represent the blue ray that suffers most refraction of all the blue rays, and C E A the red ray which suffers most refraction of all the red rays.

It thus appears that of blue and red rays reflected from the sea at the same angle, the former may reach the eye of the observer and the latter not, because, though the refraction is sufficient for the blue it is not so for the red ray, and it will be lost in the upper air. Consequently the blue rays will appear highest, and the red lowest, the other colours occupying intermediate positions according to their refrangibility. It is evident that any of these rays may be reflected too vertically from the sea, and so not be refracted to the earth again, but a considerable proportion will be thus refracted, and as has been said, more vertically inclined rays of the blue than of any other colour.

When we consider the effect of rays falling to the left of C, the phenomenon becomes more complicated. The same refraction, dispersion, and reflection take place, but the rays after reflection will mostly fall short of A, and strike the sea at various angles, producing a great variety of colour. It is not necessary for the effect that both the blue and the red from the same pencil of light should proceed to A, although this is shown for the sake of simplicity in the figure. It is sufficient if we know that blue rays, on account of their greater refrangibility, must of necessity be the highest, and the red, on account of their least refrangibility, the lowest.

If the above suggestion as to the dispersive power of the atmosphere be admitted, it is probable that the question of the colour and scintillation of stars will be directly affected by it.

Little Bromley, Manningtree, July 12 R. ABBAY

Zoological Geography—Didus and Didunculus

MR. SEARLES V. WOOD will, I trust, pardon me if I again take exception to the terms in which (*suprà*, p. 301) he still writes of *Didus* and *Didunculus*. These two birds do not belong to the same group of *Columbæ*. The fact that certain authors may have included them under the designation of "ground-doves" is no proof whatever of their relationship, any more than it is of the relationship of either to any other birds so called—for instance those of the Neotropical genus *Chamæpelia*. I have studied pretty carefully the osteology of many forms of *Columbæ* with especial reference to their affinities. *Pezophaps* and *Didus* are of course nearly allied, though even these are not congeners. *Didunculus* is at least as distinct from them as from all other *Columbæ* with the possible exception of *Otidiphaps*, which last I have not had an opportunity of examining. Furthermore, I may remark that if Mr. Wood will but look at what has been published of the habits of *Didunculus* he will find that it is as much an arboreal as a terrestrial bird, so that the name of "ground-dove" is as unhappily applied to it as is that of *Didunculus* or its ridiculous translation, "Dodlet."

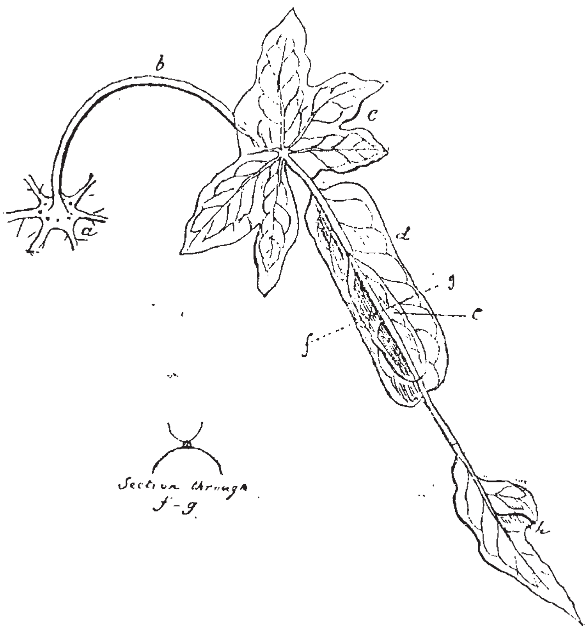
July 22 ALFRED NEWTON

Autophyllogeny

THE following case of *Autophyllogeny*, observed in a leaf of *Papaya vulgaris* (the well-known papaw-tree) appears to me of sufficient interest to be recorded in the columns of your highly interesting journal.

The letter *a* designates the central part of the primary leaf, corresponding to the apex of the petiole on the upper side of the blade. It shows some small warty protuberances, and from amidst them rises a new petiole (*b*), about six centimetres long and one and a half millimetre thick. This new petiole bears an accessory leaf of somewhat pentagonal outline (*c*), slightly crumpled and partially concave towards the upper side (the one directed downwards in the figure), as if there had been some tendency of forming a leaf pitcher. A little onwards two boat-shaped appendices are observed (*d* and *e*), the midrib or petiole forming their keel. They are real leaf pitchers, though of a rather uncommon form. The small lateral diagram represents the shape of the transversal section through *f* and *g*. The two leaves are opposed to each other by their upper sides, which are of a dark green colour; the concave parts are their under sides, as is proved by their pale green colour, which is generally the case in the leaves of the papaw-tree. The end of the petiole bears a pointed leaf (*h*), slightly contracted, and with a pitcher-like contortion on one side. The figure is about three-fourths natural size.

The case belongs to those mentioned by Masters ("Vegetable Teratology," 355, 445) under the heads of Pleiophylly and Enation from foliar organs. His explanation is certainly correct, as there cannot be any doubt that the accessory petiole *b*, but for its development in another plane, is a true homologon of the ribs of the primary leaf, and the minute warts round its base may be regarded as small or checked beginnings in this same direction.



The described anomaly does not appear to be rare in *Papaya vulgaris*. I have observed several less-developed instances; the specimen here described was given to me by one of our students, Señor Ramon Documet. A. ERNST

Caracas, June 16

Microscopy—The Immersion Paraboloid

As I am responsible for exhibiting at the Conversazione of the Royal Society, May 1, the immersion paraboloid as being "designed by Dr. Edmunds," I should wish it to be known that, since that date, my attention has been directed to evidence establishing Mr. Wenham's priority to the invention.

Before exhibiting the paraboloid at the Royal Society, I had Dr. Edmunds' assurance that he felt justified in requesting me to describe it as designed by himself. JOHN MAYALL, Jun.

224, Regent Street, London, July 16

THE GENESIS OF LIMBS [1]

III.

I HAVE found much resemblance between the skeleton of the ventral and the dorsal fins in *Notidanus*, in *Chiloscyllium*, and in *Raia*; also between the anal and ventral fins in *Notidanus*. The ventral fins of elasmobranchs generally are so different from their pectoral fins, and so much more like the azygos fins than the pectorals are, that they serve well to bridge over the differences between the orders of fins. At the same time the value of the link is enhanced by the fact that in the very peculiar genera *Callorhynchus* and *Chimæra* the ventrals resemble the pectorals in a very remarkable and exceptional manner. But perhaps the most instructive ventral fin is that of *Polyodon*, the skeleton of which consists simply of a double series of simple parallel rays without any attachment to a pelvic cartilage which is altogether absent.

These conditions, then, appear to obliterate the distinctions which are at first apparent between the skeletons

[1] Continued from p. 311.

of the azygos and paired fins themselves. It remains to speak of the supporting structures of the paired fins, the pelvic cartilages or bones, and the shoulder-girdle. At first it appears that a formidable objection against the similar nature of the paired and azygos fins may be drawn from the existence in the former of these supporting structures (which serve in the pectoral region to fix the pectoral fins to the axial skeleton), while no such connection ordinarily exists with regard to the azygos fins.

We have seen, however, that in *Pristis* and *Pristiophorus* the dorsal fin becomes directly continuous with the axial skeleton by a mass of cartilage large enough to warrant comparison with the shoulder-girdle itself, while it is more or less firmly united with the axial skeleton in *Rhyncobates*, *Squatina*, *Acanthias*, *Spinax*, *Chimæra*, and *Callorhynchus*. It must be admitted, however, that the attachments of the dorsal fin to the axial skeleton is horizontal, direct, and continuous, while the structure supporting the pectoral fin (the shoulder-girdle) extends vertically, is arched in shape, and only abuts at one end against the axial skeleton, while ventrally it joins its fellow of the opposite side. These characters seem at first to tell against the similarity of nature of the dorsal and pectoral fins. But three things should be borne in mind—(1) the pectoral fin-support could not continuously adhere to the axial skeleton antero-posteriorily without impeding the lateral flexure of the body in swimming; (2) the pectoral fins join the body at too low a level for their support to extend in horizontally to the skeletal axis; (3) and did it so extend inwards in a straight line, even obliquely, it would intrude upon the visceral cavity. For these reasons the pectoral (and ventral fins also) must (if they are to rest on a solid support to facilitate their flapping motion) have a narrow connection with a sustaining structure, which structure must not be directly continuous, in a straight line, with the skeletal axis. Moreover, to obtain a firm basis, this limb-support, if it is attached obliquely upwards to the skeletal axis, must have some point to abut against ventrally also. Thus such support must assume the form of a limb-girdle.

I think, then, that there is sufficient evidence to warrant a belief that the skeletal structures of the paired fins of fishes (and therefore the limbs of higher vertebrates also) are the result of the centripetal growth and coalescence of a primitively distinct, parallel series of cartilaginous rays, developed in a pair of lateral fins similar to those developed, and more or less coalescing and centripetally extending in the median fins above and below.

But what about the limb-girdles themselves? Mr. James K. Thacher,[1] of New Haven, Connecticut, has thrown out the suggestion that the pelvic bones and cartilages of fishes (and therefore limb-girdles generally) are also due to the further extension inwards of such centripetal growth. I regard this as a most happy suggestion, and adopt it myself. The mystery of the limb-girdles is thus satisfactorily explicable; they are neither modified branchial arches, extra-branchials, nor ribs, but parts *sui generis*, due to the ingrowth of originally superficial structures—exoskeletal hardenings which have grown inwards and become endoskeletal.

It remains to consider the question of the development of the original digit-bearing limbs, *cheiropterygium*, from the primitive fin, or *archipterygium*.

Gegenbaur at first regarded the elasmobranch fin as derived from a limb formed like that of *Lepidosiren*, but he subsequently adopted that of *Ceratodus* as the archipterygium, in which view Huxley coincides. The former naturalist, however, considers the shark's fin and the cheiropterygium as formed by the all-but complete abortion of the rays on one side of the ceratodus limb-axis, with the simultaneous shortening and thickening of that axis into a metapterygium, while the rays of the other side of the axis coalesce to form the meso and propterygium. The latter anatomist (Huxley), on the contrary, regards the ceratodus-limb axis as forming by its progressive shortening (or drawing-in) the mesopterygium of the shark's pectoral, and the limb-axis of the cheiropterygium, the latter being perfected by the atrophy of the proximal lateral rays and the hypertrophy of the distal ones, the distal end of the axis becoming the middle digit of the hand. Of these two views the latter seems to me much to be preferred, but it demands the unity of the *centrale* carpal ossicle, which now seems most probably to have been primitively double, as it is so not only in *cryptobranchus*, but also in both limbs of three species of Siberian Urodeles.[1]

I believe, however, that the limb of *Ceratodus* is far from showing us a primitive form, but is, on the contrary, a very special and peculiar structure, which is carried to a still more abnormal development in *Lepidosiren*. This view seems warranted by the theory of evolution, according to which air-breathing vertebrates must have been amongst later developments, and therefore have post-dated creatures with limbs more or less like those of Elasmobranchs and Teleosteans. The secondary

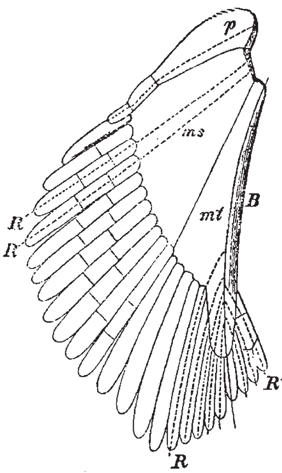


FIG. 17.—Pectoral fin of Acanthias (from Gegenbaur). *p*, propterygium; *ms*, mesopterygium; *mt*, metapterygium. The line drawn through *mt* indicates the fundamental line of the archipterygium or Ceratodus limb-axis.

fringing rays of the central limb axis of these Dipnoi may (as Peters pointed out as long ago as 1845) have arisen like the secondary fringing rays of the dorsal of the primary rays of the dorsal fin of *Polypterus*.

As to the formation of the cheiropterygium, I think that there are some reasons which favour the acceptance of the propterygium as the part in Elasmobranchs which has most relation to its primitive axis. Such are (1) the preaxial position, in the limb, of the line of the Propterygium—which is the line of support needed for the fore-limb of a quadruped which necessarily extends preaxiad, distally; (2) the apparently complete atrophy of the mesopterygium in *Chiloscyllium* and its partial atrophy in *Polypterus*, and other forms; (3) the large size of the propterygium in *Chimæra*, *Callorhynchus*, *Cestracion*, *Scyllium*, and *Pristiurus*.

On the whole, then, I feel much persuaded that vertebrate limbs have been formed as follows:—

1. Two continuous lateral longitudinal folds were developed, similar to dorsal and ventral median longitudinal folds.

[1] See *Trans.* Connecticut Academy, vol. i.i.

[1] See "Morphol. Jahrbuch" vol. ii. 3rd Heft, p. 421, Pl. 29.

2. Separate, narrow, solid supports, in longitudinal series, with their long axis at right angles with the long axis of the body, were developed in varying extent in all these four longitudinal folds.

3. The longitudinal folds become interrupted variously, the lateral folds so as to form two prominences on each side the primitive paired limbs.

4. Each anterior paired limb increased in size more rapidly than the posterior limb.

5. The bases of the cartilaginous supports coalesced as was needed according to the respective practical needs of the different separate portions of the longitudinal folds, *i.e.*, the respective needs of the several fins.

6. Occasionally the dorsal rays coalesced proximally and sought centripetally adhesion to the skeletal axis.

7. The rays of the hinder paired limbs did so more constantly, and ultimately prolonged themselves inwards by mediad growths from their coalesced base till the piscine pelvic structures arose.

8. The pectoral rays with increasing development also coalesced proximally, and thence prolonging themselves inwards to seek a *point d'appui*, shot dorsad and ventrad to obtain a firm support, and at the same time to avoid the visceral cavity; thus they came to abut dorsally against the axial skeleton and to meet ventrally together in the middle line below.

9. The lateral fins, as they were applied to support the body on the ground, became elongated, segmented, and narrowed.

10. The distal end of the incipient cheiropterygium either preserved and enlarged pre-existing cartilages or developed fresh ones to serve fresh needs, and so grew into the developed cheiropterygium.

11. The pelvic limb acquired a solid connection with the axial skeleton—a pelvic girdle—through its need of a *point d'appui* as a locomotive organ on land.

12. The pelvic limb became also elongated, and when its function was quite similar to that of the pectoral limb its structure also became quite similar. It became segmented in a way generally parallel with the segmentation of the pectoral limb, yet in part inflected inversely owing to its different mode of use.

Vertebrate limbs then are specialised differentiations of primitively continuous lateral folds, and might, for all we see, have been more numerous than two on each side, just as there are sometimes several successive dorsal fins which are all differentiations of a primitively continuous dorsal fold. The paired limbs and azygos fins may thus be all viewed as different species of one fundamental set of parts, *pterygia*, the sum total of which may be spoken of as the *sympterygium*. The paired fins of fishes are related to the limbs of higher vertebrates as structures which have diverged from their primitive condition to a less degree, not only because the piscine body is, as a whole, a more primitive structure, but also because their fins are still used for locomotion in that medium in which their primeval form—the continuous lateral fold—was first developed.

The amount of adaptive modification supposed will perhaps appear to some persons to be excessive. But I believe that the excessive plasticity of animal organisms is in general too little appreciated—a plasticity which results in, and is evidenced by, the many instances of homoplasy—the independent origin of similar structures. The existence of these adaptive modifications points to the existence of an intra-organic activity, the laws of which have yet to be investigated. The instances of serial and bilateral homology before cited from comparative anatomy, pathology, and teratology, also concur in pointing to an intra-organic activity, the laws of which are as yet unknown. The notion of an "internal force" is very repugnant to some of my contemporaries, but it is impossible to banish the idea of innate powers and tendencies, the existence of which is manifested in the inorganic world as well as in the organic world. We cannot conceive the universe as consisting of atoms acted on indeed by external forces, but having no internal power of response to such actions; and in "physiological units" and "gemmules" we have (as Mr. Lewes has remarked) "given as an explanation that very power which was pronounced mysterious in larger organisms."

Mr. Lankester[1] speaks of each animal function, even reproduction, as being "explained by its chemical and physical constitution," and of "the possibility of development" being "solely due to the physico-chemical constitution of protoplasm;" but he does not give the explanation, nor show how such constitution by itself gives developmental power. But even if he did the puzzle would but recur—By what process of the survival of the fittest did the inorganic substances obtain their various structures and innate powers?

To my mind the presence of a special internal force is made evident by the process of development; and I am disposed to concur with Milne-Edwards when he says: "Dans l'organisme tout semble calculé en vue d'un résultat determiné, et l'harmonie des parties ne résulte pas de l'influence qu'elles peuvent exercer les unes sur les autres, mais de leur co-ordination sous l'empire d'une puissance commune, d'un plan preconçu, d'une force préexistante."

Science, as I understand it, clearly points to the existence in each animal of something more than an amalgam of physical forces, to a force or principle which is *intra-organic*, as heat is in red-hot iron or light in the glowing photosphere of the sun—one with it as the impress on stamped wax is one with the material bearing such impress, though we can ideally distinguish the two. This power or force immanent in each living body, or rather which is the force of the body living (considered in an abstract way), is of course unimaginable by us, since we cannot by imagination transcend experience; nothing can be imagined by us which has not wholly or in its parts been the subject of our sensible experience, and we can have no sensible experience of this force, save as a living body acting.

It is on this account sometimes thought reasonable to deny its existence as a "figment of the intellect," forgetting the supremacy of the intellect over sense. Though no knowledge is possible to us except as following upon sensation, yet the ground of all developed knowledge is not sensational, but intellectual; it reposes ultimately not on "feelings," but on thoughts. Even in verification by sensation it is the *intellect* which doubts, criticises, and judges the action and suggestions of the senses and imagination. If then we have *rational* grounds for the acceptance of such a purely *intellectual* conception, the poverty of our powers of *imagination* should be no bar to its acceptance. We are continually employing conceptions of the kind—such, *e.g.*, as number, being, substance, causes, &c.,—conceptions perfectly intelligible, though transcending the powers of the imagination.

If, then, we should conclude that each living animal possesses a special and peculiar intraorganic force, and if such force be the imminent cause of nutritional balancings, and thereby of the facts of serial and bilateral symmetry, is it not reasonable to refer to that same cause directly adaptive modifications which, within limits, take place in response to the actions of the environment. The presence of such innate activity has been eloquently proclaimed by Hartman, though I would repudiate the contradictory term "unconscious intelligence," and would explain it in a way which differs widely indeed from his. But if such a power is the active agent in such organic adaptations, is it not reasonable to refer to it the special variations which result in the formation of new species? This is the very activity for the existence of which I have elsewhere contended,

[1] *Quarterly Journal of Micros. Science*, October, 1877, pp. 432, 433.

and to which I have applied the term "specific genesis," and it is this which I am more and more persuaded is the determining agent in, and therefore the one true cause of, the origin of species.

ST. GEORGE MIVART

VARYING EXPERIENCES

I LOVE to repeat other people's experiments, and though not in the least doubting the accuracy of recorded observations in relation to bees, clover blossoms, and fertilisation, some years ago I covered patches with wire netting, to exclude the bees, for all, every flower I believe, perfected its seeds. I hope I have earned a reputation for accuracy in my statements of facts, and that it is not necessary for me to call witnesses. I will say here, however, that about that time I was visited by Dr. Sterry Hunt, ex-President of the American Association for the Advancement of Science, and together we uncovered one patch, and examined a few mature heads, with the result as above stated.

Recently I referred to Mr. Darwin's statement that one might as well sprinkle *Linum perenne* with so much inorganic dust as its own pollen, and stated that in my own garden a plant from the Rocky Mountains perfects seeds, and can only use its own pollen. An esteemed friend takes me to task[1] for this statement, remarking that I have overlooked that Mr. Darwin's facts are confirmed by Dr. Fritz Müller, in Brazil. This, in connection with remarks made on my clover experience, leads me to suppose that some believe I have offered the facts in opposition to those of Mr. Darwin. Nothing has been further from my thoughts. My point has been to show that plants or insects do not always behave in the same manner, on all occasions, and under all circumstances. I had an interesting illustration of this in March last. Having occasion to examine a large patch of chickweed (*Stellaria media*), I was surprised to find a number of honey-bees engaged in collecting pollen from them. For the past few years I have made a point of closely watching the behaviour of insects towards flowers, and I never saw honey-bees at work on chickweed before; I never heard of any one who has. I believe the chickweed has been given up to rigid self-fertilisation. Profusely among the chickweed grew *Draba verna*. The flowers of the two are about the same size, and both white, but the bees kept with strict exclusiveness to the chickweed. Yet I know that the Draba is not obnoxious to them, for in other years I have seen them at work on these flowers. Among them also were some *Capsella Bursa-pastoris* in bloom; but they also were passed by. I have never seen bees or any insects on the shepherd's purse, but from this chickweed experience it would not be safe to say none ever do visit them. The date of this visit of the bees was March 15, the thermometer 52°, spring scarcely begun, and only these three early plants in bloom.

I had a similar instance last autumn of the honey-bee's faith in the crust of bread theory rather than have no loaf at all. We had an open mild season, and towards Christmas, long after all other flowers were gone, the *Salvia splendens*, of which I employed a large number in the decoration of my grounds, was alone in flower. On warm days they were thronged with honey-bees, and I feel almost sure they had never visited my plants in other years when other flowers were to be had. The corolla tube is too long for the bees, so they had to bore the corolla from the outside. Boring from the outside is easy work for our large humble-bees. Almost all our flowers which offer the least obstruction to mouth entrance are robbed of their sweets in this manner. Even red clover is "tapped" by them in this way. But it was very hard work for the honey-bees, and I am sure that, only for the absence of other and easier worked flowers, I should not yet be able to say that I had seen the honey-bee bore from the outside of a flower, as the humble-bee generally does. There were white-flowered varieties of this species among the scarlet ones, but all were treated alike.

It seems to me that bees are not attracted to flowers by colour or fragrance merely, but that they are influenced by labour-saving ideas. A little experience teaches them how best to work in any species to advantage, and they will of course "make time" by keeping to this one till all are done. White varieties or scarlet varieties are all one to them, they can distinguish the species by other means than colour. And then they learn where to work to the best advantage, and only glean in poor fields after the richer harvest has been gathered. These considerations will naturally lead to different behaviour in different climates, and if I note these differences it is very far from my intention to offer them as contradicting the experiences of others; on the contrary, no one has a higher appreciation of their value.

THOMAS MEEHAN

Germantown, U.S.

[1] In *Silliman's Journal*.

OUR ASTRONOMICAL COLUMN

DOUBLE STARS.—In Gilliss's catalogue of 290 double stars formed from observations made at Santiago, Chile, during the U.S. Astronomical Expedition in the years 1850-52, the conspicuous star α Eridani (*Achernar*), is reported to have been seen double, the companion being of the seventh magnitude, faint blue, and preceding, 3″ south. We look in vain for mention of this companion-star in the observations of Herschel, Jacob, and Powell, and it is especially strange that it should not have been detected by the former during his sweeps with the 20-feet reflector at the Cape. The well-known binary p Eridani is less than 2° distant, consisting of two nearly equal components of between the sixth and seventh magnitude, and at first sight it might be inferred that by a typographical error the name of the star is wrongly given by Gilliss. His position, however, is that of α Eridani, and further we happen to possess measures of p Eridani by Jacob, at the precise epoch of the Santiago observation 1850·79, giving for the angle 268°·7, and distance 4″·32; the *comes* therefore could hardly be described as preceding, 3″ south, but might rather be said to precede on the parallel. This would indicate that the star intended is really *Achernar*, and it must be left for further observation to decide upon the accuracy or otherwise of the statement made by Gilliss. If the companion exists it would be of interest to know its present position; the proper motion of the principal star is very insignificant, and marked difference from Gilliss's description would be suspicious as showing a binary character. Still it is to be observed that there are considerable discordances between the angles and distances of many of the stars in the Santiago catalogue and those in Herschel's Cape volume. The former are not the results of actual micrometrical measures. It is stated that the catalogue was formed by plotting, on a large scale, the differences of right ascension and declination of the components of the double-stars observed with the transit-circle (4½ inches aperture), and then measuring from the drawings the angles of position and distances. In most cases the right ascensions and declinations observed are given in the preceding catalogue of 1,963 stars, and the results of the graphical process can be verified by calculation. In looking through the list of double-stars the reader will note differences from Herschel's data, which are not always easily explained by possible motion, though, as some of the stars have not been properly measured since Herschel's epoch, there will remain a doubt as to the cause of these differences. As instances in point, we may mention the following numbers of the Cape cata-