LETTERS TO THE EDITOR

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SCREENING MAMMOGRAPHY GUIDELINES

To the Editor:

Dr Steven Taplin, in his editorial on breast cancer screening (*Breast cancer screening: A curious problem in primary care. J Fam Pract 1989; 29:247–248*), never quite gets to the heart of the "curious problem" of physician noncompliance with mammography guidelines. He heads off in the right direction in the next to last paragraph, when he discusses the "ambiguity introduced by indeterminate findings that leave the primary physician with the responsibility of tracking women" and not just tracking but also calming and, finally, reassuring, since the vast majority of these women worry needlessly.

More intense worry comes to those women who have positive findings on mammography but eventual negative biopsies. For every cancer found by screening, anywhere from 5.6[1] to 11[2] women must live under the cloud until their biopsy results finally come back.

But Wright et al[1] claim the ratio of harm to benefit is even higher. Their reading of the mortality-based studies indicate that cancer detected does not automatically equate to increased survival. For instance, the screening mammogram may not affect outcome in the patients at the two extreme ends of acuity spectrum: indolent cancer in situ vs early metastasis. Discounting for the minimal impact of mammography on survival in these cases, they calculated a harm-benefit ratio as high as 62 biopsies for 1 patient benefited. The authors go on to look at the number of people who need to be screened (as opposed to having a biopsy) to benefit one person. They calculate a ratio as high as 2,041:1. A European critic estimates a ratio of 10,000 women screened to 1 benefited.[3]

Robin[4] argues that even some harm comes to those whose primary mammogram is negative; the mere suggestion that screening is needed induces some level of cancer phobia. Barsky[5] supports the notion that technology has a psychological cost; he points out that although we are objectively healthier, we are worrying more. Result: a net loss in happiness.

The family physician need not consciously know all of the above statistics to become noncompliant. If he has practiced long enough to experience on one hand the relatively low yield of mammography, and on the other hand the anxiety induced by the more frequent false-positives, he may subconsciously put off ordering mammography. Or, if it all comes to consciousness, he may invoke the old saw: first do no harm. Of course, this invocation requires the suppression of the interventionist bias in American medicine[6]; it also means ignoring the growing probability that failure to screen, even in the face of patient refusal, may be grounds for malpractice.[7]

The noncompliant physician may further rationalize his benign neglect by citing cost-effectiveness data.[8] These sorts of data have no doubt influenced third party payers who continue to drag their feet on reimbursement for screening.[9,10]

Current standards of care dictate that mammography should be done, particularly in women over 50 years. Of course, primary physicians must commit to the goals of risk reduction set by the larger society. The challenge is to put our professionalism ahead of our "anecdotal" experi-

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ences even though the frequency of detection is low and the costs, both monetary and emotional, often seem high. If primary care physicians believe these costs of screening are too high, they should increase their participation in setting realistic standards. Fortunately, it seems we now have a forum where we can share our unique perspectives: the US Preventive Services Task Force.

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

I agree with Dr Lee that I never quite get to the heart of the "curious problem" about the use of mammography, but the reason is simpler than he states. No one knows where the heart of the problem resides.

He has expanded on a point I made and raised important questions about the risk of mammography (ie, unnecessary biopsies). Whether it influences physicians' use of mammography, however, has not been investigated and is only another intriguing hypothesis. This risk has not been properly highlighted in the rush to promote mammography. How much risk it represents depends not only on the proportion of mammographies that lead to biopsy, but also upon how those biopsies are performed (ie, under local or general anesthesia). The reality that this risk exists needs to be presented to women.

It is clear to me that this risk of mammography is a reason to temper how often we order this procedure, and to whom we offer it, but it should not be a reason to avoid ordering it in women aged 50 years and above. I do not think its routine use in all women 40 to 49 years old is warranted, as I have pointed out elsewhere.[1] The efficacy of mammography in reducing mortality in women aged 50 years and above is clear.[2,3] The issue now is the optimal interval in this latter age group. An alternative to a single interval in all women has been proposed in the United Kingdom and implemented in at least one HMO in the United States.[4,5] In this program, breast cancer risk factors are used to vary the interval for screening. The reasoning for varying the interval is to concentrate the use and risks of mammography in women who will most benefit.

Van Der Maas and colleagues[6] consider these risks and their costs in a recent cost-effectiveness analysis. They conclude that only one third of the screening costs will be compensated by savings in assessment and treatment of early vs late disease, that a 2-year interval appears optimal, and that a 12% reduction in mortality will occur. They point out that such a reduction will result in more lives being saved than currently occurs as a result of cervical cancer screening.

If we would save even the same number of lives as with cervical cancer screening, then the use of mammography seems warranted. I agree with Dr Lee that family physicians should be vocal in setting the standards for how that will be done, and that the US Preventive Services Task Force is the most reasonable forum.

I would add that we also need to contribute to the fund of knowledge about physician behavior with respect to the use of preventive services. Dr Lee provides a plausible but untested hypothesis about our reluctance to order mammography. The challenge is to test it.

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DIFFERENTIAL DIAGNOSIS OF VAGINITIS

To the Editor:

I read with interest your recent article by Reed et al.[1] Vaginal yeast infection is a common complaint in women of childbearing age. As discussed in their review, the diagnosis of this infection on clinical or historical grounds alone is unsatisfactory, and confirmation of yeast infection is required. Direct microscopic examination of vaginal smears is notoriously insensitive, and most investiga-

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tions report a correlation of only 36% to 45% with candidosis.[2–4] Culture methods are unable to differentiate commensal from pathogenic yeasts, and whereas the sensitivity of this method is high, positive predictive values of only 36% to 46% have been reported.[3,5] The new latex particle agglutination (LPA) test mentioned in the article of Reed et al[1] has been widely evaluated and has been found to have both high positive predictive values (76% to 100%)[2,3,6] and high sensitivity. We found the sensitivity of this test for the presence of *Candida albicans* to be greater than 65%[5] (not 36% as quoted in the article), which compares with sensitivities of 72% to 81% reported in other studies.[2,3,6] A positive correlation was observed between signs and symptoms of infection and yeast load. The likelihood of a positive reaction with the LPA test also increased with the number of yeasts present in the vagina; thus the test was able to differentiate true infection from women harboring commensal yeasts. In our hands the LPA test proved to be a satisfactory alternative for the diagnosis of vaginal candidosis and was able to yield rapid results, within 2 to 3 minutes, an important consideration for an in-office test.

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

The above letter by V. Hopwood reiterates the findings that the in-office diagnosis of *Candida* vulvovaginitis based on clinical findings and potassium hydroxide slide preparations has been notoriously inexact, as reflected in our study[1] and others. The search for improved methods for making the rapid diagnosis of *Candida* vulvovaginitis in the office setting is an important one if this inaccuracy is to be lessened.

The sensitivity of the slide latex agglutination test (SLA) as quoted in our paper reflected the comparison of this test with that of culture in all patients evaluated by Hopwood et al in their study.[2] The 65.2% sensitivity of the test, as quoted by Hopwood, indicates the comparison of the test with culture in patients meeting specific clinical criteria and having a positive culture or microscopic slide test. This discrepancy between these sensitivities exists because the slide test is more likely to be positive in those with large numbers of organisms present than in those with fewer organisms. The association between specific clinical symptoms (such as pruritus) and the numbers of organisms present has been similarly reported by Odds et al,[3] although similar associations between numbers of organisms and other symptoms and signs have not been consistent. A test such as this that may selectively identify those patients with greater numbers of *Candida* organisms may therefore be useful in clinical practice. Further research is needed to confirm that the patients identified by this test are actually those who benefit from treatment, and that those with smaller numbers of organisms who are only identified by culture do not eventually require treatment as well.

Pending further studies, the use of these in-office tests in the diagnosis of vaginitis may improve the diagnostic accuracy in many cases. When symptoms recur or persist, evaluation by culture would still be recommended.

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DECISION AND COST-EFFECTIVENESS ANALYSIS

To the Editor:

I was quite interested to see the presentation of a formal decision analysis in "Screening for Asymptomatic Bacteriuria in Pregnancy" (*Wadland WC, Plante DA. J Fam Pract 1989; 29:372–376*). Decision analysis is a very useful tool if used properly, and many "traditional" management strategies in family medicine need to be reassessed using this technique.

While I congratulate Wadland and Plante for the thoroughness of their model, I would like to point out some potential errors incorporated into their particular decision analysis. First, the estimation of the combined sensitivity and specificity of the miniculture dip slide is not in concordance with accepted methods. The dip slide tests two independent etiologies for bacteriuria, and if either paddle is positive, bacteriuria is said to be present. Therefore, this is an application of parallel testing using a disjunctive positivity criterion, which has been shown by Cebul et al[1] to yield a combined sensitivity (where T1 is test 1 and T2 is test 2) of: (sensitivity of T1) + [1 − (sensitivity of T1)]*(sensitivity of T2). The combined specificity of T1 and T2 is: (specificity of T1)*(specificity of T2). Using this method, the combined sensitivity of the two paddles of the dip slide is 1.00; the specificity is 0.321.

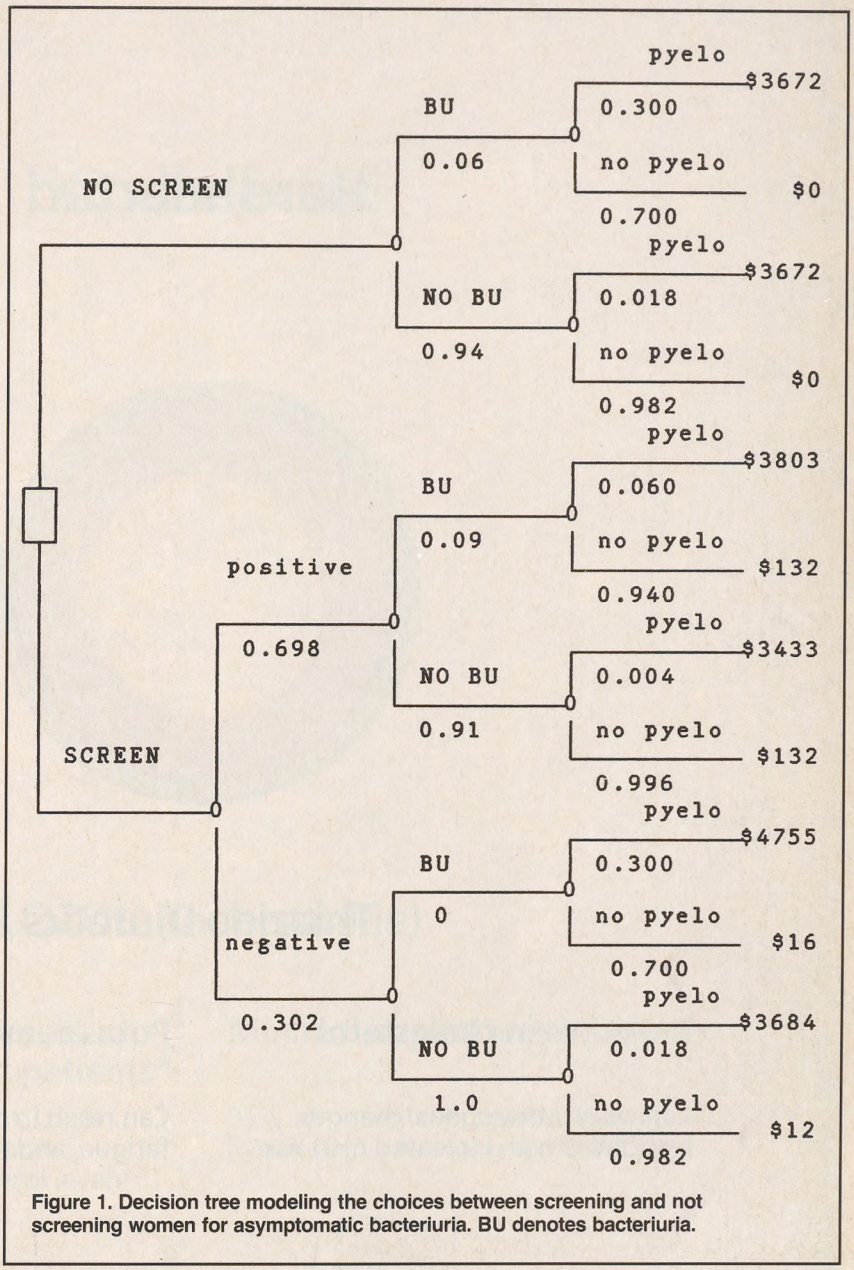

Figure 1. Decision tree modeling the choices between screening and not screening women for asymptomatic bacteriuria. BU denotes bacteriuria.

Reconstructing the decision tree using these data and Bayes' theorem, some significant changes in the probabilities of several branches of the Screen option result (see Figure 1).

Second, it is stated that the CLED panel of the dip slide identifies 5% of the pathogens responsible for urinary tract infections. In point 2 of the charge assumptions, the authors assume that 5% of positive dip slides will come from this CLED panel.

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This assumption does not logically follow from the first statement, since urine specimens with 5% of all pathogenic species may not be the same as 5% of all urine samples with pathogenic bacteria in them.

A third point has to do with the somewhat unusual presentation of the end-point values of the decision tree as "weighted branch charges." The authors presented these charges as already "averaged out" for 100 patients presenting, instead of the usual method of showing the actual charges for a given patient ending up at that end-point, as one would do prior to "averaging out" the tree. This slight departure from "standard" presentation has no effect on the final analysis.

If the model is reconstructed to include the revisions mentioned above, we find that of 100 patients presenting, if we do not screen for asymptomatic bacteriuria, 3.5 cases of pyelonephritis may result. If we do screen, only 1.5 cases of pyelonephritis should be expected. As might be anticipated, not screening costs less than screening, but the cost of screening amounts to only about $486 per case of pyelonephritis prevented, subject to the authors' baseline assumptions.

I believe that decision analysis and cost-effectiveness analysis are powerful and important tools with significant applications in family medicine. However, the possibility of faulty assumptions or methods becomes significant with these complex models, and we must take extreme care in our formulations if we expect these analyses to be credible.

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Reference

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The preceding letter was referred to Drs Wadland and Plante, who respond as follows:

We appreciate the thoughtful comments by Dr Marley. With respect to his first two points, the following replies about parallel testing are in order. The formulae he cited are correctly applied to two independent tests which are used to diagnose the same disease. However, we assumed that there are two separate etiologies for asymptomatic bacteriuria, in essence two separate diseases. In 95% of the cases the true disease is potentially identified by the MacConkey paddle. In the remaining 5% of cases the true disease is potentially identified by the CLED paddle. We assumed the paddles to be independent tests and that only one "disease" or cause for bacteriuria could exist at any one time. For these reasons, we feel that the weighted sensitivity and specificity values used in the model are valid.

With respect to the last point, the actual computer model generated cost data as suggested by Dr Marley. For example, the actual unweighted cost of the top branch for a single patient is $3672. The costs column at the right of the decision tree were "weighted" for illustrative purposes only and are similar in concept to "risk profiles."

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COSTS OF ELECTRONIC INFORMATION SERVICES

To the Editor:

The October *American Family Physician* announces that "The AAFP recently unveiled an online electronic information service that will be carried on AMA/NET." This excites and disturbs me. It is good to see the American Academy of Family Physicians' concern, but the link to AMA/NET is questionable. This be-

came apparent to me at the annual SCAMC (Symposium on Computer Applications in Medical Care) conference in Washington, DC, November 5–8, 1989.

At SCAMC I attended the panel on "Identifying and Meeting Physician Information Needs." Dr William Yasnoff of AMA/NET, Dr Edward J. Huth, prestigious writer from the American College of Physicians, Dr Brian Haynes, acclaimed internist of Canada, and Patricia Ryan of PaperChase were on the panel. Among these giants, family practice needs became lost. There was one family physician, but he was not on the official panel. Dr Huth wrote the equation *value = utility/cost,* and then Dr Yasnoff directed the discussion to focus on why AMA/NET was not being used.

The American Medical Association is pushing AMA/NET to increase its membership, not to serve the information needs of family physicians. As a family physician who first trained as a medical librarian online searcher, I use GRATEFUL MED and DIALOG. They meet my information needs for about $50 per year plus online time. GRATEFUL MED is easy to access and inexpensive. It is my primary information resource. DIALOG, the oldest and largest commercial online service, provides me with more drug databases, business files, newspaper files, government document files, and other medical files than AMA/NET. I do not want to join the AMA to obtain the lower rate AMA members enjoy to search AMA/NET to find out about the AAFP. The subscription fee for using AMA/NET is $160 a year for nonmembers plus about $16 an hour of online time. Even with the occasional user plan of $75 per year, AMA/NET is still more expensive for less content than what I already have.

An electronic information service is a dynamic outreach for the AAFP, but why through the AMA? An AAFP electronic bulletin board would have cost less than AAFP's venture with AMA. Family practice departments could easily use their university's BITNET for such a communication service.

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PRIMARY CARE: READY FOR MERGER?

To the Editor:

There is only one important question regarding the proposal to merge family medicine with the other primary care disciplines.[1,2] Will combining the primary care specialties increase their collective political clout so that the important issues can be dealt with more effectively? Considering our different vested interests, I doubt it. By itself, a merger does not even partially resolve any substantive problems.

One such problem is that the downward trend in student interest has continued unabated in spite of the stepped-up marketing of the different programs. A merger—particularly one that expands training by 1 year—will not make primary care more attractive to students. Only fundamental changes, such as reimbursement reform or quotas for residency training positions, will alter the current primary care-specialist ratio of trainees.

Another major issue is the gatekeeper's role in cost containment. Primary care physicians are put in the untenable position of having all of the responsibility and none of the authority for controlling medical costs. Expensive advances in technology, defensive medicine fostered by malpractice litigation, and patient expectation of specialty referral on demand are largely beyond the control of the primary care provider. If cost containment is going to work, fundamental changes in the medical system and in public expectations are needed to empower gatekeepers.

Perhaps the greatest problem facing American medicine is access to medical care. Basic health care is looked at as a right of the American people, yet there is confusion as to what that right is and how it can be secured. Americans have the right to health care only if they can pay for it or if they can somehow work through the patchwork maze of public health care. Because employer-sponsored insurance is not uniform and because eligibility guidelines for entitlement programs are ridiculously low, the number of patients not covered by either grows daily.

I agree that it is logical to work for an eventual merging of the primary care disciplines, but logic does not seem to be the driving force in medicine today. Before embarking on this fundamental change in primary care education, we should wait until we evolve into a more rational, humane health care system. For now, let us leave this minor issue and focus on the real problems facing us.

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