opposition to physiological, chemical and anatomical facts, which recognize a vast difference between these two organs.

The preference certainly is due to that mode of investigation which casts away all artificial proofs of closer relationship between the muscles and the electric organs and regards them as independent and well-authorized members. Indeed, the reasons given by those who accept an especial connection between the two are formed merely upon the identity of this relationship with the nervous system, and not upon any similarity to the actual qualities peculiar to these organs. Such grounds, however, can have no importance as regards this question and can bring the electric organs no nearer to the muscles than to the organs of sight or any other organ.

But are the electric organs really so independent and isolated in the animal organization? And to what freak of nature are we indebted for the remarkable fact that out of all the fish that exist, only three are distinguished by such powerful weapons? The theory of evolution which now rules organic natural sciences, always has a well-tried domestic remedy on hand for such questions. This theory discovers in formations like the electric organs which stand out as prominent exceptions to the conformity of animal construction, the distinct remains of a powerfully developed species belonging to an early epoch of geology, or, in other words, the solitary descendants of a once mighty family. According to this, the appearance of the electric organs in the three fish may seem much less mysterious, and the great anatomical diversities which exhibit themselves throughout these organs are, perhaps, best explained by the idea that in these fish we have before us the final issue of a powerful species, the last remains of an extinct family. That such a family did exist is proved by the discovery of a petrified torpedo in the tertiary strata of Monte Bolca in Verona.

But also in the cotemporary creation the electric organs are not so badly devoloped as a superficial observer might suppose. In the non-electric torpedo of the *Raja* species, and also those which are found in the African rivers, peculiarly constructed organs have been discovered from which an electric effect cannot be produced, but which, nevertheless, are composed of strata similar to the real electric organs. These may, perhaps, be correctly termed electric organs, which are either newly constructed or else in a state of incomplete development.

The materials so far collected by anatomists and physiologists concerning this question do not admit of a marked decision. The organs present many things in common with the electric strata it is true, but beyond this further investigation seems useless.

In one other respect physiology is likewise unable to give a definite explanation. E. du Bois-Reymond was the first to ask how it happened that the electric fish was not the victim of its own power, and how it was possible that the forcible electric discharges which killed other fish completely escaped the electric fish itself.

Now we all know that the nerves and muscles of the electric fish are excited by means of an electric current, and a much stronger one is perhaps required here than would be the case with other animals, yet the electric discharges, although of such force, produce no effect whatever upon the fish. There are influences at work here, which so far we are unable to understand. We naturally suppose, however, that the great dimensions of the nerve fibres and ganglion cells, together with a vigorous nervous system, have a great deal to do with it.

In conclusion, it still remains for us to put the greatest question of all concerning the electric fish, namely: what is the origin of that powerful force which at the creature's will so suddenly appears and departs with equal rapidity, and also what is the precise mechanism of the electric organs?

It has been shown that as science advanced, the electric fish became better known and more carefully studied. The ancients were only aware that such a thing existed; a conviction, however, that they were incapable of analysing further. Redi taught us to consider the electric organs as the apparatus which produced the effect. E. du Bois-Reymond put the electric strata in place of the electric organs, by proving that the mechanism of the latter was reduced to the combined action of countless analogous electro-motory monads, which was explained by the supposition that when the electric discharge occurred one part of the strata was positive and the other negative. By this means our question concerning the mechanism of the electric organs is partially answered. It now remains to ask what takes place when the electric discharge occurs?

Now, in order to imitate the effects produced by the maloptternus, it requires the strongest electro-motor apparatus that can be found. The natural philosopher must use the most powerful batteries contained in his labratory, if he wishes to *approach* the force which causes 2½ pounds of water, salt and albumen to come under its influence.

The muscles are no less powerful. The dorsal muscle of a frog consists of a few grammes of water, salt and albumen, and yet it is capable of lifting a kilometre. In both cases an extraordinary development is apparent, mechanical in one and electric in the other.

Hitherto, no one has succeeded in correctly establishing the facts relating to this mechanism. Nevertheless, concerning the electric eel there is an accepted theory, which explains all the phenomena in a most satisfactory manner.

This theory originated with Colladon and E. du Bois-Reymond, and states that in the electric substance, dispolary electro-moter molecules are to be found.

In a state of repose they turn towards their pole in every direction, or else in two ways opposed to each other, so that the electricity arises on all sides and disappears without. When the shock takes place, the possitive pole is turned quickly towards the electric organ whence the positive current proceeds.

## OBSERVATIONS ON ICE AND ICEBERGS IN THE POLAR REGIONS.*

By Lieutenant F. Schwatka, U. S. N.

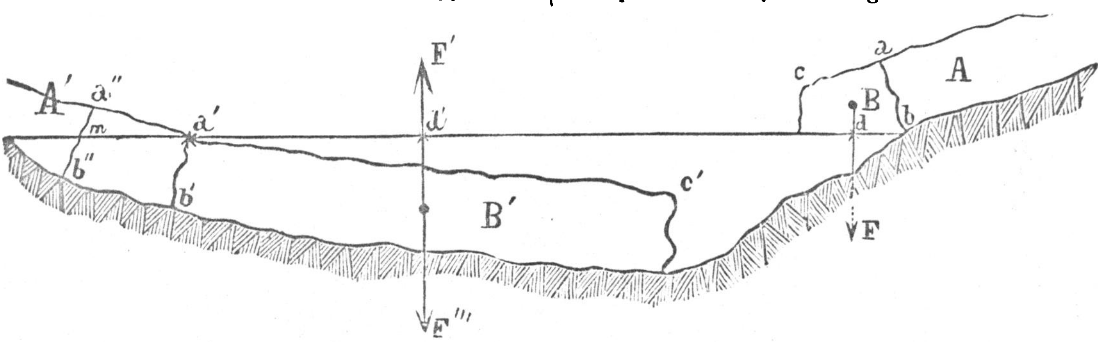
The formation of icebergs, from the terminal fronts of glaciers, has long been a disputed point among *savants*, some contending that they derive their origin from the corroding action of the water, undermining their projecting faces until the weight of the superincumbent mass, acting as a lever, overcame the cohesive power of the glacier along some line of least resistance, when the berg fell into the sea, and was wafted away by the tide-winds and currents. Others can only account for such huge mountains of ice by supposing that the glacier, slowly crawling into the sea, and plunging beneath a denser fluid, has a bouyant effort or tendency to rise, which, at last, becomes so great that it overcomes the line of least resistance, near the shore, and the berg rises into the sea, to be at the further mercy of its uncertain elements. Both theories have proved to be correct. The former generally occurs where currents, heated in more temperate climes, pour their tepid waters northward, and expend their thermal forces in contending with the vast packs, floes, and glaciers of ice, that obstruct their polar march, and whose fast corroding action has the slow glacier only a comparatively short time in its embraces before it has undermined it. The latter results where the chilled waters from the Pole have but little effect upon the glacial front; and slow as it is, it has time to crawl into the sea to give forth its mighty masses. Sometimes both

* Read before the National Academy of Sciences, New York, 1880.

kinds of forces are acting simultaneously upon the same glacier, and while huge icy mountains are at intervals of centuries rising from their dense, watery bed, other and smaller ones are more frequently dropping from i's seaward face, for those formed by dropping are far smaller than those which rise into the sea, as the following diagram will serve to show. Although about seven-eighths of an iceberg is submerged, it must not be inferred that, when its height has been determined, seven times that height is its depth below the sea level. If of a tabular shape, this proportion becomes more nearly correct; but if of a pyramidal or conoidal cross section, which is far oftener the case, the lineal proportions of height to depth approach each other more closely, while the volumes, necessary to hydrostatic equilibrium, remain invariable. Their great height, as compared with their breadth shows that these lineal proportions do not obtain beneath the sea level, or the mass, it homogeneous could not be in a state of stable equilibrium, and would topple over, which sometimes happens when the conditions of equilibrium are disturbed by the unsymetrical decrease of its different faces.

The height of bergs, estimated or measured by various Arctic voyagers, varies greatly. During the warm months of summer, when they are most frequently encountered by navigators, they are often surrounded by a hazy mist, due to the condensation of the surrounding moisture by their chilly faces, and the effect is to make them appear much higher than they really are, and to render estimates of their height particularly unreliable.

As about seven-eighths of an iceberg is under water, the curious spectacle, which has often been seen in Polar latitudes, of these monsters ploughing their way against a rapid current, loaded with heavy pack-ice, and in the very teeth of a strong gale of wind, can be readily understood on the theory that the surface current is shallow, and the drifting colossus is only obeying the mandates of a deeper and more powerful agent.




## ON HEAT CONDUCTION IN HIGHLY RAREFIED AIR.*

By William Crookes, F.R.S.

The transfer of heat across air of different densities has been examined by various experimentalists, the general result being that heat conduction is almost independent of pressure. Winkelmann (*Pogg. Ann.*, 1875, 76) measured the velocity of cooling of a thermometer in a vessel filled with the gas to be examined. The difficulty of these experiments lies in the circumstance that the cooling is caused not only by the conduction of the gas which surrounds the cooling body, but that also the currents of the gas and, above all, radiation play an important part. Winkelmann eliminated the action of currents by altering the pressure of the gas between 760 and 1 millim. (with decreasing pressure the action of gas currents becomes less); and he obtained data for eliminating the action of radiation by varying the dimensions of the outer vessel. He found that, whereas a lowering of the pressure from 760 to 91.4 millims. there was a change of only 1.4 per cent. in the value for the velocity of cooling, on further diminution of the pressure to 4.7 millims. there was a further decrease of 11 per cent., and this decrease continued when the pressure was further lowered to 1.92 millim.

About the same time Kundt and Warburg (*Pogg. Ann.*, 1874, 5) carried out similar experiments, increasing the exhaustion to much higher points, but without giving measurements of the pressure below 1 millim. They enclosed a thermometer in a glass bulb connected with a mercury pump, and heated it to a higher temperature than the highest point at which observations were to be taken; then left it to itself, and noted the time it took to fall through a certain number of degrees. They found that between 10 millims. and 1 millim. the time of cooling from 60° to 20° was independent of the pressure: on the contrary, at 150 millims. pressure the rate was one and a half times as great as at 750 millims. Many precautions were taken to secure accuracy, but no measurements of higher exhaustions being given the results lack quantitative value.

It appears, therefore, that a thermometer cools slower in a so-called vacuum than in air of atmospheric pressure. In dense air convection currents have a considerable share in the action, but the law of cooling in vacua so high that we may neglect convection has not to my knowledge been determined. Some years ago Professor Stokes suggested to me to examine this point, but finding that Kundt and Warburg were working in the same direction it was not thought worth going over the same ground, and the experiments were only tried up to a certain point, and then set aside. The data which these experiments would have given are now required for the discussion of some results on the viscosity of gases, which I hope to lay before the Society in the course of a few weeks; I have therefore completed them so as to embody the results in the form of a short paper.

An accurate thermometer with pretty open scale was enclosed in a 1½ inch glass globe, the bulb of the thermometer being in the centre, and the stem being enclosed in the tube leading from the glass globe to the pump.

Experiments were tried in two ways:—

I. The glass globe (at the various exhaustions) was immersed in nearly boiling water, and when the temperature was stationary it was taken out, wiped dry, and allowed to cool in the air, the number of seconds occupied for each sink of 5° being noted.

II. The globe was first brought to a uniform temperature in a vessel of water at 25°, and was then suddenly plunged into a large vessel of water at 65°. The bulk of hot water was such that the temperature remained sensibly the same during the continuance of each experiment. The number of seconds required for the thermometer to rise from 25° to 50° was registered as in the first case.

* Abstract of a Paper read before the Royal Society, Dec. 16, 1880.