failed. The reason for this anemia is not loss of blood, and equally contrary to known facts are the various hypotheses, based on the length of stay in the intestine, the predisposition of the host and the condition of the parasite. The view that it is due to a toxin seems at present least open to criticism.

The discussion which has been laid before you in this address involves many terms which are rarely used in zoological circles, and many animals which are perhaps equally unfamiliar. To the average zoologist parasitism is a *terra incognita,* if not a *terra evitata!* The biological problems it presents were among the very first to be indicated, but have not received their proportionate attention in the intervening years. Just now there seems to be awakened interest in the subject and the results of investigations in this field are most hopeful. The subject is one which really overlaps the boundaries of zoology and encroaches upon the field of physiology and of medicine also. Much fine work has been done on the medical side of the topic, but the best results there can not be reached without generous cooperation from this side also. It is eminently fitting in this place to recall the splendid researches on morbific Protozoa carried out by a zoologist on the faculty of Columbia University. There is urgent need for similar work on other groups that the medical investigator may be furnished with those morphological, physiological and biological data upon which the successful prosecution of his studies depends. The work of the zoologists, Howard and J. B. Smith, on mosquitoes made possible the scientific victories of American physicians over disease in Havana and New Orleans. The recognition of hook-worm disease as an important factor in American medicine came through the pioneer work of the zoologist, Stiles. The splendid investigation of Councilman and his confrères on smallpox was not complete without the work of a zoologist, Calkins. The triumphs of modern science are being won by cooperative efforts and these are nowhere more indispensable than in the study of animal life, so peculiarly and closely related is it to the progress of the human race. At no point, however, is the contact more intimate than here where the zoologist is called to join with the investigator in medicine in achieving the amelioration of man's physical condition and the suppression of disease.

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SCIENTIFIC BOOKS

The Wing Veins of Insects. By Professor C. W. Woodworth. University of California Publications, Technical Bulletins, College of Agriculture, Agricultural Experiment Station. Entomology, Vol. I., No. 1, pp. 1–152, September, 1906. Contributions from the Zoological Laboratory of the Museum of Comparative Zoology at Harvard College, under the direction of E. L. Mark. No. 181.

Probably no animal organs have been so minutely compared externally as have the wing veins of insects. Comparison is so easy, so unhampered by preliminary technique, and the significant characters are so tangible and withal so useful, they are universally employed in defining both major and minor groups. There are probably no animal organs that are dealt with in a specific manner by so many workers in zoology. Therefore, when there appears a pretentious work that assumes to extend the knowledge and advance the theory of venation it attracts immediate and wide-spread interest.

Such a work is this recent one by Professor Woodworth. Its purpose is "to develop a theory that will serve for the interpretation of the facts that have been so richly accumulated" (p. 3) and "to establish a rational theory of venation" (p. 143). It aims to cover the whole field, discussing, in order, the

origin of wings, their relation to the body, the mechanics of flight, the basal articulation, vein structure, vein development, the genesis of venation, methods of modification, and venation types. It presents some new and interesting data on the nature of degenerate and metamorphosed veins, and more especially on the mode of articulation of the wings with the thorax; but, in the main, it is a purely theoretical discussion of certain classes of facts previously well known. Professing to be based on studies of twenty-two years, on "microscopical preparations of about two thousand species representing all the principal groups, a much larger series of insects with spread wings, and practically all the published figures of insect wings," it is singularly parsimonious of new facts.

The theory is, in brief, that insect wings have arisen from tracheal gills, that the veins are inherited from gill covers and have no connection with the tracheæ inside, but that "mechanical necessities are the dominant factor in their first production and in their subsequent development," and that the venation collectively evidences three major groups of winged insects, called by the author Neuroptera, Elytroptera and Neoptera.

Gegenbauer's theory of the origin of wings from tracheal gills is accepted, and is defended probably about as well as is possible in absence of good evidence. The gill cover of a single species of Mayfly larva of the genus *Rithrogena* is used in illustration. Unfortunately, this gill cover is one of the most highly specialized for its own peculiar functions, and is quite off the line of possible wing development. Its basal articulation is said to be like that of wings, but it is not described. The diagonal brace across its lower face, cited as 'most convincing evidence' of its relation with wings, would be much less convincing with a little more knowledge of gill covers. Still, notwithstanding that the choosing between theories in this field is a matter of balancing remote possibilities, the presence of basal articulation and musculature in gill apparatus gives the Gegenbauer theory an advantage over Müller's lateral expansion parachute theory. But it does not appear why the suggestions of Dr. Tower, published some years ago in this same series of contributions, should not have been noticed.

The author's treatment of the wing tracheæ is somewhat remarkable. He reluctantly admits that these air tubes sometimes grow in wings, but he does not allow them to appear in any of his wing figures—only in the Mayfly gill cover; and there, apparently, because not coincident in position with the brace which he thinks is like a primitive vein. He seeks by argument as labored as it is unnecessary to prove that they have no air-taking function, while quite ignoring their air-distributing function. Everybody knows that in insects air is carried to all the living tissues of the body, not in blood vessels as in vertebrates, but in tracheæ, and the equanimity with which, throughout this whole discussion, these principal visceral organs of the wings are excluded from consideration is most remarkable. A few quotations will illustrate this: "In many wings, at least, tracheation is a comparatively late and entirely secondary matter" (p. 7). Quite true; but in what wings? In none but those of a few highly specialized groups. Teeth are absent from the jaws of some mammals; but their absence is not better accounted for, nor more disturbing.

"In studying the development of veins, we need only to take into consideration the constant features of the developing wing, the hypodermis of the wing pad and the cuticle that it secretes" (p. 52).

"*Accidents* of structure of a temporary adaptive organ" (p. 62). The italics are mine.

"The venation is conceived of as receiving nothing from the precursor of the wing except veins that were developed in the same way and to meet the same needs with those of the organ after it became adapted to flight" (pp. 144).

He laments that in adopting the Müllerian hypothesis Packard was "not entirely able to divest himself of his former idea of an essential connection between tracheæ and veins" (p. 62). Alas, Packard was not able; neither was that 'prince of entomologists,' Dr. Hagen;

nor was Dr. Brauer; and, doubtless, many another in days to come, observing the individuality and persistence of the tracheæ, and the regular formation of the veins about them, will be unable to ignore them with the serenity of the author.

For, in primitive insects, tracheæ develop first, and the veins later develop about them. That is not theory, but fact; any one may easily see it for himself. The very presence of the tracheæ between the two membranes of the wing when these are fusing sufficiently accounts for the primary location of the veins. Moreover, these tracheæ in generalized wings show all the usual signs of homology: likeness in form, likeness in relations and greater likeness in earlier than in later stages. If these are not good evidence, there are no homologies of any significance. Furthermore, the homologies discovered in the tracheæ are fully corroborated by those of the adult veins, previously ascertained.

The author concludes his argument for the exclusion of the tracheæ as follows: "A large amount of very strong evidence would be needed to explain away the essential identity of structure in tracheated and non-tracheated veins; the evidence obtainable seems to indicate identity rather than difference. We must conclude, then, that the presence or absence of a trachea is an incident of structure of no special significance in comparing veins" (p. 47). Precisely. Just as the presence or absence of teeth in mammalian jaw bones is of no special importance in identifying the bones. Not the presence or absence of either teeth or tracheæ is of chief significance, but the form taken on when present.

The precedence of the tracheæ and the subsequent development of the veins about them is disposed of in this way: "It is impossible to deny that the location of the veins may have been really marked out, though unrecognizable to the eye" (p. 46). This position is, of course, unassailable. Those who can attain to equal faith may find equal security in it.

If a 'rational theory of venation' requires the elimination of the tracheæ in order to get room to grow in, let us take leave of them, as does the author, and then see how the theory thrives. Without attempting to follow in detail the hypothetical explanation of the manner in which veins arise, through the activity of a hypothetical substance secreted by the hypodermis, forming folds or wrinkles, controlled by hypothetical pressures, we find the veins at length appearing after the following hypothetical fashion: first a *marginal vein* about the entire border of the wing, and a *primary vein* along the middle of it in position somewhat like that of the ridge on the under surface of the gill cover figured. Then there appear *anterior* and *posterior veins* in the spaces at either side of the primary, outgrowing from the base of the wing. Finally, a series of *independent veins* is formed by ingrowth from the marginal vein toward the base of the wing; and with subsequent attachments and adjustments of these, the venation is completed.

In this theory of ingrowing independent veins (branches of the median vein) 'a class of veins that never had basal connections,' lies the chief novelty of the paper. It becomes at once evident why the tracheæ have had to be excluded; for the branches of the median trachea are not independent, and they grow outward from the base of the wing like the other tracheæ. It is, indeed, surprising that greater care has not been taken to establish an hypothesis intended for general application on a better basis of facts. The only evidence given to show that these veins are really ingrowing is that they are usually weaker toward their inner ends and are sometimes independent (unconnected) there. The fact that this weakening is most pronounced and that the detached condition occurs in the more specialized members of the several groups is passed by unnoticed. In generalized Lepidoptera, *Hepialus,* the Psychidæ, Cassidæ, etc., these veins have basal connections, and when free proximally, their dislocated basal rudiments within the discal cell might well have been accounted for. In the Ephemeridæ, where the author finds his series of free and primitive independents most highly developed, they are, unfortunately, most free in *Callibœtis, Bœtis, Chlœon,* etc.—a bunch of genera representing the extreme

of specialization in a highly specialized order. In the Paleozoic Protephemeridæ the author meets (and frankly acknowledges) contradiction from the opposite end of the Mayfly series: "In one point, however, these early wings stand in marked contrast with those of the modern group. It is the absence of free independents. The production of free independents prior to connected ones would seem to be the natural order of evolution, but this evidence certainly does not point that way" (p. 97).

The author's account of their origin is all comprised in the following sentence: "The independents arise from the *margin of the wing,* and might possibly be considered as ingrowing branches of the marginal vein; but, since this portion of the marginal vein is commonly absent, while the independents are almost always present, this conclusion may seem to be unwarranted" (p. 68). Nevertheless, his system is built upon it.

But matters are still worse when viewed from the mechanical standpoint. This theory of veins ingrowing from the hind margin contradicts the primary principle of insect æronautics. For, as is well known, forward motion through the air is due in insects primarily to the sculling action of the wings when vibrated up and down, and that action results from the pliancy of the hind margin. The trend of specialization in the wings of all the orders is toward greater stiffness of the front margin and greater relative pliancy of the area behind it, and the obvious mechanical advantage of this is that they scull in air better. The disappearance of the marginal vein and the fading out of the base of the median, are, owing to position in the wing, the earliest and most expedient contributions to that pliancy. Girard more than half a century ago demonstrated experimentally the detriment to flight of adding weight to that portion of the wing in which these 'independents' are supposed, according to this hypothesis, to originate.

The true order of development is inverted. So it is in the case of the cross veins, whose origin is discussed on page 71. These are supposed, on this special creation theory, to grow up in the clear membrane *de novo.* Special activities of certain cells, occupying the positions that are to be those of veins and cross-veins, are made to account for the appearance of these. But for the assumption that the cells about the vein cavity show greater secretory activity, or produce more chitin, cell for cell, than those outspread in the intervening membrane, there is no proof offered: and it is not clear why the simpler and long current explanation of the differentiation veins from membrane, by accumulation of cells about the vein cavities, and the stretching of those that lie between, does not give a better basis for the application of mechanical principles. For how shall "the more rational conception that there existed in the beginning and has existed through all time to the present day a mechanical necessity in accordance with which the primitive venation was produced, and all its essential features have been maintained through all the vicissitudes of the ages" (p. 62) help us account for anything? It is merely a flow of rhetoric: not an explanation. The mechanical principle were better stated, or at least its operations detailed, with some indications of the material on which it operates. Throughout this paper controlling mechanical principles are heralded as though a new discovery in insect wings, but they never come to light. On the contrary, as we have seen, well-known mechanical principles are flatly contradicted by the theories proposed. Were it not that the principle of vein differentiation is already fairly well understood, this theory of ingrowing independent veins might possibly have made as large a contribution to the confusion of the subject as did that of Adolph, its lineal antecedent.

A reviewer of vein mechanics should have been able to see the primitive dichotomy of the branching of veins. It is a curious survey of insect venation that misses this. Dichotomy abounds in the venation of the oldest known fossils. It occurs in the generalized members of most of the groups. It occurs in the gill covers of many Mayfly larvæ and is beautifully shown in those of *Ephemerella.* It occurs in plants, also, and is the type of

branching of such thallophytes as *Dictyota dichotoma* and such liverworts as *Riccia fluitans*. It occurs, apparently, wherever a branching organ extends itself unimpeded in one plane. Its wide-spread independent occurrence is evidence that it is the result of developmental dynamics, and there can be no doubt it is primitive.

The author thus 'having traced the development of these systems of veins' (p. 73) proceeds with full assurance to the application of his theory. He has no difficulty at all in identifying his marginal vein in the catch on the inner edge of the elytron of Coleoptera (p. 75). It is safe to say that no other venation theorist has ever ridden his hobby so far as this. His primary vein, 'invariably found in functional wings' (p. 63), is absent from at least one of the wings shown on p. 100. With respect to the Odonata-anisoptera, in discussing the triangle, he says: "It does not, nor does the wing of any of the Anisoptera, show any transition between a triangular and a quadrilateral cell." The ignorance of our own fauna and its literature shown by this statement is not more surprising than the readiness with which he ignores the illustrative genera that are mentioned in the legend to the figure he is criticizing. But these and other misstatements concerning the Odonata will not mislead the students of that order. Having ignored tracheation and also the likeness in venation between Plecoptera and Orthoptera, he is able to give a different interpretation of the two orders. But the great superiority of his theory appears in the treatment of the venation of the fossil Homothetidæ (p. 102), Embiidæ (p. 106) and Physopoda (p. 126). Even the last named, for whose puzzle no one has hitherto ventured a solution, is instantly resolved by the application of the 'rational theory'! It is wonderful. The only trouble with it is that it is too easy. When in doubt about a vein call it what it most resembles in the hypothetical diagram. "Class as an independent vein anything that anywhere exhibits structures characteristic of independent veins" (p. 69). Throw away the usual safeguards against misinterpretation of parallelisms: they are all superseded by the application of a mechanical theory!

And when we reach the end of it, we find that its goal is another system of vein nomenclature! This is formally compared, vein by vein, with four[1] (out of the dozens) of systems proposed in the past. It is hard to see why the author, since he identifies all of these in detail including the generally recognized branches, should have thought to advance entomology by a new batch of names for them. It is not easy to understand why, if the new terms *marginal* and *primary*, etc., may be used in an elastic sense, as provided on p. 145, the old terms *costa, radius*, etc., might not, if it were necessary, be so used, equally well.

Other peculiarities of this work are the ignoring the literature of the subject for nearly the last decade and of important papers on the mechanics of insect wings, much older: the misspelling of the names of authors cited, Aaron, de Selys Longchamps and McLachlan (the last in two ways), and evident misstatement of facts, such as this: "An increase in the size of a wing usually results in an increase in the number of veins" (p. 65). There is no justification in morphological experience for the statement that "increase in a wing area would do just the same things that a decrease would undo" (p. 80) and that the "direct effect of environment would be sufficient" to differentiate two groups (p. 9) is surely assuming something. The statement of p. 145 that other workers have not recognized that the same names should be applied only to homologous organs, is a fine bit of assumption. There is a mysticism about the account of the genesis of the venation that is somewhat unusual in a scientific paper: page 79 is full of it; and the statement that the primary vein was developed to be the dominant vein (p. 144) reminds one of the statement of that other narrative of

[1] Those attributed to Comstock are the names selected by Redtenbacher as most available. They were in his day and are now the ones in most common use, and to their adoption no serious objection has ever been offered.

Genesis, that the lights in the firmament of heaven were to be for signs.

It is altogether probable that entomologists, before entering upon the course here marked out for them, will demand a better statement of guiding principles, and a better disposition of the ontogenetic and phylogenetic difficulties that beset the way.

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Minerals and How They Occur. By W. G. MILLER, Provincial Geologist of Ontario, formerly Professor of Geology, School of Mining, Queen's University. Toronto: The Copp, Clark Company. 1906.

In his intention of producing a book on mineralogy for 'secondary schools and prospectors' the author has succeeded admirably both because of the clearness and simplicity of his style and because of his accuracy of statement. There is room for books of this sort since, though the subject is of wide general interest, there are few sources of information which are attractive to the *beginner*. The average book offered to the beginner is not only inaccurate but lacks successful arrangement and shows poverty of facts and illustrations. Professor Miller's book contains about two hundred illustrations and these give such an idea of the subject as descriptions could not convey. They are new, well selected, and some of them are especially good (*e. g.*, Figs. 20, 47, 63, 79).

The large amount of information contained in the book is in attractive form. Upon looking at the table of contents one might be reminded of Voltaire's essay on dogs in which towards the close he says 'Speaking of dogs reminds me of cats' and proceeds to write a short dissertation on cats. One might think that the paragraphs on fossils were hardly called for a book on mineralogy. But as he becomes acquainted with the author's aim he sees that the book differs from the ordinary one which presents the science in its narrower aspects and that it has been written just as if the author were talking to interested beginners before whom he must needs start with the most obvious things—rocks, the common rocks with which his readers are familiar—and build upon them his edifice. Such work necessitates excursions into the surrounding country and the result is a building all of whose parts contribute to mineralogy.

The more involved parts of the subject are omitted or touched upon but lightly and the things which are apt to prove most attractive to beginners are presented in logical and compact manner.

A few changes might be suggested. Though crystallography is the least palatable side of the subject it is so essential as to require more attention. When the axes of the six systems are being given (Fig. 23) one should not be omitted; the orientation should be according to the almost universal method—*i. e.*, a should always be the axis pointing to the observer, should always represent the short axis in the orthorhombic and triclinic systems, and the inclined axis in the monoclinic system. β should represent the acute angle made by the intersection of c and a and a by the intersection of c and b. All of the simple holohedral forms should be pictured and with the axes drawn in them. Whether combination, twinned and hemihedral forms are presented may well depend on the space at the disposal of the author. But if crystallography is to be mentioned at all the first principles should be given with clearness.

An occasional statement like the following should be modified. "During late years this theory of origin (of petroleum and natural gas) has been questioned by many workers who are inclined to believe that both materials are of inorganic origin" (p. 59). Forty years ago Berthelot suggested that petroleum might have originated from union of carbonated waters with uncombined sodium and potassium and about ten years after that Mendeleef propounded as a possible origin the union of such waters with metallic carbides. Thus the theory can hardly be called a recent one and in addition it appears to be a theory which shows possibilities rather than the facts which the study of oil fields the world over seems to establish. The actual geological conditions in oil fields necessitate the conclusion that oil and natural gas are of organic origin.

The book is full of valuable information