Letters to the Editor

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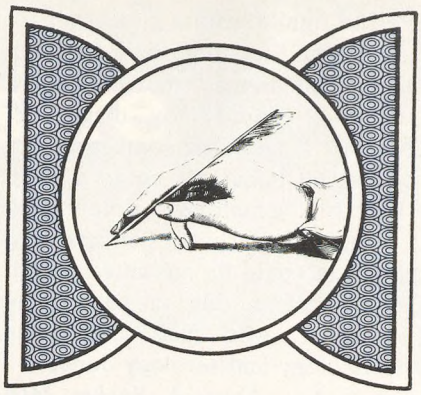

Diagnostic Tests for Infectious Mononucleosis

To the Editor:

The authors of "The Efficiency and Cost Effectiveness of Diagnostic Tests for Infectious Mononucleosis" *(English EC, Geyman JP. J Fam Pract 6:977, 1978)* are to be complimented on their effort to determine the best approach to a problem commonly encountered in family practice. However, their findings and conclusions bring up several questions.

The first of these concerns the comparability of the two practice populations studied. If the proportion of respiratory illness with adenopathy due to infectious mononucleosis was indeed higher in the student population as the article suggests, this could explain some or all of the increased "efficiency" and increased cost effectiveness ascribed by the authors to diagnostic Method II.

The second question concerns the method of calculation of the cost per positive test. The authors present no evidence that the WBC, differential, and smear done on the patients in Group I were intended to contribute in any way to the diagnosis of infectious mononucleosis. It would seem reasonable to consider the cost of the Monospot alone for patients in this group. If

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this is done, the cost per positive becomes $107 for diagnostic Method I.

In studies of the cost effectiveness of screening tests, it is customary to consider the costs of the negative screening tests as well as those of the positives.[1] To do this for diagnostic Group II one would have to calculate the cost of WBCs, differentials, and blood smears on patients who were suspected clinically of having infectious mononucleosis but did not meet the hematologic criteria. Since the authors do not state the total number of patients screened by diagnostic Method II to yield 1,969 patients who meet the hematologic criteria, it is impossible to do precise calculations. However, a rough estimate of the size of this group is possible if one assumes that the incidence of infectious mononucleosis in the two populations is similar. Of patients in the first group who had clinically suspected infectious mononucleosis, 5.6 percent had a positive serological test. If this percent is applied to the second patient population one would need 9,875 patients with clinically suspected infectious mononucleosis in order to get 553 positive serological tests. Since only 1,969 patients met the hematologic criteria, this means that approximately 7,906 patients had a WBC, differential, and smear but did not go on to have a "monoscreen." If the cost of these additional 7,906 tests are added to the calculations, the cost per positive serological test for diagnostic Method II becomes $202 compared with $107 per positive test if the Monospot were done by itself on the patients in Group I who were suspected of having mononucleosis on clinical grounds.

The final question concerns the possibility that some patients who have infectious mononucleosis might be missed by diagnostic Method II, ie, that some patients who have infectious mono do not meet the hematologic criteria set down in the article. Perhaps this question could be answered by an analysis of the data on patients in Group I who had both the hematology and serology done.

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Reference

1. Neuhauser D, Lewicki AM: What do we gain from the sixth stool guaiac? N Engl J Med 293:226, 1975

The preceding letter was referred to Drs. English and Geyman who respond as follows:

We appreciate the interest and questions raised by Drs. Becker and Treat.

We agree that there are differences in the two populations studied. A difference in prevalence of infectious mononucleosis as compared to diseases which resemble it would affect the efficiency of diagnosis. However, the most important factor, age, was similar in the two groups. Moreover, the diagnostic approaches discussed in the article are not affected by the prevalence of infectious mononucleosis.

With regard to the second question concerning the calculation of costs, the premise is that *all* patients presenting with acute

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pharyngitis and adenopathy require a WBC and differential as part of their data base. If this is so, the cost of negative screening becomes irrelevant.

Concerning the adequacy of the hematologic screen and possibility of missing patients with infectious mononucleosis using Method II, we have been impressed by the lack of documentation of the validity of hematologic criteria which are generally used. A prospective study is underway using Epstein-Barr serology as well as agglutination tests to evaluate these hematologic criteria.

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To the Editor:

I read with interest the article on serologic testing for infectious mononucleosis by Drs. English and Geyman in the May issue *(The efficiency and cost effectiveness of diagnostic tests for infectious mononucleosis. J Fam Pract 6:977, 1978).* While the article makes the important point that money is wasted in indiscriminate testing for mono, there are several problems with the paper.

The authors claim that hematologic preselection of patients for serologic study leads to a "fivefold increase in degree of accuracy of diagnosis." Unfortunately no data are presented to support this contention. To assess accuracy, one must rigidly define the disease being identified and determine with alternative measures the false positive, false negative, and reproducibility rates of the test under evaluation. What the authors have in fact reported is a fivefold increase in the *yield* of serologic studies by preselection, a much different matter.

A terminology problem exists in dealing with infectious mononucleosis: since the work of Henle and co-workers, it has been known that the Epstein-Barr virus is the usual pathogen; some authors (English and Geyman apparently among them) have begun using the term "infectious mononucleosis" as a synonym for "Primary E-B virus infection," while others continue to use it as the name of a clinical syndrome (thus the existence of the term "heterophile-negative infectious mononucleosis" for the same syndrome when caused by CMV, Toxoplasma or hepatitis viruses). A clear statement of definition should be made early in any discussion of this sort.

Table 2 purports to show "Efficiency of Diagnosis." As in accuracy, one cannot know efficiency unless one knows the number of diagnoses missed by the method under study; such data are not presented. Some insight into this problem could be had by retrospective review of leukocyte counts and lymphocyte morphology in the patients evaluated by Method I. If virtually all of the positive heterophile titers were found in patients who would also have been selected for study by Method II, some support for claims of enhanced efficiency could be had. As the study stands, for all the reader knows the increased yield has been purchased with a large number of missed positives.

The real message of this paper is simple and useful: The more it looks like classical infectious mononucleosis, the more likely a case is to have positive mono

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serology; indiscriminate serological testing probably wastes money. It is unfortunate that the authors did not report data to support the claim of increased diagnostic efficiency; that claim is probably true, and would add meaning to the enhanced cost effectiveness of Method II, if it were demonstrated.

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The preceding letter was referred to Drs. English and Geyman who respond as follows:

We thank Dr. Dale E. Hammerschmidt for his comments.

The point made that the evaluation reported is one of diagnostic yield rather than accuracy is well taken.

The definition of infectious mononucleosis seems quite clearly an Epstein-Barr infection as compared to the multiple etiologies of the clinical syndrome. The difficulty at the clinical level is the lack of availability and the cost of the more definitive viral tests. Because of this, we believe there is a need for further attention to optimizing and evaluating screening criteria, of which this study represents a first step.

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Orientation of Family Practice Residents

To the Editor:

I greatly appreciated reading Dr. John Dunn's article, "The First Month in Family Practice Residency Training," in the May 1978 issue of *The Journal of Family Practice (6:1105, 1978).* I believe that this should be required reading for all residency program directors and teachers in family medicine.

Dr. Dunn and the Air Force members at Scott Air Force Base have developed a program of training which should be widely copied. The idea of assigning to the resident during the first month of his residency families that he might follow through training is most commendable and gives a resident an opportunity to fulfill his/her role as "family doctor."

It has also been my experience in teaching at other locations that we often throw our resident into a very difficult service during his first month of residency, thereby causing feelings of inadequacy. He has not been properly introduced to the concepts of family practice or the model unit. Then, during his residency, the model unit becomes a foreign place to him and is a cause of discontent.

I do not feel that we should automatically expect a first year family practice resident to understand family practice and his role as family physician. As he struggles with the other subspecialists during his training program, he has a feeling of inadequacy and does not have the proper basis for refuting arguments toward subspecialization.

Again, I would recommend that family practice teachers communicate personally with Dr. Dunn concerning his approach as it is a most practical way of instructing family practice residents.

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