We have to define efficiency by "work out divided by work in." The teacher has also replaced the dictionary.

Our school work in what is called, from custom, reading seems to consist in reciting some "pieces," very ultra-modern, calling for some acting and a little thought. Later the pupils are required to learn what some critic has said about the great works, with perhaps extracts from the professor's doctorate thesis. It is then certain that the pupil will not read any of the books which he has heard called classics.

A teacher found that his pupils could not get what was in the book. They said: "Why do the books not present the matter as you do?" He wrote the book; he reported that the reviewers said that it was about as dry a book as they had ever seen.

JOHN N. JAMES

INDIANA, PA.

THE COTTON WORM MOTH

I WAS interested in Professor Fernald's note on the cotton worm moth in your issue of November 27. Professor Fernald reports that few of these moths were taken in Massachusetts in 1912. Now in 1912 we had a great flight of them here, the only invasion on a large scale that I have heard of in this locality. They were here by the tens of thousands, literally covering the ground for a space of 100 square feet or so under some of the street lights.

The moths arrived on the night of October 10; the night watchman in the village told me they came in all at once at about 3 A.M. and flew for a time in such swarms round the electric lights "that you couldn't see the lights for the moths." They were reported in large numbers in at least one other village near here; and my father who was then living in London, Ontario, wrote me that there had been an invasion there which arrived two or three days earlier than ours here, but which must have been on the same large scale as to numbers.

It would be interesting to know whether these were parts of the same front, or separate swarms moving independently.

In 1913 I saw none here, but during the past autumn there were a few specimens, though I have no record of the date of their appearance.

A. P. SAUNDERS

CLINTON, N. Y.

METEOROLOGICAL OBSERVATIONS IN GERMANY

A LETTER dated Berlin, November 30, 1914, from Professor Dr. Gustav Hellmann, director of the Royal Prussian Meteorological Institute in Berlin, advises us that the usual regular observations are being maintained without interruption throughout the German Empire. So far as the internal weather forecasts for Germany are dependent upon cable reports from foreign countries they are made with difficulty; all such reports are at present interrupted, even those from Iceland, since the latter come over a Danish cable that lands at Aberdeen where they are suppressed and are not permitted to reach even Copenhagen. The regular, though belated arrival of the *Meteorologische Zeitschrift*, together with other scientific publications show that the German scientific world is far from suspending its existence during its present struggle.

C. ABBE, JR.

SCIENTIFIC BOOKS

An Account of the Mammals and Birds of the Lower Colorado Valley, with Especial Reference to the Distributional Problems Presented. By JOSEPH GRINNELL. University of California Publications in Zoology, Vol. 12, No. 4, pp. 51–294, Pls. 3–13, 9 text figures, March 20, 1914.

The report before us gives the results of an expedition undertaken in the spring of 1910 by the California Museum of Vertebrate Zoology. Since the founding of this museum by Miss Annie M. Alexander, in 1908, Grinnell and his staff have spent much of their time in the field, accumulating extensive series of specimens, representing the fauna of California and adjacent states, and

of the coast region as far north as Alaska. This valuable material has been collected largely with a view to the study of geographic variation. Throughout that time, and indeed for a much longer period, the author of the present report has been active in describing and subdividing species of Pacific Coast birds and mammals.

A considerable proportion of the vertebrates of this region are represented in different parts of their range by different local races, for which the term "subspecies" has gained general acceptance. Indeed, the process of "splitting" in these groups has been carried to such lengths that a large majority of the birds and about three fourths of the mammals listed in the paper here considered are designated by trinomials. Outside of taxonomic circles the feeling is sometimes expressed that these trinomials stand for more or less fictitious entities, the product of minds in which the passion for detecting differences has almost reached the stage of paranoia. In quite a different spirit is Bateson's recent advice to the systematists to "subdivide their material into as many species as they can induce any responsible society or journal to publish," since "the collective species is a mere abstraction, convenient indeed for librarians and beginners, but an insidious misrepresentation of natural truth."[1] Whether these ultimate subdivisions are termed species or subspecies is, of course, a matter of secondary importance. The main thing is that they should be described and named.

It is probably no mere accident that several of the leading exponents of the "isolation" theory of specific differentiation have done much of their field work on the Pacific Coast of North America. Here the subdivision of the earth's surface by means of natural barriers is carried to an extreme probably not elsewhere found within the limits of the United States. It is true that in many cases the areas thus marked off differ very greatly in their climatic conditions, as witness the abrupt change which we encounter in crossing the mountains from the Mojave Desert to the orange belt of southern California. Any specific differences which are met with on the opposite sides of such a barrier might be attributed to environmental differences acting directly or indirectly. Such cases do not, of course, prove anything as to the efficacy of isolation *per se* in giving rise to divergent descent lines.

In the lower Colorado River, however, Grinnell finds what he regards as a critical instance. Here is a river, bordered on each side by a vast expanse of desert, uniform in its character for great distances, whether to the right or left. Considered as a physical environment, the California side of the river is identical with the Arizona side. Yet of the 23 species of rodents collected in the valley of the Colorado by the museum expedition of 1910 Grinnell and his party found 8 which were absolutely restricted to one or the other side of the river. These last were all strictly desert-dwelling forms which probably never visit the water's edge. On the other hand, the inhabitants of the lower reaches of the river bottom were found to be in every instance common to the two banks.

The case upon which the greatest stress is laid is that of two species of ground-squirrel, belonging to the genus *Ammospermophilus*. Twenty-four specimens of *A. harrisi harrisi* were captured at scattered points on the Arizona side of the river, while seventeen specimens of the closely related *A. leucurus leucurus* were taken on the California side, the two occupying the same "ecologic niche" in their respective territories. In no case was a single individual found on the "wrong" side of the river. These two species were seen at points only about 850 feet apart in a direct line. Commenting on this case, Grinnell remarks:

> The sharp separation of the ranges of [such] nearly related vertebrates by a barrier of such narrow width is, to the best of the writer's knowledge, not known elsewhere in North America.

The author makes the assumption common to both Lamarckians and Natural Selectionists that morphological differences must, in some

[1] Bateson, "Problems of Genetics," Yale University Press, 1913.

way, result from environmental differences. But, in this instance, "the climatic features (zonal and faunal, as well as associational) are identical on the two sides of the river." Therefore, it is "reasonable to presuppose separate and rather remote centers of differentiation, and convergent dispersal through time and space which brought the resulting types to the verge of the river, beyond which they were unable to spread." It is needless to point out that hypotheses exist, *e. g.*, that of "mutation," which do not invoke the aid of environmental differences to account for all specific change. According to such a view, the "remote centers of differentiation" could be dispensed with.

If, as Grinnell believes, the two sets of animals "have undoubtedly descended from ancestral lines, which have invaded the territory from the two opposite directions," already specifically distinct, we can not see the force of the conclusion that "adequate ground is afforded for the belief that intervention of barriers is a prime factor in the differentiation of species." All the evidence shows in this case is that the barrier has kept these species apart. It may have had nothing to do with their differentiation as species. Indeed, in the absence of experimental evidence, we can not even affirm with certainty that any physical barrier has been necessary for the continued maintenance of their specific distinctness. It is not impossible that a high degree of sterility would be found to exist between the California and the Arizona species. Nevertheless, the facts, as described, are of great interest. Further expeditions should be sent into the valley of the Colorado for the express purpose of testing some of these important questions. And the work should be done before nature's original scheme of distribution has become hopelessly muddled through man's agency.

Grinnell recognizes "three distinct orders of distributional behavior as regards terrestrial vertebrates." First,

> every animal is believed to be limited in distribution *zonally* by greater or less degree of temperature, more particularly by that of the reproductive season. . . . When a number of animals (always in company with many plants similarly restricted) approximately agree in such limitation, they are said to occupy the same life-zone.

Throughout this and many other papers by the same author the "life-zone" conception plays a prominent rôle. The zones recognized by C. Hart Merriam are adopted by Grinnell, and their existence accepted as a fundamental datum, without the necessity of their being justified to the reader. The author believes, following Merriam, that the position and extent of these "zones" is determined by temperature conditions. Yet it is obvious that, throughout considerable portions of the continent, the details of temperature distribution are not known with any approach to precision. Thus, the actual criterion which the field zoologist falls back upon in any given case is the character of the fauna and flora which he finds associated together. The presence of certain species shows him that he chances to be in this or that "life-zone." It is assumed, though apparently seldom verified, that wherever these particular species occur in conjunction, the temperature conditions are in some essential respect similar.

It would seem *a priori* that in traveling along a uniform gradient from a region of higher to one of lower average temperature, or vice-versa, one would continually pass into and out of the ranges of species which found their limits of physiological adaptability at different points along the line. One would scarcely expect to encounter critical points, where the fauna and flora as a whole, or at least the most characteristic members of it, were suddenly replaced by a quite different assemblage. Yet this is the essence of the "life-zone" conception.

It would be foolhardy, indeed, for a zoologist of limited field experience to criticize this conception. It is doubtless based upon extensive and accurate observations and represents real facts. But unfortunately they are, in a high degree, facts which, by their very nature, are scarcely communicable to most biologists. Before the life-zone conception can be of much service to the average student of evolutionary problems it will have to be expressed in terms

which he is able to comprehend without making extended explorations, under the personal escort of one of the initiated. Until then, such expressions as "Upper Sonoran," "Transition" and the like will be to him mere empty names, or at best, they will recall to his mind certain colored areas, on a map of North America, the boundaries of which seem to have been chosen quite arbitrarily.

The second type of "distributional behavior" recognized by Grinnell is that which he terms "faunal." The various life-zones are each subdivided into a number of "faunal areas" (or, more simply, "faunas"), "on the causative basis of relative uniformity in humidity." We must, at the outset, question the wisdom of appropriating the word "fauna" for use in such a restricted and technical sense, particularly since quite a variety of meanings have already been attached to it by previous writers.

Grinnell regards it as "probable that every species is affected by both orders of geographic control" (*i. e.*, temperature and humidity), though believing the influence of temperature to be the greater of the two. While this belief in the rôle played by humidity does not appear to be based, in any single case, upon exact observational data, it would surely be unreasonable to throw it out of consideration on that account. One does not require an accurate hygrometer to sense the difference in humidity between the atmosphere of the redwood district of northwestern California and that of the Mojave Desert. We can not help wondering, however, whether sufficient care has been taken to disentangle the effects of atmospheric humidity from those of rainfall and soil humidity. Regions of high atmospheric humidity *may* be regions of high rainfall as well, but the reverse is not infrequently true, as witness the coast of southern California. It is a matter of common knowledge that vegetation is far more affected by the though the latter is also an important factor.[2] moisture of the soil than by that of the air,

[2] Transeau (*American Naturalist*, December, 1905) contends that the controlling influence for plants is the ratio of rainfall to evaporation.

Since the distribution of animals is so largely conditioned by that of plants, the indirect effects of rainfall and soil humidity upon the fauna of a region are beyond doubt. With rodents and other burrowing animals it seems not unlikely that the effects are much more direct.

The third "category of distributional control" recognized by Grinnell is that which he terms "associational." By "associations" he means "tracts of relatively uniform environmental condition, including their inanimate as well as living elements." They are, of course, subdivisions of a "fauna," just as "faunas" are subdivisions of a "life-zone." The "association" proper to a species represents its habitat, in the narrower sense, as distinguished from its geographical range. It is here that we find the most conspicuous correlation of the "so-called adaptive structures of animals . . . with certain mechanical or physical features of their environment."

The associations considered in the present report are all (except one) named for some characteristic plant, and, in fact, the term itself is borrowed from the botanists, by whom this conception was first developed. Ten of these associations are distinguished in the lower Colorado Valley traversed by the expedition under consideration. This whole region, however, belongs to the "Colorado Desert Fauna" and to the "Lower Sonoran Life-Zone."

The report contains a considerable fund of valuable ecological detail, palpably based upon careful observation, and in a large degree coordinated, so as to lead to conclusions, or at least to definitely formulated problems. In this last respect it stands in gratifying contrast to the recent output of some of the professed exponents of the science of ecology. A highly interesting special instance is Grinnell's discussion of associational restriction, as illustrated by the various species of pocket-mice (*Perognathus*). Here a truly quantitative mode of treatment has been resorted to, and very instructive results reached, despite the comparatively small number of individuals.

If nothing more, they point out a promising method of detecting and measuring associational preferences among animals which may be readily trapped.

The evolutionary theories of Darwin and Wallace were largely founded upon personal observations of geographical distribution. The modern student of genetics, on the contrary, carries on his studies for the most part in the laboratory and the breeding pen. It is significant, therefore, that Bateson,[3] perhaps the foremost living Mendelian, devotes a considerable portion of a recent volume to the problems of geographic variation. And one can hardly read that volume attentively without being convinced that the field naturalist holds the key to some of the most important secrets of nature. It is not improbable, therefore, that works of the sort here reviewed will come to receive more serious consideration from those who are concerned primarily with the problems of organic evolution.

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Chemical Technology and Analysis of Oils, Fats and Waxes. By DR. J. LEWKOWITSCH. Edited by GEORGE H. WARBURTON. Vol. II. 1914. Pp. 994. $6.50.

The first volume of this work appeared in this country while the author lay dead. While the death of an eminent chemist is always to be regretted, in this case there was an additional reason for regret—the delay, or worse yet, the possible non-appearance of the remainder of the treatise. The delay has been so slight as not to be noticed and the editorial work has been most satisfactorily performed by Mr. Warburton, who for seventeen years was associated with Dr. Lewkowitsch in his analytical practise.

This volume has been increased in size by thirteen per cent.; important additions have been made in the articles on linseed, tung, soy bean, cocoanut oils and candelilla wax, as well as minor additions to other portions to bring them thoroughly up to date.

The work may fairly be described as monumental; nothing would seem to have escaped attention. Even the toxicity of the different chlorides with two atoms of carbon has been given, as having a bearing on their technological uses.

Notwithstanding the very full table of contents, the reviewer misses, and must wait a year perhaps for, an index which it would seem advisable to include in each volume. Similarly the reviewer is inclined to question the advisability of including the large amount of statistical matter about the commercial side. That, it would seem, might well form the subject of a single volume, like the author's "Laboratory Guide to the Fat and Oil Industry" and be revised and brought up to date more frequently. If the work continues to grow as it has in the past, it would seem worth while to consider its publication by some society, as its compeer "Beilstein" has been taken over by the German Chemical Society.

A. H. GILL

[3] *Op. cit.*

SPECIAL ARTICLES

THE NITROGEN NUTRITION OF GREEN PLANTS

IT is the teaching of botanists that green plants obtain their nitrogen chiefly in the form of nitrates, though ammonium salts may be utilized to some extent by certain plants at least. Exceptions to this general rule are those plants provided with root-tubercles (and bog plants and others which have mycorrhiza?). These plants obtain their nitrogen in the form of organic compounds made for them by the bacteria growing in the tubercles.

That nitrogen circulates throughout the structures of plants in organic combination is certain. There does not appear to be any reason why similar compounds which are soluble and diffusible (amino acids?) should not be taken up through the roots of plants and utilized as such. It appears to the writer that this must very probably be the case. Arguments in favor of this view are:

1. The nitrogen nutrition of the leguminous