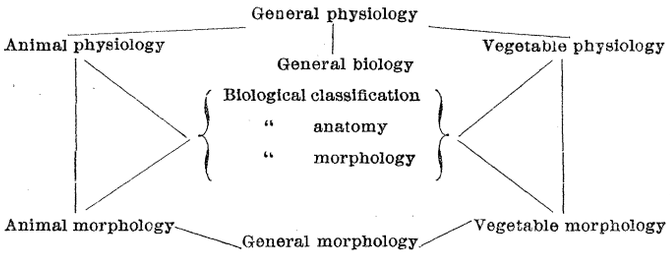
We have had for several hundred years the term physiology, which is the science of the life-phenomena.[1] There is no reason why we should not retain this name, and use it as it has been used ever since the revival of science in the sixteenth century. Biology is of later origin, it was born with evolution, and it is merely a branch of the all-embracing physiology. Biology does not consist of the entire sum of life-phenomena; it is the branch of physiology which treats of *the mutual relationship of the forms of organized matter, especially in view of the theories of adaptations and of natural selection.*

I wish to confine my remarks as far as possible to vegetable biology, and here I shall invite your attention to a very important paper by F. Delpino,[2] who regards biology as the main basis of Darwinism, and points out its importance for the theories of plant metamorphosis. With reference to the latter, we find that Warming[3] will admit only the "definition of metamorphosis" into the biology. Goebel[4] explains the state of affairs very logically in these words: "Biology regards the parts of the plants as if they were not limbs (in morphological sense), but organs, or tools," and thus he mentions one feature of biological investigation, namely, the study of correlation.

If physiology is placed at the head of natural science, and biology in its proper place as a branch thereof, we shall be able to see more distinctly how to reach the ideal, namely, the comparative physiology of animals and plants, for which so much material has been accumulated that we are able to grasp many important features of *life in general.* I have repeatedly[5] referred to this fact, but it will be admitted that the *fundamental definitions* must be logical and not ridiculous.

How biology, in the true and limited sense, branches out into the other departments of botany, I have shown in the following table. We have two features of living things: form and function, and, accordingly, the morphology and the physiology. The table shows



how we get a biological classification, or a comparative systematic botany, or zoölogy. Biological morphology is practically a morphology which deals with adaptations of the different forms to certain ends and comparatively regarded. Biological anatomy is teaching the structural adaptations in animals and plants from a comparative standpoint.

To apply biological characters and features to the systematic part of either zoölogy or botany will tend to make the registration of species and forms more valuable to physiology.

Probably it seems trifling to write quite elaborately about a question of definition. If, however, our fundamental definitions shall be not merely *adaptations for the extension of private power and influence,* we must consider them well. This is not only a question of logical consideration, but of scientific principles.

J. CHRISTIAN BAY.

Missouri Botanical Garden, April 27.

[1] See J. von Sachs, "Vorlesungen über Pflanzenphysiologie," 1887, p. 3.

[2] Fondamenti di Biologia vegetale, I.; Prolegomeni (Revista di Filosofia scientifica, Milano., I., 1880, No. 1, pp. 58-90). See Botanisches Centralblatt, vol. ix., 1882, pp. 333-335.

[3] Warming, in Meddelelser fra den bot. Forening i Kjoebenhavn, I., 192.

[4] Pflanzenbiologische Schilderungen, I., 1889, Introduction.

[5] See Science, March 24, 1893, p. 162; Bot. Gazette, xvii., 1893, p. 105; Biologisches Centralblatt, xiii., 1893, p. 38.

Epidermic Forms of Mental or Nervous Diseases or Disorders.

IT is very desirable that certain data should be gathered on "epidermic forms of mental or nervous diseases or disorders.' As an example of what is meant, I would instance "The Children's Crusade," which occurred in Europe; the persecution of certain individuals supposed to be possessed of witches in New England, and chorea, or St. Vitus's dance, occurring among school children; panic is another form very common, especially at the present day.

Could any of the readers of *Science* furnish me with any information of occurrences which have come under their notice or which they may have read about? They are certainly very common, for one reads of them very often in the daily papers. If some of your "live" readers would consider this subject seriously, and send so full reports as possible, they would not only be doing a personal favor, but would certainly be contributing toward an interesting and important collection of scientific facts.

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Color of Flowers.

I HAVE just seen Miss Neal's question in your issue of March 31, 1893, as to how to preserve the colors of flowers when pressing them. If some of your readers have not already sent a better recipe, the following may be found useful.

Immerse the stem of the fresh plant in a solution of 31 grains of alum, 4 of nitre, and 186 of water for a day or two, until the liquid is absorbed, then press the plant in the usual way, sift some dry sand over the flower, and submit to a gentle heat for about twenty hours.

I have found this process pretty successful. A. B STEELE.

Edinburgh, Scotland, April 28.

The Aurora.

IN my contribution to *Science,* April 7, on the above subject, no mention was made (as required by Dr. Veeder in his reply in the issue of April 28) of a particular instance of want of coincidence between auroral display and solar disturbance at the eastern limb, for the following reasons: First, because I have, so far, considered each phenomenon as being dissociated, or rather not connected in the manner stated; second, because I do *not* think it possible to point out such a want of coincidence with the very liberal limits of time evidently comprised in the term "eastern limb" by the advocates of this theory; and, third, amidst the bewildering number of instances, which must occur between even dissociated phenomena of such frequent occurrence, even when the limit spoken of is of reasonably brief duration, it is possible (most probable) that coincidence will be mistaken for cause. That this coincidence is not so great as claimed, seemed to me to be indicated by the results mentioned as obtained by Greenwich, as also by the same conclusion arrived at by Professor Ricco, as mentioned by Dr. Veeder; surely this is a fair assumption to make, if discussion of the same or similar records give results so widely different?

Personally, I do not wish to take any part in this discussion. Dr. Veeder's theory has constantly appeared in the press and by pamphlet without any attempted refutation; believing it to be founded on false premises, I have felt called upon to act as censor, failing any one else.

Granted a very large number of coincidences between auroral displays and the position of a disturbed area at the eastern limb of the sun; if Dr. Veeder will place a limit of, say, twenty-four hours for the term "eastern limb," and consider occurrences beyond this as not being coincidences, I believe he will find that there are as many auroras (I should be inclined with this limit to say, very many more) which occur without this particular solar source of energy as with it. Again, allowing *any* interpretation of the term "eastern limb," and, applying the same interpretation rigorously throughout, I think it will be found that the proportion of coincidences will increase from the minimum sunspot period to the maximum, and that this coincidence will vary

directly as the number of sun-spots visible. Now, if there were an intimate connection between the two classes of phenomena, the appearance of an area of great solar disturbance at the eastern limb, as is occasionally the case at the time of a minimum, should give *very* marked auroral displays, whereas it is quite certain that the coincidence is not so marked at these times (where the element of "chance" is reduced) as at the time of a maximum; is this not so?

Auroras are, or are not, an effect of sun-spots on the sun's eastern limb. I spent fourteen months in Hudson's Strait, and, to my knowledge, during the auroral season from 50 to 75 per cent of our clear nights (and clear nights were a peculiarity of the latitude in winter) had auroral displays. Assuming two such solar areas as required constantly on the sun, and representing the term "eastern limb" by twenty-four hours, we have a vastly larger number of auroras unaccounted for than this theory accounts for.

Quoting from Dr. Veeder's letter to *Science*, April 28, he says: "When, however, this area was at the eastern limb, from Jan. 7–11, *although it had not yet developed spots*, and was the seat of brilliant faculæ only, . . . great magnetic storms " were "in progress and auroras . . . reported in high latitudes."

I never saw, nor do I expect to see, the eastern or western limb of the sun when faculæ are visible to ordinary powers, when they were not more distinctly "brilliant" there than elsewhere. If this condition can be taken as a fulfilment of this theory, it is evident that the theory is beyond argument.

This quotation furnishes the required instance "in which an aurora appeared in the absence of *well-defined* solar conditions," for, according to the evidence supplied, "a great magnetic storm" was in progress from Jan. 7–11, whereas I feel certain that Dr. Veeder cannot claim that an area represented by *five* days' solar rotation (Jan. 7–11) could possess (in fact, his words show it did *not* possess) *well-defined* solar conditions of the nature required.

Sun-spots [1] have been a special object of study at this observatory since its institution. It is safe to say that something is known of their nature and origin, but that it is as nothing to that which remains to be investigated. It is possible to allow fanciful attributes to this little-known agency, which will account for any theory we may be pleased to conceive, but, treated in accordance with any known dynamical law, there seems to be no way of accounting for the peculiar action of this force, which is not equally applicable to its position at the western limb. It seems evident, from the nature of a sun-spot's formation, that the force employed is exerted in a vertical direction; it would be reasonable to expect that the resulting maximum effect should be evident, if at all, in the same direction; not horizontally, as this theory requires.

Assuming the solar force to be an "electro-magnetic" one, any resulting auroral development should bear a fixed relation to the line joining the source of energy with the earth's centre and the plane of rotation of the earth. If this is a fact, it is quite evident that points widely differing in longitude on the earth's surface will experience similar effects, as the earth's diurnal motion brings them successively under this influence, after a time-interval almost infinitely less than that represented by the difference of longitude of the two points considered. No one will surely claim that this is even approximately the case.

Again, "cosmical dust and debris" is not conclusively present in the "zodiacal light." Even accounting for the origin of the zodiacal light in this way, it is observationally evident that the rest of interplanetary space is not so filled, for this light is only visible as an appendage to the sun, in certain fixed directions; elsewhere the absence of the light proves that this "dust and debris" is not symmetrically disposed about the sun. Admitting, for the sake of argument, that interplanetary space was filled with this dust and debris, the lapsed æons of planetary existence with the countless orbital revolutions of the planets themselves must have swept out, as the masses of the planets must have aggregated to themselves, the last vestige of such dust and debris, leaving vast intervals without this assumed conducting material.

[1] "Sun-Spots: Their Maximum and Minimum Periods and Zones of Greatest Frequency." Read before the Royal Astronomical Society, April 13, 1882.

I should be pleased, and I think it would be a matter of more than personal interest, if Dr. Veeder has the time, in what I know to be a very busy life (setting the "limit" I have suggested), if he would, from out the fund of information in his possession, see how far the element of "chance" enters into this question, not admitting too much of the suppositional when sun-spots fail at the required period by the substitution of "faculæ," and at the same time show a comparison of coincidences through a semi-period, at least, of solar activity.

W. A. ASHE.

The Quebec Observatory, May 6.

BOOK-REVIEWS.

The Earth's History. An Introduction to Modern Geology. By R. D. ROBERTS. New York, Chas. Scribner's Sons. 1893. Maps and illustrations. 270 p. 12°. $1.50.

THIS volume is one of a series now being published in England by Murrray and in this country by the Scribners, as an outcome of the popular University Extension movement. The prospectus states that "the aim of these manuals is to educate rather than inform. In their preparation, details will be avoided except when they illustrate the working of general laws and the development of general principles; while the historical evolution of both the literary and scientific subjects as well as their philosophical significance will be kept in view."

The author of the present volume has been successful in carrying out this plan, for without being detailed he presents the broader aspects of the science in a familiar and pleasing manner. In the chapter on the "Agents of Destruction," he refers particularly to the Grand Cañon region, where the phenomena of denudation are shown on such a magnificent scale. This is followed by chapters on the extent of the destructive operations in Nature, and these, in turn, by other chapters on the construction of land. The constructive agents are grouped under the three heads of deposition, movements of the crust, and addition by extrusion from the interior. There are interesting accounts of shallow-water deposition, of calcareous deposits, such as coral reefs, and of deep-sea deposits. The author does not commit himself in regard to the origin of atolls, referring to Darwin's theory of subsidence, but not discussing others that have been advanced. An interesting account is given of the formation of Monte Nuovo in 1538 and of the destruction of Krakatoa in 1883.

The last part is devoted to the "Evolution of Land Areas," and we have here the application to geological phenomena of the principles enunciated in the first parts. Two chapters deal with the evolution of the British Islands. Altogether the volume gives an excellent exposition of geological phenomena and must serve as a useful compend to all who desire a knowledge of the principles without having to wade through a mass of details concerning the subject. For these details other volumes must be consulted.

JOSEPH F. JAMES.

Washington, D. C., May 3.

Public Health Problems. By JOHN F. J. SYKES. The Contemporary Science Series. New York, Charles Scribner's Sons. 8°.

THE multiplication of books relating to public health may perhaps in itself be encouraging, but the fact that the quality in no way keeps pace with the quantity is quite the reverse. The book before us covers a wide field — from "heredity" to "dwelling-houses" — but conveys, whether rightly or wrongly, the impression of being in the main the result of a "cram." The chapter on heredity, for example, opens with this remarkable statement, "The Darwinian theory of natural selection has given prominence to two schools of evolutionists, the one attributing evolution solely to selection, and the other, whilst not denying the effects of selection, valuing — perhaps over-valuing — the effects of heredity" (p. 8). If the reader be fairly conversant with modern biological literature and be in a somewhat cynical mood he will at least derive some amusement from the rest of that chapter.

It is perhaps unfair to single out the chapter on heredity for especial criticism since the subject is rather remote from the author's main theme. We regret, however, to be obliged to point