obtained from a breath analyzer calibrated at 2100:1. Also assume the subject's actual blood/breath ratio is 1000. This means the actual BAC would be about 0.05%. Using the formula defined earlier, \( E = [(O - A)/A] \times 100 = 100\% \), if the subject's ratio were 100, then the BAC would be about 0.005%, so \( E = +2000\% \). Although the magnitude of VBAC/BrAC necessarily changes from 0 to about 2000 during absorption (4), a smaller range will result under field conditions, perhaps 800–2000.

I used results from laboratory studies to assess arterio-venous differences because few data are available from field studies, although the data of Emerson et al. (10) reveal blood/breath ratios from about 1450 to 3800, indicating that at least some subjects had significant arterio-venous differences. It is true that arterio-venous differences are likely to be smaller under field conditions than under laboratory conditions, but there is little evidence to support the conclusion that these differences are negligible. Nevertheless, Dr. Jones reaches just that conclusion without citing evidence and without explanation. At this time, results from laboratory studies are about the only means of estimating these differences. Assuming arterio-venous differences under field conditions are half what they are under laboratory conditions, the results of Martin et al. (7) indicate errors of +115%, +95%, +30%, and +15% for four subjects during absorption. The time course of arterio-venous differences would be more correctly described as increasing with each drink and then declining, rather than "... gradually diminishing as the drinking continues ...". Dr. Jones also fails to mention that the reason Enticknap and Wright (11) found no arterio-venous differences was because their study was purposely designed to eliminate them (p 162, p 165, left column).

If uncertainty in the 2100:1 ratio is accounted for and reported, as required by traditional analytical methodology, then there is no effective difference between using direct or converted BrAC results. However, from a legal standpoint, use of direct BrAC for per se statutes seems to constitute an irrebuttable presumption by the legislature that all subjects tested have a ratio of precisely 2100:1 (12). Use of converted BrAC results at least leaves the value of the conversion factor more open to debate in a court of law. To satisfy constitutionality requirements, a rational connection must exist between BrAC and BAC (13). Use of 2100:1 for this connection for all subjects is rational only if the associated uncertainty is accounted for. However, at some level of uncertainty, the connection fails, and above this level, breath-test results can only be used for qualitative purposes.

Dr. Jones claims the value of the conversion factor used has no effect on the accident rate or on deterrence of drinking and driving. But the issue is, if 2300:1 is used, statistical projections from postabsorptive results of Dubowski (5) indicate at least 53% of arrested drivers have their actual BAC overestimated by breath testing. If 2100:1 is used, at least 23% are overestimated. For 2000:1 it is 12%, and for 1800:1 it is 2.5%. It is up to legislators to decide how many legally innocent people are to be prosecuted in the interest of the public good, i.e., in the interest of accident reduction and deterrence. It is the responsibility of scientists to provide legislatures with information based on proper use of scientific methods, so they can make rational, informed decisions. Although questions regarding absorption times and arterio-venous differences are very important, sufficient work has not been done to provide rigorous answers.

2 Whether this level of uncertainty is ±5%, ±15%, ±30%, or more, is an issue best addressed by scientists, even though there are bound to be disagreements over an appropriate value; e.g., analytical chemists would probably select a lower value than would clinical chemists.

Corrections to a Report

To the Editor:

In a recent article (1), I used the data of Jones (2), among others, to estimate the fraction of subjects having their actual blood alcohol concentration (BAC) overestimated during the absorption of alcohol, and to estimate the time required for absorption to be complete. Dr. Jones recently informed me that even though the caption on Table 2 reads, "... at different sampling times after drinking ...", the time was actually measured from the start of drinking. This necessitates a 20-min correction in some of the results reported in my article. On p 753, right column, the mean, ±SD, CV, and range should be 2025, ±147, 7.2%, and 990–2450 for data collected and pooled 10, 40, 70, and 100 min after the end of drinking. The comparisons made for the fraction of subjects having actual BACs overestimated, p 753, right column, would more correctly involve the first 100 min, rather than 90 min, for which 50% are overestimated and the first 70 min, rather than 60 min, for which 60% are overestimated. On p 755, left column, it should be noted that Jones allowed 100 min after drinking stopped for absorption to be complete for the purpose of determining the variability in the blood/breath ratio in
21 subjects. Since this amount of time was based on peak capillary BAC, actual absorption time would be somewhat longer than 100 min. None of these 20-min corrections require changes in either the discussion or the conclusions found in my article.

References

The percentages reported (I) are based on averages over the particular time intervals. They can also be expressed at a particular time, as is done here. From the data of Martin et al. (3), this result is overestimated in 68% of subjects 90 min after drinking stopped. The two methods of calculation indicate that 50-90% of these subjects have their actual BAC overestimated during the first 90 min after drinking.

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Cardiac Enzymes and Hypothyroidism

To the Editor:

We recently described the case (I) of a 74-year-old man admitted to hospital with hypothyroidism and in myxedema coma. His plasma cardiac enzyme activities, as measured at admission and on serial measurement thereafter, raised the possibility of acute myocardial infarction. Total creatine kinase (CK) activity was above normal, as were both the activity and proportion of the relatively myocardial-specific CK-MB isoenzyme. The CK activity declined in a manner consistent with myocardial infarction. However, aspartate aminotransferase (AST) and lactate dehydrogenase (LD) activities declined more slowly than seen in myocardial infarction. Because other investigations of his cardiac status were negative, we concluded that he had not suffered a myocardial infarction, but rather that the increased activity and proportion of CK-MB represented ectopic synthesis in skeletal muscle. Although we favored a hypothyroid myopathy as the cause of his enzyme changes, we could not completely exclude hypothermia as the cause of the observed changes. One month after the commencement of thyroid-replacement therapy, enzyme activity and thyroid function were all within normal limits.

Six months after therapy commenced, this patient was seen in the Outpatient Clinic. He admitted that he had recently ceased taking his thyroid medications. He was not hypothermic and investigations performed at this time revealed (normal values in parentheses):

- CK (25-200 U/L) 241 U/L
- CK-MB (by immunoinhibition, U/L) 19 U/L
- CK-MB (<5%)
- LD (110-200 U/L) 196 U/L
- LD1:LD2 (<0.75) 0.82
- Thyroxin (70-140) 48 nmol/L
- Thyrotropin (<5) 46 milli-int. milli-int. units/L

Results of an electrocardiogram made at this time were normal.

Three weeks after recommencing thyroid, his CK had decreased to 104 U/L and his CK-MB to 6 U/L. His thyroxin concentration in plasma was 77 nmol/L and the thyrotropin concentration was still slightly above normal, 10 milli-int. units/L.

Thus in the presence of normal body temperature we have evidence of increased synthesis of CK-MB in skeletal muscle. We conclude that the high activity and proportion of CK-MB observed on this occasion were due solely to the hypothyroid myopathy. These findings suggest that the abnormal enzyme patterns seen at the time of his admission in myxedema coma were caused predominantly by the hypothyroid myopathy rather than the associated hypothermia.

Reference

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Increased Diagnostic Potential of a Monoclonal Assay of Carcinoembryonic Antigen

To the Editor:

In a recent comparison (I) of two new commercial kits for measuring carcinoembryonic antigen (CEA), the authors indicate that use of a monoclonal method may make CEA analysis more sensitive in the diagnosis and monitoring of colorectal carcinoma compared with polyclonal methods. We would like to present some data supporting this hypothesis.

We compared the Abbott CEA RIA, a monoclonal-antibody-based assay, with an in-house radioimmunoassay, using samples from 53 patients (mean age 66 y, range 37 to 88 y) with clinically and histologically confirmed colorectal carcinoma and 12 patients (mean age 55 y, range 29 to 73 y) with other forms of cancer (nine gastric, two breast, and one ovarian). The in-house method was a double-antibody tech-