

the district where they would be most useful and instructive. Private collectors would probably show more public spirit, if greater zeal and better judgment were shown by local societies.

F. G. S.

Science Teaching in Schools

IN the number of NATURE for April 20, there is an article containing an account of a "Plan for Teaching Science in Ordinary Schools, submitted to the London School Board by Mr. J. C. Morris."

I will ask you to give me a little space for some details respecting an educational experiment I made in 1867, 1868, and 1869. My object was to test the value of a plan much resembling that referred to. By means of circulars, addressed to more than a hundred of the London clergy, I obtained permission to have the children in seven large schools instructed in science. Four competent teachers put their services at my disposal. One of these gentlemen is now chemist in iron works, two are art masters, and the fourth, having obtained one of the Whitworth Scholarships, is a student at Owen's College. I mention these facts to show the sufficiency of their knowledge. Three of them had had considerable experience in teaching. Twenty-two classes were formed, the total number of pupils exceeding 800. The principal subjects taught were chemistry, geology, physical geography, practical geometry, and mechanical drawing. The lessons were from one to two hours in duration on two days in the week at each school. But my plan differed from Mr. Morris's, inasmuch as thirty-five to fifty-five lessons were generally given in a subject before proceeding to a new one. He suggests that "a single teacher could get through three or four subjects annually, so that in two or three years he would have completed the full course in each school." This plan would give from twenty-two to thirty lessons per subject if I rightly understand his meaning. We fixed a small fee, but seldom obtained it, as we found that any attempt to press for payments would have reduced very materially the numbers in the colleges. The pupils were frequently examined, and those who appeared likely to satisfy the minimum requirements of the science department were sent in to the May examinations.

The following are some of the observations I made at the time:—

1. Few of the children appeared to obtain anything like sound and comprehensive knowledge of the facts the teachers put before them.
2. The great majority failed to express clearly on paper any ideas which an oral examination showed they had gained.
3. Most of them appeared to forget a subject within a few weeks after the discontinuance of instruction, or the substitution of another branch of science. The utter forgetfulness shown by whole classes was sometimes almost startling.
4. The papers worked by the girls at the examinations were superior to those produced by the boys, showing a more intelligent knowledge of the subjects they had been taught. This fact may, however, have resulted from accident, as comparatively few girls received instruction.

T. JONES

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Ocean Currents

MR. LAUGHTON does not seem to observe that the subject of Ocean Currents involves several distinct issues, which may be discussed apart from each other. It is, of course, obvious that if the temperature explanation of the vertical circulation fails, then no illustration of the horizontal circulation, if founded on the temperature theory, can be really effective. But it is admissible to inquire separately whether the horizontal circulation *would* result from a vertical circulation such as the temperature theory suggests. For an objection has been urged against the theory on account of the nature of the horizontal circulation (see Herschel's "Physical Geography.") The express object of the experiment I have suggested is to show that this particular objection is unsound, or rather to illustrate the theoretical considerations argued in my essay on the Gulf Stream in the *Student* for July 1868.

But even in so far as my suggested experiment, like the similar one carried out by Dr. Carpenter, illustrates the production of a vertical circulation, I deny that Mr. Laughton's objection is valid. It is quite unnecessary to have a thermometric gradient resembling that in the terrestrial oceans. Whether Dr. Carpenter's view be correct, according to which the Arctic regions are

the place where the Ocean Currents have their birth, or whether the view I have advocated be preferable, that the chief source of the oceanic circulation is to be recognised in the effects of tropical and subtropical heat, it is clear that we are rather concerned with the integrated effects of one or other cause (or of both causes combined) than with the amount by which temperature increases per mile of distance towards the equator. As I have already remarked, I conceive that any reasoning by which the contrary could be maintained would subvert the accepted and surely sufficient explanation of the trade and counter-trade winds. (The experiment described in illustration of this explanation in Daniell's Meteorology is open to much graver objections than Mr. Laughton has urged against Dr. Carpenter's experiment.) And I note that here Mr. Laughton agrees with me, except that on the strength of his thermometric gradient he is as ready to give up one theory as the other, whereas I see no objection to retaining both.

The very word "gradient" should suggest the true answer to Mr. Laughton's reasoning. A gradient of one in ten (say) will produce little velocity in a rolling body traversing such an incline for a distance of only a few feet, but if the incline be a few miles long the body rolling down it would acquire a velocity exceeding that of our swiftest express trains. Or again, suppose Dr. Carpenter, desiring to illustrate the subject of springs of water, employed a conduit-pipe inclined 45 degrees to the vertical, would it be any valid objection to the illustration to urge that in most natural springs the water gradients are very much less? He could surely answer that the principle of his illustration was in no way affected by this circumstance, for if the water-gradients in nature are small, they act over a much longer range than could be employed in his experimental illustration. So with Mr. Laughton's temperature-gradients; they are very small indeed, but their action extends over a very great distance; and as in the two former cases the total fall measured vertically is to be looked upon as the true cause of the resulting motions, so I conceive that the total difference of temperature between Polar and Equatorial waters is to be considered in discussing the temperature theory of oceanic circulation.

I note, by the way, that "solar light" (by misprint or through a *lapsus calami*) was substituted for "solar heat" in my former letter. I did not think it necessary to correct this earlier, as I imagined the error would mislead no one. Like Mr. Laughton I "do not see what effects solar light can ever be supposed to produce," on the ocean, at least, in producing circulation.

I venture to remind Mr. Laughton that Dr. Carpenter's position in this matter is very different from his or mine. We have theorised on this subject, whether with more or less soundness time will show. But Dr. Carpenter has brought striking and important facts to our knowledge; and if there has been "an air of triumph both in Dr. Carpenter's lectures and writings" about ocean currents, he has had better cause for triumph than the mere success of a lecture-room experiment could have afforded him.

RICHARD A. PROCTOR

Brighton, July 21

Western Chronicle of Science

I WOULD beg to be allowed one or two remarks with reference to the very favourable review of the "Western Chronicle of Science" which appeared in last week's NATURE.

It is not a "common Cornish habit to hang heavy jackets, great-coats, &c., on the lever of the safety-valve," and the farmers do not, *as a rule*, "mix guano with lime a few days before applying the manure." The editor has seen both these absurdities performed, and has used them as beacons to warn young men what to avoid. I may also remark that Mr. Williams's Paper is on Scientific *Mining* and not Scientific Nursing.

Falmouth, July 22

J. H. COLLINS

Formation of Flints

NOTHING can be more annoying to a reporter than to find he has not satisfied those whose statements it has been his duty to condense. I have therefore carefully examined the report to which Mr. Johnson takes exception in his letter to you of the 11th inst., and I regret that I am unable to acknowledge any error.

If Mr. Johnson will be good enough to consult some of those who were present at the meeting to which he refers, he will, I think, be more inclined to admit the accuracy of the report.

THE WRITER OF THE REPORT