

LETTERS TO THE EDITOR.

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Beliefs and Customs of the Australian Aborigines.

IN May of last year (1908) I had the good fortune to meet the Bishop of North Queensland (Dr. Frodsham) at Liverpool, and he gave me in conversation some valuable information as to the native Australian beliefs and customs based on his personal knowledge of the aborigines. He told me that he had travelled among the Arunta as well as among various North Queensland tribes, and he asked me whether I was aware that the Australian aborigines do not believe children to be the fruit of the intercourse of the sexes. His Lordship informed me that this incredulity is not limited to the Arunta, but is shared by all the North Queensland tribes with which he is acquainted, and he added that it forms a fact which has to be reckoned with in the introduction of a higher standard of sexual morality among the aborigines, for they do not naturally accept the true explanation of conception and childbirth even after their admission into mission stations. The Bishop also referred to a form of communal or group marriage which he believes to be practised among aboriginal tribes he has visited on the western side of the Gulf of Carpentaria, but, unfortunately, I had not time to obtain particulars from him on this subject.

I pointed out to his Lordship the high scientific importance of the information which he had volunteered to me, and I requested that he would publish it in his own name. He assented; but as some time has passed without his finding leisure to draw up a full account, he has kindly authorised me to publish this brief statement, which has been submitted to him and approved by him as correct. I need not indicate to anthropologists the great interest and value of the Bishop's testimony as independently confirming and extending the observations of Messrs. Spencer and Gillen on the tribes of Central Australia. In the interest of science it is much to be desired that the Bishop, or those of his clergy who know the natives, would publish fuller information on these topics. J. G. FRAZER.

Cambridge, August 23.

A Question of Percentages.

IN NATURE of August 5 (p. 159) Mr. Cunningham asked a question as to the proper method of arriving at the mean percentage of marks obtained on papers of different values in an examination, and this has been very clearly answered by Mr. Whalley. The same question, however, arises in experimental work, particularly in agricultural and horticultural experiments, and there the answer is by no means so evident. An examiner may be supposed to have sufficient knowledge to weight his papers properly, but in an experiment no data may be available for the purpose.

Take a case where three sets of different varieties of trees are subjected to some particular treatment, and compared with three similar sets not so treated, and suppose, as an exaggerated example, that the actual measurements, say, of growth, are as follows:—

	Treated	Untreated	Diff. per cent.	Diff. per cent.	
				A	B
I.	240	120	+ 100	+ 100	+ 100
II.	60	50	+ 20	+ 20	+ 20
III.	4	8	- 50	- 100	- 100
Sum	304	178	—	—	—
Mean diff. +71	+ 23	+ 7	+ 7

If the numbers of trees in the various sets are not the same, the results may, of course, be easily weighted to correct for this; but there are other differences for which they cannot be weighted, namely, those dependent of the differences in nature of the different varieties and of attendant circumstances beyond the control of the experimenter. The mean deduced from working each result out separately (+23) ignores all such differences, and is clearly incorrect; but that deduced from the sums of the measure-

ments (+71) is equally so, for it ignores the difference in habit of the different varieties, and gives undue weight to the results from that variety which happens to be the most rampant grower. This difficulty has been alluded to more than once in the reports of the Woburn Experimental Fruit Farm, and the only way out of it appears to be to take the mean of the means deduced in these two ways; at any rate, it is rarely safe to draw any conclusions as to the results of experiments unless these two means agree fairly with each other.

Similar difficulties arise in interpreting the results of other experiments; with a number of analyses, for instance, in which different quantities of material were taken, the mean of the individual results assumes that none of the errors is proportional to the quantities taken, whereas a mean deduced from the sum of the quantities taken and found assumes that all the errors are directly proportional to these quantities, neither of which assumptions is correct, as a rule.

Another source of error in horticultural experiments is that the differences observed are not unfrequently of different signs, and since a plus difference of 50 per cent. has a very different value from a minus difference of 50 per cent., the algebraic sum of such differences is fallacious. This is evident from the values given above for I. and III., in which the proportions are exactly reversed, but which figure under A as differences of +100 and -50 respectively. A more correct way of calculating such differences is to take the lowest (or highest) value in each pair of plots as the standard of comparison, instead of the value in the check plot, and to affix a + or - sign to the difference, according to whether the plot under treatment has given a larger or smaller value than the untreated plot. Such differences are given under B, and correctly represent the ratios of the experimental measurements. It would be well if such a method of calculating percentage differences could receive some special designation, so that it might become recognised, for without his its use is likely to lead to misunderstanding.

SPENCER PICKERING.

The Planar Arrangement of the Planetary System.

IN your issue of July 29 your reviewer devotes some space to my paper on the origin of the planetary system (*Astronomische Nachrichten*, No. 4308), and closes by asking, "Why, for instance, on the hypothesis of capture, are the vast majority of the orbits near the plane of the ecliptic and their motion direct?" This is because our system was formed by the unsymmetrical meeting of two streams of nebulosity or by the mere gravitational settling of a single nebula of curved and unsymmetrical figure, giving a rotating cosmical vortex, or spiral nebula, but without hydrostatic pressure as imagined by Laplace. In Lick Observatory Publications, vol. viii., Plate 38, you will find an illustration of H.V. 2 Virginis, a spiral nebula of unsymmetrical figure just beginning to coil up and form a system. What will happen in the later stages of this nebula is sufficiently shown in the Lick photographs of other nebulae given in this volume. As the mass whirls and condenses under resistance, it will necessarily retain and draw down most of the nebulosity into the plane of motion. This is exactly what has given the planar arrangement of the bodies in the solar system. In *Astronomische Nachrichten*, Nos. 4341-2, your reviewer will find a fuller explanation of the method of capture, and other papers yet to come will make the theory so clear that it need not take up more of your valuable space at present. T. J. J. SEE.

Naval Observatory, Mare Island, Cal., August 12.

The Benham Top.

My attention has been directed to a paper in the Transactions of the Ophthalmological Society, by Mr. A. S. Taylor, entitled "Colour Phenomena due to Intermittent Stimulation with Light: Note on the Colours of Benham's Top."

It is to the conclusions in the latter part of the paper that I desire to refer, as last year, in a paper before the Physical Society (see NATURE, June 18, 1908, p. 166), I endeavoured to explain this phenomenon in a somewhat