Geologists who have convictions as to classification, nomenclature, coloration, or any of the numerous subjects brought before the last congress (which are similar to those to be brought before the next): or who believe that the congress has erred in any of its recommendations: or who have original observations or deductions bearing upon any part of the seven subjects above assigned to reporters, are earnestly requested to communicate their views as soon as possible to the reporter having in charge the subject to which they relate. Those who neglect to do this cannot justly complain if their individual views are neglected in the reports.

GEO. H. COOK,
J. J. STEVENSON,
H. S. WILLIAMS,
N. H. WINCHELL,
E. D. COPE,
EUGENE A. SMITH,
PERSIFOR FRAZER,

Reporters of the American committee, International congress of geologists.

Instruction in natural history.

The recent discussion in the columns of *Science* on the teaching of natural history has revealed so wide a difference of opinion, and leaves the question in so unsatisfactory a state, that an additional word may not be out of place. It seems clear that no discussion of special methods can advance matters until naturalists reach some agreement as to the general educational uses of the biological sciences, yet the lack of such agreement is a conspicuous feature of the series of letters with which we have been favored.

It will probably be agreed that a college course in zoölogy or botany should aim, first, to arouse an interest in animals or plants, and to impart clear and accurate knowledge of them; and, second, to cultivate the power of independent observation. But, after agreeing that both these ends must be held constantly in view, we must still decide which of them shall be foremost. Which is the higher ideal of scientific study, — to have students, first of all, learn to use their own eyes, and not simply to verify some one else's description, or to weigh and discuss the nature, meaning, and causes of the relative affinities of organized beings? It is plain enough that independent observation by the student is the only method that can give life and reality to the study. It is no less certain that a main claim of natural history to a place in education rests on the value of the training afforded by observation; and we have the explicit statement of high authority that 'the first thing is to learn to observe.' But, in full view of these facts, let us suppose that an intelligent non-specialist has the hardihood to ask, "*Is* observation the first thing; or is it not, after all, a *means* rather than an end in itself?" Unless we are ready to admit that natural history is a mere drill, the answer must be that its real aim is to teach something, first, of the special phenomena of life; and, second, of the generalizations of biological science illustrated by them; and the problem to be solved is how to make this instruction most effective as an instrument of education.

Now, it is undoubtedly an effective lesson to the future naturalist to be made to stare at one dead fish for three long days, and to classify Haematon solely by the light of nature; but is such a lesson likely to develop the latent scientific tastes and capabilities of the average college sophomore? I think not; and, while no one would seriously advocate such a method for college classes, it may reasonably be asked whether the reaction against the dull and barren cramming of text-books may not sometimes carry us from one extreme to the other, and even close our eyes to the fact that the student of natural history is a rational being, who really possesses a degree of common sense comparable with that of students of other sciences.

It is my decided opinion as a practical instructor that the methods so successfully employed in elementary instruction in physics and chemistry may guide us to the true method of teaching natural history. No teacher of chemistry would commit the absurdity of setting apparatus and chemicals before the beginner and directing him simply to 'experiment' It is generally admitted that the beginner should receive precise and somewhat detailed instruction before or during the laboratory study, and that he is thus enabled to work with interest and intelligence, and to *gain time*, without loss of independence. It would be hard to find any valid reason why this is not equally true of the beginner in botany, zoölogy, or physiology. Moreover, every teacher knows that students possessing a good degree of mental power and general intelligence are not seldom more or less deficient in those practical capabilities collectively known as 'gumption.' Why should such students be compelled at the outset to fritter away valuable time in the discouraging attempt to make independent observations, which usually result in vague and confused ideas and a distaste for the study? I believe that *beginners* in natural history should be prepared for the laboratory by a clear and tolerably full account of what they are to do and see; and the more books and figures they have, the better. Afterwards, when the strangeness has worn off and a certain facility has been acquired, students can be led naturally and easily to depend more and more on themselves, and to find a pleasure and profit in independent work that was impossible at the start. Whatever be the comparative merits of such a method, there is no doubt, as a matter of experience, that it arouses interest, and gives fulness and accuracy of knowledge; that it saves time for the student, and cerebral protoplasm for the instructor, and as a matter of fact does *not* make students slavishly dependent on books or demonstrators, but, on the contrary, tends to develop independence and originality. It has been said, truly enough, that you cannot teach a boy mountain-climbing by taking him up Mount Washington on a railway. Neither can you teach him by leaving the youngster at the foot of the Alps with the parting injunction to climb immediately to the top.

X.

April 25.

Barometer exposure.

The question of barometer exposure has been prominently brought to the front by *Science*. On the one hand, it has been claimed that the wind, in blowing across the mouth of a chimney, would at times produce a vacuum amounting to .10 of an inch; and, on the other, it has been denied that any marked effect would occur, as the air would flow in through cracks, especially on the windward side, and fill up the partial vacuum, if such were

formed. Most of the observations relied upon for proving this effect have been the traces of a barograph recording upon Draper's principle, and there have been very few actual observations of a barometer. Quite recently there have been observations of a barometer, under varying conditions, on Mount Washington, with wind-velocities of eighty and ninety miles. The results have been published in the *Monthly weather review* of the signal service, for February, 1887, and are so interesting that a brief review of them is here given. The chimney in the signal office on Mount Washington is about two feet square, and has three inlets into the office-room. One of these is a ventilator near the top of the room, and the other two have stove-pipes running from three stoves. It is quite evident that the chimney has a fair communication with the room. The experiments consisted in reading a mercurial and an aneroid barometer, 1°, chimney closed; 2°, chimney opened; 3°, same as 1°; 4°, leeward window open; 5°, same as 1°; 6°, windward window open; 7°, same as 1°. The successive readings were made quite rapidly, though generally three or four minutes elapsed between each of the seven conditions. Five sets are published with the wind from sixty-five to ninety miles per hour. Under 2° (chimney open), the pressure fell twice mean −.0065 of an inch, and it rose three times mean +.0037. Under 4° (leeward window open), four times the pressure fell −.019, and once it rose +.002. Under 6° (windward window open), the pressure rose mean .043. Making due allowances for imperfect connection between the chimney and the room, it must be admitted, I think, that there is no evidence of a partial vacuum being formed by the suction of winds, up to sixty-five and ninety miles per hour, blowing across the chimney.

The most interesting results, however, are those with the window open to windward. In an eighty-mile wind, experiment would indicate an increase of pressure of about .44 of an inch, but here we find the total effect one-tenth of that. It seems to me that the effect of wind on the barometer has been much exaggerated, and we may rest assured that our observations during very high winds have not been vitiated so very much. It may be of interest to note that this same slight 'pumping' or uneasiness of the barometer was noted by Mr. Beall, the observer on Mount Washington in 1883. In making his comparative readings of the station and extra barometers at the end of each month, he found it necessary to exercise the utmost care and speed in order to make correct readings during very high winds. The total oscillation seldom reached .01 of an inch.

H. ALLEN.

Washington, D.C., April 25.

The barometer during thunder-storms.

My attention has been called to the fact that the time given for the squall of July 21, 1885 (printed '1886' by mistake in your last issue), did not agree exactly in time with the sharp depression of the barometer shown on the diagram accompanying my letter on p. 392. This was due to an error in the barograph clock, which was then new, and not well adjusted. Mr. Alexander McAdie, who had charge of the station on that day, and Mr. Frank Brown, were watching the barograph during the squall, and both state that the depression of the barograph was coincident with the occurrence of the squall. The squall was so violent that Mr. McAdie wrote that 'life for a while did not seem certain.'

H. HELM CLAYTON.

Blue Hill meteor. observ., April 23.

The source of the Mississippi.

I am in receipt of a pamphlet, entitled 'The source of the Mississippi,' from Ivison, Blakeman, Taylor & Co., and am pleased to see therein that the laurels deservedly won by Nicolett and others are maintained to them. My father, Bazil H. Beaulieu,— who had charge of a trading-post on Lake Itasca in 1846 for the American fur company, and who in 1847 accompanied, as assistant geologist, the first geological party (Dr. Norwood, Whittlesey, and others) that went over and drafted Itasca and Elk lakes in going to Red Lake, and went over the lakes again on their return, and also drafted the Mississippi from its source to Dubuque, Io., — concurs in the opinion, as established by the late survey, that Nicolett was the first man that gave to the world of science a faithful and honest report upon, and maps of, the source of 'Gitche-tebe' (or 'mighty-water') River, — the Indian term for the Mississippi. It certainly seems shameful that the vain ambition and venturesome spirit of the Captain Glazier stamp should seek at this late day to aspire to and appropriate to itself laurels nobly won by deserving men in the cause of science half a century ago.

THEO. H. BEAULIEU.

White Earth, Minn., March 21.