graduation pretending to indicate variations from a supposed *normal percentage* of urea in the urine is based on the unwarranted assumption that the daily emission of urine and urea is constant.

The ability required to make an analysis and to deduce from it correct conclusions is not greater than that necessary for a very simple calculation. There is, however, a method for the estimation of urea which avoids the necessity of any calculation except a single proportion, and which at the same time gives exact results. It is by a second and simultaneous determination of the volume of gas evolved by the decomposition of a known quantity of urea—say one centigramme—in aqueous solution. Supposing that all the nitrogen eliminated from the urine be derived from urea, the quantity in grammes of the latter in the volume of urine employed will be found by dividing one hundredth of the volume of gas obtained from it by the volume obtained from one centigramme of urea. If "the average medical man" possess a thermometer, a barometer, and a table of the tensions of aqueous vapor, he can make the calculation in less time than would be required for the decomposition of the urea solution.

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IN REBUTTAL.

BY DR. CHARLES A. DOREMUS.

The author of the preceding article has in two letters, one to the editor of a Philadelphia medical journal and another to Professor Austin Flint, Jr., which appeared in the *Medical News* of May 30th, criticised the instrument presented by me before this Society in March. The tone of the first letter was of so uncourteous a character, as it seemed to me, that no response was made to it. On the appearance of an abstract of it in the *Medical Record* of this city, an answer was written at the suggestion of friends, and would have been published had not Prof. Flint informed me of his intention of presenting to the New York County Medical Society, at Dr. Greene's request, the instrument described on the preceding pages.

Dr. Greene's letter to Prof. Flint appears along with a description of the apparatus which accompanied the instrument and some remarks made on the reclamation of priority and the comparative usefulness of the two ureometers. The publication of that article would, I thought, render any response from me unnecessary, but the foregoing personal criticism rather than that of the instrument, forces from me a reply

To those who heard the reading of my first communication it must seem incredible that so much has been said regarding the origin of the several parts of the ureometer.

My experience in operating with this instrument shows that for the purpose for which it can be legitimately used the volume of nitrogen evolved from a standard solution is remarkably constant. In the experiments that were made to verify the system of graduation a standard 1 per cent. solution of urea was made. The urea was powdered and dried over sulphuric acid for two weeks before weighing.

When Dr. Greene recommends a physician to prepare a standard urea solution for comparative testing he greatly overestimates the physician's ability to procure the urea, and his possession of the necessary apparatus with which to execute the manipulations. Prof. Wormley, in the paper already quoted, says, "During these investigations it was observed that, so long as the conditions remained the same, the relative proportion of the nitrogen eliminated was pretty uniform. Hence if the volume of nitrogen evolved from a known quantity of urea, under certain conditions, or by a given form of apparatus be determined, the result may be taken as the basis for the determination of the urea in the urine with sufficient accuracy for clinical purposes."

What the volume of nitrogen set free actually is, is differently stated by different observers. Huefner, as quoted by Neubauer and Vogel, gives 370 c. c. of nitrogen as being eliminated from 1 grain of urea, the gas measured at 0° C and 760 m. m. barometer. Wormley gives it as 372 c. c.

No stress was laid on any great originality on my part regarding the invention of the apparatus, and, though no exhaustive bibliography was given of the subject, it was certainly my intention to give such credit as would forbid offence to anyone concerned. Dr. Greene, however, states, "In October, 1884, I brought to the notice of the Philadelphia County Medical Society, and described as a modification that might be home-made, the identical tube apparatus, without base, now claimed by Dr. Doremus" (*Medical News*, April 4th, 1885).

As my experiments were begun last year about this time, I cannot have copied the "identical form" of Dr. Greene's apparatus. The general form and the method of graduation were given credit for in the paper presented in March, and there is nothing of great importance in the fact that the graduation is due to Russell and West. The facility with which an instrument is manipulated depends so greatly on the "personal equation" that I refrain from any further reply on these points. Prof. Flint's remarks in his concluding sentences are sufficient upon this point. The ureometer was purposely made without a base, and it is sold by dealers with or without as desired.

Dr. Greene seems to have thought of some advantages in that form of instrument when he devised his home-made apparatus.

I did not "overlook" the advantages that might result from using 5 c. c. of urine instead of one, but made careful experiments and decided that for the purpose for which the ureometer was intended there were more reasons for adopting the latter quantity for the test than the former.

Dr. Greene, in his Medical Chemistry, 1880, p. 110 *et seq*, gives only two processes for the determination of urea by the hypobromite solution and directs the use of 1 c. c. of urine in each case.

Prof. T. G. Wormley, in a critical examination of the hypobromite method published in the American *Journal of Medical Sciences*, July 1881, p. 128, adopts 1 c. c. of standard solution of urea for his experiments, and recommends the use of that quantity of urine in the practical examination of urine.

Where larger apparatus can be conveniently used there is certainly no objection to the use of 5 c. c. of urine, but I tried to reduce the bulk of the apparatus without diminishing sensibly its accuracy.

The method of transferring the bromine was described in my first paper. Unless one desires to be captious, it can safely be said that in drawing the bromine up in a nipple pipette by relaxing the nipple that has been compressed before the pipette is introduced below the level of the bromine and then, by quickly removing the pipette and squeezing the nipple, expelling the bromine below the sodium hydrate solution previously poured into the ureometer, "the bromine is transferred without the production of vapors."

I will not attempt to discuss Dr. Greene's remarks of a physiological character. They represent to a great extent his individual opinions, and I decline to prolong this debate by taking up new issues.

His remarks on the percentage composition are unnecessary, since the calculation to per cent. was added merely for convenience in reference, and for those who feel inclined to adopt that method of comparison.

Most works on urine give the centesimal composition of the urine though they direct that the calculations be made on quantity voided in 24 hours.

Tyson in his edition of 1883 says, in regard to the determination of urea, p. 106, "whence can be calculated either the percentage or the 24 hours quantity."

Most physicians know that to obtain a just idea of the condition of the urine the specimen examined should represent a known portion of the entire quantity voided in 24 hours. For all quantitative estimations a specimen taken from the total voiding should be analysed. When this is done and the volume of urine is close to 1,500 c. c. or can be diluted to that volume, then the normal mark on the apparatus gives at least approximate knowledge of the excretion of urea. It would be absurd to suppose that any guide could be given that is arbitrary. It is distinctly stated that the ureometer is graduated for 65° F. This temperature is easily obtained for the instrument, if necessary, at all times of the year by placing it for a few minutes in water at that temperature. That particular temperature was selected by Russell and West as being that of apartments where the instrument they devised would be most frequently used. Also because "a fortunate compensation of errors occurs with this form of apparatus under these circumstances. The tension of the aqueous vapour, together with the expansion of the gas at this temperature, almost exactly counterbalances the loss of nitrogen in the reaction." (*Journal Chemical Society*, Vol. 27, p. 751.)