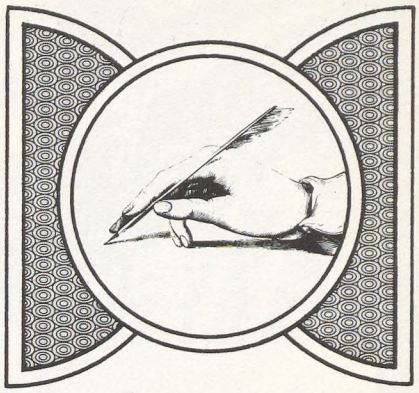
Letters to the Editor



Screening for Depression

To the Editor:

As one interested in the theory and justification for screening tests, I read with particular care the article "Recognition of Depression by Family Medicine Residents: The Impact of Screening" *(Moore JT, Silimperi DR, Bobula JA: J Fam Pract 7:509, 1978).* Although the authors show that notifying residents of possible depression increases the "diagnosis" of depression, several important questions remain.

Depsite Zung's own data on sensitivity and specificity of the screening instrument, the fact that in *this* study 96 of 212 patients were identified by the depression inventory as mildly to moderately depressed raises serious questions about either the instrument or the representativeness of the population studied. Surely a prevalence of depression of over 45 percent excedes estimates available for the primary care setting. The most likely explanation for this extraordinary rate of depression is that the instrument identified many as depressed who, on more rigorous examination, may not have been. The fact that residents notified of test results tended to agree perhaps says more about the suggestibility of residents than about the prevalence of depression.

The issue of the Zung instrument as a valid screening device is inadequately addressed in the article. What of the costs incurred by the false positive results—not only in terms of additional diagnostic tests, but the personal costs in patient anxiety about being incorrectly labeled depressed, and perhaps unnecessary drugs, counseling, and office visits? What about the depressed patients that the instrument misses? How does the Zung instrument compare to other diagnostic systems—is it the most cost effective?

I must disagree with the statement that screening for depression differs in some qualitative way from screening for other conditions. Labeling a person depressed or not depressed is difficult using any criteria, and the evaluation of a diagnostic tool as a screening test then becomes all the more critical. The criteria for depression were developed and, indeed, almost all well-designed studies of the effectiveness of treatment for depression have been done on groups of patients with advanced symptoms and fairly clear findings. Identification of earlier forms of depression is of unknown significance, particularly with respect to the effectiveness of treatment—two

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areas rather glibly taken for granted by the authors. Unfortunately, the logic followed by the authors in recommending the Zung test as a screening device is precisely that used to justify discredited screening tests such as the routine chest x-ray and multichannel blood tests: "We're just trying to stimulate physician recognition of treatable problems." A great deal more information regarding the specificity, sensitivity, costs, benefits, and outcomes is necessary before the Zung test can be routinely recommended.

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The preceding letter was referred to Drs. Moore and Bobula who respond as follows:

Dr. Berg raises several questions in his letter which we will attempt to address. Approximately 45 percent of our study's patients scored in the depressed range on the screening test. This led Dr. Berg to wonder, first, about the representativeness of the patient sample and, secondly, about the validity of the screening instrument. The issue in this study was not to systematically determine the prevalence of depression in the general population, but rather to assess the extent to which depressive symptoms were recognized by physicians in one practice setting, Thus, the representativeness question is academic; however, it is of interest to note that estimates of the prevalence of psychiatric morbidity in general medical practices vary widely, eg, a recent study identified 83 percent of one clinic's patients as having psychiatric morbidity.[1] Dr. Berg did not cite the studies supporting his feeling that 45 percent is a high prevalence finding.

While the question about the screening test seems more pertinent, Dr. Berg does not suggest any specific reasons to impugn the validity of the SDS other than the number of persons it identified as having depressive symptoms. In addition to the validation studies referenced in the manuscript, a recent study is pertinent. Raft and colleagues[2] screened 69 patients in a medical outpatient clinic with the SDS. Each patient also received a psychiatric interview. In this setting, 30 percent of patients had SDS index scores greater than 60 while 42 percent were considered depressed by the interviewer. The authors concluded that the weakness of the SDS as a screening test was the high number of false negatives, not of false positives as Dr. Berg suggests. (The number of false negatives may have decreased had the authors used an SDS index of 50 rather than 60 as an indicator of depression.) We continue in our opinion that validation studies of the SDS, coupled with its low cost and simplicity, support its usefulness as a screen for depression.

Dr. Berg states the fact that screening enhanced recognition of depression says perhaps more about the suggestibility of physicians than the prevalence of depression. Once again, prevalence was not the central issue. With regard to "suggestibility," we found that without knowledge of SDS results residents identified 22 percent of their patients with depressive symptoms (SDS≥50). With knowledge, identification rose to 56 percent. We construed this finding as

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residents' enhanced recognition of depression associated with screening. Had the recognition rate risen to 90+ percent, we too might have suspected "suggestibility"; we do not consider it the best explanation for the actual findings.

Dr. Berg disagrees with our statement that screening for depression should be thought of differently than screening for physical disease. We still contend that screening for depression differs from screening for most physical disease in that a physical screen probes for asymptomatic disease whereas the depression screen aims at identifying patients with manifest symptoms being overlooked by their physicians. We agree that proper diagnosis of depression is difficult and did not mean to imply that the SDS be used alone to make the diagnosis. But we do propose it as a reasonably valid measure of depression.

More widespread use of such measures could help prevent the documented underdiagnosis of psychiatric problems. This logic of stimulating recognition as a basis for intervention, apparently held in some disrepute by Dr. Berg, is central to all screening whether or not a particular screening method stands the test of time and continued scrutiny. We agree that questions remain about screening for psychiatric disorders—often a problem of detecting unrecognized rather than asymptomatic disease—and we hope to see more research efforts directed toward this area.

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References

1. Glass RM, Allan AT, Uhlenhugh EH, et al: Psychiatric screening in a medical clinic: An evaluation of a self-report interview. Arch Gen Psychiatry 35:1189, 1978

2. Raft D, Spencer RD, Tommey T, et al: Depression in medical outpatients: Use of the Zung scale. Dis Nerv Syst 38:99, 1977

To the Editor:

The recent article in *The Journal* highlighting the use of the Self-Rating Depression Scale developed by Zung (*Moore JT, Silimperi DR, Bobula JA: Recognition of depression by family medicine residents: The impact of screening. J Fam Pract 7:509, 1978)* was both enjoyable and stimulating. I would like to share with your readers some additional thoughts regarding the SDS as it is used in our family practice.

The SDS was introduced to me during my residency training and I think played a role in developing a sensitivity for affective states. Once the suspicion of possible depression is raised, the SDS is used to document and quantify the presence of depression. This can be used to help demonstrate to the patient that the physician's initial impression regarding the presence of depression was correct and can act as both a guideline for treatments (depending on the severity of depression) and a baseline for comparison in follow-up. In addition, certain target symptoms can be identified which are helpful when choosing medication if that is necessary.

The article also highlighted two reservations I have regarding the use of the SDS. The authors state that "failure to recognize depression among general medical patients may well subject patients

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to unnecessary, costly, and occasionally dangerous diagnostic procedures in an effort to identify a physical explanation for symptoms." Residents should be reminded that making the diagnosis of depression does not necessarily rule out organic disease. Experience may be the best guide in knowing when to proceed with tests. A second problem we have encountered is third-party reimbursement for conducting an SDS. In our area, psychologists charge an average of $10.00 for administering and scoring this test. Some third-party payers do not recognize a family physician's ability to administer this test and some will not pay for this type of test regardless of how it is administered or used. In spite of these drawbacks, we continue to find the SDS useful in clinical practice and hope that as a result of this recent paper more family physicians might become aware of this simple diagnostic tool.

Henry R. Ivey, MD
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Costs of Information System

To the Editor:

I read with great interest "*A Family Medicine Information Service: The Beginning of a Network for Practicing and Resident Family Physicians.*" *(Green LA, Simmons RL, Reed FM, et al: J Fam Pract 7:567, 1978).* I have questions relating to the cost of their computer system.

What is the cost of the programing and is programing available to other family practice programs?

The authors quote monthly costs, cost per patient encounter, and cost per registered family per month. The costs stated in the text do not coincide with the costs illustrated in Table 1. In the 35-patient per day practice, the costs illustrated in the table are higher than the intown practice by 51 percent and 53 percent; in the rural practice, the costs are higher by 78 percent and 81 percent. Likewise, the 70-patient per day practice costs again are higher in the table than in the text by 14 percent and 19 percent in the intown practice, and 33 percent and 34 percent in the rural practice.

I feel that to be fair to readers who are not aware of computer costs, the costs of programing should be stated. These costs could be amortized over a three to five-year period. For instance, if the cost of software is $25,000, you could amortize these costs over three years. In an 18,000 patient visit-per-year practice, which equals to 54,000 over three years, the additional cost per encounter is 46 cents. In your 9,240 patient per year practice, or 27,720 patients per three years, the additional cost per encounter is 90 cents.

Often, we in academic medicine and people selling computer services fail to take into account the total costs in their encounter, and if we are going to have credibility with the practicing physician and the resident in training, he/she must know the full story of costs.

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The preceding letter was referred to Drs. Green and Simmons who respond as follows:

Thank you for forwarding Dr. Merrill Anderson's letter concern-

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ing our article "Family Medicine Information System" in the September issue of *The Journal of Family Practice (Green LA,Simmons RL, Reed RM, et al: J Fam Pract 7:567, 1978)*. The following is our response.

We appreciate Dr. Anderson's comments and share his concern about costs.

The apparent discrepancies between costs mentioned in the text and costs listed in the table rises from the fact that the costs in the text refer to using FMIS in batch-mode, whereas costs in Table 1 refer to cost using FMIS on-line. Although this is stated in the text, it is certainly not emphasized and we are pleased to have the opportunity to clarify the apparent discrepancy.

Dr. Anderson's second question refers to the cost of programing this system and it implies that we have failed to account for the cost in describing the cost per encounter. The development of the FMIS was supported through sources contributed by the M/POP consortium and the federal government, both of which are nonprofit and noncommerical organizations. Unlike the commercial environment, the FMIS development costs are not amortized since they do not represent an investment to be returned to stockholders. Thus, these costs do not have to be recovered in the charges for system operation. This is the reason we believe operational and developmental costs should be kept entirely separate. We do not object to revealing developmental costs of the FMIS, but our intention in the article was to indicate what the users of FMIS are paying to operate the system in the fashion in which it is described.

As we indicated, a basic premise of FMIS is that it must be affordable to its users. We continue to be committed to the development of a system that will carry its own weight without continued subsidy from any source.

The other issue discussed in Dr. Anderson's letter relates the availability of the FMIS to other family practice programs. So far, FMIS has not been exported outside of Colorado, but we are not averse to sharing the system with other physicians or programs. We would be please to entertain requests or proposals.

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Clinical Teaching Conferences

To the Editor:

Dr. Massad's refinement of the clinical conference as a residency training tool is to be emulated *(Massad SC: The clinical conference in family practice training. J Fam Pract 7:589, 1978)*. Faced with increasing somnolence and apathy to the classical conference format, we, too, have moved to "problem solving" consultation rounds. We have amplified on this somewhat as follows.

A monthly publication containing summaries of the cases discussed is circulated by the resident in charge. Reprints of pertinent references are included as well as

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the monthly census of our Family Practice Inpatient Service.

As we are a university based "academic" program which employs the two-team paired internship concept, only half of the resident group is present for each conference. The publication of these conferences allows residents away, on vacation, or engaged in emergency care to profit from these discussions.

Sharing with the entire health care team is facilitated as residents gain a sense of problem illnesses in patients seen daily at the front desk, nursing station, or in the laboratory. They, and residents not in the hospital, become aware of which patients of the group required hospitalization and why.

Clinical faculty, who attend one half-day per week, three to six months per year, are given a sense of continuity with the model unit's patient population. Furthermore, the discussion summaries and reprints provide pertinent continuing medical education.

Finally, the universal paucity of qualified full-time faculty serendipitously has allowed UCLA to address the issues raised by Drs. Smith and Wilkins *(Smith CW, Wilkins EB: On training residents for careers in academic family medicine. J Fam Pract 7:605, 1978)*. Residents, in coordinating these conferences, gain practical teaching and writing experience, which it is hoped will lead to earlier recognition of those with the enthusiasm and dedication necessary for full-time teaching of family medicine.

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