It is indeed a strange fact that any one of us sitting quietly at his table could, on being told the two numbers just mentioned, draw out a curve on ruled paper, from which thousands of vertical lines might be chalked side by side on a wall, at the distance apart that is taken up by each man in a rank of American soldiers, and know that if the same number of these American soldiers taken indiscriminately had been sorted according to their heights and marched up to the wall, each man of them would find the chalked line which he found opposite to him to be of exactly his own height. So far as I can judge from the run of the figures in the table, the error would never exceed a quarter of an inch, except at either extremity of the series.

The principle of the law of deviation is very simple. The important influences that acted upon each pellet were the same; namely, the position of the point whence it was dropped, and the force of gravity. So far as these are concerned, every pellet would have pursued an identical path. But in addition to these there were a host of petty disturbing influences, represented by the spikes among which the pellets tumbled in all sorts of ways. The theory of combination shows that the commonest case is that where a pellet falls equally often to the right of a spike as to the left of it, and therefore drops into the compartment vertically below the point where it entered the harrow. It also shows that the cases are very rare of runs of luck carrying the pellet much oftener to one side than the other. The law of deviation is purely numerical; it does not regard the fact whether the objects treated of are pellets in an apparatus like this, or shots at a target, or games of chance, or any other of the numerous groups of occurrences to which it is or may be applied.[1]

I have now done with my description of the law. I know it has been tedious, but it is an extremely difficult topic to handle on an occasion like this. I trust the application of it will prove of more interest.

(*To be continued.*)

ON THE STRUCTURE AND ORIGIN OF METEORITES[2]

THE study of meteorites is naturally divisible into several very distinct branches of inquiry. Thus in the first place we may regard them as shooting stars, and observe and discuss their radiant points and their relation to the solar system. This may be called the astronomical aspect of the question. Then, when solid masses fall to the ground, we may study their chemical composition as a whole, or that of the separate mineral constituents; and lastly, we may study their mechanical structure, and apply to this investigation the same methods which have yielded such important results in the case of terrestrial rocks. So much has been written on the astronomical, chemical, and mineralogical aspect of my subject by those far more competent than myself to deal with such questions, that I shall confine my remarks almost entirely to the mechanical structure of meteorites and meteoric irons, and more especially to my own observations, since they will, at all events, have the merit of greater originality and novelty. Time will, however, not permit me to enter into the detail even of this single department of my subject.

In treating this question it appeared to me very desirable to exhibit to you accurate reproductions of the natural objects, and I have therefore had prepared photographs of my original drawings, which we shall endeavour to show by means of the oxyhydrogen lime-light, and I shall modify my lecture to meet the requirements of the case, exhibiting and describing special examples, rather than attempt to give an account of meteorites in general. Moreover, since the time at my disposal is short, and their external characters may be studied to great advantage at the British Museum, I shall confine my remarks as much as possible to their minute internal structure, which can be seen only by examining properly prepared sections with more or less high magnifying powers.

By far the greater part of my observations were made about a dozen years ago. I prepared a number of sections of meteorites, meteoric irons, and other objects which might throw light on the subject, and my very best thanks are due to Prof. Maskelyne for having most kindly allowed me to thoroughly examine the very excellent series of thin sections, which had been prepared for him. During the last ten years my attention has been directed to very different subjects, and I have done little more than collect material for the further and more complete study of meteorites. When I have fully utilised this material I have no doubt that I shall be able to make the subject far more complete, and may find it necessary to modify some of my conclusions. I cannot but feel that very much more remains to be learned, and I should not have attempted to give an account of what I have so far done, if I had not been particularly asked to do so by Mr. Lockyer. At the same time I trust that I shall at all events succeed in showing that the microscopical method of study yields such well marked and important facts, that in some cases the examination of only a single specimen serves to decide between rival theories.

In examining with the naked eye an entire or broken meteorite we see that the original external outline is very irregular, and that it is covered by a crust, usually, but not invariably black, comparatively thin, and quite unlike the main mass inside. This crust is usually dull, but sometimes, as in the Stannern meteorite, bright and shining, like a coating of black varnish. On examining with a microscope a thin section of the meteorite, cut perpendicular to this crust, we see that it is a true black glass filled with small bubbles, and that the contrast between it and the main mass of the meteorite is as complete as possible, and the junction between them sharply defined, except when portions have been injected a short distance between the crystals. We thus have a most complete proof of the conclusion that the black crust was due to the true igneous fusion of the surface under conditions which had little or no influence at a greater depth than $\frac{1}{100}$th of an inch. In the case of meteorites of different chemical composition, the black crust has not retained a true glassy character, and is sometimes $\frac{1}{50}$th of an inch in thickness, consisting of two very distinct layers, the internal showing particles of iron which have been neither melted nor oxidised, and the external showing that they have been oxidised and the oxide melted up with the surrounding stony matter. Taking everything into consideration, the microscopical structure of the crust agrees perfectly well with the explanation usually adopted, but rejected by some authors, that it was formed by the fusion of the external surface, and was due to the very rapid heating which takes place when a body moving with planetary velocity rushes into the earth's atmosphere—a heating so rapid that the surface is melted before the heat has time to penetrate beyond a very short distance into the interior of the mass.

When we come to examine the structure of the original interior part of meteorites, as shown by fractured surfaces, we may often see with the naked eye that they are mottled in such a way as to have many of the characters of a brecciated rock, made up of fragments subsequently cemented together and consolidated. Mere rough fractures are, however, very misleading. A much more accurate opinion may be formed from the examination of a smooth flat surface. Facts thus observed led Reichenbach to conclude that meteorites had been formed by the

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[1] Quetelet, apparently from habit rather than theory, always adopted the binomial law of error, basing his tables on a binomial of high power. It is absolutely necessary to the theory of the present paper, to get rid of binomial limitations and to consider the law of deviation or error, in its exponential form.

[2] Abstract of lecture delivered by H. C. Sorby, F.R.S., &c., at the Museum, South Kensington, on March 10.

collecting together of the fragments previously separated from one another in comets, and an examination of thin transparent sections with high magnifying powers and improved methods of illumination, proves still more conclusively their brecciated structure. The facts are, however, very complex, and some are not easily explained. Leaving this question for the present, I will endeavour to point out what appears to be the very earliest history of the material, as recorded by the internal structure.

It is now nearly twenty years since I first showed that the manner of formation of minerals and rocks may be learned from their microscopical structure. I showed that when crystals are formed by deposition from water or from a mass of melted rock, they often catch up portions of this water or melted stone, which can now be seen as cavities containing fluid or glass. We may thus distinguish between crystalline minerals formed by purely aqueous or by purely igneous processes; for example, between minerals in veins and minerals in volcanic lavas. In studying meteorites it appeared to me desirable, in the first place, to ascertain whether the crystalline minerals found in them were originally formed by deposition from water or from a melted stony material analogous to the slags of our furnace or the lava of volcanoes. One of the most common of the minerals in meteorites is olivine, and when met with in volcanic lavas this mineral usually contains only a few and small glass cavities in comparison with those seen in such minerals as augite. The crystals in meteorites are, moreover, only small, and thus the difficulty of the question is considerably increased. However, by careful examination with high magnifying power, I found well-marked glass-cavities, with perfectly fixed bubbles, the inclosed glass being sometimes of brown colour and having deposited crystals. On the contrary I have never been able to detect any trace of fluid-cavities, with moving bubbles, and therefore it is very probable, if not absolutely certain, that the crystalline minerals were chiefly formed by an igneous process, like those in lava, and analogous volcanic rocks. These researches require a magnifying power of 400 or 600 linear.

Passing from the structure of the individual crystals to that of the aggregate, we find that in some cases we have a structure in every respect analogous to that of erupted lavas, though even then there are very curious differences in detail. By methods like those adopted by Daubrée, there ought to be no more difficulty in artificially imitating the structure of such meteorites than in imitating that of our ordinary volcanic rocks. It is, however, doubtful whether meteorites of any considerable size uniformly possess this structure. The best examples I have seen are only fragments inclosed in the general mass of the Petersburg meteorite, which, like many others, has exactly the same kind of structure as that of consolidated volcanic tuff or ashes. This is well shown by the Bialystock meteorite, which is a mass of broken crystals and more complex fragments scattered promiscuously through a finer-grained consolidated dust-like ash.

Passing from this group of meteorites, which are more or less analogous to some of our terrestrial volcanic rocks, we must now consider the more common varieties, which are chiefly composed of olivine and other allied minerals. The Mezö Madaras meteorite is an excellent illustration, since the outline of the fragments is well seen, on account of the surrounding consolidated fine material being of dark colour. In it we see more or less irregular spherical and very irregular fragments scattered promiscuously in a dark highly consolidated fine-grained base. By far the larger part of these particles do not either by their outline or internal structure furnish any positive information respecting the manner in which they were formed, but careful examination of this and other analogous meteorites, has enabled me to find that the form and structure of many of the grains is totally unlike that of any I have ever seen in terrestrial rocks, and points to very special physical conditions. Thus some are almost spherical drops of *true glass* in the midst of which crystals have been formed, sometimes scattered promiscuously, and sometimes deposited on the external surface, radiating inwardly; they are, in fact, partially devitrified globules of glass, exactly similar to some artificial blow-pipe beads.

As is well known, glassy particles are sometimes given off from terrestrial volcanoes, but on entering the atmosphere they are immediately solidified and remain as mere fibres, like *Pelé's hair*, or as more or less irregular laminæ, like pumice dust. The nearest approach to the globules in meteorites is met with in some artificial products. By directing a strong blast of hot air or steam into melted glassy furnace slag, it is blown into spray, and usually gives rise to pear-shaped globules, each having a long hair-like tail, which is formed because the surrounding air is too cold to retain the slag in a state of perfect fluidity. Very often the fibres are the chief product. I have never observed any such fibres in meteorites. If the slag be hot enough, some spheres are formed without tails, analogous to those characteristic of meteorites. The formation of such alone could not apparently occur unless the spray were blown into an atmosphere heated up to near the point of fusion, so that the glass might remain fluid until collected into globules. The retention of a true vitreous condition in such fused stony material would depend on both the chemical composition and the rate of cooling, and its permanent retention would in any case be impossible if the original glassy globule were afterwards kept for a long time at a temperature somewhat under that of fusion. The combination of all these conditions may very well be looked upon as unusual, and we may thus explain why grains containing true glass are comparatively very rare; but though rare they point out what was the origin of many others. In by far the greater number of cases the general basis has been completely divitrified, and the larger crystals are surrounded by a fine-grained stony mass. Other grains occur with a fan-shaped arrangement of crystalline needles, which an uncautious, non-microscopical observer might confound with simple concretions. They have, however, a structure entirely different from any concretions met with in terrestrial rocks, as for example that of oolitic grains. In them we often see a well-marked nucleus, on which radiating crystals have been deposited equally on all sides, and the external form is manifestly due to the growth of these crystals. On the contrary the grains in meteorites now under consideration have an external form *independent of the crystals*, which do not radiate from the centre, but from one or more places on the surface. They have, indeed, a structure absolutely identical with that of some artificial blowpipe beads which become crystalline on cooling. With a little care these can be made to crystallise from one point, and then the crystals shoot out from that point in a fan-shaped bundle, until the whole bead is altered. In this case we clearly see that the form of the bead was due to fusion, and existed prior to the formation of the crystals. The general structure of both these and the previously described spherical grains also shows that their rounded shape was not due to mechanical wearing. Moreover, melted globules with well-defined outline could not be formed in a mass of rock pressing against them on all sides, and I therefore argue that some at least of the constituent particles of meteorites were originally detached glassy globules, like drops of fiery rain.

Another remarkable character in the constituent particles of meteorites is that they are often mere fragments, although the entire body before being broken may originally have been only one-fortieth or one-fiftieth of an inch in diameter. It appears to me that thus to break such minute particles when they were probably in a separate state, mechanical forces of great intensity would be required. By far the greater number of meteorites have a structure which indicates that this breaking up of the constituents was of very general occurrence.

Assuming then that the particles were originally detached like volcanic ashes, it is quite clear that they were subsequently collected together and consolidated. This more than anything else appears to me a very great difficulty in the way of our adopting Reichenbach's cometary theory. Volcanic ashes are massed together and consolidated into tuff, because they are collected on the ground by the gravitative force of the earth. It appears to me very difficult to understand how in the case of a comet there could be in any part a sufficiently strong gravitative force to collect the dispersed dust into hard stony masses like meteorites. If it were not for this apparent difficulty we might suppose that some of the facts here described were due to the heat of the sun, when comets approach so near to it that the conditions may be practically almost solar. Comets may and probably do contain many meteorites, but I think that their structure indicates that they were originally formed under conditions far more like those now existing at the surface of the sun than in comets.

The particles having been collected together, the compound mass has evidently often undergone considerable mechanical and crystalline changes. The fragments have sometimes been broken *in situ*, and "faulted;" and crystallisation has taken place, analogous to that met with in metamorphic rocks, which has more or less, and sometimes almost entirely, obliterated the original structure. The simplest explanation of this change is to suppose that after consolidation meteorites were variously heated to temperatures somewhat below their point of fusion. Those which have the structure of true lava may in some cases be portions which were actually remelted. We have also this striking fact, that meteoric masses of compound structure, themselves made up of fragments, have been again broken up into compound fragments, and these collected together and consolidated along with fresh material, to form the meteorites in their present condition. L'Aigle is a good example of this complex structure.

Another remarkable fact is the occurrence in some meteorites of many veins filled with material, in some respects so analogous to the black crust, that at one time I felt induced to believe that they were cracks, into which the crust had been injected. Akburfur is a good example of this, and seems to show that under whatever conditions the veins were found, they were injected not only with a black material, but also with iron and magnetic pyrites.

Taking, then, all the above facts into consideration, it appears to me that the conditions under which meteorites were formed must have been such that the temperature was high enough to fuse stony masses into glass; the particles could exist independently one of the other in an incandescent atmosphere, subject to violent mechanical disturbances; that the force of gravitation was great enough to collect these fine particles together into solid masses, and that these were in such a situation that they could be metamorphosed, further broken up into fragments, and again collected together. All these facts agree so admirably with what we know must now be taking place near the surface of the sun, that I cannot but think that, if we could only obtain specimens of the sun, we should find that their structure agreed very closely with that of meteorites. Considering also that the velocity with which the red flames have been seen to be thrown out from the sun is almost as great as that necessary to carry a solid body far out into planetary space, we cannot help wondering whether, after all, meteorites may not be portions of the sun recently detached from it by the violent disturbances which do most certainly now occur, or were carried off from it at some earlier period, when these disturbances were more intense. At the same time, as pointed out by me many years ago, some of the facts I have described may indicate that meteorites are the residual cosmical matter, not collected into planets, formed when the conditions now met with only near the surface of the sun extended much further out from the centre of the solar system. The chief objection to any great extension of this hypothesis is that we may doubt whether the force of gravitation would be sufficient to explain some of the facts. In any case I think that one or other of these solar theories, which to some extent agree with the speculations of the late Mr. Brailey, would explain the remarkable and very special microscopical structure of meteorites far better than that which refers them to portions of a volcanic planet, subsequently broken up, as advocated by Meunier, unless indeed we may venture to conclude that the material might still retain its original structure, due to very different conditions, previous to its becoming part of a planet. At the same time so little is positively known respecting the original constitution of the solar system, that all these conclusions must to some extent be looked upon as only provisional.

I will now proceed to consider some facts connected with meteoric irons. The so-called Widmanstatt's figuring, seen when some of these irons are acted on by acids, is well known; but in my opinion the preparations are often very badly made. When properly prepared, the surface may be satisfactorily examined with a magnifying power of 200 linear, which is required to show the full detail. We may then see that the figuring is due to a very regular crystallisation, and to the separating out one from the other of different compounds of iron and nickel, and their phosphides. When meteoric iron showing this structure is artificially melted, the resulting product does not show the original structure, and it has therefore been contended that meteoric iron was never in a state of igneous fusion. In order to throw light on this question, I have paid very much attention to the microscopical structure of nearly all kinds of artificial irons and steels, by studying surfaces polished with very special care, so as to avoid any effect like burnishing, and then acting on them very carefully with extremely dilute nitric acid. In this manner most beautiful and instructive specimens may be obtained, showing a very great amount of detail, and requiring a magnifying power varying up to at least 200 linear. In illustration of my subject I will call attention to only a few leading types of structure. In the first case we have grey pig-iron, showing laminæ of graphite promiscuously arranged in all positions, on the surface of which is a thin layer of what is probably iron uncombined with carbon, whilst the intermediate spaces are filled up with what are probably two different compounds of iron and carbon.

White chilled refined iron has an entirely different structure and more uniform crystallisation, the structure is very remarkable and beautiful, mainly due to the varying crystallisation of an intensely hard compound of iron and carbon, and the two other softer compounds met with in grey pig.

Malleable bar iron has an entirely different structure, and shows fibres of black slag, and a more or less uniform crystallisation of iron with a varying small amount of carbon.

Cast steel differs again very much from any of the previous. It shows a fine-grained structure, due to small radiating crystals, and no plates of graphite.

The difference between any of the above and meteoric iron is extremely great.

In the case of Bessemer metal we have a crystalline structure approaching in some places more nearly to that of meteoric iron. We see a sort of Widmanstatt's figuring, but it is due to the separation of free iron from a compound containing a little carbon, and not to a variation in the amount of nickel.

The nearest approach to the structure of meteoric iron is met with in the central portion of thick bars of Swedish iron, kept for some weeks at a temperature below their melting point, but high enough to give rise to recrystallisation. We then get a complete separation of free iron from a compound containing some carbon, and a crystalline structure which, as far as mere form is concerned, most closely corresponds with that of meteoric iron, as may be at once seen on comparing them.

These facts clearly indicate that the Widmanstatt's figuring is the result of such a complete separation of the constituents and perfect crystallisation as can occur only when the process takes place slowly and gradually. They appear to me to show that meteoric iron was kept for a long time at a heat just below the point of fusion, and that we should be by no means justified in concluding that it was not previously melted. Similar principles are applicable in the case of the iron masses found in Disco, and it by no means follows that they are meteoric because they show the Widmanstatt's figuring. Difference in the rate of cooling would serve very well to explain the difference in the structure of some meteoric iron, which do not differ in chemical composition; but, as far as the general structure is concerned, I think that we are quite at liberty to conclude that all may have been melted, if this will better explain other phenomena. On this supposition we may account for the separation of the iron from the stony meteorites, since under conditions which brought into play only a moderate gravitative force, the melted iron would subside through the melted stone, as happens in our furnaces; whilst at the same time, as shown in my paper read at the meeting of the British Association in 1864, where the separating force of gravitation was small, they might remain mixed together, as in the Pallas iron, and others of that type.

In conclusion I would say that though from want of adequate material for investigation I feel that what I have so far done is very incomplete, yet I think that the facts I have described will, at all events, serve to prove that the method of study employed cannot fail to yield most valuable results, and to throw much light on many problems of great interest and importance in several different branches of science.

MENDELEEF'S RESEARCHES ON MARIOTTE'S LAW[1]

FROM researches on the depression of the mercury results the possibility of introducing a precise correction relative to the volume of gas contained between the surface of the mercury and the horizontal plane which touches the summit of the meniscus. In all my researches I introduce each time a correction relative to this volume.

The volume of the reservoir which contains the mercury and the gas under various pressures undergoes two kinds of variations; first, those which are due to the difference between the pressures which act on the two sides of the vessel, and second, those which depend on differences in the volume of mercury. The compressibility of the reservoirs employed in the researches has been always determined by experiment, and their change of volume produced by the introduction of mercury can be determined by surrounding the vessel filled with mercury by another filled with the same material. When the height in the two vessels is the same, the capacity of the vessel is that which exists at the time of equality of pressure on the external and internal surfaces of the vessel. If we empty a part of the external vessel the capacity of the vessel changes in the same manner as when we fill or when we empty the vessel. Experiments of this kind have shown the possibility of determining the changes of capacity depending on the quantity of mercury. The relative corrections have in each case been introduced into the calculations.

All the practical side of the subject—the desiccation of the gas, the complete abstraction of the remains of the gas from the apparatus, the hermetical junction of the parts of the apparatus by means of mastic and mercury stop-valves, the means of maintaining the gases and the mercury at a constant temperature, the calibration of the tubes, and a number of other details have had to be elaborated more or less anew. All this will be found described in my work "On the Elasticity of Gases." I have published this work only in Russian, not having means sufficient to publish a translation of a work so voluminous, and desiring to conform to the custom existing among *savants* of all countries of describing their labours in their mother-tongue, in order to present to the scientific literature of the country where they live and work a gift in proportion to their powers.

My desire was to investigate the subject in its minutest details in order to eliminate every possibility of doubt as to the causes which determine the deviations observed from the Boyle-Mariotte Law. I know that that law is firmly established, and I believe it will remain so. Not less great is the certainty in the mind that rarefied gases approach the perfect state. That certainty I had also on commencing my experiments. It was necessary then to determine as completely as possible all the circumstances on which depend the facts contrary to the opinion generally held. This is why I have modified the apparatus, improved the methods, and employed in this work more than three years without interruption. Now so far as regards low pressures the work is finished, and I have obtained definitely certain proofs of the rigorous accuracy of my first observations.

The experiments which I have made with Kirpitchoff have proved that not only for air, but also for hydrogen, and even for carbonic acid, the deviations are positive when the gas is subjected to a very small pressure; it is found, moreover, that these deviations increase in proportion to the variation from the normal pressure. The same thing has been found in a new series of experiments undertaken by me with M. Hemilian. The experiments are described in tome ii. of my work on the "Elasticity of Gases," which I have just published. A brief extract on this subject is published in the *Ann. de Chimie et de Physique*, October, 1876. I shall quote only the results obtained by us from the experiments made in 1875 and in the beginning of 1876.

Into a new apparatus we have introduced several further improvements, of which the chief are:—(1) The baromanometer, the metre, and the reservoir, containing the gas and the mercury, have been placed in the same bath full of water; (2) We have succeeded in producing a complete vacuum in the barometric chamber; (3) The bath was maintained at an almost uniform temperature by means of an agitator, and the small differences in the temperatures of the various layers have been determined by a differential thermometer; (4) The junction between the air reservoir and the baromanometer has been made, not only without the aid of a tap, but also without the use of mastic.[1] Thus the gas was surrounded only by the glass and the mercury. We shall confine ourselves to a summary of the results of our experiments, made between 650 and 20 millimetres' pressure, with four gases — H, air, CO^2, and SO^2.

1. If, starting with a certain small pressure, we arrive at pressures smaller still, we find for all gases positive deviations, viz., $\frac{d(pv)}{dp} > 0$; the gases, then, are in this case less compressed than Mariotte's Law requires. Similar deviations were also observed for hydrogen by M. Regnault between 1 and 30 atmospheres, and M. Natterer for all gases between 100 and 3,000 atmospheres.

2. Under small pressures and for all gases, the value of the positive deviations, *i.e.*, the numerical quantity (or magnitude) $\frac{d(pv)}{dp}$, increases when the initial pressure diminishes. Thus, for example, for hydrogen at 400 millimetres—

$$\frac{d(pv)}{dp} = +0{\cdot}000002,$$

and at 120 millimetres—

$$\frac{d(pv)}{dp} = +0{\cdot}000010.$$

3. For gases like CO^2 and SO^2 we find near the atmospheric pressure, negative deviations; *e.g.*, for CO^2, $p_0 = 635$, $p_1 = 200$, $p_0 v_0 = 10{,}000$, $p_1 v_1 = 10{,}029$; but, under less pressures still, the deviations become positive even for CO^2 and SO^2. For example, for CO^2, $p_0 = 190$, $p_1 = 64$, $p_2 = 22$, $p_0 v_0 = 10{,}000$, $p_1 v_1 = 9{,}996$, $p_2 v_2 = 9{,}983$; for SO^2, $p_0 = 190$, $p_1 = 60$, $p_2 = 22$, $p_0 v^0 = 10{,}000$, $p_1 v_1 = 10{,}010$, $p_2 v_2 = 9{,}996$.

4. The existence of positive and negative deviations for the

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[1] Continued from p. 457.

[1] To attain this end the gas-vessel and the branch of the baromanometer are soldered together by a capillary tube made of a single piece.