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SCREENING SIGMOIDOSCOPY AND COLORECTAL CANCER

To the Editor: The case–control study by Selby et al. (March 5 issue)* on the effect of screening sigmoidoscopy on mortality from rectosigmoid carcinoma was interesting to me because of my previous skepticism about the benefits of the procedure, given its costs and discomfort. The results are impressive, but I have one question: How does sigmoidoscopy exert this protective effect? Sigmoidoscopy is not a therapy for established cancer; therefore, for it to affect mortality from rectosigmoid carcinoma, it must lead to the excision of small malignant or premalignant lesions. However, the number of adenomatous polyps identified in the case subjects before the 10-year study period was similar to that in the matched control group, and the paper contains no data on the detection and removal of polyps during the 10 years before the diagnosis of cancer. How does the procedure reduce mortality, if not by the identification and removal of polyps? Without the validity afforded by finding a mechanism for the effect of the procedure, this leaves me with the worry that the authors' impressive results may have been due to a bias in selection, detection, or ascertainment.

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*Selby JV, Friedman GD, Quesenberry CP Jr, Weiss NS. A case–control study of screening sigmoidoscopy and mortality from colorectal cancer. N Engl J Med 1992;326:653-7.

The authors reply:

To the Editor: We sympathize with Dr. Tierney's desire to know how sigmoidoscopy may have prevented deaths from rectosigmoid cancer in the control subjects in our case–control study. The screening sigmoidoscopies performed in these subjects during the 10-year period did yield the expected number of adenomatous polyps, so it is possible that one or more lives were saved by sigmoidoscopy in this group. It is critical, however, to understand that the control subjects were selected not because their lives had been saved but rather so that they would be representative of the general population (matched with the case subjects according to age and sex). The purpose of including them in the study was to provide an estimate of the amount of sigmoidoscopic screening occurring in this population, so that we could determine whether there was a deficit of screening among the case subjects relative to that in the population from which they came. Since the cumulative lifetime incidence of colorectal cancer is only 5 percent,* most control subjects would never have colorectal cancer whether they were screened or not.

Contrary to Dr. Tierney's statement, a greater proportion of case subjects than of controls did have colorectal polyps detected and removed before the 10-year study period (3.8 percent vs. 1.4 percent) (Table 1 of our article) — not a surprising finding, given the known association of colorectal polyps with cancer.

We believe that the absence of a comparable deficit of screening among the case subjects who had fatal colon cancers above the reach of the rigid sigmoidoscope provides a strong argument that confounding on the basis of self-selection factors does not explain the association. Moreover, all known fatal cases in the entire population were ascertained. Therefore, biases of ascertainment, selection, or detection seem unlikely.

We wish to call attention to two errors in Table 3 of our paper. The numbers of case and control subjects (268 of each) in the analyses of cancers above the reach of the sigmoidoscope were inadvertently deleted from the table, and the numbers of these subjects who had at least one sigmoidoscopy were slightly in error. The corrected numbers should be 59 case subjects (22.0 percent) and 70 controls (26.1 percent). The odds ratio in the table was correct, and the conclusions of the paper are unaffected.

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PARTNER NOTIFICATION IN CASES OF HIV INFECTION

To the Editor: Landis et al. (Jan. 9 issue)[1] have provided additional support for the concept and practice of partner notification — specifically, the provider-referral approach used in cases of human immunodeficiency virus (HIV) infection. Our experiences[2] were quite similar to those of Landis et al., and involved a quite different and more heterogeneous population that has been described elsewhere.[3] The standardized approach combining referrals by patients and by providers that our staff routinely used with patients closely resembled the provider-referral approach described by Landis et al. We found that 53 percent of the index patients (283 of 531) provided reportable and traceable contacts (1.5 per index patient). When combined referral by both patients and providers was used, 88 percent (38 of 43) of the regular partners reported within the previous 12 months were located, and 29 percent (11 of 38) were found to be HIV-positive. Of all partners evaluated between 1986 and 1989 and determined to be HIV-positive, 83 percent were traced with the approach using referral by both patients and providers.

Increasingly, studies such as that by Landis et al. and our own experience document that the provider-referral approach to partner notification is effective and that it is necessary in order to ensure that persons at high risk for HIV infection will be reached, offered counseling and testing, and encouraged to reduce their risk-related behavior. Provider referral should be considered an integral part of comprehensive programs of prevention and control addressing HIV and the acquired immunodeficiency syndrome.[2]

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To the Editor: The study by Landis et al.,[1] though generally complete, failed to report on the types of counselors used in the study and how these counselors were trained.

It seems probable that the educational background and training of the counselor and the approach and design of the partner-notification study are critical in accounting for certain variables related to outcome and evaluation that are seen in the various published reports, including the current one. Such variables, for example, probably account for the wide variation in the number of partners named by each HIV-antibody–positive person interviewed. Although most researchers report that HIV-infected persons name between 1.0 and 2.1 partners each,[2-7] a few give considerably higher numbers,[1,8-10] including the 4.2 reported by Landis et al.[1] and the 5.3 that we reported.[10] These variables probably also account for the variability in the percentage of exposed partners who, once located, agree to be tested. This figure has varied from a low of 40 to 60 percent,[1,2,9] including 46 percent in the current study, to a high of 97 to 100 percent.[6,8,10]

Although it is certainly not the only important factor in optimizing the benefits of a partner-notification program, the appropriate selection and training of counselors appears critical for communities attempting to establish their own partner-notification programs.

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The opinions expressed in this letter are those of the authors and do not necessarily reflect the opinions of their employers.

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The authors reply:

To the Editor: As Daniell and Skelly indicate, the number of partners reported, the availability of information with which to locate them, and success in locating and counseling them vary according to the characteristics of the infected persons who are offered and who accept notification services, and therefore these measures vary according to the geographic area and the client population (e.g., persons attending a sexually transmitted disease clinic, members of the military, or HIV-counseling and -testing volunteers). To assess the extent to which the rate of notification achieved with provider referral represents an improvement over the rate obtained with patient referral, an internal, preferably randomized, comparison of the two methods is essential. Among the HIV-infected clients who agreed to participate in our trial, we were able to confirm notification by the infected index patient in the case of only 7 percent of partners in the patient-referral group. In populations with substantially higher notification rates after patient referral, the case for provider referral is not as strong.

Wykoff and Jones highlight the importance of the selection and training of counselors for partner-notification programs. Although we did not specifically study characteristics of counselors, we certainly agree. Our counselors were college graduates who had received the standard state training provided in North Carolina for HIV counselors plus on-site training in field location techniques from experienced sexually transmitted disease investigators and briefings on study procedures. The counselors were trained to assist clients to solve personal problems, cope with difficult life events, and decide on appropriate courses of action.[1] Bartlett has suggested that counselors often omit the diagnostic step and instead select an educational "treatment" based on a priori assumptions about client needs[2]; a more effective method of counseling includes a behavioral diagnosis, in which barriers to behavioral change are identified on the basis of the client's cultural and racial background.[3] HIV training protocols in many states lack discussions of the techniques counselors might use to elicit information about whether clients are engaging in risky behavior and of how to identify and address obstacles to behavioral change.[4] Thus, in programs of HIV counseling, testing, and partner notification, we should train counselors to do the following: uncover clients' negative attitudes toward behavioral change

and reduce social barriers to such change; teach them to adopt safer sexual and needle-using behavior; encourage them to attend programs of group or individual support that deal with barriers to, and provide suggestions for, the maintenance of such behavior; and seek medical care for those who are HIV-infected.[5-7]

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PNEUMOCOCCAL VACCINE FOR OLYMPIC ATHLETES AND VISITORS TO SPAIN

To the Editor: Barnett et al. (June 4 issue)[1] suggested that physicians consider the use of pneumococcal vaccine for persons planning to visit Spain this summer and fall, because of the high frequency of penicillin-resistant strains of *Streptococcus pneumoniae* found in one study[2] and the likelihood of crowded living conditions. We believe that their suggestion is based on several incorrect assumptions.

The finding that 28 percent of *S. pneumoniae* strains were resistant to penicillin during an 11-year period (44 percent during 1989) in Spain must be interpreted with great caution, since the results were based on isolates of *S. pneumoniae* sent voluntarily to a national reference laboratory.[2] There were large regional differences in the submission of isolates and in the rates of resistance, so that the results do not allow an estimation of the incidence of penicillin-resistant *S. pneumoniae* in Barcelona or its region, Catalonia. In a recent study conducted in the area of Barcelona, 10 percent of *S. pneumoniae* strains isolated from patients with pneumonia were resistant to penicillin.[3]

It is very unlikely that the living conditions of any visitors to Spain will resemble those in the Texas jail where the outbreak of invasive pneumococcal disease cited by Barnett et al. occurred.[4] The three newly built Olympic villages will host about 15,000 people; the rooms will face open spaces. All hotels in Catalonia undergo periodic inspection. Finally, few if any of the Olympic athletes and not many of the visitors are likely to have underlying conditions that increase the risk of pneumococcal disease in adults.

We do not believe there is any evidence that outbreaks of pneumococcal infection should be a matter of concern for Olympic athletes or visitors to Barcelona this summer, a time of year when the incidence of the disease is lowest.[5] Therefore, we see no reason to alter the current recommendations for pneumococcal vaccination that support its use only in certain well-defined groups.[6]

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FUNCTIONAL ASPLENIA AND VASCULITIS ASSOCIATED WITH ANTINEUTROPHIL CYTOPLASMIC ANTIBODIES

To the Editor: Patients with functional asplenia are prone to life-threatening infections with gram-positive bacteria.[1] The usual causes of functional asplenia are splenic irradiation,[1] sickle cell anemia,[2] and graft-versus-host disease after bone marrow transplantation.[3] We describe a patient with vasculitis and antineutrophil cytoplasmic antibodies in whom functional asplenia developed.

Vasculitis and segmental focal glomerulonephritis with crescents in more than 50 percent of glomeruli developed in a 30-year-old man in 1983. He had a remission during treatment with cyclophosphamide and prednisolone. Thereafter, he was asymptomatic, his serum creatinine concentration was 1.5 mg per deciliter (132 μmol per liter), and he had mild hematuria and proteinuria. In 1991 he had a relapse, with generalized arthralgia, myalgia, and necrotizing cutaneous leukocytoclastic vasculitis. He then presented with bleeding due to multiple ulcers throughout the gastrointestinal tract that required transfusions. Within two weeks severe thrombocytosis developed, with the platelet count increasing from normal up to 1100×10^9 per liter, accompanied by the appearance of Howell–Jolly bodies, indicating the presence of functional asplenia.[4] The patient's renal function did not change, and he had no nasopharyngeal or pulmonary disease. Mesenteric and renal arteriography showed no abnormalities. Tests for antineutrophil cytoplasmic antibodies with a cytoplasmic staining pattern and for rheumatoid factor were positive. The patient was given a diagnosis of vasculitis associated with antineutrophil cytoplasmic antibodies, was treated with cyclophosphamide and prednisolone, and had an excellent response.[5,6] After four days of treatment and six units of blood, his hematocrit stabilized, and the thrombocytosis and Howell–Jolly bodies subsequently disappeared. The antineutrophil cytoplasmic antibodies became undetectable, and the titer of rheumatoid factor decreased substantially.

Thrombocytosis, which has been assumed to be reactive, has been noted in one third of patients with Wegener's granulomatosis and microscopic polyarteritis.[5-7] We know of no other reports of splenic involvement in patients who have a