tent that the average man knows not what to believe, he sees so many contradictory statements, drawings and photographs.

It need hardly be pointed out that little real progress can be made in any branch of scientific work until the fundamental points are placed on a much more secure foundation than are many of the most important details regarding Mars.

It would seem that the best way of finally settling some of these matters would be, as suggested by Professor Aitken, to have them passed upon by a committee of experts of such well-recognized standing as to make their unanimous verdict final and acceptable to all scientific men.

Then, and not until then, will these questions of the surface markings of Mars be upon a dependable basis.

It is also pertinent to point out the saving of time which will result in many ways and to many people by having a sure foundation in this matter.

The financing of such a project should not be at all difficult considering the general interest which attaches to Mars.

C. D. Perrine

Kircher and the Germ Theory of Disease

It would appear from Dr. Garrison's article on "Fracastorius, Athanasius Kircher and the Germ Theory of Disease,"[1] that I am in the usual plight of one who attempts to fix credit for the early suggestion of a scientific theory. Apparently there is always to be found some one who had thought it all out long in advance of—the next man. But though I have no desire to play the rôle of special pleader for Athanasius Kircher, it is only fair to point out that Dr. Garrison does this early investigator an injustice when he says that "Neither Kircher nor Leeuwenhoek could have seen bacteria of any kind with the lenses at their command. . . . His [Kircher's] glass or microscope was only 32 power at best."

Aside from Kircher's apparently loose statement that one of his microscopes showed objects "a thousand times larger," we have no direct data regarding the magnifying power of his lenses. We do know that the simple microscopes of his and Leeuwenhoek's time possessed great magnifying power and that by their use many structures were studied which at present we should not think of examining without a compound microscope. We know, too, that of the several microscopes described or figured by Kircher, one type was fully comparable to those of Leeuwenhoek and, fortunately, concerning the latter we have very full and definite information. One of the Leeuwenhoek microscopes still extant and described by Harting, had a magnifying power of 67 diameters. The twenty-six microscopes presented to the Royal Society of London, by Leeuwenhoek, varied in magnifying power from 40 to 160 diameters. The maximum power of those known is possessed by one still preserved in the Museum at Utrecht, which magnifies 270 diameters.

In the face of these facts and Leeuwenhoek's detailed description of, for instance, the organisms found in scrapings from the teeth, it hardly needs the additional evidence of his illustrations to prove that this worker really saw bacteria. No one believes that Kircher anticipated by some two hundred and fifty years Yersin's and Kitasato's discovery of the bacillus in the blood of plague patients, but I still believe that "There is no doubt that long before Leeuwenhoek's discovery, Kircher had seen the larger species of bacteria" in putrid broth, milk and the like. Imperfect and faulty as his observations must have been, he had definite observation as a basis for his theory of the animate nature of contagion. Certainly, his conception of the rôle of flies in the transmission of disease marked an advance over the theory of Mercurialis.

William A. Riley

Kahlenberg's Chemistry

To the Editor of Science: Inasmuch as possibly a large majority of teachers of first-year college students will agree with Dr. Hopkins in his criticism[1] of Lewis's review of

[1] Science, April 1.

[1] Science, N. S., XXXI., p. 539.

Kahlenberg's "Chemistry," I feel impelled, as one who has had considerable experience in teaching first-year students, to express my hearty agreement with the points made by Dr. Lewis. Let me say, to begin with, that it is not improbable the teacher who deals with the finished product of the one who has done the "first-year teaching" is better capable of judging the success of that teaching than the first-year teacher himself. I have been inclined to judge my own work by the way my students have been able to handle advanced work, rather than by their success with the first-year's work itself. I therefore believe the teacher of advanced students is the most competent critic of elementary work, and that Dr. Lewis is in the best possible position to judge of methods of laying foundations in chemistry.

The more important question at issue, however, which is squarely met by author, reviewer and critic, is whether we shall present the conceptions of modern physical chemistry to first-year students. And it should be remembered that this is not the question of the truth of a theory of electrolytic dissociation, but whether such conceptions as electrolytic dissociation, equilibrium and its disturbance, mass-action, phase-rule and others, which have furnished at least the best working hypotheses for the superstructure of modern chemistry, not merely theoretical, but industrial, shall be *used as fundamental conceptions,* for the first-year, second-year and every other year students; or shall be simply introduced in one or two chapters, apart from all the rest of the subject, as in Kahlenberg's book; or perhaps not mentioned at all in elementary chemistry, being left for some future time, should the student conclude to further pursue the branch. The two chapters in Kahlenberg's book which take up these conceptions might be absolutely omitted without injury to the rest of the book, as far as anything in the rest depends upon these two chapters. Many other older chemistries have been "brought down to date" by adding or inserting new chapters on these so-called modern conceptions. Is it not a little as if one were to modernize a medieval work on astronomy by adding a chapter on the work of Copernicus? Is it not a rather sad commentary on the chemical teaching of to-day when a professor in one of our leading and progressive colleges pleads for the "chemistry of a generation or more ago"? With no intent at irreverence, I can not refrain from quoting the lines that come to my mind from the old hymn,

> 'Twas good enough for father,
> 'Twas good enough for mother,
> 'Tis good enough for me.

Seriously, Kahlenberg's book represents probably the high-water mark of the older chemistry, and especially in presenting "just what the beginner wants to know in the way he wants to have it presented," but is it the neophyte who should be consulted regarding what he is to be taught? In my own case it has been far from an easy task to assimilate the fundamental conceptions of modern chemistry, and I do not desire that any student who goes out from my class-room shall be under the necessity of a complete mental revolution should he pursue the subject farther. It is better, even for the beginner, to study a smaller number of reactions as illustrative of fundamental laws than to make himself master of the great mass of facts of descriptive chemistry with which many of our text-books are filled. Elementary science seems ever to be the last to be influenced by great discoveries and generalizations. Only within the last decade or so have the elementary text-books on the biological sciences been appreciably influenced by the work of Darwin, so we need not be surprised if we find little evidence, even in many of our college text-books of chemistry, of the revolutions in chemical thought wrought by such men as Arrhenius, and Guldberg and Waage, and Mendeleeff, and Gibbs, and others, whose work has been before the world of chemistry for more than a quarter of a century.

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