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now grows in Europe is about fifty-three degrees. The paper concluded by stating that it would be impossible, by any arrangement of the relative positions of land and water, to produce for the northern hemisphere a climate which would explain the phenomena in a satisfactory manner. It must only be admitted that we are face to face with a problem whose solution, in all probability, must be attempted, and, doubtless, completed, by the astronomer."

I have now in my hands a paper which I am about to read, after a few words of explanation. It is written by a gentleman, a practical chemist, who had heard that Mr. Hopkins's paper would be read here this evening, and among other things that it would call attention to the now impugned doctrine that granite is an igneous formation. A friend of mine, and a member of the Institute, now present, knowing that this gentleman had been engaged in making experiments on granite, and that his conclusions were opposed to those of Mr. Hopkins, let him know that we were about to discuss this subject; and I requested that he might be invited to send us a paper giving his results, that we might hear both sides. He had said that he supposed we did not care for "facts" in this Institute; to which I replied that *facts* were what we especially cared for. I am, therefore, about to read what he has sent me,—not as a regular paper, that has been presented in the ordinary way and passed the council,—but I wish to bring it before you with this explanation; and I wish myself individually to do so, all the more, because I have, in the *Scientia Scientiarum*, and on other occasions, called public attention to the fact that the theory of granite being an igneous formation had been given up by geologists. I believe Mr. Hopkins was one of the first, if not the very first, who impugned that doctrine; for he did so nearly thirty years ago. It is certainly now acknowledged by Sir Charles Lyell, and Mr. Hamilton, the President of the Geological Society, and indeed by all "authorities" among geologists, that it was an error to suppose that granite is an igneous crystallization, or that the centre of the earth is now in an incandescent state, heated up to 195,000 degrees of temperature, as had been deduced from the nebular hypothesis. I cannot, however, say that this paper (which is by Mr. Lewis Thompson, M.R.C.S.), carries conviction to my mind. I rather think Mr. Hopkins will claim some of its facts as being rather upon his side, but that is the author's look-out. I only wish to put the arguments forward, even although I am not convinced by them, because we do wish in this society to hear all sides of every question we take up. But Mr. Thompson, I must add, although he does not believe in the aqueous formation of granite, is by no means a supporter of the nebular theory; and he endeavours to destroy that hypothesis, while believing in the igneous formation of granite. So that if Mr. Thompson's experiments are sufficient and his reasons sound, we shall have the nebular theory twice slain—first by water, and now again by fire! But let us hear Mr. Thompson himself. His paper is as follows:—

The object of the present paper is to institute an unprejudiced comparison between certain well-established facts and a particular theory of the formation of the earth, known as the "Nebular Theory." According to this theory,

the earth was at one time an immense volume of white-hot vapour, which, by loss of heat and subsequent condensation, was resolved into a globular mass of white-hot fluid, that slowly cooled down, and after an enormous lapse of time became the solid compact sphere upon which we live. Much of the argument in favour of this nebular hypothesis has been drawn from the fact that the substance called "granite," and which forms a great part of the crust of the earth, bears upon it distinct evidences of igneous fusion at some previous period of its existence. Admitting, then, the fact that much of the granite of the earth was once in a perfect state of fusion,—I ask, does the solidified granite of the present day afford, as it ought to do, undeniable proofs that it has cooled down and become solid in the extremely slow and gradual manner implied in the nebular theory? Now, I have examined a great number of specimens of granite from various parts of the world, and so far from supporting the nebular theory, they all tend to show the extreme inaccuracy of that theory. Such, at least, is my opinion; but upon this point I leave every one to form an opinion for himself, merely remarking that the experiments and results I am now about to relate may be, and I hope will be, repeated and verified or contradicted by many other inquirers after truth. I have said that granite ought to afford undeniable proofs of the rate at which it has cooled down and become solid from the fused condition, and I will here explain in what those proofs consist. Granite is made up of an aggregation of three or four different substances, which merely cohere together, and have been designated by mineralogists as felspar, quartz, mica, and hornblende. But if these substances were once in fusion, and then constituted one uniform fluid, it is clear that in cooling they must have obeyed the existing laws of chemical affinity, and have arranged themselves into their present relative positions before the period of actual solidification. That they did obey the ordinary laws of chemical affinity has been proved by their analysis, which shows that they have been formed in accordance with the rules of atomic proportion; and that the law of gravitation was then in force will not be denied by the nebular theorists, since it is upon this law that their whole theory rests. If, then, the substances constituting granite, that is to say, the felspar, quartz, mica, and hornblende, segregated themselves during the period of fusion, and were at the same time subject to the law of gravitation, it is beyond doubt that they would arrange their respective positions as regards each other in the exact order of their gravitation, or, as it is called, in accordance with the attraction of gravitation, just as we see a piece of lead sink in water, and oil swim upon its surface. That the said process of solidification was not rapid but extremely slow requires no illustration, for this constitutes a part of the nebular theory; consequently, there was abundant time to meet the requirements of gravitation. But it may be urged that perhaps the gravitating power of all these substances may be alike and uniform, consequently, there might be no disposition for any one of them to sink under or to swim upon the others, and therefore, if felspar, quartz, mica, and hornblende possess exactly the same specific gravity, a melted mixture of them would probably cool down into just such a regularly arranged granular mass as is exhibited to us by an ordinary piece of granite. Here, then, was a practical question: are the components of granite all alike in specific weight, or are they different? It is now more than two years ago since I set myself down to investigate this matter, and during that time I have examined granite obtained from almost every quarter of the globe—from Siberia, Norway, Saxony, Scotland, Ireland, Cornwall, the Mont Cenis Tunnel, Upper Egypt, the Himalaya Mountains, the Cape of Good Hope, Australia, New Zealand, Patagonia, California, and Nova Scotia. As a result of this labour, I am enabled to say most authoritatively, that not only do the components in question differ in their specific gravities, but that this difference is sufficiently

great to render the production of granite under the conditions of the nebular theory an utter impossibility. In fact, had the cooling and solidification of granite taken place in the slow and gradual manner indicated by that theory, it is certain that the felspar, quartz, mica, and hornblende would have arranged themselves into at least three separate and distinct layers, having not the least resemblance to granite. The uppermost of these layers would have been felspar, the next quartz, and the lowest would have consisted of mica and hornblende. Such a view, be it observed, is in strict accord with the laws of nature, and, in making the assertion, I become only the exponent of that force which is known as the attraction of gravitation. By an average of all my experiments I found the specific gravity of granite to be 2.654; that of felspar derived from granite 2.45; that of quartz 2.63, and that of mica and hornblende to vary from 2.82 to 3.17. If, therefore, we suppose a mixture of felspar, quartz, mica, and hornblende fused together into one fluid, and then left to cool gradually for many days under the influence of gravitation, it is undeniable that these ingredients would separate and form distinct layers exactly in the order which I have pointed out, just as mud under the same influence falls to the bottom of water, and cream rises to the surface of milk. Viewing, then, the incredible time assumed in the nebular theory for the cooling and solidification of the whole globe, it ought to follow in the face of these different specific gravities that the separation of the felspar, quartz, mica, and hornblende should be found most complete and perfect; whereas in granite we find nothing but evidences of an imperfectly crystallized and hastily cooled mass. The evidences of chemical absurdity in the nebular theory do not, however, stop at this point. By that theory it is asserted that after the vapour period the earth remained for many ages in the form of a fluid sphere, subject meanwhile to the influence of gravitation, so that all the heaviest and most fixed of its elements ought to have settled down towards the centre of the globe. As, however, we find, even in the outer crust, highly ponderous bodies like gold and platinum, we have a right to infer from the above theory that the portion of the earth under that crust is composed of matters having an enormous specific gravity. But we know by experiment that platinum is more than twenty-one times heavier than an equal bulk of water, and, following out the nebular hypothesis, we are compelled to conclude that the specific gravity of the whole globe is at least equal to that of platinum. Nevertheless, it has been proved by the most careful calculations that the whole earth, viewed as a planet, cannot be more than five or six times heavier than its own bulk of pure water; so that it is impossible for its interior to be filled with substances heavier, or even so heavy, as gold and platinum.

Having satisfied myself that granite could not have been produced according to the slow nebular notion, I determined to try what effect rapid cooling would have upon fused granite. For this purpose a cavity was chiselled out in a lump of Aberdeen granite, and a piece of granite from the Himalaya mountains in India was placed in this cavity. The piece in question weighed 740 grains, and, by the action of a powerful oxy-hydrogen blow-pipe, it was fused in less than five minutes into a fluid, having the consistency of thin syrup, and being then allowed to cool, in less than two minutes it became solid. When quite cold, the fused mass was detached and examined. Its resemblance to the mineral called obsidian, proved most striking, so much so indeed, that when compared with a sample of dark-coloured obsidian from Iceland, it was only with great difficulty identified. To granite it had not the least resemblance, and as I had entirely repudiated the idea of the production of granite by slow cooling, so now I abandoned all thoughts of a rapid cooling process; consequently nothing remained but a supposition that some length of time, though not a long time, had been employed in the granitic formation.

Looking round for something analogous to this in the processes of our manufacturing industry, I was not long in discovering one which not only resembles, but most singularly illustrates that formation. It is a process in which a fused fluid is employed, composed of different substances, having different colours and different specific gravities exactly as in the case of the components of granite; and this fluid can be cooled down rapidly or slowly, or in a way that lies between these extremes; and the results of these different rates of cooling and solidification may be watched and recorded. The substance in question is the article known by the name "mottled soap," which, as any one may see, has, when recently cut, very much of the appearance of Scotch granite. This substance on being taken from the copper, is a fused fluid, and if a portion of it is cooled rapidly, it concretes into a homogeneous solid of a dark uniform hue, somewhat like our artificial obsidian; if, however, the fused soap is cooled very slowly, the dark-coloured portions of it, which are also the highest in specific gravity, all fall to the bottom, and leave the upper portions quite white, and free from any colour or mottling. The art of making mottled soap consists in so arranging the time of cooling, as to allow the dark-coloured parts to gather themselves together in little masses, by the time the whole of the soap is so cooled as to begin to solidify, and thus prevent the descent of the heavy dark portions. The imperfectly crystallized state of granite, and the uniform diffusion throughout its whole substance of the dark and ponderous particles of mica and hornblende, all bespeak a result so identical with that produced by the above process, as to leave no doubt on an unprejudiced mind of similarity in the cause of their production. Now, it so happens, that the period of time in which the separation of the "mottle" and thickening or solidification of the soap takes place, is from twenty-four to thirty-six hours; and if I had never read in the Bible anything to guide me as to the time employed in the solidification of granite, I should have unhesitatingly fixed upon the above hours as the only period in which granite could possibly have been formed. That at the creation it was formed by the agency of the ordinary laws of nature, I entirely deny, for by these laws the interior of a large mass of non-conducting material like granite could never lose its heat so rapidly as to prevent crystallization; in proof of which, we see in extensive eruptions of volcanic lava that require years to cool, there are produced large, distinct, and well-defined crystals of basic felspar, to which mineralogists have given the name "Leucite," from their white colour; and this alone might serve to satisfy us that granite had not been slowly cooled.

With regard to the theory which considers granite to have been formed by solution from water, I feel that very little need be said. There are certainly many strong arguments of a chemical nature that stand in direct opposition to such a hypothesis; but I shall content myself by bringing forward only one objection to it. It is this, that all the water in our planet is quite insufficient to dissolve the solid portion, even if that solid portion were as soluble as common salt. A saturated solution of common salt consists of twenty-seven parts of salt, and seventy-three parts of water; consequently these would require to be the relative proportions of land and water according to this preposterous assumption of solubility. But if the specific gravity of the whole globe be 5.5, then these twenty-seven parts, or in other words the solid portion of the globe, must have a specific gravity of 17.7, which would seem to indicate that nearly all the solid matter was pure gold. In reality, however, granite, if it be soluble at all in water, is so to a very trifling extent; and to assume that it can be dissolved in 1,000 times its weight of water is therefore, to say the least of it, greatly favouring the water hypothesis. But, if the whole of the solid matter of the globe had ever been dissolved in 1,000 times its weight of water, then from the gravity of the earth it follows that the specific weight of

that solid matter must have been 4,500 times greater than that of water, and more than 200 times heavier than platinum! As to the action of water at high temperatures and under enormous pressures, it would seem that the originators of this idea are ignorant of the fact that water can only be heated up to a certain point under any pressure, without ceasing to be water. Thus M. Cagniard de la Tour long ago proved by experiment, that ether contained in a sealed-up tube and heated, became wholly converted into vapour in a space twice its original bulk, and with a pressure of between thirty-seven and thirty-eight atmospheres: alcohol did the same, exerting a pressure of 119 atmospheres; and water became altogether vapour at a temperature below that of melting zinc. To talk, therefore, of the action of red-hot or white-hot water, is simply ridiculous.

I have now arrived at that stage of my undertaking in which nothing more remains than for me to describe the simple means employed for determining the specific gravity of the constituent parts of granite, and I do this with a pleasant hope that others will be induced to repeat my labours. To ascertain whether the granite contained combined water, the sample was placed with some chloride of calcium for twenty-four hours in the exhausted receiver of an air-pump; it was then carefully weighed and heated red hot, but in no instance did any loss of weight occur; therefore granite does not contain combined water. The granite was next reduced to a coarse powder, and 500 grains of this were put into an ordinary 1,000 grain specific gravity bottle, which, being filled up with distilled water, was weighed, and the weight so found deducted from 1,500 gave a result to be used as a divisor of the 500 grains of granite, from the product of which the specific gravity of the granite was found. And I will here remark that this mode is more accurate than the common plan of weighing in water a single piece; because there are always fissures and sometimes cavities in minerals, and these fissures remain filled with air and buoy up the mineral so as to vitiate the result. To obtain the specific gravity of felspar, quartz, mica, and hornblende, the same process was followed; but much trouble requires to be taken for the purpose of separating these components of granite from each other. It is not difficult to separate the mica and hornblende from the quartz and felspar after the granite has been coarsely powdered; but it requires a strong light, good eyesight, and much patience to pick out the mica from the hornblende, and still more to separate the quartz from the felspar; and this last constitutes, in fact, the greatest difficulty in the whole proceeding.

Having read this paper, I must now once more repeat that it does not carry conviction to my mind. Without attempting to criticise it throughout, I shall briefly notice one or two of the points wherein it appears to me to be defective. Mr. Thompson promised us *facts*; but he has only given us the result of a single experiment. And what does it teach us? Not, in my opinion, what he draws from it. He melts a few hundred grains of granite and lets it cool; and he obtains something like obsidian. He tells us this fused granite cooled rapidly; and he *assumes* that had it cooled more slowly it would have cooled into granite, instead of obsidian. I must demur to that assumption. The experiment appears to me only to prove that, if the materials of granite were ever in a state of fusion, the result would be some homogeneous matter like obsidian, and not granite. He thinks the result would have been different if the fused granite had been more slowly cooled. But he has not verified that by experiment. He has given us no facts to prove this conclusion. I will notice another point where the reasoning does

not satisfy me. You will remember in one part of his paper Mr. Thompson objects to the predominance of water in the earth, and states that if that were the case, then the specific gravity of its solid parts must be nearly that of gold. Now, were that so, not only should we have a new *Plutonic* theory! but it would really after all be only in accordance with what was stated in the address of Mr. Grove, as President of the British Association, last August—namely, that instead of the heaviest matter of the earth being near its surface (as we have long been taught), it is probably more solid and heavier as it gets nearer the centre. But apparently Mr. Thompson's sole reason for rejecting this, is merely that it is contrary to the Newtonian theory as to the mass of the whole earth; for it is upon that theoretical assumption, and not upon *facts*, that the whole reasoning is based. It is enough for me to point out, that at any rate, that theory has not stood in the way of Mr. Grove propounding, as now most probable, what is not only contrary to the Newtonian doctrine as to the earth's mass, but also to the nebular notion that the earth's centre is filled with matter in a state of igneous fluidity. In conclusion, I am obliged to say that if we consider that MM. Daubrée and Bischoff made certain experiments with granite which convinced them that it is a watery crystallization, and also that they have brought over the leading geologists to this view, although it was contrary to all their preconceived notions and previous teaching, I think it was incumbent upon Mr. Thompson to have noticed the experiments of these eminent chemists, and, if he could, to have shown where they were defective and faulty; and not merely to have made a detached and single experiment of his own, which appears to prove very little, and even that little, in my opinion, to be rather against what he deduces from it.

MR. HOPKINS.—I can see clearly, from the observations of Mr. Thompson, that he has been making experiments from cabinet specimens of granite. Suppose you were to make experiments from cabinet specimens of wood, to ascertain something as to the sap of a tree in its living state, you would obtain very strange results! Now, if you want to ascertain the real constitution of granite, you should study the granite *in situ*. For instance, in one place you may have a granite undergoing change. That granite is composed of hornblende, felspar, mica, and so on, and is undergoing lamination. If you take a piece of that granite, and cut a block of it, and weigh it, you will find that it loses weight after exposure to heat, just the same as minerals. We allow so much for water, and we call that water *mechanically* combined. Granite is saturated with water; it is always saturated, and is not a mere dry block.—

REV. W. MITCHELL.—May I ask you, Mr. Hopkins, to answer one question, as you are well acquainted with deep mines, Whether you can go to any depth where you do not find water; and whether water is not the greatest enemy of the miner?

MR. HOPKINS.—It is the most difficult thing the miner has to contend with, and you cannot go to any depth without finding it. Wherever you go, you come to water, whether in granite or any other formation. With refer-

ence to the constitution of granite, if you take separate crystals, you will also find that each crystal has a certain proportion of water *chemically* or *mineralogically* combined ; and if you drive it out, the crystal becomes opaque, and loses weight, the quantity varying from two or three to twenty per cent. Without water, crystals are not formed, especially rock-crystals. Again you may have granite, with gold in saturation. In another place you will find the gold becoming gradually developed out of the granite as the granite undergoes changes, and coming out like large round balls. Elsewhere you find a little gold in dissemination, but not like the *other*. There is change constantly going on ; the condition of the rocks is never stationary, but it either changes into lamination, or into fractures, something like the bark on the trunk of a tree. Now, I say we have such an immense accumulation of facts, that we ought now to insist upon facts ; and not go on trying to find out what is in the centre of the earth, and so on. Let us attend to facts as we find them, and see what we really have ; and let us leave theories for the future. I will add one or two words with regard to minerals. I have no hesitation in stating that I will go to any rock and say what it contains by looking at it. If you let me see a good surface of it, I will state whether it contains gold, silver, tin, and so on. I am speaking as to the metal the rock will contain, and not as to the quantity of the metal, for that will depend on the amount of deposits and accumulations, but I am referring only to the nature of the constituents.

The CHAIRMAN.—I shall only make a few observations from my own point of view, in confirmation of what Mr. Hopkins has said with regard to the formation of granite. In doing so I may express some of my objections to the theory advanced by Mr. Thompson. The experiment performed by the latter gentleman on a small scale, as Mr. Hopkins has reminded us, is wrought out by nature on the most gigantic scale. Wherever we find active volcanoes, we find them melting granite, or some other primary rock. Lava, obsidian, pitchstone, and such-like volcanic products, are but molten primary rocks. Now I ask what analogy do any of these substances bear in their structure to the so-called primary rocks ? Are they anything like granite, for instance ? Mr. Thompson admits that the structure of granite could not be formed from any of these substances by slow cooling. That I take to be an important admission. I cannot believe it is produced by quick or any intermediate rate of cooling. We have not to go far even in London for a practical demonstration of the structure of the primary rocks. Our bridges and public buildings show us that granite is composed of well-formed crystals of several distinct minerals, interlacing one another in every direction ;—crystals of quartz, mica, and felspar. On London or Southwark Bridge you may see crystals of the latter substance as large, or larger, than your hand, presenting to the casual observer the appearance of large fossil bones. The constituents of granite not only contain water chemically united to them, but they also contain water mechanically diffused,—a fact which can hardly be reconciled with their production by crystallization from a molten mass. Now let us consider the crystalline constituents of granite—we have crystals of quartz,

consisting of silica in a state more or less free from admixture with foreign substances. Then we have the crystals of mica and felspar, the most composite of mineral substances. These three substances are distinct from one another in crystalline and chemical composition. But then the micas and felspars admit of the greatest and most puzzling varieties of chemical constitution; one chemical element taking the place of another, without altering the crystalline character of the mica or the felspar in which the change of composition is found. We may have some conception of the composite structures and varieties of these minerals, when we state that nearly all, if not all, the metals and the mineral constituents of the sedimentary rocks may be found in the granites or other primary rocks. We have potash and also soda felspars. In the micas as well as the felspars we have not only the principal constituents, silica and alumina, but also soda, potash, lime, iron, magnesia, and water, replacing each other with most puzzling variations. We all know how gold is diffused through the quartz of some kinds of granite. The microscope is said also to reveal native iron among the constituents of granite. Doubtless all the metals and other minerals found in the cracks and crevices of the primary rocks were once in combination with these rocks. But I never could form any clear conception of the origin of metallic and mineral veins till I read Mr. Hopkins's work on the subject. Very high geological and mineralogical authorities used to speak of gold as the most recent of all the metals;—how more recent than others, I could not conceive. Some went so far as to imagine some recent geological event, when, as it were, a golden shower had fallen from heaven to earth! The experiments of Daubr e and Bischoff have proved the mechanical and chemical combination of water in granite. Though the authorities of the Geological Society were not convinced by Mr. Hopkins, their faith in the igneous origin of granite was first shaken, I believe, by my friend Mr. Clifton Sorby's microscopical researches. By investigating microscopically the minute bubbles in crystals, he was able to determine whether the crystal was formed from an aqueous or some other liquid solution, or produced by cooling from a molten mass. With regard to Mr. Thompson's assumption of the insolubility of silica in water, the geysers in Iceland afford a direct refutation of this. How, again, without the solubility of silica, can we account for the formation of silicified woods, without injury to the most delicate vegetable fibres? Dr. Bowerbank has shown that the most delicate structures in sponges (which he had found destroyed by decomposition only a few hours after the death of the sponge), are faithfully and perfectly preserved in the flint. Before electro-metallurgy was discovered, we could form no idea as to the method nature takes to separate metals from the rocks through which they may be diffused. We have now, however, learnt the power of electricity in separating metals from the aqueous solutions of their salts. Soon after the discovery of this fact, a copper electrotype was produced without any artificial battery, by imbedding wires in two different strata of a mine, and using the galvanic current thus produced. Here then we have a demonstration of the electro-magnetic action of the earth, and of its power in the formation of mineral products. This

goes far, in my opinion, to show that Mr. Hopkins's hypothesis of the formation of metallic veins is one well supported by facts which come under our observation, analogous to those he attributes to the natural magnetic currents of the globe, operating constantly, though almost imperceptibly, on a large scale.

The meeting was then adjourned.