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,