civilian experts."— To answer these points in order, I will say, first as a matter of history, that the 'plan' of the coast-survey was compiled over forty years since by a mixed board composed in part of navy officers. This plan was legalized by Congress in 1843–44, and has been mainly in force ever since; though some modifications have necessarily been made by the judgment and experience of the eminent men who have held the offices of superintendent and principal assistants. By the plan referred to, it was made the *legitimate* duty of officers and men of the navy to execute the hydrographic part of the work; and to them has ever since been assigned the bulk of that work, except during the few years when the civil war and the subsequent scarcity of officers made it impossible to do so. That period (i.e., from 1861 to 1871) developed a good many civilian hydrographers who have no superiors in the world, but nearly all of these resumed their more legitimate work upon the return of navy officers to the survey. The *methods* of hydrography are the growth of hundreds of years, and have been contributed to by the seamen of all maritime nations; and, while the inventors of a good many instruments and special methods are known, it would be exceedingly difficult to trace the *system* to its source. The 'tricks of the trade,' so to speak, have been handed down from one to another with gradual improvement, — as a rule, too slow to give any definite point from which that improvement can be shown, though during the forty years of its existence the coast-survey has vastly improved the character of its work; but probably the improvement in its means (i.e., the introduction of steam-propelling power, etc.) deserves a good deal of the credit for improved methods. While civilians have had a share in the development, it is a long way from the fact, to ascribe all to them, as it is to assume that hydrography is a work which does not require skill, judgment, and care. Those who think the last have never worked in intricate waters. The officers engaged upon the coast-survey have been so assigned because it was a part of their regular duty, and not because 'superfluous.' Having had for five years the privilege of nominating the officers to be employed upon the coast-survey, I can speak with some authority. Officers were chosen strictly for their qualifications; and often, had it not been for the great interest taken in the coast-survey by the successive chiefs of the bureau of navigation, the officers selected would not have been spared from other duties. That all work of the coast-survey is supervised by the superintendent, an expert of high order, is an undoubted fact; but his instructions to hydrographers, unless he has some special object in view, simply assign geographical limits, but do not prescribe methods, a general printed manual covering all that is required in the latter. The work, after completion, has of course to pass the rigid scrutiny of the superintendent; but the same is the case with all other work. To this extent the work of navy officers may be said to be 'supervised by civilian experts,' but no farther. In 1873 several navy officers, who without previous experience were ordered to the coast-survey, placed themselves for a short time under the instructions of civil assistants, who had been doing their work for some years; and all of them freely and gratefully acknowledge the assistance they received. I am free to acknowledge obligations of a similar character,— of many a *point* received from my valued civil associates during the Darien Canal expedition of 1870. Nautical surveying has always been taught theoretically at the Naval academy; and as much practice as possible has generally, though not always, been given. Furthermore, nautical surveying and navigation are very near cousins, so that all the instruction needed to make a navigator a surveyor is to give him what I have called the 'tricks of the trade;' and these are being handed down by officers as they have been by their predecessors.

EDWARD P. LULL,
Captain U.S. navy, late hydrographic inspector U.S. coast geodetic survey.

[The plan of organization of the coast-survey and the plan of work of the survey are quite different things. It is the duty of the chief of the survey to arrange and supervise the latter. That the scope and character have been extended since its organization in accordance with the views of the chief is beyond question. While from the above letter it might be inferred that the nautical work of the coast-survey is confined to marine surveying in its older sense of locating rocks and shoals, and determining the boundaries of courses of the navigable waters by time-honored methods, yet from the publications of the coast-survey, and from other sources, we had gathered that the study of ocean physics, and of the conformation and character of the ocean bottom, together with the different forms of marine life, had formed, of recent years, an important part of the work of the survey, and that it was carried out in accordance with the plans of the chiefs of the survey, and by the methods devised and developed by them and by the two Agassizs, Pourtales, Thompson, Milne-Edwards, and many other eminent specialists, modified in minor details by the circumstances of each case.

It is an error to suppose we regard the employment of naval officers in this work unfavorably; for, on the contrary, we think it highly desirable that they should be employed in this routine work of collecting data and material for discussion and study by specialists; and their skill, judgment, and care, their knowledge of organization and discipline, and their close adherence to instructions, render them extremely useful. It is wise, also, that, in the present reduced condition of the navy as to ships, and its overcrowded condition as to officers, the secretary should find employment for this superfluity in the coast-survey, the fish-commission, the geological survey, the national museum, as instructors in our colleges, and as assistants in special researches. Such employment cannot but result in benefit to the navy, and assist in the advancement of science.

Yet we have still to be persuaded that it will promote the efficiency or the economy of the scientific organizations of the government if they are transferred from the supervision of the present expert civilian heads to that of the officers of the navy.]

Italics for scientific names.

I agree with the editorial remarks under this heading in *Science*, No. 49, that the proper mission of italics is for 'emphasis, or as catch-words;' and their use for scientific names of animals and plants is, it seems to me, — contrary to the opinion conveyed editorially, — of great practical utility, especially in indexing, or in searching the pages of an article or memoir for references to particular species that may be under treatment. Italicizing such words makes them 'catch-words,' and gives great facility in discovering incidental reference to species, the eye quickly catching the italicized name, and as quickly recognizing whether it is the one sought. Considering scientific names as 'a simple convenience,' and as having no higher value, their use is so necessary as

a 'handle to facts,' or as names of objects of which we have to speak, it seems desirable to have them so typographically distinguished that their presence on a printed page will quickly catch the eye as guide-posts to the subject of the immediate context.

J. A. ALLEN.

Cambridge, Mass.

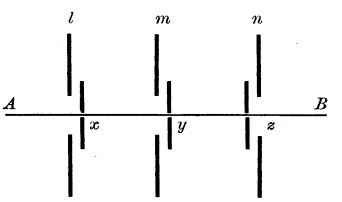
[The editor has yet to be convinced that typography should be moulded to suit the purposes of an indexer.]

Eating horns.

Indians eat the horns of the deer when in the velvet. One day on the Sioux Reservation, in Dakota, a deer was killed near camp, and brought in entire. At sight of it, Pahlani-ote, a Minneconjon of some fifty years, dropped his usual statuesque attitude, knocked off the horns, and, seating himself by the fire, began at the points to eat them, velvet and all, without cooking, as if they were most delicious morsels. The others of the party looked on as if they envied him. They said they always ate them so. S. GARMAN.

Radiant heat.

In a letter to *Science* of Dec. 21, 1883, Dr. Eddy has endeavored to show that I was mistaken in thinking that his proposed arrangement for proving that radiant heat is not subject to the second law of thermodynamics would not work.



I can most easily explain how Dr. Eddy is again mistaken by referring to my diagram which he reproduces in his letter. Dr. Eddy says that *every* time the door z is opened two quantities of heat pass into the region B, one of which had originally come from A, and the other from B. I had assumed that the occasions when it opened to let heat that had come from A pass were different occasions from those when it opened to let that from B pass. I assumed this, because I could see no way of getting the heat that had come from B back again through z in the same direction as it had come out, except by a reflection from the back of y; and of course that required y to be shut at the time of reflection, so that this heat could not reach z at the same time as any heat that had originally come from A. I have been unable to think of any method of getting the heat from A and what had come from B to travel *simultaneously* in the same direction; and I am inclined to think, that, if this were possible, Dr. Eddy's doors, etc., would not be required to enable A to radiate more heat to B than B does to A. This supposed arrangement might, as far as I can see, go on working continuously, returning the heat to B, and simultaneously transmitting that from A; for this seems to me to be what Dr. Eddy postulates as possible.

If the two quantities pass into B through z in two different directions, then two other quantities will escape from B in these two directions, and B will be in exactly the same condition as it would be according to my hypothesis that they passed into B at different times.

Dr. Eddy confesses to being unable to see how to accomplish what he postulates with my arrangement of screens and apertures; and I believe that the only reason he is unable to do so, and imagines that his own proposed whirling tables would do so, is because my arrangement is so much simpler than his, that it is almost impossible to be misled as to where and when the heat comes in and goes out; while, with his arrangement, he has so many holes that it is almost impossible to keep before one's mind all that is supposed to be going on. I cannot see how my simple arrangement is less general than Dr. Eddy's complicated one, as it seems to me that a multiplicity of holes cannot be of any real use, while they produce very serious complication; and, except in the number of holes, I think Dr. Eddy's arrangement only differs from mine in that his supplies a mechanism for opening the apertures, which, of course, has nothing to do with the question. If Dr. Eddy will explain how he manipulates so as "to bring the heat coming from A into a position such that it would be in readiness to pass into B at the same time," *and in the same direction*, "as the heat which originally came from B is returned to B," and does not rest upon the authority of Professor Gibbs that his arrangement does so, then I will agree that he has invented an arrangement by which the second law of thermodynamics may be cheated. GEO. FRAS. FITZGERALD.

40 Trinity college, Dublin,
Jan. 7, 1884.

Professor De Volson Wood makes statements in his letter published in your issue of Jan. 11 which appear to me unsupported by facts. Were your columns open to a lengthy discussion, I should like to show this in detail. Suffice it to say, that in his reference to Mr. Fitzgerald's construction he entirely overlooks the difference between radiant heat, which must be moving along given lines in a determinate direction, and other heat. The heat referred to as 'entangled in the space $m\,n$' is radiant heat alone. I have definitely traced its path, and shown that it does not move as Professor Wood states. Instead of regarding this fact, he has attributed to it the properties of heat as ordinarily existing in matter.

Professor Wood also refers to his papers in the *American engineer*, etc. The only point in that somewhat lengthy and personal discussion upon which I understand Professor Wood to finally insist, he republished in the *Journal of the Franklin institute* for May, 1883. In my reply in the same journal for June, 1883, I showed the fallacy of his objection. So far as I know, Professor Wood has taken no notice of that reply, and now completely ignores it. I may say that the proof he relied upon was of this nature. He proposed a certain construction or process (differing essentially from mine) for dealing with radiant heat, and one which would not accomplish the end sought. He then showed that his construction was a failure, and concluded that mine would therefore fail also, — a method of reasoning which seems to me inconclusive, to say the least. And now Professor Wood says that Mr. Fitzgerald's construction is 'conclusive.' All it is conclusive of is, that it will not accomplish the end which I have proposed: we all agree that it will not. I have shown, however, that my proposed construction differs from both in just those particulars necessary to make it accomplish the end sought.

It is unfortunate that the velocity of radiant heat is such as to render experimental verification a matter of great difficulty. H. T. EDDY.