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.

GATEKEEPERS IN PRIMARY CARE

To The Editor:

The article authored by Drs. Bertakis and Robbins (*Bertakis KD, Robbins JA: Gatekeeping in Primary Care: A Comparison of Internal Medicine and Family Practice. J Fam Prac 1987; 24:306–309*) would be more properly titled without the word "Gatekeeping."

Gatekeeper[1] is a buzzword that should be used precisely. The word refers to the specific function of a patient's chosen or assigned primary care physician in a prepaid health plan. The gatekeepers ". . . must approve all care provided to their patients. This approval is a condition of payment for the service. . . ."[2]

I have heard this word used as a pejorative term by both patients and physicians in our area. "Case manager" is a phrase that really describes this function and does not generally carry such an emotional connotation as gatekeeper.

The study does give an interesting view of how family practice residents compare with internal medicine residents within a training program. The study may also reflect (1) what tests or referrals the respective attending physicians suggested to the residents when asked for advice, (2) a cost-effective approach to medicine taught and modeled by family medicine faculty, and (3) feedback on test use,[3] referrals, and hospital care given to family practice residents by their department's faculty members.

The residency training period is a time when consultant physicians should be utilized more readily than later in private practice; consultants are an important source of teaching. Laboratory tests also may be ordered a bit more freely in residency than in private practice. By actually using the test, the resident learns what the test can and cannot do and sees to what extent the result can add to the history and physical examination.

It is important to teach residents cost-effective medical treatment. However, we do not want to confuse them by pushing this too hard, too early. It is best at the beginning to expose residents to the concept that, "in the practice of medicine, physicians are all obligated to do all they can for their patients without regard to any costs to society."[4] After that, they can decide whether they wish to assume the role of a "gatekeeper."

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PERINATAL OUTCOMES AND FAMILY MEDICINE

To the Editor:

Rosenblatt's editorial[1] and the articles by Franks and Eisinger[2] and Mengel and Phillips[3] will contribute to the literature needed to support the

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role of family physicians in obstetric care and research. I have a concern, however, with the editorial. I suspect that most family physicians will take exception to Rosenblatt's statement that "Family medicine [is] a medical specialty built more on [a] social role than on anatomical or technological expertise. . . ." If, however, he believes this dictum, why did he ignore the psychosocial role of the family physician as an assessor and intervenor in facilitating positive pregnancy outcomes? The measurement of psychosocial risk factors in pregnancy outcome has been a part of medical literature for 30 years.[4] The recent contributions from family medicine centers at the University of Oklahoma,[5] Case Western Reserve University,[6] and the University of Washington,[7,8] attest to the significance of the contributions of psychosocial risk to pregnancy outcome. It is unfortunate that *The Journal* editorial on adverse perinatal outcomes did not give recognition to these contributions.

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FLEXIBLE SIGMOIDOSCOPY

To the Editor:

In "A Critical Review of Adult Health Maintenance: Part 3. Prevention of Cancer" (*J Fam Prac 1986; 22:511–530*), Frame notes that patient acceptance of screening sigmoidoscopy has been a major problem. I would contend that poor compliance with sigmoidoscopy by patients can be modified significantly by the family physician.

Since 1979 I have been performing 60-cm left colon examinations. Year by year patient acceptance improves. Currently the return on examinations is 10 to 15 percent positive for polyps, of which about one third are villous adenomas.

With regard to cost, the flexible sigmoidoscope examination approximates the mammogram. My patients seem to accept the examination after a careful explanation of health-risk appraisal.

Performing the examination is an art. Careful physician awareness of the patient's comfort level and judicious use of glucagon or Versed allows good patient-to-patient reporting of the experience with the examination.

Since colon cancer does have significant morbidity, and since early diagnosis usually allows detection of less-advanced disease, the family physician is providing a caring and beneficial service by encouraging and professionally performing flexible left colon examinations.

Leon J. Davis, MD
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SPECIALTY AND PERINATAL OUTCOMES

To the Editor:

The article "Adverse Perinatal Outcomes: Is Physicians' Specialty a Risk Factor?" by Franks and Eisinger (*Franks P, Eisinger S: J Fam Pract 1987; 24:152–156*) raises questions about the samples of both family physicians and obstetricians' practices. Approximately 25 percent of both practices had fewer than four visits. There is no mention as to whether the obstetric practices also involved an obstetric clinic at Highland Hospital, which might explain this low number of visits. It is unusual for a private practice of obstetricians and even for a family practice residency model unit to have that high percentage of extremely late registrants for care.

It was noted that none of the 73 in the random sample assigned to obstetricians had prenatal bleeding, and only two of the 73 patients assigned to family physicians had prenatal bleeding. I also questioned these figures. My best guess would be that this represents poor record keeping or wishful thinking.

Joel Potash, MD
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The preceding letter was referred to Dr. Franks, who responds as follows:

Dr. Potash correctly notes an apparently high rate of late registration for care and low rate of prenatal bleeding. It was not made explicit in the original report that the chart review was based on the hospital records. It is hospital policy to request a prenatal chart, on which some of this data were based, at 36 weeks' gestation. Thus, the apparently high percentage of late registration probably reflects an artifact of visits in the last month of pregnancy not being recorded. The chart review represented a random sample of over 6,000 deliveries, and it is possible that some obstetric clinic patients were included. However, the obstetric clinic accounts for a very small percentage of the deliveries at Highland Hospital and thus is an unlikely source for bias. In general, for a potential source of bias to actually confound results, it is necessary for that variable to operate differently on patients or charts of family physician providers than on those of obstetrician providers. There is no serious evidence that this might be the case.

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DIAGNOSIS AND MANAGEMENT OF ACUTE CHOLECYSTITIS

To The Editor:

The observation and review by Dr. Whalen (*Whalen JP: A false-positive hepatobiliary scan: Case report and literature review. J Fam Pract 1987; 24:261–264*) is a useful addition to the family practice literature. However, it raises the issue of the assumption that the first episode of acute cholecystitis is necessarily a surgical disease. The clinical situation presented is one that I have seen occur time and time again in community situations, wherein a relatively young person has been automatically shunted to the surgeons when a clinical diagnosis of cholecystitis has been made regardless of whether the diagnosis is supported entirely by laboratory or scanning, x-ray, or ultrasound evidence.

For years we have supported the position that acute cholecystitis, especially when mild and the first episode, is a medical disease, especially if the evidence for cholecystitis is tenuous.

Over the past 25 years we have treated dozens of patients with mild, suspected first presentations of cholecystitis. Medical therapy has been successful in many of these cases without resorting to surgery. Occasionally we have treated mild recurrences of this condition when they have been separated by many years.

Our experience indicates that internists and, of course, surgeons are much more likely to assume that mild cholecystitis (without evidence of cholelithiasis) is a surgical disease. A "knee-jerk" surgical reaction to this disease is not always necessary.

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

I agree with Dr. Brogan that a "clinical diagnosis of cholecystitis" is insufficient evidence to justify an operation. The low specificity of symptoms referable to cholecystitis demands some sort of laboratory confirmation before surgery can be recommended.

To respond adequately to Dr. Brogan's comments, however, I would need to know his definition of "cholecystitis." Cholecystitis without evidence of cholelithiasis, so-called "acalculous cholecystitis," is an unusual disease, seen in very special circumstances such as postoperative patients and burn patients. I do not contend that the patient in my case report had cholecystitis. Rather, I am reporting a case where a test that is allegedly highly sensitive and specific for acute cholecystitis was misleading in a patient who demonstrably did not have acute cholecystitis.

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URINARY TRACT INFECTIONS IN ELDERLY WOMEN

To the Editor:

Several aspects of the report by Bertakis and Ross (*Bertakis KD, Ross JL: Office evaluation of urinary tract evaluations in elderly women. J Fam Pract 1987; 24:72–75*) concerning the evaluation of urinary tract infections in elderly women deserve comment. It would probably be more accurate to use the term *asymptomatic bacteriuria* (ABU) as other authors have,[1] instead of the term *asymptomatic urinary tract infection,* to describe their patients with positive urine cultures. The latter is a subgroup of ABU in which there is some evidence of invasion or inflammation of the urinary tract.

The authors provide a list (Table 2) of the sensitivities and specificities of tests used to detect the presence of ABU. This information is useful but incomplete. It would have been helpful had predictive values also been provided. For example, the Gram stain looks deceptively accurate with a sensitivity of 88 percent and a specificity of 86 percent. However, the positive predictive value (the proportion of patients with an abnormal Gram stain who actually have ABU) in this population is only 50 percent (7 of 14). Positive and negative predictive values could have been calculated easily for each of the tests listed.

A major flaw of this report, rendering the sensitivity, specificity, and prevalence estimates invalid, is the probable misclassification of three of the patients. Most authorities would not consider growth of Staphylococcus epidermidis, diphtheroids, or mixed flora significant, even at colony counts greater than or equal to 10^5 colonies per milliliter.[2,3] Were these the patients with normal nitrite and leukocyte esterase tests and white blood cell counts? The authors reportedly recorded epithelial cell counts, a test useful in identifying contamination of urine specimens, but failed to report these data.

Finally, though it is true that the presence of ABU has been associated with increased morbidity and mortality, I know of no study that has demonstrated that treatment of ABU improves these statistics. Therefore, the question of ". . . which tests are most reliable, cost effective and efficient in diagnosing [asymptomatic] urinary tract infections in the elderly population" becomes moot. Attempts to detect ABU in the elderly do not have the characteristics of a good screening program as described by Frame[4] and is not recommended by that author. Efforts should currently be made to determine whether treatment of ABU reduces morbidity or mortality in the elderly.

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METHODS FOR COLLECTION OF PAP SMEARS

To the Editor:

Several articles have recently appeared concerning the most effective way of collecting cervical specimens for Papanicolaou testing. Use of a Milex extended-tip spatula and moistened cotton swab have been found to increase the yield of slides with endocervical cells.[1–3] It is thought that the presence of endocervical cells is an important indicator of the adequacy for a Papanicolaou smear.[4] The physicians and nurse practitioners at our clinic have evaluated the effectiveness of the Zelsmyr Cytobrush,* a small brush that collects cells from the endocervical canal.

Four methods of collecting specimens were compared consecutively. A sample size of 85 to 100 was obtained for each method, excluding women who were pregnant or had had a hysterectomy. We found that when a wood Ayre spatula and dry cotton swab were used, 60 percent of slides had endocervical cells; with a plastic Milex spatula and dry swab, 63 percent had endocervical cells; with a Milex spatula and moistened swab, 65 percent had endocervical cells; with a Milex spatula and Zelsmyr Cytobrush, 78 percent of slides had endocervical cells.

The Cytobrush-Milex spatula combination was significantly more effective than the other methods ($P < .05$ by chi-square test) in obtaining endocervical cells. The Cytobrushes are expensive (about 25 cents apiece), but we feel that the increased effectiveness is worth the extra cost. This will be even more important if women and their physicians follow recommendations to have Papanicolaou tests done less frequently.

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* *International Cytobrush, Inc, PO Box 7733, Hollywood, FL 33081.*

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