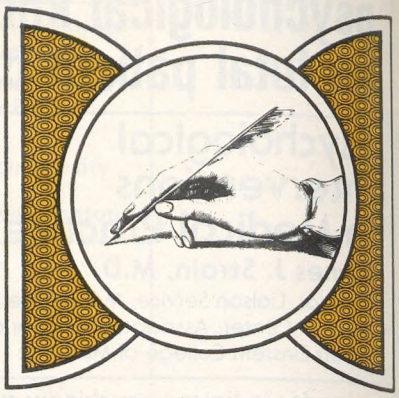
Letters to the Editor



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.

Medical Ethics Teaching

To the Editor:

I very much enjoyed Dr. Carson's and Dr. Curry's treatise, "Ethics Teaching on Ward Rounds" *(Carson RA, Curry RW Jr: J Fam Pract 11:59, 1980).*

What troubles me about their discussion is the legal vacuum in which the rounds were made.

In two of their three case histories, the authors discuss making life and death decisions for mentally incompetent patients. On page 60, they note that the prognosis decision "is a medical question best answered by a physician," and that the life prolongation decision "is a personal question best answered by the patient . . . or by a responsible family member if the patient is incompetent."

The accuracy of those statements depends *entirely* upon in which of the 50 states the questions are asked.

I trust that all thanatology teaching stresses that unfortunately the long arm of the law is the only authority for determining if pulling the plug is sound medical practice or negligent homicide.[1]

The leading, published American court expressly endorsing the authors' judgment is New Jersey in the famous decision, *In the Matter of Karen Quinlan,* in which that state's highest court properly placed medical ethics where it rightly belongs—within the physician-patient-family relationship. As the *Quinlan* Court stated, to place such responsibility in the courts is a "gratuitous encroachment upon the medical profession's field of competence."

But in the leading contrary decision, the highest court of Massachusetts has demanded that only courts make grave treatment decisions for incompetent patients. In Superintendent vs Saikewicz, that state in 1977 ruled that the court and not physicians must make such decisions, in that life and death decisions "are not to be entrusted to any other group . . . no matter how highly motivated or impressively constituted."

The authors stress an "ethicist-attending physician team" for making such awesome judgments. But in at least Massachusetts and the District of Columbia where that approach is contrary to law, the hospital's counsel and risk manager had better join the team.

My prejudice is completely in support of the authors' reverence for the sacred physician-family re-

Continued on page 621

Continued from page 618

lation. But the student-physician must be acutely aware of the purely legal context in which medical ethics are learned and applied. Otherwise, the physician may find the local probate court judge breathing hotly down his/her white coat.

Douglas Savage
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Reference

1. Savage D: After *Quinlan* and *Saikewicz*: Death, life, and God committees. Crit Care Med 8(2):87, 1980

Hospital Practice of Family Physicians

To the Editor:

The article by Slabaugh, Ringiewicz, and Babineau, "The Hospital Work of a Family Practice Group in a Medium Size Community in New England" *(J Fam Pract 11:287, 1980)*, provides an interesting analysis of inpatient family practice care in a private practice community setting.

The percentage of consultation in their study of 13.7 percent of 997 admissions reflects what I would expect in a community hospital primary care setting. The 57.9 percent rate of consultation reported by Dr. Medley and myself in our review of a residency inpatient service reflects the care of surgical patients by the family physician with consultations having been obtained for procedures such as myelograms and laminectomies.[1] Also, our consultation rate of 46.5 percent of medicine patients was largely due to procedural consultations for cardiology, pulmonary, and gastroenterology service. Dr. Slabaugh's data did not include surgical patients as these were transferred to primary management by appropriate specialists, thus keeping their percentage of consultation figures at a lower level. Our pediatric consultation rate of only 16.1 percent probably represents a figure reflective of the primary care type setting as these patients for the most part had diagnoses that did not require specialty procedural consultation.

Further distinction in the two studies is that ours was done within a teaching hospital and many of our consultations were often obtained for teaching purposes.

At this time, we unfortunately have no way of separating out our data for each medical condition for the ratio of ambulatory encounters per admission, but I do agree this is the most valid way of assessing an admission ratio.

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Reference

1. Medley ES, Halstead ML: A family practice residency inpatient service: A review of 631 admissions. J Fam Pract 6:817, 1978

Management of Urinary Tract Infections

To the Editor:

As a 20-year general practice physician, I must take sharp exception to several concepts and statements made by Dr. Jack Froom in his lead article of the September 1980 *Journal of Family Practice* entitled, "The Spectrum of Urinary Tract Infections in Family Practice" *(11:385, 1980)*. He states "The

Continued on page 623

Continued from page 621

diagnosis requires the demonstration of more than 100,000 bacteria colonies per ml in a freshly voided specimen." And also "Asymptomatic bacteriuria need not be diagnosed or treated except in pregnant women."

I do a culture screen on a midstream urine from both male and female patients when proteinuria or pyuria are present regardless of symptomatology, and have found repeatedly more than 100,000 colony count of Escherichia coli or other pathogenic organisms. I treat individuals who have minor dysuria with minimal pyuria and no proteinuria with colony counts of less than 100,000 and often find after one week of treatment pronounced pyuria as well as white blood cells and hyaline casts.

The most common cause of urinary infection in female children is poor toilet technique. Children wipe from the anus forward over the perineum, thereby contaminating the introitus with entrance into the urethra with E coli organisms; women accomplish the same contamination using pads for menses. The most common cause of cystitis in the adult female is undoubtedly intercourse because of scraping of bacteria off of the skin into the opening of the urethra during thrusting. I certainly do not tell women to stop having intercourse since they would not follow such instructions anyway. I advise them to urinate within 15 minutes after intercourse to wash the organisms out of the urethra.

I have never found any evidence to indicate that voluntary avoidance of urination and the distention of the bladder is responsible for infection in any way. On the contrary, individuals who require frequent voiding during the day and awaken several times during the night are much more apt to have infection because of urethral stenosis. Contrary to Dr. Kunin's statement, I feel that urethral instrumentation is important for those individuals who have persistent or recurring urinary infection; when it is found that the urethra is constricted to less than 25 French caliber, dilatation on a bimonthly basis should be performed until the urethra remains dilated to more than 30 fr.

Every physician who sees patients for urinary infections should do a microscopic urinalysis, perform screening cultures of pyuria but not bacteriuria, and be able to do a diagnostic-therapeutic urethral dilatation when indicated.

There is no disagreement that "asymptomatic" bacterial endocarditis should be vigorously treated because of possible sequelae, and I feel that the kidneys are just as important for long-term health.

Richard Hopkins, MD
Columbus, North Carolina

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

I appreciate Dr. Hopkin's careful reading of my article entitled "The Spectrum of Urinary Tract Infections in Family Practice" *(J Fam Pract 11:385, 1980)* and his thoughtful comments. The criterion of 100,000 colonies of bacteria per ml of urine as a requirement for the diagnosis of urinary tract infection is based on observations by Kass that 95 percent of cases of clinical pyelonephritis had counts which exceeded that number. In his series most contaminated specimens grew

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out less than 10,000 colonies. These figures appear to have been widely adopted as criteria for the diagnosis of urinary tract infection. In an article published concurrently with mine, however, Dr. Stamm et al report that infection may be present in women with the acute urethral syndrome with counts of less than 100,000.[1]

I agree that fecal contamination of the vaginal introitus can contribute to urinary tract infection and that sexual intercourse may be an additional etiological factor. At least one study, however (Buckley et al, Ref. No. 22), failed to demonstrate the therapeutic value of postcoital voiding. Dr. Hopkins may wish to refer to work by Lapides (Ref. No. 20) and Adatto (Ref. No. 21) concerning evidence that over-distension of the urinary bladder and voluntary avoidance of urination are important pathogenic mechanisms. Studies quoted in my article failed to show any benefit or even a rational basis for urethral dilatations to prevent recurrent infection. I would encourage Dr. Hopkins to furnish evidence to the contrary, either by personal research or citations of research done by others. Likewise, after many therapeutic long-term trials, there is no evidence that the therapy of asymptomatic bacteriuria confers any benefit to the patient. It appears obvious to me, at least, that asymptomatic bacterial endocarditis is not comparable with asymptomatic bacteriuria.

Ours is a difficult discipline composed of a mixture of both art and science. Having spent 21 years in private practice and ten years in an academic setting, I realize that scientific information changes with time. Nevertheless, it is necessary to use the best data available at any given time. It is incumbent on physicians who use methods such as urethral dilatation, which can be both painful and expensive for our patients, to justify such therapy with information that is more than anecdotal.

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Reference

1. Stamm WE, Wagner KF, Amsel R, et al: Causes of the acute urethral syndrome in women. N Engl J Med 303:409, 1980