usual lipid metabolism in the liver and (or) small bowel secondary to the severity of the viral infection and may explain why some patients are affected more than others.

Reference

E. Eugene Baillie
C. William Orr
Anderson Memorial Hospital
Anderson, SC 29621

Phadebas Amylase Test Kits

To the Editor:
The observation by Hafkenscheid (1) that Phadebas Amylase Test kits with bovine serum albumin (BSA) gave lower results than those without BSA is, as he infers, evidently caused by differences between the standard curves supplied by the manufacturer.

We found rather better agreement for plasma assays between the old- and new-formulation tablets (2). However, we routinely check the calibration curve for each lot of tablets against the stated values of Phadebas Reference Serum and Phadebas ACR (high amylase) serum. If there is a small discrepancy between our calibration line thus derived and that supplied by Pharmacia, we use our own line when it has been checked and we are satisfied that it is correct for our conditions. We have previously discussed this point (3).

In our comparison of the old and new tablets (2), we made a slight alteration to the calibration line for the old tablets, but found it to be unnecessary for those with BSA. This had the effect of slightly lowering the results with the old tablets. It would be interesting to know whether Hafkenscheid has any assay results for the Phadebas control sera by the two methods.

In our opinion, it would be wrong to introduce a new reference range for the tablets with BSA on the basis of the evidence now available. The discrepancy could just as easily have resulted from a faulty calibration line for the old batch of tablets.

References

C. H. Foot
K. Wiener
North Manchester Gen'l. Hosp.
Crumpsall, Manchester, M8 6RB,
U.K.

Dr. Hafkenscheid responds:

To the Editor:
Wiener and Foot observed better agreement between the amylase activity determined with the old method (tablets without BSA) and the new method (tablets with BSA), although the results of the two studies are not impressive (1, 2) (y = 0.97x + 10.91 and y = 0.93x - 14.4, respectively; where y represents results with new tablets and x with old tablets).

I did not check the calibration curves supplied by the manufacturer as did Wiener and Foot. The main point is that, once corrected for the calibration curves, exactly the same results can be obtained with the old and new method.

When control sera were used I obtained the same results. The enzymic activity in the absence of BSA is higher than in the presence of it, but the absorbances at 620 nm are the same, so there is no difference between results for normal human sera and control sera.

I agree (2) with Wiener and Foot that it will not be necessary to introduce a new normal range for amylase activity determined with the new method.

References

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Correlation Coefficients as a Verification of Linearity

To the Editor:
We noted the recent Letter by Gindler [Clin. Chem. 25, 337 (1979)] with interest. We agree that the assumption of linear relationship in regressing data must be verified. However, a simple and straightforward method is offered by calculating the correlation coefficient. This is conveniently included in most inexpensive programmable calculators to which the author refers, and correlation coefficients are routinely cited in published papers.

If the absolute value of the correlation coefficient deviates significantly from 1.00, a more thorough exploration of the causes is indicated. It may well be that the curve fitting proposed by Gindler is appropriate in some instances, but this, too, must be tested by calculating the corresponding correlation coefficient. Thus, while we fully agree that an automatic application of linear regression is wrong, we also suggest that if the relationship is not linear, more sophisticated investigation is needed than the automatic assumption that there is yet another relationship.

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Dr. Gindler responds:

To the Editor:
The suggestion of Eppstein and Levy with regard to the simplicity and straightforwardness of the correlation coefficient is, unintentionally to be sure, deceptive, because the shortcomings of that function were pointed out in reference (1) of my Letter (2). It has long been known that the scatter of data produced by chance alone will cause correlation coefficient (r) values that are considerably less than 1.00, even when the equation y = mx (y = dependent variable, x = independent variable, m = constant) holds and there are several values of x and y, such acceptable values having been tabulated (3).

On the other hand, the equation and technique which I suggested allows for such scatter due to chance and not only indicates whether or not the system is linear but also gives an inkling of what the actual relationship may be. This situation is unlike that of the correlation coefficient technique suggested by Eppstein and Levy, where a value of r that is less than 1.00, whereas unanswered the important question of whether it is due to chance or true nonlinearity, even where considerable scatter of data may exist. My associates and I have been gathering data, from the literature as well as from our own studies, which indicate that the suggested equation (2) empirically fits many practical systems, and this work has been submitted for presentation at the 31st national meeting of the AACC later this year. This verifies my statement that this equation describes at least part of an existing curvilinear relationship. At no