

1950

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Recommended Citation

R. N. Harger, Debunking the Drunkometer, 40 J. Crim. L. & Criminology 497 (1949-1950)

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AMERICAN JOURNAL of POLICE SCIENCE

"DEBUNKING" THE DRUNKOMETER*

R. N. Harger

For the past 20 years R. N. Harger, Ph.D., Professor of Biochemistry and Toxicology at the University of Indiana Medical School, has made a special study of problems relating to the accurate diagnosis of alcoholic intoxication. The so-called "Drunkometer", an instrument for measuring the alcohol content of the breath, was developed by him as the outgrowth of this work and has had wide acceptance by police departments throughout the country. Dr. Harger is a member of the National Safety Council's Committee on Tests for Intoxication and has written extensively in professional journals on these tests. The Journal is pleased to be able to present the views of an authority of Dr. Harger's standing on the accuracy of breath tests for measuring alcoholic intoxication.—EDITOR.

The use of any scientific procedure for more accurately arriving at the truth in forensic cases seems to stimulate some people to seek for flaws in it. Whenever the drunkometer is demonstrated to a group of people someone always asks how to beat it, or what points about it might be used to cause a court to doubt its reliability.

THE DRUNKOMETER AND OTHER BREATH ALCOHOL METHODS

We have developed two methods for determining alcohol in body materials. The first was finished about 1928 and was published in 1935 (1). It involves distilling the material, oxidation of the distillate with dichromate, and titration of the excess dichromate with a solution of ferrous sulfate and methyl orange. The method has been quite widely used (2-7), but it must be operated by a well trained chemist.

Since many police laboratories do not have graduate chemists, we felt that there was a need for a simpler method which could be handled by an intelligent police technician. The method we developed for this purpose analyzes breath, and the reagent for alcohol is standard permanganate in 56 per cent sulfuric acid. In this concentration of sulfuric acid, which is much higher than that commonly used by chemists for analyses with permanganate, alcohol reacts rapidly and quantitatively with the permanganate at room temperature. In the procedure we also determine the weight of carbon dioxide in the sample of breath required to decolorize the permanganate. This enables us to cal-

*A similar paper written primarily for physicians will appear in the Quarterly Bulletin of the Indiana University Medical Center, 11, (4) (October 1949).

culate the volume of lung air¹ in this sample of breath. Haldane and Priestly (8) demonstrated that normal lung air regularly contains close to 5.5 per cent of carbon dioxide. Then, since it has been found (9-11) that 2000 cubic centimeters of lung air contain practically the same weight of alcohol as 1 cubic centimeter of blood, one can use the breath alcohol result to calculate the level of blood alcohol. What we really determine in this method is the weight of alcohol in the breath which accompanies the weight of carbon dioxide found in 2000 cubic centimeters of lung air, that is, the *alcohol-carbon dioxide ratio* of the subject's breath. Our breath method was first announced in 1931 (12) and, after several years of trial during which we compared the results with direct analyses of blood, it was published more fully in 1938 (13). When asked what name we had chosen for the portable apparatus used for the test we rather jokingly called it a "Drunkometer". The name is admittedly crude but fairly expressive, and it has stuck. It even appears in a recent medical dictionary (14). In our 1938 paper we said, "The method . . . will probably not predict the concentration of alcohol in the brain quite as closely as analysis of blood, but we believe that the results are amply accurate for practical use." Improvements in the method during the following three or four years have added to its accuracy for estimating blood alcohol, but the above statement made in 1938 still holds.

Since 1938 two other breath methods have appeared. The first was reported by Jetter, Moore, and Forrester in 1941 (15). It uses exactly the same principles as the drunkometer, including the alcohol-carbon dioxide ratio, but the permanganate reagent is replaced by another procedure for determining alcohol. The alcohol, plus moisture, in the breath is absorbed by a white solid, magnesium perchlorate. Later, a chemist dissolves the solid in water, distills off the alcohol, and analyzes the distillate for alcohol by our dichromate method, (1) or a similar one. Jetter and Forrester call their apparatus an "Intoximeter".

Still another breath method was developed by Greenberg and Keator of Yale (16). The subject exhales deeply through a metal tube inside a warm box, and 100 cubic centimeters of the last portion of this breath are trapped in a metal chamber. This sample, which is lung air, is then passed through a glass tube containing hot iodine pentoxide, a white powder. This chemical reacts with the alcohol to liberate iodine which passes

¹ In this paper *lung air* refers to what physiologists call *alveolar air*. This is the air from the minute air sacs (alveoli) which are the terminal ends of the smallest branches of the windpipe (trachea). The last quarter of a deep exhalation is alveolar air.

into a second tube containing starch solution, imparting a blue color to this solution. The amount of blue color is read with an electric eye. Greenberg and Keator have named their apparatus an "Alcoholometer".

CRITICISMS OF THE DRUNKOMETER

One early critic of our breath method was the late Dr. Logan Clendening of Kansas City. He wrote a syndicated column on "Diet and Health". In 1940 (17) he tried to answer an inquiry about the chemical reaction in the drunkometer method. His explanation was partially correct, and then he added: "Confidentially, the test is fairly easy to beat. Three or four deep breaths and four or five swallows of water will clear the breath and mouth of all except the most minute traces of alcohol". The Kansas city police technicians challenged the doctor to come over and test out his claims, but he did not accept the invitation. This writer wrote Dr. Clendening a mild letter suggesting that if he would try his stunts for beating the test he would find that they would not work, as we had shown long before. He answered my letter, giving some excuses for his statements, and closed by saying, "However, I recognize that this is a good practical test and will make proper acknowledgment in the future". The good doctor passed away in 1945, but if he ever made the promised acknowledgment this writer failed to see it.

In 1940 the *Journal of the American Medical Association* published a letter from Leake, Swim, and McCawley of California (18) questioning the reliability of chemical tests for intoxication in general. It stated that experiments by them then in progress showed that lack of oxygen caused by drowning, etc., and perhaps violent exercise, will generate an alcohol-like substance in the blood of non-drinking persons and rabbits. They suggested that the reducing substance might be a common body substance such as lactic or pyruvic acid. These claims were promptly challenged in letters to the *Journal of the American Medical Association* by Heise (19), chairman of the American Medical Association committee on tests for intoxication, Jetter (20), Bavis (21), and this writer (22). We pointed out that lactic and pyruvic acid cannot distill under the conditions used, and that the alleged results were contrary to the accumulated findings of very many workers, including the writers. In our laboratory we then ran analyses on the blood of 50 rabbits which were killed by drowning. Not one yielded the slightest evidence supporting the claims of the California group. We decided to delay reporting our results until the complete work of Leake,

et al. had appeared. To date they have published nothing further on this matter, and no one else has reported any confirmation of their fantastic claims.

Cameron, of Winnipeg, criticized chemical tests for intoxication (23). He asserted that acetone, which is present in the breath in cases of severe, untreated diabetes, may give false results with the drunkometer. Like Clendening, he evidently had not taken the trouble to test his claim for, as mentioned in our 1938 paper, acetone does not react at all with our permanganate reagent.

In 1947 this writer received a letter from a traffic official in California stating that some one there had claimed that the volatile oils from the rinds of citrus fruits will react like alcohol in the drunkometer. In our 1938 paper (13) we had stated, "The breath odors resulting from the consumption of onions, garlic, sen-sen, and cloves do not affect the reagent, and tests conducted on more than 1000 normal and hospitalized subjects failed to reveal the presence of any substance in the breath of these non-alcoholic people that was capable of reducing the permanganate reagent". This matter of citrus fruit rinds presented a new "alibi" for the drinking driver, so we tried it. Orange peel produced no effect at all. We found the same for grapefruit rind, except where the breath sample was taken with the mouth filled with the masticated material. Here it took about 3 gallons of breath to cause much fading of the purple color, as compared with $\frac{1}{2}$ pint to a pint usually required to completely decolorize the reagent in the case of an intoxicated person. In reporting the result to the California official we told him that the drunkometer operator could surely detect if the subject had a mouthful of grapefruit rind. We added that we were amazed that any California citizen had to resort to eating the rinds of their abundant supply of citrus fruits.

The rumor about onions, garlic, etc., has recently been revived by Corrigan of Detroit (24), who added limberger cheese to the list. During the trial of an alleged drunken driver he was able to demonstrate that air passed through a mixture of water and hashed onions slowly decolorized the drunkometer fluid. As pointed out by Dr. C. W. Muehlberger and this writer (25), this test is unfair since, after eating onions, one does not breathe through an onion mash. While the substances mentioned may contain traces of reducing materials, the fraction returned to the breath is too infinitesimal to affect the drunkometer reagent, as we have repeatedly shown. Technicians from the National Safety Council employed the drunkometer to test

about 3,000 subjects, some of whom had been drinking. They reported (26) that not one of these subjects disputed the drunkometer result found in his case.

In 1941 Haggard et al (27) advanced three criticisms of the drunkometer: (A) that the 1:2000 ratio used by us for calculating blood alcohol from breath alcohol is wrong; (B) that moisture condensed from breath stored in a balloon removes alcohol, producing low results; and (C) that our use of the alcohol-carbon dioxide ratio gives high results because the proportion of alcohol in the mouth and windpipe is higher than that in the deeper portions of the lungs. We will discuss these three points separately.

(A) *The breath : blood alcohol ratio.* This ratio may be derived from (1) experiments with blood in a glass vessel, or (2) studies with living subjects.

(1) *Results of experiments with blood in a glass vessel.* Blood, containing alcohol, is agitated with air in a closed glass vessel maintained at a given temperature, until the air will take up no more alcohol. Portions of the air and blood are then analyzed for their alcohol content. The ratio for this temperature is constant for any alcohol concentration found in the body. During 1929 we made comprehensive determinations of the air : water alcohol partition ratio at various temperatures, to serve as a basis for evaluating our methods and results with blood. Following this we conducted similar studies with blood. These results are reported elsewhere (28). Our results for air : water agreed substantially with the few then appearing in the literature, as is shown in Fig. 1-A. Ratios for air : blood published by Liljestrand and Linde in 1930 (19) (Fig. 1-B) were somewhat higher than ours, particularly those at 37° C. (98.6° F.) which showed poor agreement among themselves. In 1934 Haggard and Greenberg (29) reported alcohol partition ratio figures for air : water and air : blood which were far higher for the air phase than those of other workers and ourselves (Fig. 1). Haggard and Greenberg's ratio for air : water at 25° C. (77° F.) was 2.4 times that found by Foote and Scholes (30), Thomas (31), Dobson (32), and ourselves (28); and their ratio at 40° was 28 per cent higher than that found by Wrewsky (36). In January, 1940, the writer sent Dr. Haggard his curves for air : water and air : blood and called his attention to published results of others. He replied, admitting errors at room temperature. Then, in 1941 Haggard et al (27) published a second set of results for air : water and air : blood (Fig. 1). These were much lower than their 1934 results with the curious exception

of blood at 40° C. (104° F.) which was a trifle higher. However, almost all of their 1941 results were still considerably higher than those of other workers, including ourselves. Haggard et al (27) attributed the large errors in their 1934 results to removal of alcohol from the air phase by condensed moisture in the cooler parts of their apparatus. They said that this source of error was "unrecognized" by them in 1934, although a brief search of the literature would have shown that it had been mentioned by Foote and Scholes of Yale in 1911 (30) and by Dobson (32) of England in 1925, which workers used proper measures to avoid it. At any rate, this explanation of Haggard et al lacks plausibility because moisture condensation would have caused the concentration of alcohol in the air to be *too low, and not too high as they reported*. Furthermore, their greatest errors were at room temperature where moisture condensation would have been negligible. It is thus evident that their 1934 results must have involved some other gross error, which they apparently failed to entirely eliminate in their 1941 studies. At a given temperature there is only one correct air : water alcohol ratio, and for normal bloods the ratio can vary but little. We would welcome check studies of this rather simple problem by other chemists.

(2) *Results of studies with living subjects.* In 1930 Liljestrand and Linde (9) ran alcohol analyses upon samples of blood and lung air taken simultaneously from human subjects. They found a constant lung air : blood alcohol ratio which was close to 1 : 2000. On the other hand the ratio for lung air : blood reported by Haggard's group was 1 : 1150 in 1934, and 1 : 1300 in 1941. In both reports they stressed the point that their ratio obtained with living subjects agreed with the ratio which they found at 38° C. (100.4° F.) in experiments conducted in a glass vessel. However, as pointed out by Winslow et al (33), the temperature of breath leaving the mouth or nose is about 34° C. (93.2° F.). The latter temperature would control the lung air : body fluid ratio. Haggard et al should have compared their results with living subjects with the ratio which they obtained at 34° C. in their experiments in a glass vessel. Our studies (11) with a large number of subjects showed a lung air : blood alcohol ratio close to the ratio we observed at 34° C. in our studies with blood and air in a glass vessel. This ratio is about 1 : 2000 and is in agreement with the findings of Liljestrand and Linde mentioned above.

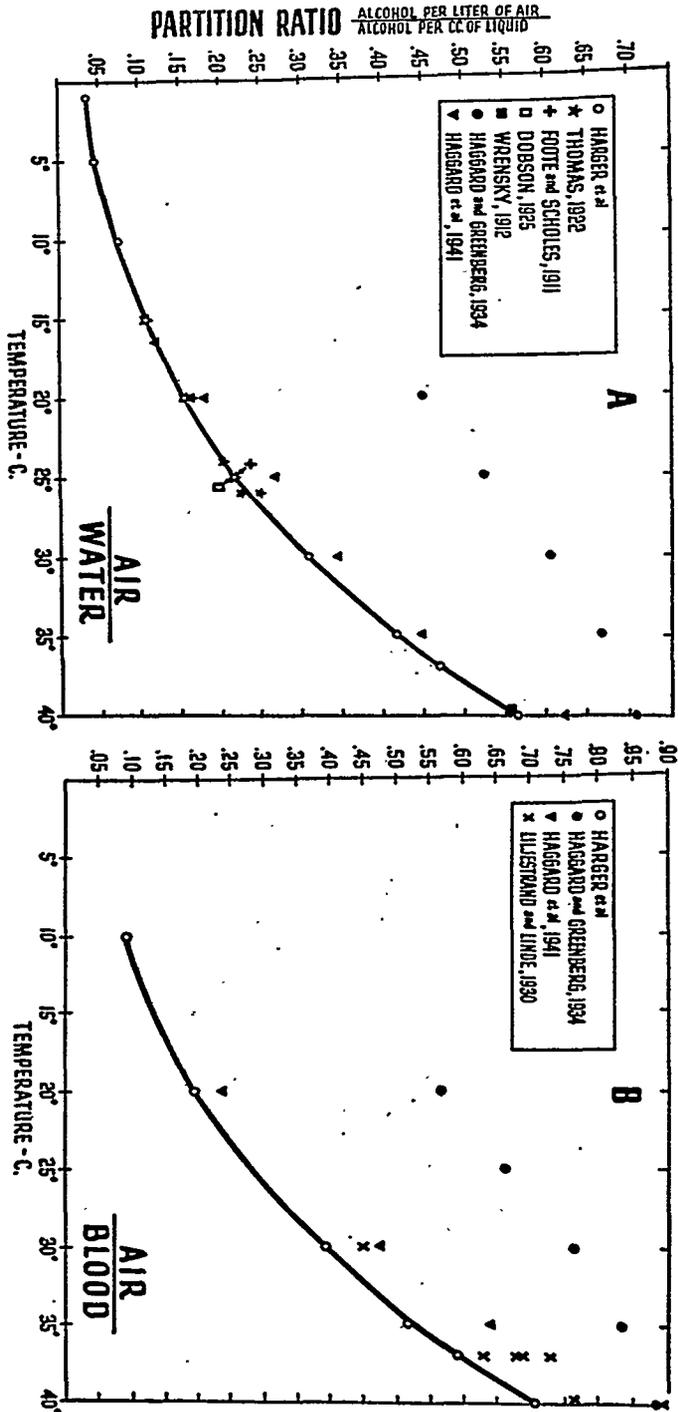


Figure 1.

Partition ratio of alcohol between air and water and air and blood for various temperatures. For air: water at 15°, 20° and 25° our results and those of Thomas are almost identical and are plotted as an open circle within a star. At 25° the results of Fote and Scholes and of Dodson are practically the same as that of Thomas and ours, as shown by the arrows.

B. Possible loss of breath alcohol from moisture condensation. Haggard et al based this criticism on curves for the drop of alcohol in air saturated with moisture at 40° C. (104° F.) when it was cooled to various temperatures. This is misleading because, as pointed out in the previous section, breath leaving the mouth has a temperature of about 34° C. (93.2° F.) with a moisture content representing saturation near this temperature. During the time required to run a drunkometer test the temperature within the ballon rarely falls below 28° C. (82.4° F.). A simple calculation will show that the total amount of water which could be formed by condensation caused by a drop in temperature from 34° C. to 28° C. could remove not more than 2 to 4 per cent of the alcohol. Jetter and Forrester (10) made breath alcohol determinations where the sample was collected in a balloon in a warm room and also where the temperature was 15° C. (59° F.). They observed no difference in the results. We ran simultaneous drunkometer analyses at 40° C. (104° F.) in an incubator, and at room temperature (11). The results of those run at 40° C. averaged only 1.8 per cent above those run at room temperature. Even assuming a greater loss of alcohol, the result would be advantageous to the subject being tested.

C. Reliability of the alcohol-carbon dioxide ratio. Haggard et al (27) reported that the ratio of alcohol to carbon dioxide is higher in ordinary expired air than it is in lung air. On the other hand, Jetter and Forrester (10) found no difference in the ratio in total expired air and in breath collected after discarding the first 500 and 700 cubic centimeters exhaled. Using a large number of subjects, we (11) determined this ratio for both lung air and ordinary breath, with the latter samples collected in rubber balloons as in the drunkometer technique. The samples of lung air and part of the balloon samples were kept at 40° C. (104° F.). Our results showed no significant difference between the ratio of ordinary expired breath and lung air. However, when we collected the ordinary breath in a flexible aluminum bag (34), which required practically no pressure to inflate, the ratio of alcohol to carbon dioxide averaged about 14 per cent higher than for lung air. This difference, which may explain the results obtained by Haggard et al, may result from a decrease in the respiratory dead space caused by contraction of the muscles of respiration required to develop the pressure needed to inflate the ballon, or perhaps from a lengthened interval between inspiration and expiration. This result is irrelevant for methods which use rubber balloons.

The acid test here is whether the alcohol-carbon dioxide ratio will reliably predict the level of alcohol in the blood. From results of 79 determinations Jetter and Forrester (10) found that the alcohol-carbon dioxide ratio of the breath is reliable as a practical means of predicting blood alcohol from breath alcohol. This was confirmed by Fabre and Leheuzey (35) of France from tests on a small number of subjects. Our results published in 1938 (13) are in agreement with those of Jetter and Forrester and Fabre and Leheuzey, although in some of our very early determinations the correlation between calculated blood alcohol and direct analysis was not too good. A later study by us (11), employing improvements in our breath method developed during the next three years, showed a satisfactory correlation between results of the breath method and direct analysis of blood.

Finally, Haggard et al reported no tests in which they used the actual technique of the drunkometer or the intoximeter. Their criticisms are based on certain isolated experiments, most of the results of which we have been entirely unable to confirm.

No one should object to any valid criticism of an analytical method. However, the criticism should not be based upon untried guesses, but upon careful tests with experimental results which can be duplicated by other workers.

REFERENCES

1. Harger, R. N., *J. Lab. Clin. Med.*, 20:746, 1935.
2. Cavett, J. W., *J. Lab. Clin. Med.*, 23:543, 1938.
3. Levine, H., and Bodansky, M., *Am. J. Clin. Path. (Tech. Suppl.)*, 4:103, 1940.
4. Jetter, W. W., *Quart. J. Studies Alc.*, 2:512, 1941.
5. Fisk, V. B., and Nelson, V. E., *Proc. Iowa Acad. Science*, 48:207, 1941.
6. Greenberg, L., *Quart. J. Studies Alc.*, 6:135, 1945.
7. "Methods for Laboratory Technicians", U. S. War Dept. Technical Manual TM8, 1946, p. 255.
8. Haldane, J. S., and Priestly, J. G., *J. Physiol.*, 32:225, 1905.
9. Liljestrand, G., and Linde, P., *Skand. Arch. Physiol.*, 60:273, 1930.
10. Jetter, W. W., and Forrester, G. C., *Arch. Path.*, 32:828, 1941.
11. Harger, R. N., Forney, R. B., and Barnes, H. B., *In Press*.
12. Harger, R. N., *Science*, 73:10, 1931.
13. Harger, R. N., Lamb, E. B., and Hulpieu, H. B., *J. A. M. A.*, 110:779, 1938.
14. *Gould's Medical Dictionary*, 5th ed., Philadelphia, 1943.
15. Jetter, W. W., Moore, M., and Forrester, G. C., *Am. J. Clin. Path. (Tech. Suppl.)*, 5:75, 1941.
16. Greenberg, L. A., and Keator, F. W., *Quart. J. Alc. Studies*, 2:57, 1941.
17. Clendening, L., *Kansas City Star*, Jan. 9, 1940.
18. Leake, C. D., Swim, J. B., and McCawley, E. L., *J. A. M. A.*, 114:1098, 1940.
19. Heise, H. A., *J. A. M. A.*, 114:1391, 1940.
20. Jetter, W. W., *J. A. M. A.*, 114:2402, 1940.
21. Bavis, D. F., *J. A. M. A.*, 115:73, 1940.
22. Harger, R. N., *J. A. M. A.*, 114:1687, 1940.
23. Cameron, A. T., *Can. Med. Assn. J.*, 43:46, 1940.
24. Corrigan, K. E., *Detroit Free Press*, Feb. 18, 1949.
25. Whittall, J. D., *Public Safety*, April, 1949, p. 6.
26. *Public Safety*, Dec., 1939, p. 18.

27. Haggard, H. W., Greenberg, L. A., Miller, D. P., and Carroll, R. P., *J. Lab. Clin. Med.*, **26**:1527, 1941.
28. Harger, B. N., Raney, B. B., Bridwell, E. B., and Kitchel, M. F., *In Press, Jour. Biol. Chem.*, Feb., 1950.
29. Haggard, H. W., and Greenberg, L. A., *J. Pharmacol. Exper. Therap.*, **52**:150, 1934.
30. Foote, H. W., and Scholes, S. R., *J. Am. Chem. Soc.*, **33**:1309, 1911.
31. Thomas, B., *J. Soc. Chem. Ind.*, **41**:34, 1922.
32. Dobson, J. E., *J. Chem. Soc.*, **127**:2866, 1925.
33. Winslow, C. E. A., Herrington, L. P., and Nelbach, J. H., *Am. J. Hyg.*, **35**:27, 1912.
34. Harger, B. N., Turrell, E. S., and Miller, J. M., *J. Lab. Clin. Med.*, **34**:566, 1949.
35. Fabre, B., and Leheuzey, P., *Bull. soc. chim. biol.*, **26**:49, 1944.
36. Wrewsky, M., *Z. physik. chem.*, **31**:1, 1912.