In discussing the foregoing paper Professor E. H. Miller pointed out the results obtained at Columbia University on the three samples sent out by the committee, which showed a very good agreement with the standard adopted by the committee, considering that they were done, as requested by the committee, by the method usually employed and without special precautions. Method 8, description of which appeared in the *Journal of the American Chemical Society,* for December, 1903, was discussed at considerable length by Professor Miller. The more important points brought out were: (1) That Waring's statement that the silica should be removed before the precipitation of ferric hydroxide was entirely correct, and the failure to observe this precaution might give rise to an error of 1 per cent. as shown by experiments made by Mr. Falk. (2) That Waring's method for the precipitation of zinc sulphide under pressure is excellent. (3) That the statement that zinc and cadmium can not be separated by hydrogen sulphide is absolutely wrong. The separation can be effected in a hot solution (90° C.) containing one cubic centimeter of concentrated HCl in each fifty cubic centimeters of solution, exactly the conditions given by Fresenius on page 457 of the Nacher's translation. That these conditions must be carefully maintained was shown by a series of experiments made by Mr. Falk to test the separation. (4) That a three per cent. uranium nitrate solution is preferable to uranium acetate as an indicator. (5) That the precipitation of zinc as zinc ammonium phosphate is excellent and was confirmed by the experience of fifteen years in the laboratory. (6) That the separation of zinc and manganese by oxidizing agents requires careful investigation and is not free from error as carried out at present.

The remainder of the evening was devoted to 'A Discussion of Radioactivity,' by Messrs. Wm. Hallock, Hugo Lieber, Jerome Alexander, G. B. Pegram and Charles Baskerville.

F. H. Pough,
Secretary.

DISCUSSION AND CORRESPONDENCE.

CONVOCATION WEEK.

To the Editor of Science: The writer has been interested in the series of letters in Science relating to the affairs of the American Association for the Advancement of Science, and he has an idea that the editor is encouraging these communications as a study in psychology. It is evident that the opinions and suggestions are so very diverse that no plan will be satisfactory to the whole membership. In response to the request for a contribution to the discussion the writer will briefly speak of the general outlook and policy of the association, which is the vital matter that involves all the minor questions of meetings and conduct.

During its earlier life, say from 1840 to 1880, the 'advancement' of science through popular summer meetings was doubtless a useful and successful function of the association; but now the diffusion of scientific education and the great volume of scientific literature have supplanted the association for this work of popularization. For ten years the country at large has paid practically no attention to its meetings. The large cities in which the meetings are held are equally indifferent. The leading citizens will allow their names to be used on local committee lists and will subscribe funds to bring visitors to the town, but not one in ten, probably not one in fifty, go to any session or pay the slightest attention to the meeting, although a few may join the society for a time, and thus give financial aid. The advancement of science through publicity of meetings has become an inconsequential element of the association's work. The reasons for this state of affairs are more or less obvious and are not the fault of the society.

The publication of scientific literature was never an important part of the work of the association, and it has become practically nothing, except as indirectly aided through the columns of Science.

While the sections of the society are still active it must be admitted that in the case of several sections this is only through the

cooperation or 'affiliation' of other special and independent societies.

The social function of the association is not alone sufficient to keep it effective. Comparison with the British Association is valueless, since the geographical and social conditions are entirely different.

The old days and old ways of the American Association are gone. It is natural that the older or more conservative members should feel regret over the changes and the evident trend and the loss of the pleasant summer meetings. But it is wiser to recognize the facts and adapt ourselves to the change than to shut our eyes to the handwriting on the wall. Scientific societies can not escape from the rapid and radical social evolution of the time, no matter whether we regard the changes as good or bad.

The association is still moving in a general way along old lines by virtue of its acquired momentum, but new outside forces are pushing it from its course. It is now upheld largely by the tacit cooperation of special societies and by the reverence and affection of men of science for the old, national, parent society. But knowledge is now so vast and diverse and the intellectual and economic forces so strong that specialization is inevitable, and no one society can expect to include the whole field.

The active, successful administration of the society during the last few years has greatly increased its membership, chiefly by securing the adherence of scientific men. This indicates a fundamental fact, that henceforth the association will be and ought to be conducted by and in the interests of men professionally scientific, with less deference to 'advancement,' of science by popular features. The 'American Science Association' would be a better name.

The above seem to the writer as basal facts which must be recognized in any wise planning for the future. The association may remain a great scientific and educational power if rightly conserved and directed. But, no matter what fine schemes individuals may devise, no one can clearly see the path more than one step in advance. The safest way is to trust the matter to a wise council, which should move slowly and feel the way and meet demands for change as they arise.

The association may properly become the central organization or national representative of the many special societies. If the association should be withdrawn from its present relationship to the special societies they would find it desirable to create a central body through which they might act and speak collectively. Some general organization is essential. This idea of the function of the association has already been recognized by giving the 'affiliated' societies the privilege of proportionate representation on the association council. In any scheme of consolidation the matters of association membership and finance are the most difficult to adjust, but they can be arranged satisfactorily when the necessity arrives. As sections are supplanted by special societies their ordinary scientific meetings might be suspended. However, the organization of the sections should be retained, at least for a time, for administrative purposes or other needs which may develop. The chairmanships and secretaryships of the sections would lose nothing of their honorary character because meetings for reading of papers are not held. The presidency and other general offices of the society would be even more dignified as being more broadly and powerfully representative of American science.

For some years the association and the special societies have been drawing together. The wise course is to let the evolution proceed naturally, a step at a time, as the road opens before us, and not to allow any strong personality nor any group of men to force a hasty decision on important matters. We should go slowly, but keep moving.

Here is one suggestion for immediate use. The council should be kept thoroughly representative not only of the association membership but of the affiliated societies. *A strong, wise, harmonious and not too radical council is the most important present consideration.* To this end the sections should be emphatically advised to select for officers and council representatives their most experienced and

wisest men, disregarding at this juncture mere scientific claims. The council should select for presidents at this time not representatives of science as merely to do honor but men of large experience and sympathy with the association affairs. It is of less consequence what the public thinks about the association than what we shall do for ourselves.

Realizing the gravity of the condition at this critical time the council will make wise decisions only after full discussions in a generous spirit; and the membership should in patience trust the collective wisdom of the council.

HERMAN L. FAIRCHILD.

ROCHESTER, N. Y.,
December, 1904.

'THE PROBLEMS OF EXPERIMENTAL PSYCHOLOGY.'

TO THE EDITOR OF SCIENCE: On p. 788 of your issue of December 9 (second column, line 8), I am made to speak of 'classification *a posteriori*.' What I wrote, what the sense requires, what I saw in proof, and what I left in proof, was 'classification *a potiori*.' On p. 794 (bottom of first and top of second columns), I am made to say: 'we analyze and trace to their conditions total consciousness.' What I wrote, what grammar requires, what I saw in proof, and what I left in proof, was 'consciousnesses.' A little knowledge, even in a proof-reader, is a dangerous thing.

E. B. TITCHENER.

CORNELL UNIVERSITY,
December 10, 1904.

[The errors probably would not have occurred if Professor Titchener had returned his proof to the editor in accordance with the instructions accompanying it. It was sent directly to the printers.—ED.]

SPECIAL ARTICLES.

A SUGGESTION LOOKING TOWARDS ULTRA-MICROSCOPY.

THE visibility of an object both to ordinary vision and when helped by telescope or microscope depends upon a favorable combination of several physical conditions. (1) The object must send us ethereal waves whose lengths lie between the limits of 0.38 and 0.76 microns or the violet and red ends of the spectrum respectively. (2) The difference between the intensity or color of these waves and those coming from the adjacent background must be appreciable to our nervous system. (3) The focus on the retina must be sharp. (4) The duration of the image on the retina, or, as the photographer would say, the length of the exposure, must be long enough to enable the brain to appreciate the details of the image.

By means of photography we are able to make long exposures and the fourth condition can be satisfied to such extent that fleeting pictures are caught by instantaneous exposures, while the faintest nebulæ and stars are caught by exposures that last many hours. Becquerel's first photograph by the rays that are called after his name was by an accidental exposure of many days.

By means of the schleier method, originally due to Foucault, we can overcome the difficulties of the second condition and photograph moving air waves when properly illuminated, and this method can be applied to microscopic objects and liquid substances as well as to the larger motions of the air that have been photographed by Mach, DuBois and others.

The ultimate limit of visibility is also defined by the second condition or the wave-length and intensity of the illuminating light that can affect the retina, or the sensitized photographic plate. An object that is visible by monochromatic violet light may not be visible by monochromatic red light or *vice versa*, just as a body that can vibrate to a given high pitch is often too small to send out a low note. An ear that is too dull to hear the low notes may hear a high pitch. Our retina is so constructed as to be insensible to ultra-violet light, but we can by fluorescence make short waves become visible, *i. e.*, an object illuminated by ultra-violet light whose wave-lengths may be anywhere from zero to 0.38 microns may be too small to be directly affected by long waves, but will, by fluorescence convert the short waves into longer ones whose lengths may be any given multiple of the ultra-violet wave, and will,