hundred and twenty hours or thereabouts that form the available allowance in a single term, even after the attainment of a fair knowledge of phænogamic botany. To acquire the necessary skill in the use of the compound microscope will alone consume no small part of the time, and without this nothing of value can be done among the cryptogams.

Again, to tell a class the name of a plant instead of teaching them how to discover it for themselves is to rob the study of much of its special value in training the faculties of observation. This part of the work compels a close and repeated examination of the plant and renders the parts and their names thoroughly familiar as no other method can do it. And speaking from a long experience, I cannot believe that the art can be acquired by less practice than that afforded by the analysis of the fifty or more specimens usually required, unless, as is sometimes, and as should be always done, the description of the plants is made a part of the work. And this description should consist not merely of the filling up of the forms usually supplied, whereby the exercise is robbed of much of its value, but by requiring the whole from the scholar, thereby training him in recollecting what to look for without suggestions or leading questions. No practice in elementary botany is so useful as this.

Of course a part of every class, especially if it is large, will shirk the labor when they are out of the class-room. But shirking in the way suggested can easily be prevented by giving a plant which has no English name and in general by testing a scholar's progress by the work done in the class-room from day to day.

I need not do more than allude to the difficulty, I may say the impossibility, of supplying elementary classes with microscopes of sufficient power for the purpose advocated in the paper here referred to, without which the study must degenerate into a mere absorption of what the teacher tells. This would be little more than a waste of time and a degradation of science to the level of a mere memory study.

On yet one other point I must disagree with this author. There was, some years ago, a disposition to begin the study of a science at the bottom and work upward, and this in spite of strong remonstrances from many teachers of great ability and experience. Even a man like Huxley fell into this error, as may be seen in the early editions of his "Biology." But a few years' test showed the many disadvantages of this method, and the opposite, or older plan has been readopted. Whatever may be urged from the standpoint of theory, practice is unanimous on the other side. Steady advance from the known to the unknown is easier than a plunge into the mysteries of cryptogamic botany with its abstruse terminology and its minute, often almost invisible structure. For every one who might be attracted by the delicacy and difficulty of the subject a thousand would be disgusted and disheartened and would forsake the study forever.

The author's illustration from geology is unfortunate because in teaching this subject the best plan is to begin neither with the superficial nor the deep rocks. This savors of book geology. The proper plan is to begin with whatever rocks happen to lie within the range of the student's investigation. Here again we work from the known to the unknown.

The object of the teacher in every study should be to stimulate to farther advance, and this cannot, I think, be accomplished except by beginning with the easy and the obvious, and by assigning tasks well within the strength of the student. If a fair acquaintance with the structure of the phænogams and the methods of phænogamic botany can be attained in the first term devoted to the study, the time will have been well spent, and neither the teacher nor the average scholar can reasonably expect much more.

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CORAL REEF FORMATION.

IN *Science* for Oct. 20, p. 214, I observe that Professor Perkins gives a succinct account of the history of the theories of coral reef formation. Darwin and Dana have, of course, their proper place in connection with the *"subsidence theory."* Agassiz is justly mentioned as declaring that there was no subsidence in the case of the Florida reefs. Guppy and Semper are very properly mentioned along with Murray in connection with the new views; but my name is not mentioned in that connection. Let me, then, quote from a paper of mine read before the A. A. A. S., Aug., 1856, and published in the Proceedings and also in the *Am Jour.*, Jan., 1857: "On sloping shores with mud bottom, such as we have supposed always existed at the point of Florida, a fringing reef cannot possibly be formed, for the water is rendered turbid by the chafing of waves on the mud bottom; but at some distance (in this case ten to twenty miles), where the depth of sixty to seventy feet is attained, and where the bottom is unaffected by waves, the conditions favorable for coral growth would be found. Here, therefore, would be formed a *barrier reef*, limited on one side by the muddiness and on the other by the depth of the water."

This is positively the first attempt to explain barrier reefs without resorting to subsidence. Captain Guppy worked out the same explanation independently long afterward, but on becoming acquainted with my paper promptly acknowledged the anticipation of his views. I quote from a communication by him to *Nature* (Vol. 35, p. 77, 1886): "When I arrived at the above conclusions I was not aware that substantially the same explanation had been advanced thirty years before by Prof. Joseph Le Conte in the instance of the reefs of Florida. * * * * The circumstance that barrier reefs are frequently situated at or near the border of submarine plateaus receives a ready explanation in the view *first advanced* by Professor Le Conte."

When I wrote my paper I did not dream of generalizing my conclusions or of invalidating Darwin's theory except as applied to Florida. The subsidence theory was to me then, as it is now, the most probable general theory for the Pacific reefs. I am little disposed to make reclamations. Except on the score of history, it matters little who first brings forward an idea. My paper is now thirty-seven years old. In the midst of all these discussions of new views I have been silent. My paper, therefore, has almost dropped out of the memory of the younger generation of naturalists. This is my only excuse for bringing it up now.

JOSEPH LE CONTE.

Berkeley, Cal., Nov. 10.

BOOK-REVIEWS.

Tables for the Determination of the Rock-forming Minerals. By F. LOEWINSON-LESSING. Translated by J. W. Gregory. New York and London, Macmillan & Co. 55p., 8vo., $1.25.

THE literature of micropetrology has of late received an interesting addition in the shape of a translation by J. W. Gregory of F. Loewinson-Lessing's tables for the determination of rock-forming minerals. Unlike the *Hülfstabellen zur Mikroskopischen Mineralbestimmung* of Rosenbusch, or the *Tableaux des Mineraux des Roches* of Michel, Levy and Lacroix, the work is something more than a bare list of the rock-forming minerals with their optical properties, but has for its avowed purpose an attempt to apply to micropetrology the system "so long applied in