were made, give a higher percentage of success than the Signal Service predictions for this vicinity. Professor Hazen made the predictions for the Signal Service during October; and if more extended comparisons between his predictions and those of Blue Hill are of importance, why not compare the Blue Hill predictions with the similar predictions of the Signal Service, published in the same newspapers? The Blue Hill predictions were made for south-eastern New England, and I am perfectly willing that they should be verified for the States of New Hampshire, Massachusetts, and Rhode Island in accordance with the published rules of the Signal Office (see chief signal-officer's report for 1886). In making the Blue Hill weather (not temperature) predictions, the phraseology and definitions of the Signal Service have been closely followed; and, if any of the readers of *Science* care to extend the comparison, I will gladly furnish them with the past or future Blue Hill predictions as they appear in the Boston papers, since I am confident that these, when verified in accordance with the published Signal Service rules, will give a higher percentage of success than the predictions of the Signal Service. When it is considered that the Blue Hill predictions are extended for nine hours longer in advance than those of the Signal Service made from the same telegraphic reports, and that less than one-third the telegraphic data at the command of the Signal Service are available at Blue Hill, it seems clear that by improved methods and more localized predictions the efficiency of the Signal Service could be greatly improved and its expenses reduced. During January the Blue Hill predictions will average something like fifteen to twenty per cent higher than the Signal Service predictions for this locality; and this seems of interest, since I understand that Professor Hazen, who is assumed to be one of the leading predicting-officers, made the Signal Service predictions for this month. H. HELM CLAYTON.

Blue Hill Observatory, Jan. 30.

Hybrid Diseases.

IN a paper presented at the recent meeting of the American Public Health Association (*Science*, x. 289), Dr. E. M. Hunt of the New Jersey Board of Health brings out some original ideas about disease-germs, that are likely to prove misleading to persons whose knowledge of the subject is derived from the public press. The etiology of so many zymotic diseases is now under investigation by experts in bacteriology, that the general reader or practitioner who is not an investigator is severely taxed to keep track of the often conflicting and incomplete results; and an especial effort should be made to avoid unnecessary complication of the subject by the introduction of theories not based on a correct understanding of what is known or extremely probable.

Excluding the protozoan claimed by Laveran and others as the cause of malarial fever, the moulds that occur in connection with certain local diseases of the ear, etc., and the *Actinomyces* of man and some other mammals, the active agents of common parasitic diseases that are at all credited are bacteria. One of the systems of classification now generally used recognizes four main divisions of lower plants below mosses and liverworts, — thallophytes, zygophytes, oophytes, and carpophytes, — beginning with the lowest. Bacteria fall by common consent into the first and lowest of these groups, — the protophytes. This group is a sort of *omnium gatherum* for many things that cannot be placed elsewhere, and is chiefly known by negative characters, the absence of much evident structural differentiation, and of any form of sexual reproduction, heading the list. This being the case, it would partake of dogmatism to make any very emphatic assertions about the plants that now find lodgement in it; yet it may fairly be said that no theory that rests upon the assumption of sexual processes in any of the protophytes is tenable. Hybridity is usually the result of sexual union between representatives of two more or less nearly related species, and in this sense is not only not known among plants of this group, but very improbable, since they have thus far given the best investigators no indication of even the simplest form of sexual union, — conjugation. The only other mode of hybridizing, if it really be such, corresponding to the formation of 'graft-hybrids' among flowering plants, could come only from the fusion of individuals of two species, and would amount to conjugation. It seems to me, therefore, that such a theory of hybrid diseases as Dr. Hunt has propounded is entirely untenable, and a very unfortunate addition to a literature already overcrowded with notions that others must eliminate.

I fear that my friend Mr. Meehan wrote his opinion on lichens rather hastily, and perhaps without intending to have it given to the readers of *Science*, or he would scarcely have expressed the belief "that all lichens are hybrids between fungi and algæ." Botanists do not agree on the lichen question, any more than physicians do on the germ-theory of disease; but neither the followers of Schwendener, nor the old school, would be likely to advocate the hybridity that Mr. Meehan believes to be conceded. The relationship of the two parts of a lichen, according to the Schwendener school, is merely that of association, either parasitic or symbiotic, and in no sense comparable to hybridization, while the advocates of lichen autonomy hold them for parts of one and the same individual.

Realizing fully the advisability of excluding dogmatism from the discussion of all that pertains to sanitation, I have written this correction in no *ex cathedra* spirit, and I trust that it will not appear to either Dr. Hunt or Mr. Meehan as any thing more than an effort to check the entrance of error into the discussion of one of the most important subjects that is prominently before the public.

WILLIAM TRELEASE.

St. Louis, Mo., Jan. 28.

Color-Blindness.

REFERRING to your comment in *Science* of Jan. 27, I would say that I have always believed that the defect of color-blindness could be accurately described only by one who, like myself, is subject to the peculiarity. From an early age I have been aware of the trouble, and by my attempts to assign names to colors have often furnished my friends much amusement. I have made many efforts to correct the defect, and am convinced that any attempts to educate the color-sense will result in no benefit to those who are really color-blind.

There are two sets of colors which in my mind will always be hopelessly confused. The greens, browns, and reds comprise the first; and the blues, pinks, and purples, the second. None of these colors seem to me absolutely alike. The contrast, however, is not striking, and I should describe each of the three as different shades of the same color.

Being near-sighted, I could not at a distance distinguish the blossoms from the leaves of a bed of scarlet geraniums. On approaching, however, I could readily detect the difference, but should describe the flowers as darker than the leaves, though to my eyes somewhat similar in color. While riding through the fields of France, members of our party frequently exclaimed at the multitude of scarlet poppies in the grass. Though I looked with longing eyes, not a poppy did I see during the entire journey. Similarly I am unable to detect cherries upon the trees, or strawberries on their vines, unless quite near to them. Notwithstanding this confusion of green, red, and also of brown, I can, by the worsted test, detect a difference in all the shades of these three colors. If I attempted to assign names to the various hues, it would of course be mere guess-work. The neutral tints of a November landscape, too, possess great beauty for me. The green of the grass, the browns of the leafless trees or of the soils in adjoining fields, the sombre hues of the sky, are all pleasing to my eye. Such being the case, the term 'color-blindness' seems altogether a misnomer.

The second set of colors I should describe as follows: pinks, blues, and purples are closely allied; I should call them all blue. Pink seems a lighter, and purple a darker, shade of the same hue. But, as in the case of the first set, all variations of these three colors are readily manifest to my eye.

It may seem too strange to be true, but I have frequently arranged flowers into bouquets which have been perfectly satisfactory to those who are not color-blind. I have, of course, no means of determining whether a brilliant sunset is more charming to others than to myself. I fancy that my defect deprives me of very little of its beauty.

Although in the rainbow I can distinguish only the red, yellow, and blue, it is probably as attractive to me as to others. I have as

yet failed to find any one who can readily detect the seven primary colors. It is said of Dalton, from whom color-blindness was once named, that he could distinguish only the colors of blue and yellow in the solar spectrum. Dr. Mitchell tells of an officer who chose a blue coat and a red waistcoat, believing them to be of the same color; of a tailor who mended a black garment with a crimson patch, and put a red collar on a blue coat. Such mistakes seem quite as ridiculous to me as to others. Yellow and black I have never confounded with other colors.

There is such a diversity in color-blindness, that it seems impossible to determine the cause. I am convinced that it is a physical defect. The eye, as a mechanical instrument, has not been found at fault. The cause is undoubtedly due to some peculiarity of cerebral formation. Like the cause of left-handedness, which is due to unusual development of the right brain, color-blindness is due to a freak of nature.

The education of the color-sense among the children of the primary schools has proved of great value in removing that uncertainty in distinguishing colors which of course may be found among most ignorant people, old or young. This has its parallel in the education of the ear to the appreciation of all the variations of the musical scale. But for one who is really color-blind, education can be of little avail in correcting the defect. W. B. HARLOW.

Syracuse, N.Y., Jan. 27.

A New Text-Book on Zoology.

THERE can be no better evidence of the growing interest on the part of certain reading-classes of all ages, and the importance that is being daily attached to biological studies by school authorities and educators, than the ever-increasing demand for good text-books in zoölogy, and the frequency with which such volumes put in an appearance. We now have before us a thoroughly revised edition of Steele's 'Fourteen Weeks in Zoölogy' (New York, Barnes), — a little work that held its place with great popularity for ten years, and which has now been almost entirely rewritten by Prof. J. W. P. Jenks of Brown University, who is quite responsible for its present form.

From the author of the work I learn that the volume in scope is principally designed for beginners in our high schools and academies at the average age of fifteen to eighteen years, in which schools they have no special means for illustration. Moreover, to be efficient as a text-book, it is intended to be used only by a class of teachers who presumably possess quite a thorough knowledge of general zoölogy, drawing, dissecting, zoölogical aids and appliances, and kindred subjects. Taken as a whole, were this volume placed in the hands of such a teacher, and its chief aim to be to impart a notion of general zoölogy to a class of students of the average age mentioned, after faithfully following out its chapters for three or four months, we must believe that no better work has yet appeared having a higher claim to such an end. Its pages are crowded with beautiful cuts of the forms used in illustration of its text, which cuts and illustrations have been for the most part admirably chosen; and, notwithstanding its unavoidable brevity, the subject-matter, as a rule, is presented in a manner calculated to interest and instruct the student at every step. It seems to me, however, that even in a work of this character its author should add a page to his preface, and explicitly state in words and figures and acknowledge to whom he is indebted for his illustrations. We find here numerous drawings of birds taken from Audubon and Wilson, and many others, without a word of such acknowledgment, and the oversight occurs throughout the work. We must believe that even young academical students should be taught that this is not the proper custom; but where an author meets with such material assistance, it should be duly noted. An excellent feature of the work consists in properly dividing and accenting the technical names to assist in their pronunciation; while, on the other hand, a serious defect is evidenced in the absence of a 'glossary of terms' at the end of the volume.

In the main, the classification adopted shows the impress of recent views in the premises; but here, as much as anywhere else, it needs the explanation of a skilled teacher, as the student would gain but a very erroneous idea of the subject from this work alone, as no family nor generic lines are drawn. Take, for example, the order *Passeres*, where lyre-birds, birds-of-paradise, finches, crows, and larks, follow each other in the order I have given them, without a single word of explanation as to their affinities. Then again we find the author at total variance with the leading authorities in placing the bats in the order *Insectivora*, without a word as to why such a step should be taken. Nor will he meet with full support in his order *Bimana*, containing only "one genus and a single species," and that species having "the rank of a being who is alone declared to have been created in the image of God" (p. 277). We have no scientific proof for this latter view. Beneath about half the figures we find given in parentheses each one's proportionate size as compared with the living subject: we regret that this excellent idea was not carried out through the entire work, and it will be well for future text-books in zoölogy to adopt this plan. Written, as the author of this work declares it is, for a class of students as late as eighteen years of age, to my mind it exhibits another thoroughly fatal omission, for it has not a word to say of that great universal law pervading all nature and the world, which explains the very origin of organic forms and the relations of the living ones to those now extinct. Should a young man of eighteen years of age complete the course pointed out by this work, and yet be ignorant of the law of evolution, I hold his zoölogical studies have been but poorly grounded. A companion work to the one under consideration on physics would be in the same case, had it omitted the law of gravitation.

The object of a text-book in zoölogy for a class of students from fifteen to eighteen years of age should not have as its aim the endeavor to teach the greatest number of names of animate objects, for at the present day that is a hopeless task, even were it a desirable end. It should, on the other hand, undertake to make clear the general principles of biological classification; it should by a careful, detailed study of a few types, both vertebrate and invertebrate, clearly point out the universality of morphological laws, then these two lessons should be combined; next, it should be clearly shown the relation between living and extinct types, and finally, by a few clear examples, show the origin of certain forms, as the birds from reptiles, and the ancestry of the horse, and so on; all of which is far more comprehensible than a jumble of isolated facts unconnected by any known law. Such a course, properly expanded and illustrated by a competent teacher, will give a student at once a more intelligent appreciation of life and living forms; make him a better observer; create in his mind a more healthy interest in the subject; and finally send him forth with a kind of stimulation and systematized knowledge which fits him to further pursue biological research, should it happen in any given case to be imparted to the mind of a student cast in the biological mould.

R. W. SHUFELDT.

Fort Wingate, N. Mex., Jan. 9.

The Flight of Birds.

IT is with great diffidence that I take part in a discussion participated in by such eminent authorities as Professor Newberry and Professor Trowbridge, and it is with still more hesitation that I venture to disagree with any opinions brought forward by either of these gentlemen. Nevertheless, I can but feel that undue stress has been laid upon certain facts, while others of equal importance have been overlooked or incorrectly stated.

To a great extent the discussion hinges on the assumption that birds need some mechanical device to relieve the muscles of strain while soaring, — an assumption whose truth seems open to question, as many of the lower animals are capable of automatic muscular movements of very long duration.

Among mammals the cetaceans are almost constantly on the move both by day and by night, while others rest in positions that seem to entail considerable muscular strain. Thus horses very frequently sleep in a standing posture, and the skunk and baboon have been observed to seek repose lying flat upon their backs, with all four legs stiffly extended in the air; a very good example of unrelieved muscular strain may be seen in the tail of the spider-monkey, whose prehensile power is sufficient to sustain the animal after life is extinct; some birds, during their migrations, fly or swim for immense distances without stopping for rest, and there is very good reason for believing that many of the petrels keep on the