Re: Lymphoscintigraphy with Tc-99m Labeled Dextran

In their unrestrained enthusiasm over the potential use of Tc-99m dextran for lymphoscintigraphy (*1*), the authors have attributed to me statements that are incorrect and do not appear in the reference quoted (*2*) or in any of my other publications. As stated by the authors, studies with radiocolloid that depend upon particle size and functional status of the RE system and do not reflect lymphatic flow "may account for the reported finding that approximately 50% of normal parasternal lymph nodes failed to trap colloid activity and thus were not distinguishable from lymph nodes with metastases." This statement on page 923 is then repeated on pages 927-928. If this had been my experience with $^{99m}TcSb_2S_3$, on what basis would I have been so confident of the anatomic validity of the internal mammary lymphoscintigram? The observation that internal mammary lymphoscintigraphy data compare so well with results of cadaver dissections, which has been confirmed by others as well (*3,4*), is a clear indication that 100% of normal nodes, not 50%, can be and are visualized with subcostally injected $^{99m}TcSb_2S_3$. I would appreciate it if the authors would indicate from whence this statement attributed to me was derived.

The statement that radiocolloid clearance from the injection site varies from 1%-35% is taken out of context. These values were obtained following subcostal—not epigastric—injection in patients with breast carcinoma—not in the hind feet of healthy mongrels.

It has been shown by others (*5*) that transport of radiocolloid is not entirely macrophage dependent and that small particle colloids do enter the lymphatics at the interstitial injection site.

The lymphoscintigraphic images with Tc-99m dextran convincingly demonstrate lymphatic pathways and intense aggregates without any discrete, distinguishable components. The potential diagnostic value in oncology of information of this character awaits much further work. Limited numbers of patients have been studied, but we are not privy to how many or with what conditions.

Until Tc-99m dextran has been shown to be equal or superior to radiocolloids currently in use for the specific requirements fulfilled by lymphoscintigraphy, let us make a concerted effort to accept the facts as they presently stand.

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REFERENCES

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Reply

Our paper entitled "Lymphoscintigraphy with Tc-99m Dextran" (*1*) was published as a preliminary note and should be considered as such. It was not our intention to question the usefulness of Tc-99 antimony sulfide colloid for lymphoscintigraphy or the well-known work done by Dr. Ege as also stated in our introductory remarks (*1*). We thank Dr. Ege for her interest in our work, in particular for her comments. In our paper (*1*), there is an unfortunate error regarding Refs. 2 and 3. In the last line on p. 923 as well as on the third line on p. 928, the reference cited should be 3 instead of 2, where erroneously we attributed to Dr. Ege the work of Aspergen et al (*2*). We apologize for this mistake, which, however, does not change the validity of our statements in the introduction or in discussion of the paper (*1*). The work by Aspergen et al (*2*) indicates a serious limitation exists with the use of Tc-99m sulfide colloid for lymphoscintigraphy in patients with breast cancer. These authors (*2*) reported a failure of Tc-99m sulfide colloid to accumulate in normal mammary lymph nodes in seven of 16 patients and concluded that "Since absence of incorporation indicates inflammation or malignant invasion, the use of the present Tc-99m sulfur colloid was a high risk of over diagnosis". Accordingly, these authors considered unethical the future evaluation of patients with breast cancer using Tc-99m sulfide colloid. This high rate of almost 50% false positives is also quoted in a recent paper in this *Journal* by Strand et al. (*3*).

The rate of false-positive tests may be substantially lower with Tc-99m antimony sulfide colloid as suggested by Dr. Ege and others (*3*). We never questioned this finding in our article, however, antimony sulfide colloid is still considered an investigational drug in the U.S. and is thus not available for routine use. In addition, an intensive computer search of the literature did not reveal any study, not even in Dr. Ege's work, documenting that 100% of normal mammary lymph nodes are visualized with Tc-99m antimony sulfide colloid. Neither study cited in Dr. Ege's letter (*4,5*) was designed to examine the efficacy of normal lymph node visualization nor did these studies document a 100% visualization of normal lymph nodes with Tc-99m antimony sulfide colloid.

We do not agree with Dr. Ege's assertion in the second paragraph. The statement in our manuscript was not out of context. In her own paper (*6*), Dr. Ege states on p. 102 that "Considerable individual variability in anatomic, physiologic and pathologic factors contributes to the rate of removal and dispersion of a radiocolloid. Estimates of the quantity of radiocolloid transported from the *interstitial* injection site over the first 24 hr have varied from 1% to 35%". Also, Dr. Ege gives no indication as to how these values were derived. No information is given with respect to whether they were derived after subcostal or epigastric injection or whether they were obtained from animal or human studies. This particular statement was referenced as a personal communication from M. J. Bronskill.

We cannot comment on Dr. Ege's remarks made in her third paragraph since the reference quoted (*7*) will be published in 1983 and was thus not available to us. Yet, we never questioned the possibility of very small particles entering the lymphatics at the interstitial injection site without being transported by macrophages.

In the last paragraph, Dr. Ege almost literally quotes our conclusion (*1*). We emphasized that "the potential value of this new radiopharmaceutical [Tc-99m dextran] for diagnosis and follow-up of patients with cancer, lymphoma and primary lymphatic disease awaits clinical trials" (*1*). We do not believe that such a statement can be interpreted as "unrestrained enthusiasm" or as intended "to distort the facts as they presently stand", an opinion that was shared by the reviewers as well as the author of the teaching editorial (*8*).

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