Letters to the Editor

To the Editor:—I read with interest the results of the 1996 survey Gordon and Sobel performed on the interest level of clinicians in "alternative therapies" (Use of and Interest in Alternative Types of Therapy among Clinicians and Adult Members of the Kaiser Permanente Northern California Region: Results of a 1996 Survey, Permanente Journal, 1999;3:44-55). They asserted that nearly 90% of clinicians reported recommending at least one type of alternative therapy. I would suggest that this number is quite inflated since many of the higher percentage modalities were merely either standard, supportive types of things a doctor might say or simple extensions of standard medical practice. They even alluded to this in their note about special diets, at the bottom of Table 1. Other examples are the following:

• Massage therapy. This is nothing more than good backrubs. Giving this some legitimacy by calling it a therapy is typical of the good PR sought by purveyors of nonscientific belief systems. They want to make you believe they actually have something to offer when in fact it is either snake oil or just plain common sense. A backrub makes anyone feel better. It is not a therapy and giving it this weight only inflates the number. This accounted for 42.5% of the "manipulation therapies." It should not have accounted for any of it and similarly, should have reduced the total number interested in alternative therapies.

• Meditation/mindfulness. More psychobabble. If I were to tell someone to close their eyes, try to relax, and think of vacations in Hawaii, does that make me a supporter of "alternative therapies"? Spare me and count me out. Those who voted for this (a hefty 48.9%) should also be voted out and should not have been counted.

• Relaxation techniques. Once again, if I tell you to relax, does that mark me as an alternativist? Should I have crystals sitting in each exam room? Does that mean coffee enemas are just around the corner from my office? Those choosing this accounted for 67% of their sample choosing "mind/body therapies."

• Last but not least, I was amazed that they chose to include the entire supportive therapies category as an indicator of interest in "alternative therapies." Since when is a 12-step program or support group an indication of interest or support for alternative therapies? Same for psychological counseling, unless these people are being referred to unlicensed charlatans practicing New Age gobbledy-gook. A full 85% of the clinicians indicating interest in alternative therapies chose this supportive therapies category. The authors inferred this as a marker of interest in alternative therapies. That is an indefensible extrapolation of data. The clinicians surveyed were only confirming that these interventions are a part of good standard medical care. They were not reaching out to exciting new vistas of new millennial medicine. It smacks of favoritism and a lack of objectivity. One wonders if the authors have an interest in seeing "alternative therapies" broadly added as covered benefits. I'm afraid their manipulation of the data smells of this bias.

In summary, I believe their study is seriously flawed and its conclusion that "clinicians are highly interested in incorporating CAM into their medical care" (P. 54) is at best soft, and at worst, an unsubstantiated affirmation that can lead us down the road back to the Middle Ages.

Promoting nonscientific populist nostrums is a disservice we don't need to offer to anybody, demand be damned. The Enlightenment already occurred two hundred years ago. Do we have to relearn those lessons?

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In Reply.—Our critic believes that our study and our interpretation of the results[1] are seriously flawed, and that our conclusion about clinician interest represents "an unsubstantiated affirmation [of 'nonscientific populist nostrums'] that can lead us down the road back to the Middle Ages."

The key questions arising in response to the letter are:

1. Were the definitions used in the study comparable to the definitions used in other studies and the by NIH?
2. Were the definitions of different alternative therapies clearly spelled out in the survey?
3. Were our comments about the extent of clinician interest in alternative medicine justified by the survey data?
4. Did we intend to imply the scientific validity of different alternative therapies and promote their use or to describe the results of a scientifically conducted study?

The first question addresses our critic's remark that our report inflated the use of alternative therapies as treatment modalities by TPMG clinicians in the Northern California Region by including many modalities that are either "standard, supportive types of things a doctor might say or simple extensions of standard medical practice." Our list of alternative therapies

parallels that used by Eisenberg et al in their widely cited survey of alternative therapy use among adults in the United States.[2] In developing our list, we decided to omit commercial weight-loss programs and exercise because we felt that these methods had already become part of conventional medicine within our setting. We added psychological counseling to the list because TPMG physicians were being encouraged to refer patients for counseling when they felt a patient's health problems could be improved by psychological intervention. The other therapies the writer singles out—massage therapy, meditation/mindfulness, relaxation techniques, and 12-step/support groups—were all included in the Eisenberg et al list, and have also been included in the lists of most of the physician surveys we cited in our article, and at least currently fall under the NIH Office of Alternative Medicine's definition of alternative therapies, ie, "those forms of medicine not traditionally taught in Western medical schools."[3]

In response to the second question, to ensure that clinicians and members knew what we meant by different types of alternative modalities, we included brief descriptions of each therapy in the survey questionnaire. For example, meditation/mindfulness techniques were described as techniques "used to relax the body and mind by focusing attention on different objects, including sound, a repeated word or mantra, a visual image, or body sensation. Mindfulness meditation involves focusing on the sensations of breathing to improve concentration and calming of body and mind." Relaxation techniques were also described as "techniques used to relax the body and mind. Different types of relaxation techniques include deep breathing, progressive muscle relaxation, and imagining a relaxing scene." Neither of the descriptions of these techniques, which have been used successfully by psychologists and other behavioral health specialists for years to treat stress and stress-related health conditions, can be construed as simply telling a patient to "relax or think about a vacation." In most cases, the clinicians were not actually teaching their patients how to do these techniques but instead were referring them to psychologists or behavioral health education programs for instruction. Finally, almost anyone who has had a massage from a trained massage therapist would say that it is more than a "good backrub" which an untrained spouse or friend might deliver.

The third question addresses the critic's concern that we imply that clinician use/recommendation of supportive therapies (psychological counseling, 12-step and support groups, religious health or prayer) is an indicator of clinician interest in all alternative therapies. When we interpreted the results of our survey as indicating a fairly high level of primary care clinician interest in use of alternative therapies, we based this on clinicians' responses to two general questions: "In general, how interested are you in the use of alternative therapies to treat health problems, alone or in combination with more traditional Western medicine approaches?" and "In the future, would you like to see Kaiser Permanente health care providers have greater opportunity to use alternative therapies to treat health problems?" Two thirds of APC physicians and three fourths of ob/gyn clinicians indicated that they were moderately or very interested in use of alternative therapies, and similar percentages answered "probably yes" or "definitely yes" to wanting greater opportunity to use them within their practice.

When we described the proportions of clinicians using/recommending alternative therapies, we clearly stated that 93% of both APC and ob/gyn clinicians had used or recommended to patients *at least 1 of the 20* alternative therapies on the list we provided. We reported that this percentage dropped to 89% *when we excluded special diet and the supportive therapies* (psychological counseling, religious healing or prayer, 12-step or support groups), the very categories that the writer has criticized us for including in our list. We did not infer, as our critic suggests, that these clinicians used or endorsed all 20 therapies. In fact, it is the author of the letter who seems to lump together relaxation, crystals, and coffee enemas. Our purpose was to identify the differences in interest of the many different forms and varieties of alternative medical practice. We did this in Table 1, which presents data on the percentages of APC and ob/gyn clinicians who reported use or recommendation of these 20 different therapies during the previous 12 months. In Table 3, we presented data about why primary care clinicians have been reluctant to use or recommend specific alternative therapies, and on page 47 reported the percentages of primary care clinicians that would like or would not want to have specific alternative therapies covered by the Health Plan.

Finally, in response to the fourth question, our purpose was to assess interest and use, not to comment on the scientific validity of the practices. We agree that decisions to recommend or cover with

insurance should ideally be evidence-based—this should apply to conventional Western medical practice as well as to alternative therapies. We need more objective, scientific study to counter both uncritical acceptance of all therapies as well as rejection based on anger and bias.

References
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2. Eisenberg DM, Kessler RC, Foster C, et al. Unconventional Medicine in the United States: Prevalence, costs, and patterns of use. NEJM 1993;328(4):246-252.
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To the Editor.—Lopez and Buell in "Coming Clean: Community Partnerships for Tattoo Removal" (Permanente Journal, 1998:2;88-91) describe an admirable program to treat tattoos among street people and gang members. While we support the goals and objectives of the program, we would remind the doctors and others embarking on such programs of the "Law of Unforeseen Consequences."

Tattoos have been used over the millennia in a wide variety of cultures for group identification and protective effects.[1] Other physicians, and we who treat large number of homeless and indigent patients, have noted that patients with four or more amateur tattoos are much more resistant to major illness and likely to survive diseases which would kill most people whose skin has not been discolored by the ink of cheap ballpoints inserted into pinholes. This rule was supported in an informal poll by every clinician working with homeless people who attended a national meeting of medical directors of homeless health centers. One went so far as to affirm, "No one with H, A, T, and E tattooed on the knuckles of the right hand and L, O, V, and E on the left has ever died of a medical illness."

Thus, the Law of Unforeseen Consequences would suggest that by removing tattoos from their patients now, Lopez et al may be putting them at risk of future death. This observation opens the subject for several research projects. I would suggest that a cooperative Kaiser Permanente study of intensive care unit admissions could follow patients with more than four tattoos and compare their APACHE scores and mortality rates against known standards. Alternatively, outpatients who qualify could be matched with untattooed controls and followed over time. We hope that Lopez et al plan to follow their cohort to see what their experience after the tattoos are removed proves to be. It may be that erasure of the tattoos will restore these patients to the higher risk group. Or, the tattoos may continue to have their protective effect even after removal, which might indicate that it is not the tattoos themselves, but some other demographic or medical factor for which tattooing is only a marker.[2]

In sum, while we support the efforts of Lopez et al to erase unwanted tattoos in homeless people, we urge both caution in embarking on any project that may have unforeseen consequences and further study of the questions of tattoos' long-touted, but unproven, protective effects.

References
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2. Sacket DL et al. Clinical Epidemiology: A Basic Science for clinical Medicine, 2nd ed. Boston, Little Brown, 1991.

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In Reply.—As the clinicians involved in the tattoo removal project (Coming Clean: Community Partnerships for Tattoo Removal, Permanente Journal, 2:1998, 88-91), we are pleased to respond to the interesting and provocative letter from Drs. Rendleman and Bennett.

While the claims struck us as unorthodox, we decided that we would apply the basic principles of evidence-based medicine to see if there was any good information available to answer the contentions in the letter.

We asked Trina Histon of our Care Management Institute here in Oakland to do a search of the medical literature, specifically looking for articles and studies on the potential health effects of tattoo removal. She searched MEDLINE, STAT-REF and various journals unsuccessfully, finding only the following quote from her Bible study memories. "Thou shalt not make cuts unto your body for the sake of the dead or tattoo any marks upon you; I am the Lord" (Leviticus 19:28).

Her formal search did not turn up any studies documenting health effects of tattoo removal. Tattoos themselves have been associated with several blood-borne diseases. Of interest, as is too often the case, when

we finally were able to obtain an English translation of the referenced German publication, it had nothing to do with the points being made. The title of that German article is "The Significance of a Single Tattoo in Sociopaths."

Had we found studies detailing health effects of tattoo removal, we would have graded them on the basis of strength of experimental design, summarized the findings in an evidence table, and attempted to construct a balance sheet detailing the benefits and harms to the patient by the intervention of removing existing tattoos.

Tattoos have been used medically in breast reconstruction surgery, for marking polyps in the colon, and in managing gingival vitiligo. Removing tattoos from gang members seeking to reenter mainstream society has been studied, and the benefits have been documented. The young people whom we work with commit themselves to community service and undergo a mildly painful procedure in their desire to be free of their gang stigmata.

In summary, in the absence of any scientific evidence of harm from removing tattoos, we plan on continuing a community collaborative program voluntarily entered into by the young people who are hoping to redirect their lives.

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To the Editor:—Just got the latest (Summer 1999) issue of *The Permanente Journal* and want to say that you are doing a great job. I particularly like the Permanente Abstracts section and the Original Research because they are unique to our organization and help raise our consciousness. It would be better if we could dispense this info to the outside world, since our excellence is one of the best kept secrets in US medicine. Preaching to the choir is fine, but it is even better if the sermon is more widely heard. How about a more widespread circulation list à la *Mayo Clinic Proceedings*, *university wellness letter*, etc.?

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To the Editor:—Thank you for the excellent article on hemochromatosis by Dr. Vincent Felitti (Hemochromatosis: A Common, Rarely Diagnosed Disease, Permanente Journal, 1999;3:10-20). After reading this, I began to do some screening of patients in my internal medicine practice and, in particular, one male patient in his thirties who was completely asymptomatic but who had borderline elevated liver enzymes for a year and a half with negative findings otherwise. This patient tested positive with a serum ferritin level over 5,000 µg/L. He has been referred now for further testing and treatment, and the family was also notified. His father, also asymptomatic, also tested positive for hemochromatosis and is receiving treatment as well. Prior to review of this article, I had not been aware that this medical condition could be so prevalent. I will certainly not overlook it in the future and hopefully will have a positive impact in maintaining the health of my patients because of this excellent article.

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Focus

You can't depend on your eyes when your imagination is out of focus.
Mark Twain