

LETTERS TO THE EDITOR

Thyroid Remnant Ablation: Questionable Pursuit of an Ill-Defined Goal

There is no doubt that the routine application of 30 mCi doses for the ablation of thyroid remnants after surgery for thyroid carcinoma should be abandoned (1). We know, however, that we can really eliminate thyroid tissue with approximately 50 Gy, and therefore an adequate dosimetry for the ablation of thyroid remnants should be done (2); sometimes you will end with as little as 20 mCi, sometimes you need 100 mCi. On the other hand, the real ablation of thyroid remnants has been so valuable in our series of more than 1000 patients (Fig. 1) that we and others (3) are convinced that this therapeutic approach should stay. Today thyroid cancer has become one of a few malignancies that can really be cured by a combination of surgery, radionuclide therapy, hormone therapy, etc., in the majority of afflicted patients. If now surgeons, nuclear physicians, and oncologists start again to discuss the advantages and possible disadvantages of surgical and nuclear medicine approaches to thyroid carcinoma, I am sure that many advantages gained over the past decades will be lost (4). As soon as TSH-receptor assays (5) are as routine as estrogen-receptor studies in breast cancer, one might start to discuss the subject again, but until then let us just stay with the concept that thyroid remnants after surgery for thyroid carcinoma should be really eliminated with I-131 under the conditions mentioned above.

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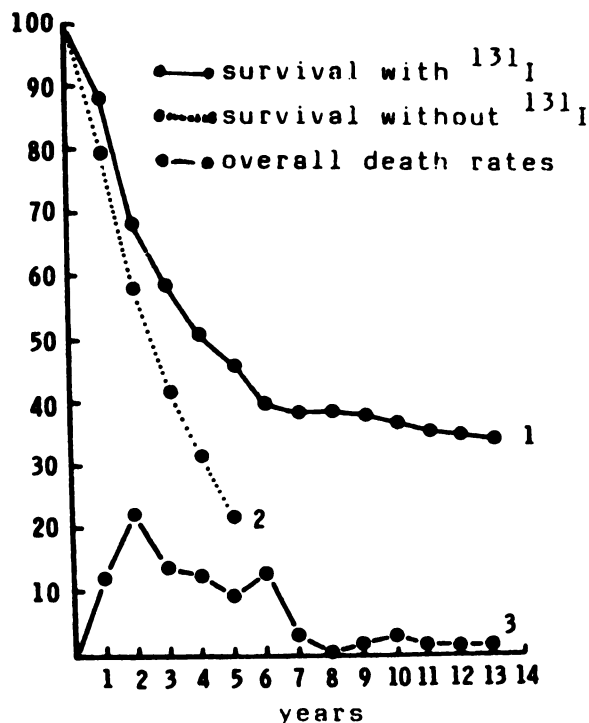


FIG. 1. Thyroid cancer under I-131 therapy and without (n = 752)

REFERENCES

1. STANBURY JB, HETZEL B: *Endemic goiter and endemic cretinism*. New York, John Wiley & Sons, 1980, pp 333-350
2. RICCABONA G: Therapie der Struma maligna, *Nucl Med Suppl.* 18, Schattauer, Stuttgart-New York, 1981, pp 932-940
3. BEIERWALTES WH: Therapy and follow-up of thyroid cancer. *Acta Endocrinol Suppl.* 252:71, 1983
4. SNYDER J, GORMAN C, SCANLON P: Thyroid remnant ablation: Questionable pursuit of an ill-defined goal. *J Nucl Med* 24:659-665, 1983
5. ANDREOLI M, MONACO F, ROBBINS J: *Advances in Thyroid Neoplasia 1981*. Field Educational Italia, Roma, 1981, pp 23-37

Reply

Unfortunately, the above letter is yet another testimonial to what has plagued the evaluation of different approaches to the treatment of thyroid-cancer patients. Were Dr. Riccabona's patients randomized as to treatment? Were variables such as age, sex, histological type, histological grade, extent of disease, extent of surgery, and use of thyroid suppression therapy taken into account? What method was used in the detection of postoperative functioning tissue? Was ablation aimed at presumably normal thyroid tissue, or known residual thyroid cancer in the thyroid bed, or extrathyroidal functioning metastases? Were these variables considered in the survival curve presented?

Reports continue to be published supporting a less aggressive approach to thyroid cancer, implying that less-than-total thyroidectomy followed by thyroid suppression therapy may be adequate (1,2).

Reports also continue to appear raising questions as to the appropriate place for radioiodine in the therapeutic approach to these patients. Investigators at the University of Texas M.D. Anderson Hospital and Tumor Institute recently reported (3) that ablative iodine reduced the frequency of recurrence but that survival was not altered. When various variables were analyzed, it was noted that recurrence rates were not different in patients with pure papillary carcinoma or in those patients who had less-than-total thyroidectomy. As discussed in our paper (4), numerous studies support the use of radioiodine in the treatment of metastatic disease, but there is simply no solid evidence that supports the routine use of radioiodine for remnant ablation. As pointed out by Dr. Sisson (5), until a national cooperative study carefully evaluates ablation as a step in the therapy of well-differentiated thyroid cancer, physicians must form conclusions as to whether to apply it in any one patient, from a knowledge base that is incomplete.

Work with the thyrotropin receptor on neoplastic tissue is an active area of investigation (6), but its clinical relevance has yet to be demonstrated. It may prove to be another variable worth considering in our therapeutic approach to these patients.

REFERENCES

1. CHRISTENSEN SB, LJUNGBERG O, TIBBLIN S: Surgical treatment of thyroid carcinoma in a defined population: 1960 to 1977. Evaluation of the results after a conservative surgical approach. *Am J Surg* 146:349-354, 1983
2. BARTOLO DC, KAY PH, TALBOT CH: Treatment of thyroid carcinoma in 107 cases. *Ann R Coll Surg Engl* 65:301-303, 1983
3. SAMAAN NA, MAHESWARI YK, NADER S, et al: Impact of therapy for differentiated carcinoma of the thyroid: An analysis of 706 cases. *J Clin Endo Metab* 56:1131-1138, 1983
4. SNYDER J, GORMAN C, SCANLON P: Thyroid remnant ablation: questionable pursuit of an ill-defined goal. *J Nucl Med* 24:659-665, 1983
5. SISSON JC: Applying the radioactive eraser I-131 to ablate normal thyroid tissue in patients from whom thyroid cancer has been resected. *J Nucl Med* 24:743-744, 1983
6. CLARK OH, GEREND PL, GORETZKI P, et al: Characterization of the thyrotropin receptor-adenylate cyclase system in neoplastic human thyroid tissue. *J Clin Endo Metab* 57:140-147, 1983

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Influence of Scan and Pathologic Criteria on the Specificity of Cholescintigraphy: Concise Communication

Drs. Freitas et al. are to be commended for their insightful study on the influence of image and pathologic criteria on the specificity of cholescintigraphy (1). Although briefly mentioned by the authors, I feel that another variable should be emphasized as having an equally significant impact on such specificity: the prevalence of chronic cholecystitis, however defined, in the population under study. Since, as stated in this and other articles, the large majority of false-positive cholescintigrams are associated with chronic cholecystitis, it follows that the specificity, false-positive rate, and predictive value of a positive test will be closely tied to the prevalence of chronic cholecystitis in the population under study (2,3). One could easily expect that this prevalence would be quite different in a population of patients referred from an emergency room as opposed to patients referred from a surgical clinic. Likewise the prevalence of chronic cholecystitis would be markedly different if a significant portion of the population under study consists of young traumatized males (Harborview Hospital, Seattle) as compared with middle-aged female native Americans (Alaska Native Medical Center Hospital, Anchorage).

This point was made recently in an editorial by Warren C. Phillips et al. in the *American Journal of Roentgenology* using one of our articles on the evaluation of acute right upper quadrant pain (2,4). This article reported accuracy, sensitivity, specificity, predictive values of a positive and of a negative test, and false-negative and false-positive rates for cholescintigraphy and sonography. The author of the editorial asked how many of these figures would be useful to a community hospital radiologist. The answer is "None!" because the prevalence of disease differs between the community hospital and our referred hospital.

As with most tests, the value of cholescintigraphy depends strongly on the prevalence of the disease under study (acute cholecystitis) in the population under study. But it depends equally on the prevalence of an accompanying morbid disease (chronic cholecystitis) that causes false-positive results (5). This latter

prevalence can vary considerably between populations, independently of how it is defined.

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REFERENCES

1. FREITAS JE, COLEMAN RE, NAGLE CE, et al: Influence of scan and pathologic criteria on the specificity of cholescintigraphy: Concise communication, *J Nucl Med* 24:876-879, 1983
2. SHUMAN WP, MACK LA, RUDD TG, et al: Evaluation of acute right upper quadrant pain: sonography and ^{99m}Tc -PIPIDA cholescintigraphy. *Am J Roentgenol* 139:61-64, 1982
3. MAURO MA, MCCARTNEY WH, MELMED JR: Hepatobiliary scanning with ^{99m}Tc -PIPIDA in acute cholecystitis. *Radiology* 142:193-197, 1982
4. PHILLIPS WC, SCOTT JA, BLASCZYNSKI G: Statistics for diagnostic procedures. 1. How sensitive is "sensitivity"; how specific is "specificity"? *Am J Roentgenol* 140:1265-1270, 1983
5. RANSOHOFF DF, FEINSTEIN AR: Problems of spectrum and bias in evaluating the efficacy of diagnostic tests. *N Engl J Med* 299:926-930, 1978

Reply

I thank Dr. Shuman for his interest in our article. As outlined in our introduction, this paper dealt primarily with only one of four factors that may have a profound influence on cholescintigraphic results. I agree with Dr. Shuman's desire for emphasis of the prevalence of chronic cholecystitis and its impact upon the specificity of cholescintigraphy. This is why the prevalence of chronic cholecystitis in the group reported in this prospective study is emphasized. In the clinical setting of acute right upper quadrant or epigastric pain, the prevalence of chronic cholecystitis varies from 20-33% in the literature (1-6). These prevalence rates were found in both community and university centers. In our prospective study, the gallbladder of 91.4% of our patients with chronic cholecystitis and with symptomatic right quadrant or epigastric pain were visualized by 4 hr.

Previously we had looked at our patients with asymptomatic chronic cholecystitis and found that 90% visualized their gallbladder by 1 hr, and 95% by 4 hr, whether in the asymptomatic or symptomatic state.

Using the cholescintigraphic specificity for chronic cholecystitis of 90%, and holding the prevalence of acute cholecystitis constant at 30%, the overall specificity of cholescintigraphy in a population changes little as the prevalence of chronic cholecystitis is increased from 10 to 70% (Fig. 1). Note also what happens to the specificity of real-time ultrasound as the prevalence of chronic cholecystitis increased. Obviously, with increasing prevalence of chronic cholecystitis in a population, the specificity of real-time ultrasound declines markedly, whereas the effect upon cholescintigraphy is minimal.

Regarding the Editorial by W. C. Phillips et al., it is stated that the positive and negative predictive values will vary depending upon the prevalence of disease in that particular community or university center. Dr. Shuman omitted Phillips's next sentence, "However, if the disease prevalence were known, the predictive values could be determined." Thus, as in our article, the predictive values give meaningful information if the prevalences of the disease (both acute and chronic) are specified. Qualitative measurements of a test's performance must be viewed within the context of the study's