Letters to the Editor

The Journal welcomes letters to the editor. If found suitable, they will be published as space allows. Letters should be typed double-spaced, should not exceed 400 words, and are subject to abridgment and other editorial changes in accordance with Journal style. All letters that reference a recently published Journal article are sent to the original authors for their reply. If no reply is published, the authors have not responded by date of publication. Send letters to Paul M. Fischer, Editor, The Journal of Family Practice, 519 Pleasant Home Rd, Suite A-3, Augusta, GA 30907-3500, or Fax (706) 855-1107.

TOBACCO AND DRUG USE

To the Editor:

Patients who use tobacco but want to quit face many barriers, including the marketing activities of tobacco companies, socioeconomic factors, and the degree of nicotine dependence.[1] We are concerned about another potential barrier to tobacco cessation that is under development: the direct-to-consumer marketing of prescription nicotine medications. Recently, we noticed promotions in local newspapers and television for a prescription-strength nicotine gum (Nicorette, Marion Merrell Dow). These promotions are part of a Food and Drug Administration (FDA)–approved multicenter clinical trial of 2500 patients to assess over a 12-week period the feasibility of making this prescription-only gum available as an over-the-counter (OTC) product. To gain entry into the study in our community, the advertisements provided a telephone number of a local pharmacy to call. If the patient arrived at the retail pharmacy in Chapel Hill, NC, that served as a center for the trial, details of this trial were provided.

After informed consent was obtained, enrolled patients were able to select the dosage of nicotine gum to be used (2 or 4 mg strength) based on their smoking habits. In addition to the nicotine gum, patients would receive a user's guide and an audiotape on smoking cessation. If requested, a telephone number for a local smoking cessation support group was provided. The study was closed in this area after 1 to 2 weeks because of the large number of inquiries.

The FDA has removed several OTC smoking cessation aids (eg, lobeline) from the market because they have no proven efficacy. In addition, smoking cessation programs consisting primarily or solely of pharmacologic interventions (in this case, nicotine gum) have proved to have poor long-term success rates.[2,3] It is doubtful that the FDA would approve an antihypertensive agent with only a 16% rate of successful response. A potential rationale for the current study is that the patent status for nicotine gum will expire in the near future, and since the gum has not been successfully utilized by most health care professionals, perhaps the marketers of the product feel that they will be more successful in their direct-to-the-public campaigns.

We are concerned about attempts to move nicotine gum from prescription to OTC status for several reasons: (1) the largely unrestricted access of nicotine-containing smoking cessation products to minors provides another route of entry into the use or sustained use of tobacco products, particularly for teenage girls who are concerned with weight control, as the gum has been used to control increased appetite in people who stop smoking; (2) unrestricted access of nicotine-containing products designed for smoking cessation to patients with cardiac conditions such as hypertension or arrhythmias without the intervention of a health care professional (ie, physician or pharmacist) is potentially dangerous; (3) there is no mechanism to ensure that nicotine gum users will receive appropriate counseling and follow-up, two ingredients critical to successful tobacco use cessation; (4) OTC nicotine gum would be readily available to people who are chemically dependent on nicotine, have no desire to quit, and wish to use a substitute for a tobacco product in public places where smoking is not allowed, thus sustaining tobacco use; and (5) nicotine gum has had poor success in practice, and no theoretical basis exists to suggest that it will work with less oversight as an OTC product. Are patients enrolling in such studies truly being given informed consent by being told about the many clinical studies that show a high likelihood of failing to quit tobacco use with the use of this gum? Independent of the potential benefits that might be obtained, all of these factors mitigate against the unrestricted access to such products.

If patients consider using a potential product after encountering direct-to-consumer advertising, this may provide the misguided impression that a "prescription-strength" product, whether currently available via prescription or OTC, will confer an additive therapeutic benefit without adverse events. With tobacco use, greater success in "kicking the habit" comes from clear patient education and support programs, not product-focused services. As is true with most other commercial products, the phrase *caveat emptor* is still the best advice.

Timothy J. Ives, PharmD, MPH
Adam O. Goldstein, MD
University of North Carolina
Chapel Hill, North Carolina

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SCREENING SIGMOIDOSCOPIES

To the Editor:

I am responding to an article about screening flexible sigmoidoscopy in a low-risk, highly screened population *(Sakamoto MS, Hara JH, Schlumpberger JM. Screening flexible sigmoidoscopy in a low-risk, highly screened population. J Fam Pract 1994; 38:245–8)*. I would like to pose a question to the authors, and indeed, to every medical practioner who does screening flexible sigmoidoscopies. That is, how many of you wish to be screened by a test that reveals, at most, 80% of the known pathology?

In reviewing the concept of flexible sigmoidoscopy, I believe it should be done only on people who have had prior total colonoscopies or contrast barium enemas. When the disease is known to exist only within the limits of the flexible sigmoidoscope, then by all means, use that to follow disease. However, I don't see any reason to use flexible sigmoidoscopy as a screening tool. Upon review of the article, I found that the data in Table 2 of this article indicate that there was a total of 16 additional lesions found by colonoscopy over the total found by sigmoidoscopy. That figure alone should

deter anyone from limiting this screening examination of the colon to sigmoidoscopy.

I am a general practioner and have been in practice for 23 years.

Clinton Faber, MD
Hiway Medical Center
Reedsport, Oregon

The preceding letter was referred to Drs Sakamoto, Hara, and Schlumpberger, who respond as follows:

We would like to thank Dr Faber for his letter, which focuses the discussion on colorectal cancer screening in an important direction.

Our recent article[1] reports that repeat screening flexible sigmoidoscopies may not be indicated in persons with a previous history of negative screening flexible sigmoidoscopy. We did not mean by this that the best screening examination for colorectal cancer is necessarily flexible sigmoidoscopy. In fact, colonoscopy may be the most appropriate screening examination for colorectal cancer. Screening colonoscopy has been shown to have a higher yield as compared with screening flexible sigmoidoscopy.[2,3] Recent studies certainly suggest that fecal occult blood test screening is also not an ideal screening test.[4,5]

A single initial screening colonoscopy has been proposed as the most appropriate screening strategy.[6] We agree that colonoscopy is certainly the most direct screening method for colorectal cancer, but it remains to be determined if colonoscopy is the most cost-effective screening method as well.

Milton S. Sakamoto, MD
Jimmy H. Hara, MD
Jay M. Schlumpberger, MD
Kaiser Permanente Los Angeles Medical Center
Los Angeles, California

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PSA SCREENING

To the Editor:

We would like to make readers aware of recently published data that significantly affect the likelihood of risks associated with radical prostatectomy, and therefore, the utility of prostate-specific antigen (PSA) screening for asymptomatic prostate cancer. Fowler et al[1] estimated the probabilities of complications of radical prostatectomy by interviewing a representative nationwide random sample of Medicare patients (65 years of age and older) who had undergone a radical prostatectomy 2 to 4 years earlier. The authors point out that the results are likely to be more representative of the experience of Medicare patients throughout the country than previously published "best" results from selected institutions. They also caution that the worst outcome, early treatment-related death, is not represented in their sample.

Impotence. Although 91% of patients said that they had been able to have erections to at least some extent before radical prostatectomy, 61% said they have had *no* partial or full erections since surgery. Only 11% said they had any erections firm enough for intercourse during the month before the survey. Overall, 15% of the sample reported that they had had some kind of treatment for sexual function, but only 28% of this treated group reported success (erections firm enough for intercourse in the past month). We also should remind readers that about 2% of men over 50 develop erectile problems each year, even when they have not undergone prostatectomy.

Incontinence. For the sample as a whole, only 37% said they had no current problem with wetness, 31% said they wear pads, adult diapers, or a clamp (mostly pads) to deal with wetness, and 6% had surgery after the radical prostatectomy to treat incontinence.

Urethral stricture. Twenty percent reported having had postsurgical treatment for urethral strictures.

We recently proposed suggested patient information for PSA screening for asymptomatic prostate cancer.[2] A table was included in our proposed patient handout that gave estimates of risks associated with radical prostatectomy: 20% for impotence, 5% for incontinence, and 10% for urethral stricture. We agree with Fowler et al that our risk estimates, which were derived from a meta-analysis of previously published results from selected institutions, are almost certainly inaccurate representations of the nationwide Medicare experience. We suggest that patient information regarding PSA screening for prostate cancer be revised to include the more accurate estimates provided by Fowler et al.

David L. Hahn, MD
Richard Roberts, MD, JD
Madison, Wisconsin

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2. Hahn DL, Roberts RG. PSA screening for asymptomatic prostate cancer: truth in advertising [editorial]. J Fam Pract 1993; 37: 432–6.

ALLERGY TREATMENT

To the Editor:

Ten minutes ago, I finished reading the article by LaForce and colleagues (*LaForce CF, Dockhorn RJ, Findlay SR, et al. Fluticasone propionate: an effective alternative treatment for seasonal allergic rhinitis in adults and adolescents. J Fam Pract 1994; 38:145–52*) in the February issue. It concluded that fluticasone aqueous nasal spray was an effective alternative to beclomethasone for the treatment of seasonal allergies. I attempted to look that up in the new *Physicians' Desk Reference*, in which it is nowhere to be found. I called three of my local pharmacies to see if they had it, and none of them had even heard of it. I certainly want to be on the cutting edge of medicine, but I'd rather

spend my time reading about a treatment that I can get hold of *now*.

G. S. Mitchell, Jr., MD
Geriatrics Director
Riverside Family Practice Center
Newport News, Virginia

The preceding letter was referred to Dr LaForce, who responds as follows:

When we submitted this paper, we believed that the product would be available during the 1994 spring allergy season. Unfortunately, this did not occur as planned, as there were delays in the Food and Drug Administration's approval process. We are now hopeful that intranasal fluticasone propionate will be approved soon.

Craig LaForce, MD
Carolina Allergy and Asthma Consultants
Raleigh, North Carolina

INTENTIONALLY HASTENING DEATH

To the Editor:

I am alarmed that Dr Dozor was willing to make his agreement to help Keith "die with dignity" during their very first appointment. It seems as though Dr Dozor is confusing his familiarity with AIDS and its myriad complications with what AIDS means to this patient at this time in his life. I believe that such dramatic agreements best evolve out of a long-standing doctor-patient relationship.

It is also striking that Keith shows up for this first appointment with a very involved partner and a lawyer. How convenient! But what do I do about the patient who comes in with a very messy domestic life and an ambivalent spouse, or worse yet, a person who is legally incompetent as a result of the current disease or mental retardation. What then? Who is to speak for these patients and tell us in detail their wishes? It is very seductive to use people like Keith as examples of how neat and tidy end-of-life issues can work, but we must not be oblivious to the needs of the majority of patients who will be in less well-defined positions. It is the role of the physician to protect these patients and their rights.

Lastly, I am struck by Dr Dozor's sense of the family's relief that "we" had "let out a toxic secret—death." I wonder why he is projecting his own issues with death on this family. When I have been in these situations and the scales finally fall from my eyes and I realize that a patient is dying, the family is not relieved because we can speak of "it" (ie, death), but because they know that I understand what they and the patient have often known for some time—that the patient is dying and therapeutic goals need to be reevaluated. And above all, the patient needs to be reassured that I won't abandon him or her because he or she is no longer "on protocol" or a challenge to "save" or interesting. Given this understanding, I wonder if Dr Dozor would have found Keith and his dying as interesting if he had chosen continued aggressive therapy or were embittered about his dying. He notes that "our agendas were mutual and I believed I could provide care that would be 'special' to them and meaningful for me." It is the role of the physician to treat the patient in a way that respects the meaning that the patient assigns to his dying—even if that is not the same meaning that the physician would like to assign to Death.

It seems appropriate for Dr Dozor to write of these matters "to resolve his cognitive dissonance," but I think these thoughts are better reserved for a private journal than for *The Journal of Family Practice*.

Patrick B. Herson, MD
St. Cloud, Minnesota

To the Editor:

Dr Dozor's worry that he somehow became a murderer because he indirectly shortened a patient's life in order to relieve the agony of a dying patient[1] is a good example of the increasing confusion of clinicians engendered not only by our increased technology but also by the culture wars in medical ethics.

In traditional ethics, the principle of "double effect" says that although one cannot perform a bad deed in order to cause a good effect, one is permitted to perform a good deed in order to cause a good effect, even though an undesired bad side effect may occur.

Therefore, Dr Dozor's actions are ethical, since "In omission, no human agent causes the patient's death . . . he dies his own death from causes that it is no longer merciful or reasonable to fight by means of possible medical intervention."[2]

Some modern ethicists, such as Rachels[3] and Brody,[4] argue against this subtle moral distinction. If I might be cynical, I wonder if they do so not to "place fences around the law," but to ultimately destroy the safety rails of our Hippocratic and moral traditions in order to promote a much broader social agenda.[5]

Actions such as those described by Dr Dozor are common. If we can convince physicians that limiting medical intervention that would only prolong dying is the same as direct killing, we will weaken the taboo against killing, and ultimately there will be less opposition to direct killing. Indeed, in a recent article, Brody does just this. As for Rachels, he not only defends euthanasia in some circumstances as ethical but also is on record as defining a "person" so that those who are brain-damaged would, like the fetus, no longer be under the protection of law.[6]

As the old song says, one "need not be a weatherman to see which way the wind is blowing."

N. K. O'Connor, MD
Nanty Glo, Pennsylvania

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To the Editor:

As a practicing physician, I believe that the ethicist's advice to Dr Dozor ("Intentionally Hastening Death") was terribly wrong. The patient clearly had a fatal disease. He was dying a respiratory death by all clinical criteria. Heroic measures, respirators and the like, were clearly not wanted by the patient, family, or friends. Dr Dozor's terminal care decisions were most certainly correct, in my opinion. There is a huge moral and medical difference, I believe, between actively administering a fatal substance, as so publicly promoted by Dr Kevorkian, and the

alternative compassionate withdrawal of fruitless medical support. "Comfort measures only," as in Dr Dozor's use of morphine, is a concept and tradition that is so appropriate and helpful at crisis times such as these.

Dr Dozor's only mistake, it seems to me, was in giving his ethicist's opinion any credence at all. He did not "intentionally hasten death." He simply bowed to death as the inevitable fate of us all. He supported his patient with diligence and compassion as good physicians have always done.

Greg A. Gehred, MD
Santa Fe, New Mexico

To the Editor:

It is time for ethicists and clinicians to have a meaningful dialogue on the issue of "orchestrating death." The dialogue should focus on the ethical aspects of the treatment of the dying patient and should examine such issues as pain and symptom control, terminal weaning, and withdrawal of therapy in the context of the limits of the ethically permissible. The discussion must accept the practical realities of the clinical context of death—that it often occurs by a decision to limit or withhold therapy[1] and that physicians have uncomfortable and ambiguous feelings about their role in this process.[2,3]

Ethicists must be willing to reexamine the principle of double effect as the justification for treatment decisions in terminal care. This should be done in light of specific troublesome scenarios, such as terminal weaning in the conscious patient, as well as to redefine it for physicians who have intellectual and/or psychological difficulty with not wishing the death they know is inevitable.

Both clinicians and ethicists must articulate distinctions between clinically appropriate symptomatic therapy, such as oxygen, narcotics, and anxiolytics, and nonappropriate therapy, such as potassium chloride and pancuronium, so that dying patients are not deprived of symptom-relief by those afraid of ethically inappropriate acts of hastening death.

Finally, we must attempt to clarify both the language by which we characterize acts of terminal care as well as the moral implications we impute to such a characterization. Terms such as "allowing to die," "intentionally hastening death" and "helping to die" carry different implications, both operationally and ethically. Physicians caring for terminal patients often prescribe medications that may hasten death. If appropriate doses of appropriate drugs are given, must the person ordering them also be morally pure in intent? Lumping together acts of appropriate therapy with ambiguous (clinically realistic?) intent, such as that described by Dr Dozor,[4] with those of inappropriate therapy or euthanistic intent, as described by an anonymous physician in JAMA,[5] under the rubric "intentionally hastening death" confuses colleagues (particularly those in training), patients, and families.

The debate about physician assistance in planned death is not aided by broadening the scope of activities described by that term. Nor is the clinical care of the dying patient facilitated by linking often inevitable decisions about symptom relief to concepts such as "intentionally hastening death."

If we do not clarify our language to distinguish between acts that cross the line of accepted treatment to actively assist in a patient's death, such as Dr Quill described in *The New England Journal of Medicine*,[6] and acts that lie within the boundaries of acceptable terminal care, as described by Dr Dozor,[4] then we run the risk of continuing our current practice of undertreating terminally ill patients[7] for fear of hastening their deaths.

Denise A. Niemira, MD
Wichita Falls, Texas

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To the Editor:

It was with much interest that I read the article "Intentionally Hastening Death" in the March issue (*Dozor RB. Intentionally hastening death. J Fam Pract 1994; 38:295–7*). As a much older physician soon to retire from the practice of medicine, and also having been intimately involved with a large number of hospice and nursing home patients over the last several years of my career, I readily sympathize with the author's feelings.

I was greatly helped in my early days in approaching death by a statement made by Dr Eben Alexander, Professor of Neurosurgery, Bowman Gray School of Medicine: "As a physician you have every obligation to prolong life, but remember, as a physician, you have no right to prolong death." Over the years, I have used this as a means of considerable help in my approach to the many patients I have seen who were approaching death.

I strongly feel that many times, we are prolonging death rather than prolonging life in the activities in which we now engage in advanced medical care. As was well pointed out in this article, this young man was certainly in the process of dying. To have prolonged this process with the many heroic measures that medicine can provide would have been no service and would certainly have not been a prolongation of life in my view. Too often we have equated life with respiration and heartbeat rather than the fullness of being able to respond to the environment around us, respond to our friends and neighbors, respond to our loved ones, and both give and take in all that is a definite part of life from its very beginning.

I would encourage Dr Dozor to continue to provide solace, comfort, and the necessary medical means of that comfort with his patients in the future.

Morris E. Powell, MD
Bullhead City, Arizona

The preceding letters were referred to Dr Dozor, who responds as follows:

I appreciate the mostly kind and uniformly perceptive comments on Keith's story.[1] A majority of the letters came directly to my office, all from physicians, none of whom criticized the morality of my actions, except one doctor who felt I had mildly prolonged Keith's dying. Dr Herson is alarmed and concerned that I am pushing a pro-death agenda; nonetheless, the kinds of actions I took in this case are evidently quite common. With this kind of support, I think that I can now articulate a less defensive, more affir-

mative account of helping a patient die well. What is needed is an unflinching look at the human process of dying and what it means to all concerned.

Dr O'Connor quite correctly highlights the role of "double effect," characterizing it as "the safety rails of our Hippocratic and moral traditions," but I would reframe the problem completely. Traditional bioethics poses the problem of terminal care as an attempt to mitigate the suffering engendered in keeping a dying person alive, thereby balancing two prime directives of medical care—treating suffering and maintaining life. The principle of "double effect" permits both agendas in principle, but exacerbates the tension. "Double effect" also seems like a deliberate self-delusion, as if we can pretend we do not know the effect of our actions and do not intend to help patients die. The real dilemma is the conflict between the affirmation of life represented by fighting death, and the affirmation of death implied by the notion of the Good Death.[2]

It certainly does not seem safe—at least in the medical subculture—to affirm death, yet the Good Death is a powerful and ancient cultural reality.[3–6] I felt very vulnerable during Keith's last days and later, because I was torn between affirming his death and fighting it; I was a microcosm of Dr O'Connor's "culture wars in bioethics." Dr Gehred's support means so much to me, because I have internalized the ethicist's voice. Ultimately, I feel that Keith's death was a good one—meaningful to him, his family, friends, other health care professionals, and me. I intentionally hastened his death, thus violating the principle of "double effect," offending the ethicist, Dr Herson, and others because another value, the Good Death, had overridden the "no killing" value. Though I crossed over a moral safety rail, did I venture onto the same slippery moral slope as Dr Kevorkian? I think the difference is that I was Keith's doctor in trying to keep him alive as well as in helping him die well. I was not a technician providing an escape from an intolerable life. The difference is that I do not believe that having a particular diagnosis means someone is dying. Most AIDS patients are not dying of AIDS, they are living with AIDS. At some point, something happens, a corner is turned, and death comes into clear sight. Dying can be a meaningful process even then, or perhaps it is more likely to become a meaningful process then.

I enthusiastically endorse Dr Niemira's proposal that "it is time for ethicists and clinicians to have a meaningful dialogue on the issue of 'orchestrating death.' " The decisive issue then becomes recognizing dying.[7] The turning point in my care of Keith occurred when I recognized, albeit belatedly, that he was dying. Once I recognized it, I could stop deluding myself with "double effect" and intentionally orchestrate a Good Death.

I would like to clarify Keith's story in response to Dr Herson's comments. "Intentionally Hastening Death"[1] was not a confession, since it was addressed to my colleagues and not a priest. Clearly this narrative has touched a nerve in many family doctors and is therefore vastly more appropriate for *The Journal of Family Practice* than for my diary. For Keith to "die with dignity" was his agenda on day one. Since this was so congruent with my own ideas about a good death, I agreed easily. Having the two primary health care agents of his Durable Power of Attorney for Health Care present at the first visit certainly set the tone for what followed. I did not project my issue about death on this family. Keith's mother came to my office a few days after his death and volunteered to me how much better they all felt after I had asked Keith how he felt about dying. I do not think that all patients or all families can hear or should be subjected to such a dramatic question, but I was deeply immersed in this particular family and knew it was the right thing to say. It is usually much more difficult or impossible for me to "intentionally hasten death" or "orchestrate a Good Death." I have been involved in many bad deaths. I feel I have learned from this case that making the Good Death an open and conscious value as a family physician may help me help my dying patients better in the future.

I appreciate Dr Powell's expression of solidarity. Dying is part of life, and the Good Death is actually an expression of the sanctity of life.

Robert B. Dozor, MD
Calistoga, California

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PAIN MEDICATION

To the Editor:

Most family practitioners have many pain patients. My pain patients are divided into two main groups: those with "personal gain [eg, patients involved in accident-related litigation]" and those with "no personal gain."

"Gain" patients do not respond to any therapy, and their prognosis is usually grim. I feel that if you are going to do a study or true analysis of therapy, drug or otherwise, the only valid route is the "no personal gain of any kind" patients.

I have tried tricyclic antidepressants of the Elavil class with great success in my well-motivated "no gain" patients. It is a safe, inexpensive, effective, and nonaddicting therapy.

Robert A. Kopecki, DO
Wilmington, Delaware

To the Editor:

I read with interest the article by Turner and Denny[1] and the letter to the editor by King and Sengstaken.[2] It appears to me that this is a classic example of "When you ain't got nothing else, you use what you got," ie, our armamentarium in chronic pain management is limited. We would all like a "magic bullet" that could ease chronic pain without resorting to narcotics.

Antidepressants are a logical alternative, since there is a frequent overlap of depression and chronic pain. In my experience, many pain patients achieve remarkable results with low-dose antidepressants. Tricyclic antidepressants (primarily amitriptyline) are well tolerated by the vast majority of patients, particularly if started at a low dose (10 or 25 mg) and increased slowly (25 mg every 1

to 2 weeks). Perhaps this success is due to the sedative properties causing improved sleep and therefore improved outlook, perhaps it is because I am treating subclinical depression, or perhaps it is all placebo effect.

I have the utmost respect for the research community and believe it is important for us to objectively evaluate treatments. However, as one of the "guys in the trenches," I am stuck treating patients. If I can give a trial of a nonaddictive, fairly well-tolerated, inexpensive (generic amitriptyline costs about $5 to $10 a month) treatment modality that may improve my patient's outlook, it would seem cruel not to do so, particularly when the alternative is not to treat at all. When it comes to helping patients, I'll take placebo effect if I can get it, at least until we can find a more viable alternative.

Wayne S. Strouse, MD
Kingsport Family Practice
Kingsport, Tennessee

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The preceding letters were referred to Dr Turner, who chose not to respond.

PRENATAL SMOKING CESSATION

To the Editor:

I would like to congratulate you on the articles in the March issue of *The Journal of Family Practice* relating to children and passive smoking,[1] and smoking and preterm birth and low birthweight.[2]

As a medical director for an HMO, I know that managed care has a great opportunity to have an impact on this problem because the patient does not have any added costs if she accesses obstetrical care early and smoking cessation programs are offered as part of her benefits.

At Maxicare, we have developed a serialized self-help smoking cessation program for pregnant women.[3] We try to identify all smokers at the beginning of their pregnancies.

Pregnant patients are reported by the Independent Physicians' Association and medical groups to Maxicare when they first consult for prenatal care. The women are sent a variety of informational items at each trimester. For those who state that they are smokers, we send eight weekly issues of a smoking cessation booklet. These are attractive, helpful, and easy to understand. A population-based randomized clinical trial that tested the effectiveness of the program was undertaken. The results showed that 22.2% of the women in the 8-week series quit as compared with 8.6% of the controls. Maxicare would be happy to share this program with any interested parties.

The increased risks of maternal smoking include low birthweight, prematurity, spontaneous abortion, perinatal mortality, sudden infant death syndrome, and long-term neurotoxicity affecting neurobehavioral development.[2,4] We know that our efforts will not only result in better health for our children but also have a significant effect on immediate and future costs.

William S. Weil, MD
Vice President and Medical Director
Maxicare
Los Angeles, California

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ASTHMA MANAGEMENT

To the Editor:

I am in full agreement with Barach[1] that outpatient management of acute exacerbations of asthma should include objective measurements of airway obstruction. As Barach states, hand-held peak expiratory flow rate (PEFR) meters are readily available, inexpensive, and easy to use, and they contribute important quantitative information to the decision whether to hospitalize. In my opinion, every site that manages acute asthma should measure PEFR during acute exacerbations of asthma. Regarding chronic asthma, I would like to share some of my perspectives on spirometry, education, and rising prevalence, which may differ somewhat from those of Barach.

My experience after performing a large number of tests is that patient compliance is a minor problem in obtaining reproducible spirograms (ATS criteria) in older children and adults. Serial measurements of FVC, FEV_1, and midflow rates (FEF 25%–75%) are more sensitive indicators of pulmonary function than PEFR, and are valuable for primary care clinical research on obstructive airway disease.[2] My experience has been that serial spirometry is also feasible in the clinical management of individual patients. In a survey of 240 family physicians belonging to the Wisconsin Research Network (WReN), 119 (49.5%) reported the presence of a spirometer in the practice and presumably use spirometry at least to some extent. I agree that excessive charges for spirometry may be a limiting factor.

Barach makes the point that family physicians are in a unique position to provide ongoing education to patients with asthma. That home peak flow monitoring will improve asthma outcomes is an attractive but unproven hypothesis, and there is evidence that a simple diary is just as effective in identifying exacerbations.[3] "Brief" (3 hours) education to improve inhaler skills and adjust drug dosages according to a treatment plan can decrease hospital admissions and emergency department visits.[4] My beliefs are that primary care asthma education teams, adequate compensation for them, and research on their effectiveness will be necessary before the promise of decreased asthma morbidity will be realized in this country. It is sad that almost all published primary care asthma outcomes research has been performed outside the United States.

Like Barach, I believe that better clinical management can decrease asthma morbidity, although I am uncertain whether better clinical management will affect mortality as much as morbidity, because mortality may be significantly influenced by sociocultural factors beyond the reach of our clinical skills. Unlike Barach, I do not believe that asthma prevalence can be influenced by currently recom-

mended therapies, which are simply palliative and cannot be expected to affect possible underlying causes of asthma.[5]

David L. Hahn, MD
Madison, Wisconsin

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