We try to publish authors' responses in the same edition with readers' comments. Time constraints might prevent this in some cases. The problem is compounded in a bimonthly journal where continuity of comment and redress are difficult to achieve. When the redress appears 2 months after the comment, 4 months will have passed since the article was published. Therefore, we would suggest to our readers that their correspondence about published papers be submitted as soon as possible after the article appears.

Using Two Alcohol Screening Tests With Elderly Patients

To the Editor: In their excellent recent article about screening older patients for alcohol problems, Nguyen and colleagues[1] mention three alcohol screening tests of limited use for the elderly. Other screening tests—the Michigan Alcohol Screening Test (MAST[2]) and Short Michigan Alcohol Screening Test (SMAST[3])—might also show limited usefulness for older patients.[4,5] A key problem of these tests could be a problem in other screening tests, too.

Of the 13 SMAST items, 7 are worded in the past tense, eg, "Has drinking ever created problems between you and your wife, husband, a parent, or other relative?" A "yes" answer—noted about a past problem—would contribute to a higher SMAST sum score even if the person were no longer drinking. A former drinker who answered 5 items "yes" could be classified as alcoholic,[3] even if the person were now abstinent. (Indeed, an older person scoring 3 might be classified as alcoholic.[6]) Because people tend to give up drinking as they age,[7] the SMAST could become a progressively less useful index of current drinking in older patients. Additionally, it does not ask respondents whether they currently drink.

Concerned about similar ambiguity in the MAST, Zung[8] asked young and middle-aged psychiatric patients when they last drank and compared their answers to MAST scores; used as an index of current drinking, the MAST had a 50% false-positive rate. In an older sample, in which a higher proportion of patients might have stopped drinking, the false-positive rate could be higher. In one study[5] the SMAST classified 24% of an older sample as problem drinkers compared with 7% and 11% using physicians' diagnoses or another measure.

Furthermore, because younger men tend to experience more alcohol problems than do younger women, use of the SMAST could contribute to a higher rate of false-positives among men. Indeed, the SMAST shows lower specificity (more false-positives) and lower correlation with biologic markers of alcohol use for men than for women.[9,10]

In sum, alcohol screening tests with many items worded in the past tense might become less accurate as people age. This issue is of special relevance to primary care physicians. Older drinkers tend not to seek counseling or alcohol treatment; primary care providers are most likely to have the opportunity to be aware of the older patient's alcohol problems and to intervene.[11,12]

Physicians' cross-validation of screening scores with another screening or clinical validation[13] can spare patients embarrassment at being misdiagnosed as alcoholic. Furthermore, use of alcohol screening tests designed for older patients (the MAST and SMAST were not) might more sensitively detect problems of special salience to elderly,[14] such as alcohol-medication interactions. As Nguyen et al[1] remark, use of an appropriate alcohol screening test can elicit information that older patients rarely discuss with physicians, and medical intervention can make a real difference.

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Evidence-Based Advertising?

To the Editor: Dr. Gutknecht's findings[1] suggest that pharmaceutical advertisements are often incomplete and inconsistent in reporting the methodology and results of the original study on which the advertisement is based. He argues that advertisements should be adequately detailed so that the practicing physician can make informed decisions regarding the merits of the underlying study without consulting the original publication. This thesis presupposes that the original publication contains sufficient detail to make an informed decision. Unfortunately, numerous studies suggest that the original publications share many of the shortcomings that Dr. Gutknecht reported to be associated with pharmaceutical advertising.

For instance, DerSimonian et al[2] reported that only 12% of the clinical trials in the *New England Journal of Medicine*, *Lancet*, the *Journal of the American Medical Association*, and the *British Medical Journal* discussed the power of the study and that only 19% reported the method of randomization. More recently, Schumm et al[3] reported that among major surgery journals, only 32% of the clinical trials discussed statistical power, 40% described the method of randomization, and less than one half reported whether the person who assessed outcome was blind to the patient's treatment assignment. Gluud[4] further delineates various methodological errors commonly made that affect the internal and external validity of clinical trials.

Nearly 20 years ago, Steckman[5] and Sackett[6] admonished the *New England Journal of Medicine* for printing the Method section in smaller print than the "conjectures and special pleadings" (Introduction and Discussion) of the article. Using a smaller print for the Method section diminishes its importance and encourages the reader to proceed to the larger print of the Discussion section. Interestingly, *New England Journal of Medicine* still relegates the Method section to small print.

Journal editors have the power to dictate the minimum information to be included in the publication of a clinical trial.[2] To hold advertisements of the pharmaceutical industry to a higher standard than reports of original research by authors and tacitly by journal editors is to have one's head firmly planted in the sand. Before we tilt at the pharmaceutical industry, we need to appraise our standards for the publication of original research. When we do, I think we will find that those most culpable are journal editors and reviewers (myself included), not the pharmaceutical industry.

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24-Hour Ambulatory Blood Pressure Monitoring

To the Editor: Gardner and Schenider's article on 24-hour ambulatory blood pressure monitoring (ABPM) in primary care is missing many key elements as an original research article (Gardner SF, Schneider EF. 24-hour ambulatory blood pressure monitoring in primary care. J Am Board Fam Pract 2001;14:166–71). There is no clearly stated hypothesis, key question, or purpose to be tested. The authors state, "a MEDLINE search failed to find any published studies describing the role of 24-hour ABPM in the office setting to improve management of suspected or established hypertension in their patients." The reader subsequently assumes such will no longer be the case once the authors' research is presented. This assumption, however, is incorrect, because the authors go on to describe something entirely different.

With considerably more attention to an appropriate study design and the properly anticipated results, this study could contain valuable information for primary care physicians. An example of an ideal research question would be, "after 24-hour ABPM and subsequent pharmacy recommendations, does the patient's blood pressure improve?" The authors report nothing more than the referring physicians' accepting 100 percent of the pharmacists' therapeutic recommendations. They failed to document any sort of improvement in the patients' blood pressure as a result of the pharmacists' interventions. Simply because physicians accepted the pharmacists' recommendations does not imply patient benefit from the intervention, as the authors repeatedly report. The authors seemingly gathered a great amount of blood pressure data on 660 study participants. The obvious means of determining the feasibility of 24-hour ABPM would be to compare the mean blood pressure after the pharmacists' recommendations compared with the mean clinic blood pressure before the intervention. These results could then be analyzed with a simple statistical test to determine the significance of the intervention after 24-hour ABPM.

Aother vital component in reporting the results of a study is describing the methods of data analysis. The data analysis section of the authors' article lacks any sort of scientific merit. In fact, the authors lack any data analysis whatsoever, and their analysis more appropriately belongs somewhere in the results and discussion sections.

The discussion contains many vague statements. As one example, the authors state, "Unnecessary intervention was avoided in approximately one third of patients referred for evaluation of isolated office hypertension." The authors take quite a leap to draw such a conclusion when no data regarding the impact of their interventions are included in the article.

In summary, after reading this article, the primary care physician would still be unclear about the true benefit, in terms of improving patients' care, of 24-hour ABPM. Any clinical application of this study is completely lacking. The only conclusion that can be made is that physicians accepted 100 percent of the pharmacists' recommendations. Whether 100 percent of these recommendations resulted in improvement of the patient's hypertension remains to be answered.

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CPT Coding by Family Physicians

To the Editor: I read with interest the article "Accuracy of CPT Evaluation and Management Coding by Family Physicians" by King and colleagues (King MS, Sharp L, Lipsky MS. J Am Board Fam Pract 2001;14:184–92). This study highlights that our current system of billing for office visits is dysfunctional and unnecessarily complicated.

Last year I attended a small-group workshop of expert coders. We were presented with several written cases similar to those described by the authors. We had the written instructions for determining the correct code in front of us. We were given 15 minutes to review each note and assign a code. There was not one case during the entire workshop on which we all agreed. Even after discussion and consensus had been achieved, this consensus sometimes disagreed with the "correct" answer given in the workbook.

For most patients I see in the office, the diagnosis is readily apparent, and the treatment is straightforward; the most complicated cognitive function I have to perform is the correct assignment of the CPT/EM code. I also have to do this as the patient is walking out the door, before I ever dictate the office progress note and long before I can review it in writing.

The study by King and colleagues serves again to illustrate the point that our current scorecard approach to office billing is dysfunctional. It is too flawed to fix and needs to be completely abandoned for a simpler system.

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Fragrances and Allergic Reactions

To the Editor: The incident cited by Dr. Lessenger[1] can be added to the growing number of clinical and anecdotal accounts of fragrance causing and triggering serious health problems. Fragrances are an emerging health concern.

The use of scented products has increased dramatically since the 1970s. Formulations of scented products have also been changed. More than 80 to 90 percent of fragrance materials are now synthesized, the most from petrochemicals. Powerful synthetic materials are used at higher levels, and three to five compounds might make up to 80 percent of the fragrance formula. Products are designed to be strong and tenacious. Fragrances are complex mixtures of volatile compounds, most with little available health and safety data.

Fragrances pose many health concerns. They add to indoor air pollution and are respiratory irritants. There are no tests to determine whether they are respiratory allergens, but they are frequently cited by asthmatics as causing or triggering asthma. The Institute of Medicine (IOM), at the request of the Environmental Protection Agency, reviewed the medical literature on the impact of fragrances on asthma. The IOM placed fragrances in the same category as second-hand smoke in triggering and exacerbating asthma in school-age children and adults.[2] Respiratory effects are not the only concerns related to fragrance.

Fragrance is second only to nickel as a skin allergen.[3] Phthalates used in fragrances are suspected of being hormone disruptors.[4] Synthetic musk compounds bioaccumulate in human tissue and are found in breast milk.[5] Citral, a common fragrance flavor compound, causes enlargement of the prostate gland and has estrogenic effects.[6] Fragrance is also a frequent trigger for migraines.

Primary care physicians need to be aware of the impact scented products might have on the development and exacerbation of respiratory conditions. Patients need to be educated about the impact fragrances can have on their health.

Scented products used by staff and in cleaning products can adversely affect both patients and staff. Materials used for cleaning should be fragrance free as much as possible. Air fresheners and other sources of scent should not be used. Staff should use laundry and personal care products that are free of perfumes. There needs to be awareness that scented products in health care facilities can be a barrier to accessing health care for those that are severely sensitive to fragrance.

Scented products are ubiquitous. There is widespread exposure to virtually every segment of the population, including the unborn. Much more study and awareness are needed to assess the impact these products might have on health.

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Sexually Transmitted Disease

To the Editor: I was confused by the proposal in the recent article by Lambert (Lambert EC. College student's knowledge of human papillomavirus and the effectiveness of a brief educational intervention. J Am Board Fam Pract 2001;14:178–83). The theme of the article was preventing the spread of sexually transmitted diseases by using condoms. In particular, it focused on human papillomavirus (HPV). According to a family practice monograph from the late 1990s, the spread of HPV was increased by use of condoms. Thus, condom use increases the risk of genital warts and cervical cancer. This article completely ignores this information. There is also mention in the same monograph that a study found that 29 of 89 condoms tested leaked human immunodeficiency virus (HIV)-size particles. Meta-analysis had shown that the risk of HIV infection was decreased by 69 percent when using condoms. That still leaves a considerable risk.

In practice I try to give my patients information that is accurate and that will help them live happier, healthier, longer lives. Recommending condoms as a means of safe sex is not included. Locally we use the You Are Unique program to teach sexual abstinence outside marriage. A questionnaire is completed before and after the program. There are always students who change their attitudes concerning sex outside marriage. The program is an abstinence-only program. Programs that recommend condoms yet call themselves abstinence based typically result in either a rise or no change in the teen pregnancy rate. I find it much more beneficial to the patient to recommend the only safe sex, which is a mutually monogamous marriage. Lying to a patient about condoms and HPV to decrease (not prevent) the risk of HIV infection is unethical.

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Children of Divorce

To the Editor: This letter is in response to the article by Dr. Bryner, which appeared in the May issue of the *JABFP*.[1]

In view of current psychosocial research, it is incomprehensible for anyone to assert that the differences between children affected by divorce and children in intact families are extremely small statistically and nearly insignificant clinically. Before his assertion, Dr. Bryner compares divorce to the death of a parent, and he states that divorce might actually be harder on a child because it lacks the concrete cause and finality. Following this line of reasoning, should we believe that a parent is of little consequence in the life of a child?

Dr. Bryner correctly alludes to our many self-serving and reassuring notions about children of divorce as illusions that come from short-term studies. In fact, very few researchers have studied children of divorce longitudinally into adulthood. Those who have, such as Judith Wallerstein and colleagues,[2] have discovered that not only is the emotional damage after divorce long lasting, but the full effects of divorce might not be realized until well into adulthood.

Although the preponderance of contemporary social and behavioral research supports keeping both parents substantially involved in the lives of their children after divorce, our child custody statutes and judicial policies still overwhelmingly promote sole maternal legal and physical custody after divorce. Because the noncustodial parent is given little or no legal parental responsibility or privileges, and because the standard visitation-parenting time schedule is essentially 4 days a month (every other weekend), this particular parenting arrangement effectively creates or promotes the formation of single-parent families. It is disappointing that there has been no particular challenge by the medical or mental health profession to this traditional postdivorce parenting relationship, despite a huge and ever-growing mass of evidence that such relationships are not sufficiently nuturing or stabilizing for many children.

In a study published in the June 2000 issue of *Pediatrics*, Kelleher and colleagues state that from 1979 to 1996 psychosocial problems have increased from 6.8 to 18.7 percent of all pediatric visits to primary care physicians among 4- to 15-year-old children.[3] These problems have become the most common reason, of all chronic conditions, for pediatric visits to a physician. Dr. David Satcher, in his Surgeon General's Report on Mental Health (1999), cites research that estimates almost 21 percent of US children aged 9 to 17 years have a diagnosable mental or addictive disorder associated with at least minimum impairment.

Kelleher et al point out that this marked decline in the well-being of our children clearly parallels a dramatic increase in the proportion of single-parent households and associated childhood poverty within the last two decades. These two specific variables, among others, have been unequivocally validated by other research as key indicators of poor outcomes for children. Although the problems facing our children are undoubtedly multifaceted, a compelling argument can be made that the most important means to prevent childhood psychosocial morbidity lie less within the primary health care system and more within the realm of our public policies, which ultimately cause or promote the formation of single-parent families.

Although it is well and good that our profession develop strategies to address better our children's mental health and emotional needs, I hear no one suggesting that part of this effort should include advocating a change in our public policies. Even though our children are clearly in crisis, our child custody laws and judicial attitudes still perpetuate the notion that our society is somehow better off by discouraging the meaningful and substantial involvement after divorce of one of the two most important persons in a child's life—a father. This belief continues despite the all too apparent tragic consequences of absent or inadequate parenting on the streets of our cities, in our schools, in our foster care system, in our courts and juvenile justice system, and within our medical and mental health systems. No matter how many programs our government funds, improving our children's life prospects will ultimately depend on protecting and strengthening our children's family relationships.

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