which, however, may be due to his registering days as rainy on which there was a fall of local mist or mizzle—*légères bruines.* (*Annuaire*, 1825, p. 167.)

I may mention in conclusion, that there appears to be much in the chapters relating to the subject in this second volume to countenance the belief that the moon's surface radiates heat unequally; so that it is perhaps to the different extent of the absorbing surfaces, and the length of time during which, at the several phases, they are exposed to the solar rays, that one may ascribe the difference in the effects which have been noticed. It is evident that the influence must vary with the moon's position; and it may be further subject to other changes, for which the discoveries of M. Nièpce de St. Victor and—I venture to add (from the frequency of storms at certain periods of the moon's age, and the sudden nature of other phænomena)—the experiments of electricians will possibly afford an explanation.

XXIV. *On Regelation, and on the Conservation of Force. By* Professor FARADAY.

[The volume of reprinted 'Experimental Researches in Chemistry and Physics,' by Prof. Faraday, which has just been published, contains the following new matter in relation to the above subjects. We think it expedient to transfer it to our pages.]

On Regelation.

THE subject of regelation has of late years acquired very great interest through the experimental investigations of Tyndall, J. Thomson, Forbes and others, and in its present state will perhaps justify a few additional remarks on my part as to the cause. On the first observation of the effect eight years ago, I attributed it to the greater tendency which a particle of fluid water had to assume the solid state, when in contact with ice on two or more sides, above that it had when in contact on one side only. Since then Mr. Thomson has shown that pressure lowers the freezing-point of water*, and has pointed out how such an effect occurring at the places where two masses of ice press against each other, may lead first to fusion and then union of the ice at those places, and so he explains the fact of regelation. Prof. J. D. Forbes† does not think that pressure causes regelation in this manner, though it favours it by moulding the touching surfaces to each other. He admits Person's view

* Belfast Society Proceedings, December 2, 1857.
† Royal Society Edinburgh Proceedings, April 19, 1858.

of the gradual liquefaction of ice*, and assumes that ice must be essentially colder than ice-cold water, *i. e.* the water in contact with it.

I find no difficulty in thinking it would be easy to arrange a mixture of water and snow in such a manner that it might be kept for hours and days without any transition of heat either to or from it; but I find great difficulty in thinking that the particles of snow, small as they may be made, would remain for the whole of the time at a lower temperature by 0°·3 F. than the particles of water intermingled with them. Still, admitting for the present the possibility that Prof. Forbes's view may be correct, and also the truthfulness of Mr. Thomson's principle, and its possible action in regelation, I wish to say a few words on the other principle already referred to, which was originally assumed by myself, which, in relation with the mechanical theory of heat, has been adopted by Dr. Tyndall, and which, after all, may be the sole cause of the effect.

The principle I have in view being more distinctly expressed is this:—In all uniform bodies possessing cohesion, *i. e.* being in either the solid or the liquid state, particles which are surrounded by other particles having the like state with themselves tend to preserve that state, even though subject to variations of temperature, either of elevation or depression, which, if the particles were not so surrounded, would cause them instantly to change their condition. As water is the substance in which regelation occurs, I will illustrate the principle by the phænomena which it presents. Water may be cooled many degrees below 32° Fahr.† and yet retain its liquid state for, as far as we know, any length of time without solidification; yet, introduce a piece of the same chemical substance, ice, at a higher temperature, and the cold water freezes and becomes warm. It is certainly not the change of temperature which causes the freezing, for the ice introduced is warmer than the water. I assume that it is the difference in the condition of cohesion existing on the different sides of the changing particles which sets them free and causes the change. The cold water particles would willingly, as to temperature, have solidified without the ice, but were held fluid by the cohesion with them of other like fluid particles on all sides.

In the other direction, Donny's experiments have taught us that the cohesion amongst the particles of water is so great

* *Comptes Rendus*, 1850, xxx. 526.

† Water may be cooled to 22° F. It is probable that if it were perfectly freed from air it would remain fluid at a much lower temperature; for the air is excluded at the freezing-point, and the occurrence of this exclusion would break cohesion.

that it will support a column of the fluid four or more feet high when there is no other power to sustain it; or will cause it to resist conversion into the state of vapour at temperatures so much higher than its ordinary boiling- or condensing-point, that explosion will occur when the continuity, and therefore the cohesion, is destroyed. The water may be exalted to the temperature of 270° Fahr. at the ordinary pressure of the atmosphere, and remain as water; but the introduction of the smallest particle of air or steam will cause it at once to burst into vapour, and at the same time its temperature falls.

This ability which water has to retain by cohesion its liquid state, refusing to solidify when below the freezing-point, or to become vapour when above the boiling-point, it has in common with many other substances. Acetic acid, sulphur, phosphorus, many metals, many solutions, may be cooled below the congealing temperature prior to the solidification of the first portions; many other substances, such as alcohol, sulphuric acid, ether, camphine, &c., boil with bumping, or boil with different degrees of facility in vessels of different substances*. The conclusion, that these differences are due to a certain range of cohesion in the case of each body, seems to me both simple and natural; this cohesion enabling the substances to withstand a change of temperature which, without the cohesion, ought to have caused a change of state. The effect of extraneous matters as nuclei also appears to me to be simple; for though when introduced, as into cooled or heated water, their particles may exert a cohesive force (so to say) upon the particles of the fluid, the force so exerted in the first instance is rarely equal to the force exerted between the water particles themselves. Extraneous substances require preparation before their adhesion to fluid is at a maximum; glass will permit water to boil in contact with it at 212°, or by preparation will remain in contact with it at 270° Fahr., as in Donny's experiment. It will also remain in contact with water at 22° Fahr. without causing its solidification, and yet an ordinary piece of glass will set it off at once.

Enough has been said, I think, to show that water particles surrounded by water tend to retain their fluid state in both directions at temperatures which are abundantly sufficient to make it equally retain the solid or the vaporous state when either of them is conferred upon it. There is nothing against the assumption that ice has the like kind of power, *i. e.* the power of retaining its solid state at temperatures higher than the temperature of ice against water. Nevertheless the fact is more difficult to show; still some experiments may be quoted

* Marcet.

in favour of the view. If hydrated crystals of sulphate of soda, carbonate of soda, phosphate of soda, &c.*, be carefully prepared in clean basins, by spontaneous evaporation of the water, they will retain their form unbroken, and their hydrated state undisturbed, through the high temperatures of a whole summer, though, if broken or scratched even in winter, they will commence to effloresce at the place where the cohesion, and with it the balance of force, was disturbed, and will from thence change progressively throughout the whole mass†. As regelation concerns the condition of water, there is perhaps no occasion to go further. Such facts as the following, however, concern the extension of the principle, and illustrate the power of cohesion, especially in cases where it is coming into activity. Camphor in bottles, or iodide of cyanogen in proper glass vessels, produces crystals sometimes an inch or two in length, which grow by the deposition of solid matter on them from an atmosphere unable to deposit like solid matter upon the surrounding glass, except at a lower temperature. Crystals in solution grow by the deposition of solid matter on them which does not deposit elsewhere in the solution. Many suchlike cases may be produced.

Returning to the particular case of regelation, it is seen that water can remain fluid at temperatures below that at which ice forms, by virtue of the cohesion of its particles; and in so far the change is rendered independent of a given temperature. Next, I rest on the fact that ice has the same property as camphor, sulphur, phosphorus, metals, &c., which cause the deposition of solid particles upon them from the surrounding fluid, that would not have been so deposited without the presence of the previous solid portions,—a fact sufficiently proved by the growth of fine crystals of ice in ice-cold water. This effect was admirably shown in Mr. Harrison's freezing apparatus, where beautiful thin crystals of ice, six, eight, and ten inches long, would form in the surrounding fluid; and these crystals, which could not be colder than the surrounding fluid, exhibited the phænomena of regelation when purposely brought in contact with each other.

The next point may be considered as an assumption: it is that many particles in a given state exert a greater sum of their peculiar cohesive force upon a given particle of the like substance in another state than few can do; and that as a consequence a water particle with ice on one side and water on the other,

* Philosophical Transactions, 1834, p. 74; or Exp. Res. Electricity, vol. i. p. 191, note.

† Such a case shows combined solid water at a temperature ready to separate and change into vapour, yet not changing, because, as far as we can see, the undisturbed cohesion holds all together.

is not so apt to become solid as with ice on both sides; also that a particle of ice at the surface of a mass in water is not so apt to remain ice as when, being within the mass, there is ice on all sides, temperature remaining the same. If that be admitted, then regelation is sufficiently accounted for. Difference of temperature above or below that of the changing points of water is not alone sufficient to cause change of state, the change being independent of temperature throughout a large range. At such times the particles appear to be governed by cohesion. Cohesion resolves itself into the force exerted on one particle by its neighbours; and this force seems to me to be sufficient, under the circumstances, to account for regelation.

Supposing this to be the true view of the state of things, then a particle of ice within ice can exist at a temperature higher than a like particle of ice on its surface in contact with water; and though it does not appear at present how a higher temperature could be communicated to the interior of a mass of freezing ice than that existing over its surface, still there may be principles of action in radiation, and even in conduction and liquefaction, producing that effect. Assuming, however, that a piece of freezing ice is in such a state, then, if it were to be pulverized, it ought to produce a mixed mass of ice and water colder than the ice was before. Such seems to be the result in one of Prof. Forbes's experiments, in which ice rapidly pounded showed a temperature of $0^\circ{\cdot}3$ Fahr. below the temperature of snow in a thawing state. The experiment, however, would require much consideration in every point of view, and much care before it could be considered as telling anything beyond the temperature of ice-cold water.

On the other hand, if a spherical cup of ice could be prepared containing water within, to which no heat could pass except by conduction through the ice itself, that water ought to be a little colder than the ice cup around it: also if a mixture of snow and water were pressed together, the temperature should rise whenever regelation occurred, being an effect in the contrary direction to that which Prof. J. Thomson contemplates; and such a mixture, as a whole, ought to be warmer than the water in the ice sphere mentioned above. No doubt nice experiment will hereafter enable us to criticise such imaginary results as these, and, separating the true from the untrue, will establish the correct theory of regelation.

On the Conservation of Force.

During the year that has passed since the publication of certain views regarding gravitation, &c., I have come to the knowledge of various observations upon them, some adverse, others favourable: these have given me no reason to change my own mode of viewing the subject; but some of them make me think that I have not stated the matter with sufficient precision. The word "force" is understood by many to mean simply "the tendency of a body to pass from one place to another," which is equivalent, I suppose, to the phrase "mechanical force;" those who so restrain its meaning must have found my argument very obscure. What I mean by the word "force," is the *cause* of a physical action; the source or sources of all possible changes amongst the particles or materials of the universe.

It seems to me that the idea of the conservation of force is absolutely independent of any notion we may form of the nature of force or its varieties, and is as sure and may be as firmly held in the mind, as if we, instead of being very ignorant, understood perfectly every point about the cause of force and the varied effects it can produce. There may be perfectly distinct and separate causes of what are called chemical actions, or electrical actions, or gravitating actions, constituting so many forces; but if the "conservation of force" is a good and true principle, each of these forces must be subject to it: none can vary in its absolute amount; each must be definite at all times, whether for a particle, or for all the particles in the universe; and the sum also of the three forces must be equally unchangeable. Or, there may be but one cause for these three sets of actions, and in place of three forces we may really have but one, convertible in its manifestations; then the proportions between one set of actions and another, as the chemical and the electrical, may become very variable, so as to be utterly inconsistent with the idea of the conservation of two separate forces (the electrical and the chemical), but perfectly consistent with the conservation of a force, being the common cause of the two or more sets of action.

It is perfectly true that we cannot always trace a force by its actions, though we admit its conservation. Oxygen and hydrogen may remain mixed for years without showing any signs of chemical activity; they may be made at any given instant to exhibit active results, and then assume a new state, in which again they appear as passive bodies. Now, though we cannot clearly explain what the chemical force is doing, that is to say, what are its effects during the three periods before, at, and after the active combination, and only by very vague assumption can approach to a feeble conception of its respective states, yet we do not suppose the creation of a new portion of force for the active moment of time, or the less believe that the forces belonging to the oxygen and hydrogen exist unchanged in their amount at all these periods, though varying in their results. A part may at the active moment be thrown off as mechanical force, a part as radiant force, a part disposed of we know not how; but believing, by the principle of conservation, that it is not increased or destroyed, our thoughts are directed to search out what at all and every period it is doing, and how it is to be recognized and measured. A problem, founded on the physical truth of nature, *is stated*, and, being stated, is *on the way* to its solution.

Those who admit the possibility of the common origin of all physical force, and also acknowledge the principle of conservation, apply that principle to the sum total of the force. Though the amount of mechanical force (using habitual language for convenience sake) may remain unchanged and definite in its character for a long time, yet when, as in the collision of two equal inelastic bodies, it appears to be lost, they find it in the form of heat; and whether they admit that heat to be a continued mechanical action (as is most probable), or assume some other idea, as that of electricity, or action of a heat-fluid, still they hold to the principle of conservation by admitting that the sum of force, *i. e.* of the "cause of action," is the same, whatever character the effects assume. With them the convertibility of heat, electricity, magnetism, chemical action and motion is a familiar thought; neither can I perceive any reason why they should be led to exclude, *a priori*, the cause of gravitation from association with the cause of these other phænomena respectively. All that they are limited by in their various investigations, whatever directions they may take, is the necessity of making no assumption directly contradictory of the conservation of force applied to the sum of all the forces concerned, and to endeavour to discover the different directions in which the various parts of the total force have been exerted.

Those who admit separate forces inter-unchangeable, have to show that each of these forces is separately subject to the principle of conservation. If gravitation be such a separate force, and yet its power in the action of two particles be supposed to be diminished fourfold by doubling the distance, surely some new action, having true gravitation character, and that alone, ought to appear, for how else can the totality of the force remain unchanged? To define the force as "a simple attractive force exerted between any two or all the particles of matter, with a strength varying inversely as the square of the distance," is not to answer the question; nor does it indicate or even assume what are the other complementary results which occur; or allow the supposition that such are necessary: it is simply, as it appears to me, to *deny* the conservation of force.

As to the gravitating force, I do not presume to say that I have the least idea of what occurs in two particles when their power of mutually approaching each other is changed by their being placed at different distances; but I have a strong conviction, through the influence on my mind of the doctrine of conservation, that there is a change; and that the phænomena resulting from the change will probably appear some day as the result of careful research. If it be said that "'t were to consider too curiously to consider so," then I must dissent: to refrain to consider would be to ignore the principle of the conservation of force, and to stop the inquiry which it suggests,—whereas to admit the proper logical force of the principle in our hypotheses and considerations, and to permit its guidance in a cautious yet courageous course of investigation, may give us power to enlarge the generalities we already possess in respect of heat, motion, electricity, magnetism, &c., to associate gravity with them, and perhaps enable us to know whether the essential force of gravitation (and other attractions) is internal or external as respects the attracted bodies.

Returning once more to the definition of the gravitating power as "*a simple attractive force exerted between any two or all the particles or masses of matter at every sensible distance, but with a* STRENGTH VARYING *inversely as the square of the distance*," I ought perhaps to suppose there are many who accept this as a true and sufficient description of the force, and who therefore, in relation to it, deny the principle of conservation. If both are accepted and are thought to be consistent with each other, it cannot be difficult to add words which shall make "varying strength" and "conservation" agree together. It cannot be said that the definition merely applies to the *effects* of gravitation as far as we know them. So understood, it would form no barrier to progress; for, that particles at different distances are urged towards each other with a power varying inversely as the square of the distance, is a truth: but the definition has not that meaning; and what I object to is the pretence of knowledge which the definition sets up when it assumes to describe, not the partial effects of the force, but the nature of the force as a whole.

XXV. *On a Method of Observation applied to the study of some Metamorphic Rocks; and on some Molecular Changes exhibited by the action of Acids upon them. By* ALPHONSE GAGES, *M.R.I.A.**

CHEMICAL analysis makes us acquainted with the constituents of rocks, and with the relative proportions in which they are combined; but, generally speaking, it can tell us no-

* Communicated by the Author, having been read at the Meeting of the British Association at Leeds.