

Addendum to the Note of Williams and Watson

By JAMES W. MCBAIN

I do not consider that the note by Williams and Watson calls for any detailed comment. The two sentences published by McBain and O'Sullivan, one sentence in a preliminary communication and the other single sentence in a footnote in a paper of eleven printed pages entitled "The Development of the Air-Driven Spinning Top as a Transparent Ultracentrifuge," still appear to be ample notice of the physical phenomenon that was observed, on November 15, 1934, with the old circular cell whose use was discontinued that year. The cell contained pure water and a crystal of mercuric chloride. Sufficient of the latter dissolved in the lower portion of the cell and then through an accident of convection currents, the absorbing material (HgCl_2) was sedimented downward again with a fairly sharp boundary as shown by the photographs and microphotometer photographs, moving 0.102 cm. in 8400 seconds at 108,600 r. p. m. with an average radius of 1.054 cm., giving an observed $S = MD \times (1 - V)/RT = 0.890 \times 10^{-13}$, which, as a mere scientific curiosity, agreed with the value predicted. The precise agreement is of course fortuitous and should not have been expressed to three significant figures, as is obvious to anyone who has any knowledge of an ultracentrifuge.

Professor Williams has acknowledged Dr. O'Sullivan's statement that the sedimentation force was 138,000 times gravity, but Williams and Watson prefer to state "it is concluded that it was not over 350,000 times gravity."

McBain and O'Sullivan stated that this result was an accident and that in the ordinary course, where convection does not occur, a higher order of magnitude of centrifugal force than that in any existing ultracentrifuge anywhere today would be required to obtain sedimentation velocities of such small molecules regularly.

It is entirely a matter of individual opinion as to whether any significance whatsoever attaches to this photographed and microphotometered observation, and Williams and Watson are convinced on the basis of their experience that none does. We still think that it did offer an actual semi-quantitative measurement.

As regards the fourth paragraph of the note of Williams and Watson devoted to discussing the state of development of our air-driven ultra-

centrifuge as described at the April, 1935, meeting of the Society, it is only necessary to say that our statements were carefully made and stand accordingly as referring to the now superseded models as developed at that time; and that the appreciable modifications and the very simple temperature control in the later model referred to in the footnote by H. J. Fouts inserted at the end of that paper in the final proofs have resulted in uniform success in sedimenting smaller protein molecules such as carbon monoxide hemoglobin—as was, for example, demonstrated at the Western Protein Conference, June 6, 1936. There appears now to be no reason why any sedimentation velocity or equilibrium observable in the Svedberg ultracentrifuges, or the equally good air-driven models now developed, for example, at the Rockefeller Institute for Medical Research [see for example *J. Exptl. Medicine*, **64**, 39 (1936)], or the model supplied by the Sharples Specialty Company of Philadelphia should not also be capable of quantitative study in the McBain-O'Sullivan-Fouts transparent ultracentrifuge. All parts of this cell are easily kept at any desired constant measured temperature within 0.02° . All forms of the air-driven ultracentrifuge appear to be in process of still further rapid improvement.

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Chemical Nature of δ -Follicular Hormone

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In 1932 Schwenk and Hildebrandt¹ reported the isolation from the urine of pregnant mares of a new oestrogenic substance, isomeric with oestrone (theelin), which they designated δ -follicular hormone. Wintersteiner, Schwenk and Whitman² subsequently showed that this substance (m. p. 209°) was not a ketone, but a dihydroxy compound. Recently larger amounts of crude phenolic fractions from mares' urine were separated in the laboratories of the Schering Corporation into ketones and alcohols. The alcoholic portion was worked up for δ -hormone at Columbia University. Our present experience with material of this type makes it appear doubtful whether the earlier preparations represented a chemical individual,

(1) Schwenk and Hildebrandt, *Naturw.*, **20**, 658 (1932).

(2) Wintersteiner, Schwenk and Whitman, *Proc. Soc. Exptl. Biol. Med.*, **32**, 1087 (1935).