The "Slow Code": A Hidden Conflict

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Residency is a time when new physicians learn to integrate the biology, psychology, and sociology of human illness. One facet of this ambitious endeavor is learning how best to care for demented and terminally ill patients. It is a challenging, rapidly changing field that remains in the forefront of medical ethics today.[1,2] Questions, however, always seem more abundant than answers. When is aggressive intervention appropriate for the demented and the terminally ill? When is giving comfort alone the kindest action? Who is to decide? Conflict is inevitable. One of the more insidious manifestations of this conflict is the use of the "slow code" designation by resident physicians.

The "slow code" has many names—"intern's code," "Hollywood code," "light blue code." It is used to designate certain demented or terminally ill patients whom the residents have chosen not to resuscitate in the event of a sudden cardiorespiratory collapse. It is a secret designation, circulated only among house officers. In the hospital record the patient remains classified as full code. An informal survey of residents at teaching hospitals in the Seattle area found that between 5 percent and 10 percent of the patients on certain medical wards were classified as "slow codes."

Why do residents find it necessary to use the "slow code" designation? Partially responsible is the legal process of obtaining a no-code status, which can be long and agonizing. Living wills are rarely filled out, and families are difficult to locate during emergency admissions and other crises. Hospital policies concerning code status are viewed by residents as rigid and difficult to apply to the ambiguities of clinical practice. Ethics (or prognosis) committees do not offer opinions in the early hours of the morning, and primary providers are not always immediately available. Clearly, determining a code status uniquely fitted to the individual is often impossible in the hours to days following the admission of a demented or terminally ill patient to the hospital.

Nevertheless, some code status must be assigned during this period when so much important information is absent. It is generally agreed that to initiate cardiopulmonary resuscitation and other aggressive interventions is appropriate as long as these interventions are perceived to be in the patient's best interest. It is easy to see, however, how conflicts might quickly arise over the interpretation of "best interest." Various members of the admitting team—residents, attending faculty, and primary provider—may have very different but equally emphatic opinions. When the resident strongly feels that resuscitation is not in the patient's best interest, and a higher authority (faculty attending, primary provider, hospital policy) disagrees, the resident may feel compelled by conscience to do secretly what he or she feels is right while appearing to conform to authority. A "slow code" is created.

Unfortunately, residents rarely have special talents for making code status decisions in these ambiguous situations. Most fundamentally, they frequently do not know the patient. They have only the oddly skewed view of the person as a patient in acute medical decompensation. At best they have only second-hand knowledge about the patient's baseline level of functioning. In addition, residents are usually strangers to the social millieu that surrounds the patient and have little insight into the patient's personal interpretation of his or her illness and its role in the functioning of the patient's family and culture. Any quality-of-life assessment by the resident is therefore highly questionable.[3] Further, certain residents may have trouble dealing with life-support questions in general because of their inexperience with the issue and because of normal concerns about their own medical competence and mortality.[4] Finally, an overextended resident may hesitate to initiate cardiopulmonary resuscitation because saving the patient means extra work and more lost sleep.

What can be done to prevent these destructive clashes of opinion over code status? Recent attention has focused on certain patients with overwhelming medical illness who may legitimately be given do-not-resuscitate status under the "futility of treatment" argument (ie, a code is not performed because it will not revive the patient).[5] Unfor-

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tunately, data on the exact clinical guidelines to use when invoking the "futility of treatment" argument are scant at present; so although this line of inquiry is very promising, it is currently an area of vast uncertainty where conflict remains inevitable.

The conflicts that spawn "slow codes" likewise will not be reduced by more legislation. Even if risk-management departments send memorandums stating that "slow codes" are an invitation for law suits and must be stopped—and such memorandums may indeed be sent—little will change. A resident's reluctant hand still must operate the defibrillator in a timely fashion. The conditions that foster passive-aggressive behavior will continue to exist; the conflict will have simply been driven further underground.

Conflict between members of the admitting team over code status decisions can be reduced in two relatively simple (although not easy) ways.

First, and most important, there must continue to be an effort to document the wishes of terminally ill and demented patients (or their families) before medical decompensation. Although it is preferable to have a formal living will filled out, it is not absolutely necessary. A simple notation in the chart, signed by the patient or the family (if the patient is incompetent), is sufficient. Few residents, faculty, or hospital administrators would feel compelled to argue with such a directive. The major burden falls on patients' primary providers to be more compulsive about discussing and documenting code status preferences in the outpatient setting. Hospital personnel also need to accept the responsibility of at least raising code status issues during any admission of a patient with a poor long-term prognosis. Doing so is asking a lot, especially in a society that so stongly denies death.

The second major task in decreasing conflict over code status is to improve communication between all parties involved. The process needs to begin with the primary provider and the patient and family. Sensitive communication is vital if the physician is to understand the role of the illness in the life of the patient. With time and the development of greater rapport, patients and primary providers should be more comfortable broaching the often-difficult subject of personal and family preferences surrounding illness and death. Where primary providers are employees of the teaching institution, this communication needs to be fostered by policies that promote continuity of care. Primary providers' written records of patient and family preferences somehow need to be made readily available to admitting teams.

Strong communication is likewise vital among residents, attending faculty, and primary providers, especially when code preference has not been documented prior to an acute change. In the simplest scenario the primary provider may have special knowledge (not documented) that allows the admitting team to reach a consensus. When the primary provider can offer no special insight or is unavailable, faculty physicians need to be able to discuss comfortably with the residents issues of the patient's "best interests" and the "futility of treatment." Personal feelings of frustration, anger, fear, and helplessness need to be shared openly. Hidden prejudices need to be drawn out and examined. This communication provides an opportunity for both the resident and the attending faculty to grow as persons and as physicians, and if undertaken in good faith, consensus is much more easily reached. Additionally, the resident-faculty exchange is excellent role modeling for the time when young physicians must discuss code status options with patients of their own. In some settings a common ground for discussion should be created by formal curricula about ethical decision-making and code status issues for physicians at all levels of training within the teaching institution.

Conflicts over code status issues need to be recognized as inevitable and healthy. Dealing with conflict among members of the admitting team in a straightforward manner allows for the development of consensus and promotes a sense of team unity. It is that sense of unity—of all involved pulling together for the good of the patient—that will ultimately put an end to the "slow code."

References

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Preserving the Passion. *Phil R. Manning, Lois DeBakey. Springer-Verlag, New York, 1987, 297 pp., $350.*

This book is of variable interest to family physicians, although it contains a number of valuable and interesting perspectives on the process of caring for patients, attitudes of physicians, personal approaches to learning, and keeping up with current medical practice. It is based on 600 interviews with a galaxy of "star" physicians, and contains a number of personal essays by them. These essays are the least successful portions of the book, being mostly statements of personal beliefs and descriptions of the amazing effort, discipline, and time commitment required for those at the top in medicine. (One physician regularly reads his medical journals from 4 AM to 6 AM every day!). I did not find this necessarily admirable, since these may not be the correct role models for generalists.

There are some good short chapters dealing with areas not often addressed in medical books: collegial networks, personal information resources, how to do a consultation and learn from one, and how to manage relationships in a practice.

This book is for dipping into and adding insights into one's practice, and would make an excellent gift to a young physician.

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Setting Limits: Medical Goals in an Aging Society. *Daniel Callahan. Simon & Schuster, New York, 1987, 256 pp., $18.95.*

Occasionally one comes across a book so provocative that the reviewer risks failure in conveying its impact. Such a book is *Setting Limits,* by Daniel Callahan, founding director of the Hastings Center. This book should be a high priority for anyone interested in health policy, in the care of the elderly, in medical ethics, or in the future of our medical care system, which surely includes most readers of *The Journal of Family Practice.*

Subtitled "Medical Goals in an Aging Society," *Setting Limits* explores life, meaning, significance, aging, and death. In the discussions about this book, one will hear the basic argument boiled down to making age a criterion for withholding medical treatment, but such a reduction trivializes a perceptive and compassionate view of what it means to be born, live, age, and die in our society.

Callahan begins by reconstructing the ends of aging: what should the elderly contribute to society? His view is that the elderly have a responsibility to "pass life and culture on to the next generation." Next comes a discussion of the appropriate ends of medicine in the elderly. He argues for the achievement of a full and natural life span, not the extension of life as such. Callahan deals specifically with the seduction of an open-ended and technologically oriented medicine that denies the appropriateness of death at the end of a long life.

Chapters on what the young owe the old and on allocating resources to the elderly consider the powerful moral arguments for apportioning resources fairly between the generations. These chapters lead to the crucial penultimate chapter on care of the elderly dying, in which Callahan addresses squarely the implications of his reasoning on the clinical care of the elderly. The final chapter discusses some of the barriers ahead if his proposals are to move toward acceptance.

Callahan presents his views with clarity and profound perception. He writes beautifully; the style is compact yet thoroughly readable. Readers should beware of anyone attempting to summarize this book in a few polemical sentences or paragraphs. It is all too easy to select some small section out of its context and ridicule Callahan's arguments (as I have already heard and seen done). The presentation is complex yet subtle; each sentence is all of a piece with the whole, and it is the whole that compels respect and attention.

Already a flood of reviews are in print (even in the *Wall Street Journal*), the topic and the arguments tend to take center stage in conferences on aging, and snippets are being quoted in manuscripts and grant applications. *Setting Limits* will be one of the key books in medical ethics and care of the elderly for the next decade; it is essential reading.

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Family-Centered Medical Care: A Clinical Casebook. *William J. Doherty, Macaran A. Baird (eds). The Gullford Press, New York, 1987, 302 pp., $30. ISBN 0-89862-070-8.*

The essence of clinical practice is the relationship and interaction between the physician and the patient. As a discipline, family practice has attempted to consider the role of the patient's family in the clinical practice of medicine. In this book Doherty and Baird have compiled a series of vignettes illustrating the potential rewards and difficulties of "working with families."

The purpose of this text is to provide the reader with a view of "family-centered practice" as experienced by physicians throughout the United States. The introduction provides a theoretical framework describing five levels of physician involvement with families. The cases are organized and briefly analyzed in the context of this framework.

This text provides a useful orientation to family-centered practice for the physician who is interested in learning about this approach to medical care. The case histories make it easy for the physician to view the application of this model in the hands of experienced practitioners. The major limitations of this book are inherent in its casebook structure. It is best used to complement other more theoretical texts in this area, such as Doherty and Baird's *Family Therapy and Family Medicine: Toward the Primary Care of Families* (The Guilford Press, 1983) or Janet Christie-Seely's *Working With Families in Primary Care* (Praeger, 1984).

Unfortunately, physicians experienced in family-centered medical care or attempting to teach these concepts to residents and students may find this book of limited usefulness. The case material often does not have the detail to illustrate theoretical concepts effectively. The presence of basic family

continued on page 434

continued from page 432

data, such as genograms, for each case, would make this book an excellent text for teaching the family-centered approach to care. Its absence limits its usefulness in the educational setting.

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Primary Care Medicine: Office Evaluation and Management of the Adult Patient, (*2nd Edition*). *Allan H. Goroll, Lawrence A. May, and Albert G. Mulley, Jr. J. B. Lippincott Company, Philadelphia, 1987, 1001 pp., $49.50. ISBN 0-397-5826-0.*

The first edition of this book was the first in what has become a "hot" new textbook field, primary care internal medicine. The book "attempts to delineate rational approaches to . . . both common and 'must not miss' clinical problems in office practice," for those who provide primary care to adults. Competitors in this arena that I examined for comparison include *The Principles of Ambulatory Medicine* (2nd edition, by L. R. Barker, et al), *Office Practice of Medicine* (by W. T. Branch), and the *Textbook of General Medicine and Primary Care* (by J. Noble).

The content of this book is extremely relevant to family medicine, though the orientation is inevitably that of internal medicine (ie, physician rather than patient and family centered and diagnosis emphasized over patient management). The text is highly readable with each problem addressed in about three to five pages, including a short, annotated bibliography. Topics cover a broad spectrum of primary care issues including screening and evaluation of symptoms as well as specific clinical entities. The problem-oriented approach is helpful, and each section follows a similar pattern that usually includes the consideration of tests in terms of their contribution to decision making. (The continued recommendation of the use of transtracheal aspiration in the management of ambulatory pneumonia is one amusing exception!)

There are many useful tables, but few illustrations (hardly surprising given the modest cost for such a comprehensive text). Though the second edition is much improved, the main limitation for the practitioner remains that there is insufficient detail to guide management with complex diagnostic and therapeutic problems.

The audience best served is the student, as this book provides a useful orientation toward problem-oriented thinking for the ambulatory setting. Although it may also be recommended for the practitioner and resident as an overview for most problems, I prefer the book by Barker et al because it does provide adequate detail and is useful both as a reference and resource for teaching. If you can stretch your budget, I would recommend both books.

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Questions & Answers on AIDS. *Lynn Rober Frumpkin, John Martin Leonard. Medical Economics Books, Oradell, New Jersey, 1987, 190 pp., $19.95 (paper). ISBN 0-380-75467-3.*

Fears about AIDS have permeated the consciousness of America and the world. This excellent book was written in response to a perceived "lack of easy-to-read, accessible literature that would enable health workers to acquire general but comprehensive information about AIDS." The author's goal was "to educate the health care worker by answering questions [the authors] that might arise concerning AIDS." They have succeeded admirably. The foreward, written by Paul Volberding and Michael McGrath, authorities on AIDS, provides endorsement of the book by the AIDS medical establishment in the United States.

Written in a question-and-answer format, which is very readable, the book's ten chapters provide concise information on every aspect of the disease, including the definition and origins of AIDS, manifestations, risk groups, modes of transmissibility, antibody positivity, protecting the individual and the health care worker, epidemiology, research and funding, resource centers nationwide, and eth-

ics. A helpful glossary is included, and extensive references at the end of the book are keyed to individual questions. Three very good electron micrographs illustrate the virus and several tables help to organize the information. Addresses and telephone numbers of resource centers for AIDS information and support are included.

The authors remind us that "much remains to be learned before the final word on AIDS is in." In the meantime, this book will greatly assist all health care workers to answer their own and their patients' questions about this terrible epidemic.

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The Physician as Teacher. *Thomas L. Schwenk, Neal Whitman. Williams & Wilkins, Baltimore, 1987, 203 pp., $21.50 (paper). ISBN 0-683-07613-2.*

My initial reaction in flipping over the pages of this small book (which is just too big for the pocket of a white coat) was that it was unlikely to provide much useful information for the novitiate or experienced teacher. There were not that many references, and the style seemed simplistic, so it could not be very academic!

After reading it, I feel very differently and strongly recommend it to all teachers and learners in family medicine. The main objective of the book is to help full- and part-time academic physicians improve their teaching skills, given the unproven but probably correct hypothesis that in these times most medical school faculty are hired to do research and their teaching skills are an afterthought. The authors' basic premise is that teaching is essentially a communication and interpersonal skill that is often learned by physicians through their patient contacts.

The book is organized into two parts. The first deals with communications, teacher-learner relationships, teacher and learner roles, and methods of giving different types of feedback. The second part describes a number of teaching situations: the lecture, group discussion, teaching rounds, morning report, bedside teaching, and education in the ambulatory care setting. Tips and techniques abound in this section. Not only did I find this most useful in realizing that one can teach an old dog new tricks, but it made me think about ways in which teaching in our own program could be enhanced. In fact, reviewing chapters of this book in sequence would provide an excellent basis for a series of faculty development seminars.

The book is well organized, visually pleasing, and clearly printed for the benefit of aging professors. Illustrations are few, but telling in their message and the references, adequate and useful.

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continued from page 360

On the other hand, it is not surprising that a program such as that described by Balaban et al would show no impact on the measured outcomes. There were only about three visits per year to a patient population who averaged five diagnoses apiece and who experienced more than 25 percent mortality in two years. Patient satisfaction and the development of meaningful physician-patient relationships may have been impeded by the presence of "a program physician and nurse . . . , family practice residents, medical students, nursing students, and other health care providers." The large number of visitors and outcome measurements may also have introduced a substantial Hawthorne effect.

The reasons for making house calls in clinical practice are only indirectly related to a global aim of improving "the function and well-being of the patient and the family." Convenience is often salient, quite justifiably when the patient is legitimately homebound and the physician's travel time is not unduly burdensome. It is also useful to see the patient's physical and human environment. These are humane, people-oriented considerations that do not lend themselves well to the type of quantitative analysis undertaken by Balaban et al.

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1. Hanchett E, Torrens PR: A public health home nursing program for outpatients with heart diseases. Public Health Rep 1967; 82:683–688

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

We agree with Dr. Gillette that too frequently interpretations of reported results go beyond the conclusions of the investigators of what most would consider a reasonable interpretation of the data. We share this concern but believe that the advantages of reporting study results to the scientific community outweigh the disadvantages of potential inappropriate interpretation or use of the data. Furthermore, it has increasingly been recognized that not publishing clearly stated results is harmful to the continuity of scientific investigation and to the public good. We believe the limitations of our study were clearly stated in the paper.

We also agree with Dr. Gillette that home visits may have subtle effects easily overlooked and difficult to quantify, and that convenience is worthwhile paying attention to for patients who are legitimately homebound, if physician's travel time is not unduly burdensome. Our study does not suggest that physicians should stop providing care to immobile patients or assessing a patient's living situation; both are appropriate practices essential for high-quality care.

Although we have used the Solomon four-way design to control for possible Hawthorne effects in other studies we have carried out, we could not do so in this follow-up study. We would expect, however, that any Hawthorne effects would exaggerate benefit rather than mitigate it.

The US population is aging and home care services, including physician visits, are increasingly available. Despite the feelings shared by many of us that such care is humane and appropriate, however, there is little evidence that it is efficacious, let alone cost effective. If one accepts that health resources are finite, then, in our view, there must be continued rigorous scientific evaluation of medical and health care interventions, particularly potentially expensive ones, to determine benefit (or possible harm) in specific populations or subgroups.

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MANAGEMENT OF PHARYNGITIS

To the Editor:

In a recent editorial Dr. Wald recommends the standard throat culture results as a guide to the management of cases of acute pharyngitis (*Wald ER: Management of pharyngitis re visited. J Fam Pract 1988; 26:367–368*). She further states that "if the results of a properly obtained throat culture are negative for GABHS [group A β-hemolytic Streptococcus], antibiotic therapy should be withheld or promptly discontinued if it had been presumptively initiated."

Her recommendations, however, fail to address the issue of the false-negative rates of throat cultures. It may be reasoned that the person with a false-negative test will subsequently go on to develop a more clinically positive picture and will later be found to be culture positive, but from most patients' point of view (and my own), this is not a satisfactory approach to the problem. Also noted in the same editorial is that many of the patients included in this recent resurgence of rheumatic fever "did not have antecedent clinical illnesses that were remarkable or suggestive of GABHS disease." It is not hard for me to imagine, then, a patient presenting with a sore throat that appears clinically normal, has a negative culture (or other test for GABHS), yet is truly colonized and infected with GABHS. This same patient, if untreated, may well seek out another physician in a day or two, who repeats the test for GABHS, finds it to be positive, and treats the patient. The result, however, is an unhappy patient who is unhappy because he or she had to pay for two physician visits to be treated properly.

Most patients do not understand the scientific rationale behind our approaches to their illnesses, and it is practically impossible to explain these to most of them within a reasonable amount of time (ie, five minutes). I have found that it requires a repetition of the concepts about three times and in about three different ways before they get the gist, and even then I am not quite sure they believe what I am saying.

In addition, Dr. Wald would say that it is good that this patient went without treatment for a couple of days because this enabled the patient to build up more antibody against the

continued on page 438

continued from page 436

infection, which will subsequently reduce recurrence. I am surprised that she so quickly embraces this theory on such minimal scientific evidence. Finally, Dr. Wald also failed to address the other causes of pharyngitis such as Hemophilus influenzae, Branhamella catarrhalis, Mycoplasma, and TWAR (the newly discovered Chlamydia organism that causes acute respiratory tract infections and also pharyngitis).

Personally, I treat all patients who have the complaint of sore throat with an antibiotic (usually amoxicillin or erythromycin, depending on the age of the patient and the clinically suspected illness). For those in whom I suspect a viral illness (besides mononucleosis), I tell the patient I suspect that they have a viral infection and that viral infections are not cured with antibiotics. I then tell them that I am prescribing an antibiotic for them just in case they truly do have a bacterial infection. If the patient requests a test for Streptococcus, I obtain one, but I explain to the patient that there are false-negative results to these tests, and for this reason I would still like to prescribe the antibiotic. Most patients can understand the concept of "false negative." Those who cannot are also unable to understand the concept of nontreatment. The only problem that I have had with this approach is one or two patients who actually had mononucleosis and acquired rashes from the amoxicillin. With Dr. Wald's approach, which I used when first in practice, the number of patient complaints were distressing: Some were just not getting better and wanted treatment but did not want to spend more money to be seen again, some had subsequently seen another physician who either told them they had streptococcal pharyngitis or obtained a culture or test and then told them they had streptococcal pharyngitis and treated them appropriately.

The art of medicine is finding creative ways to meld scientific reality with the realities of day-to-day practice. After five years of practice in the real world, I much prefer the approach I am using now.

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

Dr. Walker raises the issue of falsely negative throat culture as a deterrent to the development of treatment strategies for patients with pharyngitis based on throat culture results. I am familiar with the commonly quoted figure of 10 percent as the false-negative rate of throat cultures performed by nurse practitioners. I am grateful for this opportunity, however, to stress that I regard the performance of a throat culture seriously in order to maximize its value as a diagnostic test. Accordingly, with the tongue depressed, I swab both tonsillar pillars and the posterior pharynx, and I try to get the swab into and out of the mouth without touching the tongue or the buccal mucosa. Often this requires the assistance of a nurse or parent to help restrain the child. I believe my rate of false-negative throat cultures is close to zero. In the case of a persistently symptomatic patient with a negative throat culture, the throat culture can be repeated. This is unlikely to be required very often. Two negative throat cultures are strong evidence against a streptococcal cause for the sore throat. Most persistent sore throats are likely to be caused by a virus, especially adenovirus or Epstein-Barr virus.

In contrast to Dr. Walker, I find it is easy to explain the rationale for performing throat cultures to patients and parents. A major part of our responsibility as physicians is to educate our patients concerning health care. In our community patients are surprised when clinicians do not perform throat cultures prior to initiating antibiotics for sore throat.

My editorial was invited after I was asked to review the article entitled "Prevalence of Chlamydia trachomatis and Mycoplasma pneumoniae in Children With and Without Pharyngitis," which appeared in the same issue of *The Journal of Family Practice* (*Reed BD, Huck W, Lutz LJ, Zazove P: J Fam Pract 1988; 26:387–392*). As noted in this article and in another cited in the editorial, Mycoplasma pneumoniae has not readily been shown to be a cause of pharyngitis. In addition, neither Hemophilus influenzae nor Branhamella catarrhalis are recognized as pharyngeal pathogens; rather, they are regarded as normal flora. Nontypable H influenzae are found in the throats of 60 to 70 percent of normal children.

Personally, I almost always culture the throats of children with pharyngitis as a prelude to planning treatment. Overall, on an annual basis 90 percent of sore throats are not caused by GABHS. Although others have calculated cost-benefit ratios of treatment strategies with and without the performance of carefully performed cultures, I find it intellectually satisfying to carefully perform laboratory tests in selected patients, to make clinical and laboratory correlations, and to reserve antibiotic therapy for appropriate indications.

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LETTERS TO THE EDITOR

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.

MENTAL HEALTH CONSULTATION AND REFERRAL

To the Editor:

It was with great interest and pleasure I read the article "Screening for Psychosocial Problems in Primary Care" by Harold D. Hase, and Joseph A. Luger (*J Fam Pract 1988; 26: 297–302*).

I certainly support the use of the Multifactor Health Inventory as an important diagnostic screening tool for the family physician and would wish and hope that the great number of primary care physicians serving individuals, couples, and families would be interested enough to make use of the inventory. I think, however, that it is important to be realistic in our expectations of the extremely busy family physician.

In as much as many family physicians have sufficient office space that is not always required by the practice itself, there may be value in supporting the concept that family physicians buttress their armamentarium of diagnostic sophistication in terms of mental health by considering rental of that additional space to a qualified mental health practitioner. A busy physician would thus have available a much more comprehensive diagnostic and treatment capability in the area of mental health. It enables the physician to obtain consultation regarding the advisability of medication therapy, screening, and ongoing mental health therapy without going through the process of referring to persons who are located outside of the practice. The family physician would be able to introduce the patient to the specialist personally and to enunciate the issues as perceived by the physician so that exactly what is sought in consultation can be made quite clear to the consultant and the patient.

As a specialist who provides the service to a very busy and complex family practice that includes not only a family physician but also an orthopedic surgeon and an obstetrician-gynecologist, it has been my experience that in addition to the obvious benefit of providing a more effective and comprehensive range of services to the patient, this arrangement also enables the referring physicians to have available the kind of clinical data which are necessary to assess, evaluate, and identify their own areas of relative diagnostic acuity and weakness. This results in a much more effective referral process and is a predicator of patient success and recovery. Patients routinely report very positively about this approach to the important area of mental health consultation and referral, and as the consulting therapist, it has been my experience that we achieve a much more appropriate referral practice and that we provide a more effective level of service to patients.

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GRADUATE TRAINING FOR FAMILY PRACTICE

To The Editor:

In response to Drs. Ferentz et al (*Ferentz KS, Sobal J, Colgan R: Family medicine residency training—Three or four years? J Fam Pract 1988; 26:415–420*), I would like to urge a broader change in residency training in family medicine than they considered. To add merely an additional year of training, regardless of the areas covered, is most likely to be unacceptable to young physicians, most of whom have staggering debt loads. The poor showing of most family medicine residency programs this year in the Match tends to support this view.

I favor a four-year minimum training period for family physicians, but training must become more efficient. The major source of inefficiency in current training programs is that we spend too much time covering all the major skill areas for all residents despite the obvious fact that many will abandon up to one third of these skills in their first year out of training. The plain fact is that we are spending too much effort hammering the full panoply of knowledge and skills into physicians who simply do not yet know what they want to do, ie, what specific shape their own practice will take.

This is not necessary if we are willing to consider more unorthodox alternatives. I propose offering a split training period. The initial two years would be spent in a way very similar to existing curricula but with more emphasis on basic ambulatory care skills. The next two years would be spent in actual practice at a primary care practice site of the resident's choosing at full practice-level compensation. Continuing formal education would be maintained and monitored by regular online computer contact with the residency program. One year of residency credit would be given for the two years of practice experience if certain knowledge and skill performance criteria are met. In this time the physician would gain firsthand knowledge of what practice style realistically suits his or her needs. The final year of training would be back at the residency program site and would consist of electives designed to meet the specific skill needs of the physician for a personally selected practice-style, eg, more obstetrics, orthopedics, hospital-based care, or whatever experience is known to be needed for an effective practice for an individual. We should be pre-

continued on page 464

continued from page 462

pared to extend this final training period as long as necessary to accomplish the specific objectives identified.

In this way I believe we could obviate the enormous waste of which many of our training programs are guilty, and we could do this in a way to conserve our residents' most valuable resources—their time, their lifestyle, and their assets. As the nature of the crisis in modern primary and family-oriented care escalates, let us begin serious discussion of truly novel solutions.

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RETURN TO WORK CERTIFICATION

To the Editor:

I would like to respond to the commentary by Robert A. Fried, concerning absenteeism certification (*Mayhew HE, Nordlund DJ: Absenteeism certification: The physician's role. Fried RA: Commentary. J Fam Pract 1988; 26:651–655*).

I have found from my experience in a combined rural and industrial practice of some 33 years that the best solution for the return to work certification is not one suggested by Fried. If the worker comes to me for permission to go back to work without having evidence of current illness, I prefer to examine the patient, determine that he or she is disease free, and then give a statement which indicates the patient has reported such an illness and by examination he is now free of disease and able to return to work.

This does push the resolution of the problem back to the managers, where I think it rightly belongs. The manager must then determine whether this individual is "goldbricking" or whether he is an habitual offender.

I am sure management has discovered many years ago that if they were to grant certain days off for "illness" that did not require medication supervision, their rate of absenteeism would probably increase 30 percent. It has been my experience that the worker on the assembly line will willingly use very flimsy excuses to account for his absenteeism, in many cases because he may be paid 90 percent of his take-home pay when he stays home (as a number of workers have reported to me); under these circumstances it does not pay him to run his automobile to the job and back.

In contrast, however, the individual farming who is totally responsible for his own work will still be out doing chores even if he has pneumonia or a fracture, which normally would require being housebound.

In summary then, I believe that the managing of this problem belongs with industry and that incentives by industry might well be provided for those individuals by rewarding them for an excellent work record and penalizing those individuals who are found to be malingerers.

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

Dr. Ash and I agree that having physicians certify only what they personally observe puts the problem back in the lap of personnel managers. It still fills the physician's office with healthy people and costs management something for the return-to-work examinations—possibly more than they would spend if all malingerers took unfair advantage of sick-leave policies.

I am not as sure as Dr. Ash that management has really thought through this problem. If the system was working so well, then why did Mayhew and Nordlund find so much dissatisfaction with it? It is precisely the message of the research study—and, I hope, the discernible point of my commentary—that the conventional wisdom about absenteeism certification is probably wrong. That is why a commonplace event—a patient asking a physician for a medical excuse from work—evokes research-

continued on page 465

continued from page 464

able questions that are worthy of our attention.

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ROUTINE MATERNAL SERUM α-FETOPROTEIN TESTING

To the Editor:

In the recent article and commentary on routine serum maternal α-fetoprotein (MSAFP) testing, both Weiss[1] and Osborn[2] make excellent points regarding this new screening procedure: There are multiple ethical considerations that have been inadequately addressed, the emotional stress of a false-positive MSAFP should not be underestimated, and screening tests that are developed and standardized in a subspecialty clinic may have limited applicability in the general population. The article and accompanying commentary, however, contain multiple statements and concepts that are very misleading, and far too much weight is given to the research findings.

1. "It has been reported that less that 20 percent of women who have an elevated MSAFP level will be delivered of an infant with a congenital defect. The specificity of low MSAFP levels is even lower. . . ." This statement confuses specificity with positive predictive value. As illustrated in Figure 1 of Dr. Osborn's commentary, stating that 20 percent of those with a positive test have the disease describes positive predictive value, not specificity. To make matters worse, it is unclear where the 20 percent figure comes from, as both referenced articles[3,4] claim a 5 to 10 percent positive predictive value for neural tube defects, a specificity greater than 90 percent, and a sensitivity of 80 to 90 percent.

2. "All current MSAFP screening recommendations are based on data gathered in practices of obstetricians whose patients have higher risk profiles than patients typically managed by family physicians." If the recommendations *were* based on data gathered on high-risk patients, then the author's conclusions would be justified. Those studies referenced by the author,[3–5] as well as others,[6,7] however, were prospective evaluations of large (10,000 to 50,000) numbers of patients from populations comparable to those typically managed by family physicians. In addition, the recommendations of at least two national groups, the American College of Obstetricians and Gynecologists[8] and the American Society of Human Genetics,[9] have recently stated that MSAFP screening for Down's syndrome is investigational. Thus, the major positive finding of the study, that of a low positive predictive value for a low MSAFP, agrees with current screening recommendations.

3. Table 1 in Dr. Osborn's commentary contains multiple errors. First, it incorrectly calculates prevalence in the high prevalence case as 10 percent instead of 11.1 percent. It also incorrectly calculates positive predictive value in the low prevalence case as 2 percent instead of 2.2 percent. In addition, the specificity varies in the two examples: the specificity in the high prevalence case equals 55 percent, and the specificity in the low prevalence case is 60 percent. To compare the effect of prevalence on predictive value, both sensitivity and specificity must be constant. Though the errors do not introduce any substantive changes, the tutorial becomes confusing and therefore difficult for a novice to follow.

4. "MSAFP testing is extremely nonspecific, resulting in the application of potentially harmful technological interventions, such as amniocentesis. . . ." Most authorities suggest that an amniocentesis be done on all women older than 35 years; therefore, it is reasonable to perform an amniocentesis on any person who has a risk of Down's syndrome or other chromosomal abnormality comparable to a 35-year-old woman (ie, about 1:270). The purpose of a screening test such as the MSAFP is to sufficiently increase the probability of disease to justify the more definitive (and usually more expensive and dangerous) diagnostic test.

For example, the articles referenced by Weiss and the recent California experience[10] show that despite its low positive predictive value, a low positive MSAFP can increase the risk of Down's syndrome to 1:121. A positive low test therefore places a woman at a higher risk for a Down's syndrome infant than women who are aged 35 years and older, thus justifying the amniocentesis. According to these data, denying an aminiocentesis to a woman with a low positive MSAFP while recommending an amniocentesis to a woman over the age of 35 years is inconsistent.

5. The study lacks a sufficient sample size. In the recent California experience with nearly 200,000 patients, 5 percent of the patients had an abnormal test (approximately one half had high values and one half had low values). Of those, 65 percent were confirmed after checking the ultrasound and a repeat test, and about 70 percent of the women with the positive test had an amniocentesis. Of the approximate 2,000 amniocenteses done because of a low α-fetoprotein level, 16 cases of Down's syndrome were diagnosed and 12 other chromosomal abnormalities found, for a ratio 1:121 for Down's syndrome and 1:69 for all chromosomal abnormalities. It is not surprising that Weiss's review (89 total patients and 14 patients with a low level) showed no positives.

The MSAFP is a screening test for neural tube defects and may be useful in screening for chormosomal abnormalities. That a university medical center laboratory based its normal values on an abnormal group is a lesson we can all learn from but does not affect the validity of a properly performed MSAFP screening program.

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continued on page 469

continued from page 465

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The preceding letter was referred to Dr. Weiss and Dr. Osborn, who respond as follows:

Dr. Ganiats raises several issues regarding my report of one year's experience with routine maternal serum α-fetoprotein (MSAFP) testing in a family practice. It is of note that our previous publications on this topic also generated a significant response from readers.[1–7] Obviously, MSAFP testing is controversial, both from methodologic and ethical points of view.

Dr. Ganiats' comments refer both to my article and to Dr. Osborn's accompanying commentary. I will limit my remarks to those issues directly pertaining to my article.

Dr. Ganiats correctly points out that my sample size was small. Indeed, the report included only 89 pregnancies; therefore, the study was clearly identified as a preliminary first-year report.

Nonetheless, simple power calculations* indicate that to reliably detect an event, such as an abnormal MSAFP level, which is expected to occur in 5 percent of cases, only 31 total cases are needed to achieve an 80 percent chance that at least one abnormal MSAFP level will occur. A sample size of 89 subjects gives a 99 percent chance of detecting at least one subject with an abnormal MSAFP level. Thus, although preliminary, my report offers a high degree of certainty that the rate of abnormal MSAFP in our family practice was, in fact, considerably lower than would have been expected from reports in the literature, which were based on experience in the practices of obstetricians.

Dr. Ganiats suggests that MSAFP protocols generated in obstetrical practices are applicable to family physicians' practices because obstetricians' patients are comparable to family physicians' patients. I disagree. Most family physicians evaluate their prenatal patients with both formal and intuitive risk-assessment techniques. These same family physicians refer significant percentages of their prenatal patients to obstetricians because of factors or conditions that might increase pregnancy risk; these same cases might be routine for an obstetrician. The growing concern over liability issues has increased the tendency for such referrals to be made.

Thus, many family physicians do not provide prenatal care to patients with gestational diabetes, preeclampsia, history of prior adverse pregnancy outcomes, and a variety of other conditions. How many of the obstetricians whose articles Dr. Ganiats cites routinely refer away patients with these same risk factors? The answer, of course, is none. Patients who family physicians consider to be at increased risk are regularly managed by obstetricians; therefore, research based on the practice experience of obstetricians (who care for average and high-risk patients) is not necessarily applicable to the low-risk populations seen by family physicians.

Finally, I agree with Dr. Ganiats that given our current level of knowledge, aminocentesis should be offered to women with low MSAFP levels if their risk for trisomy 21 is equal to or greater than the risk in an otherwise low-risk 35-year-old. The literature supporting this approach was cited in my manuscript.

It is important that readers not interpret my report as suggesting that MSAFP testing is inappropriate. In fact, I recommend the test to all of my prenatal patients. The lesson that should be learned from my study, however, is that when interpreting MSAFP (or other) test results, family physicians should be certain that the values used to determine normality were derived from a population of patients similar to those seen in a family physician's practice.

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$beta = (1-p)^n$ where p = event rate, n = number of cases, 1-beta = power

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continued on page 550