balances the difference of pressures. At that height it remains, and no further movement takes place, so long as the relative pressures remain the same. A fluctuation of the pressures will give rise to alternate ingoing or outgoing currents, but a continuous stream in one direction can only be produced by a continuous increase or decrease of one or the other pressure.

But independently of the equilibrium, which must, so far as the pressure is concerned, be established, the difference of level caused by these differences of pressure is extremely trifling. The barometric difference between the patch of high pressure in latitude 30° and the equator is about $\frac{3}{10}$ths of an inch, or equivalent to a column of water 4 inches in length. A difference in level of 4 inches in 1,800 miles can scarcely under any circumstances give rise to a current of twenty miles an hour.

J. K. LAUGHTON

Royal Naval College, Portsmouth, Jan. 23

IT is singular that diversity of opinion should still exist as to whether ocean currents are due to the impulse of the winds, or to difference of specific gravity. That ocean currents are not caused by difference of specific gravity between the waters of equatorial and polar regions can be proved, as the amount of force from this cause acting on the ocean to produce a current, can be readily calculated.

Assuming, which is not the case, that difference in saltness between the water of equatorial and polar regions does not in any wad tend to neutralise the effect resulting from difference of temperature, in other words, that the sea in polar regions is as salt as the sea in equatorial regions, it can be shown that the force resulting from the difference of temperature, tending to produce a current towards the poles, amounts to only $\frac{1}{1820000}$ of that of gravity. For example, the force impelling a cubic foot (64lb.) of sea water at the surface of the ocean towards the poles is scarcely equal to the weight of one-fourth of a grain.* A force so infinitesimal, acting on a fluid even so perfect as water, can produce absolutely no motion. M. Dubuat found by direct experiment that it requires a force four times greater than the above to produce even sensible motion.

Ocean currents are due alone to the impulse of the wind. In the latter half of my paper on the Cause of Ocean Currents, which will shortly appear in the *Philosophical Magazine*, I hope to be able to show that the objections to this theory are founded upon misconceptions regarding the way in which winds produce the great system of oceanic circulation.

JAMES CROLL

Dr. Frankland's Experiments

IN last week's NATURE Dr. Frankland describes some experiments, apparently under the impression that they were similar to one (No. 20) published by me in NATURE, No. 36, p. 200. The results which he obtained were in reality totally different from those which I obtained, although those who read Dr. Frankland's communication are lead to believe that the results were almost wholly similar. The inference to be drawn from what he has written is that we differ merely as regards the interpretation of the nature of what was seen.

Dr. Frankland says he made use of tubes of "hard Bohemian glass," and that, on examining them "when they came out of the digester, it was evident that the *interior walls of the glass tubes had been corroded* by the enclosed fluid." After a time the "liquid in all the tubes became more or less turbid, and, in some cases, a small quantity of a light flocculent precipitate subsided to the bottom." After five months two of the tubes, which exhibited "the greatest turbidity," were selected for examination, and the "flocculent sediment" in the tubes was more especially subjected to a careful microscopical examination. This scrutiny was conducted by Professors Frankland and Huxley and Mr. Busk. Dr. Frankland then says: "So far as the optical appearances presented by the sediment go, they may be appropriately described in the terms which Dr. Bastian applied to the matter found by him in a solution of like composition, and similarly treated."

Now, that any real similarity did exist, I feel most strongly inclined to doubt, because the solution examined was not similar in constitution to my own, and because no such "flocculent sediment," as that to which Dr. Frankland alludes, ever existed in my flask.

In the experiment of mine to which reference is made, the precise quantities of carbonate of ammonia and phosphate of soda employed are not known. In this first experiment the ingredients were not weighed, although, subsequently, solutions have been prepared for me of the strength which Dr. Frankland names.

Then, although my tube with its contained solution was exposed to the same temperature as that employed by Dr. Frankland, its internal walls were not in the least corroded, and no "flocculent sediment" appeared in the solution. And, in addition, two other tubes which were prepared for me by Dr. Frankland (which did contain solutions of the same strength as those which he employed) have not had their transparency in the least impaired, although they were submitted to precisely the same temperature; neither have they shown a trace of the "flocculent sediment" previously mentioned. Seeing, however, that in one experiment (May 11th), with a solution of the same strength, a tube of English glass was employed by Dr. Frankland's assistant, and that the internal walls of this tube were corroded; and seeing, moreover, that a "flocculent sediment" did form also in this particular tube of mine, I cannot help fancying that Dr. Frankland may be mistaken as to the nature of the glass employed in his experiments. If, as in this experiment of mine, it was really English glass instead of hard Bohemian, almost the whole of the small quantity of phosphoric acid originally in the solution would probably have been deposited in the form of an insoluble phosphate of lead, and thus the character of the solution would have been entirely changed.

In my previously published experiment the fluid was examined at the end of thirty days. "When this flask was received from Dr. Frankland, the fluid was somewhat whitish and clouded. During the last ten days a thin pellicle had been seen gradually accumulating on its surface, and in the latter four or five days this increased much in thickness, and gradually assumed a distinct mucoid appearance. The fluid itself was tolerably clear, though an apparent turbidity was given by the presence of a fine whitish deposit on the sides of the glass. When the flask was opened the reaction of the fluid was found to be *neutral*. Portions of the *pellicle* were at once transferred to a glass microscope slip," &c. (NATURE, No. 36, p. 200.) In portions of this pellicle were found the "*five spherical or ovoid spores*," upon the finding of which alone I laid any stress as indicative of the presence of living things. The presence of mere particles having a movement indistinguishable from Brownian movements, has never been adduced by me as evidence that living things had been evolved in a solution, although the representations of others would lead the public to believe that I have done so.

In the face of these differences, therefore, I was somewhat surprised at the intimations contained in Dr. Frankland's letter. He believes, and would lead your readers to believe also, that the microscopical appearances presented by the "flocculent sediment" and *débrés* of corroded glass obtained from his tubes were similar to the microscopical appearances of a *pellicle* obtained by me from a tube in which there had been no corrosion. This, however, I am the less inclined to believe, because I also have had the opportunity of examining a flocculent sediment and *débris* of corroded glass from a tube previously referred to, which was opened on October 21, and in which also no living things were found. Microscopical specimens of the "pellicle" and of the "sediment" are now in my possession.

Perhaps I may venture to recommend Dr. Frankland to destroy the other two tubes which are corroded, as being worthless, and to hope that, in any future experiments, he will subsequently expose his fluids to a somewhat higher temperature, and also, before immersing his experimental tubes in any fluids, that he will thoroughly satisfy himself as to the transparency of such fluids to the actinic or chemical rays of light. We are informed that his tubes were "exposed to bright diffused daylight, and sometimes to sunlight," but any amount of exposure to light would be more or less useless if strong sulphuric acid and strong carbolic acid are as black to the chemical rays of light as nitrite of amyl and other fluids have been shown to be. Dr. Frankland makes no statement concerning this very important point.

H. CHARLTON BASTIAN

20, Queen Anne Street, W., Jan. 22

The Tails of Comets, the Solar Corona, and the Aurora, considered as Electric Phenomena

MY attention has been called to a rudely worded attack by a certain Mr. Bedford, Phil. D., on Professor Reynolds, of Owens College. This is not the first time Mr. Bedford has offended in this way. Prof. Reynolds has not seen Mr. Bedford's pamphlet.

* *Philosophical Magazine* for October 1870, p. 249.