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. All letters that reference a recently published Journal article are sent to the original authors for their reply. If no reply is published, the authors have not responded by date of publication. Send letters to Paul M. Fischer, Editor, The Journal of Family Practice, 519 Pleasant Home Rd, Suite A-3, Augusta, GA 30907-3500, or Fax (706) 855-1107.

SCREENING FLEXIBLE SIGMOIDOSCOPY AND INSURANCE REIMBURSEMENT

To the Editor:

Colorectal cancer is the second most common malignancy in the United States.[1] The American Cancer Society, National Cancer Institute, and American College of Physicians recommend screening flexible sigmoidoscopies (SFS) every 3 to 5 years for everyone over the age of 50 years who is not at high risk for colon cancer.[2] Recent studies[3,4] and editorials[2] continue to support SFS as a successful tool for reducing mortality from colorectal cancer. *Family Practice News*[5] reported a survey of 237 general internal medicine patients seen at a teaching hospital regarding SFS. Seventy-two percent of the patients said they would schedule a SFS if their doctor advised it, yet 53% of the patients said their doctor had never recommended or mentioned the test. We surveyed 166 family physicians (FPs) in Montgomery County in Ohio to determine what factors would prompt family physicians to promote SFS among their patient populations.

Respondents were asked to choose as many items as applied from a list of factors related to increased performance of SFS: (1) all insurance companies reimbursed for SFS; (2) easier-to-clean equipment; (3) better training in the procedure; (4) less expensive equipment; (5) better data to support SFS; (6) greater patient comfort during the procedure. One hundred twenty-six (76%) of the 166 FPs responded. Insurance reimbursement was the most frequently reported response (54%), and easier-to-clean equipment was the second most frequently indicated response (37%). Twenty-three percent or 24% chose each of the four other responses.

In addition to the questionnaire, representatives from 20 major insurance companies were interviewed by telephone regarding the amount each company reimburses for SFS. Six (30%) of the insurance companies reimburse for SFS, six (30%) do not, and eight (40%) are policy-specific. Medicare pays 80% of the charge; Medicaid does not reimburse.

Although survey research is susceptible to various biases, we believe our data show that insurance reimbursement for SFS is poor, and FPs find this poor reimbursement an obstacle to performing SFS. Perhaps reimbursement for SFS would be increased if organizations such as the American Medical Association, American Board of Family Practice, American Cancer Society, and American Gastroenterological Society as well as health care professionals urged insurance companies and government agencies to reimburse for SFS. Better reimbursement would encourage more FPs to perform SFS, which in turn, would decrease mortality from colorectal cancer.

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ALTERNATIVE THERAPIES

To the Editor:

In response to the article by Borkan et al on referrals for alternative therapies,[1] I believe some education is needed regarding the nature of hypnosis. I would like to take issue with the inclusion of hypnosis in the list of "alternative" therapies.

A "mainstream," if not seminal thinker, the esteemed allopath S. Freud, MD, used hypnosis in his practice and derived insights from such, which he further pursued in his psychoanalytic theory. In 1955, the British Medical Association endorsed hypnosis as an acceptable modality of treatment. In 1958, the American Medical Association recognized hypnosis as a therapeutic adjunct, and the American Psychiatric Association endorsed hypnosis in 1961.[2]

As a practicing allopath, I rather resent this therapy being lumped in with reflexology, naturopathy, homeopathy, and chiropractic. Not one of these approaches, to my knowledge, carries any endorsement from "the professional hegemony of allopathic medicine."

The American Society of Clinical Hypnosis was formed in 1957 and now has over 4000 physician, dentist, psychologist, and other professional members (ie, Master's level counselors). I doubt that many consider these providers to be "alternative."

David L. Smith, MD
Voorhees, New Jersey

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NEBULIZED SALINE AND BRONCHITIS

To the Editor:

The recent article by Reynolds and Smith (*Reynolds RD, Smith RM. Nebulized bacteriostatic saline as a cause of bronchitis. J Fam Pract 1995; 40:35–40*) purporting a causal relationship between the benzyl alcohol component of nebulized saline and the subsequent development of bronchitis in healthy adults at-

tracted our interest. Given the recent reports of increased mortality among asthmatic patients using excessive albuterol nebulization, this is an important and timely topic for investigation.

While we were impressed with the elegance of the study design and the dramatic bronchoscopic photos documenting the presence of bronchitis, the claims of causality made in the article have no basis in the presented data. Of the nine subjects completing the study, four of five in the benzyl alcohol group and two of four in the non–benzyl alcohol group developed bronchitis. Analysis with Fisher's exact test (2-tailed) indicates no significant difference in outcomes for these two groups ($P>.05$). While the failure of the study to find a significant difference may simply reflect a type 2 error, given the small sample size, claiming a causal relationship between benzyl alcohol and bronchitis at this point seems to be an inappropriate leap of faith.

Although we disagree with the stated conclusions, we were struck by the high rates of postnebulization bronchitis in both groups. If bronchitis develops this often following qid nebulization for 2 weeks regardless of the diluent used, perhaps the increased mortality in patients who overutilize nebulized albuterol may be related to the act of nebulization itself, rather than the albuterol per se. This possibility clearly merits further study.

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The preceding letter was referred to Drs Reynolds and Smith, who respond as follows:

Certainly our study results do not meet a 2-tailed Fisher's exact test if the endpoint is the development of bronchitis. However, we found different carinal biopsy results in the subgroups of patients who developed bronchitis.[1] Those who were exposed to benzyl alcohol developed a lymphocytic mucosal infiltrate, matching what was found in animal toxicological studies.[2] Those who nebulized saline placebo developed a polymorphonuclear mucosal infiltrate. We feel that this difference distinguishes the two groups.

We have recently realized that our study design created a distinctly unusual situation for the placebo volunteers who nebulized sterile saline. When nebulizer therapy is prescribed for an ill patient, the nebulizer solution always contains some preservative. Unlike such patients in everyday clinical situations, our placebo volunteers had *no* preservative in their nebulizer solution.

At the time of our study in February 1991, we were unaware that nebulizer setups can become rapidly contaminated with gram-negative bacteria.[3] We were further unaware of recent recommendations to rinse nebulizer cups with sterile solutions after cleaning.[4] In retrospect, we feel that the placebo volunteers may have contaminated themselves with bronchial deposition of their own oral flora, or were at higher risk of developing a viral respiratory infection, given the absence of preservative in their nebulizer solution. However, we did prove that their Bronchosaline (Blairex Laboratories, Columbus, Ind) saline placebo was not the source of bacterial contamination.[1]

We agree that the phenomenon of nebulizer-induced bronchitis warrants further study. However, our study created preservative-free conditions and, unfortunately, we specified less than ideal nebulizer decontamination. It is premature to implicate nebulizer therapy as dangerous based on our unusual study conditions. We hope that as a result of our study, clinicians will become more aware of the diluent they prescribe and the nebulizer decontamination procedures they specify.

We stand by our conclusion that nebulized bacteriostatic saline causes bronchitis; specifically, a lymphocytic mucosal infiltration.

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Richard M. Smith, MD
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SPECIALTY CHOICE BY MEDICAL STUDENTS

To the Editor:

I was very pleased to see the article entitled "A Multivariate Model for Specialty Preference by Medical Students,"[1] since the authors emphasize the utility of using a theoretical framework to investigate medical student career choices. However, I am writing to address two concerns about their report. First, they were incorrect in their critique of my study, which they cited.[2] Second, the study design and the findings reported do not permit the conclusions that Gorenflo et al make.

Our study used the theory of reasoned action as a framework to investigate medical students' choice of a career in family practice. The authors correctly described our finding concerning attitude. Unfortunately, they did not describe the more interesting findings concerning attitudinal beliefs and values as determinants of family practice career choices. In criticizing our study, the authors indicated that we "did not take the model to completion in terms of combining the outcome likelihood and importance scales." In fact, our article clearly describes how we computed attitude scores by multiplying students' expectancy (outcome likelihood) ratings by their respective value ratings, and summing these products. Our main results table presents findings concerning these product scores.

Second, I believe that their study design undermines the conclusions that the authors make concerning differences in attitude scores between students choosing primary care and nonprimary care specialties. The design had each student rate his or her *first specialty preference* on scales that were used to compute attitude scores. For example, students who preferred surgery rated the likelihood that a surgical specialty would involve operating on patients, whereas students preferring family practice rated the likelihood that family practice would involve operating on patients. The authors found that students choosing a primary care specialty rated that specialty higher on General and Lifestyle scales and lower on the Surgery scale than other students' ratings of their nonprimary care specialties, and they made conclusions about differences in interests and concerns between these two groups of students. This is clearly a circu-

lar argument. The study design confounds student group (those preferring primary care vs nonprimary care) and the specialty that students rated. Thus, one is unable to determine whether the study findings are due to differences between the specialties rated or to differences between the types of students who made the ratings. Therefore, the attitudinal findings as presented provide no conclusive information that will assist us in recruiting students to primary care or identifying students best suited for primary care.

Two solutions to this problem are possible. The first would be to use a different study design, having all students make ratings about the same primary care specialty, and test to see whether attitudes toward that specialty differ between students choosing primary care and students not choosing primary care. This was the design that we used. The second solution would be for the authors to disentangle the likelihood ratings from the importance ratings, and compare primary care and nonprimary care choosers on their value (importance) ratings alone, since these ratings, as compared with outcome likelihood ratings, were *not* specific to their specialty preference.

Daniel E. Montaño, PhD
Seattle, Washington

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2. Montaño DE, Neighbor WE, Carline JD, Wright C, Phillips TJ. A survey of fourth-year medical students' decisions regarding family practice as a career. J Med Educ 1988; 63:830–8.

The preceding letter was referred to Drs Gorenflo, Sheets, and Ruffin, who respond as follows:

Daniel Montaño has taken offense at the evaluation of his study[1] and leveled criticism at the study of Gorenflo et al.[2] We would like to clarify our concerns with the study by Montaño et al and respond to his criticism.

Our concerns with the study by Montaño et al centered on three issues: (1) their use of a single dimension for attitude assessment, when the 19 attitude items clearly reflected different issues; (2) the complete absence of multivariate analysis; and (3) the fact that Montaño et al conducted approximately 50 *univariate t* tests without some form of *P* value adjustment for their very high type I error rate.

With respect to the conclusions presented by Gorenflo et al, we conducted the analysis that Montaño suggested and found that it did not change the model or overall conclusions presented. We communicated these results to Montaño over the telephone, and he responded that this finding "strengthens your conclusions."

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2. Gorenflo DW, Ruffin MT, Sheets KJ. A multivariate model for specialty preference by medical students. J Fam Pract 1994; 39: 570–6.

REVOLUTION IN THE AMERICAN MEDICAL SYSTEM

To the Editor:

Richard L. Garrison, MD (*The five generations of American medical revolutions. J Fam Pract 1995; 40:281–7*), uses the military metaphor of the American Revolution to emphasize his view that US medicine is resisting instead of embracing change. He believes a revolution is needed to replace forcibly the specialist/technologist–dominated medical hierarchy with one run by generalists. This raises questions about the telling and retelling of history, and the question of who owns the past.

We may need not a revolution, but a *reversion*. Yes, to generalism, but to the new generalism, which rejoices in the forging ahead of technology and the uncovering of new medical truths for patients and physicians, while preserving intellectual and compassionate traditions, and the freedoms for which the American Revolution was fought.

Medicine has survived many historical hurdles. There have been forces for good and evil. We are now caught in a force that appears relentless, yet which could have been and still could be reversed by our will and action, if only we would see the future evils as our successors will, and would rise up and throw them off. Much of our present activity involves identifying with the aggressors. We are making Faustian pacts with devils, hoping to gain salvation, or at least comfort and security, right now, until we retire or our kids go to college. But history moves in everchanging directions. We can't go back but we can regress.

Mark Twain said that the very ink that history is written in is liquid prejudice. We choose to remember and retell that which is consonant with our deepest fears or our most wishful thinking.

Is it too late for Medicine to reclaim its rightful place in history? To remain powerful and independent, with patients who control their own destinies by choosing freely from physicians whose agendas include only their own best efforts at patient care?

Only if patients' own money ceases to be confiscated by insurance schemes or government can it be used fully in their own choices of doctors and venues. This would reflect a valid, real marketplace. The so-called medical market of today's scene is a distortion, where an aspirin "costs" $5.00, and physicians are paid by the care they don't give ("capitation"). The term "capitated lives" is used in an attempt to impart seriousness to a business entity's activities. In reality, saving lives remains in the hands of skilled practicing physicians and nurses, not in a boardroom where middlemen (or physicians-turned-middlemen) maximize their profits.

While the corporate, hospital, and insurance domination of Medicine has been called "market forces" and "capitalism finally at work in the medical field," it has been possible only because of legislation such as the 1972 HMO Act, the McCarran-Walter Act, and others: they are well meaning but cause severe market distortions.

Patients and doctors could regain control with minimal legislative incentives (such as the Patient Protection Act and Medical Savings Accounts), with public health ongoing education, catastrophic insurance, and with good will and cooperation by physicians and patients.

This would constitute not a revolution, but a reversion to first and basic principles: a doctor and a patient, sickness and healing. This is not a reactionary

harking back to the old days and ways, but a reaffirmation and a new valuing of what was lasting, and an embrace of what is innovative, whether general or "technical."

Pepi Granat, MD
South Miami, Florida

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

Dr Granat's comments describe a better way than the way of revolution. He advocates intellectual honesty, unsparing self-assessment, selflessness, cooperation, and love of fellow man. He offers hope that moral right can conquer by power of persuasion.

Given any choice, I would choose the way of Dr Granat.

Since he is a student of history, however, he may come to agree eventually that the evil that mankind does goes beyond efforts at rehabilitation. When a paradigm reaches the degree of senescence seen in our fifth-generation medical care system, it will reject efforts at reform. No fifth-generation power broker known to me has ever surrendered power merely because the harm of his actions was explained to him.

Every revolution *is* a reversion. All revolutions take us back to similar x and y coordinates. But a return to a previous position can never occur. For as the planet orbits the star, the star proceeds on its own course; consequently Earth does not return to the same place each January 1, for the Sun has moved on. The z coordinate will never be the same. That is why we should, as Dr Granat observes, "return" to generalism, but we should not return to the *same* generalism.

I welcome debate on the topic of the economic blessings of technology. I believe that nearly all technologies make life better for those who can afford them, while making life more wretched for those who cannot. This seems to be due to some overall effect of technology to concentrate wealth and power in the hands of those who are early users at the expense of those who are not. This topic needs further airing.

A collapse of the present system, in the absence of a *coup de grâce* administered by the generalist revolutionaries, would pave the way for a total takeover of health care by business tyrants. They would give no quarter as they plundered the ruined landscape of what had been the world's best health care system.

Consequently, revolution must come.

Richard L. Garrison, MD
Houston, Texas

PROMOTES DIAZEPAM AS SAFE, EFFECTIVE

To the Editor:

Benzodiazepines have been prescribed in this country since 1959, and worldwide for over 40 years. The number of individuals treated with benzodiazepines in 1993 exceeded 20 million in the United States and 60 million worldwide.

Peak use of diazepam in this country (61 million prescriptions) occurred in 1975. Nevertheless, the total number of prescriptions for all benzodiazepines and related Food and Drug Administration (FDA) Schedule IV anti-anxiety drugs has been increasing each year. There have been many survey papers written on benzodiazepine dependence,[1–3] of which the total reported cases exceed 400 patients. It is noteworthy that all these reports include very few patients who were dependent on both low-dose (20 mg per day or less diazepam equivalents) benzodiazepines and either alcohol or illicit drugs.

The use of benzodiazepines by drug addicts has been discussed by Iguchi et al,[4] who noted that "subjects also reported that they were seeking to 'boost' the effects of methadone with the benzodiazepines, and that they rarely took the drugs when they were not on methadone maintenance." The use of benzodiazepines by alcoholics has been discussed by Ciraulo et al,[5] who stated that "we feel that benzodiazepines are relatively safe drugs with many uses in the treatment of alcoholics when prescribed rationally." The test of time has proven Ciraulo et al correct.

A careful review of the world literature in a clearly objective fashion brings to light an interesting observation about diazepam use: despite the many millions of individuals for whom these medications are prescribed annually, including both alcoholics and drug addicts, the number of patients who have become diazepam-dependent after taking it in reasonable dosage (ie, 20 mg per day or less) and for reasonable time periods (ie, less than 6 months) approaches zero.

This conclusion does not apply to diazepam withdrawal, in which the dosage is suddenly markedly reduced or eliminated.[6] Similarly, this conclusion must, of course, recognize the rare anecdotal case in which, for some unexplained reason, the dosage and duration have been less than that noted. Considering the use of benzodiazepines by many millions of individuals each year, some anecdotal cases of this type are the rule rather than the exception.

Thus, limited-term use of reasonable doses of diazepam is an entirely safe and effective medical practice. After almost 40 years of use, there is no credible evidence to the contrary.

Philip I. Hershberg, MD
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SEMEN RESPONSIBLE FOR FISHY VAGINAL ODOR

To the Editor:

A fishy vaginal odor is a frequent female complaint. Its origin has been unclear. We embarked on a study to determine the relationship between the fishy vaginal odor and sexual intercourse.

Our 30-day study involved one heterosexual monogamous couple having unprotected vaginal intercourse. Every morning, for 30 consecutive days, vaginal cultures were obtained, along with smears for *Trichomonas*, *Candida*, and clue cells. Additionally, a smell for fishy odor was performed by both the physician and the patient. All cultures were performed on a daily basis at Path Laboratories, Los Gatos, California.

Continued on page 540

Continued from page 538

Our results demonstrated a fishy odor only on the morning following intercourse. All cultures for *Neisseria gonorrhoeae*, *Ureaplasma*, mycoplasma, and chlamydia, and routine urogenital microcultures were negative. Vaginal flora showed no pattern of change. Smears were negative for *Trichomonas*, *Candida*, and clue cells. Only during the menstrual flow was the fishy odor absent postcoitally.

We believe that semen is responsible for the fishy odor that some women experience postcoitally.

Nayvin Gordon, MD
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DRUG INTERACTION INDUCES HYPOGLYCEMIA

To the Editor:

I describe two stable non–insulin-dependent patients with diabetes mellitus who developed hypoglycemia while taking enalapril and glyburide concomitantly.

Case 1. A 54-year-old woman with non–insulin-dependent diabetes mellitus had been taking glyburide 5 mg per day for 9 months with excellent control of serum glucose values (120 to 140 mg/dL). When she developed essential hypertension, enalapril 5 mg per day was prescribed to control her hypertension. When the patient was examined in the clinic about 3 weeks later, her blood glucose levels were in the range of 50 to 60 mg/dL. She stated that she had been having attacks of hypoglycemia since the introduction of enalapril therapy and had even fallen on a couple of occasions. Blood glucose level in the clinic on this visit was 48 mg/dL. Therefore, she was advised to reduce the dose of glyburide to 1.25 mg per day. There have been no more attacks, and the blood glucose levels have remained within 125 to 140 mg/dL.

Case 2. A 48-year-old man with non–insulin-dependent diabetes mellitus had been taking glyburide 5 mg per day for 18 months, with good control of serum glucose values, ie, 115 to 148 mg/dL. Enalapril 5 mg per day was added to this regimen for essential hypertension. After ingesting one dose of enalapril, the patient developed sweating, palpitation, anxiety, confusion, and disorientation to time, space, and person. Blood glucose

level was 50 mg/dL. Following administration of 50% dextrose as intravenus bolus, the confusion disappeared and glucose levels came up to a satisfactory level (110 mg/dL).

The patient was advised to decrease the dose of glyburide to 1.25 mg per day. When he was seen again 3 weeks after this episode, his blood glucose levels and blood pressure control were excellent.

Review of the published literature has revealed reports in which one patient who was receiving sulfonylureas and captopril,[1] and another two patients who were taking a combination of sulfonylureas and biguanide developed hypoglycemia while on concomitant ACE-inhibitor therapy.[2]

The mechanism may be that the ACE-inhibitors induce a decrease in hepatic glucose[3] and reduce insulin resistance in hypertensive subjects with diabetes mellitus. A reduction in glomerular filtration rate would increase the glyburide concentration by 50% and may increase insulin levels.[4,5] It is very good medical practice to use ACE-inhibitors in patients who are both diabetic and hypertensive as long as this drug–drug interaction is borne in mind. The dose of sulfonylureas may need to be reduced to obviate hypoglycemia. Other drugs described as having interacted with enalapril include indomethacin, other nonsteroidal anti-inflammatory agents, and lithium. There is a case report in which the patient developed anaphylaxis and severe coronary spasm, culminating in acute myocardial infarction, owing to concomitant administration of enalapril and allopurinol. In view of this clinical observation, I suggest that glyburide enalapril interaction also be added to this list.

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MORE MEDICAL MALAPROPISMS

To the Editor:

The recent article entitled "Medical Malapropisms (or a Stitch in Time Gathers No Moss)," by Davis and Kenyon,[1] explored the humor we occasionally find in patients' malapropisms. In many cases, the funny blunders contain a thread of meaning which, if recognized, can provide some helpful insights into the patient's understanding of an illness or medical experience. There are two types of malapropisms presented in this letter: the first type is patients' use of a sound-alike approximation of a term they don't understand (eg, *smilin' mighty Jesus* for spinal meningitis, or *peanut butter balls* for phenobarbital); and the second type is the "Freudian slip," or parapraxis, in which the patient reinvents a well-worn phrase (eg, "He stopped smoking *cold duck*"), thereby revealing unconscious material which, if pursued, may provide unexpected insights. Parapraxes have long been seen as windows into the unconscious and have been used to analyze artwork[2] and understand memory.[3] However, it doesn't take a psychoanalyst to understand the meaning behind parapraxes such as one spoken by Dick Cheney: "We need a bomber that can strike deep into enemy territory and carry a large payroll—uh, I mean payload."[4]

Freud pursued parapraxes through a chain of associations to a "disturbing element," leading him to a clearer notion of the speaker's thoughts.[5] As family physicians, we have a similar opportunity with our patients' parapraxes. I have found even the most apparently inconsequential slip of the tongue can be the impetus for a more effective encounter.

I recently had an interesting experience. One of my regular patients came in for his usual appointment for hyperlipidemia and gout. He greeted me in his normal friendly way, except that instead of, "Hi, Dr Dave Buck" he said, "Hi, Dad Buck." We both chuckled, but it occurred to me there may be something to this. I said, "Dad Buck" and smiled. He then told me about his wife, who was dying of cancer. He related how he has been caring for her at home, giving her daily baths, and cooking for the family. He then told me the last time he faced something like this was when his dad died. We had an unusual encounter. On the way out, he chuckled and said, "I guess you kind of remind me of my dad." I said, "I think he must have been very proud of you."

David S. Buck, MD, MPH
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CORRECTIONS

On page 342, second column, line 10 of the April issue (*Ely JW, Levinson W, Elder NC, Mainous AG, Vinson DC. Perceived causes of physicians' errors. J Fam Pract 1995; 40:337–44*), the range of physicians' errors was inaccurately published as 1 to 9. The correct range is 1 to 90.

On page 379 of the April issue, Table 1, under the column heading "Mechanism" (*Yamreudeewong W, Henann NE, Fazio A, Lower DL, Cassidy TG. Drug-food interactions in clinical practice. J Fam Pract 1995; 40: 376–84*), the effect of warfarin on the availability of vitamin K was inaccurately reported. Warfarin *increases* the availability of vitamin K for activation of vitamin K-dependent clotting factors.