Thus we may say, in very young Ammocœtes the parietal eye possesses black pigment, in older Ammocœtes white pigment, and in adult Petromyzon there is a reversion to black pigment. In what relationship these three pigments stand to each other I am unable to say.

The last point concerns the hypotheses as to the origin of the eye. These were really two in number. The first of them—that which derives the paired eyes and the parietal eye from one common dorsal sense-plate—I hold to be fairly certain, and, indeed, there are many facts to support it.

The second, which derives the parietal eye as a later involution of a portion of this same plate, an involution which was supposed to have taken place after that of the paired eye "Anlage," I only believe to be conceivable. My hope of establishing it lay in the verification of an observation of Goette's; there are no facts to support it, and from more recent investigations of the development I am disposed to attach less value to it. For, from these developmental researches, from studies of the types of eye presented by vertebrates and some invertebrates, and lastly, but not least, from valuable discussion with and criticism by Prof. Wiedersheim, a new track has been found, which gives the explanation of a good deal, but the problem is too long and complicated for treatment here.

The first hypothesis mentioned above is taken as the starting-point, but for the further details there are several other questions which have first to be solved. J. BEARD.

Anatomisches Institut, Freiburg i/Br., July 20.

### Physiological Selection.

LIKE so many others who have written on this subject, Mr. Rusden freely criticises my views without having deemed it desirable to read my paper. Had he taken the trouble to do so, he would have found a sufficient recognition of the general fact that instinctive habits not unfrequently serve to mitigate the swamping effects on incipient varieties of intercrossing with their parent forms. Moreover, he would have found that there are others of these habits mentioned by me which are probably much more effectual in this respect than is the one to which he draws attention. Nevertheless, it appears to me evident that all these habits taken together cannot count for much, even where they occur; while it is unquestionable that they occur only in a very small fractional part of organic nature considered as a whole—namely, in some among the more intelligent species of animals. The whole of the vegetable kingdom, an immense majority of the Invertebrata, and a considerable majority of the Vertebrata, cannot possibly have had any of their specific differentiations influenced by any of these forms of what I have already designated as "psychological selection." This sufficiently obvious consideration appears to have entirely escaped Mr. Rusden. He adduces a well-known and a comparatively limited form of psychological selection as a "simple solution" of the difficulty from free intercrossing in all cases!

The other parts of his letter merely indorse the views which are published in my paper. I there say that the theory of natural selection is not, strictly speaking, a theory of the origin of species, but a theory of the development of adaptations. Having read this statement, your correspondent writes:—"To consider the theory of natural selection as a theory of the origin of species is, therefore, clearly an error. . . . The theory of natural selection is one, not of the origin of species at all, but of the preservation of particular varieties," *i.e.* those which present an adaptive character. I do not see how his agreement with my views in this matter could be more clearly expressed, and therefore I cannot understand why he supposes that he is here criticising anything which I have written. If the point of his criticism is that I imagine Mr. Darwin to have fallen into the error of regarding the theory of natural selection as (primarily) a theory of the origin of species, this would merely show again that he has not read my paper. My contention from the first has been that upon this point I am in full agreement with Mr. Darwin, and differ only from those Darwinians who differ from their master in holding that *all* specific changes are likewise adaptive changes, and *vice versâ*. It is only in the presence of this non-Darwinian assumption that specific changes and adaptive changes become synonymous terms, with the consequence that the theory of natural selection is to be regarded as in all cases the only theory of the origin of species.

And this leads me to the last point in my critic's letter. I have argued that the above-mentioned non-Darwinian assumption is opposed to observable fact, seeing that "in a large proportional number of cases" specific characters appear to be wholly useless. Nothing has surprised me so much on the part of my critics as to have found this statement vehemently challenged by so accomplished a naturalist as Mr. Wallace, and therefore I am now engaged in collecting a quantity of evidence upon the subject. But the point here is that Mr. Rusden appears to think there is some ambiguity attaching to the terms "use" and "utility." For he asks whether these words have "any real significance outside human interests and considerations." Now, I can scarcely understand how anyone at this time of day could suppose that when these words are employed in their Darwinian sense they are intended to have any reference to human interests. When an evolutionist speaks of the utility of an organ, it is hardly conceivable that anyone should understand him to mean anything else than the utility of that organ to the species which presents it. Therefore, the term "utility" is equivalent to the term "adaptation," and to say that any organ or structure is of use is one and the same thing as to say that it is adapted to the performance of a function which is of benefit to the organism or to its species. Such, at any rate, is the only sense in which I have myself employed these words; and in doing so I have, of course, followed the terminology of Mr. Darwin, as my critic might have observed without going beyond one of the quotations which he himself makes from the "Origin of Species"—namely, "I have called this principle by which each slight variation, if useful, is preserved by the term 'natural selection.'"

GEORGE J. ROMANES.

Geanies, Ross-shire, N.B., July 29.

### The Droseras.

MISS ANNE PRATT in her "Wild Flowers," vol. ii. p. 155, in describing the three British species, after stating the character of the stems and flowers, remarks, "but many persons who know the plant well have never seen the flowers fully open." Two of the species, *D. rotundifolia* and *D. longifolia*, are found in a bog on a common near here, and these have lately flowered in captivity. They were transferred from their habitat and placed in a large saucer with peat and Sphagnum, under a bell glass. The flowers have expanded from 10 a.m. to noon each day, after which the sun left them. A *D. longifolia* in another position was seen to flower at 2 p.m. Moisture and sun seem the conditions to bring out the blossoms. I am not aware whether they have flowered *in situ*, as my plants were gathered in the early morning.

*Ramondia pyrenaica*, brought from Bagnères de Luchon ten years ago, has flowered each year on an outside rockery in my garden. J. RAND CAPRON.

Guildown, Guildford, July 28.

### Comrades.

MY children and their governess, when staying in the north of Ireland lately, witnessed the following curious display of feeling, in animals not usually credited with feelings. A boar pig was in the habit every morning of going to the basket where a blind kitten of about six weeks old was kept, allowing the little thing to creep on to his back, and then taking it about and caring for it during the day. The kitten got its food at the same time as the pig, and at the same trough. In the evening the man who saw to the animals used to carry the kitten back to its basket to pass the night. "Où donc la vertu va-t-elle se nicher?"

Pollokshields, Glasgow, August 1. E. R.

## *A NEW COSMOGONY.*[1]

### II.

DR. BRAUN has earned by his excellent series of observations on sunspots (NATURE, vol. xxxv. p. 227) a title to be heard with particular respect on subjects connected with solar physics. In unfolding his views

[1] "Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.) Continued from p. 323.

regarding them in the three concluding sections of his work on Cosmogony, he by no means underrates the difficulties they present. The range of our sensible experience shrinks into absolute insignificance when compared with the exalted conditions reigning in the sun. The temperature at its surface may well reach 40,000° to 100,000° C.; near the centre it mounts probably (our author considers) to ten, possibly to thirty or more million degrees. This unimaginable vehemence of heat is balanced by an unimaginable urgency of pressure. The statement that, in the depths of the sun's interior, it reaches a maximum of 2,000,000,000 atmospheres gives only nominal expression to its value. Figures can at times keep pace with facts only on the condition of being reduced to empty and meaningless symbols.

Gravity and molecular motion—the two universal antagonists—here carry on a conflict intensified far beyond the control of "laws" derived from terrestrial observation. The correlation between elasticity on the one side, and pressure and temperature severally on the other, established by Boyle and Gay-Lussac, holds good only over a strictly limited range of conditions. Calculations founded on the supposition of its continued prevalence in the sun lead at once to manifest incongruities. Solar speculators are thus left, to a great extent, without the guidance of ascertained principles. In the region frequented by them, the scientific imagination has free play. Apposite facts are scarce; misleading analogies too much abound. It cannot then be wondered at if theories of the sun often include extravagances which it is easier for their critics to discern than for their constructors to avoid.

A futile debate has sometimes been raised as to whether the interior constitution of the sun is liquid or gaseous. The truth seems to be that neither word is properly applicable. Without unduly stretching its original meaning, neither describes with even approximate accuracy the state of things prevailing there. The notion of "critical points" has been called in question, and may be inexact. But its introduction has at least had the not unimportant effect of abolishing an artificial distinction. It has shown the separation of the various states of matter to be merely provisional. Their characteristic qualities depend upon circumstances for their development or maintenance. At transcendental temperatures and pressures, the ordinary—probably (as Dr. Braun remarks) even the scientific—criteria of gases and liquids disappear; one state merges into the other; they interchange natures; so that we may indifferently regard the sun's interior as composed of vapours compressed, in despite of their almost boundless calorific energy, to the consistence of fresh putty, or of liquids restrained from boiling by the main force of the strata loaded upon them, while expanded to four or five times their ordinary bulk, and rendered internally mobile by the prodigious elevation of the temperature. An indisputable fact, however, and one fundamental to solar physical theory, is that the sun constitutes a vast reservoir of opposing, tremendously-constrained forces, the delicate equilibrium of which cannot be disturbed, however slightly, without producing effects on a commensurate scale.

Upon such inevitable disturbances Dr. Braun founds his *rationale* of the more obvious solar phenomena. The cooling of a body like the sun does not assuredly proceed quite equably. Local excesses of temperature lead to what we may call local revolts against gravity, signified by swift uprushes from great depths of inconceivably heated substances. These are the so-called "metallic prominences." But where the forces called into play lack energy to produce, or the attendant circumstances are not sufficiently favourable to permit, an actual outbreak, an uplifting of the unbroken photospheric surface takes place, and we see a "facula." "Hydrogen-prominences" mark a medium stage of vehemence. They originate from a commotion which primarily fails to outpass the limits of the chromosphere. The injection, however, into it of a prodigious bulk of metallic vapours rapidly heats the circumjacent hydrogen; it spouts upward in a stream which aërostatic pressure tends to perpetuate, and forms, high up above the sierra-edge of the agitated ocean it springs from, a rosy cloud conspicuous by reason of its incandescence.

But the connexion here indicated, to be significant, should be invariable, which is very far from being the case. Metallic intrusions into the chromosphere are by no means a condition *sine quâ non* to the development of quiescent prominences.

Solar theorists are now for the most part agreed that spots must be ascribed *immediately* to falls of relatively cool matter upon the photosphere; they divide on the question whether the initial disturbance comes from beneath or above it. Dr. Braun ranges himself on the side of those who assimilate outbursts on the sun to volcanic commotions on the earth. Uprushes of vividly glowing substances due to the temporary preponderance of heat over pressure are answered by downrushes of obscure absorbing vapours. Spots would thus be the reactive effects of flames or prominences. Their occurrence would be impossible without preliminary eruptions. But it is at least doubtful whether in this hypothesis the real sequence of events be not inverted. The whole tenor of Mr. Lockyer's observations goes to prove that the yawning of the photosphere leads the way as a symptom of its agitation. After a spot has begun to form, its flame and facular garnishings are added. M. Trouvelot has, indeed, often perceived a nascent spot to be completely masked by towering masses of faculæ; but it is none the less there, waiting to be disclosed. Prof. Young considers the appearance of a spot to be commonly heralded by manifest disturbances of the surface; but since the disturbance is evidenced as well by the presence of "pores" (which may be termed embryo spots) as of faculæ, his authority can scarcely be invoked as decisive of the question of precedence.

This is really the touchstone of the rival theories. Outbursts from the photosphere are either the cause or the consequence of the obscurations of it termed "spots." If the former, they should unfailingly and unmistakably take the initiative. But facts certainly warrant no such rigid conclusion. Admitting then the alternative order of connexion, we can understand that descents of relatively cool matter from coronal regions, perforating the photosphere, must overturn the precarious equilibrium of heat and gravity reigning beneath it, and may thus occasion the tumultuous heavings visible as faculæ, and the amazing escapes of imprisoned vapours challenging attention as flames.

Dr. Braun's papers on the constitution of the sun were published in *Natur und Offenbarung* previous to the appearance of Mr. Lockyer's "Chemistry of the Sun." Hence, perhaps, his complaint that the observed facts regarding the solar rotation had as yet been included in no "plausible" hypothesis. We cannot think him successful in his effort to supply the want.

Adventitious arrivals of nebular supplies from interstellar space play, as our readers are already aware, an indispensable part in the theory of planetary development sketched in the earlier chapters of the work now, in its concluding portion, engaging our attention. By their agency the primitive nebula was set whirling with a motion accelerated outward, its central sluggishness persisting throughout, and modifying the whole of its long history The inequality is perpetuated within the body of the sun itself, the innermost parts of which may require, our author thinks, as much as forty or fifty days to complete a rotation performed at the equatorial surface in twenty-five. The quickening of angular rate continues with ascent into the solar atmosphere, until, in its higher

regions, the period is reduced to ten, if not to five or fewer days. All this, we are asked to believe, is the work of the latest of our nebular annexations, which, forming an equatorial girdle round the sun, partially, and in a degree varying inversely with latitude, communicated its own more rapid movement to the superficial layers of the globe it encompassed.

The process is not even yet concluded. What Dr. Braun holds to be indisputable proof of atmospheric acceleration is derived from Prof. Young's spectroscopic measurement, in 1876, of the sun's rotational velocity. But this is to lay upon the observations in question a burden of inference heavier than they will bear. The rate of equatorial movement, as computed from the observed translation of spots, is 1·25 miles a second; it came out 1·42 miles from the Dartmouth College measures. Considering, however, the extreme minuteness of the entire displacements due to this speed, amounting, for the D lines, to but $\frac{1}{77}$ of the interval between them, the discrepancy is hardly surprising; and it is well known that, in this particular class of determinations, errors lie almost always on the side of excess. Prof. Young himself, it is true, was "inclined to think" that his result betrayed an actual sweeping forward of the absorbing layers over the underlying surface; but even were the fact established, we should expect to find for it a cause less remote than the inrush of a nebula uncounted millions of years ago. Undoubtedly, however (so far we are in agreement with Dr. Braun), that cause would be found to be closely connected with the anomalies of the sun's rotation.

As regards the distribution and periodicity of spots, we are in the present work offered simple and avowed conjectures at which we need only glance in passing. The nebulous swathing not yet completely incorporated with the sun's mass impedes, and has during past ages still more effectually impeded, equatorial radiation. Hence, cooling has, in polar tracts, penetrated further into the interior, with the result of generating an internal spheroidal surface at which temperature-gradients attain a maximum, and from the middle latitudes of which special facilities are afforded for eruptive outbreaks.

But this device is assuredly not a practicable one. The highly artificial arrangement it establishes could not endure one hour. Convection-currents would speedily and without ceremony abolish it. Indeed, augmented radiation from near the poles (which is equivalent to more rapid cooling), besides being contradicted by observation, might be expected to produce just the opposite effect of intensifying the disturbances attendant on cooling. Spots and flames should then, on the hypothesis advanced, be transferred from their "royal" zones to the polar calottes.

Heat-pulsations in a period of $11\frac{1}{9}$ years, occasioned, perhaps, by a slow mechanical oscillation of the sun's volume, the progressive contraction of which may be conducted rhythmically, or by regular alternations of shrinking and swelling, are invoked (certainly under every reserve) to solve the puzzle of the sunspot cycle. The difficulties attending what might be called the "disturbed thermal equilibrium" theory of solar phenomena could not be more forcibly illustrated than by the straits to which it reduces its advocates.

The study of coronal appearances compels our author to take refuge in the unassailable stronghold of electricity. We are far from asserting that he is not fully justified in this measure: the circumstances indeed seem to prescribe it; yet it is always felt to be a desperate one, for the reason that it lands us, almost completely, in the region of the unknown. It is right to add that Dr. Braun is at all times evidently loth to separate from the company of ascertained facts and laws. He advances without them only where their escort cannot be made available.

A. M. CLERKE.

## MUSIC IN NATURE.

IN the February number of *Longman's Magazine*, there is a remarkable article "On Melody in Speech," by Mr. F. Weber, Resident Organist of the German Chapel Royal, St. James's Palace. The object of the writer is more comprehensive than his title expresses, for he says in his opening paragraph, "There is an infinite variety of interesting and pleasing sounds in Nature's music around us that may be noted by an attentive ear." This may be readily granted; but Mr. Weber goes on, "These sounds are mostly melodious and harmonious, or *in some harmonious connexion, and form exact intervals and chords.*"

This last sentence is the point of the article. Mr. Weber is not expressing himself figuratively: he writes as a musician, and he distinctly asserts that many of the sounds spontaneously produced in Nature are truly MUSIC in the musician's, and not the poet's, sense of the term. To illustrate this assertion he has taken the trouble to identify and write down, in actual musical notes, the musical passages which he considers he has recognized in a great variety of these natural sounds, and so has challenged the public judgment on the accuracy of his theory.

Now, Mr. Weber is a gentleman of eminence in his profession, and what he says deserves attention. It is easy to say that he has given his imagination too much play in his supposed identifications; but it seems to me the subject ought to be approached from a more comprehensive point of view. The question is, Do such sounds or series of sounds constitute music? or do they not? And if not, why not? If Mr. Weber is wrong, it is probably because he has formed too hasty a view of what music really is; and this is a point that requires serious discussion.

Mr. Weber is not the first who has had this idea. Half a century ago, Gardiner, of Leicester, also a clever musician, published a book called, I think, the "Music of Nature," in which he wrote down musical passages professedly representing a vast number of natural sequences of sounds. There are many other persons, who, while they would not go to the same length as Mr. Weber or Gardiner, still believe that music may be found in the sounds of Nature, and it is worth while to see what grounds there are for such a belief.

Music, in its modern form, is a very complicated structure, combining many elements, such as melody, harmony, counterpoint, tonality, measured time, rhythm, form, expression, tone-colour, and so on. But no one will suppose that the combination of all these is necessary to make what may be strictly called music. We must begin at the other end, and ask what music is if reduced to its simplest possible form? What are the fewest and least conditions absolutely necessary to constitute music, *i.e.* to give the name of music to a combination of sounds?

In the first place, we must have the proper material, namely *musical sounds*, and we must be particular that the sounds are really of a musical character. I am not going into acoustics. I need only say that the most essential quality necessary to give a sound this character is that it must have a *fixed and definite pitch.* A sound that is wavering and indefinite, like the sighing of the wind, or the *portamento* of a voice or violin, though it may be loosely said to be musical, is not strictly a "musical sound." It cannot be defined by the number of its vibrations, it cannot be expressed in any musical notation, and it cannot be used to form musical structure. For this purpose a sound, though it may be short, must be perfectly definite.

Now, suppose we have a sound of this kind, producing say this note  . Does the sounding of this note of itself constitute music? We must say No; for the