bolism. Second, as in the evaluation of any diagnostic procedure, independent verification of the final disease state is essential. Dr. Wilson and his colleagues imply that their 22 patients had a subjective diagnosis of pulmonary embolism. Thus, I would conclude that while their supposition may be direct, their data do not yet support their conclusions.

BARBARA J. McNEIL
Brigham and Women's Hospital
Boston, Massachusetts

Re: Correlation of Contrast Angiography and Histological Pattern with Gallium Uptake in Primary Liver-Cell Carcinoma: Noncorrelation with Alpha-Feto Protein

In their article, Waxman et al. comment that "the hepatitis A antigen also did not correlate with the findings of the gallium scan" (*1*). As there is no known association between hepatitis A (as opposed to hepatitis B) and hepatocellular carcinoma, the relevance of this observation escapes me. Did this represent a repeated typographical error or a misunderstanding?

THOMAS G. COONEY
University of Oregon Health Sciences Center
Veterans Administration Medical Center
Portland, Oregon

REFERENCE

1. WAXMAN AD, RICHMOND R, JUTTNER H, et al: Correlation of contrast angiography and histological pattern with gallium uptake in primary liver-cell carcinoma: Noncorrelation with alpha-feto protein. *J Nucl Med* 21:324-327, 1980

Reply

In response to Dr. Cooney's letter, the statement should read, "the hepatitis B antigen showed no correlation with the findings of the gallium scan." The hepatitis B surface antigen has been shown to have a high association with hepatocellular carcinoma, especially in nonalcoholic cirrhosis (*1,2*). The hepatitis B surface antigen has also been called the hepatitis associated antigen or HAA. The abbreviation was initially used and subsequently changed in the final draft of the paper and incorrectly referred to as hepatitis antigen A, instead of hepatitis associated antigen for hepatitis B surface antigen. We are indebted to Dr. Cooney for his observation. We too know of no known association of hepatitis A and hepatocellular carcinoma.

ALAN D. WAXMAN
Cedars-Sinai Medical Center
Los Angeles, California

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2. VOGEL CL, ANTHONY PP, MODY N, et al: Hepatitis-associated antigen in Ugandan patients with hepatocellular carcinoma. *Lancet* 2: 621-624, 1970

Re: A Radiometric Microbiologic Assay for the Biologically Active Forms of Niacin

Concerning the paper by Judith A. Kertcher et al: "A Radiometric Microbiologic Assay for the Biologically Active Forms of Niacin" published in this *Journal* (*1*), we have been seriously misquoted. They state: "Another microbiologic assay using the protozoan *Tetrahymena pyriformis* is said to be more sensitive and more specific. This organism, however, is slow and difficult to grow and was shown to respond to several naturally occurring, biologically inactive derivatives of niacin." Their reference for such statements was to our paper "Nicotinic acid assay in blood and urine" (*2*).

The point of our paper was that the niacin requirement of *T. pyriformis* closely—hence usefully—parallels that of man and higher animals (our paper, p. 575), and that niacin derivatives having *no animal activity* were *inactive* (our italics) for *T. pyriformis*, in contrast to their incorrect allusion (*vide supra*) to our work (*2*). Indeed, we stated that "Trigonelline (the betaine of nicotinic acid), which has *no animal activity, proved inert* for *T. pyriformis*" (our italics), and "*T. pyriformis* does not respond to nicotinuric acid, whereas *Lactobacillus plantarum* does, another point of specificity favoring *T. pyriformis*." We never stated, as these authors wrote, that *T. pyriformis* responded to "biologically inactive" niacin derivatives. We did note, as they confirm, that *L. plantarum* does respond to animal-inactive niacin derivatives, e.g., trigonelline and nicotinuric acid (an excretory product), thus making *L. plantarum* less specific than *T. pyriformis* for detecting and estimating biologically active niacin.

In contrast to what the authors state in their paper (*1*), *T. pyriformis* is easy to grow. The maintenance medium consists of glucose and proteose-peptone, and gives as heavy a growth in 3 days as does the basal medium (*2*). Also the *T. pyriformis* method does not require elaborate, costly arrays of equipment and supplies—as for example, a radioactivity counter, radiolabeled metabolites, etc.; one needs only flasks, tubes, pipettes, and a simple photometer. For over 20 years we have routinely used the *T. pyriformis* assay for niacin, and we process over 100 blood or tissue samples with ease in 2 hr, with a turnover time of 3 days. We think their present method (*1*) cannot render as good performance.

We reiterate that *T. pyriformis* mirrors the response of only biologically active niacin; *L. plantarum* does not. The authors should be aware of this point; their quotations badly misguide the reader. Indeed, the protozoan response to vitamins that are biologically active only for man makes them suitable and specific microanimals, parallel to the higher animals, for analyzing vitamins in biologic fluids and tissues. Their clinical correlations make them far superior to radionuclide methods (*3,4*), as for example in the comparison with radioisotope assays for vitamin B_{12} (*4*), a point they left unmentioned before decrying the use of protozoan assays (*1*).

It has long been recognized that assays for B vitamins, mainly based on protozoan assays, including that of *Tetrahymena* for niacin, yield results of especially close clinical validity, as recognized by our invitation to describe assays for vitamins in this (*3*) and the previous edition.

HERMAN BAKER
OSCAR FRANK
New Jersey Medical School
East Orange, New Jersey
S. H. HUTNER
Haskins Laboratories of Pace University
New York, New York

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1. KERTCHER JA, GUILARTE TR, CHEN MF, et al: A radiometric microbiologic assay for the biologically active forms of niacin. *J Nucl Med* 20:419-423, 1979
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