improvement, marketplace competition, or payment incentives as the most effective way to improve quality of care, evidence and experience to date suggest that none of these taken alone will prove up to the challenges we face. The challenges may be stated simply: (1) to always provide effective care to those who could benefit from it, (2) to always refrain from providing inappropriate services, and (3) to eliminate all preventable complications.

Although regulation is not currently fashionable, states are pursuing it vigorously as a means to control perceived abuses in managed care. Regulation is the only mechanism we have to protect the public from egregiously poor providers. Another of its advantages is that it can reach every corner of the delivery system as compared with improvements made by a single hospital or health plan. Although it can establish minimum standards of performance reasonably well, uniform enforcement of those standards has proved far more problematic. In addition, regulation is inflexible, difficult to modify quickly as knowledge changes, and not well suited to motivate those already performing well to strive for even greater achievement.

Continuous quality improvement emerged from the industrial sector as an effective package of theory and practical tools to reduce errors in the production process. Although widely praised in business circles, it is far less widely adopted. As applied to health care, it has been similarly praised but has also spread slowly. Its most exemplary practitioners, who have achieved notable successes,³⁻⁵ emphasize that it is most effective when used as an integral part of a scientific approach to improving clinical practice. Very few data document the effectiveness of continuous quality improvement, however,³⁹ and even exemplary practitioners have had difficulty in disseminating its benefits uniformly throughout their institutions. Among its potential strengths are an ability to motivate good performers to excel and an emphasis on generating new methods for achieving improvement. Among its limitations are a too narrow focus on administrative (as opposed to clinical) aspects of care and a lack of attention to problems of overuse or underuse. Future experience may yield increased effectiveness. Current experience in both health care and other sectors of the economy suggests that its impact will be useful but may be limited.⁴⁰

Marketplace competition is the engine driving many changes in health care.

Market advocates believe that providing more information about quality to the public will induce health plans, hospitals, and physicians to compete by improving the quality of their care in the expectations of increasing market share. Skeptics point out that no health care market currently competes on the basis of improving quality and there is little theoretical basis in economics to predict that this change will occur.⁴¹

Many experts believe that payment incentives (to health plans, hospitals, or physicians) can be powerful forces to drive improved quality. Unfortunately, the dominant methods of payment in use today do not achieve this goal. Unadorned fee-for-service payments encourage overuse, whereas capitation payments encourage underuse. No current payment system systematically rewards excellence in quality. The immediate prospects for change are not bright, although some health plans have begun to develop performance-based payment systems as incentives to improve quality. These efforts have yet to be evaluated. In another area, the difficulties of assembling sufficient data with which to construct risk-adjustment methods have hobbled efforts to counteract the powerful incentives health plans now face to avoid sick individuals and market their services only to healthy people.

Furthermore, and perhaps most important, even if the right set of strategies could be devised to encourage quality improvement, there are no clear role models of exemplary delivery systems to emulate. Whether one examines hospitals, medical groups, health plans, or integrated delivery systems, no institution in any of these categories can provide a blueprint for solving the multitude of current quality problems. Neither has academic medicine met its part of the challenge to modernize its education and training methods so young physicians can begin practice with an understanding of health care quality and the tools needed to engage in a career-long effort to assess and improve the quality of care they provide.

AN URGENT NEED FOR RAPID CHANGE

Who should be concerned about health care quality problems and who should be involved in their solution? The answer is everyone: health care professionals, patients and their families, consumer advocates, health care administrators (whether serving in health care plans, hospitals, medical groups, nursing homes, or other facilities), private and public purchasers of health care services, and policymakers at the national, state, and local levels.

The roundtable believes that health care professionals should take the lead in improving quality, and it strongly urges leaders in the health care professions as well as practicing clinicians to actively do so. Leadership in quality improvement is also a joint responsibility of all who serve in health care organizations, including managers, data and information specialists, laboratory technicians, housekeeping staff, dietary personnel, nurses, and physicians. Individual patients must have the opportunity and the information they need to participate in their own care and to take responsibility, where necessary and appropriate, for their own health. Consumer advocates and purchasers can press to keep quality of care at the top of the agenda as an issue of concern throughout the health care system and to seek effective ways for health care professionals, administrators, and others to be accountable to patients and to society for the quality of care. Policymakers at all levels of government can foster opportunities for communication of best practices and other innovations, increase research on quality measurement and improvement, and assist the development of more effective information and delivery systems. We should all strive for such fundamental improvement that health care becomes not only technologically dazzling but also compassionate, reliable, appropriate to a patient's needs, and safe.

The burden of harm conveyed by the collective impact of all of our health care quality problems is staggering. It requires the urgent attention of all the stakeholders: the health care professions, health care policymakers, consumer advocates, and purchasers of care. The challenge is to bring the full potential benefit of effective health care to all Americans while avoiding unneeded and harmful interventions and eliminating preventable complications of care. Meeting this challenge demands a readiness to think in radically new ways about how to deliver health care services and how to assess and improve their quality. Our present efforts resemble a team of engineers trying to break the sound barrier by tinkering with a Model T Ford. We need a new vehicle or, perhaps, many new vehicles. The only unacceptable alternative is not to change.

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The human suffering related to the 17 deaths, nearly 6000 hospitalizations, and more than 330 000 ED visits a year and the associated economic impacts justify further efforts to prevent dog bites.

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1. Weiss HB, Friedman DI, Coben JH. Incidence of dog bite injuries treated in emergency departments. *JAMA*. 1998;279:51-53.
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This letter was shown to Mr Weiss, who declined to reply.—ED.

Treatment of Smokeless Tobacco Addiction With Bupropion and Behavior Modification

To the Editor: An estimated 6.9 million people in the United States use smokeless tobacco, such as chewing tobacco or snuff. The health risks associated with smokeless tobacco use include increased rates of oropharyngeal cancer and increased subsequent cigarette smoking.¹ Despite the widespread use of smokeless tobacco, relatively few data have appeared in the literature regarding treatment of addiction to it.^{2,3} We describe a case of successful treatment of smokeless tobacco use with an approach that combined pharmacotherapy and behavior modification.

Report of a Case. A 31-year-old man had an 11-year history of using 1 can per day of smokeless tobacco and denied any history of smoking. The patient previously had made several attempts to stop use of smokeless tobacco with nicotine patches and abrupt cessation but had only limited success for a short time. He agreed to a trial of bupropion hydrochloride and a 4-week course of behavior modification. These sessions covered effective withdrawal strategies, coping skills for cravings, initial tobacco cessation, and extended maintenance skills. During the first session the patient was asked to set a quit date that would occur while he was in the group. The patient started treatment with bupropion hydrochloride (150 mg twice daily) 1 week prior to group treatment. After approximately 1 week of taking medication, the patient noted a reduction in cravings for smokeless tobacco, and at 5 weeks he was tobacco free. He noted few adverse effects associated with the medication but reported a change in the taste of the smokeless tobacco as the most prominent effect. After taking bupropion for approximately 3 days, the patient described the smokeless tobacco as "tasting terrible," and he felt the poor taste was 1 factor in becoming tobacco free. He also felt the

copied skills learned in the group allowed him to withstand tobacco cravings and avoid relapse. The medication was continued for a total of 10 weeks, and the patient had no difficulties and did not experience any withdrawal symptoms (eg, irritability, anxiety, headaches, dizziness) after its discontinuation. The patient was followed up at 1, 3, and 6 months and remains tobacco free at 8 months.

Comment. Recent research suggests a 2-pronged approach to tobacco cessation with use of both pharmacologic treatment and behavior modification provides maximum efficacy for tobacco cessation.⁴ Bupropion is considered a first-line therapy for smoking cessation.⁵ However, to our knowledge, use of bupropion for treatment in smokeless tobacco addiction has not been reported previously. The behavior modification used a psychoeducational format to develop effective coping strategies for tobacco cessation. These classes have been shown to significantly increase rates of tobacco cessation.⁴ Further studies are needed with a larger population to quantify the efficacy of bupropion hydrochloride and behavior modification in the treatment of nicotine addiction caused by smokeless tobacco.

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Disclaimer: Conclusions and opinions expressed are those of the authors and do not necessarily reflect the position or policy of the US government, Department of Defense, Department of the Army, US Army Medical Command, or the 82D Airborne Division.

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CORRECTIONS

Incorrect Web Site Address: In the Consensus Statement entitled "The Urgent Need to Reform Health Care Quality: Institute of Medicine National Roundtable on Health Care Quality," published in the September 16, 1998, issue of THE JOURNAL (1998;280:1000-1005), there was an incorrect Web site address. On page 1000, in the last sentence of the first paragraph of text, the Web site address should have read "http://www2.nas.edu/hcs/."

Incorrect Reference: In the Editorial entitled "Low-Tech Autopsies in the Era of High-Tech Medicine: Continued Value for Quality Assurance and Patient Safety," published in the October 14, 1998, issue of THE JOURNAL (1998;280:1273-1274), there was an incorrect reference. On page 1274, reference 10 should have read "Nichols L, Aronica P, Babe C. Are autopsies obsolete? *Am J Clin Pathol*. 1998; 110:210-218."

Responding to Patient Requests for Physician-Assisted Suicide

To the Editor: While I admire Dr Emanuel's¹ intent to synthesize an emerging literature about how to respond to requests for physician-assisted suicide (PAS), it is hard to imagine showing that algorithm to a patient and saying, "Here's what we're going to do." I think many patients would conclude that they were being forced into dropping their request, in a linear process that terminates at "no." Although I have mixed feelings about PAS, I think that physicians evaluating requests need to create an encounter in which meaningful dialogue can occur. The clinical approach must be open-minded and practical.

Emanuel's algorithm ought to have a box that explicitly recognizes that a physician could feel that a patient's request was authentic, uncoerced, uninfluenced by depression or cognitive impairment, and persistent despite very good palliative care. The reason? Physicians ought to acknowledge the possibility that a particular patient's request might be legitimate. This physician open-mindedness, I believe, is an essential prerequisite for a meaningful dialogue between patient and physician. A physician who ultimately recognizes that a patient's request is legitimate is not required to provide PAS illegally. But even a physician who believes that PAS is morally unacceptable should acknowledge the tension inherent in a controversial situation involving 2 individuals with very different moral viewpoints. This acknowledgment can enable a patient and physician to continue a dialogue that improves end-of-life care in ways other than a prescription meant to hasten death.

In addition, the algorithm lacks real-world practicality. In the algorithm, a physician would evaluate depression, then competence, then symptoms, in a linear sequence of encounters. Yet there is no reason that a physician should treat depression before symptoms; they ought to be treated simultaneously. Also, the algorithm implies that treating depression will cause a patient to drop the request, but this is an unverified assumption. A completely realistic algorithm would include the possibility that physicians will act illegally to provide a patient with the means for PAS, which has been documented in Washington State and nationwide.^{2,3}

While I agree that the 2 principles of nonintrusion and relief of suffering are important in handling requests for PAS, I do not understand why Emanuel emphasizes that only 2 principles are necessary. Why is ethical parsimony desirable? Requests for PAS are messy, clinically complex, and emotionally challenging. In these situations, I need all the help I can get.

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1. Emanuel LL. Facing requests for physician-assisted suicide: toward a practical and principled clinical skill set. *JAMA*. 1998;280:643-647.

2. Back AL, Wallace JI, Starks HE, Pearlman RA. Physician-assisted suicide and euthanasia in Washington State: patient requests and physician responses. *JAMA*. 1996;275:919-925.

3. Meier DE, Emmons CA, Wallenstein S, Quill T, Morrison RS, Cassel CK. A national survey of physician-assisted suicide and euthanasia in the United States. *N Engl J Med*. 1998;338:1193-1201.

To the Editor: As consultation psychiatrists we are disturbed by Dr Emanuel's¹ 8-step approach to dying patients who persistently request assisted suicide. The approach is burdensome, and some steps offer no benefit to the patient. Emanuel states that decision-making capacity should be assessed first to determine if the request is valid. We know of no circumstance in medicine in which it is recommended that informed consent be obtained for a procedure that will, under no conditions, be offered. Evaluating whether the patient has depression is important because the patient may have an illness for which successful treatment may improve his or her final days. However, Emanuel states that depression should be evaluated to determine whether the request is rational. Again, if assisted suicide is not an option, why take up the patient's time in making this distinction? Moreover, Emanuel assumes that the presence of depression, by itself, makes the request for assisted suicide irrational and therefore makes the patient incompetent. In fact, available data indicate a weak and variable effect of depression on end-of-life decisions, including assisted suicide.^{2,3}

In this approach the patient is not informed until late in the process that assisted suicide was never an option. The patient should not be brushed off with a refusal at the first request, because an opportunity to explore the meaning behind the request may be lost.⁴ Additional steps in Emanuel's approach might be pursued if the patient and physician share the goal of diminishing the patient's suffering. Some patients might be enraged if they invest scarce time and energy completing these steps only to be informed that there was never any intent to comply with the request.

Emanuel only outlines the approach to the bold, persistent patient who continues to request a lethal prescription. Such a patient may thrive on engaging the physician on this issue. But what about the patient who stops asking? Should the physician ask if the patient's desire has diminished or remains unspoken? In psychiatric practice, it is the standard of care to actively elicit thoughts of suicide in recently suicidal patients. How should the physician respond to the patient who quietly continues to desire assisted suicide, but perceives the physician's reluctance and stops asking?

GUIDELINES FOR LETTERS. Letters discussing a recent *JAMA* article should be received within 4 weeks of the article's publication and should not exceed 400 words of text and 5 references. Letters reporting original research should not exceed 500 words and 6 references. Please include a word count. Letters must not duplicate other material published or submitted for publication. Letters will be published at the discretion of the editors as space permits and are subject to editing and abridgment. A signed statement for authorship criteria and responsibility, financial disclosure, copyright transfer, and acknowledgment is essential for publication. Letters not meeting these specifications are generally not considered. Letters will not be returned unless specifically requested. Also see Instructions for Authors (January 6, 1999). Letters may be submitted by surface mail: Letters Editor, *JAMA*, 515 N State St, Chicago, IL 60610; e-mail: jama-letters@ama-assn.org; or fax (please also send a hard copy via surface mail): (312) 464-5824.

Edited by Margaret A. Winker, MD, and Phil B. Fontanarosa, MD, Senior Editors.

Overall, Emanuel claims to be motivated by the principles of noninvasion and comfort. Her approach is intrusive and may not benefit the patient because the overall goal is not to advance patient benefit but rather to relieve the physician of the requests for assisted suicide.

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1. Emanuel LL. Facing requests for physician-assisted suicide: toward a practical and principled clinical skill set. *JAMA*. 1998;280:643-647.
2. Ganzini L, Lee MA. Psychiatry and assisted suicide in the United States. *N Engl J Med*. 1997;336:1824-1826.
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4. Lee MA, Ganzini L, Brummel-Smith K. When patients ask about assisted suicide. *West J Med*. 1996;185:205-208.

To the Editor: Dr Emanuel's¹ 8 progressive steps for responding to requests for PAS are a welcome set of recommendations, especially given her clear articulation of their basis in 2 allegedly uncontroversial moral principles. Although I support their use, I wonder if her second principle—"the physician's obligation to provide suffering patients effective comfort care"—will remain as uncontroversial as she claims it is when the length to which she carries it is fully grasped.

In her seventh step, Emanuel wants physicians both to ensure that care plans are being followed and "to secure maximum relief of suffering." Within the latter she includes the option of "anesthetic coma" for patients whose pain cannot be otherwise controlled. She advises that the coma be monitored to maintain its level, "preventing either anesthesia-induced death or unwanted return to consciousness." The use of last-resort anesthetic coma, she believes, pulls any remaining psychological ground out from under further requests for PAS: while the patient may continue in this comatose state for days (months?) before dying, the patient is no longer suffering.

But how different really is this approach from PAS? Note what is being done: the physician actively induces a permanent state of coma. Permanent, because if pain could not have been controlled in any other nonlethal way and the induction of anesthetic coma was desired by the patient, the physician will not be justified in allowing any return to consciousness. If the patient is now unconscious, how can the physician justifiably believe that that level of anesthetic is no longer wanted?

Is such active induction of permanent coma really notably different from killing the patient? Kill the person, kill the patient—why not take your choice? Or perhaps the problem is that Emanuel's position still needs the principle of double effect: the physician intends the anesthetic to control the pain but not to induce permanent coma, while she knows coma is coming. But then why not use double effect to justify dosages that she knows will be lethal?

I doubt that Emanuel's approach ultimately will stave off the momentum toward PAS. Will society really embrace the image

of active inducement of permanent coma but reject the image of active inducement of equally voluntary death?

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1. Emanuel LL. Facing requests for physician-assisted suicide: toward a practical and principled clinical skill set. *JAMA*. 1998;280:643-647.

To the Editor: Although I appreciated Dr Emanuel's¹ enumeration of principles to be used in assessing patient requests and physician responses, her article seemed to emphasize and weight one or another of the principles to arrive at a predetermined result. To avoid "unwanted intervention" a patient may be permitted to starve to death, but "relief of suffering," including psychological suffering, is prohibited because of a predetermined decision that PAS is wrong? I think not. The principles enumerated could just as easily have been used to justify PAS. While I generally agree with the process and results included in Emanuel's article, in no way does her reasoning reflect an objective approach and response to a very complicated and difficult problem.

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1. Emanuel LL. Facing requests for physician-assisted suicide: toward a practical and principled clinical skill set. *JAMA*. 1998;280:643-647.

In Reply: I appreciate the opportunity to further explain aspects of the protocol that I proposed for clinicians responding to a request for PAS. The algorithm is a method for organizing thinking rather than a step-by-step recipe. As with most algorithms, clinicians must tailor it to the individual patient.

While some believe that PAS can be legitimate, others do not. Physicians are not obliged to agree with a patient who requests it. Rather, physicians need to appreciate and acknowledge the patient's experience. This difference becomes stark when by agreeing with the patient, the physician is perceived by the patient as not valuing the patient's life. The patient's perceived worthlessness and burdensomeness feel confirmed, and the patient is propelled toward suicide.¹⁻⁴

A patient cannot be either "brushed off with a refusal" or misled into believing that suicide assistance will be given. Physicians may well need to inform patients early on that PAS is not a possibility, and they should do so while still providing support and engaging in a joint search for sources of control, comfort, dignity, and welcome.

Depression is predictive of requests for PAS.^{5,6} A meaningful conversation is not possible if the request is driven by a pathological and treatable cause. Also important, for a physician to avoid diagnosis, treatment, or follow-up inquiry would be terrible; it is not the standard of practice and is not suggested.

The intent of the protocol is to assess and treat root causes of suffering. Competence is not assessed to obtain consent for a procedure that will not be offered, but rather to assess whether the dialogue can be meaningful and, if not, then to assess and treat the causes of decision-making incapacity. This protocol is far from intrusive. Rather, these oddly intrusive interpretations invite coun-

terproductively polarized debate. It is time that medical professionals get on with helping one another take good care of dying patients.

Induction of what may become permanent anesthesia is not, properly used, equivalent to killing the patient. The difference between induction of permanent anesthesia and induction of death is that in the former the administered agents do not cause death. So there is no known-but-unintended death to justify. There is rather known and intended coma. Death results from the illness.

This protocol, far from prohibiting relief of suffering, requires diagnosis and treatment of suffering, including the psychological suffering that drives many PAS requests. Furthermore, the 2 invoked principles of nontransgression and of comfort care cannot justify PAS. Commentators who argue for PAS tend to invoke the larger notion of autonomy, and the position remains controversial. Since it also remains illegal in all states but 1, PAS is not included in the protocol. I do argue elsewhere against PAS. However, no position on the matter is necessary for the logical coherence of this protocol. Rather, the protocol aims to allow physicians from both sides of the debate to better benefit patients whose lives feel worse to them than death.

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1. Miles S. Physicians and their patients' suicides. *JAMA*. 1994;271:1786-1788.
2. Meier D. Should we ever hasten death? In: Emanuel LL, Zeman A, eds. *Neurology & Ethics*. Philadelphia, Pa: WB Saunders. In press.
3. Emanuel EJ, Fairclough DL, Daniels ER, Clarridge BR. Euthanasia and physician-assisted suicide: attitudes and experiences of oncology patients, oncologists, and the public. *Lancet*. 1996;347:1805-1810.
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5. Chochinov HM, Wilson KG, Enns M, et al. Desire for death in the terminally ill. *Am J Psychiatry*. 1995;152:1185-1191.
6. Breitbart W, Rosenfeld B, Passik S. Interest in physician-assisted suicide among ambulatory HIV-infected patients. *Am J Psychiatry*. 1996;153:238-242.

Sunlight Exposure and Cataract

To the Editor: Dr West and colleagues¹ demonstrate an association between light and cortical cataracts and the attenuation of cataract development in persons who wear glasses. From this they suggest that there is a causal relationship between UV-B radiation and the development of cortical cataract, but I question whether this is due just to UV-B. This term was developed by dermatologists to define a specific wavelength range in the UV spectrum (290-320 nm) that causes erythema of the skin. This distinguishes it from UV-A (320-400 nm), which does not cause erythema.

There are 3 reasons UV-A cannot be excluded from cataract formation: (1) the amount of UV-B reaching the surface of the lens through the cornea is small, less than 3% of the total UV radiation²; (2) this study discriminated between UV-A and UV-B by determining whether glasses were worn, but plastic glasses now in vogue absorb light out to at least 350 nm and some out

to 400 nm. By integrating the area under the wavelength-dependent solar intensity, I have determined that even the minimally absorbing 350-nm plastic glasses cut out 7 to 8 times more UV-A than UV-B (unpublished data, 1998); and (3) the assumption is that the decrease in cataract formation is due to the light-filtering characteristics of spectacles, but it has been suggested that cortical cataracts in humans are due in part to an *albedo effect*,^{3,4} in which the cornea focuses light on the inferonasal portion of the lens. This is the portion of the human lens where most cortical cataracts originate. It may be that spectacles refocus light, disrupting that effect. Thus, it is equally valid to interpret their observations in terms of spectacles refocusing the light and disrupting this effect as to attribute their observations to filter effects alone.

Epidemiological studies do not support the contention that UV-B alone causes cortical cataract. Consideration of the absorption spectrum of the aged human lens also strongly indicates that UV-B cannot be the only damaging form of light. The primary absorbing species in older human lenses is yellow lens protein,⁵ which has a broad, structureless spectrum extending out to approximately 550 nm. Photobiological damage can also be initiated by UV-A and near visible wavelengths, which are incident on the lens at much higher intensities than UV-B. This is analogous to the photochemistry of melanin in which photobiological damage is elicited throughout the entire absorption spectrum and does not exhibit a sharp cutoff at wavelengths longer than UV-B.

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1. West SK, Duncan DD, Muñoz B, et al. Sunlight exposure and risk of lens opacities in a population-based study: the Salisbury Eye Evaluation Project. *JAMA*. 1998; 280:714-718.
2. Zigman S. Environmental near-UV radiation and cataracts. *Optom Vis Sci*. 1995; 72:899-901.
3. Coroneo MT. Albedo concentration in the anterior eye: a phenomenon that locates some solar diseases. *Ophthalmic Surg*. 1990;21:60-65.
4. Narayanan P, Merriam J, Vazquez M, Dillon J. Experimental model of light focusing of the peripheral cornea: the spatial distribution of light on the human lens. *Invest Ophthalmol Vis Sci*. 1996;37:37-41.
5. Dillon J, Wang RH, Atherton S. Photochemical and photo-physical studies on human lens constituents. *Photochem Photobiol*. 1990;52:849-854.

To the Editor: There is more and earlier evidence for sunlight exposure as a risk factor for lens opacities or cataract than was cited in the article by Dr West and colleagues.¹ Hiller et al,² in an analysis of the population-based 1971-1972 National Health and Nutrition Examination Survey (NHANES), found an association between lifetime exposure to UV-B radiation in sunlight (estimated from average annual daily UV-B counts at examination sites provided by the US National Oceanic and Atmospheric Administration) and cortical cataract. In the first large-scale case-control study of risk factors for age-related cataract, Mohan et al³ found a significant association between lifetime exposure to sunlight (as estimated from residence-specific records of average cloud cover) and nuclear, cortical, and posterior subcapsular (PSC) lens opacities. Subsequently, other studies, which were to varying degrees less ecological and more refined in individual exposure estimation, have reported similar although not always

consistent findings. Methods for assessing lens opacities and ocular exposure to sunlight have become increasingly objective and sophisticated. West et al, applying standardized methods for lens photography and grading together with their state-of-the-art modeling of ocular sunlight exposure, have confirmed with more credibility the emerging consensus that prevalent cortical lens opacity is associated with UV-B. Nuclear opacity and PSC lens opacities showed no association, although this study like most preceding it—Mohan et al³ being a notable exception—is underpowered to address PSC lens opacity, which is relatively rare in population-based studies.

Is the cup half empty or half full? The good news is that the risk of cataract from sunlight exposure in the general population may be modest and possibly only for cortical opacities. Cortical cataract is rarely the primary reason for cataract surgery. The story on PSC lens opacities is less clear until addressed by a study adequate in both numbers and methodology. Use of methods to avoid ocular exposure to sunlight may be prudent but of low priority for public health programs until the story is more complete and accompanied by greater risk.

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1. West SK, Duncan DD, Muñoz B, et al. Sunlight exposure and risk of lens opacities in a population-based study: the Salisbury Eye Evaluation Project. *JAMA*. 1998;280:714-718.
2. Hiller R, Sperduto RD, Ederer F. Epidemiologic associations with nuclear, cortical, and posterior subcapsular cataracts. *Am J Epidemiol*. 1986;124:916-925.
3. Mohan MD, Sperduto RD, Angra SK, et al. India-US case-control study of age-related cataracts. *Arch Ophthalmol*. 1989;107:670-676.

In Reply: Dr Dillon raises an important point about the specificity of the wavelengths that may be cataractogenic. We have investigated the association of cortical cataract with UV-B radiation, because our previous work and that of others suggested an association with UV-B in particular. Moreover, animal studies^{1,2} also suggested that the maximum efficiency for experimental lens opacities was about 300 nm, extending from 295 to 320 nm, which is the UV-B range. However, there is clearly a need for additional work to determine the contribution of UV-A to cataractogenesis. The reasoning for this should not be cast in the framework of the relative amount of the various wavelengths that reach the lens, as far more visible and infrared radiation reaches the lens than either UV-A or UV-B, but few would argue that these wavelengths have photobiological effects. Nor, in our opinion, has the albedo effect actually been shown to be relevant for lens exposure. Rather, the rationale rests with the presence in human lenses of compounds able to absorb UV-A, and that absorption can produce a damaging reaction that contributes to cataractogenesis. Only 1 epidemiological study has examined markers of UV-A exposure and found no association with cataract,³ but clearly more work is indicated.

We chose not to perform an exhaustive review of the ecological associations between sunlight exposure and cataract as this has been reviewed elsewhere,⁴ although we did cite the earlier publication by Hiller et al on the same NHANES data set.⁵ We disagree with Dr Milton's statement that methods to avoid ocu-

lar exposure to sunlight might be prudent but of low priority because of the modest risk. The odds ratio, or relative risk, tells only part of the story of the public health significance of an association. The prevalence of the exposure is also important. Since exposure to UV-B in sunlight is virtually ubiquitous and ways to avoid exposure are simple and inexpensive, it would seem an ideal candidate to add to public health messages about health effects from sun exposure.

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1. Pitts DG, Cullen AP, Hacker PD, Parr WH. *Ocular Ultraviolet Effects From 295 nm to 400 nm in the Rabbit Eye*. Washington, DC: US Dept of Health, Education and Welfare; 1977. NIOSH publication 77-175.
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3. Taylor HR, West SK, Rosenthal FS, et al. Effect of ultraviolet radiation on cataract formation. *N Engl J Med*. 1988;319:1429-1433.
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5. Hiller R, Giacometti L, Yuen K. Sunlight and cataract: an epidemiological investigation. *Am J Epidemiol*. 1977;105:450-459.

Mental Health of Women in Afghanistan

To the Editor: In the article on women's health and human rights in Afghanistan,¹ the authors provided valuable information regarding abuses promulgated by "religious rights" in that country.

However, 3 elements of the report do not ring true. First, the rate of "major depressions" was reported as 97% among their 160 survey subjects. Such high rates of major depressive disorder (MDD) have not been observed among refugees, combat veterans, rape survivors, prisoners of war, or other victimized groups.^{2,3} Depressive symptoms well short of MDD, reported in groups exposed to ongoing social adversity,⁴ should not be confused with MDD. A problem may be use of a symptom checklist "to predict the clinical diagnosis of major depression."

Second, the reported rate of posttraumatic stress disorder (PTSD) in this sample was 42%, based on interview by a "trained health professional." The authors do not clarify whether they were assessing acute or chronic, current or lifetime PTSD. This rate exceeds that observed among combat veterans, rape survivors, and other victimized groups⁵ and is in the range of lifetime (not current) PTSD observed among severely abused prisoners of war.⁶ Definitions, criteria, and data instruments should be specified.

Third, the percentages of families reporting "1 or more family members . . . killed in the wars" is 84%, and the number of "war-related injuries among their families" was 70%. Since the number of war wounded usually bears a ratio of 1 killed to every 3 or 4 wounded, these figures are unusual and warrant explanation. If subjects define their extended kin group as "family," it is conceivable that the "killed in action" remote relatives are better known than the "wounded in action" remote relatives.

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In Reply: Dr Westermeyer suggests that the high response frequencies among Afghan women for depression and PTSD and the number of family members reported killed in war “do not ring true.” The finding that 97% of Afghan women reported symptoms of major depression is indeed striking. However, high rates of depression have been observed in other populations. For example, Carlson and Rosser-Hogan¹ have demonstrated rates of depression of 80% among a random sample of Cambodian refugees who resettled in the United States. Since the participants in our study continue to experience considerable hardships, it does not seem that the rates of depression in our study are so high as to “ring untrue.” The symptom checklist we used to predict MDD was the Hopkins Symptom Checklist-25 (HSCL-25), an instrument that has been demonstrated to be 86% sensitive and 93% specific in identifying the diagnosis of MDD.² We disagree that the depressive symptoms identified by the HSCL-25 fall “well short of MDD.”

Our study clearly defines PTSD according to *Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition* criteria: at least 1 of the 4 intrusive symptoms, 3 of the 7 avoidance symptoms, and 2 of the 6 increased arousal symptoms for a duration of at least 3 months over the past year.³ Rates of PTSD, whether current or lifetime, vary considerably among victimized groups. In fact, the study by Sutker et al cited by Westermeyer reports rates of 70% and 78% for current and lifetime PTSD, respectively, among prisoner of war survivors compared with rates of 18% and 29%, respectively, for combat veterans. The rate of PTSD observed among Afghan women, 42%, refers to current PTSD and is consistent with similar studies on traumatized groups.^{1,4-6}

In our study, the proportion of Afghan women reporting 1 or more family members killed in war (84%) exceeded the number of women who reported war-related injuries (70%). We believe this may be attributable to a combination of factors: (1) death may be more easily recalled than injuries, (2) women may not have reported minor injuries, and (3) the paucity of medical services in Afghanistan may have resulted in increased mortality rates for many injuries. It has been estimated that only 26% of the Afghan population has access to medical services for treatment of common diseases and injuries.⁶

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Time to Clinical Stability for Patients With Community-Acquired Pneumonia

To the Editor: Dr Halm and colleagues¹ assessed the point of stabilization of clinical signs and symptoms of pneumonia as patients recover. However, the critical issue is whether a patient who is being treated for community-acquired pneumonia (CAP) needs to have full resolution of these signs of infection prior to changing to oral antibiotic therapy. To my knowledge, no study has proven that intravenous (IV) therapy is critical to patients' recovery from pneumonia if patients are able to take oral medications.

In our study of CAP in 75 hospitalized veterans, we randomized patients to 2, 5, or 10 days of IV antibiotics, and all patients completed 10 days of antibiotic therapy with IV cefuroxime or oral cefuroxime axetil.² Only patients who remained febrile or experienced clinical deterioration were not changed to oral therapy. No difference in outcomes was observed. As long as the patient can swallow and absorb oral antibiotics, complication rates might be the same, even if oral antibiotics are begun on day 2, as in our study, or after 1 IV dose.

The lung is a highly vascular organ. If there are no signs of parenchymal breakdown, such as necrotizing pneumonia or abscess impeding drug penetration, and the infecting organism is susceptible to the antibiotic, IV therapy may offer little benefit. What is now needed is a prospective randomized study of patients hospitalized with CAP in which patients are converted from IV to oral therapy based on a clear point of clinical stability vs a group converted after a predetermined brief period of IV therapy. I believe the outcomes will be the same, and the 3 to 4 days of potentially unnecessary hospitalization will be saved.

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1. Halm EA, Fine MJ, Marrie TJ, et al. Time to clinical stability in patients hospitalized with community-acquired pneumonia: implications for practice guidelines. *JAMA*. 1998;279:1452-1457.
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In Reply: We agree that the traditional practice of treating patients hospitalized for CAP with 7 to 10 days of IV antibiotics is

based on custom, not substantive data. While the impetus for early conversion to oral antibiotics has been primarily financial (shortening length of stay), there are other clinical advantages, including lower risk of phlebitis, line sepsis, and fluid overload, as well as earlier mobilization and return to usual activities.

The median duration of IV antibiotics in our observational study was 6 days. However, the total number of days of parenteral therapy is less important to us than the number of potentially unnecessary days. Once patients are clinically stable, they should not need to continue to receive IV antibiotics in the absence of serious bacteremia or metastatic infection. The purpose of our study was to develop a definition of stability that was objective, clinically sensible, and a valid predictor of good outcomes. We found a median delay of 3 days between stabilization and conversion to oral antibiotics; thus, there is ample opportunity for improvement. To further validate this strategy, we are conducting a prospective, randomized controlled trial of an actively implemented practice guideline emphasizing early conversion to oral antibiotics and timely discharge once stability is attained.

Dr Siegel appears interested in "pushing the envelope" further to switch patients to oral antibiotics after a brief course of IV therapy, even if they have not fully stabilized. Several studies indicate that regimens of 2 to 5 days of IV therapy appear to be effective and less expensive.¹⁻³ Earlier change to oral antibiotics may be appropriate in low-severity, uncomplicated patients for whom the baseline risk of an adverse outcome is quite low. However, many of these studies had methodologic limitations that should be addressed in future trials. Many studies were small and underpowered to be robust "equivalence" trials. In addition, initial pneumonia severity should be more carefully measured so that the need for hospitalization at all is clear. Our previous work suggests that one third of patients hospitalized for pneumonia are low risk and can be safely treated as outpatients.^{4,5}

Finally, we remain wary of "one size fits all" clinical algorithms that prespecify days that certain decisions should occur; for example, all patients should be switched to oral therapy after 2 days. The sickest patients in our study (Pneumonia Severity Index risk class V) did not become clinically stable for a median of 7 days. Programmatic discontinuation of IV antibiotics in this subset of patients after 2 days would have been inappropriate in most cases. We advocate basing the antibiotic conversion (and discharge) decision on objective, validated stability criteria, but we would welcome well-designed trials comparing alternative strategies.

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tive randomized study of inpatient IV antibiotics for community-acquired pneumonia: the optimal duration of therapy. *Chest.* 1996;110:965-971.

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Hospitalizations for Dog Bite Injuries

To the Editor: Although the recent report by Mr Weiss and colleagues¹ provided a national estimate of 333 687 dog bite injuries treated in emergency departments (EDs) each year and other work has estimated approximately 17 deaths annually from dog bites,² we know of no national estimates of hospitalizations for dog bite injuries. The Health Care Utilization Project (HCUP) database maintained by the Agency for Health Care Policy and Research contains standardized hospital discharge summaries (including diagnoses, external causes of injury, and total hospitalization charges) for all hospital discharges from 904 sampled hospitals in 17 states. When weighted, the data provide national estimates.

From HCUP, we identified records with mention of E-906.0 (the *International Classification of Diseases, Ninth Revision* external cause-of-injury code for dog bite) and then estimated 5991 hospital discharges for dog bite injuries in 1994. Discharge rates per 100 000 appeared inversely related to age: 0- to 4-year-olds, 5.0; 5- to 9-year-olds, 4.9; 10- to 14-year-olds, 2.6; 15- to 19-year-olds, 1.2; 20- to 39-year-olds, 1.6; and those aged 40 years or older, 2.0. Those injured were male in 55% of the cases. Length of stay on average was 3.6 days and was longer for older persons (2.7 days for 0- to 4-year-olds compared with 4.7 days for those aged 40 years or older). Hospital charges for persons with dog bites totaled \$40.5 million. Mean hospital charges were higher at extremes of the age range (\$6369 for 0- to 4-year-olds, \$4622 for 15- to 19-year-olds, \$6842 for those aged 40 years or older).

These hospital charges do not include charges for physician services or subsequent postdischarge care. Inpatient physician fees are estimated as an additional 25% of hospital charges,³ and the sum of hospital charges and inpatient physician fees represents only 81% of the total charges incurred during the year following discharge for a traumatic injury.⁴ Thus, we estimate \$62.5 million in charges related to hospitalizations. Combining this figure with the estimate made by Weiss et al of \$102.4 million for ED visits,¹ direct medical care charges for dog bites are estimated at \$164.9 million. Moreover, direct costs represent only 65% to 70% of the total costs of injury⁴ (ie, total costs of \$235.6 million-\$253.7 million). This total is based on the HCUP estimate of 5991 hospitalizations for dog bite-related injuries. However, figures from the report by Weiss et al suggest about 13 000 hospitalizations for dog bites.¹ Thus, our numbers may well be underestimates.

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The human suffering related to the 17 deaths, nearly 6000 hospitalizations, and more than 330 000 ED visits a year and the associated economic impacts justify further efforts to prevent dog bites.

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This letter was shown to Mr Weiss, who declined to reply.—ED.

Treatment of Smokeless Tobacco Addiction With Bupropion and Behavior Modification

To the Editor: An estimated 6.9 million people in the United States use smokeless tobacco, such as chewing tobacco or snuff. The health risks associated with smokeless tobacco use include increased rates of oropharyngeal cancer and increased subsequent cigarette smoking.¹ Despite the widespread use of smokeless tobacco, relatively few data have appeared in the literature regarding treatment of addiction to it.^{2,3} We describe a case of successful treatment of smokeless tobacco use with an approach that combined pharmacotherapy and behavior modification.

Report of a Case. A 31-year-old man had an 11-year history of using 1 can per day of smokeless tobacco and denied any history of smoking. The patient previously had made several attempts to stop use of smokeless tobacco with nicotine patches and abrupt cessation but had only limited success for a short time. He agreed to a trial of bupropion hydrochloride and a 4-week course of behavior modification. These sessions covered effective withdrawal strategies, coping skills for cravings, initial tobacco cessation, and extended maintenance skills. During the first session the patient was asked to set a quit date that would occur while he was in the group. The patient started treatment with bupropion hydrochloride (150 mg twice daily) 1 week prior to group treatment. After approximately 1 week of taking medication, the patient noted a reduction in cravings for smokeless tobacco, and at 5 weeks he was tobacco free. He noted few adverse effects associated with the medication but reported a change in the taste of the smokeless tobacco as the most prominent effect. After taking bupropion for approximately 3 days, the patient described the smokeless tobacco as "tasting terrible," and he felt the poor taste was 1 factor in becoming tobacco free. He also felt the

copied skills learned in the group allowed him to withstand tobacco cravings and avoid relapse. The medication was continued for a total of 10 weeks, and the patient had no difficulties and did not experience any withdrawal symptoms (eg, irritability, anxiety, headaches, dizziness) after its discontinuation. The patient was followed up at 1, 3, and 6 months and remains tobacco free at 8 months.

Comment. Recent research suggests a 2-pronged approach to tobacco cessation with use of both pharmacologic treatment and behavior modification provides maximum efficacy for tobacco cessation.⁴ Bupropion is considered a first-line therapy for smoking cessation.⁵ However, to our knowledge, use of bupropion for treatment in smokeless tobacco addiction has not been reported previously. The behavior modification used a psychoeducational format to develop effective coping strategies for tobacco cessation. These classes have been shown to significantly increase rates of tobacco cessation.⁴ Further studies are needed with a larger population to quantify the efficacy of bupropion hydrochloride and behavior modification in the treatment of nicotine addiction caused by smokeless tobacco.

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Disclaimer: Conclusions and opinions expressed are those of the authors and do not necessarily reflect the position or policy of the US government, Department of Defense, Department of the Army, US Army Medical Command, or the 82D Airborne Division.

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CORRECTIONS

Incorrect Web Site Address: In the Consensus Statement entitled "The Urgent Need to Reform Health Care Quality: Institute of Medicine National Roundtable on Health Care Quality," published in the September 16, 1998, issue of THE JOURNAL (1998;280:1000-1005), there was an incorrect Web site address. On page 1000, in the last sentence of the first paragraph of text, the Web site address should have read "http://www2.nas.edu/hcs/."

Incorrect Reference: In the Editorial entitled "Low-Tech Autopsies in the Era of High-Tech Medicine: Continued Value for Quality Assurance and Patient Safety," published in the October 14, 1998, issue of THE JOURNAL (1998;280:1273-1274), there was an incorrect reference. On page 1274, reference 10 should have read "Nichols L, Aronica P, Babe C. Are autopsies obsolete? *Am J Clin Pathol*. 1998; 110:210-218."