Colour Vision and Colour Vision Theories.

WITHOUT interfering in any current controversy, an experiment suggests itself, from the concluding sentence of Dr. Edridge-Green in NATURE of December 22, p. 900, which if carefully tried might be instructive.

I presume that no one, whatever his theory, will deny the bare fact that a spot or rectangle of yellow can be projected on a screen by the superposition of red and green. A similar spot of pure spectrum yellow can be projected alongside. The two spots can be made indistinguishable by the normal eye ; though a prism discriminates at once. So far, I know.

Let these two similar-looking spots be studied for positive after-image. Will one turn into green, because of red-fatigue, while the other will not ? I do not know the answer.   OLIVER LODGE.

SURELY it might have been better had Dr. Edridge-Green omitted the remark in his letter (NATURE, December 22) regarding the attitude of certain physicists towards physiology. But that is a small matter. I should be quite prepared, if it stood in need of defence by a physicist, which I am quite certain it does not, to defend physiology against Dr. Edridge-Green's assertion that it limits us to one set of fundamentals for normal vision for one person, unless he alludes explicitly to the *absolute* fundamentals. In that case physics and physiology are at one. But he refers explicitly to the special fundamentals, chosen for various reasons by various workers ; and of these there may be within certain limits a triply infinite choice, each framed legitimately from the single absolute set. If the existence of four fundamentals were admitted, the choice would similarly be quadruply infinite.

Dr. Troland's results, like those of Prof. Frank Allen, are of the type which will gradually pave the way towards knowledge of the variables which essentially affect the threshold values of stimuli, and towards an understanding of the corresponding relations amongst these quantities.

In the second last sentence of his letter, Dr. Edridge-Green makes statements, regarding my explanations, which puzzle me as to their origin ; and, in the last sentence, he merely repeats the error of postulation to which I objected, and pointed fully out in my last letter (NATURE, November 24).

I agree with him as to the simplicity of the trichromatic theory. That, apart from sufficiency, is its most outstanding merit. Nevertheless, for the complete elucidation of its higher developments, especially in the matter of the absolute fundamentals alluded to above, it requires some knowledge of what I may term elementary advanced mathematical physics. It is well in this connexion to remember the words of another who, like Helmholtz, stood amongst the giants, " I have no faith in speculations of these kinds unless they can be reduced to exact analysis."

Finally, I would say that, in our discussion, I have only attacked Dr. Edridge-Green's attacks upon the trichromatic theory ; for these, in my view, have been entirely undeserved.   W. PEDDIE.

University of St. Andrews,
          December 22.

Deferred Annuities (Two Rates of Interest).

SIR RICHARD REDMAYNE in NATURE of December 22, p. 893, gives, as the value of a deferred annuity in which two rates of interest are involved, $a/(1 + AR/100)$ or $a/(1 + Aj)$ where the remunerative rate of interest is R per cent. or j per unit, a is the amount of $1l.$ per annum accumulated at r per cent. or i per unit for the period of enjoyment (e), and A the amount of $1l.$ per annum at the same rate for the total period, *i.e.* period of deferment (d) *plus* period of enjoyment. The underlying idea of the two rates of interest according to Mr. George King, who is cited by Sir Richard Redmayne, is that " a lender grants an advance at a higher rate of interest than he can secure from other investments and that he wishes to realise the higher rate on the whole of the capital during the entire term of the annuity," and the formula quoted above is given in all modern text-books on interest for the limiting case, $d = 0$, that is, for an immediate annuity. The value in such cases is always lower than that obtained by using the remunerative rate throughout, which is naturally the object of the assumptions made by the lender. When however we come to a deferred annuity, we find that the values by the formula are in some cases higher than those found by using the remunerative rate alone—see Table, cols. (3) and (4), where $i = 0.025$ and $j = 0.05$. Similar results will be found with other rates of interest if i and j differ considerably.

TABLE SHOWING VALUES OF ANNUITIES.

$e.$ (1)	$d.$ (2)	$a/(1 + Aj).$ (3)	At j throughout. (4)	$a/\{(1 + aj)(1 + j)^d\}.$ (5)
5	0	4·16	4·33	4·16
5	5	3·37	3·40	3·26
5	10	2·77	2·66	2·56
10	0	7·18	7·72	7·18
10	5	5·91	6·05	5·63
10	10	4·92	4·74	4·41
15	0	9·45	10·38	9·45
15	5	7·87	8·13	7·41
15	10	6·62	6·37	5·80

I think the formula advocated by Sir Richard Redmayne and Prof. Louis is open to objection when used for deferred annuities because, while a lender will accumulate at the low accumulative rate any sums available to repay his capital, he would never agree to accumulate additions to the debt at such a rate ; yet this is what the formula seems to assume. I suggest that if P be the value required we might reach a better formula as follows :

$$P(1 + j)^d = a/(1 + aj)$$

or

$$P = a/\{(1 + aj)(1 + j)^d\}.$$

This assumes that the lender will require interest on his original investment at the remunerative rate to be added to and become part of his capital each year (or interval) and will only use the accumulative rate when he has a sum available for accumulation. The values resulting from the formula last given are shown in the Table, col. (5).

Sir Richard Redmayne is no doubt aware of objections to the formula he mentions, and it is his reference to the " vexed question of the proper formula to apply " that is my excuse for writing.
          W. PALIN ELDERTON.

December 24.

Mendelism and Evolution.

MAY I be allowed to direct the attention of readers of NATURE to an article by W. Johannsen, the distinguished Danish geneticist, entitled " Some Remarks about Units in Heredity " (*Hereditas*, vol. iv., 1923, p. 133) ? Johannsen maintains that the term unit-character should be " exterminated " : the following extracts will show the nature of his conclusions.

" Certainly by far the most comprehensive and most decisive part of the whole genotype does not