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.

CAUSE OF DIPHTHERIA IN THE FAMILY OF
SAMUEL CRUMP, ESQ., MONTCLAIR, N. J.
By J. W. PINKHAM, M.D., Montclair, N. J.

ALTHOUGH I saw but little of the cases referred to below, having had under my care only the fourth child after the third day of his attack, I have had an opportunity to collect some instructive facts connected with the matter, which I think will be of general interest.

Diphtheria in Montclair has been a thing of rare occurrence, only thirteen fatal cases having been recorded in the last fourteen years.

At the time the disease showed itself in Mr. Crump's family there were no other cases in town, and I believe there have been no cases since.

Mr. Crump's residence is situated on a hillside, about three hundred feet above the sea level. The ground is dry and slopes toward the east. The house is exposed on all sides to the direct rays of the sun. The cellar is dry.

The plumbing of the house is confined to the kitchen, the laundry, the bathroom, and one bedroom on the second floor. Every waste pipe is trapped and every trap is ventilated. The soil pipe extends above the roof, and is supplied with foot ventilation. The cesspool is thoroughly ventilated. Nearly every room in the house is supplied with a separate ventilating shaft. In the opinion of experts the house could not have been unhealthy from sewer gas nor from soil dampness.

The house is warmed by a hot-air furnace, called the Gould Hygienic Furnace, the peculiarity of which is that it presents a large radiating surface and thus does not over-heat the air. Mr. Crump, in order still further to insure himself against the evils of overheated air, caused his furnace (*i. e.*, the radiating surface) to be covered with a coating of clay; and, to prevent the air from becoming too dry, a large evaporating dish was placed in the hot-air chamber.

The household consisted of thirteen members, seven adults and six children. The adults were Mr. and Mrs. C., two lady relatives, and three servants. The children were all under eleven years of age. The family was an unusually healthy one, and no member of it, so far as can be ascertained, had been exposed in any way to the contagion of diphtheria. There had been no case of the disease in the vicinity of Mr. Crump's house for many years, so far as known. Twenty-seven feet from the cold-air box which supplies the above-mentioned furnace with air, is the fence which separates Mr. Crump's lot from the adjoining premises. This cold-air box was, previous to the attack, on a level with the ground, and presented in transverse section an area of two square feet.

On the eighth day of December, 1880, there were spread on the grounds of the above-named adjoining premises the contents of a cemented pit, which, during the summer, had received the waste from the stable and the kitchen. At the time it was spread it was noticed that the odor was very bad, and it was particularly noticed by those who passed along the carriage-way between Mr. Crump's house and the place on which the refuse was spread.

Now, keeping in mind the relative position of the two places, viz., that Mr. Crump's lies to the east and is somewhat lower, let us glance at the following table taken from the carefully kept meteorological record of William A. Whitehead, Esq., of Newark:

Dec., 1880.	Temperature.			Wind.	State of Weather.
	Min.	Max.	Mean.		
8.....	14	27	20	N N. W.	
9.....	11	12	11+	W.	
10.....	6	23	15	N. W.	
11.....	6	23	15	N. to W.	
12.....	19	37	28	W. N. W.	
13.....	32	39	35	W. S. W.	Cloudy.
14.....	39	40	35	W.	Fair.
15.....	32	39	35	W. S. W.	Clouds and rain.
16.....	33	38	35	W. S. W.	Clouds and rain.
17.....	21	30	25	N. W.	Fair.
18.....	23	32	27	W N. W.	Fair.
19.....	19	33	26	N. N. W.	Fair.
20.....	23	31	27	N.	Cloudy.
21.....	27	32	30	N. E.	

It will be seen that the thermometer remained below the freezing point until the 13th, when it reached 39°. On this day it was cloudy and the wind was westerly. On the 14th the temperature was 40°+, with the wind directly west. On the 15th it was 39°+, wind westerly, with clouds and rain. On the 16th it was 38°+, wind westerly, with clouds and rain.

Thus, for nearly five days the temperature was considerably above freezing nearly all the time, the wind was constantly from a westerly direction, and during the two days when the temperature was highest there were clouds and rain. During these warm, moist days the odor from the refuse described above was very marked, and as its source was only twenty-seven feet from the entrance to Mr. Crump's cold-air supply-box, and as the wind was constantly in the direction toward Mr. Crump's house, the gases which made themselves so perceptible in the neighborhood must have been drawn into the furnace with the inflowing air, and must have been distributed to that part of the house heated by the furnace.

Mr. and Mrs. Crump and the children slept in bedrooms warmed by this heated air from the furnace.

The remaining five members of the family occupied bedrooms which were not so warmed. On the 22d of December, one of the children, aged six years, after two or three days' illness, presented unmistakable evidences of diphtheria, and died on the 26th. A second child was attacked on the 26th, a third child and Mrs. Crump on the 27th, and a fourth child on the 29th. The second and third child died on the 31st of December and the 1st of January respectively. Mrs. Crump and the fourth child recovered. In the course of seven days, of the eight persons who slept in the furnace-warmed rooms, five were attacked with diphtheria, three cases terminating fatally. The remaining members of the household, five in number, who occupied rooms not supplied with registers, all escaped.

It is worthy of notice that of all the attendants, physicians, nurses, and friends who were brought into close relation with the sick day and night for more than ten days, with possibly one exception, not a person contracted the disease. The exception referred to was that of a colored nurse, the

brevity of whose attack (two or three days) makes it fairly questionable whether it was diphtheria she had, or some affection which simulates it. Now, when we consider the immunity of so many people, for so long a time, and so fully exposed to the disease in so malignant a form, does it seem probable that one of these children should have contracted it from a person so slightly affected as to be unconscious of illness, or should have come in contact with the poison in a place where the disease did not exist, and have transmitted it in so short a time to so many members of the family?

On the other hand, when we consider the close proximity to the large cold-air box of a mass of putrefying animal and vegetable matter, the effluvia from which were, for five days, constantly mingled with the air which, in a continual stream, passed through the hot-air chamber of a furnace which heated it, we may fairly suppose, to a degree not too great to do otherwise than render more active the already poisonous gases with which it was loaded, when we take into account the large evaporating dish contained in this hot-air chamber, which supplied an abundance of moisture, and the still more important fact that, of those most exposed to this air from the furnace, five out of eight were seized with diphtheria in the space of one week, while the five who were least exposed all escaped, does it not seem probable, whatever views we may entertain concerning bacteria and specific germs, that we have in these poisonous effluvia the essential cause of the disease in this particular instance?—*Med. Record.*

NATURAL BONE-SETTING.

THE career of the Italian female bone-setter, Regina Dal Cin, who has just returned to her native hills after a triumphal year of residence in this country, is like a bit of the surgery of a past age, revived from its sleep of a hundred years, to give us a demonstration of the manner in which surgery was divided into fragments, and illiterate specialists itinerated from place to place seeking occupation. The stone-cutting expeditions through Europe of Friar Jacques strongly resemble the bone-setting journeys of Madame Dal Cin to Vienna, Trieste, and America. Even the record of the dirty monk that "most of the Parisians looked upon him as a physician sent from heaven for the relief of mankind," is equaled by the claims for a special heaven-endowment made for the illiterate peasant woman. A native of an obscure village in the north of Italy, the daughter of an inn-keeper, without education, unable even to read in her own language, self-instructed, she has acquired such tact and facility in the manipulation of joints which have been the subject of chronic diseases, that in selected cases she has been able to accomplish most excellent curative results. Her local reputation in Italy attracted the attention of an officer of high rank in the U. S. Navy, who took his child, the subject, as was believed, of an incurable joint-disease, to her for treatment. The child having been greatly benefited, the case became the occasion of a highly wrought article descriptive of the powers of the *manipuliste*, from the pen of the father, in one of the most popular monthly magazines. A wealthy and influential family of Brooklyn, one of whose daughters was crippled from spinal and joint affections, attracted by her reputed powers, took their daughter to Italy and placed her under the care of this person, and eventually persuaded her to return to this country with them, where her stay was prolonged more than a year. Her arrival was heralded by long articles written by a clergyman and published in the daily papers, in which her supposed wonderful, mysterious, and God-given gifts of bone-setting were set forth with all the details which a vivid imagination and a good command of adjectives made possible. Large numbers of those who were halt and crippled from chronic hip and knee diseases were taken to her. As in the case of most others of these less pretentious natural bone-setters, who are found scattered more or less thickly about the country, ignorant of anatomical knowledge, and innocent of even the meaning of the word pathology, the invariable pathology of the cases brought to this woman, as she claimed, was that a bone was "out," and that when by her manipulations the bone was restored to its place, a cure would be effected.

Her method of treatment was to keep the affected joint continuously poulticed for from ten days to two weeks with a poultice of marsh-mallows; during this time gentle movements and firm rubbings were daily impressed upon the joint, and the expectation of the patient constantly excited as to the time when the final maneuver was to be made by means of which the distorted bone was to be put back in its place. After sufficient of this preparatory treatment had been made, the final *coup de grace* was effected by a series of quick movements and an exclamation from the operator that at last it was done! As a rule, all her manipulations, even her final climacteric performances, were nearly painless. Great improvement in the usefulness of the affected joints resulted in many cases. This woman, now about sixty years of age, is said to be modest, kind, and gentle in her manners, and is undoubtedly firmly and honestly persuaded that the conditions which she imagines to be present in her patients really exist; for how otherwise explain the success of her treatment, which has been so great as to secure for her, after repeated legal prosecution for practicing without qualifications, an official decree from the Minister of the Interior in the Kingdom of Italy, authorizing her to treat dislocations, fractures, and hip disease!

A review of her methods shows that they are as perfect as if they had been devised by one profoundly versed in psychology and pathology. The expectation of the patient, his confidence in the result, and the co-operation of his will are all enlisted by them. She probably never heard of Tuke on "The Influence of the Mind upon the Body," but she could not have done more wisely if she had closely studied it. She believes in herself as thoroughly as Bonaparte did in his "Star of Destiny;" to her every case is an engagement, and every patient she makes an Old Guard. The real condition of the joints in the manipulation of which her success is so signal, can very rarely be that of dislocation or even of subluxation.

Those who have read that most delightful and profitable little book of Wharton P. Hood, entitled "On Bone-Setting," will retain a vivid remembrance of the clear manner in which he demonstrates the function of persistent bands of adhesion, both extra and intra-articular, in producing permanent lameness of joints after all inflammation has subsided. The profession owes a debt of gratitude to Dr. Hood that he availed himself of the opportunity to study the methods of one of the ablest of bone-setters, and that, having pursued his studies after scientific methods, he has given us the results as he has. These cases of lame joints confront every practitioner continually; too often they constitute an *opprobrium chirurgicum*; and when, after having been long unrelieved through the want of skill or attention

of the educated practitioner, they are cured by the manipulations of a bone-setter, they bring scientific surgery into discredit. It is our conviction that in the majority of cases of stiff joints the obstacle to motion exists less in intra-articular effusions and adhesions than it does in the changes which the extra-articular structures have undergone; stiffened and adherent muscles and ligaments, contracted fasciæ, tendons and sheaths inseparably blended, conditions which have developed from old inflammations about a joint or from prolonged disuse after bruises or other injuries, these are the chief causes of disability. How admirably the emollient poultices, the rubbings, and the movements practiced by the Italian woman are adapted to promote the absorption of effusions, the stretching and rupture of adhesions, and the restoration of tone to debilitated structures! It is true that no new principle, no new method, is found in her practice. She is but a skillful *masseur*. The reason why physicians in general so often fail to relieve the cases in question, and so permit them to fall into the hands of empirics, is that these details of poulticing and rubbing and moving demand a degree of patience and devotion to trifling details which is possessed by but few. As in so many other things, the brilliant successes of such a person as Madame Dal Cin alone are heard of, while of the many failures and occasional disasters nothing is said. We are cognizant of one case in which she reawakened in a hip joint acute inflammation, which terminated in abscess and necrosis. The scientific surgeon weighing carefully all the elements of the problem, which each case that comes before him presents, will determine what cases are the proper ones to submit to manipulations, and what to leave undisturbed. In his hands the same brilliant successes will be secured by the same methods in the one class of cases, while in the other disasters will be prevented. Under the guidance of the educated surgeon, Madame Dal Cin and such as she might do work of yet greater value. The methods of the natural bone-setter, as in the case of the English Hutton and the Italian Dal Cin, may sometimes repay the consideration of the educated surgeon. It would be a mistake to always ignore them, or to invariably dismiss their work with a sneer and the epithet of quackery.—*Annals of Anat. and Surg.*

EFFERVESCENT BEVERAGES.

A CORRESPONDENT of the London *Chemist and Druggist* supplies the following formulæ:

GINGER BEER.

Brown sugar..... 2 pounds.
Boiling water..... 2 gallons.
Cream of tartar..... 1 ounce.
Bruised ginger root.... 2 ounces.

Infuse the ginger in the boiling water, add your sugar and cream of tartar; when lukewarm strain; then add half pint good yeast. Let it stand all night, then bottle; if you desire, you can add one lemon and the white of an egg to fine it.

LEMON BEER.

Boiling water..... 1 gallon.
Lemon, sliced..... 1
Ginger, bruised..... 1 ounce.
Yeast..... 1 teacupful.
Sugar..... 1 pound.

Let it stand 12 to 20 hours, and it is ready to be bottled.

HOP BEER.

Water..... 5 quarts.
Hops..... 6 ounces.

Boil three hours, strain the liquor, add

Water..... 5 quarts.
Bruised ginger..... 4 ounces.

and boil a little longer, strain, and add 4 pounds of sugar, and when milk-warm, 1 pint of yeast. Let it ferment; in 24 hours it is ready for bottling.

SPRUCE BEER.

Hops..... 2 ounces.
Chip sassafras..... 2 "
Water..... 10 gallons.

Boil half an hour, strain, and add

Brown sugar..... 7 pounds.
Essence of spruce..... 1 ounce.
Essence of ginger..... 1 "
Ground pimento..... ½ "

Put into a cask, and cool, add 1½ pints yeast, let stand 24 hours, fine, draw it off to bottle.

CAMPBOR PRODUCTION.

SINCE the abolition of the camphor monopoly, trading in this article is equally permitted to Chinese and Europeans. Nevertheless, the purchase of camphor in the interior, owing to the peculiar difficulties connected with it, is surrendered by the foreigners exclusively to the Chinese. The camphor tree occurs on the hills of the interior in large forests, which are inhabited by wild tribes, who annually kill a number of the Chinese engaged in felling the trees. Each year it is necessary to penetrate further into the interior, because *no new trees are planted in place of those cut down*. While this short-sighted method will sooner or later excite a fatal influence upon the camphor trade, it has, on the other hand, the advantage of causing a more rapid civilization of the island, since the wild tribes are continually pushed further inland into the central and inhospitable mountainous regions. The class of Chinese who are engaged in the production of camphor may be designated as the scum of the Chinese nation, being altogether composed of persons who prefer the dangers of the wilderness to law and order. In the neighborhood of Yokohama murders of camphor tree cutters are said to be of daily occurrence. The export of camphor in 1878 amounted to 13,305 piculs (1 picul = 133½ lb.), being 2 per cent. more than the export in 1877, 53 per cent. over that of 1876, and 84 per cent. over that of 1875. The increase was chiefly caused by the fact that merchants upon Formosa found it more advantageous to remit payment to the Hong Kong importer in form of camphor than in funds. According to the latest custom reports, the price of camphor in Formosa is \$9 to \$10 per picul and in Hong Kong, \$18 to \$20, but owing to the great loss, which sometimes amounts to 20 per cent., and the expenses of freight, commission, etc., the profit is much less than would appear from these figures.—*Oesterr. Monatschr. f. d. Orient.*