The Journal welcomes Letters to the Editor. If found suitable, they will be published as space allows. Letters should be typed double-spaced, should not exceed 400 words, and are subject to abridgment and other editorial changes in accordance with Journal style.

OBSTETRIC RISK SCORING

To the Editor:

The recent review of obstetric risk-scoring systems by Dr Wall[1] and study of a scoring system in current use at the Oregon Health Sciences University[2] are timely efforts at clarifying this chapter in medical decision making. It is alarming to read in the latter report that official endorsement has been given to a scoring system, while the objective data of the report do not support such endorsement.

An effective heuristic model is needed to allow understanding of risk-scoring systems in the context of medical practice. The three dimensions of *predictive accuracy, usefulness,* and *acceptability* are described in the literature relating to assessment of predictive instruments.[3] Addressing each of these dimensions allows the researcher to describe fully the decision-support tool as a part of the environment in which it is intended to be used. Reviewing the report of Wall et al, using the above model, it becomes clear that there is little objective support for the scoring system.

The report addresses only the dimension of *predictive accuracy.* Characterizing the predictive accuracy of a scoring system in a given population requires measurement of strength of association as well as demonstration that an association is statistically significant. Prediction requires far stronger correlation than is generally necessary to reach statistical significance in a study of adequate size. In this report the strength of association between initial score and length of hospitalization (Pearson's $r = .18$) is actually quite low despite the fact that statistical significance was reached ($P \leq .01$). The risk-scoring system is within 8% of the performance of a coin toss at predicting adverse outcomes on initial visit, and within 14% of a coin toss when the score at 37 weeks is used (abstracted from Table 3 of the study). The receiver operating characteristic curve is a useful tool in visually representing this type of analysis.[4]

The dimensions of *usefulness* and *acceptability* similarly require structured analysis. Details of this type of analysis have been described elsewhere.[3,5,6] Endorsement of a scoring system should occur only after its performance in all three dimensions has been studied and found to be satisfactory.

George A. Corey, MD
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1. Wall EM: Assessing obstetric risk. A review of obstetric risk-scoring systems. J Fam Pract 1988; 27:153–163
2. Wall EM, Sinclair AE, Nelson J, Toffler WL: The relationship between assessed obstetric risk and maternal-perinatal outcome. J Fam Pract 1989; 28:35–40.
3. Corey GA, Merenstein JH: Applying the acute ischemic heart disease predictive instrument. J Fam Pract 1987; 25:127–133
4. Berwick DM, Thibodeau LA: Receiver operating characteristic analysis of diagnostic skill. Med Care 1983; 21:876–885
5. Corey GA: Computer-derived protocol to predict myocardial infarction in patients with chest pain, letter. N Engl J Med 1988; 319:790–791
6. Corey GA: Cardiac ischemia decision-support tools, letter. J Fam Pract 1988; 27:576

The preceding letter was referred to Dr Wall, Ms Sinclair, and Dr Toffler, who respond as follows:

While we appreciate the comment by Dr Corey, we are somewhat puzzled by his reference to the positive predictive value of a coin toss. Assuming the sensitivity and specificity of the toss of a proper coin to be 50%, we have constructed the table below. We have also calculated the percentage improvement over the coin toss for each of the positive predictive values of the risk-scoring systems that appeared in Table 3 of our article.[1] We are not certain exactly what Dr Corey means by "within 8% of the performance of a coin toss," but we suspect the percentage improvement figure may be reasonably analogous.

While the positive predictive values of the assessment scores are low, they are, at least, quite a bit better than a "heads or tails" scoring. Interestingly, negative predictive values for these assessment scores showed a range of only 1% to 3% improvement over the toss of a coin! We attribute this to the high negative predictive value of a coin toss, which is a direct function of the very low incidence of the adverse outcomes studied.

Finally, we agree with Dr Corey

POSITIVE PREDICTIVE VALUE (Percentage Improvement Over Coin Toss)			
Outcome	"Coin Toss"	Antepartum Initial Assessment	37-Week Assessment
Cesarean section delivery	.15	.26 (73%)	.20 (33%)
1-minute Apgar < 7	.18	.26 (44%)	.28 (56%)
5-minute Apgar < 7	.02	.02 (0%)	.00 (neg)
Birthweight < 2500	.05	.07 (40%)	.20 (300%)
Estimated gestation age < 37 weeks	.07	.09 (29%)	.16 (129%)

that endorsement of a scoring system should occur only after its predictive accuracy, usefulness, and acceptability have been studied. A rationale for our study was the lack of objective data supporting the existing scoring system. As we all are aware, many things in medicine are implemented prior to their full evaluation (coronary care units, electronic fetal monitoring, etc). Clearly, obstetric risk-scoring systems must be added to this list.

Eric M. Wall, MD, MPH
Ann E. Sinclair, MS
William L. Toffler, MD
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Reference

1. Wall EM, Sinclair AE, Nelson J, Toffler WL: The relationship between assessed obstetric risk and maternal-perinatal outcome. J Fam Pract 1989; 28:35–40

GENERALIST-SPECIALIST BOUNDARY

To the Editor:

Boundary issues between the generalists and the specialists will probably never be resolved to the satisfaction of everyone concerned, and, frankly, I don't see why they have to be.

The distinguishing characteristic of the generalist is precisely that his knowledge and interests and skills cover a broad array of medical problems; even among generalists themselves, there are no set boundaries unless one considers attitude and judgment as qualifying characteristics.

The generalist by temperament and training has—or should have—a humanistic outlook on the practice of medicine, and his judgment is such that he relies, when necessary, on the specialized, scientific, more-abstruse knowledge of the specialists.

Generalists as a group within themselves have different interests and skills; some are more adept than others at cardiology, or dermatology, or whatever. Their attitude, however, is one of concern for the total patient, which is quite different from that of the specialist, who by temperament and training is more interested in an organ system, and usually disdains getting involved in reassurance, speaking with family members, and arranging for comprehensive ongoing care of the patient's general medical problems.

Therefore, I think that boundary issues are a myth, a misperception; they lead to confusion.

Edward J. Volpintesta, MD
Bethel Medical Group
Bethel, Connecticut

GERIATRIC ASSESSMENT PROJECT

To the Editor:

I would like to invite your readers to share information to be used in a multidimensional assessment project designed to provide comprehensive health and medical information in a manner that will enable physicians to focus on primary, secondary, and tertiary preventive programs needed to minimize morbidity and maintain the highest level of physical and mental function among the elderly.

This project incorporates information about functional and preventive health, medical problems, social criteria, cultural characteristics, economic resources, and spiritual composition by integrating a validated assessment protocol oriented to the aged. The product we hope to develop will integrate, synthesize, and analyze data including physical, laboratory, and functional measurements.

The multidimensional assessment system is targeted for publication after field trials and will offer a succinct, complete personalized inventory and problem list for use by the attending physician.

For further information contact D. Robert Howard, MD, Geriatric Assessment Project, Department of Family and Community Medicine, Mercer University School of Medicine, 1550 College Street, Macon, GA 31207 (912-744-4104); or Russell M. Hostetler, MD, Assistant Professor, Department of Family and Community Medicine, Mercer University School of Medicine, 1550 College Street, Macon, GA 31207 (912-744-4095).

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CORRECTION

To the Editor:

I wish to bring to your attention an error in the Letters to the Editor in the June 1989 issue of *The Journal of Family Practice* on page 732. I believe this one an oversight.

The letter which I authored was written with the purpose of bringing to the attention of the readers an important study *[E. Barrett-Connor, M.H. Criqui, J.L. Witztum, et al: Population-based study of glycosylated hemoglobin lipids and lipoproteins in nondiabetic adults. Arteriosclerosis 1987;7:66–70]* which was not cited in either the article by Drs Urberg and Rajdev *[A correlation between serum cholesterol and glycosylated hemoglobin in nondiabetic humans. J Fam Pract 1989; 28:269–274]* or in my accompanying commentary. I had not seen the Letter to the Editor by Dr Mark Knudson which appears on page 732 and, consequently, I did not address the criticisms he raises which I, indeed, believe are important and had raised myself when originally reviewing the paper.

William E. Neighbor, Jr, MD
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Seattle

The Publisher replies:

The Journal regrets that an error in the course of page makeup misidentified Dr Neighbor's letter as a response to Dr Knudson's letter.

MYOCARDIAL INFARCTION AND DENIAL

To the Editor:

I very much enjoyed reading Dr Fields' article "Myocardial Infarction and Denial" *(J Fam Pract 1989; 28:157-161)*. The only reason that I could think of why J.W. denied having an acute myocardial infarction was his young age of 37 years; he certainly had all the risk factors for coronary heart disease: cigarette smoking, hypercholesterolemia, and uncontrolled hypertension.

Dr Fields did not tell us how J.W.'s left ventricular function was following his subendocardial infarction. The long-term prognosis of any patient with an acute myocardial infarction depends upon the amount of heart muscle damage. The earlier an acute myocardial infarction is diagnosed, the more prompt appropriate treatment can be instituted, and, it is hoped, the less permanent myocardial damage will result. In the current era of availability of intravenous thrombolytic therapy in the community hospital, it is very important to get a patient with acute myocardial infarction to be admitted to the hospital as early as possible. But the patient has to initiate this process first by contacting his family physician immediately upon development of the symptoms or their prodromata.

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The preceding letter was referred to Dr Fields, who responds as follows:

I appreciate Dr Cheng's comments about my article. In reviewing the literature, however, I did not find support that denial is age related. In spite of J.W. being only 37 years old, he had specifically been warned by an internist only one month prior to admission to watch for symptoms that might suggest myocardial infarction.

When I admitted J.W., I was working in a 50-bed rural hospital. At that time I did a predischarge exercise tolerance test and then enrolled him in a cardiac rehabilitation program. He declined referral to a university center for further diagnostic testing. From his good performance on the exercise tolerance test, I suspect that he had good left ventricular function, but his cardiac output and ejection fraction were never specifically measured.

Karl B. Fields, MD
Greensboro, North Carolina

EXERCISE TOLERANCE TESTING

To the Editor:

We read with interest the recent Controversies in Family Practice section in your journal entitled, "Is Exercise Tolerance Testing Indicated for Diagnoses and/or Screening in Family Practice?" by Drs Mead and Hindman (*J Fam Pract 1989; 28:473–480*). Of particular concern to us as family physician educators actively involved in teaching exercise testing was the opposing view by Dr Hindman.

Despite "limited independent and specific diagnostic information," exercise testing has clearly moved into the forefront as an extremely useful evaluative procedure with increasing application to the practice of ambulatory medicine. Increasing emphasis on risk-factor identification and modification, and performance of regular exercise, coupled with enhanced recognition of the presence and importance of silent ischemia in a society such as ours with a high prevalence of coronary artery disease, should all expand the role of exercise testing even further. If, as Dr Hindman states, exercise testing is a test for assessing cardiovascular function with optimal interpretation when results are considered in the context of a patient's risk factor profile, the medical history, and the physical examination, who better than a specialist in primary care is there to interpret results of the test in this context?

The issue, as we see it, is succinctly expressed by Dr Mead. "There is essentially no logical alternative to family physicians performing exercise tolerance testing. Who else will perform screening exercise testing for patients 35 to 40 years old who are either asymptomatic or have coronary risk factors? Who will evaluate the individual who wants to exercise but is out of shape? Who will evaluate the competitive athlete? Who will evaluate the middle-aged and elderly patients for silent ischemia? Who will look after coronary artery disease in individuals living in suburban and rural areas?" Even if there were enough cardiologists in the country to perform these tasks, patients simply don't present to the cardiologist's office in a totally or relatively asymptomatic state often enough with these concerns on their mind for evaluative exercise testing.

Even among mild to moderately symptomatic patients with risk factors who are otherwise functioning well, a strong case can be made for the family physician as the logical person to perform exercise testing. "Does it make sense to disrupt the family physician's care of an individual who has stable coronary artery disease with repeated cardiological consultations?"

Dr Hindman eloquently discussed many aspects of the methodology, safety, interpretation, and application of exercise testing. Yet he fails to present any concrete evidence for his view that the procedure is "most appropriate for cardiologists to perform" other than stating that "exercise testing should be performed by physicians with knowledge and special expertise in the cardiovascular response to exercise and in the diagnostic and therapeutic roles of exercise in individuals at risk for developing or already having coronary disease."

Exercise testing is not a procedure to be taken lightly. It is not without risk. However, properly and cautiously performed by an adequately trained physician, the risk is minimal while potential benefits to management are tremendous. Granted, even if exercise testing equipment were universally affordable and available, the procedure should not be performed by all family physicians. Special interest and training are required, and active supervision of a certain

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number of exercise tests by a physician with expertise in the procedure are mandatory prerequisites. However, there is no reason why family practice residents and physicians in practice cannot undertake such training and develop sufficient expertise to perform submaximal and symptom-limited exercise testing. Many are doing so already.

We agree with the parallel cited by Dr Mead—that exercise testing by family physicians is very similar to the situation with flexible sigmoidoscopy, which only a few short years ago was a procedure reserved for the gastroenterologist. Whereas initial concerns of the specialist may have been about "turf," screening sigmoidoscopy by family physicians is now accepted as optimal practice and results in increased consultations of the specialist (for polyp removal), as well as enhanced patient care. Although initially the 35-cm sigmoidoscope was "the limit," increasing numbers of family physicians have now become comfortable with more extensive screening.

So it should be with exercise testing. Routine performance of maximal exercise testing in symptomatic high-risk individuals or those with recent (within 2 to 3 weeks) myocardial infarction is still probably best left to the cardiologist. Yet submaximal testing and symptom-limited testing of less than very high risk individuals can be capably performed by family physicians, will result in enhanced patient care, and ultimately will generate additional consultations for our cardiology colleagues.

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QUALITY-OF-LIFE MEASURES

To the Editor:

The article by Hume[1] and the accompanying commentary by Taylor[2]

recently reviewed quality of life and quality-of-life measures in medicine. While their reviews are generally excellent, a few additional points should be made.

First, it is helpful to decide whether one is interested in quality of life or *health-related* quality of life. Quality of life is ultimately important, but as Dr Taylor points out, it is very difficult to measure. Spiritual, cultural, and socioeconomic factors, such as job, salary, and neighborhood, are very important components of quality of life and should be addressed by the practitioner, but they are usually out of our domain to effect change. Health-related quality of life, on the other hand, is more directly applicable to medicine and is what the measures noted by Taylor (primarily health status indices and functional status measures) are designed to evaluate. In fact, a close examination of Ware's framework of the patient functioning dimensions that Taylor mentions reveals only health-related quality-of-life concepts.

I agree with Dr Hume that economics is often a factor in choosing a treatment plan, and that the study by Croog et al[3] introduced a bias by using patients with a median income of $30,000. It is interesting, however, that Hume claims that economic status is a component of functional capacity. While economics does affect some people's quality of life, it rarely affects the patient's functional capacity. None of the common health status indices evaluate the patient's economic status.

On the other hand, costs are often used in the assessment of medical interventions. For example, in a standard cost-effectiveness analysis a ratio of dollar costs to health outcomes is calculated. Contrary to Hume's assertion, there are several methods for evaluating whether the amount of benefit derived justifies the expense of a program. These methods include the standard gamble and time trade-off methods[4] in addition to evaluating societal preferences for medical programs.[5]

There does exist one health status index not mentioned by Taylor that addresses most of the stated limitations. The Quality of Well-being (QWB) scale has been used since the mid-1970s in a variety of research settings. In addition to the five "nonclinical uses of measures of functional status" mentioned by Taylor, the QWB has been successfully applied to cost effectiveness, resource allocation, medical care quality, community health status, and program analysis.[5] Its validity and reliability are well documented,[5] and it is the only measure recently reviewed by McDowell and Newell[6] to also have the properties of a ratio scale, an important feature when used in health policy formulation. While the QWB has, like the Sickness and Impact Profile, limited usefulness in the clinical setting, it has been successful in areas where, as Taylor notes, other scales are limited: following individuals over time, and in patients with rheumatoid arthritis.[7] In fact, it has been successfully applied to a wide variety of disease states.[5]

Quality of life is a key concern of the family physician, and I applaud Hume and Taylor for their efforts. The science of health status indices and functional status assessment is in a state of alchemy, and family medicine researchers should be part of the multidisciplinary effort necessary to expand our ability to measure quality of life and to bring these measurements to the outpatient setting.

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2. Taylor TR: Applying quality of life data in practice, commentary. J Fam Pract 1989; 28:407–411
3. Croog SH, Levine S, Testa MA, et al: The effects of antihypertensive therapy on the quality of life. N Engl J Med 1986; 314:1657–1664
4. Torrance GW: Utility approach to measuring health-related quality of life. J Chron Dis 1987; 40:593–600
5. Ganiats TG, Schneiderman LJ: Principles of cost-effectiveness research. J Fam Pract 1988; 27:77–84
6. McDowell I, Newell C: Measuring Health: A Guide to Rating Scales and Questionnaires. New York, Oxford University Press, 1987
7. Bombardier C, Ware J, Russell IJ, et al: Auranofin therapy and quality of life in patients with rheumatoid arthritis. Am J Med 1986; 81:565–78

GRADUATE TRAINING IN FAMILY PRACTICE

To the Editor:

I was most interested to read the letter from Colin P. Kerr *(Kerr CP: Graduate Training for Family Practice. J Fam Pract 1988; 27:462–464).* A national program of postgraduate training for family practice, in operation in Australia since 1974, has the pattern outlined by Dr Kerr—2 years hospital-based, 2 years of general practice (ie, family practice), and the opportunity to pursue what are termed *advanced training posts* in obstetrics, sports medicine, palliative care, geriatrics, etc.

Continuing formal education is maintained by half-day and full-day "release" schemes, where the "trainees" attend a centrally located centre for further education. There are also computer-assisted education programs accessed through the telephone system using special units (Viatel) or a personal computer and modem.

The program suggested by Dr Kerr is not unorthodox. It works very well, as our Australian experience demonstrates.

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