these into consideration, my analysis shows that: (A) the statistical correction may easily become negative; that is, the true correlation may be considerably lower than the observed correlation. On the other hand, if "errors" are independent (or as my analysis shows, for particular values of correlation between errors), then (B) the correlation may be positive as found by Chapman, and the true correlation higher than the observed. The question is: under which category (A) or (B) above does the work of Dines fall?

In the case of a balloon meteograph, all measurements are made on one and the same trace,[6] and the heights are calculated with the help of Laplace's formula.[7] This formula involves both pressure and temperature, and a detailed examination shows that it serves to introduce, through "interpolation," correlation between errors of measurement in pressure and temperature. Besides this "interpolation" effect, correlation may also be introduced through what Karl Pearson[8] calls the "atmosphere" of measurement and through correlation of successive judgments.[9] It is, therefore, not improbable that Dines's work falls under (A) and gives values of correlation coefficients higher than their true values. My contention is this: (C) in the absence of definite proof that Dines's work falls under (B), Chapman's corrections cannot be accepted as real, and, to be on the safe side, Dines's coefficients must be looked upon as giving superior limits to the true correlation.

Douglas[10] found the values of correlation between pressure and temperature at 10,000 feet to be 0·65, which is considerably lower than Dines's figure 0·77 (and still more so than Chapman's corrected value). I quoted Douglas's result, as I thought his work to be free from the peculiar "interpolation" correlation introduced by the use of Laplace's formula. On this view, Douglas's work would probably come under (B) and would give values of correlation lower than true values. I now find stated in the note in NATURE that I have fallen into error in thinking "that Douglas's coefficients are based on true heights." (The fault, however, is scarcely mine, for Douglas himself definitely stated[11] that his observations "refer to actual heights above mean sea-level, and not to aneroid heights.") On the present view, Douglas's work also would probably come under (A) above, and even 0·65 would seem to be too high a value for the true correlation. This corroborates my contention (C) that Dines's coefficients are probably too high. It is, therefore, clear that the rectification of my error has further strengthened my conclusion. I may note in passing that the low values of the coefficients obtained by Douglas may be easily explained in accordance with my analysis if we assume that the magnitude of the correlations between errors of measurement are lower in his case.

In my other memoir[12] I pointed out certain statistical discrepancies in the coefficients published by Dines. It is stated in the note in NATURE that I seem "to have confused the T_m used by Dines, namely, the mean temperature between 1 and 9 kilometres, with the mean temperature between 0 and 9 kilometres," and that this supposed confusion on my part "fully explains the discrepancies" noted by me. I am unable to agree with this, as I do not think I have made any confusion between the two mean temperatures referred to above. On p. 1 and p. 3 of my memoir I have explained clearly that T_z represents the mean temperature between 0 and Z kilometres, and I have kept T_z and T_m distinct throughout. It is true I have substituted $dT_z = dT_m$, but this is quite different from putting $T_z = T_m$, since dT_z and dT_m are both statistical differences (which would ultimately be summed and averaged out) and not analytic differentials. This substitution is further discussed on p. 6 of my memoir. Now if this substitution is justified, then it follows from Laplace's equation that: (D) in the case of the figures published by Dines it is actually possible to obtain higher values of the correlation coefficients at levels considerably lower than 9 kilometres. In view of the assumption involved it is, however, necessary to test (D) by direct examination of the data concerned. But in the absence of such examination it is not sufficient to state that "discrepancies can be explained."

To sum up, the main problem is to find (*a*) the true correlation, and (*b*) the region of the best correlation in the case of upper air variables. It would seem that in view of (A), (C), and (D) above, the work of Dines and Chapman (which is flatly contradicted by that of Douglas) cannot be accepted as final either as regards (*a*) or as regards (*b*). Further advance is not possible without a thorough statistical scrutiny of the original data.

May I, therefore, suggest that (i.) the original material of Dines and Douglas (as well as other fresh material, if available) be published with clear statements about methods of measurement employed and actual formulæ (rigid or otherwise) used for computation of heights, and that (ii.) such material be submitted to some statistical expert like Prof. Karl Pearson for examination and report.

P. C. MAHALANOBIS.

Presidency College, Calcutta,
June 20.

THE results of the British Registering Balloon Ascents are published in full by the Meteorological Office in the Annual Supplement to the *Geophysical Journal*. A full description of the instruments, methods, and formulæ used have also been published by the M.O., and will be found in the "Computer's Handbook," M.O. 223, Section II., subsection ii. They are open to anybody for use, and if Prof. Mahalanobis will carry out the computation he desires he will earn the thanks of meteorologists.

It is difficult, however, to see how Prof. Mahalanobis can obtain a perfectly correct correlation coefficient, in view of the fact that, with a coefficient of 0·70 based on 400 observations, the causal standard error is as high as 0·025. This fact suffices to explain the differences between Dines's and Douglas's results, which can scarcely be called a "flat contradiction."

With reference to Prof. Mahalanobis' assumption, that $dT_z = dT_m$, it may be pointed out that the result of making this assumption is discussed in the papers to which he referred, and also that no claim to extreme accuracy in the correlation coefficient is made by Dines. (See M.O. 210*b*, bottom of p. 43, and p. 44, line 11; also *Beiträge zur Physik der freien Atmosphäre*, V. Band, Heft 4, pp. 222, 223, and 225.) THE WRITER OF THE NOTE.

Tubular Cavities in Sarsens.

WITH regard to Mr. F. Chapman's letter on the probable æolian origin of sarsen rock (NATURE, August 18, p. 239), and his reference therein to my previous note, may I say that I was not referring to

[6] M.O. No. 210*f*, Geophys. Mem. 6, 1914.
[7] M.O. No. 223, "Computer's Handbook," Section 2.
[8] Phil. Trans. 198 A, 1902, "Errors of Judgment," etc.
[9] Egon S. Pearson, *Biometrika*, xiv., 1922.
[10] Quar. Jour. Met. Soc., xlvii., January 1921, p. 28, etc.
[11] *Ibid.* p. 25.
[12] Mem. Ind. Met. Dept., vol. xxiv. Part I., "The Seat of Activity in the Upper Air."