the journals, and not a few private ones to ourselves. Of course this copious response was for the most part valueless, further than to show a general belief among fanciers and breeders in the facts of telegony, coupled, however, with great differences of opinion touching the frequency of its occurrence. Nevertheless, out of all this medley of unscientific assertion, there were a comparatively few cases where it did not appear that coincidence, pre-formed ideas, mal-observation, atavism, &c., could be reasonably assigned, and these served to indicate the most promising varieties with which to work in future experiments.

The general result of our inquiry thus far has been to corroborate the opinion with which we both started, viz. that although the fact of telegony is of very much rarer occurrence than is generally supposed, it nevertheless does appear to take place occasionally, and especially, as Mr. Herbert Spencer has recently observed, where the first offspring has been a hybrid, as distinguished from a mongrel.

On the other hand, there does not seem to be any good evidence of the phenomenon in the case of mankind. For although I have met with an alleged instance of a white woman who, after having borne children to a negro husband, had a second family to a white one, in which some negro characteristics appeared, I have not been able to meet with any corroboration of this instance. I have made inquiries among medical men in the Southern States of America, where in the days of slavery it was frequently the custom that young negresses should bear their first children to their masters, and their subsequent children to negro husbands; but it never seems to have been observed, according to my correspondents, that these subsequent children were other than pure negroes. Such, however, was not the same case as the one above mentioned, but a reciprocal case; and this may have made a difference.

So much, then, for the facts. As regards their interpretation, Mr. Herbert Spencer says, speaking on behalf of the Lamarckians, "And now, in the presence of these facts, what are we to say? Simply that they are fatal to Weismann's hypothesis. They show that there is none of the alleged independence of the reproductive cells; but that the two sets of cells are in close communion. They prove that while the reproductive cells multiply and arrange themselves during the evolution of the embryo, some of their germ-plasm passes into the mass of somatic-cells constituting the parental body, and becomes a permanent component of it. Further, they necessitate the inference that this introduced germ-plasm, everywhere diffused, is some of it included in the reproductive cells, subsequently formed. And if we thus get a demonstration that the somewhat different units of a foreign germ-plasm permeating the organism, permeate also the subsequently-formed reproductive cells, and affect the structures of the individuals arising from them, the implication is that the like happens with those native units which have been made somewhat different by modified functions: there must be a tendency to inheritance of acquired characters." (*Contemporary Review*, March.)

On the other hand, Prof. Weismann says that, even admitting the facts, they in no way militate against his theory of germ-plasm. For, as he says, "such cases could be accounted for from our point of view by supposing that spermatozoa had reached the ovary after the first sexual union had occurred, and had penetrated into certain ova, which were still immature. The immediate fertilisation of the latter is rendered inconceivable by the fact of this immaturity; but the sperm-cell must have remained in the body of the ovum until the maturation of the latter, with the nucleus of which it then united in the process of amphimixis." ("The Germ-Plasm," pp. 385-6.)

It seems to me that we have here, in principle, a sufficient answer to the Lamarckian interpretation of the facts alleged. I say "in principle," because the obvious objection that mammalian spermatozoa cannot be held capable of delving their way through the stroma of an ovary in order to reach unripe ova, may be obviated by supposing that it is the "ids" and "determinants" of disintegrated spermatozoa which do so. For, if there are any such things as ids and determinants, it is certain (from the facts of atavism) that they can survive the disintegration of their containing spermatozoon, and also that they can then penetrate somatic tissues to any extent.

But I have discussed the whole subject in a lengthy appendix to my recently published "Examination of Weismannism," to which I must refer for all details, both as regards the alleged facts and their rival interpretations. My object in raising the issues in these columns is to ascertain whether further light can be thrown upon the subject by any of your numerous readers. Therefore I will merely add that numerous experiments which during the last eighteen months I have been conducting with birds, have yielded uniformly negative results. Scores of purely bred ducks (white Aylesbury), and dozens of purely bred chickens (Polish) have been hatched; but in no one case has there been the smallest resemblance to their telegonous sires. In some cases a year, and in others only a fortnight was allowed to elapse between the successive impregnations; but in all cases the broods are as purely bred as if their respective mothers had not previously borne offspring to males of widely different breeds.

GEORGE J. ROMANES.

Christ Church, Oxford, September 16.

Quaternions and Vectors.

IN his recent letter (NATURE, August 17, p. 364), which is avowedly a reply to my paper (Proc. R.S.E., 1892-93) on "Recent Innovations in Vector Analysis," Prof. Gibbs does not seem to me to discuss the real point at issue.

At the end of that paper I summarised the arguments in favour of quaternion vector analysis under five heads.

The first of these was: "The quaternion is as fundamental a geometrical conception as any that Prof. Gibbs has named." This argument, which was a direct criticism of Prof. Gibbs's attack on quaternions in his letter to NATURE of two years ago, is not even referred to in his recent letter. It may reasonably be assumed that silence means consent.

The second summarised argument was: "In every vector analysis so far developed, the versorial character of vectors cannot be got rid of." Regarding this, which was a direct criticism of the position of Mr. Heaviside and Prof. Macfarlane, I am glad to find that Prof. Gibbs is virtually at one with me, and brings to my support the great names of Lagrange and Poisson. Now Hamilton's quaternions is admittedly the only vector calculus which takes direct cognisance and makes full consistent use of this principle, the logical consequences of which form the subject of my third and fourth summarised arguments. Thus the quaternion wins all along the line.

The fifth and last summarised argument was: "The invention of new names and new notations has added nothing of importance to what we have already learned from quaternions." This, probably, has most direct connection with Prof. Gibbs's recent letter, which is to a large extent an exposition of his own system. And interesting though this may be in itself, it does not really make out a case against quaternions; and that, be it remembered, is the point at issue. Indeed, Prof. Gibbs himself admits that the quaternion notation has a certain advantage in simplicity. This is plainly so in the case of ∇, of which in its quaternionic form Prof. Gibbs gives a very neat application in an equation whose physical interpretation is the solution of an important problem. But in this very connection, carried away by the exuberance of his humour, he seems to imagine that the *name* Nabla is of the essence of quaternions, and that the quaternionist has no right to use the word potential.

I am not aware that I anywhere expressed a dislike to the notations $[\phi]$, ϕ_s, $\phi_\times$, which represent quantities most emphatically quaternionic, or at least Hamiltonian, in their origin. What I wished to emphasise was that, in getting at the conception of the quantities ϕ_s, $\phi_\times$, Prof. Gibbs makes use of the so-called indeterminate product, which is no vector but is analytically the same kind of thing as the quaternion product, and that consequently his pamphlet and his first letter to NATURE are hardly consistent with each other.

I am accused of an inadvertence in the interpretation of certain integrals. I have not Prof. Gibbs's pamphlet by me at present, but, if I recollect aright, there is no explicit mention in it of the restriction that the operand is to be a constant vector. Nor do I see that such a restriction is necessarily implied in a system in which operators, whether under an integral sign or not, are represented symbolically apart from the operand. The operand is virtually there all the time. The equations are meaningless without it. To introduce the unexpressed operand is therefore a very different thing from the act of introducing an altogether extraneous vector. With the required restriction, however, it appears that Professor Gibbs's integral operators are not of such general applicability as had been hoped.

But even granting that I have been guilty of an inadvertence on this point, that in no way affects the general argument.

Satisfactory reasons have still to be given for deserting the quaternion highway. The asserted weakness of Hamilton's calculus, as contrasted with the implied strength of its rivals, has still to be disclosed.

With a view to bring us all to one mind, Prof. Alfred Lodge suggests (NATURE, June 29) that the quaternion be regarded as the difference of its vector and scalar parts, so that the square of a vector becomes *minus* the scalar product of a vector into itself. It is not easy to see what ultimate advantage this change of sign would bring. The most obvious disadvantage would be that it would to a large extent render Hamilton's and Tait's classical treatises of little service to the student. Moreover, it would bring in the quaternion in a very artificial manner, as a kind of after-thought, so to speak; it would, I think, confuse the beginner by forbidding him to make use of powers of vectors in the way generally familiar in analysis; it would accentuate the importance of the product at the expense of the quotient of vectors; and it would tend to obscure the significance of the versor. I am afraid it is too much to ask of any who have got accustomed to the quaternion method to introduce confusion by such a change of sign. Up to a certain point, and along certain lines, Gibbs's and Heaviside's systems lead to results identical with those obtained by quaternions. It has not been shown that they lead to these results more simply or more directly, or that they are more easily mastered by the student than is the calculus of Hamilton. And the same may be predicted of the modified quaternionic system suggested by Prof. Lodge.

Musselburgh, September 4. C. G. KNOTT.

Grassmann's "Ausdehnungslehre."

SIR ROBERT BALL asks why no one has translated the "Ausdehnungslehre" into English. The answer is as regretable as simple—it would not pay. The number of mathematicians who, after the severe courses of the universities, desire to extend their reading is very small. It is something that a respectable few seek to apply what they have already learnt. The first duty of those who direct the studies of the universities is to provide that students may leave in possession of all the best means of future investigation. That fifty years after publication the principles of the "Ausdehnungslehre" should find no place in English mathematical education is indeed astonishing. Half the time given to such a wearisome subject as Lunar Theory would place a student in possession of many of the delightful surprises of Grassmann's work, and set him thinking for himself. The "Ausdehnungslehre" has won the admiration of too many distinguished mathematicians to remain longer ignored. Clifford said of it: "I may, perhaps, be permitted to express my profound admiration of that extraordinary work, and my conviction that its principles will exercise a vast influence upon the future of mathematical science." Useful or not, the work is "a thing of beauty," and no mathematician of taste should pass it by. It is possible, nay, even likely, that its principles may be taught more simply; but the work should be preserved as a classic.

I should be glad to subscribe £10 towards the expenses of translation. If others will join, perhaps some publisher will take the matter up. Is there no machinery by which the universities could be induced to subscribe?

A good book on the subject, entitled "The Directional Calculus," by Prof. E. W. Hyde, is published by Ginn and Co., Boston; and a valuable and very clever elementary exposition, on a geometrical basis, of important parts of the Calculus, by M. Carvallo, appeared in the *Nouvelles Annales de Mathématiques* of January, 1892. The latter will, in one day, enable a student to comprehend the power and elegance of Grassmann's methods. R. W. GENESE.

Astronomical Photography.

THE nature of chromatic correction adopted for visual telescopes is uniform enough to make it possible to state what kind of photographic plate is desired for use with such telescopes.

A plate which is sensitive to light between C and F in the solar spectrum, with a marked maximum between D and *b*, and insensitive to other light, would be suitable for nearly all visual telescopes, which might in other respects (*e.g.* aperture, focal length, position as affected by climate) be available for taking special photographic records. With existing plates, so far as I have been able to acquaint myself with them, the sensitiveness in the blue and violet is the difficulty.

But whilst such a special plate as I describe would be warmly welcomed, we must not forget that the proved goodness of the photographic star-images of what may be called violet refractors, *i.e.* refractors corrected so that the minimum focus is for violet light, is in great measure to be attributed to the fact that light of short wave length is used. The increase in the diameter of star-images with increased exposures or great brightness of the star, may be, as Scheiner has lately suggested, due to defects in the mode of support of the object-glass or mirror, but doubtless the *goodness* of the images with proper exposures must be connected with the smallness of the scale of the diffraction pattern, and with the concentration of light to the centre of the pattern, which may be got at smaller expense with a violet refractor than with a visual.

Probably few astronomers would have been bold enough, if no photographic plates had been available except plates sensitive only to yellow and green, to urge the preparation of plates sensitive in the violet, on the ground that a violet refractor would give much better results, because short wave lengths were used. And yet a comparison of the results obtained with violet refractors and with reflectors would lead one to the view above expressed, and, I believe, generally accepted.

The increased *range* of sensitiveness of modern photographic plates, with respect not only to the colour, but also to the intensity of the light affecting them, is all in favour of the reflector. A greater and more desirable advance than even the preparation of plates to suit visual telescopes would, I think, be made if the difficulties of supporting, adjusting, and maintaining a mirror were overcome; so that the measurement of star-images may be regarded with as much confidence in the case of plates exposed in reflectors as in refractors. H. F. NEWALL.

Madingley Rise, Cambridge, September 25.

Hering's Theory of Colour Vision.

I AM very much surprised to see that Prof. Ebbinghaus, in the last number of the *Zeitschrift für Psychologie*, announces as new a discovery which has a critical bearing upon Hering's theory of colour-vision—the fact, namely, that two grays composed the one of blue and yellow, and the other of red and green, and made equally bright at one illumination (by admixture of black with whichever of them turns out to be the brighter), do not continue to be equally bright at a different illumination. If two complementary colours were purely antagonistic—that is, if the colour-processes simply destroyed each other, as processes of assimilation and dissimilation must do, and if the resulting white was solely due to the residual white which accompanies every colour and gives it its brightness, then the relative brightness of two grays composed out of different parts of the spectrum could not change with change of illumination. The fact that they do change is therefore completely subversive of the theory of Hering, or of any other theory in which the complementary colour-processes are of a nature to annihilate each other. This consequence of the fact, as well as the fact itself, I stated at the Congress of Psychologists in London in August, 1892, and it was printed in the abstract of my paper, which was distributed at the time, and also in the Proceedings of the Congress.

Prof. Ebbinghaus' discovery is apparently independent of mine, for he supposes that the phenomenon cannot be exhibited upon the colour-wheel. This is not the case; with fittingly-chosen papers (that is, with a red and green which need no addition of blue or yellow to make a pure gray, and with a corresponding blue and yellow) it is perfectly evident upon the colour-wheel. The same paper circles which I used to demonstrate it in Prof. König's laboratory in Berlin are, at the request of Prof. Jastrow, now on exhibition at the World's Fair at Chicago. While Prof. Ebbinghaus' discovery of the fact is therefore doubtless independent of mine, I allow myself to point out that mine is prior to his in point of time.

Baltimore. CHRISTINE LADD FRANKLIN.

"Megamicros."

IN NATURE of August 24 the following extract from the *Bulletin de l'Académie de Belgique*, No. 6 (1893), is given, viz.:—

"According to Laplace, if the dimensions of all the bodies of the universe, their mutual distances and velocities were to increase or diminish in a constant proportion, these bodies