LETTERS TO THE EDITOR

ETHICS IN MANAGED CARE SETTING

To the Editor:

Recent articles on ethics in the managed care setting have caught my attention. I am happy to see many family physicians taking a stand as patient advocates in the face of what at times seem overwhelming pressures to fit either government or corporate "systems" where the dollar reigns supreme.

While I am not an advocate of "rationing," we must admit that rationing *has* always occurred and will always occur because in this world there are limited resources to deal with unlimited needs. However, since rationing is such a distasteful concept to those of us who want to help all of our patients, we unfortunately allow the definition of "health" and "medical necessity" to float to suit the political and economic realities of the particular system we are working in. That provides homeostasis for our psyche, but at the expense of a clear vision of truth.

Sooner or later it will be helpful for the medical profession to carefully define what its independent standards are. Articles on ethics often use somewhat vague expressions such as "minimum professional standards" and "quality of care" *without defining* these terms. An older physician mentor of mine has said, "Our society has a unique penchant for solving its problems by redefining its words." Now is the time for *great care* in word usage.

As I work with patients as a family physician in the outpatient office, nursing home, and hospital, I use the following guidelines to help me make decisions regarding "quality of care":

1. Honesty: keep all of the cards on the table in your conversations with patients. In addition to diagnostic and treatment information traditionally given, inform them of the economic and political dimensions of their particular health care system.

2. Do unto others as you would have them do unto you (some old rules are hard to improve upon).

3. Work to restore *maximal function* in the context of *this* patient's "life story."

4. Respect the sanctity of human life.

5. Help the patient get well and stay well (functioning as independently from the doctor as possible).

These little phrases that run through my mind over and over again may seem simplistic, but have been a big help to me in sorting through the multiple, difficult ethical dilemmas of our current health care environment.

Philip D. Ranheim, MD
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ONYCHOMYCOSIS

To the Editor:

The onychomycosis article (*Zaias N, Glick B, Rebell G. Diagnosing and treating onychomycosis. J Fam Pract 1996; 42:513-8)*) was interesting, but I am still concerned about relapse. The authors state: "Relapse rates appear low with oral terbinafine. In one multicenter study, 90.9% of infected toenails remained cured 6 months after treatment." What about experience 2 and 5 years out? In my experience with standard therapy, recurrence is common. How do we know that this will not happen with the newer drugs? I remain skeptical about such therapy. Should I loosen up?

Henry Domke, MD
Family Care Associates
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The preceding letter was referred to Dr Zaias, who responds as follows:

Onychomycosis is a hereditary predisposition to grow *Trichophyton rubrum* on the skin and nail bed. Any drug will cure the present episode, after which the patient will again acquire the disease, if presented with the agent and the right set of circumstances.

Nardo Zaias, MD
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APPROPRIATENESS OF ANTIBIOTICS

To the Editor:

I am writing regarding the article "Antibiotics and Upper Respiratory Infection" by Mainous et al (*Mainous AG, Hueston WJ, Clark JR. J Fam Pract 1996; 42:357-61)*. Being in a busy primary care practice, I have a strong suspicion that the data on which results and conclusions were drawn were flawed. In most primary care offices the form used for billing and for noting the diagnosis code is not all-inclusive of either procedures or diagnoses. Very commonly, in selecting a diagnosis for a relatively simple problem, I will mark the one that is most accessible on the page (ie, URI in place of Sinusitis or Bronchitis).

Unless a reviewer looks at the details of a progress note in the

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patient's chart, there is no way to determine the appropriateness of antibiotics. From my perspective, research based on insurance claim data reminds me of that old computer saying, "garbage in, garbage out." Certainly without doing my own study, I cannot prove that the evaluation and conclusions are flawed, but I suspect that the majority of physicians are a whole lot smarter than one would assume based on the conclusions of this research.

Jeffrey M. Keegan, MD
Richmond, Virginia

The preceding letter was referred to Drs Mainous and Hueston, who respond as follows:

Dr Keegan makes an important point in his comment on our study. The use of preprinted billing forms may result in some misclassification of conditions in any administrative database. For just that reason, we chose the most unequivocal diagnosis code (acute nasopharyngitis) possible for an investigation of treatment patterns for the "common cold." It seemed unlikely that individuals were coded as a "common cold" when, in fact, their conditions were actually diagnosed as bronchitis or sinusitis. The work of Vinson and Lutz[1] would suggest exactly the opposite—individuals with colds would be likely to be coded as having a more anatomically focal upper respiratory infection that would suggest antibiotic treatment. Further, since the data are claims paid by Medicaid, a certain degree of validity is expected, since Medicaid does not pay every claim and audits claims. Finally, although our data are based on Medicaid claims, self-report surveys have shown similar patterns of high rates of antibiotic prescribing for upper respiratory infections.[2]

Dr Keegan is right to be alarmed about this practice pattern. Contrary to his perception, we did not intend to imply that physicians were ignorant about the lack of effectiveness of antibiotics for URIs. We recognize that the decision to prescribe antibiotics is a complex process often influenced, as Vinson and Lutz demonstrated, by patient expectations of treatment. However, with the growth in antibiotic-resistant bacteria and concerns about rising medical care costs, physicians should recognize that antibiotic prescribing behaviors are going to be viewed with greater scrutiny.

Arch G. Mainous III, PhD
William J. Hueston, MD
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STATE MEDICAL BOARDS' POSITION ON CHAPERONES

To the Editor:

There is a paucity of information regarding policies on the use of chaperones.[1] We felt it would be of interest to ask the state medical boards what they currently recommend. All 64 medical boards queried responded. They were asked, "Does your state have any of the following regarding the use of chaperones for physical examinations?" Forced selections included Law, Rule, Policy, or Position Paper. Seven (10.9%) state medical boards indicated that they had either a policy or position paper. A review of the copies of the position papers and policies from the seven medical boards revealed that four of the boards had a similar position paper/policy to that of the State of Ohio Medical Board regarding the use of chaperones. One board had similar content and the other addressed it as general principle. The Ohio position paper, entitled "Physical Examinations by Physicians" (March 8, 1989),[2] appeared to be used as a model for the majority of the boards who had a position paper or policy.

Of the 57 boards who responded "no," some commented. Most of them indicated that a chaperone should be used during examinations of the opposite sex and was "good policy," although they felt that they could not mandate by law the use of chaperones. Some of the states indicated that they are currently looking at the use of chaperones and will be developing a policy in the future. Several of the states indicated that they required chaperones for disciplinary action.

Adoption of a uniform policy that standardizes the use of chaperones is inherently a difficult process. This difficulty does not diminish the physician's responsibility to be sensitive to patients' needs. The fact that the policy or position paper is in place in some states alerts physicians to the wisdom and reasonableness of the use of a chaperone, yet still provides some flexibility, depending on individual physician and patient circumstances.

Suggestions from an American College of Obstetricians and Gynecologists publication may be helpful when one is considering the use of chaperones: "The request by either a patient or a physician to have a chaperone present during a physical

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examination should be accommodated irrespective of a physician's gender. Local practices and expectations differ with regard to the use of chaperones, but the presence of a third person in the examination room can confer benefits for both patient and physician, regardless of the gender of the chaperone. If a chaperone is present, the physician should provide a separate opportunity for private conversation. Family members should not be used as chaperones unless specifically requested by the patient."[3] Gabbard and Nadelson[4] report that the use of chaperones is always a matter of good clinical judgment and they strongly recommend having one in the following situations: (1) a patient who has a known history of sexual abuse, (2) a patient who has extreme anxiety or a psychiatric disorder, (3) a litigious patient, (4) a patient undergoing a pelvic exam, and (5) a patient who, for any reason, raises concerns about the physician.

The secretary-treasurer of the State Medical Board of Maryland and editor of The Board of Physician Quality Assurance stated, "Given the magnitude of the consequences a physician may suffer if a patient makes a credible claim of sexual misconduct or assault, physicians should give serious thought to their policies on the use of chaperones in their practice."[5] We agree and feel that the state medical boards should provide strong recommendations to help assist and protect physicians and patients in these types of matters.

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SPINAL MANIPULATION

In a recent article, "Complications of Spinal Manipulation—A Comprehensive Review of the Literature (*Assendelft WJJ, Bouter LM, Knipchild PG. J Fam Pract 1996: 42:475-80*), the authors state: "The majority of complications reported is ascribed to chiropractors." This statement is followed with three alternative explanations about why this is a correct conclusion: "(1) relatively large contribution to the total number of manipulations applied, (2) many reviews are based on searches of databases originating in English-speaking countries, where chiropractors are the main providers of spinal manipulative therapy, and (3) spinal manipulative therapy injuries are often misclassified." The authors do not, however, provide any clear discussion about the *rate* of occurrence of complications from spinal manipulation performed by chiropractors compared with the rate of occurrence at the hands of other practitioners.

According to the most recent study of the proportion of manipulative procedures done by various health care practitioners, in this country at least, the Rand Corporation indicates that approximately 94% of the manipulation in the United States is done by doctors of chiropractic. If that statistic is accurate, and I have no reason to believe it is not, then I find a significant disproportion in the percentage of all complications caused by manipulative therapy when performed by chiropractors. According to the statistics offered by the authors, out of 295 complications, only 135 (47.5%) occurred at the hands of the chiropractic doctor. By comparison, 14% occurred when treated by the doctor of medicine. These figures would suggest to me that the *rate* of complication must be significantly lower within the population of patients cared for by chiropractors as compared with those cared for by MDs, DOs, physical therapists, or others.

The authors conclude that "referral for spinal manipulation therapy should not be made to practitioners applying rotary cervical manipulation." Clearly, if one of the objectives of spinal manipulative therapy is to carry the joint into the paraphysiologic range for the purpose of releasing adhesions, then rotational manipulation is almost, by definition, a necessity. Just as the application of allopathic treatment should be titrated appropriately for the individual involved, the degree of rotation, the depth of thrust, the speed of the thrust, and the strength of the thrust are all to be appropriately "titrated" by the chiropractic doctor in the application of manipulative therapy. To say that people should not be referred to chiropractors who use rotary manipulation would be akin to saying that patients should not be referred to an allopath who uses analgesics.

James F. Winterstein, DC
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To the Editor:

In their recent review regarding the complications of spinal manipulations,[1] the authors overlooked several important sources that have estimated the risk of stroke from neck manipulation. For example, a report published by Carey[2] reviewed all malpractice claims in Canada involving an alleged cerebral vascular accident (CVA) from chiropractic manipulation over a 5-year period from 1986 to 1991. During that time there were 13 significant CVA incidents reported throughout Canada, without a single

death, among an estimated 50 million neck manipulations. Carey suggests that the incidence of CVA from neck manipulations is much lower than previously reported—about one incident per 3 million neck manipulations. Further data from the National Chiropractic Mutual Insurance Company (NCMIC), which insures more than one half of American chiropractors, shows that from 1991 through 1993, there was an annual average of 20 claims settled with payment for CVA.[3] According to national averages, NCMIC's 24,000 chiropractors each perform about 1800 cervical manipulations per year, yielding a rate of less than one stroke per 2 million cervical manipulations.

Malpractice statistics may not always be reliable for estimating risk, but surely they should carry more weight than informal audience polls taken at conventions and unsubstantiated claims made in editorials.[4] If a serious and disabling complication such as stroke really occurred as frequently as one per 20,000 manipulations as the authors imply, it is hard to believe that less than 1% of those injuries are actually reflected in malpractice claims. Considering the full body of research on this topic, a much more reasonable estimate of the risk of stroke from cervical manipulation is one-half to two incidents per million manipulations performed, and the risk of death from manipulation-induced CVA is less than one in 4 million manipulations.[5]

There seems to be a consistent pattern in the medical literature of looking at the risks of manipulation in a clinical vacuum. Rather than focusing solely on the risks of manipulation, it is much more meaningful to ask how manipulation compares with other treatments for similar conditions regarding safely and effectiveness. For example, nonsteroidal anti-inflammatory drugs (NSAIDs), the cornerstone of conservative medical management of musculoskeletal pain, carry significant but often unappreciated risks. One study[6] found an annual death rate of 4 per 10,000 from NSAID-induced ulcers among patients taking them for osteoarthritis and related conditions. Even short-term NSAID therapy carries a considerable risk of serious and potentially fatal gastrointestinal injury.[7] A strong argument can be made that the relative risks of neck manipulation are comparable to or less than the risks of NSAIDs and other conventional treatments for similar conditions. No existing scientific evidence suggests that manipulation is any less effective than these alternatives.[5]

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The preceding letters were referred to Drs Assendelft and Bouter, who respond as follows:

The issues raised by Winterstein and Lauretti, and the resulting confusion, clearly underline the need for well-designed studies to provide valid estimates of the risk of spinal manipulation for various indications and by different professional groups.

Winterstein argues that we blame the chiropractors too much. However, we addressed this issue by indicating that "their [the chiropractors'] relatively large contribution to the total number of manipulations applied is one of the putative reasons for the high percentage of chiropractic complications.[1] Furthermore, we think that the type of calculation made by Winterstein is not justified. Our review deals with descriptions of complications over a period of several decades, therefore the current percentage of chiropractic manipulations does not apply. In addition, we did not restrict our review to reports from the United States.

Lauretti confuses the number of *manipulations* applied with the number of *patients* treated. We decided to extract data from articles without any transformation, implying that we reported the findings from Haynes[2] as one per 20,000 *patients*, not *manipulations*. Based on the assumption of Dabbs and Lauretti,[3] that a typical course for patients with neck pain or tension headache involves 10 to 15 cervical manipulations, the findings of Haynes[2] can be expressed as one vertebrobasilar accident (VBA) per 200,000 to 300,000 manipulations. Lauretti also questions the credibility of the findings we presented, referring to claim data and surveys. In our opinion, claim data cannot be regarded as valid estimates of the actual incidence. We feel supported in this view by the author of one of the claim-data studies, who concludes: "The actual incidence following manipulation is unknown and this would require a carefully conduced epidemiological study."[4]

Lauretti pleads for comparison of the risk of spinal manipulation with that of other treatment for similar conditions, later focusing on NSAIDs. Although from a conceptual point of view this would be informative, data from similar age groups with identical complaints (neck pain or headache) concerning alternative treatment options (spinal manipulation and NSAIDs) are simply not available, reducing every attempt to a compari-

son of apples with oranges. The method of data collection used in cohort and case-control studies on complications of NSAIDs[5,6] are much more trustworthy than those for spinal manipulation. Reviews of the risks of NSAIDs [5,6] indicate that the risk of complications is, to a great extent, dependent on comorbidity and comedication, as well as on age. Therefore, a comparison of figures for NSAIDs with those for spinal manipulation, without correction for these factors, is completely unjustified.

Both authors comment on our recommendation not to refer patients to practitioners applying manipulation with a rotatory component. Our point of view was based on the pathophysiology of VBAs and on recommendations made by chiropractic authors.[7,8] We appreciate Winterstein's explanation that the different alternative techniques may vary in the stress to the vertebral arteries. It is exactly this type of discussion between referring physician and treating spinal manipulator that we aimed at. However, in view of the potential, though rare, severe consequence, we think that the issue of cervical manipulation techniques should continue to be the subject of debate.

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