

Letters to the Editor.

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Some Problems in Evolution.

SINCE I am not, in the ordinary meaning, a biologist, I have sometimes difficulty in understanding biological language. Doubtless, also, I am often ignorant of recent developments in knowledge and thought. But certain problems of disease and education interest me, and I cannot get on with them unless some points, essentially biological, are cleared up. In the hope of enlightenment I wrote to NATURE. Immediately the discussion became acrimonious: at least, I became acrimonious. I was told, in effect, that I had no business in the august deliberations of biologists. It is not in human nature, or my variety of it, to accept that pontifical attitude. However, there seems now some prospect of the desired lucidity, and I shall be very ready to accept it with an humble and a contrite heart.

I fear, however, that Dr. Cunningham's letter in NATURE of January 12 does not greatly help. He writes:—"Sir Archdall Reid argues, as though it were a remarkable discovery, that characters are not present as such in the fertilised ovum from which an organism develops." But is that quite fair? I argued only that if, as all biologists are aware, no characters as such are present in the germ, then it must follow that, in the case of any and every character, nothing but germinal potentiality (predisposition, diathesis, capacity, ability) to produce (in response to fitting nurture) can be transmitted; whence it follows further that all characters are alike as regards innateness, acquiredness, and inheritability; whence, again, it follows that if we classify characters with respect to these qualities, there is, as Prof. Goodrich says, only one kind of character. On the other hand, as all biologists know (I protest I do not claim this as a new discovery), there are two kinds of variations: (a) those which result from germinal, and (b) those which result from nurtural, differences.

Of course, we can classify characters in all sorts of ways, useful and useless—according to colour, weight, size, shape, obviousness of recapitulation, frequency of reproduction, and so on. In a classification which physiologists have found useful, characters are ranked according to the influences which cause them to develop. This tabulation has the merit of forcing the inquirer to bear in mind the plain truth that frequency of reproduction depends (except when germinal variations occur) altogether on the frequency with which fitting natures are experienced, and not at all on the frequency of inheritance. For example, under this scheme of classification the inquirer bears in mind that rose comb and single comb in poultry are not more inheritable than corns on oarsmen's hands, but that they are more frequently reproduced only because the proper nurture is more frequently experienced. With respect to inheritance, his mind is fixed on the nature of the individual (the germ-plasm); with respect to reproduction, on the nurture received. Moreover, the student is compelled to realise that when he transfers the distinguishing terms "innate," "acquired," and "inheritable" from likenesses and differences between individuals to the characters in which those likenesses and differences are revealed, he has shifted his ground. It is one thing to compare separate individuals, and quite

another thing to compare characters which may occur in the same individual. The old terms may still be applicable; but that is the question which has been raised. It will be gathered that they do not seem applicable to me, and that their constant and (to me) inexplicable transference is one of the causes of my puzzlement. It may be noted also that Darwin, in all that remains permanent of his work, used these terms in relation to variations, while Lamarck and Weismann applied them especially to characters. I may be mistaken, but I believe that I am right when I say that no one (including Darwin) has ever doubted the all-sufficiency of natural selection unless he has, in his thinking, transferred the terms "innate," "acquired," and "inheritable" from variations to characters, or has confused inheritance with reproduction.

Again, we may employ our words with unusual meanings and reason on that basis. Thus "inherit" may be used in the sense of "reproduce," when, of course, the "intensity of inheritance" of combs is infinitely greater than that of corns. But now we are asking for trouble and in sight of confusion. We are in danger of using as counters in thought and discussion, not realities in nature, but mere words. Our inquiries, notwithstanding our language, relate not to the natures of individuals, but to their natures. Does or does not the impure dominant inherit the recessive trait which it does not reproduce? Does the pure extracted recessive which is unlike its parent inherit nothing? When a pigeon or a fowl belonging to a fancy breed reproduces the wild ancestral coloration, from whom does it inherit? From an exceedingly remote ancestor? It passes my non-biological comprehension to understand how an individual can inherit except through, and therefore from, his parent. In practice the difficulty is surmounted by using "inherit" with the usual, or with the unusual, meaning as exigencies of argument dictate. For example, Dr. Cunningham employs the word with the ordinary meaning when he declares "a character may be inherited when it is apparent only in one parent or in neither," and with the unusual meaning when he insists that combs are more inheritable than corns.

Consider the Lamarckian dictum: "Acquired as well as innate characters are inheritable"; and the neo-Darwinian: "Innate, but not acquired, characters are inheritable." What do "innate" and "acquired" mean here? No one can tell. Definitions are impossible, for none can be framed which cover the whole of common and accepted usage. What does "inherit" mean? When applied to "innate" characters it may have, as already indicated, its ordinary meaning, or it may mean "reproduce." If a cock reproduced a comb under the same conditions as those in which its parent produced it (in response to similar nurture) all biologists would regard the comb as inherited—and rightly, for reproduction under the same conditions implies inheritance, though inheritance does not necessarily imply reproduction. The case is different with respect to "acquired" characters. If a child reproduced an oarsman's corn under the same conditions as the parent produced it, few biologists would regard the corn as inherited. It would be regarded as inherited only if the child developed it under conditions in which the parent did not and could not have developed it. The word now means "vary," i.e. non-inherit, for non-inheritance is variation. It seems, then, that an acquired character is not inherited when it is inherited, and is inherited when it is not inherited—i.e. a single word in a single sentence has two con-

trary meanings. Biologists say they understand one another, and therefore I suppose they do; but I wish, in pity, they would enlighten me. Why do Lamarckians and neo-Darwinians say "inherit" when they mean "vary"? Why do Mendelians and biometricians say "inherit" when they mean "reproduce"? Meanwhile, I cannot help suspecting that something is wrong. Consider what has happened—Lamarck's theory and half a century of stasis; Darwin's brilliant lucidity and twenty years of progress, with biology in its splendour, a great intellectual force; Weismann's effort, and nearly half a century of controversy, with interest in the subject limited to some (not all) zoologists and botanists, and of these few a majority resentful of trespassers.

I propose in two or three letters to adopt the physiological classification when dealing with three or four biological subjects. Biologists, I hope, will be tolerant towards one who uses this classification because, admittedly, he does not understand the difficult language they speak.

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9 Victoria Road South, Southsea, January 16.

Atmospheric Refraction.

DR. BALL is surely wrong in suggesting in NATURE of January 5, p. 8, that the difference between Mr. Mallock's figure for the radius of curvature of a nearly horizontal ray and that given by Dr. de Graaff Hunter is accounted for by any consideration of the curvature of the wave-front. If such were the case, then an observer looking towards the sea horizon would see a ray of light in different directions for different initial curvatures of the wave-front. Suppose an observer from the bridge of a ship were looking at a searchlight placed at sea-level at the extreme limit of visibility. The rays of the searchlight beam would be plane waves, those coming from the barrel of the searchlight spherical. Does Dr. Ball wish us to infer that in such circumstances the visible beam would appear to the observer to issue from a point *above* the projector?—for that is what his suggestion leads to.

To my mind, a great deal of the confusion between refraction figures given by different authorities lies in their attempt to connect refraction with variations of temperature before they have properly considered the subject from the point of view of variations in refractive index. If we assume that, over the sea at all events, the refractive index stratification is one which is spherical and concentric with the earth, then the general equation of any ray of light is

$$pn = \text{constant},$$

where n is the refractive index and p the perpendicular upon the tangent to the ray from the earth's centre (see Herman, "Geometrical Optics," p. 305, or Heath, "Geometrical Optics," p. 329).

If r is the distance of any point upon the ray from the earth's centre, h the height of the point above the earth's surface, and R the earth's radius, then $r = R + h$.

Now n must be some function of the height $= f(h) = f(r - R)$, and hence the " p, r " equation of the ray is

$$pf(r - R) = \text{constant} = C.$$

The radius of curvature of the ray is thus

$$\sigma = r \frac{dr}{dp} = - \frac{r^2 f'(r-R)^{1/2}}{C} \frac{df}{dr},$$

or

$$= - \frac{rn^2}{C} \frac{dn}{dh}.$$

As we are dealing with a ray which is nearly horizontal.

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zontal, variations in r and n^2 cannot have large effects upon σ . The variations in r might amount to 1 part in 200,000 if the ray never gets above 100 ft. above the surface of the sea; the refractive index, which at the sea-level is 1.00029, could scarcely be reduced below 1.00027 in the same height, so that variations in n^2 could not exceed 4 parts in 100,000. It follows that the curvature of such rays is essentially proportional to the refractive index gradient. Since by Dale and Gladstone's law $n - 1$ is proportional to ρ , the density, the curvature of the ray-path becomes immediately proportional to the density-gradient. If we attempt to translate density-gradient into temperature-gradient, I see no means of doing so other than by making the assumption that the atmosphere is statically in equilibrium, in which case the formulæ given in my letter in NATURE of January 5 result immediately. But I have the gravest doubts of the legitimacy of such an assumption for the lower levels of the air. A steady motion leading to a dynamical relationship between pressure, density, and temperature is much more likely, but is, from the mathematician's point of view, a hopeless thing to try to set down owing to the impossibility of dealing with all the factors of the problem, such as rate of radiation of heat-energy from the earth or sea, rate of thermal conduction in the air, nature of the upward air-currents, and so on.

If however, we leave all such considerations aside and deal only with the established connections between curvature of the ray-path and the density-gradient, then we can only admit uniform curvature if we are prepared to admit that the density of the air in its lower levels is a linear function of the height. To such an admission I take the strongest exception. It is quite insufficient to account for a refraction of the visible sea horizon above the true horizontal—a phenomenon which, as every seaman knows, is by no means uncommon.

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Admiralty Research Laboratory, Teddington, Middlesex, January 7.

The Colours of Tempered Steel.

THE well-known and characteristic tints that appear on the surface of a tarnishable metal when it is heated in contact with air have been usually regarded as interference colours due to the formation of a thin film of oxide on the surface of the metal. The correctness of this explanation has, however, recently been questioned (A. Mallock, Proc. Roy. Soc., 1918), and rightly so, as a continuous film on a strongly reflected surface cannot on optical principles be expected to exhibit such vivid colours as those observed.

I have recently made some observations which shed a new light on this subject. It is found that the *missing colours* complementary to the tints seen by reflected light appear as light *scattered* or *diffracted* from the surface of the metal. In other words, if a plate of blue-tempered steel be held in a beam of light and viewed in such a direction that the regularly reflected light does not reach the eye, the metal shows a straw-yellow colour, and not the usual blue. It will be understood that the scattered light, being distributed over a large solid angle, appears much feebler than the regularly reflected colour, and in order to observe the effect satisfactorily the metal should have a smoothly polished surface before being heated up. Scratches and other irregularities show the ordinary colour of the film, and not the complementary tint. The most attractive effects are those exhibited by a heated copper plate, both on account of the vividness