An Example of Spurious Correlation.

IF I am not mistaken, the first method of forecasting the summer season proposed by Mr. A. B. MacDowall in NATURE of September 16, 1909 (vol. lxxxi., p. 335), is based upon a spurious correlation. If we take a series of departures from normal of a meteorological element and tabulate the sums of consecutive groups of thirty, there will always be a relationship between these sums, although the original departures may be entirely independent, and hence the relationship between the sums cannot be utilised for forecasting an individual term of the original series. That such sums of independent departures are not independent may be seen in the following way. If we denote the original independent departures by d_1, d_2, . . ., and the sum of thirty quantities beginning with d_p by s_p, the correlation coefficient between such quantities as s_p and s_{p+1}, as given by statistical methods, will be the mean value of a long series of products $s_p\, s_{p+1}$ divided by the product of the square roots of the mean values of s^2_p and of s^2_{p+1}. Now as d_p, d_q are independent the mean value of the product $d_p\, d_q$ will be zero; and it is easily seen that the correlation coefficient in question is the mean value of $(d^2_{p+1}+d^2_{p+2}+\ +d^2_{p+29})$ divided by the product of the square roots of the mean values of $(d^2_{p}+\ .\ .\ .\ +d^2_{p+29})$ and of $(d^2_{p+1}+\ +d^2_{p+30})$; if we denote the mean value of d^2_q by m^2, this becomes $29m^2/30m^2$, or 29/30. Thus the thirty-year sums of independent annual departures will tend to vary closely together, and the dots in a diagram like that of p. 335 would tend to lie on a straight line.

The relationship actually found by Mr. MacDowall between the sums does not appear, therefore, to afford a satisfactory basis for a forecast. GILBERT T. WALKER.

India Meteorological Department, Simla,
December 16, 1909.

On Fluorescence Absorption.

IT is desirable to direct attention to Prof. R. W. Wood's most important paper in the *Philosophical Magazine* for December, 1908, on a method of showing fluorescent absorption directly if it exists; but it seems certain that he has, at the end, drawn a conclusion from his experiments the very opposite, as I venture to think, to that to which they really lead. He compares the light apparently transmitted by a fluorescent body when fluorescence is, and is not, taking place, and finds that there is no difference in the resultant effect. This, I think, is as it should be; but the inference he draws is that there is no difference in the absorption. For my part I must admit that it only confirms my results published in the Philosophical Transactions, vol. cxci., A, 1898, that there is such an absorption; for if there were none such the light *apparently* transmitted would be less when the body is not fluorescing, owing to the fact that the fluorescent light would increase the *apparent* transmission, and a flickering should ensue; but Wood's experiment demonstrates that this is not so. The inference I should draw, then, is that during fluorescence there is an increased absorption of the light transmitted.

Prof. Wood appears to assume, moreover, that the resultant effect on the retina of two successive flashes is equal to the sum of the two acting simultaneously, which is not the case, since the successive flashes act merely as an intermittent single flash would do.

Nichols and Merritt, who have fully confirmed my results spectroscopically, have shown that the absorption effect diminishes as the intensity of the transmitted light increases, so that when the intensity of the transmitted light is large in comparison with that of the fluorescent light there is no effect at all, owing to the fact that this transmitted light itself is sufficiently intense to excite fluorescence, and there is therefore no change of state in the two cases.

If uranium glass is used for the absorption—it was with uranium glass that I observed the effect—the source of the transmitted light should also be uranium glass. I may add that the best results were obtained by using the light from the spark between cadmium electrodes for exciting fluorescence. With a suitable Leyden jar in the circuit, the illumination is sufficiently steady, and any errors in this respect can be detected by the null method I have described. J. BUTLER BURKE.

December 18, 1909.

Adsorption.

"THE above effects, however, become of consequence in those frequent cases in which a muddy liquid is only partially filtered through a dry filter in order that some analytical estimation may be made in a given volume of the filtrate. *The first drops of the filtrate must therefore be discarded.*" The above quotation is from Ostwald's "Foundations of Analytical Chemistry" (English translation), the italics being in the text. Ostwald makes this a purely theoretical deduction, but the practice of discarding first drops does not, I think, originate with him. Doubtless many analysts neglect the precaution, but many use it.

Some experimental work on adsorption which I am at present carrying on seems to point to the practice being quite uncalled for in at least the majority of cases. I am not yet ready to speak definitely, but it seems to be as unnecessary as it would be to reduce weighings to a vacuum standard in everyday analytical work. Ostwald's extreme positiveness, however, makes me wonder whether I have overlooked some serious fault in my experimental methods, and I should be much indebted to anyone who would point out to me any references in the literature which give an *experimental* justification of the practice. The absence of any library facilities in this place makes a systematic search of the literature impossible to me.

ALFRED TINGLE.

Imperial Chinese Pei Yang Mint, Tientsin,
December 8, 1909.

The Terminal Velocity of Fall of Small Spheres in Air.

AT the Winnipeg meeting of the British Association Prof. Zeleny and Mr. McKeehan read a paper on the terminal velocities which they had found when Lycopodium and other small spores fall through air. The measured terminal velocities were only about half those calculated by Stokes's formula. The fall was steady, no Brownian motion or rotation being visible. The authors of the paper have since succeeded (see NATURE, December 9, 1909, p. 158) in making minute spheres of wax and mercury which do obey the theoretical law, but add that the reason for the deviations in the former cases is not clear.

May not the reason for these deviations be the roughness of the spore? The drops, through surface tension, are smooth and practically perfect spheres, whereas a spore of Lycopodium is very rough relative to its size. (Using a microscope objective with large aperture, and oblique illumination, Lycopodium spores of about 14 μ radius were seen to be coated with hair-like projections.) The spore would, from its roughness, leave a tail of small eddies behind it. The increased energy of this turbulence represents the increased resistance which the spore experiences on account of its roughness, as compared with that experienced by the smooth drop considered in the theoretical law, much as the speed of a ship is lessened when its bottom is foul.

As a suggested experimental test, an increase in the pressure of the air will not affect the viscosity, but will alter the energy in this tail of small eddies. So also would a moderate decrease in the pressure, while yet it would probably not bring the relative size of the spore and of the gaseous molecular free path too close for the theory to be applicable. Should this be the case, however, it would be shown by the appearance of Brownian motion. EDITH A. STONEY.

Positions of Birds' Nests in Hedges.

LIEUT.-COLONEL TULL WALSH's observations as to the positions of nests (NATURE, December 16) are interesting, as they tally with the aspect of arboreal cryptogams, as already noted by me. South-west winds depositing sulphurous and nitrous products to leeward of towns cause lichens and mosses to flourish best on the eastern side of trees and hedges; and, moreover, this is general, for winds bearing spores from the south-west continually play on the trunks and blow away spores as they settle. If it were not for a kind of capillary attraction or rotary motion drawing the spores round the trunk to leeward, or east or north-east, they would never germinate. So the eastern side is the most productive, though often the western