blood glucose concentrations should not be assessed by fructosamine measurements in diabetic patients with marked albuminuria (11). Furthermore, we cannot understand why Johnson and Baker think that we would report two sets of fructosamine concentration, when those corrected for albumin would be both more correct and sufficient.

The previous publications and the Letter of the New Zealand group have clearly shown that different fructosamine values can be obtained by using different calibrants in their assay. Adoption of secondary standards of any type does not alleviate the basic problem of calibration (12), because the fructosamine concentrations of these standards must be assigned by prior analyses performed with deoxymor- pholinofructose as primary standard. Modification of the reaction conditions of a widely used commercial reagent kit, based on the original method of Johnson et al. (5), can significantly affect the results (13); namely, higher fructosamine values resulted from use of a lower temperature, longer or shorter incubation times, and a longer wavelength, whereas a smaller sample volume and a shorter wavelength gave lower results. Thus, the fructosamine assay may be inexpensive, practicable, and easy to automate, but it is not robust.

In addition to the considerable analytical difficulties, none of which appear to be easy to resolve, there are also several problems in the clinical interpretation of results.

We have shown (1) that, for fructosa-
mine, the ratio of intra- to interindivi-
dual variation is low, demonstrating that population-based reference values are of limited usefulness and may be misleading. Also, fructosamine concentra-
tions in diabetics overlap consider-
ably with those found in the healthy, rendering the test unsuitable for diag-
nosis or for use in screening programs. Furthermore, fructosamine concentra-
tions are dependent on the rate of protein turnover and are therefore in-
fluenced by thymometabolic status (14). It may be that fructosamine depends on superoxide dismutase status (15). In addition, fructosamine concentrations increase with age (16), so that stratified reference values are required.

The major published rationale for the clinical use of fructosamine deter-
minations is that fructosamine and hemoglobin A1c concentrations are signifi-
cantly correlated. The truly pertinent question to pose, therefore, is whether fructosamine assays should be adopted either in addition to or perhaps, in the current economic climate, as a replace-
ment for hemoglobin A1c assays. We believe, as do Sundkvist et al. (17), that the analytical and interpretative diffi-
culties support a negative decision. One possible use for the available as-
says, however, is in the small labo-
tory that lacks the facilities or re-
sources to perform acceptable assays of hemoglobin A1c but wishes to provide some monitoring of diabetic patients.

Zweig and Robertshaw (18) have stated that, as diagnostic tests proliferate, public demand and private enterprise plunge them into common use, and the need for careful trials to evaluate their worth becomes pressing; critical evalu-
ations occasion savings by forestalling inappropriate use of less-effective tests, while improving clinical care by en-
couraging use of the most efficacious.

We do not believe that current fruc-
tosamine assays pass the required crit-
ical evaluation.

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Reappraisal of Carnitine Concentrations in Blood

To the Editor:

Katrib et al. (1) recently reported concentrations of free and acylated carni-

tine in whole blood, plasma, poly-

morphonuclear leukocytes (PMN), mononuclear cells (MNC), and erythro-

cytes. We isolated PMN from eight healthy adults according to the method of Metcoff (2) and determined concentra-
tions of free carnitine (FC), short-

chain acylcarnitine (SCC), and long-

chain acylcarnitine (LCC) by applying the method of Rössel et al. (3), after disruption by thawing and freezing two times, followed by sonication. We also measured concentrations of the carnitine fractions in whole blood and in plasma, erythrocytes, and leuko-
cytes (PMN and MNC) in four healthy adults, following the same isolation procedures as applied by Katrib et al. (1), but using the above-described method of disruption. Our results are shown in the following tabulation.

Obviously, we observed considerably lower concentrations of total carnitine (TC) and its subfractions in leukocytes.
than those reported by Katrib et al. (1). In contrast with their results, erythrocytes reveal measurable concentrations in our studies, in good agreement with previous communications (4, 5).

The calculated contribution of carnitine from trapped leukocytes in erythocyte specimens corresponds to only inconsequential amounts (2% to 3%) of the total carnitine in such samples. Thus, contamination with leukocytes conceivably does not lead to erroneously high values for carnitine in erythrocyte specimens.

A possible explanation of the disputably high carnitine values claimed by Katrib et al. (1) might be their use of Tris buffer instead of 4-(2-hydroxyethyl)-1-piperazineethanesulfonic acid (HEPES) buffer. Because Tris buffer may act as acetyl acceptor for carnitine acetyltransferase (6), presumably it could interfere with their assay.

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Routine Use of β2-Microglobulin Assays

To the Editor:

Wu et al. (1) indicate the importance of routinely estimating β2-microglobulin (β2-M), and they developed and characterized a simple radial immunodiffusion assay. We agree with their point of view and have emphasized that β2-M assays may be useful in evaluating the prognosis of development of AIDS in individuals who are HIV (HTLV-III/LAV) antibody positive (2, 3). Wu et al. have made the point that the frequency of ordering β2-M is low. The question to be raised, therefore, is why this is so, despite its documented uses (4–6). Typically, the response we hear is that β2-M is not specific and that it may possibly be partially denatured in acid urine—correct, but in our opinion not a real reason for the limited use of β2-M assay. We say this because the specificity issue (e.g., the nature of the disease) is usually resolved by the time one would consider requesting a β2-M assay. It must be remembered that the true value of measuring β2-M is to assist in prognosis and followup.

Other reasons for not offering β2-M assays might include unavailability of functional reagents, their instability, or their high cost. Wu et al. noted that RIA reagents are commercially available. They did not indicate that enzyme immunoassay reagents with longer shelf-life are also available. As with many other analytes, the cost is high if only a single sample is assayed; however, β2-M and other laboratory tests are rarely run as a single test when batching is possible. The current reagent cost per patient’s sample in our laboratory is $6.78 (based on 42 patients’ samples run per kit). This cost is justified in view of the benefit the patient receives through initiation of earlier therapy and in cost savings realized from fewer hospital days (7). In our opinion the major reason for the minimal use of β2-M is lack of information. We believe that use of β2-M assay will be more common as information is extended further through the medical community. We hope this Letter, and the articles by Wu et al. and others, will encourage further evaluations of β2-M and result in increased use of this valuable assay.

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Salivary Melatonin Assay in Laboratory Medicine—Longitudinal Profiles of Secretion in Healthy Men

To the Editor:

The functional significance of the principal pineal hormone, melatonin, has yet to be established (1). Current thinking proposes a human neuroendocrine role, perhaps in some way similar to that documented in animals (2, 3), but no convincing endocrinology has been ascribed to date (1, 4). Currently