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The above letter was referred to the author of the article in question, who offers the following reply:

To the Editor: Dr. O'Connor takes me to be stating that "really important clinical ethics happens only in very sick patients, usually at tertiary care centers, and most often in ICUs." She obviously champions the view that family physicians face ethical issues in their daily practice, which are just as frequent and just as important as any other area of medicine. I agree. Nothing that I wrote disagrees with that view.

If I had read only the abstract by Orr and Moss, I could have read that they are addressing the different topic of family physicians as "future teachers, researchers, institutional leaders, and policy makers in clinical ethics." Moreover, the role of the clinical ethicist vis-à-vis ethics committees figures largely in their article. Given this orientation, I must ask whether family physicians are trained to fulfill these roles and to address these issues. When setting policy about whether to do liver transplants on alcoholics with end-stage liver disease who refuse to enter Alcoholics Anonymous, should the family physician ethicist be called? Second, most issues that come to the ethics committee do indeed involve ICUs and tertiary care centers, and if someone is going to be a consultant to such an ethics committee, that person must respond to its real needs. If the family wants a consultation before disconnecting the respirator of a patient supposedly in a persistent vegetative state, should the family physician be called? Of course, many ethical issues exist in family medicine that could come to such committees, and if Dr. O'Connor is correct that family physicians qua ethicists can be patient advocates, perhaps they will soon be raising such issues with such committees (or advising their patients of the existence of such committees if their patients experience ethical problems with physicians).

Dr. O'Connor falsely accuses me of embracing a slippery slope down the quality-of-life trail. While it is true that I have defended the Dutch system of physician-assisted suicide among terminally ill patients, unlike America, Holland has cradle-to-grave medical care and no families or patients who may decide to die to save money for their children or society. In other research I have concluded that competent, disabled patients such as Elizabeth Bouvia and Larry McAfee have a right to die, but I also believe that both struggled heroically against prejudiced systems. As American medicine begins now to embark on cost-saving schemes, I am cynical about our ability to create better systems for the disabled; I would rather see a great system, but until that comes, I want the competent disabled person to be empowered with a right to not suffer and to die. More generally, O'Connor does me injustice in that I have criticized the *Quinlan* decision in 1975 for lumping together incompetent with competent patients and the Baby Jane Doe case for biased, incompetent reporting that — amazingly! — was awarded a Pulitzer Prize.[1]

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Unsolicited Gifts from Pharmaceutical Companies

To the Editor: In the 10 years since my graduation from medical school, I have received innumerable gifts from pharmaceutical companies through the postal service, including puzzles, magnifying glasses, messages in plastic bottles, and nonmedical books, to name a few. These gifts have been unsolicited, have been of no value to me, and have made me less likely to use the product than otherwise might have been the case. Many of the items are nonbiodegradable, adding more problems to our troubled environment. The dollars invested in this advertising would be better spent on further research, lowering the cost of medication, or helping provide prescriptions to needy patients. I believe that this form of marketing is entirely inappropriate.

My criticism should not be confused with "pharmaceutical industry bashing." The pharmaceutical companies in this country have made a vital difference in the quality of medical care provided through research and development of new and important products. These firms have a right to market their products to physicians, if done in an appropriate manner.

During the past year, my colleagues and I have begun writing letters to pharmaceutical companies who send out these gifts. It is our hope that if a sufficient number of physicians express their concern in this fashion, the pharmaceutical firms will cease with this illegitimate, noneducational form of marketing.

Nick W. Turkal, MD
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Techniques of Meta-Analysis

To the Editor: As a physician who appreciates the value of meta-analysis, I was gratified to see two meta-analyses appear in the March-April 1993 issue.[1,2] Because meta-analyses can have such far-reaching implications, however, I believe that it is particularly important that they be properly conducted and reported. Therefore, I am writing to express some concerns about these two meta-analyses.

Although Hawley, et al.[2] attempted to locate unpublished studies, both meta-analyses could have fallen victim to publication bias. The use of only published studies might severely alter the conclusions of the body of literature. Minimally, both meta-analyses should have constructed a funnel graph[3] — plotting effect size against sample size — to seek visual evidence of publication bias.

In the study by Schneider, et al.[1] the description of the statistical methods used is scanty but suggests that these studies did not include control groups. In addition, at least some of the analyses are presented in such a way as to suggest that the subjects were pooled across studies rather than the effect sizes being pooled. A true meta-analysis involves pooling of study effect sizes rather then pooling of individual subjects. According to Feinstein,[4] the pooling of subjects is appropriate only if three criteria are met: (1) all data are from randomized clinical trials; (2) there is homogeneity of protocols including similar patients, treatment, and follow-up; and (3) individual study results are similar to each other. Based upon the heterogeneity of these studies, data pooling in this case would appear to be inappropriate. The authors cite Yusuf, et al.[5] for their methodologic adaptation of the Mantel-Haenszel methods. Yusuf, et al.[5], however, were conducting a meta-analysis using randomized trials. Hence, their methods might not be appropriate to this study.

The meta-analysis by Hawley, et al.[2] raises other concerns. I applaud the use of a quality assessment of the study protocols but question the wisdom of summing the ratings on individual study criteria. Horwitz and Feinstein[6] suggest that it is inappropriate to sum such methodologic assessments for deriving an overall study quality score in epidemiologic studies. In addition, when utilizing such subjective variables as quality assessment, it would be helpful to determine the interrater reliability for each criterion.

Although Hawley, et al.[2] are cautious about the relation between prepregnant contraceptive exposure and risk of breast cancer, they suggest that their meta-analysis supports such a relation. When interpreting a meta-analysis, however, it is important to remember that you are looking at relations among studies, not individuals. Hence, this meta-analysis has found a relation between the duration of prepregnant exposure in studies and their overall effect size, but it has not addressed the relation between individual durations of exposure and risk of breast cancer.

Sacks, et al.[7] previously reviewed the quality of a group of meta-analyses based upon criteria that they established. The concerns I have with these two meta-analyses are frequently seen in other meta-analyses. Sacks, et al.[7] found that only 7 percent of meta-analyses adequately describe their study protocol and only 5 percent presented measures of interobserver agreement on coding. In fact, only 2 percent of the meta-analyses adequately addressed the issue of publication bias.

Because both of these meta-analyses sought to explain conflicting results within a body of literature, it would have been particularly helpful if they had assessed the impact of different study characteristics and potential biases upon their outcomes. Unfortunately, neither meta-analysis appears to have explained the conflicting results within the literature. The potential for such an explanation is one of the strengths of meta-analyses as a technique. In conclusion, I am gratified to see an increasing number of meta-analyses appearing in the literature. Like Sacks, et al.,[7] I ask that the quality of conduct and presentation of meta-analytic results be improved.

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