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