Correspondence

We will try to publish authors' responses in the same edition with readers' comments. Time constraints might prevent this in some cases. The problem is compounded in a bimonthly journal where continuity of comment and redress are difficult to achieve. When the redress appears 2 months after the comment, 4 months will have passed since the original article was published. Therefore, we would suggest to our readers that their correspondence about published papers be submitted as soon as possible after the article appears.

In Utero Exposure to Medroxyprogesterone

To the Editor: In a letter published in the last issue of the *JABFP*, Dr Coutts[1] has extended the lessons to be learned from our case of in utero exposure to injectable medroxyprogesterone. In our brief report, we focused exclusively on a review of the literature regarding fetal effects. Dr. Coutts has expanded that focus to include clinical clues to the prevention of administration of medroxyprogesterone to women already pregnant. In our case the patient was apparently 5 to 6 weeks pregnant when the second injection of medroxyprogesterone was given, which is long enough to at least question the patient about symptoms of pregnancy and maintain a low threshold for doing a pregnancy test.

The rates of women with amenorrhea increase with use of medroxyprogesterone from 30 to 50 percent after the first year to 80 percent by the end of the fifth year.[2] I am not aware of the amenorrhea rate after the first 13-week period. An additional form of contraception for the first 2 weeks after initial injection is recommended only if the injection is not given during the first 5 days of a normal menstrual period.[2] Repeated injections should be given within 91 days to maintain adequate protection; however, once well established, medroxyprogesterone actually will provide a grace period of 2 weeks or longer beyond the 91-day period. Most clinicians would recommend obtaining a pregnancy test before reinjection if the patient delayed beyond 91 days.

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References

1. Coutts LC. In utero exposure to medroxyprogesterone. J Am Board Fam Pract 1996;9:467.
2. Hatcher RA. Contraceptive technology. 16th revised edition. New York: Irvington Publishers, 1994:285-326.

Evidence-Based Medicine and the Art of Medicine

To the Editor: Dr. Berg's experiences on clinical practice guideline panels and his apparent dismay at the results of his efforts (Berg AO. Clinical practice guideline panels: personal experience. J Am Board Fam Pract 1996;9:366-70) displays an attitude that has been prevalent in many academic centers since I began medical school more than 40 years ago. The argument is that if we can only prove with double-blind crossover studies that what we do is the right thing to do, we will be able to provide better medical care (now the emphasis is on providing cheaper care, but we used to be interested primarily in quality).

The literature of medicine for the past century is replete with apparently sincere and conscientious efforts to quantify in one form or another the biologic phenomena of health and disease. We have enlisted the aid of mathematicians and statisticians, who have developed complex formulas into which we dump large amounts of data. With ever more powerful calculating devices, we have massaged those data until now we can prove almost anything we wish assuming we can find the correct statistical test.

Now we who practice in the real world are faced with a problem. Articles showing statistical significance among a limited number of variables (that the authors apparently believe are the only important, or the most important) fill our most prestigious medical journals. The caveats "may be related," "seem to," or "appear to" seem to get lost in the translations we hear on network news or read in *Reader's Digest*. That there might be no clinical importance to the statistical significance is rarely mentioned. Subsequently, those who wish to seek the truth based on larger, more substantial studies, will do meta-analyses combining the results of several studies (assuming that the variables from one study are truly comparable with the same named variable in another study done at another center, perhaps in another part of the world) in ever-increasing mathematical efforts to determine biologic truth. If some should challenge the value of meta-analyses compared with personal experience in clinical practice, we declare the individual inexperienced in scientific methods or unfamiliar with evidence-based methods, which, of course, in our minds relieves us of the necessity of considering that diverse opinion.

I sympathize with Dr. Berg's plight. It is hard to deal with those who think their clinical experience is as likely to be valid as his scientific evidence. As a longtime clinician who has watched so-called truth come and go in medicine, however, I wonder whether "the poor quality of scientific information that supports the common practice" is always truly less well tested and certified than the latest statistical massaging of the data. Do numbers always mean something of importance? Is statistical significance usually (always? occasionally?) related to clinical worth? We can report hemoglobin levels to the nth significant digit, but is it clinically more valuable than the first two or three digits? In our efforts to improve quality of care (actually,

today, to reduce costs), are we ignoring one of the traditionally powerful forces in healing, ie, the art of medicine? Dr. Berg might find it strange that in this scientific era, one would support such nonsense as a patient's faith and belief in his or her physician having something to do with the healing process (certainly impossible to quantify).

Dr. Berg's comment "Without exception these experiences show that the layer of scientific evidence upon which much of medical care is based is very thin indeed" seems to me to be based more on his clinical experience with four panels than a true, evidence-based conclusion. It is certainly a poorly substantiated generalization appearing to be based more on anger and frustration than documentation of evidence.

I would not disagree that we need to pursue rational justification for the things we do in medicine. I think we must also guard against the elitist (and usually academic) view that if a practice or a method or treatment can't be proved with a scientifically designed study and if we can't get the important data into our computers to manipulate, then somehow that method or practice is less worthy because that attitude itself is unscientific and cultist. It is important for all of us to understand the evidence-based systems, but we must include in them those much more difficult studies that are much less amenable to statistical manipulation, ie, studies that have to do with the effects of the art of medicine. I believe we leave the art of medicine out of our equations at our patients' and our own peril.

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To the Editor: I read with pleasure the recent special communication on clinical practice guideline panels by Dr. Berg (Berg AO: Clinical practice guideline panels: personal experience. J Am Board Fam Pract 1996; 9:366-70). His discussions, cautions, and suggestions underscore the degree to which medical practice has been guided, albeit somewhat blindly, by scientific doctrine.

One area Dr. Berg did not specifically address, which I believe is relevant to clinical practice guidelines, is the depiction of knowledge in a graphic-based format. Several of the panels with which Dr. Berg has been involved have created small algorithmic approaches to clinical decision-making and practice guidelines. Graphically linking decisions with particular outcomes greatly enhances and clarifies many of the issues within a particular area. Having been involved with graphic depictions, I see their continued emergence as valuable and expect that they will be included more often in future practice guidelines.

Another area is the incorporation of computers into medicine. I expect computers and expert systems to be increasingly used in clinical practice guidelines and look forward to future panels that utilize this form of communication.

Dr. Berg's article, personal experience, and example not only stand as a tremendous source of strength for the family physician who attempts to integrate multiple systems and family concepts into decision making but also underscore the complexity involved in even the most apparently simple clinical issues.

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Mental Health Patient Profile

To the Editor: The study by Mazonson, et al,[1] who screened waiting patients for anxiety, was well supported until the concluding paragraph. The authors then state, "Our results show that patient self-reported information on anxiety and psychological health, collected in a manner that places minimal burden on primary care physicians and their staffs, can lead to heightened physician awareness." The authors screened 7914 patients to find 618 patients meeting the study criteria for anxiety. Thirty-four patients in the intervention group were referred for a mental health evaluation, and 45 were placed on psychotropic medications. By comparison, 7 patients were referred for evaluation and 37 patients were prescribed medications in the control group. The additional 35 patients found through this intervention represent 0.5 percent of the 7914 patients initially screened. The authors fail to support their conclusion that screening represented a minimal burden to the other 99.5 percent.

Greater Valley Medical Group is also a mixed-model health maintenance organization serving 60,000 patients in Los Angeles, a practice similar to the study practice. Our new patients spend 30 to 45 minutes completing our front office forms and eligibility checks. Patients already enrolled often spend a similar amount of time waiting when they change insurance carriers or jobs. This wait not only engenders complaints but creates a burden for our patients and our staff. For this reason, we recently reviewed and rejected a request to add additional screening questions for sexually transmitted diseases, risk factors for infection with human immunodeficiency virus, and exercise and diet to our initial new patient questionnaire. We considered adding these questions because such screening is recommended by the US Preventive Services Task Force[2] and is included by health plans in their office record audits. In contrast, the USPSTF recommends against screening for depression. According to the Clinician's Handbook of Preventive Services, "The performance of routine screening tests for depression in asymptomatic individuals is not recommended.[2]" Anxiety screening is not even mentioned in this reference.

Screening for depressive illness would create more than a minimal burden on a busy office. It would have been helpful to measure the patients' and staff's response to the screening procedure. Would the staff have been willing to continue with the extra forms and