question at present before us. In the result Mr. Gulick completely agreed with me, that it cannot signify *how* or *when* the physiological variation of initial cross-infertility arises; for to whatever *causes* it may be due, and at whatever *time* in the process of varietal divergence it may first occur, it must alike furnish as highly important a condition to the origination of species as Mr. Wallace has eventually himself assigned to it.

I say "eventually," because Mr. Wallace has never before expressed himself to the effect that, in his opinion, cross-infertility is a factor of such prime importance in the origination of species. Why has he never done so? Surely the matter is one of sufficient magnitude to have justified some mention in one or other of the many valuable "contributions" which he has made to the theory of evolution. Or, not to go further than his past criticisms of my own paper on the subject, what pages of controversy he might have saved in this journal and elsewhere by stating, at any time within the last four years, that he had no disagreement with me touching the probable occurrence, and the important consequence, of some degree of infertility characterizing varieties which afterwards, and on this account, develop into species; but merely doubted whether any degree of infertility could ever arise before differentiation of some other kind had begun to take place. Such criticism would have been mild indeed. But hitherto the crown and front of opposition to the theory of physiological selection has been that, in representing cross-infertility as a factor of any great importance in the origination of species, the theory is not only untrue in itself, but tends to "shrive up natural selection to very small dimensions." Now, however, criticism "changes front." It is no longer denied, but actually upheld, that "selective fertility" is as highly important a "co-operative cause in the origination of species" as I have ever claimed; and the new attack is directed only to a very subordinate point—a point, moreover, which both Mr. Gulick and myself had expressly anticipated, fully discussed, and shown not to belong to "the essence of the theory."¹

Oxford, December 22, 1890.

GEORGE J. ROMANES.

Molecular Dispersion.

IN the notes that appeared in your journal of December 11 (p. 133), you gave a very full account of some papers lately published in the *Bulletin de la Société Chimique de Paris*, on optical dispersion, by Messrs. Barbier and Roux.

This investigation is a remarkable instance of how laborious and intelligent work may be almost wholly thrown away for want of the knowledge of what has been previously done in the same direction. The authors commence their first paper with the astounding statement: "Dispersion has never been studied from the point of view of the relations which connect this physical property of bodies with their composition, their molecular weight, and their chemical constitution." This, however, was attempted by Sir John Herschel more than half a century ago, though with little success: and most of the scientific men who have best elucidated the subject of molecular refraction, such as Mascart, Brühl, and Nasini, have paid some attention also to dispersion. Mr. Dale and I gave numerical values for dispersion equivalents, analogous to Landolt's "refraction equivalents," for CH_2 , Cl, &c., as far back as 1866. The dispersion of isomeric bodies was treated by me in 1881; and within the last few years I have communicated papers on the subject of molecular dispersion to learned Societies in England, France, and Switzerland. Some of the substances worked on by Messrs. Barbier and Roux have not, I think, been optically examined before; but the value of their careful observations is much diminished by their having measured two lines of the tin spectrum, instead of the A and H of the solar spectrum, or the α

¹ P.S.—Mr. Wallace alludes to my "standards of scientific reasoning and literary consistency." As regards the former, I am satisfied with a full and independent corroboration by a consistent and a logical mind. As regards the latter, it is enough to quote the concluding words of my reply to Mr. Wallace's first criticism of four years ago:—"The main feature of the theory is what my paper states it to be—viz. that sterility with parent forms is one of the *conditions*, and not *always* one of the *results*, of specific differentiation. But, if so, is it not evident that all causes which induce sterility are comprised by the theory, whether these causes happen to affect a few individuals sporadically, a number of individuals simultaneously, or even the majority of an entire species?" (*Nineteenth Century*, January 1887). And is it not equally evident, as elsewhere stated, that it does not signify whether the sterility arises before or after the "differentiation" has begun; seeing that, in either case, without the sterility the differentiation (as Mr. Wallace now says) will usually fail to proceed to the formation of distinct species? I have no space to discuss Mr. Darwin's views on this subject; but assuredly they are far from those which are expressed either here or in "Darwinism."

and γ of that of hydrogen; thus their results cannot easily be compared with the hundreds of measurements of dispersion already published by others. They have unfortunately employed Cauchy's formula, and have taken as the "specific dispersive power," $\frac{B}{d}$; that is, Cauchy's B divided by the density

of the substance, instead of $\frac{n_a - n_b}{d}$, the difference between the

refraction of two lines divided by the density. Brühl commenced to work in the same way, but shows in a paper in *Liebig's Annalen* for August 1886 that the method is unsatisfactory. The generalizations of our present authors are definite, and apparently correct, but they could have been mostly foreseen and explained if they had more clearly grasped the idea of molecular dispersion.

In the early summer I sent Messrs. Barbier and Roux copies of my papers on the subject; on August 1 they acknowledged receipt, and promised to refer in a forthcoming paper to previous work. As the paper reviewed in *NATURE* appeared in a preliminary abstract in the *Comptes rendus* of July, it is evident that they have yet something to bring before the public.

J. H. GLADSTONE.

17 Pembridge Square, December 27, 1890.

Weighing with a Ternary Series of Weights.

IN a former communication (*NATURE*, November 13, p. 30) I omitted, in order to avoid prolixity, to show how readily any given number may be expressed in the notation therein proposed. To effect this, it is only necessary to express the given number in the ternary scale, and then, beginning on the right, to substitute for 2, wherever it occurs, -1, whilst increasing the previous figure by 1. When 3 occurs in the application of this rule, it must of course be replaced by 0, the previous figure being again increased by 1.

Examples:—

41 is in ternary notation 1112, in new notation 1111.
500 " " " " 200112, " " 110111.
71 " " " " 2122, " " 10101.

Or (still more briefly) in dividing by 3, to express the number in the ternary scale, we may substitute for the remainder 2, wherever it occurs, -1, and increase the quotient by 1.

Examples:—

425	474	500
142 remainder - 1	158 remainder 0	167 remainder - 1
47 " - 1	53 " - 1	56 " - 1
16 " - 1	18 " - 1	19 " - 1
5 " - 1	6 " 0	6 " 0
2 " - 1	2 " 0	2 " 0
1 " - 1	1 " - 1	1 " - 1
0 " 0	0 " 0	0 " 0
<hr/>		
3 + 27 + 729	729 - (3 + 9 + 243)	27 + 729
-(1 + 9 + 81 + 243)		-(1 + 3 + 9 + 243)

Bradford, December 13, 1890.

J. WILLIS.

PROF. EVERETT's rule (*NATURE*, December 4, p. 104) is needlessly complicated. All that is necessary is to express the given weight in the ternary scale with digits that are either 0, +1, or -1. His example is thus solved:—

$$\begin{array}{r}
 3 \overline{) 500} \\
 \underline{3 \ 107} \quad -1 \\
 3 \overline{) 56} \quad -1 \\
 \underline{3 \ 19} \quad -1 \\
 3 \overline{) 6} \quad +1 \\
 \underline{3 \ 2} \quad +0 \\
 1 - 1 \quad \text{or } 500 = 110111 \text{ ternary} \\
 = 729 - 243 + 27 - 9 - 3 - 1.
 \end{array}$$

We thus see generally that, to express weights in units and powers of n , we must be provided with $\frac{1}{2}n$ or $\frac{1}{2}(n-1)$ balance-