and 0.003 after softening. The purifying effect is thus very marked, and affords the means of reducing the organic matter at least as low as that of Loch Katrine water, and producing nearly the same softness. The possible value of an application of the softening process in such a way that the large and costly reservoirs are unnecessary is thus very great, and those interested in water supply may see the system in operation at the Westminster Aquarium, where it has been in use a few months on a small scale, softening and filtering about a thousand gallons per hour for the fresh-water fish-tanks. The Thames water from the water company's mains is admitted into the usual supply cistern. From this a pipe conveys nine-tenths of the water used, directly to the top of a series of metal disks, where it is met by and mixed, by running over the disks, with the one-tenth of water which has passed through a tank containing slaked lime. The mixed waters then pass into a tank containing a filter, which consists of a series of disks covered with filter cloth, and is softened and filtered continuously for from eight to twelve hours. The filter is fitted with an arrangement of brushes in contact with each cloth-covered disk, and at the end of the above number of hours all that is necessary to be done in order to continue filtration for another period of ten hours, say, is to turn a handle, by which the brushes are caused to rub off all the whitening or spent lime and carbonate of lime from the water which has accumulated on the disks. The whitening is run off with a small quantity of water into a settling tank, where it hardens and is removed at convenience. Messrs. Atkins have thus made it possible to treat Thames and other river water in such a way that all the advantages of Dr. Clarke's methods of softening may be secured without those disadvantages which have hitherto attended it. The softening effect of the process is not at all affected by the continuous method of applying it, as is shown by analyses of the water at Goodwood, which accompany a report of Mr. T. W. Keates, the consulting chemist to the Metropolitan Board of Works. The total hardness of this water is 17.6 degrees before softening by the continuous method, and 4.5 after it, while the water at the Aquarium is reduced from 17 to 6 degrees of hardness.

The value of the softening process has long enough been admitted, but the large reservoir area necessary has militated against its employment. If the mixing and filtering arrangement described can be used on the largest scale, as it is proposed it should be, it should not only afford the means of securing the economy and improvement of the water due to softening, and of reducing the sand filter-bed area required by the different London water companies, but should afford the means of largely removing those foreign matters from the Thames water which form the sole cause of complaint of those who do complain.—*The Engineer*.

HOT ICE.

At a recent meeting of the London Chemical Society, Dr. Carnelly explained and performed his remarkable experiment of heating up ice, without melting it, by means of pressure.

Dr. Carnelly then explained the device which he had adopted in order to secure and maintain a vacuum in the case of ice. For the success of the experiment the tension must be below five millimeters. The apparatus consisted of a wide glass tube three-quarters inch in diameter, and about five to six feet high. This is placed in a vertical position, and is connected at its upper end with a strong glass flask placed horizontally, and surrounded with a freezing mixture. The apparatus having been inverted and filled with mercury, the lower end of the tube is closed with the thumb, and placed under the surface of a layer of mercury about ten inches deep. On withdrawing the thumb the mercury sinks in the tube to the barometric height, and a large Torricellian vacuum is obtained, which is surrounded, as far as the flask is concerned, with a freezing mixture. A small quantity of boiled water is now introduced, which rises to the top of the mercurial column, and surrounds the bulb of a thermometer suspended inside the tube. The water is then frozen, and the depth of the layer of mercury in which the tube stands reduced to about three inches; in consequence the mercury in the tube sinks, and leaves a detached column of ice with the thermometer bulb in its center. This column acts as a cork, shutting off the large vacuum space above from the small vacuum below. By carefully heating the tube the ice is melted round the circumference of the plug, and a fine annular opening is made between the ice and the inside of the glass tube. This restores the communication between the upper and lower portion of the vacuum. As soon as this is effected, any aqueous vapor which is formed is at once condensed by the freezing mixture, and the vacuum is kept intact. Under these circumstances the author has made the ice so hot that the thermometer in the center of the cylinder stood at 180° C. before the ice melted. In the experiment shown to the Society the thermometer only rose to 30° C., when the cylinder (which was too large and therefore too heavy) dropped off the thermometer. To prove that the ice was really hot the author has contrived and carried out some experiments, in which the cylinder of hot ice was dropped into a small calorimeter filled with water; the temperature rose when the ice was introduced, whereas if ordinary ice it would of course have been lowered. The author then showed two experiments with camphor and mercuric chloride, which were perfectly successful. The camphor was contained in a glass tube closed at one end and connected at the other with a Sprengel pump. On heating the tube the camphor melted, but on starting the Sprengel pump the camphor, as the pressure decreased, solidified, though the heating was continuous. The mercuric chloride was similarly raised many degrees above its ordinary melting point, when kept under diminished pressure, without liquefying; but on allowing the atmospheric pressure to enter, by cutting the tube, the solid mass immediately melted and began to boil.

Prof. McLeod suggested that the cylinder of ice might be used as its own calorimeter on admitting the atmospheric pressure. If the ice were at 180° C. it should liquefy into water at about 100°.

Prof. Ayrton pointed out that although these results seemed at first sight at variance with the experiments of Sir William Thomson and the well-known calculations of James Thomson, according to which an increase of pressure lowers the melting point, there was really no direct opposition between them, because the constants on which Carnot and James Thomson based their calculations were only constants at temperatures at and below 32°, and we had no evidence until these experiments of Dr. Carnelly as to the value of these constants at higher temperatures. Under ordinary conditions an increase of pressure produces a slight diminution of the melting point, yet it was perfectly in accord with the principles of thermo-dynamics to suppose that

at temperatures above 32° a slight diminution of pressure might enormously raise the melting point.

Mr. T. Brown said that he did not quite understand how removing the pressure from the outside of the ice could affect the ice in the interior, and it was difficult to see why the ice in the interior should not liquefy if raised to 180°, even if the diminution of pressure prevented the liquefaction of the surface layer.

A vote of thanks to Dr. Carnelly from a crowded meeting was then passed by acclamation.

SAPONIFICATION OF FATS.

By DR. VON DER BECKE.

THE name fat is generally applied to a mixture of the triglycerides of palmitic, stearic, and oleic acids. As regards the animal fats this assumption has been in all cases verified, but the vegetable fats display certain not unimportant deviations. J. König, J. Kiesow, and B. Aronheim, in saponifying vegetable fats, obtained invariably less glycerine than is required for forming the glycerine ethers of the fatty acids—a fact pointing to the conclusion that free fatty acids must be present, since the quantity of cholesterol occurring in the plants is too small to combine with the fatty acids. For saponification potassium and sodium hydrate were used along with the other basic oxides, the latter substances being considered equal in value to the former, the only difference being that the products in the one case are termed “soaps,” and in the other “plasters.” It was assumed hitherto that the triglycerides, like other ethers, were completely decomposed by the above-named ethers into salts of the fatty acids and glycerine, and that equal quantities of glycerine were obtained in all cases. For the saponification of fats and the separation of the products, J. König had proposed a process which consists essentially in treating the fat operated upon with an excess of lead oxide in presence of water at 90° to 100°. The author when attempting at his request to saponify cacao butter in this manner—in order to discover a process detecting the sophistications of this product—found that it could not be saponified with lead oxide, at least not in this manner. It was found on further experimentation that the quantity of glycerine obtained on saponification with potassium hydrate was in all cases considerably the highest. In the easily saponifiable fats, butter, lard, and olive oil, the difference was found less manifest, but it was much more distinct in those which are hard to saponify. Cacao butter and tallow, if saponified with lead oxide, yield scarcely traces of glycerine. A mixture of an easily saponifiable fat-like butter with cacao butter gave the same quantity of glycerine as if butter alone were employed. It is possible that the reaction when once set up may extend itself. Hence, it appears that in the case of some fats the method of saponification with oxide is not trustworthy, and that when the accurate determination of the proportion of glycerine in a fat is required, the saponification must be effected with potassium hydrate.

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 millimeter (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 millimeters, 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 millimeters there was a further decrease of 11 per cent., and this decrease continued when the pressure was further lowered to 1.92 millimeters.

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 one millimeter. They inclosed 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 millimeters and 1 millimeter the time of cooling from 60° to 20° was independent of the pressure; on the contrary, at 150 millimeters pressure the rate was one-and-a-half times as great as at 750 millimeters. 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 inclosed in a 1½ inch glass globe, the bulb of the thermometer being in the center, and the stem being inclosed 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.

It was found that the second form of experiment gave the most uniform results; the method by cooling being less accurate, owing to currents of air in the room, etc.

The results are embodied in the following table:

(Rate of Heating from 25° to 50°.)

TABLE I.

Pressure.	Temperature.	Seconds occupied in rising each 5°.	Total number of seconds occupied.
760 millims.	25°	0	0
	25 to 30	15	15
	30 to 35	18	33
	35 to 40	22	55
	40 to 45	27	82
1 millim.	45 to 50	39	121
	25°	0	0
	25 to 30	20	20
	30 to 35	23	43
	35 to 40	25	68
620 M.*	40 to 45	34	102
	45 to 50	48	150
	25°	0	0
	25 to 30	20	20
	30 to 35	23	43
117 M.	35 to 40	29	72
	40 to 45	37	109
	45 to 50	53	162
	25°	0	0
	25 to 30	23	23
59 M.	30 to 35	23	46
	35 to 40	32	78
	40 to 45	44	122
	45 to 50	61	183
	25°	0	0
23 M.	25 to 30	28	28
	30 to 35	33	61
	35 to 40	41	102
	40 to 45	55	157
	45 to 50	70	227
12 M.	25°	0	0
	25 to 30	30	30
	30 to 35	37	67
	35 to 40	41	108
	40 to 45	58	166
5 M.	45 to 50	86	252
	25°	0	0
	25 to 30	38	38
	30 to 35	43	81
	35 to 40	54	135
2 M.	40 to 45	71	206
	45 to 50	116	322
	25°	0	0
	25 to 30	41	41
	30 to 35	51	92
	35 to 40	65	157
	40 to 45	90	247
	45 to 50	165	412

* M = millionth of an atmosphere.

There are two ways in which heat can get from the glass globe to the thermometer—(1) By radiation across the intervening space; (2) by communicating an increase of motion to the molecules of the gas, which carry it to the thermometer. It is quite conceivable that a considerable part, especially in the case of heat of low refrangibility, may be transferred by “carriage,” as I will call it to distinguish it from convection, which is different, and yet that we should not perceive much diminution of transference and consequently much diminution of rate of rise with increased exhaustion, so long as we work with ordinary exhaustions up to 1 millimeter or so. For it, on the one hand, there are fewer molecules impinging on the warm body (which is adverse to the carriage of heat), yet, on the other, the mean length of path between collisions is increased, so that the augmented motion is carried further. The number of steps by which the temperature passes from the warmer to the cooler body is diminished, and accordingly the value of each step is increased. Hence the increase in the difference of velocity before and after impact may make up for the diminution in the number of molecules impinging. It is, therefore, conceivable that it may not be till such high exhaustions are reached that the mean length of path between collisions becomes comparable with the diameter of the case, that further exhaustion produces a notable fall in the rate at which heat is conveyed from the case to the thermometer.

The above experiments show that there is a notable fall, a reduction of pressure from 5 M. to 2 M. producing twice as much fall in the rate as is obtained by the whole exhaustion from 760 millimeters to 1 millimeter. We may legitimately infer that each additional diminution of a millionth would produce a still greater retardation of cooling, so that in such vacua as exist in planetary space, the loss of heat—which in that case would only take place by radiation—would be exceedingly slow.—*Chem. News*.

THE FATAL DOSE OF CARBON OXIDE FOR VARIOUS ANIMALS.

By M. GRÉHANT.

AIR containing one-three-hundredth of its volume of carbon oxide proved fatal to a dog when inhaled for fifty minutes. With another dog of the same size the fatal dose was one two-hundred-and-fiftieth. A rabbit resisted various proportions up to one-sixtieth. A sparrow perished with one-five-hundredth.

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