Correspondence

We will try to publish authors' responses in the same edition with readers' comments. Time constraints may prevent this in some cases. The problem is compounded in the case of a bimonthly journal where continuity of comment and redress is 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.

Recruitment to Family Practice Residencies

To the Editor: In his recent letter,[1] Dr. Herbert F. Laufenburg relates his concerns regarding changes in recruitment tactics of many family practice residencies — changes that include promotional efforts, which seem to focus more on salary and benefit issues and less upon educational quality.

Following Dr. Laufenburg's letter, an article appeared in *American Medical News*[2] describing a plan at the University of Oklahoma to increase family practice residents' salaries by an additional $17,000 per year for students agreeing to serve in rural areas. The article quoted some educators as believing such efforts will "unleash cut-throat competition among programs." Another educator stated, "You should pick a program for educational quality, not for the money."

While I understand the concern expressed in the *American Medical News* article and in Dr. Laufenburg's letter, I do not necessarily agree with their contentions.

Educational quality and financial factors are independent issues. The Residency Review Committee does not make accreditation decisions based upon a program's signing bonus or relocation allowances. Nor has it been my observation that students entering family practice are abandoning educational concerns. Nevertheless, applicants now seek financial information as well.

And why not? Most students are graduating with staggering debts. The larger question is this: Are such financial incentives ethically wrong? After careful consideration, I believe they are not.

Rather than considering which programs are "best" academically, students consider which programs offer the unique educational experiences most pertinent to their future needs, e.g., extensive obstetric experience, a broader internal medicine foundation, a stronger behavioral science basis. Students narrow their program choices accordingly. Only at this point, I believe, do prospective residents bring in financial issues. Clearly, these issues can determine final program ranking.

Will competition for family practice residents increase? Of course it will. As more community hospitals realize that the most effective primary care recruitment tool is through the sponsorship of family practice residencies, new programs will be developed with strong financial backing and aggressive marketing to students. The situation that Dr. Laufenburg describes ("overzealous competition, based on incentives, may harm good programs that are walking a tight rope . . . ") will likely worsen.

I doubt that any of the family practice organizations can mandate "caps" on salaries, benefits, or bonuses. Nor do I believe the number of students entering family medicine will suddenly explode. Despite strong efforts at many medical schools, only 24 additional Unites States seniors matched in family practice this spring compared with spring 1991.

Reality must be faced. As long as there are inadequate supplies of students entering family practice, skyrocketing student debts, increasing demands for the recruitment of family physicians, and a willingness of sponsoring institutions to use *both* strong financial and academic incentives to attract students, it will be impossible to prevent an escalation of what Dr. Laufenburg describes as a "bidding war" for a limited number of graduates. We should focus less upon how to control competition between programs and more upon the perverse health care environment that makes this competition possible.

A good place to start would be by acknowledging that the availability of residency slots in the various specialties has no relation to the health care needs of this country. National demands for family physicians cannot be met as long as an excess of subspecialty training positions continues to siphon off potential family practice residents.

What is desperately needed is national regulation of house staff slots — specifically reduction in the surplus training positions in subspecialties. Such regulation would be aimed at bringing the total number of residency positions, as well as the percentage of slots devoted to family practice, more in line with national health care needs.

Preferably such regulation should come from the medical profession. If organized medicine lacks the fortitude to deal with this problem, however, I am sure our friends in government would be happy to take up the issue.

To many, this measure seems too drastic; however, I see it as the only effective means of dealing with the issues Dr. Laufenburg raises. The choice is ours. We can either begin the important task of developing a rational mechanism to insure that the output of various specialists actually meets our national health care needs, or we can all go back to our sponsoring

institutions and request ever-increasing sums of money to compete with one another as the family practice bidding war heats up.

Donald R. Frey, M.D.
United Hospital Center
Clarksburg, WV

References

1. Laufenburg HF. Recruitment to family practice residencies. J Am Board Fam Pract 1992; 5:109.
2. Page L. Oklahoma luring FP residents with 65% raise. American Medical News 1992 January 20.

Physical Activity and Coronary Heart Disease

To the Editor: I was pleased to see the meta-analysis conducted by Dr. Eaton on the relation of physical activity to coronary heart disease.[1] It is exciting to see meta-analyses appearing in the family medicine literature; however, I wish to raise four issues that, I believe, are relevant to the study.

First, with the exception of comparing cohort and case-control studies, there is no assessment of study quality and its relation to effect size. Sacks, et al.[2] emphasize the importance of such analysis.

Second, the use of historical cohorts in two of the studies is problematic. With the probable exception of Glass, most meta-analysts would recommend excluding studies that did not use equivalent controls.[3]

Third, of more than 75 articles reviewed, only 14 studies were used. Optimally, we should be given a list of rejected studies with the reason for rejection. Minimally, a list of rejected studies should be available upon request.[2]

Perhaps my greatest concern is the lack of recognition of possible publication bias. That the unpublished literature frequently differs from the published literature in its findings is well documented.[4] Although some meta-analysts have rationalized ignoring the unpublished literature,[5] the appropriateness of this approach has not been proved.[6] When conducting a meta-analysis, every attempt should be made to locate unpublished studies. If this search is not done or is unfruitful, other techniques should be employed. In Dr. Eaton's study, he reports the overall weighted relative risk (RR) = 1.37 using 12 studies. Using the natural logarithmic transformation and Rosenthal's formula[7] for fail-safe N — the number of unpublished studies with null results needed to overturn an overall significant effect size — I calculated that less than one study would be needed. Using Orwin's formula,[8] I calculated that 26 studies having RR ≤ 1.1 would be needed. Although Orwin's formula produces somewhat reassuring results, if we remember that an estimated 2 to 10 unpublished studies are needed for every published study, then needing 25 unpublished null studies to overturn the results of 12 published studies is of concern. Using a funnel graph plot[9] — effect size versus sample size — also raises concern because the only published study with

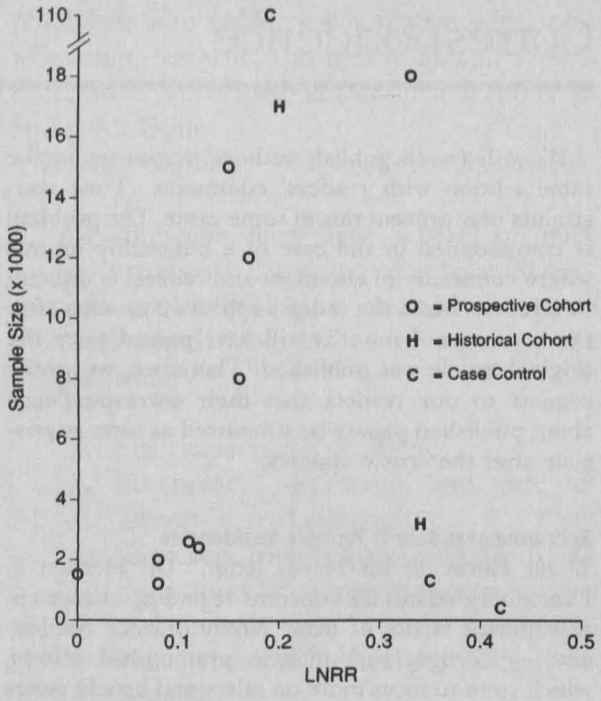

Figure 1. Funnel graph.

sample size < 1000 had the greatest relative risk. In fact, as a rule, the cohort studies had the lowest RRs (Figure 1).

Although I enjoyed Dr. Eaton's article and believe that the conclusions are probably valid, I think the concerns that I have raised do have important implications.

David A. Katerndahl, M.D.
University of Texas Health Science Center
at San Antonio

References

1. Eaton CB. Relation of physical activity and cardiovascular fitness to coronary heart disease, Part I: a meta-analysis of the independent relation of physical activity and coronary heart disease. J Am Board Fam Pract 1992; 5:31-42.
2. Sacks HS, Berrier J, Reitman D, Ancora-Berk VA, Chalmers TC. Meta-analysis of randomized controlled trials. N Engl J Med 1987; 316:450-5.
3. Bangert-Drowns RL. Review of developments in meta-analytic method. Psychol Bull 1986; 99:388-99.
4. Katerndahl DA. Believing everything you read. Fam Pract Res J 1991; 11:343-7.
5. Steinberg KK, Thacker SB, Smith SJ, Stroup DF, Zach MM, Flanders WD, et al. Meta-analysis of the effect of estrogen replacement therapy on the risk of breast cancer. JAMA 1991; 265:1985-90.
6. Smith ML. Publication bias and meta-analysis. Eval Educ 1980; 4:22-4.
7. Rosenthal R. The file drawer problem and tolerance for null results. Psychol Bull 1979; 86:638-41.
8. Orwin RG. A fail-safe N for effect size in meta-analysis. J Educ Stat 1983; 8:157-9.